Pieces of Paper uſed for Hangings were laid cloſe together upon the Ground, to the Breadth of ten Feet, in the Line of a Fowling-piece, between it and a Frame of 10 Feet ſquare, covered over with Paper. Upon pointing the Piece towards the Middle of the Frame, and *diſcharging* it ſeveral times *with* and *without Ball*, ſome Powder was always collected, but mixed with a great deal of Dirt.

It is however to be obſerved, that in two Experiments made the 22d of *July*, near the Artillery-Ground, before the Preſident and ſome of the Fellows of the *Society*, with a finer ſort of Powder, in a Barrel of 3 Feet 9 Inches in Length, and $\frac{1}{2}$ of an Inch Bore, with 12 *dwt.* of Powder the firſt time, and 24 *dwt.* the ſecond Time, without Ball or Wadding, no Powder could be found ſcattered on the Paper laid before the Piece, nor ſticking to a Board at the Diſtance of about 10 Feet, againſt which the Piece was pointed. But when the ſame Powder was fired in a ſhort Barrel of 5 $\frac{2}{10}$ Inches in the Chace, either with or without Ball, ſome Quantity of Powder was always collected.

Other Experiments were afterwards made before the Committee, by firing a Fowling-piece charged with 5 *dwt.* of Powder, againſt a Sheet of whited-brown Paper, at the Diſtance of 2 or 3 Yards; the Paper was found pierced with ſeveral Hundred Holes, and the Jags of the Paper appeared on the Backſide. In a ſecond Trial with 10 *dwt.* the Paper had more Holes in it. A third Trial was made with 5 *dwt.* of Powder and Ball, and then few Holes appeared in the Paper. In a fourth Experiment made with a ſhort Screw-barrel Piſtol, with a Charge of 1 *dwt.* 2 Grains of Powder and a Ball, ſeveral Holes were found in the Paper *.

But the Irregularities in this manner of collecting the Powder unfired, giving reaſon to ſuſpect, that ſome Powder eſcaped ſideways, beyond the Paper laid to receive it, it was propoſed to have a Machine made, which being cloſe every where but at the End where the Muzzle of the Piece was to be placed, might thereby hinder the Powder from being diſſipated. Such a Machine was contrived by Mr *Ellicot*, and by him preſented to the Committee, being a Frame of Wood in Shape like a truncated quadrangular Pyramid; at the ſmaller End was a Board to receive the Shot, and the 4 Sides of the Machine were covered with thick Paper ſtrongly paſted together, and ſo prepared as to prevent it's taking Fire. This Machine, ſupported by Props, was placed upon one of it's Angles, the Carriage for fixing the Barrels was placed cloſe to the greater Baſe, which was left open. The Reſult of the ſeveral Experiments were as follows:

The 3 firſt Experiments were made with a Barrel $\frac{8}{10}$ of an Inch Diameter of the Bore, and the Length of the Chace 5 $\frac{2}{10}$ Inches. The Charge

* That the Paper in theſe Experiments was pierced by the unfired Powder, appears, becauſe ſeveral Grains were found lying behind the Frame, to which the Paper was fixed, and ſome few ſtuck in the Paper.

each time was 6 *dwt.* of Powder without Ball; the Quantities of Powder collected were refpectively, 1 *dwt.* 19 Grains; 1 *dwt.* 21 Grains; and 1 *dwt.* 20 Grains.

Three other Experiments were made with the fame Piece, and with 12 *dwt.* Charge, without Ball. The Quantities of Powder collected were 4 *dwt.* 18 Grains; 4 *dwt.* 2 ½ Grains; and 4 *dwt.* 22 Grains.

The next 3 Trials were with the fame Piece, the Charge 6 *dwt.* with a Ball weighing one Ounce 4 *dwt.* being a Mixture of Lead and Tin, and fitting the Piece exactly.

The Quantities of Powder collected each time were refpectively 1 *dwt.* 5 Grains; 1 *dwt.* 5 Grains; and 1 *dwt.* 11 Grains.

The laft 3 Experiments with the fame Piece, were made with a Charge of 12 *dwt.* the Weight of the Ball as before; and the Quantities of Powder collected, were found to be 1 *dwt.* 12 Grains; 1 *dwt.* 9 Grains; and 1 *dwt.* 8 ½ Grains.

The Waddings ufed in all thefe and the following Experiments, were of thick Leather cut round, to fit the Bore of the Piece.

The Committee then proceeded to examine what Alteration might arife from a greater *Length of Chace.* The Experiments in this Cafe were made with a Barrel 3 Foot 9 Inches in Length, and ¾ of an Inch in the Bore; the Charges of Powder, and Weight of leaden Balls, were as before.

In the firft 3 Experiments with 6 *dwt.* Charge, without Ball, the Quantities of Powder collected were 3 Grains; 9 Grains; and 9 Grains, refpectively. In the 3 next Experiments, with twelve *dwt.* Charge, without Ball, the Quantities of Powder collected were 13 Grains; 9 Grains; and 16 ½ Grains. The 3 following Experiments were with 6 *dwt.* Charge and a Ball. The Powder collected was 2 Grains; 3 Grains; and 2 Grains.

The laft Experiments were made with 12 *dwt.* Charge and Ball as before; the Quantities of Powder collected from 2 Difcharges were refpectively, 2 Grains; and 4 ½ Grains. The Frame being broke, a third Experiment could not be made.

The Powder collected after the feveral Difcharges, was put into feparate Boxes; it feemed much bruifed, and mixed with Dirt. Yet feveral of the Parcels being tried, fired with brifk Explofions; and fome of the Powder collected from the Experiments with the fhort Barrel, amounting to 6 *dwt.* 16 Grains, being put into the long Barrel, and fired with Ball, went off with a ftrong Report; and the Ball pierced the Deal-board, at the End of the Frame, and penetrated 2 Inches deep into an Elm-plank placed to receive the Balls.

Some Gentlemen, prefent at thefe Experiments, fufpecting that Part of the Powder might efcape at the open End of the Frame; the fhort Barrel was fired with 12 *dwt.* of Powder and Ball, as before; through a very large Funnel, the Quantities found, after three Difcharges, were feverally, 1 *dwt.* 2 Grains; 16 Grains; and 15 Grains.

Whereas

Whereas upon removing the Funnel, and diſcharging the Piece, as before, 1 *dwt.* 11 Grains was collected, agreeably to former Experiments; it ſeems that the Funnel had a like Effect as lengthening the Piece.

Some Experiments were alſo made with the ſhort Barrel, filled up with Lead, ſo as to leave but 3 ¼ Inches for the Chace, the Piece being then charged with 12 *dwt.* of Powder and Ball, as before; the Surface of the Ball was but $\frac{8}{10}$ of an Inch within the Mouth of the Piece, and the Powder collected, after 3 Diſcharges, was reſpectively, 2 *dwt.* 2 Grains; 1 *dwt.* 17 Grains; and 1 *dwt.* 11 Grains.

The Barrel being further filled up, ſo as to leave but 2 $\frac{8}{10}$ Inches for the Chace, and charged as before, the Ball riſing about ⅕ of an Inch beyond the Mouth of the Piece, the Powder collected, after the Diſcharge, was 2 *dwt.* 6 Grains. Upon a ſecond Trial, the Ball being as much within the Mouth, 1 *dwt.* 16 Grains was collected. And at the third Trial, the Ball being level with the Mouth, 2 *dwt.* 6 Grains were again found.

The Committee alſo cauſed ſome Experiments to be made of the Effect of a *Touch-hole* near the Forepart of the Charge. They found upon diſcharging the ſhort Piece of 5 $\frac{2}{10}$ of an Inch Chace, the Charge 12 *dwt.* and Ball, as before, the Touch-hole being near the Fore-part of the Powder; the Quantities of Powder, ſeverally collected, were 1 *dwt.* 7 ½ Grains; 1 *dwt.* 6 Grains; and 1 *dwt.* 4 Grains. And upon a Diſcharge made with a little more Powder, which filled the Barrel exactly to the Edge of another Touch-hole, the former being ſcrewed up, the Quantity collected was 1 *dwt.* 9 Grains.

The Effect of firing with *heavy Slugs* was alſo examined: The Weight of the Slugs and Quantities of Powder collected, were as follows; the Charge in the ſhort Barrel being 12 *dwt.*

Diſcharge.	Weight of Slugs.			Powder collected.	
	Ounces.	*dwt.*	*gr.*	*dwt.*	*gr.*
I.	2.	13.	0.	1.	3.
II.	2.	11.	14.	0.	17.
III.	2.	12.	0.	0.	8.
IV.	5.	5.	6.	0.	13.
V.	5.	3.	0.	0.	8½.

The Powder uſed in all theſe Experiments, made before the Committeee, was preſented to them by Mr *Walton*, and is ſuch as he makes for the King's Service. To aſcertain as nearly as poſſible, that the Powder had not undergone any conſiderable Alteration by Damps or otherwiſe, a Standard Experiment was previouſly made at every Meeting, with the ſhort Barrel charged with 12 *dwt.* of Powder, and with a Ball of 24 *dwt.*; and the Quantity of Powder collected was from 1 *dwt* 8 Grains, to 1 *dwt.* 12 Grains; which is as great a Regularity as can well

well be expected. This Powder of Mr *Walton*'s being *sifted*, and divided into a *fine* and a *large Sort*, the following Discharges were made with 12 *dwt.* of each, and Ball as usual:

Discharges with fine Powder.	Powder collected. *dwt.*	*gr.*
I. — — — — — — —	1.	4.
II. — — — — — — —	0.	21.
III. — — — — — — —	0.	12.

In this third Experiment the Bullet, not being so exactly turned as the others, was rammed down with great Force.

Discharge with large Powder.	Powder collected. *dwt.*	*gr.*
I. — — — — — — — —	1.	11.
II. — — — — — — —	1.	16.
III. — — — — — — —	1.	21.

And the Powder being bruised in a Mortar, and sifted through a Lawn Sieve, the Charge and Ball being as before, what was collected after 3 Discharges. was one *dwt.* 10 Grains, 1 *dwt.* 8 Grains, and 17 Grains.

Mr *Watson* having had two Parcels of Powder delivered to him, the one fresh, and the other collected after Discharges with Ball, gave an Account of the Quantity of Nitre he had separated from them, *viz.*

	dwt.	*gr.*
Separated from 9 *dwt.* of fresh Powder — — —		
Nitre — — — —	6.	2.
Residuum — — —	2.	7.
Loss — — — —	0.	15.

	dwt.	*gr.*
From 9 *dwt.* of Powder collected after having been discharged with Ball — — — — — — —		
Nitre — — —	4.	18.
Residuum — — —	2.	15.
Sand, &c. — — —	0.	11.
Loss — — — —	1.	14.

Twelve Grains of the Powder gathered and put into separate Boxes, after firing with Ball out of the short Piece, as before-mentioned, being fired in the exhausted Receiver, sunk the Mercurial Gage from 29 $\frac{1}{10}$ Inches to 23 $\frac{6}{10}$. And the same Weight of fresh Powder being fired in the same manner, sunk the Gage to 22 $\frac{3}{4}$ Inches; the Difference being $\frac{85}{100}$ or $\frac{17}{20}$ of an Inch.

From these Experiments the Committee are of Opinion, that the first Part of the first Question, *Whether all the Powder of the Charge be fired?* is sufficiently determined in the *Negative.*

As to the Second Part of the first Question, *Whether all the Powder that is fired, be fired before the Bullet is sensibly moved from it's Place?* the Committee are of Opinion, *That the Bullet is sensibly moved from it's Place, before all the Powder that is fired, has taken Fire.*

This, indeed, has not been determined by any direct Experiment, but seems a Consequence of the Determination of the first Part of the Question, that the whole of the Charge is not fired.

For let it be considered, that from the Moment any Part of the Powder within the Barrel takes Fire, the Flame of the Powder already fired is always contiguous to some Part of the Powder as yet unfired; and consequently some Part of this last must be continually taking Fire, so long as any unfired Powder remains within the Barrel; that is, the firing of the Powder cannot be over, till all the unfired Powder is driven out of the Gun: But before any Part, how small soever, of the unfired Powder is driven out of the Gun, the Bullet which lies between the Charge and the Muzzle, must necessarily have been driven out of the Gun. Therefore the firing of the Powder is not over, or all the Powder that is fired, is not fired, till after the Bullet is driven out of the Gun. And consequently the Bullet must be sensibly moved from it's Place, before all the Powder that is fired, has taken Fire.

As to the second Question, *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?*

The Committee are of Opinion, *That the Change of the Form in the Chamber, will produce a Change of the Distance to which the Bullet is thrown.* Their Opinion is grounded upon the following Experiments, in which the *longest Chamber* of equal Capacity drove the Ball farthest.

Three brass Chambers were made, whose Depths were respectively 3 Inches; 1 ½ Inch; and ¾ of an Inch; so turned as to fit the Chamber of Mr *Hauksbee*'s Mortar; each of these Chambers contained, when full, 1 Ounce *Troy* of Powder. The Ball was of Brass, weighing 24 Pound, 6 ½ Ounces *Avoirdupois*, that is, nearly 356 Ounces *Troy* *.

The Ball touched the Powder of the Charge in all these Experiments. With the first Chamber of 3 Inches deep, the Elevation of the Mortar being 45 Degrees, the Ranges at 4 different Trials were found to be,

* Supposing 14 Ounces 11 dwt. and 15 Grains and an half, *Troy*, equal to 1 Pound *Avoirdupois*.

Shot.

Shot.	*Chains.*	*Links.*	
I. — — — — —	11.	39.	or nearly 752 Feet.
II. — — — — —	10.	38.	685.
III. — — — — —	11.	17.	737.
IV. — — — — —	11.	10.	733.

In the Second of theſe Experiments, the braſs Chamber, not being ſufficiently thruſt home before the Diſcharge, was by the Violence of the Powder driven in ſo, that it could not be got out again without the Help of an iron Screw, and a vaſt Force applied to iron Wedges. This was doubtleſs the Cauſe of the great Irregularity obſerved in this Caſe. The mean Diſtance, collected from the other 3 Experiments, is nearly 741 Feet.

Then 3 Diſcharges were made with the Chamber $\frac{1}{4}$ of an Inch deep, with Ball, Powder, and Elevation, as before. The Ranges were,

Shot.	*Chains.*	*Links.*	
I. — — — — — —	7.	6.	or 466 Feet nearly.
II. — — — — —	7.	2.	463.
III. — — — — —	7.	2.	463.

The mean Diſtance to which the Ball was thrown in theſe three Experiments is 464 Feet.

The Chamber 1 $\frac{1}{2}$ Inch deep, was alſo tried; but this not fitting the Mortar ſo well as the other 2, the Ranges were found to be very irregular, being

Shot.	*Chains.*	*Links.*	
I. — — — — —	10.	40.	or nearly 686 Feet.
II. — — — — —	9.	6.	598.
III. — — — — — —	7.	8.	467.

The laſt Shot, falling ſo much ſhort, may be aſcribed to the Damp, it being late in the Evening when it was fired.

That Moiſture greatly weakens the Effect of Powder, is commonly known; and the Committee found by an Experiment, That Powder dried by means of a Phial in *Balneo*, and put warm into the Chamber, threw the Ball twice as far as the ſame Quantity of Powder taken out of the ſame Barrel, before it was dried.

VII. This Treatiſe contains 2 Chapters. The Firſt treats of the Force of Gunpowder, and the Velocities communicated to Bullets by it's Exploſion: The ſecond conſiders the Reſiſtance of the Air to Bullets and Shells moving with great Velocities; and endeavours to evince, that this Reſiſtance is much beyond what it is generally eſteemed to be; and conſequently that the Tract deſcribed by the Flight of theſe Projectiles,

An Account of a Book intituled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder;

and an Investigation of the resisting Power of the Air to swift and slow Motions; by B. R. F. R. S. as far as the same relates to the Force of Gunpowder. Read April 14. *and* 21. 1743.

is very different from what is usually supposed by the modern Writers on this Subject.

The principal Points endeavoured to be established in the first Chapter are these, "That the Force of fired Gunpowder is no more than the "Action of a permanent elastic Fluid, which is produced by the Explosion; that this Fluid observes the same Laws with common Air "in their Exertion of it's Pressure or Elasticity;" and consequently, "That the Velocities communicated to Bullets by the Explosion, may "be easily computed from the common Rules, which are established "for the Determination of the Air's Elasticity."

The two first Propositions contain the Proofs that a permanent elastic Fluid is constantly generated in the Explosion of Gunpowder; this is evinced by well known Experiments daily repeated, and acquiesced in by all who have frequented the usual Courses of Experimental Philosophy, of which these Experiments generally make a Part; so that the Author presumes he may consider this Point as incontestibly established, at least he has never yet met with any who have questioned it.

The third Proposition is, That the Elasticity of this Fluid produced by the firing of Gunpowder, is, *cæteris paribus*, directly as it's Density; and the Experiment by which this was confirmed, was letting fall separately 2 Quantities of Powder, the one double the other, on a red-hot Iron included in an exhausted Receiver; and it appeared by the Descent of the Mercury, that the Elasticity of the Fluid produced from the double Quantity of Powder, was nearly double the Elasticity of that produced from the single Quantity; that is, the Elasticity was nearly as the Density of the Fluid.

But it may perhaps be thought, that a single Experiment is too slender a Foundation on which to build so material a Principle, since all subsequent Reasonings on the Force of Powder in some measure depend on it. In Reply to this it may be said, that the Author recited this single Experiment on account of the great Quantity of Powder made use of in it, which was $\frac{1}{16}$ of an Ounce; but that he had really made many more equally conclusive, which he thought it unnecessary to mention. However, those who doubt of this Proposition, may satisfy themselves herein by some Experiments made by the late Mr *Hauksbee* before this *Society*, though with a different View; where, by the firing of 26 Quantities of Powder successively, the mercurial Gage was sunk from $29\frac{1}{2}$ Inches, to $12\frac{1}{2}$; for by comparing these Experiments together, and making the necessary Allowances, it will be found, that the Elasticity was nearly proportional to the Density in all that Variety of Densities.

In this Proposition, the Analogy between the Fluid produced by the Explosion of Powder and common Air, is established thus far, that they exert equal Elasticities in like Circumstances; for this Variation of the Elasticity, in proportion to the Density, is a well known Property of common Air. But other Authors, who, since the Time of Mr *Boyle*, have

have examined the factitious elastic Fluids produced by Burning, Distillation, &c. have carried this Analogy much farther, and have supposed these Fluids to be real Air, endued with all the Properties of that we breathe; particularly the Reverend Dr *Hales*, who has pursued this Examination with the greatest Exactness, in a Series of the best contrived Processes, constantly affixes the Denomination of Air to these factitious Fluids, he having found, that their Weight is the same with that of common Air, and that they dilate with Heat, and contract with Cold; and that they vary their Densities under different Degrees of Impression in the same Proportion with common Air; and from hence, and other Circumstances of Agreement between them, he supposes them to be of the same Nature with Air, and conceives them to be fitly designed by the same Name.

But so perfect a Congruity between these factitious Fluids and Air is not necessary for the Purposes of this Treatise. The fundamental Positions of this first Chapter supposing no more, than that the Elasticity of the Fluid produced in the Explosion of Gunpowder is always, *cæteris paribus*, as it's Density; and that the Force of fired Gunpowder is only the Action of that Fluid modified according to this Law. It has been already mentioned, on what Grounds the First of these Principles hath been asserted, as contained in the Third Proposition; and it remains to explain the Reasons urged for the Support of the last in the 8 succeeding Propositions.

The Law of the Action of this Fluid being determined, 2 Methods offer themselves for investigating the absolute Force of Powder on the Bodies it impels before it. The first by examining the Quantity of this Fluid produced by a given Quantity of Powder, and thence finding it's Elasticity at the Instant of the Explosion; the other by determining the actual Velocities communicated to Bullets by known Charges, acting through Barrels of different Dimensions. The first is the most easy and obvious, but the second the most accurate Method; and therefore the Author has separately pursued each, and he has found, that their Concurrence has greatly exceeded his Expectation, and thereby both of them receive an additional Confirmation.

The Quantity of the elastic Fluid, produced by the Firing of a given Quantity of Powder, is determined by firing it in an exhausted Receiver, and observing how much the mercurial Gage subsides thereby, making a proper Allowance for the Increase of it's Elasticity from the Heat of the included hot Iron. But then as the Subsiding of the Mercury is not measured till the Flame of the Powder is extinguished, and the Fluid is reduced somewhat near the Temperature of the external Air, it is evident, that the Elasticity thus estimated is much short of what it really was in the Instant of Explosion; and therefore, to obtain that Elasticity, which is the Force sought, it is necessary to make some Estimate of the Increase of the Elasticity of the Fluid by the Fire and Flame of the Explosion. For this Purpose it is examined in the Fifth

Propoſition, how much the Elaſticity of common Air is increaſed by a Degree of Heat equal to that of Iron beginning to grow white hot; and it is found, at a Medium, to be thereby augmented ſomething more than 4 Times; whence, as the Fluid produced by any Quantity of Gunpowder takes up, when compreſſed by the Weight of the incumbent Atmoſphere, a Space ſomething leſs than 250 times the Bulk of the Powder; it follows, that if it's Elaſticity in the Inſtant of Exploſion be ſuppoſed to be increaſed in the ſame Proportion with that of the Air laſt-mentioned, it becomes by this means about 1000 times greater than the Preſſure of the Atmoſphere; that is, conceiving it to be contained in that Space only which the Powder occupied before it was fired.

Thoſe who have not been converſant in theſe Experiments, may poſſibly ſuppoſe, that the Elaſticity of the Powder at the Inſtant of Exploſion may be immediately known by the firſt ſudden Deſcent of the Mercury: But many Circumſtances concur to render this Method impracticable; amongſt the reſt it muſt be remembred, that ſome Air is conſtantly left in the Receiver, which is heated by the Blaſt, and unites it's Effects in the firſt Inſtant with the Action of the Powder: Beſides, the firſt Deſcent may be varied, by varying the Tube, although all things elſe remain unchanged.

By the Method hitherto deſcribed, it is collected, that the Elaſticity of the Fluid produced from fired Gunpowder, when contained in the Space which was taken up by the Powder before the Exploſion, is about 1000 times greater than the Elaſticity of common Air, or, which is the ſame thing, 1000 times greater than the Preſſure of the Atmoſphere.

But, beſides the Determination of the Quantity of Fluid produced from a given Quantity of Powder, (the Method on which this Deduction is founded) there is another Method of diſcovering the ſame thing, which, though leſs obvious, is yet (as hath been already obſerved) more accurate: That is, by examining the actual Velocities communicated to Bullets by the Exploſion of given Charges in given Cylinders; and this is the Subject of the 7th, 8th, and 9th Propoſitions.

And firſt, it is evident, that this Examination cannot take place, unleſs a Method of diſcovering the Velocities of Bullets be previouſly eſtabliſhed. Now the only known Means of effecting this was, either by obſerving the Time of the Flight of Bullets through a given Space; or by finding their Ranges when they were projected at a given Angle, and thence computing their Velocity on the Hypotheſis of their parabolic Motion. The firſt of theſe Methods was often impracticable, and in all great Velocities extremely inaccurate, both on account of the Shortneſs of the Time of their Flight, and the Reſiſtance of the Air. The ſecond is ſtill more exceptionable, ſince, by reaſon of the Air's Reſiſtance, the Velocities thus found may be lefs in any *Ratio* given, than the real Velocity fought. Now, to avoid thefe Difficulties, the Author has invented a Method of determining the Velocities of Bullets, which may be carried to any required Degree of Exactnefs, and is no-ways liable to the forementioned Exceptions; for, by this Invention, the Velocity of the Bullet is found in any Point of it's Track, independent of the Velocity it had before it arrived at that Point, or of the Velocity it would have after it had paffed it: So that not only the original Velocity, with which it iffues from the Piece, is hence known, but alfo it's Velocity, after it has paffed to any given Diftance; and therefore the Variations of it's Velocity from the Refiftance of the Air may be alfo afcertained with great Facility. The Machine for this Purpofe is defcribed in the 8th Propofition, and the Principle it is founded on is this fimple Axiom of Mechanicks; *That if a Body in Motion ftrikes on another at Reft, and they are not feparated after the Stroke, but move on with one common Motion, then that common Motion is equal to the Motion with which the Firft Body moved before the Stroke:* Whence, if that common Motion and the Maffes of the 2 Bodies are known, the Motion of the Firft Body before the Stroke is thence determined. On this Principle then it follows, that the Velocity of a Bullet may be diminifhed in any given *Ratio*, by it's being made to impinge on a Body of a Weight properly proportioned to it; and hereby the moft violent Motions, which would otherwife efcape our Examination, are eafily determined by thefe retarded Motions, which have a given Relation to them. Hence then, if a heavy Body greatly exceeding the Weight of the Bullet, whofe Velocity is wanted, be fufpended, fo that it may vibrate freely on an Axis in the manner of a Pendulum, and the Bullet impinges on it when it is at Reft, the Velocity of the Pendulum after the Stroke will be eafily known by the Extent of it's Vibration, and from thence, and the known Relation of the Weight of the Bullet and the Pendulum, and the Pofition of the Axis of Ofcillation, the Velocity with which the Bullet is impinged will be determined, as is largely explained in the 8th Propofition. Where note, that there is a Paragraph by Miftake omitted in that Propofition, which fhould increafe the Velocity there found in the duplicate Proportion of the Diftances of the Points of Ofcillation and Percuffion from the Axis of Sufpenfion; but this only affects that particular Number, for it was remembered in the Computations of the fucceeding Experiments, the Numbers of which are truly ftated.

It being explained how the Velocities of Bullets may de difcovered by Experiment: The next Confideration is, from thofe Velocities to determine the Force which produced them.

And the Author thought, the beft Method of effecting this was by computing what Velocities would arife from the Action of fired Powder, fuppofing it's Force to be rightly affumed by the Procefs in the preceding Part; that is, fuppofing the Elafticity of the Fluid thence

arifing

ariſing to be at firſt 1000 times greater than that of common Air; for then, by comparing the Reſult of theſe Computations with a great Number of different Experiments, it would appear whether that Force was rightly aſſigned; and if not, in what Degree it was to be corrected.

Preparatory to this Computation, the Author aſſumes in his 7th Propoſition theſe Two Principles:

1ſt, That the Action of the Powder on the Bullet ceaſes as ſoon as the Bullet is got out of the Piece.

2dly, That all the Powder of the Charge is fired, and converted into an elaſtic Fluid, before the Bullet is ſenſibly moved from it's Place.

And in the annexed Scholium he has given the Arguments and Experiments which induced him to rely on theſe Poſtulates, all which is neceſſary at preſent to diſcuſs more at large.

If the Force of Gunpowder was ſuppoſed capable of being determined with the ſame Accuracy and Rigour, which takes place in Subjects purely Geometrical, the firſt of theſe Poſtulates would be doubtleſs erroneous, ſince it cannot be queſtioned but the Flame acts in ſome Degree on the Bullet after it is out of the Piece.

But it is well known, that in Experimental Subjects no ſuch Preciſeneſs is attainable; for thoſe verſed in Experiments perpetually find, that either the unavoidable Irregularities of their Materials, or the Variation of ſome unobſerved Circumſtance, occaſion very diſcernible Differences in the Event of ſimilar Trials. Thus the Experiments made uſe of for confirming the Laws of the Colliſion of Bodies, have never been found abſolutely to coincide either with the Theory, or with each other. The ſame is true of the Experiments on the Running and Spouting of Water, and other Fluids, and of the Experiments made by Sir *I. Newton*, for the Confirmation of his Theory of Reſiſtances; in which, though they often differ from each other, and from that Theory by $\frac{1}{20}$, $\frac{1}{15}$, and even ſometimes $\frac{1}{4}$ Part, yet thoſe ſmall Inequalities have never been urged as invalidating his Concluſions, ſince, in Experiments of that Nature, it was rather to be wondered at, that the Difference between the different Trials was ſo ſmall.

And if ſome minute Irregularities are the neceſſary Concomitants of all complicated Experiments, it may be well ſuppoſed, that the Action of ſo furious a Power as that of fired Gunpowder, which viſibly agitates and diſorders all Parts of the Apparatus made uſe of, cannot but be attended with ſenſible Variations; and it in Fact appears, that in the Table of Experiments inſerted in the 9th Propoſition, the Velocities of Bullets fired from the ſame Piece, charged with the ſame Powder, and all Circumſtances as near as poſſible the ſame, do yet differ from each other by $\frac{1}{50}$, $\frac{1}{40}$, and ſometimes more than $\frac{1}{30}$, of the Whole; and yet the Author does not conceive, that theſe ſmall Differences are any Exception to the Concluſiveneſs of his Principles;

but

but he preſumes, that had he pretended, without diſcloſing his Method, to have computed the Force of Powder, and the Velocities of Bullets, in different Circumſtances, to a much leſs Degree of Accuracy than this, he ſhould have been cenſured, as boaſting of what would have been thought impracticable.

If then the Action of the Flame on the Bullet after it is out of the Piece, is ſo ſmall as to produce no greater an Effect than what may be deſtroyed by the inevitable Variations of the Experiments, the neglecting it entirely, and ſuppoſing no ſuch Force to take place, is both a convenient and a reaſonable Procedure: For indeed, without the Aſſumption of Poſtulates of this kind, it were impoſſible to have proceeded one Step in Natural Philoſophy, ſince no Mechanick Problem hath been ever ſolved, in which every real Inequality of the moving Force hath been conſidered.

Now what induced the Author to ſuppoſe, that this Poſtulate (though not rigorouſly true) might be ſafely aſſumed, was the Conſideration of the ſpreading of the Flame by it's own Elaſticity, as ſoon as it eſcapes from the Mouth of the Piece: For by this means he conceived that the Part of it which impinged on the Bullet might be ſafely neglected, although the Impulſe of the entire Flame was a very remarkable Force.

With regard to the Second Poſtulate, "That all the Powder is "fired before the Bullet is ſenſibly moved from it's Place;" it is incumbent on the Author to be ſtill more explicit, as this *Society* did ſome time ſince appoint a Committee for examining this very Poſition, who, after making a great Number of Experiments, have determined, * *That all the Powder is not fired before the Bullet is ſenſibly moved from it's Place*; and they have at the ſame time aſſigned the Quantities remaining unfired under different Circumſtances.

Theſe Determinations of the Committee are moſt true; but the Author muſt obſerve, that from the Experiments recited by them, and the Quantity of unfired Powder, which they collected, it may be concluded, that in a Barrel of a cuſtomary Length, charged with the uſual Quantity of Powder, the Deficiency of Velocity occaſioned by the Powder remaining unfired will be ſcarcely ſenſible; and in the ſhorteſt Barrel ever uſed by the Author, where the Space the Bullet was impelled through was not five Inches, and where of courſe this Deficiency of Velocity ought to be the greateſt, it cannot amount to $\frac{1}{15}$ Part of the Whole; and conſequently this Poſtulate, though not rigorouſly true, may yet be ſafely aſſumed, in the inveſtigating the Effects of Powder. But before this is more particularly examined, it is neceſſary to explain the Opinions, which have formerly taken place on this Subject.

* See the preceding Paper.

Thoſe who have hitherto wrote on the Manner in which Powder takes Fire, have ſuppoſed it to be done by regular Degrees; the firſt Grains firing thoſe contiguous, and they the next ſucceſſively; and it has been generally thought, that a conſiderable Time was employed in theſe various Communications: For Mr *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from ſome Experiments made at *Petersburgh*, that the greateſt Part of the Charge eſcapes out of the Piece unfired, and that the ſmall Part, which is fired, does not take Fire till it is near the Mouth. Many Theories too have been compoſed on the Time of the Progreſs of the Fire amongſt the Grains, and the different Modifications which the Force of Powder did thence receive; and it has been generally conceived, that the proper Lengths of Pieces were determinable from this Principle; "That they ſhould "be long enough to give Time for all the Powder to fire."

But the Author being ſatisfied, that no ſuch regular and progreſſive Steps could be obſerved in the Exploſion; and having found, that by loading with a greater Weight of Bullet, and thereby almoſt doubling the Time of the Continuance of the Powder in the Barrel, it's Force received but an inconſiderable Augmentation; and finding too, that doubling or trebling the uſual Charge, the Powder thus added always produced a correſpondent Effect in the Velocity of the Bullet; and diſcovering likewiſe in a Piece near 4 Feet in Length, charged with an uſual Charge of Powder, that the Velocity communicated to the Bullet, during the firſt 3 Inches of it's Motion, was full half the Velocity which it acquired in it's whole Paſſage through the Barrel, and that the Elaſticity or Force of the Powder, in the firſt 3 Inches of it's Expanſion, was, at a Medium, near 8 times greater than in the laſt 2 Feet of the Barrel; he concluded from all theſe Circumſtances, that the Time employed by the Powder in taking Fire was not neceſſary to be attended to in theſe Computations; but that the whole Maſs might be ſuppoſed to be kindled, before the Bullet was ſenſibly moved from it's Place.

And the Experiments reported by the Committee are the ſtrongeſt Proofs, (as far as they extend) that Powder is not fired in the progreſſive Manner uſually ſuppoſed; for when the ſhort Barrel was charged with 12 *dwt.* and with 6 *dwt.* reſpectively, the Quantity of Powder which was collected unfired from 12 *dwt.* did not exceed by 3 Grains, at a Medium, what was collected from 6 *dwt.* although the Bullet was a leſs Time in paſſing through the Barrel with 12 *dwt.* than with 6 *dwt.* it having a leſs Way to move; conſequently the Quantity remaining unfired of the 6 *dwt.* did not continue unfired for want of Time, ſince, when the Piece was charged with 12 *dwt.* the additional 6 *dwt.* was conſumed in a ſhorter Time.

And again, when the Barrel was ſo ſhortened, that the Bullet, being placed cloſe to the Wad, lay with it's outer Surface nearly level with the Mouth of the Piece, ſo that it had not more than half an

Inch

Inch to move before the Flame would have Liberty to expand *itself*; yet, even in this short Transit of the Bullet, only 2 *dwt.* 1 $\frac{1}{2}$ *gr.* was collected unfired, at a Medium; which is about $\frac{1}{6}$ of the whole Charge, or, if properly reduced, not more than $\frac{1}{12}$ of the Charge: An obvious Confutation of the gradual firing of the Powder in it's Passage through the Barrel, and an easy Proof, how small an Error will be occasioned by supposing the whole Charge to fire instantaneously, since the Error in the Velocity of the Bullet, arising from a Deficiency of $\frac{1}{12}$ of the Charge, is $\frac{1}{24}$ of that Velocity only.

I say, that the $\frac{1}{6}$ of the Charge, which remained unfired, amounts to no more than $\frac{1}{12}$ when it is reduced as it ought. This Reduction is founded on the other Experiments reported by the Committee, and on the Circumstances of those Trials on which the Author founded the present Postulate. The Author has supposed the Powder, on which he reasons in this Treatise, to be of the same sort with that made for the Service of the Government, a Parcel of which he was favoured with by Mr *Walton*. But this he chiefly kept for a Standard, and generally used other Powders, which, on Examination, he found to be of equal Force. These Powders were of a very small and even Grain, and the Committee have found, that by sifting the Government Powder, and making use of the smaller Grains, the Quantity remaining unfired was less, at a Medium, in the *Ratio* of 5 to 3, than when it was used without sifting.

And again, it was found by extracting the Saltpetre from the Powder collected unfired, that there was less Saltpetre contained in it than in real Powder, and this nearly in the *Ratio* of 9 to 7: These two Proportions compounded make the Proportion of 15 to 7, and in this Proportion must the Quantities of Powder collected unfired be reduced, in order to determine the Quantities of real Powder remaining unfired, in similar Experiments made by the Author.

And from hence it follows, that in the Experiments made with a Barrel of 5 $\frac{1}{3}$ Inches in Length, where the Ball had not 3 Inches to move, and where the Irregularity arising from the Powder unfired ought to have been the most sensible, the Quantity of real Powder collected unfired from a Charge of 12 *dwt.* would have been no more than 16 Grains at a Medium, or $\frac{1}{18}$ of the whole Charge; and it being found by Experiment, that the Velocities of Bullets placed in the same Situation vary in the subduplicate Proportion of the Charges, the Deficiency of Velocity arising from the Loss of the $\frac{1}{18}$ of the Charge would be about $\frac{1}{36}$ of the whole Velocity only, which, in the present Case, is not $\frac{2}{10}$ of an Inch in the Chord of the Arch described by the Pendulum measuring the Velocity, and is a less Difference than what frequently occurs in the exactest Repetition of the same Experiments.

Other Circumstances occur, which reduce the Inequality arising from the unfired Powder still lower; but it is thought, that this is fully ſufficient to juſtify the Poſtulate in Queſtion, eſpecially as, in all Caſes of real Uſe, the Length of the Barrel in proportion to the Quantity of the Charge will be much greater than in the preſent Inſtance: Whence the Author preſumes, that, in computing the Velocities communicated to Bullets by the Action of Powder, it may be ſafely ſuppoſed, that the whole Charge is fired before the Bullet is ſenſibly moved from it's Place; at leaſt there is no Foundation, from the Experiments made on this Subject by the Committee, to ſuſpect that when ſmall-grained Powder is made uſe of, any greater Irregularity will ariſe from the Application of this Suppoſition, than what would otherwiſe take place from the Intervention of unavoidable Accidents.

It has been thought neceſſary to diſcuſs more at large theſe two Poſtulates, becauſe the laſt of them being almoſt in the very Words of one of the Queſtions propoſed to be examined by the Committee of this *Society*, and having by them been determined in the Negative, thoſe who have not attended to this Subject might ſuppoſe, that thereby the Author's Principles were entirely overturned: Now this would be a great Injuſtice to him, ſince he has not relied on this Poſtulate as rigorouſly true; for he knew, and has himſelf taken notice in the preſent Propoſition, that ſome of the Powder eſcapes unfired; and he has there made ſome Conjectures on the Cauſe of it; but, without inſiſting on the Reality of thoſe Conjectures, he adds, that, "Be that as it may, the Truth of our "Poſition cannot in general be queſtioned."

And though it appears from what has been already ſaid, that the Experiments recited by the Committee rather confirm than invalidate the general Senſe of that Poſtulate; yet it is but Juſtice to own, that they are a full Confutation of the Conjectures of the Author in relation to the Cauſe why ſome Part of the Powder comes out unfired; for the Author has ſuppoſed, after *Diego Uſano*, that the Part which thus eſcaped, was ſcattered in the Barrel, and not rammed up with the reſt, or elſe that it was of a leſs inflammable Compoſition: But the Experiments made on this Occaſion entirely deſtroy this Suppoſition.

As this, or any other Conjecture on the Cauſe of this Accident, (for it plainly appears not to be for want of Time only) has nothing to do with the general Reaſoning of the preſent Treatiſe, it is not neceſſary to enter into it in this Place; but it may not be improper to mention, that, on computing the Quantities of Powder collected from different Charges, one of the Committee was led to conjecture, that what was thus collected was only Parts of Grains that had been fired, but were extinguiſhed by the Blaſt before they were entirely conſumed. This Conjecture is ſtrengthened by the extreme Minuteneſs of the Particles of all the Powder which was collected, and from the Deficiency of the Saltpetre found in it on Examination: It may be added too, that the Author, by gradually heating a Parcel of Powder, hath ſet it on Fire, and blown it out again, for at leaſt a Dozen times ſucceſſively; and he will undertake to repeat the Experiment at any time, if it ſhould be doubted of.

The

The Poſtulates hitherto diſcuſſed are preparatory to the 7th Propoſition. That Propoſition is employed in computing the Velocity which would be communicated to a Bullet in a given Piece by a given Charge of Powder, on the Principles hitherto laid down, that is, ſuppoſing the Elaſticity of fired Powder to be at firſt 1000 times greater than that of common Air.

In the 9th Propoſition theſe Computations are compared with a great Number of Experiments, made in Barrels of various Lengths, from 7 Inches to 45 Inches, and with different Quantities of Powder, from 6 *dwt.* to 36; and the Coincidence between the Theory and theſe Experiments is very ſingular, and ſuch as occurs in but few philoſophical Subjects of ſo complicated a Nature.

By this Agreement between the Theory and the Experiments, each Part of the Theory is ſeparately confirmed; for by firing different Quantities of Powder in the ſame Piece, and in the ſame Cavity, it appears that the Velocities of the Bullet, thence ariſing, are extremely near the ſubduplicate Proportion of thoſe Quantities of Powder, and this independent of the Length of the Piece: Whence it is confirmed, that the Elaſticity of fired Powder in various Circumſtances, is nearly as it's Denſity; and this does not only ſucceed in ſmall Quantities of Powder, and in ſmall Pieces, but in the largeſt likewiſe, under proper Reſtrictions; at leaſt there are Experiments which could not be influenced by this Theory, where the Quantities of Powder were above 100 times greater than what are uſed by this Author, and in theſe Trials this Circumſtance takes place to ſufficient Exactneſs.

It is preſumed then, that by this Theory a near Eſtimate may be always made of the Velocities communicated to Shells or Bullets by given Charges of Powder; at leaſt theſe Experiments evince how truly the Velocities of ſmall Bullets are hereby aſſigned; and the Author can ſhew by the Experiments of others, that in a Shell of 13 Inches Diameter, impelled by a full Charge of Powder, the ſame Principle nearly holds: It is true indeed, that when the Charge is much ſmaller than the uſual Allotment of Powder, there are ſome Irregularities, which are particularly mentioned at the End of Prop. 9. to which Head too, perhaps, muſt be referred the Experiments made by the Committee on the Effect of different ſmall Chambers; but in the cuſtomary Charges, the Velocities of Bullets reſulting from all the Experiments hitherto made, are really ſuch as the Theory laid down in the preceding Part of this Treatiſe requires. And it appears, that theſe Velocities are much greater than what they have been hitherto accounted: And there are Reaſons from the Theory to believe, that in Cannon-ſhot the Velocities may ſtill exceed the preſent Computation.

The aſcertaining the Force of Powder, and thence the Velocities of Bullets impelled by it's Exploſion, and the aſſigning a Method of truly determining their actual Velocities from Experiments, are Points from whence every neceſſary Principle in the Formation or Management of

Artillery may be easily deduced: Considering therefore the infinite Import of a well-ordered Artillery to every State, the Author flatters himself, that whatever Judgment may be formed of his Success in these Enquiries, he will not be denied the Merit of having employed his Thoughts and Industry on a Subject, which, though of a most scientific Nature, and of the greatest Consequence to the Publick, hath been hitherto almost totally neglected; or, at least, so superficially considered, as to be left in a much more imperfect State than many other philosophical Researches.

With regard to the second Chapter of this Treatise, relating to the Resistance of the Air, the Author has in his Preface mentioned his Intention of annexing to it a Series of Experiments, on the real Track of Bullets, as modulated by that Resistance: And therefore, as he proposes to complete those Experiments this Summer, unless unforeseen Accidents prevent him, he chooses to postpone any Account of the Subject of the second Chapter till that time, when he intends to lay the Result of those Experiments before this *Society*, in order that any Exceptions or Difficulties relating to them, may be examined and discussed before they are published to the World.

An Account of an Instrument or Machine for changing the Air of the Room of sick People in a little Time, by either drawing out the foul Air, or forcing in fresh Air; or doing both successively, without opening Doors or Windows. No. 437. p. 41. Apr. &c. 1735. Fig. 94.

VIII. *Fig.* 94* represents a Case D E C B, containing a Wheel of 7 Feet in Diameter, and 1 Foot thick; being a cylindrical Box, divided into 12 Cavities by Partitions directed from the Circumference towards the Centre, but wanting 9 Inches of reaching the Centre, being open towards the Centre, and also towards the Circumference, and only closed at the Circumference by the Case, in which the Wheel turns by means of a Handle fixed to it's Axis A, which Axis turns in two Iron Forks, or half concave Cylinders of Bell-Metal, such as A, fixed to the upright Timber or Standard A E.

From the Middle of the Case on the other Side behind A, there comes out a Trunk or square Pipe, which we call the Sucking-Pipe; which is continued quite to the upper Part of the sick Person's Room, whether it be near or far from the Place where the Machine stands, in an upper or lower Story, above or below the Machine. There is a circular Hole in one of the circular Planes of the Machine of 18 Inches Diameter round the Axis, just where the Pipe is inserted into the Case, whereby the Pipe communicates with all the Cavities; and as the Wheel is turned swiftly round, the Air which comes from the sick Room, is taken in at the Centre of the Wheel, and driven to the Circumference, so as to go out with great Swiftness at the Blowing-Pipe B, fixed to the said Circumference.

As the foul Air is drawn away from the sick Rooms, the Air in the neighbouring Apartments will gradually come into the Room through the smallest Passages: But there is a Contrivance to apply the Pipes

* *The Model of this Machine, made by a Scale of an Inch to a Foot, was shewn the* Royal Society June 13, 1734. *By Dr* J. T. Desaguliers, *F. R. S.*

2 which

which go to the ſick Room to the Blowing-Pipe B, while the Sucking-Pipe receives it's Air only from the Room where the Machine ſtands. By this means freſh Air may be driven into the ſick Room after the foul has been drawn out.

This Machine would be of great uſe in all Hoſpitals, and in Priſons: It would alſo ſerve very well to convey warm or cold Air into any diſtant Room; nay, to perfume it inſenſibly, upon occaſion.

Fig. 95. repreſents the Inſide of the Flat of the Wheel which is fartheſt from the Handle, and next to the Sucking-Pipe. Fig. 95.

1, 2, 3, 4. repreſents the Cavity or Hole which receives the Air round the Axis, having about it a circular Plate of Iron to hold all firm; which Plate is made faſt to the Wood and to the Iron Croſs that has the Axis in it.

g g g, denotes, by a pricked Circle, a narrow Ring of thick Blanketting, which (by preſſing againſt the outſide Caſe, whilſt it is fixed to the outſide of the Flat of the Wheel) makes the Paſſage into the Wheel tight.

H H H is another Circle of Blanketting, likewiſe fixed to the outſide of the Wheel, and rubbing againſt the Caſe, that the Air violently driven againſt the inner Circumference of the Caſe, may have no way out, but at the Blowing-Pipe at B.

There is on the outſide of the other Flat of the Wheel, where the Handle is fixed, a Ring of Blanketting, like H H H, oppoſite to it; but none oppoſite to *g g g*, becauſe the Wood there is not open, but comes home cloſe to the Axis.

Fig. 96. gives a vertical Section of the Wheel and Caſe a little forward of the Axis, drawn by a Scale twice as large as that of the other two Figures. Fig. 96.

A *a*, the Axis ſupported by the Irons A, *a*, cylindrically hollowed, except the upper Part, where a Pin keeps in the Axis. B D, the Caſe with the Sucking-Pipe S *a*. E A, the Prop for one End of the Axis. 1, 2, the Opening into the Wheel. *g g*, the Eminence of the Wood to which is fixed the ſmall Ring of Blanketting. The four black Marks, one of which is near H, repreſent the Sections of the two other Rings of Blanketting.

IX. 1. When the Wheel revolves upon it's Axis, which is performed in this Machine every Revolution in about half a Second, the Air may be conſidered as divided into as many concentrical Circumferences as there are Particles of Air contained between the leaſt and the greateſt Circle, conſequently the centrifugal Forces will be as the Radii; that is in an arithmetical Progreſſion.

A Calculation of the Velocity of the Air moved by the new invented Centrifugal Bellows of 7 Feet in Diameter, and 1 Foot thick within, which a Man can keep in Motion with very little Labour, at the Rate of two Revolutions in one Second. By J. T. Deſaguliers, *F. R. S. Ibid.* p. 44.

Let

		Feet
Let $R =$	Radius of the greatest Circle	3.5
$r =$	Radius of the least Circle	0.75
$m =$	Radius of the middle Circle	$2.125 = r + \frac{R - r}{2} = \frac{R + r}{2}$,
$v =$	Velocity or Space described in 1″ in the middle Circle, upon the Supposition that the Wheel revolves 2 Revolutions in 1″.	26.21
$S =$	Space described in 1″ by the Action of Gravity.	16.1

$s = \left\{ \begin{array}{l} \text{Space that a Particle of Air receding from the Centre would} \\ \text{describe in 1'' by the Action of the centrifugal Force at the} \\ \text{Circumference of the middle Circle.} \end{array} \right.$

$2\,m : v :: v : s$; therefore $\frac{v\,v}{2\,m} = s$, by *Huygens*'s Rule. Let G and c, express the Force of Gravity, and the centrifugal Force at the middle Circle. Since the Spaces described in the same Time by the Action of 2 Forces are as those Force $S : s :: G : c$, and $\frac{s\,G}{S} = c$, and substituting in this Expression $\frac{v\,v}{2\,m}$ instead of s, we have $\frac{v\,v\,G}{2\,m\,S} = c$; and putting $\frac{R + r}{2}$, instead of it's equal m, $\frac{v\,v\,G}{\overline{R + r} \times S} = c$. So that the Ratio of Gravity to the centrifugal Force, at the middle Circle, is that of G to $\frac{v\,v\,G}{\overline{R + r} \times S}$ or that of 1 to $\frac{v\,v}{\overline{R + r} \times S}$; which being multiplied by the Number of the revolving Circles $R - r$, gives for the Pressure of the Column of Air $R - r$ proceeding from Gravity $R - r$, and the Pressure proceeding from the centrifical Forces $\frac{\overline{R - r} \times v\,v}{\overline{R + r} \times S}$, wherein $R - r$ being a Factor common to both, may be thrown out of the Expression: And since the Velocities produced from different Pressures are as the square Roots of the Pressures, the Velocity Gravity would give from the natural Weight or Pressure of $R - r$ will be to the Velocity, the same Column would have from the Pressure occasioned by the centrifugal Force, as $\sqrt{1}$, or 1 to $\sqrt{\frac{v\,v}{\overline{R + r} \times S}}$.

Lastly,

Lastly, Since the Velocity proceeding from the Action of Gravity upon a Column $= R - r$, is always a known Quantity; it may be called $= a$ (equal in this Case to 15 38 Ft. *per* Second) and consequently the Velocity proceeding from the centrifugal Force will be $a \times \sqrt{\dfrac{v\,v}{R + r \times S}}$ or, $a\,v \times \sqrt{\dfrac{1}{R + r \times S}}$ or $\dfrac{a\,v}{\sqrt{R + r \times S}}$: That is, in this Machine $\dfrac{15.38 \times 26.71}{\sqrt{4.25 \times 16.1}} = 49.67$ Ft. *per* Second. And if we add to this the Velocity of the outer Circle in the Tangent of which the Air escapes, which (in the Supposition we made of 2 Revolutions in 1'') is 44 Feet *per* Second, we shall have = 93.67 Feet *per* Second.

N. B. This Calculation suppposes the Bore of the Sucking-Pipe sufficiently great to furnish as much Air as would escape, according to this Velocity; but in this Machine the Sucking Pipe being no greater than the Ajutage or Blowing-Pipe, the Velocity proceeding from the Pressure occasioned by the centrifugal Force, and from the Velocity in the Tangent (which may be represented by a Column of Air of sufficient Height to give the Velocity of 93.67 Ft. which is 145.882 Ft.) must be divided into 2 equal Parts, one half employed in sucking, and the other in blowing; therefore the Half of 145.882 Feet, which is 72.941 Feet, will represent the Height of a Column of Air, that would occasion the same Pressure with which the centrifugal Force and the circular Motion act in this Machine; and a Column of this Height producing a Velocity of 68.53 Feet *per* Second. This Number will express the Velocity with which the Air is sucked into the Wheel; and the same Number will also express the Velocity of the Air out of the Blower, proceeding from the centrifugal Force, and the circular Velocity of the outer Circle, which is the real Velocity of the Stream of Air out of the Blower of this Machine, *viz.* 68.53 Feet *per* Second, which is at the Rate of a Mile in about 77'', or about 7 Miles in 9'.

2. I send you a further Account of my centrifugal Wheel, which is now fixed in a Room above the House of Commons, to draw away the hot Steam arising from the Candles, and the Breath of the Company in the House, when it is very full, in warm Weather; as also afterwards to drive in a Stream of fresh Air, to spread uniformly all over the House, by coming in at the Middle of the Cieling. *—The Uses of the foregoing Machine, by the same,* Ibid: p. 47.

The Uses of this Machine for sick Rooms, for Prisons, for warming, cooling, or perfuming any Chambers at a distance, were spoken of in the Explanation of the Model I shewed the *Society*. The Machine may also serve in a Man of War, to take away the foul Air between Decks, occasioned by the Number of Men in the Ship, and to give them fresh Air in a few Minutes. In every Part of the Vessel every foul Hole may

be

be rendered wholesome, and even the Stench and foul Air from the Surface of the Bulge-Water may be carried off. In regard to Mines, the Machine must prove of excellent Use; for as the Damps (either fulminating, which, taking Fire, destroy the Men and ruin the Works, or arsenical, which kill by their poisonous Nature) are some specifically lighter, and some specifically heavier than common Air, this centrifugal Wheel can in a little Time drive down Air through wooden Trunks (or Launders) of 7 Inches bore, in such Quantities into the deepest Mines as to cause all the light Damp to come out at the Top of the Pit; or, by only altering two Sliders, suck away all the heavy poisonous Damp, whilst wholesome Air goes down from above Ground into the Pit, so as to fill all the subterraneous Caverns with fresh and wholesome Air.

Likewise a great many of the Difficulties which attend the carrying on subterraneous Passages for the Conveyance of Water from Mines (called Soughs, Adits, or Drifts) may be removed by the Help of this Wheel; for the fresh Air may be driven in a very little Time to the Place where the Men are at work, though at the Distance of 2, 3, or 4, Miles, and therefore also to any intermediate Space; whereas the Practice now is, either to make a double Drift with Communications between the two for the Circulation of the Air, or to sink perpendicular Shafts or Pits from the Top of the Hill over the Adit; both which Methods are very expensive, and (I dare say) will, upon Tryal, be out-done by the Application of my Machine.

A Description of a new Invention of Bellows, called Water-Bellows, by Martin Triewald, *F. R. S. Captain of Mechanics, and Military Architect to his Swedish Majesty.* No. 448. p. 231. June &c. 1738. *dated Stockholm* May 26. 1736. Fig 97.

X. These *Water-Bellows* A and A, are made of Wood, not unlike the Shape of Diving-Bells, in the Form of a *Conus Truncatus*, and consequently wider below than at top, where they are furnished with close Heads B and B, but at the lower Ends E and E, quite open. At the Heads B and B, are two Valves V and V, which open inwardly, and are made like the Claps of other *Bellows*, with their Hinges, and the Valves themselves covered with Hatters Felt, and are shut by an easy Steel Spring, till the Air from above opens the same, which happens only when these *Bellows* receive their Motion upwards; but are shut by means of the Pressure of the Air within, when they sink down into the Water. On the very same Heads are two pliable Leathern Tubes R and R, fixed one at the Top of each *Water-Bellows*, which Tubes are made and prepared in the same manner as those used in Water-Engines for extinguishing of Fire. These Leathern Tubes or Pipes reach from the *Bellows* to Wooden Tubes T, T, which carry the Wind into the Iron Furnace M, or any other Place, according to Pleasure.

These *Bellows* are likewise provided with Iron Chains *k*, K, which are fastened to two Sweeps S, S, by which means they hang perpendicular from the Beam of the Balance, and at the same Distance from the Centre of it's Motion C.

On the Balance are two sloping Gutters F, F, into which the Water alternately runs from the Gutter G, and so gives Motion to the whole Work; so that these last-mentioned Gutters F, F, do the same Service

as

as an Over-ſhot, or any other Water-Wheel, and coſt a great deal leſs, but give as even and regular a Motion, as any *Pendulum*, for meaſuring of Time; for as ſoon as ſo much Water runs into either of the aforementioned inclined Plains of the Gutters, ſo that the *Momentum* of the Water exceeds the Friction near the Centre of Motion C, the Gutter immediately moves down with a Velocity increaſing, till the Balance meets with the Reſiſtance of the Wooden Springs H and H, and at the ſame time raiſes the oppoſite *Water-Bellows*, or that *Bellows* which is fixed under the oppoſite Gutter. In the ſame Moment again as the ſaid Gutter begins it's Motion, being come down on the Spring, delivers all the Water it has received; at the very ſame time the Water begins to run into the oppoſite Gutter, which receives it's Load of Water almoſt as ſoon as the former is emptied; ſo that one of the Gutters does it's Effect, as ſoon as the other has done his, and this alternately one after another.

Theſe ſloping Gutters on the Balance do therefore all the Service and Effect which a Water-Wheel does in working the ordinary *Bellows*, and that by means of the Power which the Water applies to the Wheel of giving the ordinary *Bellows* their Motion, after the ſame manner does the Water here empower the ſloping Gutters to do the ſame Work.

But as for the manner and by what means theſe *Water-Bellows* are fit to blow the Fire, and to perform the ſame as Leathern or Wooden *Bellows*, there is no other Reaſon, but the very ſelf-ſame wherein the Effect of the ordinary *Bellows* conſiſts. For an ordinary pair of *Bellows* blow for no other Reaſon, but that the Air, which enters the *Bellows*, and which they contain when raiſed, is again compreſſed or forced into a narrower Space, when the *Bellows* cloſe: Now ſince the Air, like all other Fluids, moves to that Place where it meets with the leaſt Reſiſtance, the Air muſt conſequently go through the Opening which is left for the ſame, with a Velocity proportioned to the Force by which the Air is compreſſed, and muſt of neceſſity blow ſtronger or weaker, in regard to the Velocity by which the Top and Bottom of the *Bellows* meet; the Blaſt alſo will laſt in Proportion to the Quantity of Air that was drawn into the *Bellows* through the Valve or Wind-clap.

This does after the ſame manner happen in our *Water-Bellows*; for the Air, which they contain, cannot force itſelf down through the Water more than through a well-ſecured Deal-board with Pitch; when the *Bellows* are lowered down into the Water, the Air which they contain muſt neceſſarily be compreſſed by the Water, which riſes alternately into the *Bellows* A and A; ſo the Air muſt recede and go through the Leathern Tubes R, R, where the Air meets with the leaſt Reſiſtance. From all which it undoubtedly follows, that the larger, that is to ſay, the more Air theſe *Water-Bellows* are made to contain, and the greater the Velocity is by which they are made to deſcend into the Water, ſo much greater is their Effect; and that the Effect which they are able to perform, muſt be equal to that of Leathern or Wooden *Bellows* of the ſame Capacity, in containing an equal Quantity of Air.

As to the Advantages which this new Invention has in regard to thoſe uſed hitherto, it is a known Thing, that the Power which works your common *Bellows* uſed at Iron Furnaces, muſt be ſufficient not only to compreſs the *Bellows*, but at the ſame time to force down the Leaver with it's Weight or Counterpoiſe; which Leaver ſerves again to raiſe the *Bellows*, when the Cog or Button on the Axle-tree of the Water-Wheel ſlides off from the *Bellows-tree*, ſo that the Power muſt be ſufficient at once to produce two different Effects; whereas theſe new *Water-Bellows* require ſcarce any greater Power but what is neceſſary to overcome the Friction near the Centre of Motion, or the Axis C; for in this my Invention an Advantage is obtained, which very rarely happens in Mechanicks, viz. *That the Weight to be moved is, as here, on the Balance in Æquilibrio*; ſince the *Bellows* A and A cannot be otherwiſe conceived than as two equal, though heavy, Weights in a pair of Scales, which balance one another, although their Weight be ever ſo great; ſo that, if each of theſe *Bellows* ſhould weigh a Ton, they muſt ſtill equiponderate; which is ſo much eaſier attained to, ſince it requires very little Art to make them both of a Weight, and order them at equal Diſtances from the Centre of Motion. It is conſequently known how ſmall a Power is required to ſet the Scales of a Balance with equal Weights in Motion, notwithſtanding the Weight may be as great as poſſible; all which may with good Reaſon be applied to theſe *Water-Bellows*.

And though it cannot be denied, but that the *Bellows* which ſinks down into the Water-hole or Sump N, grows ſo much lighter, as it loſes of it's Weight in Water, by which means the *Water-Bellows* to be raiſed grows ſo much heavier, as the former loſes of it's Weight by being let down into the Water; yet this is compenſated, if we conſider, that the Water which falls down along the ſloping Gutter, acquires a Power of a falling Body; which Power increaſing in the ſame Proportion as the *Bellows* to be raiſed grows heavier, this Power ſuits admirably well the Weight to be raiſed; for the *Bellows* that ſinks down into the Sump N, does not at once loſe it's Weight in the Water, but gradually as it comes deeper into the ſame; and after the ſame manner the aſcending *Bellows* does not grow at once heavier than the other, but gradually, growing heavieſt juſt when the lowermoſt Edge gets even with the Surface of the Water; and that happens at the ſame Inſtant of Time when the Power of the Water in the ſloping Gutter is at the higheſt pitch, or has received it's greateſt *Momentum*.

This ſhews, I hope, very plain, that the Power required to work theſe *Water-Bellows*, is far leſs, and conſequently leſs Water will be conſumed in working theſe *Bellows* than thoſe commonly uſed; and again, that an Iron Furnace, which for want of Water to work the common *Bellows*, cannot be kept at work longer than 6 Weeks, though

it

it be provided with all other Neceſſaries, may, by means of ſuch *Water-Bellows* as here deſcribed, be kept at work at leaſt as long again.

It is furthermore a known thing to Miners, of what prodigious Loſs and Inconvenience it is, when the Hearth or Mouth of an Iron Furnace is placed low, in a wet and damp Place, which they oftentimes are forced to do, in regard to the Axle-tree of the Water-Wheel which works the *Bellows*; for which Reaſon ſuch Furnaces as ſtand in the like moiſt Places, give daily conſiderably leſs Iron, than others which are better ſituated. There is likewiſe not a ſmall Difficulty to find a fit Stuation for ſuch Iron Furnaces where Iron Guns are caſt, and require deep Pits under the Mouth of the Furnace: But by means of this new Invention of *Bellows*, one may be at Liberty to place the Mouth of the Furnace as high as one pleaſes, ſeeing it is very eaſy to guide the Blaſt by means of Wooden or Leaden Tubes, as far as neceſſary, and in a proper Direction into the Furnace; which Advantage cannot ſo eaſily be obtained by thoſe *Bellows* in common uſe.

Further, this may be accounted as no ſmall Advantage which theſe *Bellows* afford, in being of ſo very eaſy a Structure, that any Carpenter at firſt Sight is able not only to conſtruct the whole Engine, but eaſily repair every Part of the ſame, requiring at the ſame time the leaſt Repairs of any that can be uſed; and if the *Bellows* ſhould be caſt Iron, they would laſt for ſeveral Ages; and when caſt ſtrong, they would not require any Weight to ſink readily in the Water. One might cauſe them to be covered with Lead, or make them of thin Copper with a thick Leaden Hoop at top, to make them ſink. As for their Shape, it is not abſolutely neceſſary they ſhould be of the ſame as the Figure denotes; for in caſe one would not beſtow Iron Hoops on the *Bellows*, they might be made ſquare, in a Triangle, or any other Shape, provided they be as wide again at bottom as at top; and if they be made of Wood, it will be neceſſary to provide an Edge round the Tops, for containing Stones or Leaden Weights, as much as will be found neceſſary to make them ſink readily, when they are lowered down into the Water.

Laſtly, If we will conſider the Charge of thoſe *Bellows* made uſe of at Iron Furnaces, as to the *Bellows* themſelves, the Water-Wheel and it's Axle-tree, *&c.* and compare the ſame with the Coſt of theſe, we ſhall eaſily find a vaſt Difference, not to mention the vaſt Charges of keeping the common *Bellows* in Repair. But before I conclude, I think myſelf obliged to mention, that the Blaſt of theſe *Bellows* is governed and moderated in the ſame manner as the common ones, *viz.* by ſetting more or leſs Water into the ſloping Gutters, and by taking out and letting in Plugs for that purpoſe placed in Holes near the Top of the *Water-Bellows.*

An Account of ſome new Statical Experiments, by J.T.

XI. When a long and heavy Body lying on the Ground is to be raiſed up at one End, (like a Leaver of the ſecond Kind) while the other End keeps it's Place and becomes the Centre of it's Motion; the

Desaguliers, *LL. D. F.R.S.* No. 445. p. 62. Jan. &c. 1737.

the Prop, that is made use of to support it at any Point in it's whole Length, sustains a certain Pressure from the Beam. Now the Experiments which I shall make are to shew, by a Force drawing always in the Direction of the Prop, what is the Quantity of the Pressure on the Prop, according to the Length of the Prop, the Angle which it makes with the Beam, or with the Horizon, and the Distance from the Centre of Motion of the Beam at which the Prop is applied. For when the Prop is taken away, the Force drawing in the Direction of the Prop will keep the Beam in *Æquilibrio*; and a Force ever so little superior to the Friction added to the Power, will make it overpoise the Beam and raise it higher; but overcome the Power and bring down the Beam, if it be added or applied to the Beam.

Though in every Case and Experiment we have this Analogy taken from mechanical Principles, *viz.* that

The Intensity of the Power:
Is to that of the Weight;
As the Distance of the Line of Direction of the Weight:
Is to the Distance of the Line of Direction of the Power.

Yet to find those Distances nicely in the several Applications of the Prop, we must have Recourse to geometrical Constructions and Reasonings. With these and the algebraical Expressions of the same, the Experiments exactly agree.

I design to give to the Society a Paper upon this Subject, wherein will be explained not only the Investigation of the Proportion between the Power or Pressure sustained by the Prop and the Weight of the Body supported, but also the Determination, of the *Maximums* of Pressure, where there are any, and the Nature and organical Descriptions of some particular Kinds of Curves of the third Order, described by one End of the Prop in it's successive different Situations.

The Numbers made use of in these Experiments are the result of the Calculations; and all I propose now is to shew the Experiments by Means of a Machine which I contrived for the Purpose, and got executed with great Nicety, not in Ornaments, but only where Nicety in a mechanical Instrument ought to be observed; a Caution useful in many other Machines.

In this Machine, the Iron Bar, or Parallellipiped representing the heavy Body, weighs 12 Drams, 12 *dwt*, 12 Grains, or 6060 Grains, and it's Centre of Gravity is at the Distance of 20 Inches and a half from it's Centre of Motion.

The Props I make use of are, the one of five, and the other of ten Inches. To overcome the Friction, allowed for by certain Rules in all Cases, I use a nice Brass Pulley of three Inches Diameter, whose Pivots are but $\frac{25}{1000}$ of an Inch in Diameter; so that the 60th part of the Power added to it, will, in all Cases, overcome the Friction.

FIRST

FIRST CASE.

In which the Prop is perpendicular to the Horizon exemplified by two Experiments.

The Prop is equal to 5 Inches, and placed under a Point in the Bar 10 Inches diſtant from the Centre of Motion. Here the Power acting in the Direction of the Prop, able to keep the Bar in that Situation, or the Preſſure ſuſtained by the Prop, will be found 250 Ounces, 17 *dwt.* 15 Grains; and the Friction 8 *dwt.* 15 Grains. The Foot of the Prop is to be at 8 Inches and $\frac{66}{100}$ from the Centre of Motion. Exp. I.

If the ſame Prop of 5 Inches is placed under a Point in the Bar at 30 Inches from the Centre of Motion, the Power or Preſſure will be 8 Ounces, 12 *dwt.* 13 Grains; and the Friction equal to 2 *dwt.* 21 Grains. The Foot of the Prop is to be diſtant from the Centre of Motion 29 Inches $\frac{58}{100}$. Exp. II.

SECOND CASE.

In which the Prop is prependicular to the Bar, exemplified by three Experiments.

Now let the Prop (ſtill five Inches long) be placed ſo as to be perpendicular to the Bar in a Point 12 Inches diſtant from the Centre of Motion. Here the Power expreſſive of the Preſſure ſhould be 19 Ounces, 18 *dwt.* 4 Grains, and the Friction 6 *dwt.* 15 Grains; but on account of a Correction neceſſary to be made to this, (becauſe the Bar is thick as well as heavy, and the Centre of Gravity above the Surface to which the Prop is applied) the Power or Preſſure ſuſtained will be only 19 Ounces, 15 *dwt.* 5 Grains, and the Friction 6 *dwt.* 14 Grains. Exp. I.

N. B. The Diſtance of the Foot of the Prop in this Caſe is 13 Inches from the Centre.

The Prop here is 10 Inches long, (ſtill perpendicular to the Bar) under a Point in the Bar, 24 Inches diſtant from the Centre. The Power equal to the Preſſure ſuſtained ſhould be (if the Bar was only heavy, and not thick) 9 Ounces, 19 *dwt.* 4 Grains; the Friction 3 *dwt.* 11 Grains and an half; but with the proper Correction, which I ſhall explain hereafter, it muſt be only 9 Ounces, 17 *dwt.* 15 Grains; the Friction 7 *dwt.* 7 Grains. Here the Foot of the Prop is to be 26 Inches from the Centre. Exp. II.

If the End of the Prop is placed under a Point in the Bar, ſo that the Horizontal Diſtance of the Foot of the Prop be exactly equal to the Diſtance of the Centre of Gravity from the ſaid Centre of Motion, *viz.* 20,5 Inches; the Power or Preſſure ſuſtained by the Prop will be preciſely equal to the Weight of the Bar, *viz.* 12 Ounces, 12 *dwt.* 12 Grains. Exp. III.

Grains. In this Cafe, the Prop is diftant from the Centre of Motion on the Bar 17,9 Inches, and the Friction 4 *dwt.* 5 Grains.

The Third Case.

In which the Angle made by the Prop with the horizontal Line is given, either acute or obtufe.

As this Cafe is very intricate, (on Account of the feveral Powers of the Sine and Cofine of the given Angle, which are multiplied into the Prop and into the Weight of the Beam) we will exemplify it only in one Experiment; which is, when the Angle made by the Prop, with the horizontal Line contained between the Foot of the Prop and the Centre, is acute: Then there is a *Maximum* of Preffure, which I will fhew by Experiment to be the very fame as the Calculation gives. I fuppofe the Angle made by the Prop and the horizontal Line to be 60 Degrees: The Calculation of this *Maximum* fhews, that if the Prop is 10 Inches long, the Diftance meafured upon the Bar, to which the upper End of the Prop muft be applied, will be 10 Inches $\frac{96}{100}$, the Bar itfelf making then an Angle of about 52 Degrees 12 Minutes; and the horizontal Diftance between the Centre of Motion and the Foot of the Prop is then 11 Inches $\frac{72}{100}$.

N. B. Three Things are to be remarked in this Cafe:

1. That when the Angle made by the Prop and the horizontal Line, contained between the Centre of Motion and the Foot of the Prop, is acute, as in the laft Experiment, there is always a *Maximum:* Whereas if the fame Angle was obtufe, there would be no pofitive *Maximum*; for then the Preffure would continually increafe, the nearer the Prop is to the Centre of Motion.

2. That when the Angle of the Prop with the Horizon is acute, as in the laft Experiment, the Bar, or long and heavy Body, can be raifed by applying the Power or Prop always with the fame Angle to the Horizon, quite up to a vertical Situation.

3. That the firft Cafe, which is when the Prop is perpendicular to the Horizon, is only a particular Cafe of this more general one.

The Fourth Case

Is, when the Angle made by the Prop with that part of the Beam contained between the Point to which it is applied, and the Centre of Motion, is given either acute or obtufe.

As the Expreffion of the Power in this Cafe is fully as intricate as in the laft, I will only give one Example or Experiment; and, for the greater Satisfaction of thofe that fee it, I chofe that, wherein the Preffure is in it's *Maximum.* I fuppofe, as before, the Angle made by the Prop, (ftill 10 Inches long) with that Part of the Beam contained

2 between

between the Point to which it is applyed, and the Centre of Motion, to be acute and of 60 Degrees; then the *Maximum* of Preſſure will be, when the part of the Beam intercepted between the Centre of Motion and the upper End of the Prop is 12 Inches $\frac{21}{100}$; the Bar is then elevated about 50 Degrees 13 Minutes, and the horizontal Diſtance between the Centre of Motion and the Foot of the Prop is then 11 Inches $\frac{27}{100}$.

N. B. Obſerve alſo in this Caſe as in the laſt.

1. If the Angle made by the Prop, and the part of the Beam intercepted between the Point of Application and the Centre of Motion, is acute, there will always be a *Maximum*. The contrary will happen, if that Angle is obtuſe.

2. If the Angle is acute, the Bar cannot be raiſed up to a vertical Situation by applying the Power or Prop conſtantly with the ſame acute Angle; but it may be raiſed quite up, if the Angle of the Prop with the Beam is obtuſe.

3. The ſecond Caſe is but a particular Caſe of this general one. For the Reaſons of all thoſe Things, the Corrections neceſſary to be made on account of the Thickneſs of the Bar, the Nature and organical Deſcription of ſome Curves, and ſeveral other remarkable Conſiderations on this Subject, I muſt refer to the Paper I ſhall give in to the Society.

The Figure of a Machine *for grinding* Lenſes *ſpherically, invented by Mr* Samuel Jenkins, No. 459. p. 555. Jan. &c. 1741. *dated* Nov. 29. 1737.

XII. The Advantage it would be to have *Lenſes* of the ſpherical Kind, Segments of a true Sphere, hath occaſioned the Invention of many Machines and Methods of Grinding, in order to produce ſuch Segments: But nothing hitherto made publick hath anſwered the End propoſed.

The beſt Methods now in Uſe will only produce an Approximation to a truly ſpherical Figure, but demonſtrably not one, though the Artificer ſhould employ the utmoſt Skill and Care in the Uſe of the beſt Machines hitherto invented: And indeed, at preſent, Gentlemen have nothing to depend on, that their *Lenſes* are nearly ſpherical, but the Care and Integrity of the Workmen; in which how often they are deceived, is too obvious to every one who hath Occaſion to uſe ſuch *Lenſes*.

I therefore beg Leave to ſubmit to your Conſideration the Effects of a Machine, which, as it is contrived to turn a Sphere at one and the ſame time on two *Axes* which cut each other at Right Angles, with equal Velocity and Preſſure on each of them, I conceive it is demonſtrable, that (without any Skill or Care in the Workman) it will produce a Segment of a true Sphere, barely by turning round the Wheels; which if ſo, the Conſequences will be,

1ſt, That all Grinders of ſuch Glaſſes, *&c.* will gladly uſe them; a labouring Man, whom they hire for leſs Wages, being, by the Help of this Machine, able to do more Work in a Day, that a ſkilful Artificer, without it, in two Days. And, 2*dly*, All Gentlemen will have the Pleasure to know the *Lenses* they make use of are truly spherical, it being impossible this Machine should produce any other Figure.

Explanation of Fig. 98.

A. *A Globe covered with Cement, in which are fixed the Pieces of Glass to be ground. This Globe is fastened to the Axis, and turns with the Wheel* B. C. *Is the brass Cup, which polishes the Glass: This is fastened to the Axis, and turns with the Wheel* D. *So that the Motion of this Cup* C *is at Right Angles with the Motion of the Globe* A.

CHAP. VI.

HYDRAULICKS.

Of the Measure and Motion of running Waters.

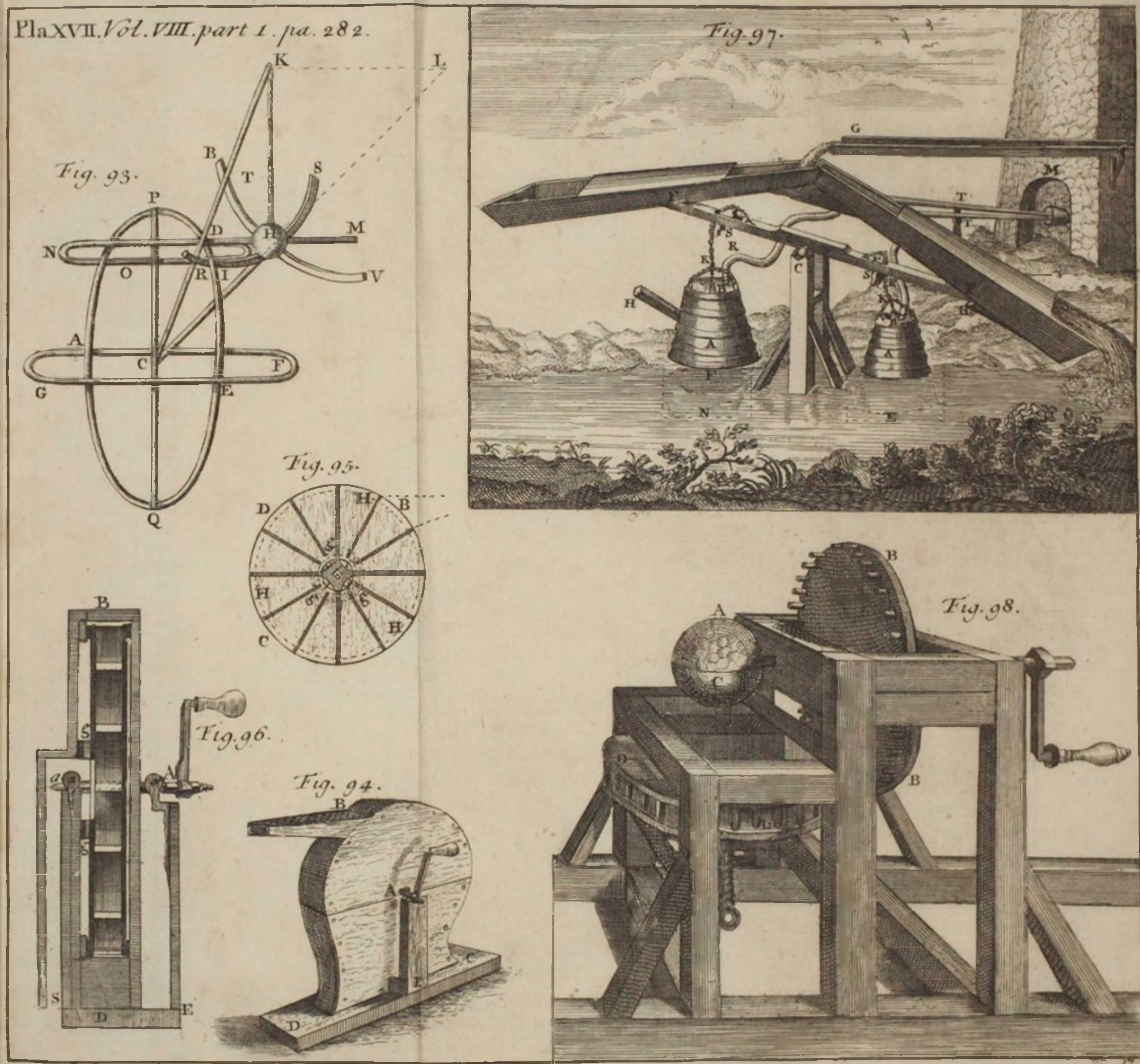
Essay I. *of Water running out of a Vessel always full thro' a round Hole, and of it's Resistance arising from a Defect of Lubricity; by* James Jurin, *M. D. F. R. S.* No. 452. p. 5. Jan. &c. 1739.

I. THE Ancients had no other *Measure* of running Waters, than that uncertain and fallacious one, which having no Regard to the Velocity, depended wholly on the perpendicular Section of a Stream. The first, who opened a way to the Truth, was *Benedict Castelli*, an Italian and Friend of *Galileo*. He having discovered that the quantity of Water flowing through a given Section of a Stream is not given, as the Ancients thought, but that it is proportional to the Celerity with which the Water is carried thro' the given Section, by this noble Discovery laid the Foundation of a new and most useful hydraulick Science. This Discovery therefore engaged the Philosophers to study this Doctrine with so much Diligence, that after *Castelli*'s Time there was hardly any eminent Mathematician, who did not endeavour to add something thereto, either by Experiments, or by Reasonings and Arguments *à priori*.

But most of them, notwithstanding their great Abilities, had no Success therein, because of the exceeding Difficulty of the Work. For those, who studied the Theory, laid down such Theorems as were found to be false, when brought to the Test by Experiments; and those, who laboured in making Experiments, omitting to observe some minute Circumstances, the Importance of which they had not yet perceived, differed greatly from one another, and almost all of them erred from the real *Measure*.

Of this there cannot be given a better Example than that simple and easy one, which has generally been a Foundation to all the rest, and is what we have now undertaken to handle diligently, when Water runs out thro' a circular Hole made in the Bottom of a Vessel constantly full, with a constant Velocity. *Poleni* alone has given the true Measure of the Water flowing out, or at least very near the true one; and Sir *I. Newton* alone has laid the Foundation of discovering that Measure; tho'

Pla XVII. Vol. VIII. part 1. pa. 282.



tho' moſt have rejected it, and ſome, concealing the Author's Name, have pretended it to be their own.

We ſhall therefore make our Attempt under the Conduct of theſe two Leaders; and in the firſt place propoſe under the name of *Phænomena* ſuch things, as either appear from Experiments, or are confirmed by certain Reaſonings drawn from them; and in the laſt place, we ſhall attempt the Solution of thoſe *Phænomena*.

Phænomena of the flowing of Water out of a Hole in the Bottom of a Veſſel conſtantly full.

1. The Depth of the Water, and the Time of flowing out being given, the *Meaſure* of the effluent Water is nearly in Proportion to the Hole.

2. The Depth of the Water, and alſo the Hole being given, the *Meaſure* of the effluent Water is in Proportion to the Time.

3. The Time of flowing out, and the Hole being given, the *Meaſure* of the effluent Water is nearly in a ſubduplicate Proportion to the Height of the Water.

4. The *Meaſure* of the effluent Water is nearly in a *Ratio* compounded of the Proportion of the Hole, the Proportion of the Time, and a ſubduplicate Proportion of the Depth of the Water.

5. The *Meaſure* of Water flowing out in a given Time is much leſs than that, which is commonly aſſigned by Mathematical Theorems. For the Velocity of effluent Water is commonly ſuppoſed to be that, which a heavy Body would acquire *in Vacuo* in falling from the whole Height of the Water above the Hole; and this being ſuppoſed, if we call the *Area* of the Hole F, the Height of the Water above the Hole A, the Velocity which a heavy Body acquires in falling *in Vacuo* from that Height V, and the Time of falling T, and if the Water flows out with this conſtant Velocity V, in the Time T; then the Length of the Column of Water, which flows out in that Time will be 2 A; and the *Meaſure* of it will be 2 A F. But if we calculate from the moſt accurate Experiments of *Poleni* *, we ſhall find the quantity of Water, which flows out in that Time, to be no more than about $\frac{571}{1000}$ of this *Meaſure* 2 A F.

This Illuſtrious Perſon's Experiments, are in my Opinion preferable to all others, not only becauſe of his extraordinary Diligence and Accuracy, but on other Accounts alſo. He found, that the Quantity of Water flowing out of a Veſſel thro' a cylindrical Tube far exceeded that, which flowed through a circular Hole made in a thin *Lamina*, the Tube and Hole being of equal Diameter, and the Height of the Water over both being alſo equal. And he found it to be ſo, when the Tube was inſerted, not into the Bottom, which others had obſerved before, but into the Side of the Veſſel.

Now a Hole made in the thinneſt *Lamina* muſt be conſidered as a ſhort cylindrical Tube. Whence it appears that a greater quantity of Water runs thro' a Hole made in a thin *Lamina*, than would have run

* *Polenus de Caſtellis*, Art. 35, 38, 39, 42, 43.

out, if the Thickneſs of the *Lamina* had been infinitely ſmall, as they expreſs themſelves. But as ſuch a *Lamina* can neither exiſt, nor even be conceived by the Imagination, it remains that we increaſe the Diameter of the Hole, that the Thickneſs of the *Lamina* may bear the leaſt Proportion poſſible to the Diameter of the Hole.

This *Poleni* performed with great Judgment, when he made uſe of a Diameter of 26 Lines, and a *Lamina* not quite a Line thick; whereas before him hardly any one made uſe of a Diameter of above 6 or 7 Lines, or ever attended to the Thickneſs of the *Lamina* or Bottom of the Veſſel, except Sir *I. Newton*, who mentions his making uſe of a very thin *Lamina*.

But *Poleni* exceeded all others, in conſidering not only the Size of the Hole but of the Veſſel alſo, that the Water might deſcend toward the Hole with the greateſt Freedom and leaſt Impediment, ſo that there can be no doubt but that the *Meaſures* taken by him come much nearer the Truth than any other.

6. Since, as we have juſt now ſeen the *Meaſure* of the Water running out in the above-mentioned Time T, is $2\,A\,F \times \frac{571}{1000}$, the Length of the Column of Water, which runs out in that Time, is $2\,A \times \frac{571}{1000}$. Therefore if each of the Particles of Water, which are in the Hole in the ſame Space of Time, paſſes with equal Velocity, it is plain that the common Velocity of them all is that with which the Space $2\,A \times \frac{571}{1000}$ would be gone over in the Time T, or the Velocity $V \times \frac{571}{1000}$. But this is the Velocity with which Water could ſpring *in Vacuo* to near $\frac{1}{3}$ of the Height of the Water above the Hole.

7. But when the Motion of Water is turned upwards, as in Fountains, the Fountains are ſeen to riſe almoſt to the entire Height of the Water in the Ciſtern. Therefore the Water, or at leaſt ſome Portion of the Water, ſpouts from the Hole with almoſt the whole Velocity V, and certainly with a much greater Velocity than $V \times \frac{571}{1000}$.

8. Hence it is evident, that the Particles of Water, which are in the Hole at the ſame Point of Time, do not all burſt out with the ſame Velocity, or have no common Velocity. The Mathematicians have hitherto taken the contrary to be certain.

9. At a ſmall Diſtance from the Hole, the Diameter of the Vein of Water is much leſs than that of the Hole. For Inſtance, if the Diameter of the Hole is 1, the Diameter of the Vein of Water will be $\frac{21}{25}$ or 0,84 according to Sir *I. Newton*'s Meaſure, who firſt obſerved this wonderſul

wonderful *Phænomenon*; according to *Poleni*'s Meaſure $\frac{20}{26}$ or $\frac{20,5}{26}$; that is, if you take the mean Diameter, 0,78 nearly.

We ſhould now proceed to the Solution of theſe *Phænomena*; but before we do this, it will be convenient to acquaint the Reader with the following Particulars.

1. We conſider Water no otherwiſe than as a fluid, continuous, Body, the Parts of which yield to the leaſt Force, and are thereby moved amongſt themſelves.

2. By effluent Water we underſtand that Quantity of Water, which actually paſſes out of the Hole: and tho' it may ſeem unneceſſary, yet I have thought it proper to mention, that in my *Diſſertation on the Motion of running Waters*, inſerted about 24 Years ago in the *Philoſophical Tranſactions*, by defluent Water I underſtood that whole Quantity of Water, which is put in Motion within the Veſſel, and deſcends towards the Hole.

3. We conſider the Amplitude of the Veſſel as infinite, or at leaſt ſo great, that the Decreaſe of the Depth of Water therein in the whole Space of Time, in which the Water flows out of the Hole, is imperceptable.

4. We conſider Water as running out with a conſtant Velocity. At the beginning indeed of the Motion it runs out for a very ſmall Space of Time with a leſs Velocity than afterwards. But we paſs over the very beginning of the Motion, and inveſtigate the *Meaſure* and *Motion* of Water, when it has acquired it's utmoſt Velocity. Now this muſt neceſſarily be conſtant, as long as the Height of the ſuperincumbent Water remains the ſame.

5. We conceive the Bottom of the Veſſel no otherwiſe than as a Mathematical Plane, or at leaſt as ſo thin a *Lamina*, that it's Thickneſs is hardly any with regard to the Diameter of the Hole.

6. By the *Meaſure of effluent Water* in the following Pages we always underſtand that Quantity of Water, which flows out of the Hole in the ſame Space of Time that a heavy Body falling *in Vacuo* would take in paſſing through the Height of the Water above the Hole.

7. By the *Motion of effluent Water* we underſtand the Sum of the Motions of all the Particles of Water, which run out of the Hole in the above-mentioned Space of Time. But the Motion of every Particle is as the *Factum* of the Particle itſelf, and of the Velocity with which it burſts out of the Hole.

8. That what we ſhall ſay hereafter may be the more eaſily conceived, we ſhall firſt propoſe the more ſimple Caſes, and then proceed to thoſe which are more compound, but nearer to the true ſtate of things.

Thus in the firſt Problem, that the Solution may be the more ſimple, we ſuppoſe the Water to run out of the Hole into a *Vacuum*, and the Particles

Particles of Water, whilſt they deſcend towards the Hole, to be without any Reſiſtance ariſing from a Defect of Lubricity.

In the ſecond and third Problem the Efflux of the Water is ſtill ſuppoſed to be *in Vacuo*, but we conceive the Particles of Water, whilſt they deſcend towards the Hole, to meet with ſome Reſiſtance for want of Lubricity, but ſo ſmall, that the Decreaſe of the *Motion* of the Water running out of the Hole occaſioned thereby, is to be accounted as nothing.

In the fourth and fifth we ſtill retain the the Suppoſition of the *Vacuum*; but the decreaſe of the *Motion* of the effluent Water for want of Lubricity is ſuppoſed to be ſenſible.

Laſtly, in the ſixth and following Problems, we conſider the thing as it really is, when it is tranſacted in the Air, ſo that the Particles of Water ſuffer a ſenſible Reſiſtance, not only from each other for want of Lubricity, within the Veſſel, but alſo after their going out of the Veſſel, from the Attrition of the ambient Air.

Prob. I. *To determine the* Motion, Meaſure, *and Velocity of Water running into a* Vacuum *thro' a Hole in the Bottom of a Veſſel, where the Particles of Water meet with no Reſiſtance for want of Lubricity.*

So long as the Hole is ſtopped, the Stopper ſuſtains the Weight of a Column of Water lying perpendicularly over it. On removing the Stopper, the Column of Water, which lies perpendicularly over it, being no longer ſuſtained, by it's Preſſure cauſes the Water to run out thro' the Hole, and after having brought it to it's due Velocity, keeps the Velocity of the effluent Water conſtant by it's conſtant Preſſure.

It muſt be conceived indeed, that the *Motion* of the Water running out of the Hole is derived not only from the Weight of the perpendicular Column, but partly from the Preſſure of this Column, and partly from the Preſſure of the ſurrounding Water. But this makes the *Motion* of the effluent Water neither greater nor leſs, than if it aroſe from the Preſſure only of the perpendicular Column: not leſs, becauſe the Preſſure of the perpendicular Column, if it is not obſtructed, will generate a *Motion* proportionable to itſelf, and it cannot be hindered but ſo far as the ſurrounding Water urges the effluent Water: not greater, becauſe the Preſſure of the ſurrounding Water can add nothing to the *Motion* of the effluent Water, unleſs it takes away as much from the Preſſure of the perpendicular Column.

Therefore the adequate Motion of the Water flowing out of the Hole is the Preſſure, or Weight, of the Column of Water over the Hole. But a given Force, howſoever applied, generates a given Quantity of Motion in a given Time, towards thoſe Parts whither the Force tends. Therefore the Weight of the incumbent Column generates a like Quantity of Motion in a given Time in the effluent Water, as it could generate in the ſame Time in the Column itſelf falling freely thro' a *Vacuum*.

Now becauſe, by the *Hypotheſis*, the Particles of Water find no Reſiſtance for want of Lubricity, and all thoſe Particles, which are juſt going out in the very Hole, are urged by an equal Preſſure of the ſuperincumbent Water, it is plain that the Velocity of all theſe is equal.

Let v be that common Velocity; a the Height, in falling from which *in Vacuo* that Velocity would be acquired; A the Height of the Water above the Hole; V the Velocity acquired by falling *in Vacuo* from the Height A; T the Time of falling from the ſame Height; F the *Area* of the Hole; and let the Water flow out of the Hole in the Time T.

Now becauſe in the Time T, with the Velocity V, the Space 2 A will be run over, the Space $\frac{2Av}{V}$ will be run over in the ſame Time, with the Velocity v. Therefore this will be the Length of the Column of Water, which flows out of the Hole in the Time T; and the Magnitude of this Column, or the *Meaſure* of the Water flowing out in the Time T, will be $\frac{2AvF}{V}$ and the *Motion* of the ſame will be $\frac{2AFv^2}{V}$.

But the *Motion*, which can be generated in the Column of Water over the Hole, in the ſame Time T, if carried by it's own Weight thro' a *Vacuum* is thus.

It's Velocity will be V, and as it's Magnitude is A F, it's *Motion* will be A F V.

But that *Motion*, from what has been ſaid above, is equal to the *Motion* of the Column of Water flowing out in the Time T, or $AFV = \frac{2AFv^2}{V}$.

Hence $V = \frac{2v^2}{V}$, or $v^2 = \frac{V^2}{2}$, and $v = \frac{V}{\sqrt{2}}$

Moreover the Meaſure aſſigned above of the Water running out in the Time T, or $\frac{2AFv}{V} = \frac{2AF}{V} \times \frac{V}{\sqrt{2}} = \frac{2AF}{\sqrt{2}} = AF \times \sqrt{2}$. Q. E. I.

Since $a : A :: v^2 : V^2$; therefore $a = \frac{Av^2}{V^2}$, that is, $a = \frac{A}{V^2} \times \frac{V^2}{2}$, or $a = \frac{A}{2}$. Therefore the Height a, which the effluent Water can reach by turning the Motion upwards, is half the Height of the Water in the Veſſel above the Hole; which is the very Height determined by Sir *I. Newton*, *Princip. Ed.* 3. *Lib.* 2. *Prop.* 36. Coroll. 1.

Coroll. 2. If we ascribe to the effluent Water that Velocity, which is acquired by falling from the whole Height of the Water above the Hole, that is, if we suppose the Velocity $v = V$, then the above determined Motion of the Water $\frac{2\ A\ F\ v^2}{V} = 2\ A\ F\ V$, or double that Motion, which can be generated by the Column over the Hole, and therefore not to be generated but by double this Column; as we are taught by Sir *I. Newton*, *Princip. Ed.* 2 and 3. *Lib.* 2. *Prop.* 36.

Scholium. This *Measure* here determined $\frac{2\ A\ F}{\sqrt{2}}$, or $2\ A\ F \times 0{,}707$, as it falls far short of that which is generally determined by Mathematicians, namely 2 A F, so it far exceeds that *Measure* which is shewn by *Poleni*'s Experiments, or $2\ A\ F \times 0{,}571$, and no wonder, for what is supposed in this Problem, that the Particles of Water find no Resistance in running down, the *Hypothesis* is far from the true state of things.

Prob. II. *To determine the* Motion, Measure, *and Velocity of Water running out into a* Vacuum *thro' a circular Hole in the middle Part of the Bottom of a cylindrical Vessel, where the Particles of Water find some Resistance for want of Lubricity, but so small, that the Decrease of the* Motion *of the effluent Water occasioned thereby cannot be accounted any thing.*

Fig. 99. Let A B C D be an immense cylindrical Vessel, E F a circular Hole made in the middle Part of the Bottom, and the Water being perfectly at Rest and unmoved in the Vessel, let the Stopper be removed from the Hole, that a Passage may be opened for the Water thro' the Hole.

Then because the Water has been hitherto unmoved, and now begins to run out thro' the Hole, and the Water placed above follows that which runs out, and the natural Motion of the Water is not disturbed by pouring any over it, and the Hole is in the very middle of the Bottom, that Portion of Water, which is in Motion, and descends towards the Hole, will necessarily assume some regular Figure A H E F K B, of which the lower Base is the Hole itself, and the upper Base, the upper Surface of the Water A B, and all the horizontal Sections are circular. We call this a *Cataract*, but we do not yet examine what is the Figure of the *Cataract*: it is sufficient for our present Design, to observe that it is regular, and that the same Quantity of Water passes in a given Time thro' each of it's horizontal Sections.

Now because all that Water which tends downwards, is contained in the *Cataract*, it follows that the rest of the Water A H E C, B K F D, which is without the *Cataract*, has no Motion at all, and is perfectly at Rest. Therefore in any horizontal Section of the *Cataract* H *c* K, whose Centre is *c* the Points H, K shall represent the Bounds between the Water descending towards the Hole, and the surrounding quiescent Water.

Moreover

Moreover, as the Point K is the Bound of Motion and Reſt, and the Particles of Water, whilſt they are in Motion, find a Reſiſtance for want of Lubricity, the Particle of Water α within the *Cataract*, and next to the Point K, muſt be carried downwards only with the leaſt Velocity. Otherwiſe it would neceſſarily carry with it the next Particle a, placed without the *Cataract*, contrary to the *Hypotheſis*. But the Particle β, which is contiguous within to the Particle α, will not deſcend but with the leaſt relative Velocity with regard to the Particle α; becauſe otherwiſe it would carry the Particle α away with it by accelerating it, and this Particle α, being now in a quicker Motion, would carry away with it the Particle a. In like manner the Particle γ being placed more within, and contiguous to the Particle β, will deſcend with the leaſt relative Velocity with regard to the Particle β, and the other Particles δ, ε, &c. being placed one more within than another, will deſcend with the leaſt relative Velocity with regard to each of the Particles lying next to each of them without. And by this means the abſolute Velocity of the Particles muſt neceſſarily increaſe gradually from the bound toward the Centre c, that the Velocity of the Water may be greateſt in the very Centre, and leaſt at each Bound K and H. Fig. 100.

But it is neceſſary that the Reſiſtance, which each quicker Particle finds from the Friction of the adjacent ſlower Particle placed without, ſhould be perpetually equal thro' the whole Section of the *Cataract*. Otherwiſe that Particle, which finds the greater Reſiſtance, will accelerate the adjacent ſlower Particle, till the Reſiſtance is by this means diminiſhed, and becomes equal to that Reſiſtance, which is found by the other Particles. But if the Reſiſtance is equal every where thro' the whole Section of the *Cataract*, the relative Velocity of the Particles will be alſo equal every where, when one of them neceſſarily follows another.

Therefore the abſolute Velocity of every Particle, which is the Sum of all the relative Velocities, from the Circumference of the Section to that very Particle, taken all together, is in the *Ratio* of the Diſtance of the ſame Particle from the Circumference of the *Cataract*.

Now let r be the *Radius* of the Hole, m to 1 in the Proportion of the Circumference to the Diameter, mr^2 the Area of the Hole, v the Velocity with which the Water deſcends in the Centre of the Hole, a the Height by falling from which *in Vacuo* the Velocity v is acquired, A the Height of the Water above the Hole, V the Velocity acquired by falling *in Vacuo* from the Height A, T the Time of falling from the ſame, z the Diſtance of every Particle from the Centre of the Hole, and let the Water run out in the Time T.

Now the *Meaſure* of the Water, which goes out of the Hole in the Time T, will be found after this manner: z will be the *Radius* of every Circle within the Hole, $2mz$ the Circumference of the ſame, $2mz\dot{z}$ the

the *annulus nascens* adjacent to that Circumference, $\frac{v\times\overline{r-z}}{r}$ the Velocity of the Water in the *annulus nascens*.

Since V : $v\times\frac{\overline{r-z}}{r}$: : 2 A : $\frac{2Av\times\overline{r-z}}{Vr}$, therefore $\frac{2Av\times\overline{r-z}}{Vr}$ will be the Space, which the Water makes in flowing thro' the *annulus nascens* in the Time T, and the *Measure* of the same Water will be $2mz\dot{z}\times\frac{2Av\times\overline{r-z}}{Vr}=\frac{4mAv\times\overline{rz\dot{z}-z^2\dot{z}}}{Vr}$.

But the *Measure* of the Water passing thro' the *annulus nascens* is the Fluxion of the *Measure* of the Water passing thro' a Circle whose *Radius* is z. Therefore the *Measure* of the Water, which passes thro' this Circle in the Time T, is the fluent quantity of the Fluxion just now mentioned $\frac{4mAv}{Vr}\times\overline{rz\dot{z}-z^2\dot{z}}$, that is $\frac{4mAv}{Vr}\times\frac{\overline{3rz^2-2z^3}}{6}$ $=\frac{2mAv}{3Vr}\times\overline{3rz^2-2z^3}$. And supposing $z=r$, the *Measure* of the Water passing thro' all the Hole in the Time T will be found, namely $\frac{2mAvr^2}{3V}$.

But the Motion of the same Water will be found thus.

The *Measure* of the Water running thro' the *annulus nascens* in the Time T is, as we have just now seen, $\frac{4mAv}{Vr}\times rz\dot{z}-z^2\dot{z}$, and as it's Velocity $v\times\frac{r-z}{r}$, it's *Motion* will be $\frac{4mAv}{Vr}\times\overline{rz\dot{z}-2z^2\dot{z}}$ $\times\frac{v}{r}\times\overline{r-z}=\frac{4mAv^2}{Vr^2}\times\overline{r^2z\dot{z}-2rz^2\dot{z}\times z^3\dot{z}}$, the fluent Quantity of which is $\frac{4mAv^2}{Vr^2}\times\overline{\frac{r^2z^2}{2}-\frac{2rz^3}{3}+\frac{z^4}{4}}=\frac{mAv^2}{3Vr^2}$ $\times\overline{6r^2z^2-8rz^3+3z^4}$, which is the *Motion* of the Water flowing thro' a Circle whose *Radius* is z. And supposing $z=r$, we have the *Motion* of the Water running out in the Time T thro' all the Hole, $\frac{mAv^2r^2}{3V}$.

But this *Motion*, by the Solution of Prob. I, and by the *Hypothesis* of this, is equal to the *Motion*, which the Column over the Hole can acquire in the same Time T, by falling with it's own Weight thro' a *Vacuum*, that is to the *Motion* A F V, or A V × mr^2. Therefore

$$\frac{mAv^2r^2}{3V} = mAVr^2.$$

Hence $v^2 = 3V^2$ and $v = V \times \sqrt{3}$.

Moreover the above-mentioned Measure of the Water running out thro' the Hole in the Time T, namely $\frac{2mAvr^2}{3V} = \frac{2mAr^2}{3V} \times V \times \sqrt{3} = \frac{2Amr^2}{\sqrt{3}}$ Q. E. I.

Since $V^2 : v^2 :: A : a$, therefore $a = \frac{Av^2}{V^2} = \frac{A}{V^2} \times 3V^2 = 3A$. *Coroll.* 1.

Therefore the Height, to which the Water can rise with that Velocity, with which it runs out in the Centre of the Hole, is triple the Height of the Water above the Hole.

The Figure of the *Cataract* will be determined after the following manner. *Coroll.* 2.

Let H K be any Section of a *Cataract*, whose Centre is c, and let it's *Radius* be c K $= y$, the Height of the Water above that Section, or I $c = x$, t the Time of falling *in Vacuo* from the Height x, and, as before, let L F $= r$, and I L $=$ A. Fig. 101.

Now the Water passes thro' this Section H K in the same Quantity as it runs out of the Hole E F.

But if the Vessel is shortened, so that it's Height is reduced from I L to I c, and so that Section H K now becomes the very Hole in the Bottom of the Vessel, the Water will pass in a given Time, thro' this Section, in a Quantity neither greater nor less than it passed before thro' the same, before the Vessel was shortened: not greater, because that Section is pressed only by the same Weight of the superincumbent Column, by which it was pressed before; not less, because the lower Water H K F E does not hinder the Motion of the Water, as it passes thro' the Section H K.

Now the Vessel being shortened, the *Measure* of the Water running out of the Hole H K in the Time t, by the preceding Solution, is $\frac{2xmy^2}{\sqrt{3}}$, and the *Measure* of the Water running out in the Time T is $\frac{2xmy^2}{\sqrt{3}} \times \frac{T}{t} = \frac{2xmy^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}}$. For T : t :: $\sqrt{A} : \sqrt{x}$.

But from what has been said above, the *Measure* of the Water running out of the Hole H K, when the Vessel is shortened in the given Time T, is equal to the *Measure* of the Water passing in the same Time

 thro'

thro' the Section H K, when the Vessel is entire, or to the *Measure* of the Water running out of the Hole E F in the same Time. Therefore $\frac{2\,x\,m\,y^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}} = \frac{2\,A\,m\,r^2}{\sqrt{3}}$, or $y^2\sqrt{x} = r^2\sqrt{A}$, or $y^4 x = r^4 A$, which is the very Equation of the hyperpolical Curve, by the Rotation of which I formerly shewed the Figure of the *Cataract* to be generated *.

Schol. I. The *Measure* of the Water now found $\frac{2\,A\,m\,r^2}{\sqrt{3}}$, or $2\,A\,m\,r^2 \times 0{,}577350$ is a small matter greater than the *Measure* $2\,A\,m\,r^2 \times 0{,}571$, which is obtained from *Poleni*'s Experiments. But this difference, at least in some Part, proceeds hence, that in this Problem the Decrease of the *Motion* of the Water arising from Resistance is accounted for nothing.

Schol. II. The *Measure* of the effluent Water determined by this Solution is right, if we consider the Height of the Vessel as infinitely great with Regard to the Diameter of the Hole. But as this Height has a finite Proportion to the Diameter of the Hole, the *Measure* will be something less, so that, when the Height is 5 times greater than the Diameter, it will differ from the truth only $\frac{1}{32000}$, and when it is double, only about $\frac{1}{51200}$, which Differences are smaller than can be discovered by any Experiment.

But this very small Difference proceeds from this, that the abovementioned relative Velocity, and therefore the absolute Velocity of the Particles of Water, which we have considered as in a Direction perpendicular to the Horizon, are really in a Direction something oblique, when every Particle comes nearer to the *Axis* of the *Cataract* in descending.

But if any one desires to obtain a true and accurate Solution, when the Altitude of the Water has any Proportion whatsoever to the Diameter of the Hole, it may be done after the following manner.

From the property of the *cataractic* Curve explained in Cor. 2. of this Problem, by which $y^4 x = r^4 A$, the Subtangent of this Curve will be found to be to the Circumference of the Hole $4\,A$, and to the Circumference of any Section the Subtangent will be $4\,x$, that is, equal to the Height of the Water above that Section taken 4 times.

But such a *cataractic* Curve is discribed not only by the outer Water, which flows beyond the Circumference of the Hole, but also by that Part of the Water, which flows thro' any *Annulus* of the Hole; that is, every Particle of Water describes such a Curve.

Now let z be the Distance of any Particle placed in the Hole from the Centre of the Hole, and let this Particle descend thro' the least

* Vol. IV. Part. 1. Ch. V. §. iv.

Space

Space imaginable in a Tangent to the *cataractic* Curve. Hence it's Velocity will be in the Direction of this Tangent, or the Velocity $v \times \overline{\frac{r-z}{r}}$ explained in this Problem to the Velocity of the same in a Direction Perpendicular to the Horizon as $\sqrt{16\,A^2 + z^2} : 4\,A$.

Therefore the Velocity in a Direction perpendicular to the Horizon is $v \times \overline{r-z} \times \frac{4\,A}{\sqrt{16\,A^2 + z^2}}$.

Hence also, by following the Steps of the above Solution, you will have for the *Measure* of the Water passing thro' the *annulus nascens* $\frac{16\,m\,A^2\,v}{r\,V} \times \frac{r\,z\,\dot{z} - z^2\,\dot{z}}{\sqrt{16\,A^2 + z^2}}$.

Now the fluent Quantity of this Fluxion will be found, by the Cotesian Measures of *Ratios Form.* V. and VI. to be $\frac{16\,m\,A^2\,v}{r\,V}$

$$\times \overline{\frac{2\,r - z}{2} \sqrt{16\,A^2 + z^2} + 8\,A^2 \left| \frac{z + \sqrt{16\,A^2 + z^2}}{4\,A} \right.}$$ and

by making first $z = 0$, and then $z = r$, you will have $\frac{16\,m\,A^2\,v}{r\,V}$

$$\times \overline{\frac{r}{2} \sqrt{16\,A^2 + r^2} - 4\,A\,r + 8\,A^2 \left| \frac{r + \sqrt{16\,A^2 + r^2}}{4\,A} \right.}$$ for the

Measure of the Water passing thro' all the Hole in the Time T.

Moreover, by proceeding after the same manner, you will have for the Motion of the Water passing thro' the *annulus nascens* $\frac{64\,m\,A^3 v^2}{r^2\,V}$

$$\times \overline{\frac{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}{16\,A^2 + z^2}}.$$ Of which Fluxion the fluent Quantity,

by the Cotesian *Form.* I and II, will be found $\frac{64\,m\,A^3\,v^2}{r^2\,V}$ in $\frac{z^2 - 4\,r\,z}{2}$

$$+ \frac{r^2}{2} \left| \frac{16\,A^2 + z^2}{16\,A^2} - \frac{16\,A^2}{2} \right| \frac{16\,A^2 + z^2}{16\,A^2} + 2\,r\sqrt{-16\,A^2} \left| \frac{z + \sqrt{-16\,A^2}}{\sqrt{16\,A^2 + z^2}} \right.,$$ and by supposing $z = r$, you will have $\frac{64\,m\,A^3\,v^2}{r^2\,V}$

in

in $\frac{r^2 - 16A^2}{2} \Big| \frac{16A^2 + r^2}{16A^2} + 2r\sqrt{-16A^2} \Big| \frac{r + \sqrt{-16A^2}}{\sqrt{16A^2 + r^2}}$

$- \frac{3r^2}{2}$, which is the *Motion* of the Water passing thro' the Hole in the Time T.

Now let $M = \frac{r}{2}\sqrt{16A^2 + r^2}$,

$$N = 8A^2 \Big| \frac{r + \sqrt{16A^2 + r^2}}{4A}, \text{ or}$$

$$N = 4A^2 \Big| \frac{16A^2 + 2r^2 + 2r\sqrt{16A^2 + r^2}}{16A^2}$$

$$K = \frac{r^2 - 16A^2}{2} \Big| \frac{16A^2 + r^2}{16A^2}, \text{ and}$$

$$L = 2r\sqrt{-16A^2} \Big| \frac{r + \sqrt{-16A^2}}{\sqrt{16A^2 + r^2}}, \text{ or}$$

$L = 2r \times 4A$ (Rad : Tang : Sec : : $4A : r : \sqrt{16A^2 + r^2}$) and the *Measure* of the Water passing thro' the Hole in the Time T will be $\frac{16mA^2v}{rV} \times \overline{M + N - 4Ar}$; but the Motion of the same Water will be $\frac{64mA^3v^2}{r^2V} \times \overline{L + K - \frac{3r^2}{2}}$.

But $\frac{64mA^3v^2}{r^2V} \times \overline{L + K - \frac{3r^2}{2}} = mr^2AV$, wherefore $v^2 = \frac{r^4V^2}{64A^2 \times \overline{L + K - \frac{3r^2}{2}}}$, and the Measure of the Water running thro' the Hole in the Time T is $2mAr \times \frac{M + N - 4Ar}{\sqrt{L + K - \frac{3r^2}{2}}}$.

But if instead of the *Measures* of *Ratio's* and Angles, you would rather make use of infinite *Series*, the above *Measure* of the Water running thro' the *annulus nascens*, $\frac{16mA^2v}{rV} \times \frac{rz\dot{z} - z^2\dot{z}}{\sqrt{16A^2 + z^2}}$, must be reduced to this Form, $\frac{mv}{rV} \times \overline{rz\dot{z} - z^2\dot{z}} \times \frac{16A^2}{\sqrt{16A^2 + z^2}}$; and by reducing

16

$\frac{16\,A^2}{\sqrt{16\,A^2 + z^2}}$ to an infinite Series, you will have $\frac{m\,v}{r\,V} \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}}$ in $4\,A - \frac{z^2}{8A} + \frac{3\,z^4}{8^3\,A^3} - \frac{5\,z^6}{4 \times 8^4\,A^5} + \frac{3^5\,z^8}{8\ \,A^7}$, — &c. for the *Measure* of the Water running thro' the *annulus nascens*; and by the fluent Quantity of this Fluxion, or by $\frac{m\,v}{V}$ in $\frac{2\,A\,r^2}{3} - \frac{r^4}{20 \times 8\,A} + \frac{r^6}{14 \times 8^3\,A^3} - \frac{5\,r^8}{36 \times 8^5\,A^5} + \frac{7\,r^{10}}{22 \times 8\ \,A}$, &c. we shall have the *Measure* of the Water running out thro' all the Hole.

Moreover the above *Motion* of the Water passing thro' the *annulus nascens* $\frac{64\,m\,A^3\,v^2}{r^2\,V} \times \frac{\overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}}{16\,A^2 + z^2} = \frac{4\,m\,A\,v^2}{r^2\,V}$

$\times \overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}} \times \frac{16\,A^2}{16\,A^2 + z^2} = \frac{4\,m\,A\,v^2}{r^2\,V}$

$\times \overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}$ in $1 - \frac{z^2}{16\,A^2} + \frac{z^4}{16^2\,A^4} - \frac{z^6}{16^3\,A^6}$

$+ \frac{z^8}{16^4\,A^8} - \frac{z^{10}}{16^5\,A^{10}}$, + &c. and by the fluent Quantity of this Fluxion, or by $\frac{4\,m\,A\,v^2}{V}$ in $\frac{r^2}{12} - \frac{r^4}{60 \times 16\,A^2} + \frac{r^6}{168 \times \overline{16}^2\,A^4}$

$- \frac{r^8}{360 \times \overline{16}^3\,A^6} + \frac{r^{10}}{660 \times \overline{16}^4\,A^8}$ — &c. we shall have the *Motion* of the Water running out thro' all the Hole.

Therefore $A\,m\,r^2\,V = \frac{4\,m\,A\,v^2}{V}$ in $\frac{r^2}{12} - \frac{r^4}{60 \times 16\,A^2} +$ &c. or

$$V^2 = v^2 \text{ in } \frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c. or}$$

$$v^2 = \frac{V^2}{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c.}}$$

$$\text{and } v = \sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c.}}$$

Whence the *Measure* of the Water running out thro' the Hole, or

m

$$\frac{mv}{V} \text{ in } \frac{2Ar^2}{3} - \frac{r^4}{20\times 8A} + \frac{r^6}{14\times 8^3A^3} - \frac{5r^8}{36\times 8^5A^5} + \&c.$$

$$= \frac{m}{V} \text{ in } \frac{2Ar^2}{3} - \frac{r^4}{20\times 8A} + \frac{r^6}{14\times 8^3A^3} - \frac{5r^8}{36\times 8^5A^5} + \&c.$$

$$\times \frac{V}{\sqrt{\frac{1}{3} - \frac{r^2}{15\times 16A^2} + \&c.}}$$

$$= m \text{ in } \frac{\frac{2Ar^2}{3} - \frac{r^4}{20\times 8A} + \&c.}{\sqrt{\frac{1}{3} - \frac{r^2}{15\times 16A^2} + \&c.}}$$

Whence at length the *Measure* of the Water running out of the Hole is found to be $\frac{2Amr^2}{\sqrt{3}}$ in $1 - \frac{r^2}{20\times 16A^2} + \frac{r^4}{56\times 16^2A^4} - \&c.$ Hence by supposing A infinite with respect to the Diameter of the Hole, the *Measure* comes out $= \frac{2Amr^2}{\sqrt{3}}$, as we have determined in this Problem.

When $A = 10\,r$, the *Measure* $= \frac{2Amr^2}{\sqrt{3}} \times \overline{1 - \frac{1}{32000}}$, or thereabouts.

When $A = 4\,r$ the *Measure* $= \frac{2Amr^2}{\sqrt{3}} \times 1 - \frac{1}{5120}$, or thereabouts.

Therefore instead of the true *Measure*, we may take the *Measure* $\frac{2Amr^2}{\sqrt{3}}$, without Danger of any sensible Error, even in so small an Altitude, and much more in an Altitude many times greater, as it is usually in Experiments; and by this means the Computation, from being very laborious and intricate, becomes most easy.

Prob. III. *The same being supposed, and neglecting the Acceleration of the Water without the Hole, to determine the Diameter of the Vein of Water to the small Distance without the Hole, where the Vein is most contracted, and the Velocity of the Water in the Vein so contracted.*

In the Solution of the former Problem it was shewn, that the Particles of Water bursting out of the Hole, do not come forth with one Velocity common to them all, but with the greater Velocity as they are less Distant from the Centre of the Hole; and that the relative Velocity of the

the inner Particles, with regard to the Particles that touch each of them on the outside, is constantly equal thro' all the Hole; and that this relative Velocity proceeds from the Resistance, which the Water finds, as it descends toward the Hole, from the surrounding Water.

But after the Water is gone out of the Hole, and it's outer Surface no longer finds any Resistance from the surrounding Water, nor from the ambient Air, being carried thro' a *Vacuum* by the *Hypothesis*, that relative Velocity, or Inequality of absolute Velocity, can no longer remain. For now the swifter Particles must necessarily accelerate the slower contiguous Particles, and must also themselves be retarded by the slower, till they have all acquired one Velocity common to all the Particles: Which will happen within a small Space after their being come out of the Hole.

But whilst all the Particles are acquiring this common Velocity, the Diameter of the Vein must necessarily be contracted. This happens in the same manner, as when a rapid River is joined with a slower, for Instance the *Rhone* with the *Saone*. In the common Channel the Velocity of the Water brought from both Rivers is equal, and the Water is transmitted thro' a Section of this Channel in like Quantity as it was before transmitted thro' the Sections of both Rivers: But a Section of the *Rhone*, after it has received the *Saone*, is far less than the Sum of the Sections of the *Rhone* and of the *Saone*, before their Conflux.

Therefore let the *Radius* of the contracted Vein of Water, where all the Particles in the same Section of the Vein have acquired an equal Velocity, be ϱ, and let that common Velocity be called v.

Now the *Measure* of the Water flowing thro' a Section of the contracted Vein in the Time T will be thus.

$V : v :: 2A : \frac{2Av}{V}$, which is the Length of the Vein of Water passing thro' this Section in the Time T. And $\frac{2Av}{V} \times m\varrho^2$ is the *Measure* of the Water passing thro' this Section in the same Time.

And the *Motion* of the Water passing thro' the Section of the Vein in the Time T is $\frac{2Av}{V} \times m\varrho^2 \times v$, or $\frac{2Am\varrho^2v^2}{V}$.

But the *Measure* of the Water passing thro' the Section of the Vein is equal to the *Measure* of the Water running out thro' the Hole in the same Time, that is, $\frac{2Am\varrho^2v}{V} = \frac{2Amr^2}{\sqrt{3}}$, or $2\varrho^2v = \frac{2r^2V}{\sqrt{3}}$.

Moreover the *Motion* of the Water bursting out of the Hole, as it is not altered by the Action of the Particles on each other, will be equal to the Motion of the Water running thro' the Section of the Vein, that is $A\,V\,m\,r_2 = \frac{2\,A\,m\,\varrho^2\,\upsilon^2}{V}$, or $2\,\varrho^2\,\upsilon^2 = r^2\,V^2$.

But $\upsilon = \frac{2\,\varrho^2\,\upsilon^2}{2\,\varrho^2\,\upsilon} = r^2\,V^2 \times \frac{\sqrt{3}}{2\,r^2\,V^2}$, that is $\upsilon = \frac{V\sqrt{3}}{2}$, and $\upsilon^2 = \frac{3\,V^2}{4}$.

And $\varrho^2 = \frac{r^2\,V^2}{2\,\upsilon^2} = \frac{r^2\,V^2}{2} \times \frac{4}{3\,V^2}$, or $\varrho^2 = \frac{2\,r^2}{3}$, and $\varrho = \frac{r\sqrt{2}}{\sqrt{3}}$

Q. E. I.

Coroll. Since $\upsilon^2 = \frac{3\,V^2}{4}$, and the Altitudes are in a duplicate *Ratio* of the Velocities generated by falling from thence, it is manifeſt, that this is the Velocity of the Water in the contracted Vein, by which it can riſe upwards *in Vacuo* to $\frac{3}{4}$ of the Height of the Water above the Hole.

Schol. I. This wonderful Contraction of the Vein of Water was firſt of all diſcovered, about 30 Years ago, by Sir *I. Newton*, when he was conſidering the Motion of effluent Water more attentively, on Account of ſome Difficulties propoſed by Mr *Cotes*, who was then taking care of the ſecond Edition of the *Principia*; and *Poleni*, afterwards confirmed it by many Experiments. From that Time this *Phænomenon* has more than enough exerciſed the Wits of Philoſophers: But the true Cauſe of this Contraction has hitherto eſcaped them all.

The *Radius* of the Vein determined by this Problem $\frac{r\sqrt{2}}{\sqrt{3}}$, or $r \times 0{,}8165$, is a little leſs than the *Radius* $r \times 0{,}84$, delivered by Sir *Iſaac*: and a little greater than the *Radius* $r \times 0{,}78$, according to *Poleni*'s Meaſure, and is almoſt a mean between them both.

But the Velocity above determined $\frac{V\sqrt{3}}{2}$, by which the Water can riſe upwards to $\frac{3}{4}$ of the Height of the Veſſel above the Hole, differs very far from the Experiments, by which Fountains are found to riſe to almoſt the entire Height of the Ciſtern. Now that Difference of Velocity, proceeds from the Reſiſtance of the ambient Air, which is ſo far from diminiſhing the Height of the Spout, as is commonly believed, that it does not a little increaſe it, as will appear from the Solution of *Prob.* VII.

Schol. II. From what has been ſaid above in *Prob.* II. *Schol.* 2. it appears that theſe Values of ϱ and υ, cannot be accounted accurate, unleſs the Altitude of the Water be accounted infinite with regard to the Diameter of the Hole, but that they approach very near to the true Values if the Altitude of the Water is double, or more than double the Diameter of the Hole. But if you would accurately determine the ſame Values, you may uſe the *Meaſure* determined in the ſame *Scholium* or $2\,m\,A\,r \times \frac{\overline{M + N - 4\,A\,r}}{\sqrt{L + K - \frac{1}{2}r^2}}$, whence you will have $v = \frac{r\,V}{2} \times \frac{\sqrt{L + K - \frac{1}{2}r^2}}{\overline{M + N - 4\,A\,r}}$, and $\varrho = \sqrt{2} \times \frac{\overline{M + N - 4\,A\,r}}{\sqrt{L + K - \frac{1}{2}r^2}}$. You may alſo make uſe of the infinite Serieſes in the ſame Scholium.

The Water running out from a circular Hole in the Middle of the Bottom of a cylindrical Veſſel, where the Particles of Water in running down within the Veſſel find ſo great a Reſiſtance from the Want of Lubricity, that the Motion of the Water is thereby remarkably diminiſhed, as alſo the given Meaſure *of the effluent Water, to determine the* Motion *of the ſame, and the Velocity with which it goes out through the Middle of the Hole.* Prob. IV.

Let the given *Meaſure* of the Water running out in the Time T be $2\,m\,r^2\,A\,q$. Therefore the *Meaſure* aſſigned by *Analyſis* in the Solution of Prob. II. will be equal to it, namely $\frac{2\,m\,r^2\,A\,v}{3\,V}$, that is $2\,m\,r^2\,A\,q = \frac{2\,m\,r^2\,A\,v}{3\,V}$, or $v = 3\,V\,q$.

But the *Motion* of the ſame Water aſſigned by the *Analyſis* in the ſame Problem is $\frac{m\,r^2\,A\,v^2}{3\,V}$, and by ſubſtituting inſtead of v^2 it's Value juſt now found, that *Motion* becomes $\frac{m\,r^2\,A}{3\,V} \times 9\,V^2\,q^2 = 3\,q^2\,m\,r^2\,A\,V$. Q. E. I.

If from the *Motion*, which can be generated in the Time T by the Column of Water over the Hole, or from $m\,r^2\,A\,V$, be ſubtracted, the *Motion* of the Water running out in the ſame Time, $3\,q^2\,m\,r^2\,A\,V$, there remains *the Motion* loſt in the Time T by the Reſiſtance $m\,r^2\,A\,V \times \overline{1 - 3\,q^2}$. Coroll.

If you deſire an accurate Solution, you muſt have Recourſe to *Prob.* II. *Schol.* 2. after this Manner; $2\,m\,r^2\,A\,q = \frac{16\,m\,A^2\,v}{r\,V} \times \overline{M + N - 4\,A\,r}$, whence $v = V\,q \times \frac{r^3}{8\,A \times \overline{M + N - 4\,A\,r}}$. And the *Motion* Schol.

of the Water running out in the Time T will be $m r^2$ A V $\times q^2 r_2$ $\times \frac{\overline{L + K - \frac{1}{2} r^2}}{\overline{M + N - 4 A r}|^2}$, whence the *Motion* lost by the Resistance in the Time T will be $m r^2$ A V $\times \overline{1 - \frac{q^2 r^2 \times \overline{L + K - \frac{1}{2} r^2}}{\overline{M + N - 4 A r}|^2}}$.

Prob. V. *With the same Positions and* Data, *and neglecting the Acceleration of the Water without the Hole, to determine the Diameter of the Vein of Water at a small Distance without the Hole, where the Vein is most contracted, and the Velocity of the Water in the Vein so contracted*

By *Prob.* III. the *Measure* of the Water passing through a Section of the Vein in the Time T is $\frac{2 m \varrho^2 A v}{V}$: But this is equal to the given *Measure* $2 m r^2 A q$; whence $\varrho^2 v = r^2 V q$.

Moreover, by the same *Prob.* III. the *Motion* of the Water passing through a Section of the Vein in the Time T is $\frac{2 m r^2 A v^2}{V}$, to which is equal the *Motion* determined by the former Problem, $3 q^2 m r^2$ A V, wherefore $2 \varrho^2 v^2 = 3 q^2 r^2 V^2$.

But $v = \frac{2 \varrho^2 v^2}{2 \varrho^2 v} = \frac{3 q^2 r^2 V^2}{2 q r^2 V} = \frac{3 q V}{2}$.

And $\varrho^2 = \frac{r^2 V q}{v} = r^2 V q \times \frac{2}{3 q V} = \frac{2 r^2}{3}$; wherefore $\varrho = \frac{r \sqrt{2}}{\sqrt{3}}$

Q. E. I.

Coroll. 1. The same Proportion remains between the *Radius* of the Hole, and the *Radius* of the contracted Vein, whether the *Motion* of the effluent Water be in any Manner diminished by Resistance, as in this Prob. or not diminished, as in *Prob.* III. seeing it is either way $\varrho = \frac{r \sqrt{2}}{\sqrt{3}}$.

Coroll. 2. When the Motion of the effluent Water is diminished by Resistance, the Velocity is at the same Time diminished in the contracted Vein. For when in *Prob.* III. it had been $v = \frac{V \sqrt{3}}{2}$, it now becomes $v = \frac{3 q V}{2}$, that is, v is diminished from V $\times$ 0,866 to V $\times$ 0,856, taking $q = 0{,}571$, according to *Poleni*'s *Experiments*.

Schol. Accurately it will be $v = V \times r^2 q \times \frac{\overline{L + K - \frac{1}{2} r^2}}{\overline{M + N - 4 A r}|^2}$ & $\varrho = \sqrt{2}$ $\times \frac{\overline{M + N - 4 A r}}{\sqrt{L + K - \frac{1}{2} r^2}}$, in like Manner as it was found in *Prob.* III. *Schol.* 2.

The

The Water running out through a circular Hole in the Middle of the Bottom of a cylindrical Vessel, when the Particles of Water, as they flow downwards within the Vessel, suffer so great a Resistance from a Want of Lubricity, that the Motion *of the Water is notably diminished thereby, and also the* Measure *of the effluent Water being given, to determine the* Motion *of the same, and the Velocity with which it goes out through the middle of the Hole.* Prob. VI.

Let the given *Measure* of the Water running out in the Time T be $2\,m\,r^2\,A\,q$, as in *Prob.* IV. and by help of the same Problem we shall have the *Motion* of the same $3\,q^2\,m\,r^2\,A\,V$, and the Velocity, with which it goes out through the Centre of the Hole, or $v = 3\,q\,V$. Q. E. I.

When q is given, v is as V, that is, as $\surd$ A. *Coroll.*

You will find these accurately determined in *Prob.* IV. *Schol.* *Schol.*

The Water running out into the Air, and neglecting the Acceleration of the Water without the Hole proceeding from Gravity, if any 2 *of the* 3 *following are given, namely the* Measure *of the effluent Water, the Velocity in the* Axis *of the contracted Vein, and the Diameter of the same Vein, to determine the remaining one.* Prob. VII.

When the Water bursting out of the Hole is carried through a *Vacuum*, it is shewn in the Solution of *Prob.* III. that the Velocity of the Particles of Water becomes equal through the whole Section of the contracted Vein: But now, when the Vein is carried through the Air, that Equality of Velocity must necessarily be taken away. For the outer Parts of the Vein stir the surrounding Air into Motion, and are retarded by it, so that they cannot acquire an equal Velocity with the rest. But the outer Parts, when they are retarded by the Air, retard the inner contiguous Parts, and they the next; and by this Means every inner Particle is carried swifter than the contiguous outer one, so that the Velocity is greatest in the Axis of the Vein, and least in the Circumference. And as the outer Parts are carried more slowly through the Air, than they would be carried through a *Vacuum*, it thence comes to pass, that the middle Parts are carried more swiftly, the Air surrounding the Vein, than they would be carried, on the Removal of the Air. For which Reason the middle Parts of the Water in Fountains rise much higher in the open Air, than they would rise *in Vacuo*, as we observed at the latter End of *Prob.* III. *Schol.* I.

Moreover, those Parts of the Air, which are contiguous to the Vein of Water, when they are stirred into Motion by the Water, stir others into Motion, that lie near them on the outside, and these the next outer ones, and those the rest successively to some certain Distance from the Circumference of the Vein.

But the Velocity of the Particles of Water, must necessarily so decrease from the *Axis* of the Vein to it's Circumference, that the relative Velocity of every Particle wheresoever situated, may be every-

where

where one and the fame, with refpect to the Particle lying on the outfide, for the Caufes mentioned in the Solution of *Prob.* II. For if any Particle has a greater relative Velocity than the reft, it muft find a greater Refiftance from the Attrition of the adjacent Particle outwards, and by that means will be brought to an equal relative Velocity with the reft. In like Manner every Particle of the furrounding Air, which is ftirred into Motion, will have one, and the fame relative Velocity with Refpect to the adjacent Particle of Air outwards.

But the relative Velocity of the Particles of Water among themfelves, is very different from the relative Velocity of the Particles of Air, which may be conceived in this Manner.

Any Particle of Water in the outer Part of the Vein is follicited by the next Particle of Water inwards to accelerate the Motion; and is alfo retarded by the next Particle of Air: And when that outer Particle has acquired the due Velocity, thefe two contrary Forces muft needs be equal, one of which retards the Particle, and the other accelerates it. But that cannot be done, unlefs the *Factum* of the relative Velocity, and of the Denfity of the accelerating Particle of Water is equal to the *Factum* of the relative Velocity, and of the Denfity of the retarding Particle of Air. But the Denfity of Air, is to the Denfity of Water as 1 to 900 nearly. Therefore the relative Velocity between the outer Particle of Water, and the next of Air, is to the relative Velocity of the 2 next Particles of Water as 900 to 1 nearly.

Moreover, that inmoft Particle of Air is follicited by the next contiguous Particle of Water to accelerate the Motion, and retarded by the next Particle of Air outwards. And as here two contrary Forces are equal to one another, the *Factum* of the relative Velocity and Denfity of the accelerating Particle of Water, will be equal to the *Factum* of the relative Velocity and Denfity of the retarding Particle of Air. Wherefore the relative Velocity, which is between thofe 2 Particles of Air, will be to the relative Velocity, which is between the inmoft Particle of Air, and the next of Water, as 900 to 1 nearly; and it will be to the relative Velocity, which is between the 2 next Particles of Water, as 900×900 to 1 nearly: And this fo great relative Velocity will always be conftant through the whole Thicknefs of the Ring of Air, which is ftirred into Motion by the running Water.

Now let the fame be fignified by the Letters r, m, v, a, V, A, T, as in *Prob.* II. Alfo let υ be the Velocity of the Water in the *Axis* of the contracted Vein of Water, ρ the *Radius* of the fame Vein, R the *Radius* of an imaginary Vein, by which the Velocity υ, by decreafing gradually, in like Manner as it decreafes in the true Vein, is reduced to nothing.

Alfo let the *Meafure* of the Water running out through the Hole in the Time T, be $2qmr^2$ A.

Now

Now the *Meaſure* of the Water running in the ſame Time through the contracted Vein, by the Method laid down in *Prob.* II. will be found $\frac{2mAv\rho^2}{3RV} \times \overline{3R - 2\rho}$.

But theſe *Meaſures* are equal, that is, $2qmr^2A = \frac{2mAv\rho^2}{3RV} \times \overline{3R - 2\rho}$, or $3qr^2RV = v\rho^2 \times \overline{3R - 2\rho}$.

Moreover, as the *Meaſure* of the Water running through the Hole in the Time T, is $2qmr^2A$, the *Motion* of the ſame, by *Prob.* VI. is $3q^2mr^2AV$.

And the *Motion* of the Water running through the Vein in the ſame Time, by the Method uſed in *Prob.* II. is found $\frac{mAv^2 \times \overline{6R^2\rho^2 - 8R\rho^3 + 3\rho^4}}{3VR^2}$.

Now theſe are equal, that is, $3q^2mr^2AV = \frac{mAv^2 \times \overline{6R^2\rho^2 - 8R\rho^3 + 3\rho^4}}{3VR^2}$, or $9q^2r^2R^2V^2 = v^2 \times \overline{6R^2\rho^2 - 8R\rho^3 + 3\rho^4}$.

Theſe 2 Equations being rightly reduced, in order to extirpate R we come to the following Equation, $\rho^4v^2 = 2qvVr^2\rho^2 + 12q^2V^2r^2\rho^2 - 9q^2V^2r^4$, wherefore $\rho^2 = \frac{qVr^2}{v^2} \times \overline{v + 6qV - 2\sqrt{3qvV + 9q^2V^2 - 2v^2}}$, and hence is obtained ρ itſelf, or the *Radius* of the contracted Vein, ſeeing q and v are given.

Moreover from the ſame Equation is drawn $v = \frac{qVr}{\rho^2} \times \overline{r + 2\sqrt{3\rho^2 - 2r^2}}$.

Laſtly $q = \frac{\rho^2 v}{rV \times \overline{r + 2\sqrt{3\rho^2 - 2r^2}}}$. Q. E. I.

We ſuppoſed above, that the *Motion* of the Water running through the contracted Vein is equal to the *Motion* of that which runs through the Hole. But this is not true in Mathematical Strictneſs. For the *Motion* of the Water running through the Hole is equal to the *Motion* of the Water running through the contracted Vein, and to the Motion of the Ring of Air ſurrounding the Vein, which Air is ſtirred into Motion by the Water running through the Vein, taken together. But we look upon the *Motion* of the Ring of Air as nothing, ſince it's Thickneſs is not greater than $\frac{R - \rho}{900 \times 900}$, and it's Denſity is not greater than $\frac{1}{900}$ Part of the Denſity of the Water; Schol. I.

and

and by doing this we render the Equations far more simple than otherwise they would be.

Schol. II. By *Prob.* V. *Cor.* 1. when the Water runs out into a *Vacuum*, the same *Ratio* continues between the *Radius* of the Hole, and the *Radius* of the contracted Vein, whether the *Motion* of the effluent Water be in any Manner diminished by Resistance or not. Wherefore, as in a Physical Matter, we think it very near the Truth, that the *Ratio* between these *Radii* should be considered as given, even when the Water runs through Air, howsoever the *Motion* of the effluent Water may be diminished by Resistance, or at least, that this *Ratio* should be changed as little as possible. And as this is found to agree with the Experiments hitherto made, as will more plainly appear below, we shall look upon it as true, till we shall be informed of something more certain by more accurate Experiments.

Moreover, if a *Ratio* is given between r and ϱ, a *Ratio* is also given between r and R, or a *Ratio* between the *Radius* of the Hole, and the imaginary *Radius*, by which the Velocity υ, by gradually decreasing is reduced to nothing.

For by eliminating υ from the 2 above Equations, $9 q^2 r^2 R^2 V^2 = \varrho^2 \upsilon^2 \times \overline{6 R^2 - 8 R \varrho + 3 \varrho^2}$, and $3 q r^2 R V = \varrho^2 \upsilon \times \overline{3 R - 2\varrho}$, we come to the Equation $\varrho^2 \times \overline{9 R^2 - 12 R \varrho + 4 \varrho^2}, = r^2 \times \overline{6 R^2 - 9 R \varrho + 3 \varrho^2}$, wherefore $R = \frac{\varrho}{3} \times 2 + \frac{r}{\sqrt{3 \varrho^2 - 2 r^2}}$.

Besides, from one of these Equations, $3 q r^2 R V = \varrho^2 \upsilon \times \overline{3 R - 2 \varrho}$; we have $3 r^2 R : \varrho^2 \times \overline{3 R - 2 \varrho} :: \upsilon : q V$, and since the former *Ratio* is given, the latter *Ratio* is also given, that is, the Quantity $\frac{\upsilon}{q V}$ is given.

We shall afterwards demonstrate, how great these 3 given *Ratios* are.

The same continued. No. 453. p. 65. Apr. &c. 1739. *Of the Resistance of the Parts of Water among themselves, proceeding from a Want of Lubricity.*

2. Before we proceed any farther, we must consider that Resistance of Fluids, which arises from the Motion of the same Parts among themselves, and is called by Sir *I. Newton*, a *Resistance* arising from a want of Lubricity.

He makes it of two Sorts, one arising from the Tenacity of the Fluid, the other from the mutual Attrition or Friction of the Parts of the Fluid between themselves.

He thinks the First is uniform in a given Surface, or that it produces an Effect proportional to the Time; which Opinion is favoured by Experiments: He is of Opinion that the latter is increased in Proportion to the Velocity, or in a Proportion something less. But he does not determine any Thing about this, for want of suitable Experiments.

But

But these two Resistances have a different Proportion between themselves, not only according to the diversity of the Fluid, as for Instance, there is a greater Tenacity and less Attrition in Oil or melted Suet than in Water; but also in the same Fluid, according to the different Velocity with which the parts of the Fluid are moved among themselves. But in a given Fluid there must necessarily be some certain Velocity, where these Resistances are equal between themselves; and if we could find that Velocity by Experiment, their Proportion might be determined in any other Velocities. But we have no Experiments, that I know of, nor is it easy to contrive any, by means of which that Velocity may be known, which may serve for a Foundation to the rest.

We suspect indeed, nay we think it probable, that the very least of that fundamental Velocity is not in Water from one Cause, where the Resistances arising from Tenacity and Friction are equal between themselves. But this being granted, when, as the Velocity increases, the Resistance from Friction in like manner increases, but the Resistance from Tenacity in no wise increases, it is plain, that this last Resistance has but a very small Proportion to the first, where the Parts of the Fluid are moved among themselves with any notable Velocity; and therefore, that it may safely be neglected.

However, whether we neglect this, and take only the other Resistance, which arises from Friction, or comprehend both under the name of *Resistance* arising from want of Lubricity, certainly the Laws, by which this *Resistance* increases or is diminished, are to be sought only from Experience. Therefore when we ascribe to it the following laws of increasing, tho' after a diligent Consideration of the Experiments hitherto made, they may seem to have a great Probability, and this we do with an Intent, that if future Experiments should teach any thing more certain, we may not unwillingly change our Minds.

The *Resistance*, which arises from a want of Lubricity of the Water, is in a *Ratio* compounded of the 3 following: *Hypothesis.*

1. Of the *Ratio* of the Surface of the Parts which are moved. This, I think, all Philosophers admit.

2. Of the *Ratio* of the relative Velocity, by which the Parts of Water are moved among themselves. This, if I mistake not, is admitted by the rest, Nor does Sir *I. Newton* much differ.

3. Of the subduplicate *Ratio* of the Altitude of the Water. For we assume this, being led by Experience, and in some Measure also by Sir *I. Newton*, who thinks that the Attrition of the Parts becomes stronger, and their Separation from each other more difficult by greater Pressure *.

To explain the Resistance of the Parts of the Cataract, *which arises from a want of Lubricity.* Prob. VIII.

* *Princip.* Lib. II. Prop. lii. Schol.

Let r be the *Radius* of the Hole, A the Altitude of the *Cataract*, y the *Radius* of any horizontal Section, x the Altitude of the *Cataract* above that Section, z the *Radius* of any Circle in that Section, v the Velocity of the Water in the Centre of the Hole.

Now $\frac{vx^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ will be the Velocity of the Water in the Centre of the Section, whose *Radius* is y. For the Velocity in the Centre of the Section, is the same as if that Section was a Hole in the Bottom of a shortened Vessel, whose Altitude is x; and therefore is as $x^{\frac{1}{2}}$ by *Prob.* VI.

Coroll. Also $\frac{y-z}{y} \times \frac{vx^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ will be the Velocity of the Water in the Circumference of the Circle, whose *Radius* is z; $\frac{\dot{z}vx^{\frac{1}{2}}}{yA^{\frac{1}{2}}}$ the relative Velocity $2mz\dot{x}$ the Surface of the nascent Cylinder, whose *Radius* is z and Altitude $\dot{x}$, and by our 3 Positions the *Resistance* of the Surface of this Cylinder, as $2mz\dot{x} \times \frac{\dot{z}vx^{\frac{1}{2}}}{yA^{\frac{1}{2}}} \times x^{\frac{1}{2}} = \frac{2mv\dot{x}x\dot{z}z}{yA^{\frac{1}{2}}}$.

Now let $\dot{x}$, x, and y be considered as constant Quantities, whilst z flows till it becomes equal to y; and the fluent Quantity of the Fluxion $\frac{2mv\dot{x}x\dot{z}z}{yA^{\frac{1}{2}}}$ will be $\frac{2mv\dot{x}xz^2}{2yA^{\frac{1}{2}}}$, or $\frac{mv\dot{x}xz^2}{yA_{2}}$, or (making $z = y$) $\frac{mv\dot{x}xy}{A^{\frac{1}{2}}}$, as the Resistance of the nascent Cylinder, whose *Radius* is y, and Altitude $\dot{x}$.

But by the property of the *cataractic* Curve $y^4 x = r^4 A$, and $y x^{\frac{1}{4}} = r A^{\frac{1}{4}}$: Whence the *Resistance* of this nascent Cylinder will be as $\frac{mv\dot{x}xrA^{\frac{1}{4}}}{A^{\frac{1}{2}}x^{\frac{1}{4}}}$, or as $\frac{mvr\dot{x}x^{\frac{3}{4}}}{A^{\frac{1}{4}}}$, and the *Resistance* of the whole *Cataract* will be as the fluent Quantity of this Fluxion, or as $\frac{mvrx^{\frac{7}{4}}}{A^{\frac{1}{4}}}$

$\times \frac{4}{7}$, or, making $x = \dot{A}$ as $\frac{4}{7} m v r A^{\frac{3}{2}}$. And since by *Prob.* IV $v = 3 q V$, the *Resistance* in the *Cataract* will be, as $\frac{12 q m V r A^{\frac{3}{2}}}{7}$, or as $q V r A^{\frac{3}{2}}$. Q. E. I.

Since V is as $\sqrt{A}$, the *Resistance* in the *Cataract* will be as $q r A^2$. Coroll.

In the Solution just now made, instead of the Surface of the *cataractic Taleola*, whose *Radius* is z, according to which the Particles of Water pass by each other with an equable relative Velocity, we made use of the Surface of the nascent Cylinder, whose *Radius* is z, and Altitude $\dot{x}$, or of the Surface $2 m z \dot{x}$, when really the Surface of that *Taleola* is $2 m z \times \sqrt{\dot{x}^2 + \dot{z}^2}$. Schol.

But if that is corrected, the *Resistance* of the Surface of this *Taleola* will be found as $2 m z \sqrt{\dot{x}^2 + \dot{z}^2} \times x^{\frac{1}{2}} \times \frac{z v x^{\frac{1}{2}}}{y A^{\frac{1}{2}}}$

$$= \frac{2 m v x \dot{z} z \sqrt{\dot{x}^2 + \dot{z}^2}}{y A^{\frac{1}{2}}}$$

And since by *Prob.* II. *Schol.* 2. the Subtangent of the *cataractic* Curve is $4x$, and the Tangent itself $\sqrt{16x^2 + z^2}$, therefore $4x : \sqrt{16x^2 + z^2} :: \dot{x} : \sqrt{\dot{x}^2 + \dot{z}^2} = \frac{\dot{x}\sqrt{16x^2 + z^2}}{4x}$.

Therefore the *Resistance* of the Surface of the *Taleola* will be as

$$\frac{2 m v x \dot{z} z}{y A^{\frac{1}{2}}} \times \frac{\dot{x}}{4x}\sqrt{16x^2 + z^2} = \frac{m v \dot{x}}{2 y A^{\frac{1}{2}}} \dot{z} z \sqrt{16x^2 + z^2}$$

$$= \frac{m v \dot{x} \dot{z} z}{2 y A^{\frac{1}{2}}} \text{ in } 4x + \frac{z^2}{2 \times 4x} - \frac{z^4}{8 . \overline{4x}^3} + \frac{z^6}{16 \times \overline{4x}^5} - \frac{5 z^8}{128 \times \overline{4x}^7}$$

$$+ \frac{7 z^{10}}{256 \times \overline{4x}^9} \text{ \&c.} = \frac{m v \dot{x}}{2 y A^{\frac{1}{2}}} \text{ in } 4x \dot{z} z + \frac{\dot{z} z^3}{2 \times 4x} - \frac{\dot{z} z^5}{8 \times \overline{4x}^3}$$

$$+ \frac{\dot{z} z^7}{16 \times \overline{4x}^5} - \frac{5 \dot{z} z^9}{128 \times \overline{4x}^7} + \frac{7 \dot{z} z^{11}}{256 \times \overline{4x}^9} - \frac{21 \dot{z} z^{13}}{1024 \times \overline{4x}^{11}} \text{ \&c.}$$

 But

But by taking the Quantities $\dot{x}$, x, and y for constant ones, the Fluent of this Fluxion will be $\frac{m v \dot{x}}{2 y A^{\frac{1}{2}}}$ in $\frac{4xz^2}{2} + \frac{z^4}{8 \times 4x}$ $- \frac{z^6}{48 \times \overline{4x}^{3}} + \frac{z^8}{8 \times 16 \times \overline{4x}^{5}} - \frac{z^{10}}{256 \times \overline{4x}^{7}} + \frac{7z^{12}}{12 \times 256 \times \overline{4x}^{9}}$ $-$ &c.

And by supposing $z = y$, this Fluent will be $\frac{m v \dot{x}}{2 A^{\frac{1}{2}}}$ in $2xy$ $+ \frac{y^3}{\| \times 4x} - \frac{y^5}{48 \times \overline{4x}^{3}} + \frac{y^7}{8 \times 16 \times \overline{4x}^{5}} - \frac{y^9}{256 \times \overline{4x}^{7}}$ $+ \frac{7y^{11}}{12 \times 256 \times \overline{4x}^{9}}$ $-$ &c. which will be as the *Resistance* in the cataractic *Taleola*, whose *Radius* is y, and Altitude $\dot{x}$.

But this is as the Fluxion of the *Resistance* in the whole *Cataract*, and by putting $y = \frac{r A^{\frac{1}{4}}}{x^{\frac{1}{4}}}$, it becomes $\frac{m v \dot{x}}{2 A^{\frac{1}{2}}}$ in $\frac{2 r x A^{\frac{1}{4}}}{x^{\frac{1}{4}}} + \frac{r^3 A^{\frac{3}{4}}}{8 \times 4 \times x^{\frac{7}{4}}}$ $- \frac{r^5 A^{\frac{5}{4}}}{48 \times 4^3 \times x^{\frac{17}{4}}} + \frac{r^7 A^{\frac{7}{4}}}{8 \times 16 \times 4^5 \times x^{\frac{27}{4}}} - \frac{r^9 A^{\frac{9}{4}}}{256 \times 4^7 \times x^{\frac{37}{4}}} +$ &c. $= \frac{m v r}{2 A^{\frac{1}{4}}}$ in $2 \dot{x} x^{\frac{3}{4}} + \frac{r^2 A^{\frac{1}{2}} \dot{x}\, \overline{x}^{-\frac{7}{4}}}{32} - \frac{r^4 A \dot{x}\, \overline{x}^{-\frac{17}{4}}}{48 \times 4^3} + \frac{r^6 A^{\frac{3}{2}} \dot{x}\, \overline{x}^{-\frac{27}{4}}}{8 \times 16 \times 4^5}$ $-$ &c. But the fluent Quantity of this Fluxion is $\frac{m v r}{2 A^{\frac{1}{4}}}$ in $2 x^{\frac{7}{4}} \times \frac{4}{7}$ $+ \frac{r^2 A^{\frac{1}{2}}\, \overline{x}^{-\frac{3}{4}}}{32} \times \frac{4}{3} - \frac{r^4 A\, \overline{x}^{-\frac{13}{4}}}{48 \times 4^3} \times \frac{4}{13} + \frac{r^6 A^{\frac{3}{2}}\, \overline{x}^{-\frac{23}{4}}}{8 \times 16 \times 4^5} \times -\frac{4}{23}$ $-$ &c. And this, supposing $x = A$, becomes $\frac{m v r}{2}$ in $\frac{8 A^{\frac{2}{2}}}{7}$ $- \frac{r^2}{3 \times 8 A^{\frac{1}{2}}} + \frac{r^4}{12 \times 13 \times 4^3 A^{\frac{5}{2}}} - \frac{r^6}{32 \times 23 \times 4^5 A^{\frac{9}{2}}} +$ &c.

or

or $\frac{4 m v r A^{\frac{3}{2}}}{7}$ in $1 - \frac{7 r^2}{3 \times 4^3 A^2} + \frac{7 r^4}{6 \times 13 \times 4^5 A^4} - \frac{7 r^6}{23 \times 4^9 A^6}$ + &c. which is as the *Reſiſtance* thro' the whole *Cataract.*

But if the Altitude be accounted as infinite with reſpect to the Diameter of the Hole, the *Reſiſtance* will be as $\frac{4 m v r A^{\frac{3}{2}}}{7}$ as was determined in the former Solution.

If $A = 10 r$, the *Reſiſtance* will be as $\frac{4 m v r A^{\frac{3}{2}}}{7} \times \overline{1 - \frac{1}{2743}}$ nearly.

If $A = 4 r$, the *Reſiſtance* will be as $\frac{4 m v r A^{\frac{3}{2}}}{7} \times \overline{1 - \frac{1}{439}}$ nearly.

We may therefore uſe $\frac{4 m v r A^{\frac{3}{2}}}{7}$ for the Meaſure of the Reſiſtance, without Danger of any ſenſible Error, even when the Altitude of the Water does not exceed two Diameters of the Hole, and much more in a far greater Altitude.

The Meaſure *being given of the Water running out thro' a given circular Hole in the Middle Part of the Bottom of a Cylindrical Veſſel of a given Depth, to determine the* Meaſure *of the Water running out of another Veſſel of any given Depth, thro' any given circular Hole whatſoever.* Prob. IX.

Let r be the *Radius* of the given Hole, A the given Depth, $2 q m r^2 A$ the given Meaſure of the Water running out in that Time, in which a heavy Body would fall *in Vacuo* thro' the Altitude A.

Hence by *Prob.* IV. $3 q^2 m r^2 A V$ will be the *Motion* of the Water running out in the ſame Time: and by *Prob.* IV. *Cor.* the *Motion* loſt in the ſame Time by the Reſiſtance will be $m r^2 A V \times \overline{1 - 3 q^2}$. Therefore the equal Force of Reſiſtance can generate this *Motion* in the ſame Time.

But the *Motions* are generated in the ſame Space of Time with Forces generating the ſame proportional.

Therefore the *Motion* $m r^2 A V$, which the Weight of the Column of Water $m r^2 A$ can generate in this Time, by *Prob.* 1. when all *Reſiſtance* is away, is to the *Motion* $m r^2 A V \times \overline{1 - 3 q^2}$, which the *Reſiſtance*

I can

can generate in the same Time, as the Weight $m r^2 A$ to the *Resistance* itself. Wherefore the Resistance $= m r^2 A \times \frac{m r^2 A V \times \overline{1 - 3 q^2}}{m r^2 A V} = m r^2 A \times \overline{1 - 3 q^2}$.

After the same manner, by putting s and E for the *Radius* of the Hole, and the Altitude of the new Vessel, and $2 p m s^2 E$ for the Measure of the Water running out in the same Time, in which a heavy Body would fall *in Vacuo* thro' the Altitude E, you will have the *Resistance* in the new Vessel $= m s^2 E \times \overline{1 - 3 p^2}$.

But by *Prob.* VIII. *Cor.* these 2 *Resistances* are to each other as $q r A^2$ to $p s E^2$.

Therefore $m r^2 A \times \overline{1 - 3 q^2} : m s^2 E \times \overline{1 - 3 p^2} :: q r A^2 : p s E^2$, or $r \times \overline{1 - 3 q^2} : s \times \overline{1 - 3 p^2} :: q A : p E$, or $p r E \times \overline{1 - 3 q^2} = q s A \times \overline{1 - 3 p^2}$, which Equation being rightly reduced we come to the following, $p = \sqrt{\frac{1}{3} + \overline{\frac{r E \times \overline{1 - 3 q^2}}{6 q s A}}^2} - \frac{r E \times \overline{1 - 3 q^2}}{6 q s A}$, or making $r E = n s A$.

$$p = \sqrt{\frac{1}{3} + \overline{\frac{n \times \overline{1 - 3 q^2}}{6 q}}^2} - \frac{n \times \overline{1 - 3 q^2}}{6 q}.$$

Whence we have $p \times 2 m s^2 E$, which is the *Measure* of the Water running out of the second Vessel, in the Time that a heavy Body falls *in Vacuo* thro' the Altitude E. Q. E. I.

Coroll. 1. If the Diameters of the Holes shall be in a *Ratio* of the Altitudes of the Water, the *Ratio* of the *Measures* will be the same, as if the Water ran out without any *Resistance.*

For if $r : s :: A : E$, $r E = s A$, and $n = 1$, wherefore $p = \sqrt{\frac{1}{3} + \overline{\frac{1 - 3 q^2}{6 q}}^2} - \frac{1 - 3 q^2}{6 q}$, and by Reduction $p = q$; wherefore $2 q m r^2 A : 2 p m s^2 E :: 2 m r^2 A : 2 m s^2 E$, which is the *Ratio* of the *Measures*, when all *Resistance* is away.

Coroll. 2. If E is accounted for nothing with Regard to the Altitude A, then n also must be accounted for nothing, whence $p = \frac{1}{\sqrt{3}}$. Therefore, the smaller the Altitude E is taken, the nearer p comes to $\frac{1}{\sqrt{3}}$.

If

If s is to be accounted infinitely great with reſpect to the *Radius* r, then $p = \frac{1}{\sqrt{3}}$. Therefore the greater the *Radius* s is taken, the more p verges to $\frac{1}{\sqrt{3}}$. Coroll. 3.

The Water running out into the Air, to determine the Proportion between the Diameter of the Hole and the Diameter of the contracted Vein. Prob. X.

This Proportion cannot be determined without the help of Experiments. By *Prob.* VII. $\varrho^2 = \frac{qVr^2}{v^2} \times \overline{v + 6qV - 2\sqrt{3qvV + 9q^2V^2} - 2v^2}$ whence q and v being known ϱ is determined.

But we have no Experiments, that I know of, by which we may meaſure q and v.

Poleni's Experiments exhibit the *Meaſure* of the effluent Water whence q is known; but they do not ſhew the greateſt Diſtance, to which the Water is carried that comes horizontally out of the Hole, or the Diſtance to which the middle Part of the Vein reaches, that comes out with the Velocity v.

But *Mariotte*'s Experiments meaſure the greateſt perpendicular Height, to which Water riſes, when it's Motion is turned upwards, or the Height, which the Water coming out from the middle of the Vein reaches, whence v^2 is known; but they do not exhibit the *Meaſure* of the effluent Water.

Therefore for want of fit Experiments, we ſhall hardly be able to determine the Proportion ſought any otherwiſe than probably; and this we ſhall do in the following manner.

In *Prob.* VII. *Schol.* 2. we ſhewed it to be probable, that the *Ratio* is conſtant between theſe *Radii*, or at leaſt that it is very little changed.

It is manifeſt from *Mariotte*'s Experiments, that the difference between the Altitude, which the Water ſpringing upwards reaches, and the Altitude of the Veſſel, has nearly a duplicate *Ratio* of the Altitude of the Veſſel.

Therefore let a be the Height, to which the Water running thro' the *Axis* of the Vein with the Velocity v can riſe; then, by *Mariotte*'s Experiments, $A - a$ as A^2, and $\frac{A^2}{A - a}$ will be the given Quantity.

But in one Experiment, which *Mariotte* reckons a fundamental one, A was $= 60$ *Paris* Inches, and he found $a = 59$ *Paris* Inches, the Diameter of the Hole meaſuring ½ an Inch. Therefore in this caſe $\frac{A^2}{A - a} = 3600$, and as this Quantity is given, it will always be $3600\, a = 3600\, A - A^2$, or $a = \frac{3600\, A - A^2}{3600} = A - \frac{A^2}{3600}$.

Therefore

Therefore if $A = 1$ Inch, or is double the Diameter of the Hole, $a = 1 - \frac{1}{3600}$. But $u^2 : V^2 :: a : A :: 1 - \frac{1}{3600} : 1$.

Therefore as the Altitude of the Veſſel is double the Diameter of the Hole, we may have $u^2 = V^2$, or $u = V$.

Moreover, by *Prob.* IX. *Cor.* 4. E decreaſing, p tends to $\frac{1}{\sqrt{3}}$.

Therefore when the Altitude of the Veſſel is very ſmall, as if it does not exceed 2 Diameters of the Hole, we may have p or $q = \frac{1}{\sqrt{3}}$.

But by *Prob.* VII.

$\varrho^2 = \frac{qVr^2}{u^2} \times \overline{u + 6qV - 2\sqrt{3quV + 9q^2V^2 - 2u^2}}$, and ſubſtituting inſtead of u and q the Values of the ſame juſt now found, or V and $\frac{1}{\sqrt{3}}$, it becomes $\varrho^2 = \frac{r^2}{V\sqrt{3}} \times \overline{V + 2V\sqrt{3} - 2\sqrt{V^2\sqrt{3} + 3V^2 - 2V^2}}$

$= \frac{r^2}{\sqrt{3}} \times \overline{1 + 2\sqrt{3} - 2\sqrt{1 + \sqrt{3}}}$, or $\varrho^2 = r^2 \times 2 + \frac{1}{\sqrt{3}} - 2\sqrt{1 + \sqrt{3}} = r^2 \times 0{,}6687553907$ whence $\varrho = r \times 0{,}81777466$.

Here therefore is the Value of ϱ, when the Altitude of the Water is double the Diameter of the Hole; and as by *Prob.* VII. *Schol.* 2. ϱ obtains a conſtant Proportion to the *Radius* of the Hole, it will obtain the ſame Value in any Altitude of Water. Q. E. I.

Coroll. 1. By *Prob.* VII. $R = \frac{\varrho}{3} \times \frac{2 \times r}{\sqrt{3\varrho^2 - 2r^2}}$, and by the Value of ϱ juſt now found, we have $R = r \times 3{,}98877150$, which is the Value of R, when the Altitude of the Water is double the Diameter of the Hole; and as by *Schol.* 2. of the ſame *Prob.* there is a conſtant Proportion between r and R, therefore R will obtain this very Value, whatſoever may be the Altitude of the Water.

Coroll. 2. Becauſe u is almoſt $= V$, and q is almoſt $= \frac{1}{\sqrt{3}}$, when the Altitude of the Water is double the Diameter of the Hole, therefore it will be to this Altitude of the Water $\frac{u}{qV} = \sqrt{3}$ very nearly. And as by *Prob.* VII. *Schol.* 2. the Proportion is conſtant between u and qV, therefore $\frac{u}{qV} = \sqrt{3}$, whatſoever the Altitude of the Water may be.

The

The Water running out of a given Vessel always full, thro' a given Hole, into the Air, and any one of the 3 following Quantities being given, namely, the Measure *of the effluent Water, the* Velocity *in the* Axis *of the contracted Vein, or the* Altitude, *to which the middle Part of the Vein can rise, the Motion being turned upwards, to determine the rest.* Prob. XI.

Let A be the Altitude of the Vessel, r the *Radius* of the Hole, $2qmr^2$ A, the measure of the effluent Water, v the Velocity in the *Axis* of the contracted Vein, a the Altitude, to which the Water running out thro' the *Axis* of the Vein can rise, and first let $2qmr^2$ be given, whence q is given.

By *Prob.* X. *Cor.* 2. $\frac{v}{q\mathrm{V}} = \sqrt{3}$, whence $v = q\mathrm{V}\sqrt{3}$. Hence $v^2 = 3q^2\mathrm{V}^2$.

But $\mathrm{V}^2 : v^2 :: \mathrm{A} : a = \frac{v^2\mathrm{A}}{\mathrm{V}^2} = \frac{3q^2\mathrm{V}^2\mathrm{A}}{\mathrm{V}^2} = 3q^2\mathrm{A}$.

If, secondly, v is given, then $q = \frac{v}{\mathrm{V}\sqrt{3}}$, and $2qmr^2\mathrm{A} = \frac{2mr^2\mathrm{A}^2}{\mathrm{V}\sqrt{3}}$.

Moreover $a = \frac{v^2\mathrm{A}}{\mathrm{V}^2}$.

Lastly, if a is given, since $a = 3q^2\mathrm{A}$, therefore $q^2 = \frac{a}{3\mathrm{A}}$ and $q = \sqrt{\frac{a}{3\mathrm{A}}}$.

Also $v^2 = \frac{a\mathrm{V}^2}{\mathrm{A}}$, whence $v = \mathrm{V}\sqrt{\frac{\mathrm{A}}{a}}$. Q. E. I.

The Altitude being given, to which, when the Motion is turned upwards, Water rises issuing thro' the Air from a Vessel of a given Altitude thro' a given circular Hole, to determine the Altitude, to which, when the Motion is turned upwards, Water will rise, when it issues from a Vessel of any given Altitude, thro' any given circular Hole. Prob XII.

Let the letters r, s, A, E, q, p, express the same as in *Prob.* IX; and let a and e be the Altitudes to which Water can rise, issuing out of Vessels, the Altitudes of which are A and E respectively.

Now by *Prob.* XI. $a = 3q^2\mathrm{A}$, $e = 3p^2\mathrm{E}$, whence $3q^2 = \frac{a}{\mathrm{A}}$, $1 - 3q^2 = \frac{\mathrm{A} - a}{\mathrm{A}}$, $q = \sqrt{\frac{a}{3\mathrm{A}}}$, $p = \sqrt{\frac{e}{3\mathrm{E}}}$, and $p^2 = \frac{e}{3\mathrm{E}}$.

And since by *Prob.* IX. $p=\sqrt{\frac{1}{3}+\overline{\frac{rE\times\overline{1-3q^2}}{6qsA}}\Big|^2}-\frac{rE\times\overline{1-3q^2}}{6qsA}$,

or making $rE=nsA$, $p=\sqrt{\frac{1}{2}+\overline{\frac{n\times\overline{1-3q^2}}{6q}}\Big|^2}-n\times\frac{1-3q^2}{6q}$;

hence by substituting $\frac{A-a}{A}$ for $1-3q^2$, and $\sqrt{\frac{a}{3A}}$ for q, and writing α for $A-a$, it will be $p=\frac{\sqrt{4Aa+n^2\alpha^2}-n\alpha}{2\sqrt{3Aa}}$,

and $p^2=\frac{2Aa+n^2\alpha^2-n\alpha\sqrt{4Aa+n^2\alpha^2}}{6A\alpha}$.

But $p^2=\frac{e}{3E}$, whence $\frac{e}{E}=\frac{2Aa+n^2\alpha^2-n\alpha\sqrt{4Aa+n^2\alpha^2}}{2A\alpha}$,

or $e=E\times\frac{\overline{2Aa+n^2\alpha^2-n\alpha\sqrt{4Aa+n^2\alpha^2}}}{2Aa}$, whence by writing ε for $E-e$ it becomes $\varepsilon=\frac{nE\alpha}{2Aa}\times\overline{\sqrt{4Aa+n^2\alpha^2}-n\alpha}$. Now ε or $E-e$ being given, e also is given, or the Altitude to which the Water is carried, when it issues out of the new Vessel.

Coroll. 1. If the Holes in both Vessels shall be equal, or $s=r$, then $E=nA$, or $n=\frac{E}{A}$, whence $\varepsilon=\frac{n^2\alpha}{2\alpha}\times\overline{\sqrt{4Aa+n^2\alpha^2}-n\alpha}$.

Coroll. 2. If the Altitudes of the Vessels shall be equal, or $E=A$, then $r=ns$, or $n=\frac{r}{s}$, whence $\varepsilon=\frac{n\alpha}{2a}\times\overline{\sqrt{4Aa+n^2\alpha^2}-n\alpha}$.

Coroll. 3. If the Diameters of the Holes shall be in a *Ratio* of the Altitudes, the Waters will spout to Altitudes proportional to the Altitudes of the Vessels. For if $r:s::A:E$, $re=sA$, and $n=1$, whence $\varepsilon=\frac{E\alpha}{A}$, or $\varepsilon:\alpha::E:A$, or $E-e:A-\alpha::E:A$, or $e:a::E:A$.

Coroll. 4. Since $p\times2\sqrt{3Aa}=\sqrt{4Aa+n^2\alpha^2}-n\alpha$, therefore $\varepsilon=\frac{nE\alpha}{2Aa}\times2p\sqrt{3Aa}=\frac{pnE\alpha\sqrt{3}}{\sqrt{Aa}}$, whence by substituting for $\sqrt{a}$ it's above-mentioned Value $q\sqrt{3A}$, and by a due Reduction, ϵ becomes $= \frac{pnE\alpha}{qA}$, or $\epsilon = \frac{prE^2\alpha}{qsA^2}$.

Hence, by making $p = q$, $\epsilon = \frac{rE^2\alpha}{sA^2}$, or $\epsilon : \alpha :: rE^2 : sA^2$. *Coroll.* 5.

That is, the Defects of spouting Waters, or the Differences between the Altitudes of the Spouts, and the Altitudes of the Vessels are in a *Ratio* compounded of the duplicate *Ratio* of the Altitudes of the Vessels directly, and of the *Ratio* of the Diameters of the Holes reciprocally. And this Rule is exactly true, when $sA = rE$ by *Prob.* IX. *Cor.* 1. and comes very near the Truth, when E and s are increased or diminished in the same Proportion nearly; and it errs but little from the true Altitude of the salient Water in any case, provided E be not greater than 50 Feet, and at the same Time s be not less than 3 Lines.

When $s = r$, $\epsilon = \frac{E^2\alpha}{A^2}$ nearly, that is, when the Holes are equal, *Coroll.* 6. the Defects of the Altitudes of spouting Waters are almost in a duplicate *Ratio* of the Altitudes of the Vessels, which is the very Rule of *Mariotte*.

When $E = A$, $\epsilon = \frac{r\alpha}{s}$ nearly, that is, when the Altitudes of the *Coroll.* 7. Vessels are equal the Defects of the spouting Waters are almost as the Diameters of the Holes reciprocally.

If any one has a mind to examine the Truth of this Theory by Experiments, I would desire him, *First General Scholium.*

1. To use a Vessel that is very large, at least in the upper Part, that, during the whole Time of making the Experiment, the Altitude of the Water may not sensibly be changed. But if the Vessel is not so large, but that during the Efflux from the Hole, a remarkable Decrease of the Water is found, then the just intermediate Altitude between the greatest and the least Altitude of the Water is to be taken for the constant Altitude; which is better than disturbing the natural Motion of the Water, by pouring fresh Water upon it.

2. Let the Vessel be of such a Depth, that if you would let out the Water thro' a Hole made in the Side, the Velocity of the Water going out thro' the Centre of the Hole may be safely taken for any Velocity, with which the Water will issue thro' all the Hole, when there is no Resistance.

3. Let the *Lamina*, in which the Hole is made, be so thin, or at least have so thin an Edge in the Circumference of the Hole, that the Thickness of that Edge may be accounted as nothing with respect to the Diameter of the Hole. But the Thickness of the *Lamina* should be shaved on the outer Face of the *Lamina*, leaving the inner Face next the Water

 plain;

plain; and the Angle of this Edge should be so acute, that the Water issuing thro' the Hole may not adhere to the outer Side of the *Lamina*.

These things being prepared, the following Experiments may be made, by which, as by so many *Criteria*, we may judge of the Certainty of the above Doctrine.

Exp. 1. When the Water is let out thro' a Hole in the Side of the Vessel, let the Diameter of the contracted Vein be measured very diligently, observing whether it remains always the same; howsoever the Altitude of the Water may be changed.

Exp. 2. Let it be observed, whether this Diameter has always the same Proportion to the Diameter of the Hole, when Holes of different Magnitudes are used.

Exp. 3. The Water issuing, either strait down thro' the Bottom of the Vessel, or horizontally thro' it's Side, let it be very carefully observed how much runs out in a given Time, using different Altitudes of Water, but one and the same Hole.

Exp. 4. Let the same be observed, when Holes of a different Magnitude are used, but keep the same Depth of Water.

Exp. 5. Observe how much runs out in a given Time, in 2 different Cases, in each of which there is the same Proportion of the Diameter of the Hole to the Altitude of the Water. For if the *Measures* shall be found in a *Ratio* compounded of a duplicate *Ratio* of the Diameters, and a simple *Ratio* of the Altitudes, as in *Prob.* IX. *Cor.* 3. you will have a great Confirmation of our Theory.

Exp. 6. In the same 2 Cases, the Motion of the Water being turned upwards, by means of a large Tube fitted to the Side of the Vessel, and perforated at the upper Part, observe to what Altitudes the Water will rise. For if these Altitudes are found proportional to the Altitudes of the Water in the Vessel, as in *Prob.* XII. *Cor.* 3. you will have another most certain Confirmation of this Theory.

Exp. 7. The same Hole continuing, but the Height of the Water being changed, observe to what Height the Water is carried.

Exp. 8. Let the same be observed, when the Magnitude of the Hole is changed, the Height of the Water continuing the same.

But of all these Experiments those are to be preferred, by which the Height, to which the Water rises, is noted, when the Motion of the Water is turned upwards. For this Height may far more easily be taken, than the *Measure* of the running Water, and the Error, if there is any, in taking the Altitude, is of far less Moment, than that which is committed in estimating the *Measure*. For as by *Prob.* XI. the Altitude of salient Water is $3\,q^2$ A, it is plain that the least Error admitted in the *Measure*, or in q, will be almost doubled in q^2, and so it will be doubled in the Altitude of the salient Water.

But the least Error admitted in the Altitude of the salient Water, or in $3\,q^2$ A, is reduced to almost half in estimating q, that is in the Measure of the effluent Water.

In

In the mean Time, till those Experiments are made by such Persons as have Leisure, as well as a Desire of knowing the Truth, we must use, as far as we can, those Experiments, with which we have been furnished by the Diligence of our Predecessors. **Second General Scholium.**

These are of 3 kinds: For they measure either,

1. The Diameter of the contracted Vein; or
2. The *Measure* of the effluent Water; or
3. The Altitude to which the Water rises.

1. The *Radius* of the contracted Vein, as measured by Sir *I. Newton*, $r \times 0,84$, when the Diameter of the Hole is $\frac{5}{8}$ of a *London* Inch.

The same, as measured by *Poleni*, is $r \times 0,78$ nearly, when the Diameter of the Hole is $2\frac{1}{6}$ *Paris* Inches.

By our Calculation it is $r \times 0,818$ nearly, whatsoever is the Diameter of the Hole, which is about the intermediate Magnitude between the Measures of *Newton* and *Poleni*.

2. It happens very unluckily, that none of the *Measures* of effluent Water, except those taken by *Poleni* are of any Use to our Purpose. For as he informs us, this *Measure*, when the Water issues thro' a Tube, is far greater than when it issues from a naked Hole. And as Holes made in *Laminæ* are to be looked upon as short Tubes, at least if the Thickness of the *Laminæ* is not as small as possible with respect to the Diameter of the Hole, and thence it comes to pass, that all the *Measures* of effluent Water taken before him are found to be greater than the Truth.

Therefore we must use only the Measures taken by *Poleni*. And these *, which were taken with that great Hole of 26 Lines, are 10 in Number, namely by supposing a heavy Body to fall *in Vacuo* thro' 15 Feet, 1 Inch, 10 Lines *Paris* Measure, in 1'', the Measure is

1	$= 2\,m\,r^2\,A \times$	0,5772
2	— — —	0,5772
3	— — —	0,5731
4	— — —	0,5710
5	— — —	0,5690
6	— — —	0,5675
7	— — —	0,5689
8	— — —	0,5703
9	— — —	0,5732
10	— — —	0,5613
		5,7087

Of all which the intermediate is $2\,m\,r^2\,A \times 0,571$ nearly. Therefore we have this for *Poleni's Measure* of effluent Water, when the Altitude of the Vessel is 33 *Paris* Inches, which is the intermediate Altitude between those which were used by *Poleni*.

But the *Measure*, which is taken to this Altitude by our Calculation from *Mariotte's* fundamental Experiment, which we shall produce presently.

* *Polenius de Castellis*, Art. 35, 38, 39, 42, 43; & *Epist. ad Marinonium.*

presently, is $2\ m\ r^2\ A \times 0{,}5768$, which exceeds *Poleni*'s Measure about $\frac{1}{98}$, Part. But so small a Difference might arise either from an Error of $\frac{1}{100}$ Part of an Inch in estimating the Diameter of the Hole; or from the Vessel that receives the effluent Water being about $\frac{1}{100}$ Part greater than in *Poleni*'s Computation; or partly from both. Add, that this Difference is twice as little as what is found between *Poleni*'s own Experiments.

3. We shewed before, that *Poleni* has rendered all the Experiments of his Predecessors useless concerning the Measure of effluent Water, because they took no Account of the Thickness of the *Lamina*, thro' which the Water issued. Whence some may not unreasonably suspect, that there is the same Fault in those Experiments, by which the Height of the salient Water was discovered. But *Poleni* has removed this Doubt by another excellent Observation. For he discovered the Measure of the Water to be greater in flowing from a Tube than from a naked Hole; but, what is wonderful, that Water issuing thro' Tubes * of 7 or 13 *Paris* Lines in Length, reaches only to the same, or very little less horizontal Distance, than it does when it issues from a naked Hole. Therefore the greatest Velocity of Water is very little less after it's Exit from a Tube, than after it's Exit from a Hole, when the Tube is not very short: but when the Tube is very short, such as a Hole in a *Lamina* that is not very thin, the greatest Velocity of the Water may be accounted the same after it's Exit from this Tube, as after it's Exit from a Hole in a very thin *Lamina*.

Therefore, to find out the Certainty of our Theory, let us make use of *Mariotte*'s Experiments concerning the Altitude of Fountains, in like manner as if the Holes that he made use of had been made in very thin *Laminæ*.

Let us therefore assume some one of his Experiments, which may be taken as a Foundation for finding the Altitude in the rest of the Experiments by our 12th Problem.

He indeed proposes that for a fundamental Experiment, where the Depth of Water in the Vessel is exactly 5 *Paris* Feet. But since ever so little an Error, suppose of 2 Lines, in this Experiment, may produce a considerable Error, namely of more than 8 Inches, in a 7 times greater Depth, which *Mariotte* uses afterwards; we will choose that Experiment for a fundamental one, in which that greatest Altitude, 7 times greater than the first, is applied.

Therefore let that Experiment of *Mariotte*, in which the Diameter of the Hole is 6 Lines and the Depth of Water in the Vessel 34 Feet, 11 ½ Inches, or 419 ½ Inches, *Paris* Measure, for the Foundation of our Inquiry.

When he applied this Altitude, he found the Water issuing from the Hole to rise to the Height of 31 Feet, 8 or 9 Inches, that is, to the Height of 380 ½ Inches.

* *Epist. ad Marinonium.*

Therefore

Therefore A = 419,5 Inch. . a = 380,5 Inch. and α = 39 Inches.

In another Experiment, where E, or the Depth of Water in the Veſſel, is 26 Feet 1 Inch, the Water riſes thro' the ſame Hole, according to *Mariotte*, to the Height of 24 Feet, 2 ½ Inches. But e, or the Height of the ſalient Water, by *Prob.* XII. *Cor.* 1. is 24 Feet 3 Inches.

But, for the better comparing of the Altitudes, which *Mariotte* found the ſalient Water to reach, with thoſe Altitudes, to which it ought to ariſe by our Calculation, we have thrown both into Tab. I. where you ſee the Calculation to agree ſo with the Obſervations, that nothing can be better. And as theſe Experiments are made with the ſame Hole with the Diameter of 6 Lines, the Altitude only being changed, it can ſcarce be doubted, but our third Poſition, by which the *Reſiſtance*, *cæteris paribus*, is in a ſubduplicate *Ratio* of the Altitude, is right.

TAB. I.

Diameter of the Hole of 6 Lines.

Altitude of Water in the Veſſel.		Altitude of the ſalient Water, according to *Mariotte*.		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
34.	11,5	31.	8,5	31.	8,5
26.	1	24.	2,5	24.	3
24.	5	22.	10	22.	10
12.	4	12.	0	11.	11
5.	6	5.	4,75	5.	5
5.		4.	11	4.	11,2 lin.
35.	5	32.	0	32.	1

TAB. II.

Diameter of the Hole of 4 Lines.

Altitude of Water in the Veſſel.		Altitude of the ſalient Water according to *Mariotte*.		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
32.	11,5	30.	0	30.	0
24.	5	22.	8,5	21.	11
5.	6	5.	4,7	5.	4,4

TAB.

TAB. III.

Diameter of the Hole of 3 Lines.

Altitude of Water in the Vessel.		Altitude of the salient Water according to *Mariotte.*		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
34.	11,5	28.	0	28.	
26.	1	22.	0	22.	1
24.	5	22.	2	20.	11
5.	6	5.	4,7	5.	3,7

When instead of the Hole of 6 Lines *Mariotte* made use of the Hole of 4 Lines, he found the Water issuing from a Vessel of the above-mentioned Altitude, 34 Feet 11 ½ Inches, to reach the Height of 30 Feet. It ought to have reached by *Prob.* XII. *Cor.* 2. to 30 Feet 2 ½ Inches nearly.

Afterwards when he used the Hole of 3 Lines, the Water issuing from the same Vessel reached the Height of 28 Feet. It ought to have risen by the same Corollary to 28 Feet 9 Inches nearly.

But these differences between the Altitudes from Calculation, and those observed by *Mariotte* might proceed from a small Error in taking the Diameters of such small Holes.

For if the *Radius* of the greatest Hole, which *Mariotte* makes equal to 3 Lines, exceeded 3 Lines by $\frac{1}{100}$ Part of a *Paris* Inch; or if the *Radius* of the second Hole, which *Mariotte* makes equal to 2 Lines wanted $\frac{1}{176}$ Part of a *Paris* Inch of 2 Lines; in either Case the Water will rise by the Calculation to the Height of 30 Feet, as *Mariotte* observed.

Also if the *Radius* of the least Hole was less than 1 ½ Line by $\frac{1}{100}$ Part of a *Paris* Inch, and at the same Time the *Radius* of the greatest Hole exceeded 3 Lines by $\frac{1}{100}$ Part of an Inch, the Calculation will give the Altitude of the salient Water 28 Feet, as *Mariotte* found it.

The Calculation being thus corrected *Tab.* II. and III. exhibit the Altitudes of *Mariotte* compared with our Calculation.

But here it must be observed, in *Tab.* II. that the Altitude of the Water spouting from a Vessel of 24 Feet 5 Inches, according to *Mariotte*'s Observation, reaches to 22 Feet 8 ½ Inches, and in *Tab.* III. that the Altitude of the Spout from the same Vessel is 22 Feet 2 Inches, both which greatly exceed the Altitudes assigned by our Calculation.

But it is manifest, that *Mariotte*'s Numbers are corrupted. For,

1. The above-mentioned Rule of *Mariotte*, which, as he himself testifies, agrees well enough with the Observations, exhibits much smaller Numbers, which come pretty near to our Calculation.

2. It can never be, that the Water issuing from the Hole of 4 Lines should reach the Height of 22 Feet 8 ½ Inches, nor that the Water issuing from the Hole of 3 Lines should reach the Height of 22 Feet 2 Inches, for Water issuing from a Hole of 6 Lines reaches only to the Height of 22 Feet 10 Inches, which will easily appear from the Analogy of *Mariotte*'s Observations.

3. If the true Height is 22 Feet 2 Inches in *Tab.* III. the Water issuing from a Vessel 24 Feet 5 Inches deep, rises to a greater Height than when it issues from a Vessel 26 Feet 1 Inch deep, which is manifestly absurd.

Hence I am induced to believe, that *Mariotte*, when he spake of the first of these Experiments, wrote in his *Adversaria*, *Le jet de quatre lignes n'a été plus bas que* d'onze pouces ou onze pouces & demi, *que celui dont l'ajutage étoit de six lignes*; whence *De la Hire* transcribed *plus bas que* d'un pouce ou un pouce & demi. Now this Correction being made, the Altitude observed by *Mariotte* will be 21 Feet 11 Inches, or 10 ½ which agrees exactly with our Calculation.

It will not seem strange that such Mistakes should happen, if we consider, that *De la Hire* himself, who, after *Mariotte*'s Death, had the Care of printing his Papers, in the Preface to this Work speaks in the following manner: *La moitié de cet ouvrage étoit assez au net pour être imprimée; mais le reste m'a donné beaucoup de peine à rassembler sur le memoires qui m'en ont été mis entre les mains apres sa mort.*

But, every thing being well weighed, our Calculation agrees so well with the Experiments of this famous and diligent Observer, as also with *Poleni*'s Measure of effluent Water, and with the Measures of the Diameter of the contracted Vein taken by Sir *I. Newton* and *Poleni*, that it can hardly be doubted, but that the above Theory is either true, or very near the Truth.

It is easily extended to Water issuing thro' any square or rectangular Hole, and also to an annular Hole, such as surrounds Sir *I. Newton*'s *Circellus* *, whence many things deduced from the Contemplation of this *Circellus*, in the Resistance of continuous Fluids must be altered; which seems necessary to be mentioned to the Learned, to excite them to a more accurate Examination of what has been said.

An Account of a new Engine for raising Water, in which Horses or other Animals draw without any Loss of Power, (which has never yet been practised) and

II. The Animals all draw horizontally, and in a strait Line, and at right Angles, whereby they exert their utmost Force. By these Advantages a far greater Power is gained from the Strength of Horses, *&c.* than by their going round in a Circle; for by the Twist and Acuteness of the Angles, they draw in towards the Centre, whereby they waste their Power, and also shorten their Levers: Besides their Muscles and Tendons from their hinder Legs all along their Sides to their Necks are unequally strained, as the Duty is hardest on one Side, even tho' their Walk is large. Therefore each of those Inconveniences must be attended

* *Princip.* Lib. II. Prop. xxxvi. Cor. 7, 8, 9, 10.

how the Strokes of the Pistons may be made of any Length, to prevent the Loss of Water, by the too frequent opening of Valves, with many other Advantages altogether new; the Model of which was shewn to the R. S. Nov. 28, by Walter Churchman, *the Inventor of it.* No. 434. p. 401. Sept. &c. 1734.

with Pain to the Animals when at Work, and a great Loss of their Strength.

2. A Crank does not rise quite ⅓ of it's Circle, neither do the Regulators or Rods rise or fall perpendicular, but obliquely, by which an oval Figure is made by the Piston's Motion in every Cylinder, which occasions great Friction and a Loss of Water, and every Arm of it is continually varying in it's Power whilst working, as it's Lever is distant from the perpendicular Line, and 2 of the Arms (supposing it a quadruple one) as they cross the Perpendicular are always drawing to and from their own Centre, by which the Power is not only lost, but the Time also; and farther yet, by the shortness of the Strokes, all the adjacent Water is frequently contrarily moved, and by the often opening and shutting of the Valves, there is also a great Waste of the Water, besides the many heavy Bearings, Frictions, Surges, and Repairs, belonging to it; all which Inconveniencies and Impediments being thoroughly considered, there must certainly be required a much greater Power to work the same than by my Method. For, hereby, a Stroke of 24 Feet will rise, and by enlarging or diminishing the fixed Wallower, you obtain a Stroke of any required Height, even to the extent of the Atmosphere's Pressure. By this great Advantage, the Water rises freer, and with greater Velocity, and as the Lifters or Forcers rise and fall exactly perpendicular, and with an equal continued Strain, and as the Bearings also are fewer and lighter, consequently the Friction in all these will be a great deal less than with the Crank, &c. And, Lastly, ⅞ of that Water which is always lost by the slow opening and shutting of the Valves will be saved.

From the above Considerations, and by the many Experiments I have made on this Occasion, in order to know the real Difference between these different ways of Working, I find, that near twice the Quantity of Water will be raised to the same Height, in the same Time, with the same Power, by my Method, more than with the best Crank-work that has ever been yet erected.

Description of the Engine. Fig. 102, 103, 104.

Fig. 102. *a. a. a. a.* Is the great Frame, the ends of which under the Pine-apples are to be contracted to the place of the little Frame, so that the Cross-piece at III. may support the 3 Bearings now shewn in the little one, for a better view only.

b. b. The little Frame on which the Cap Brasses are, which receive the turned T Gudgeons in the 3 horizontal Shafts.

c. c. The strong Supporters by the loose Wallowers.

d. d. The loose Wallowers, whose turned Rounds geer truly with the Coggs in the great Wheel.

e. e. e. The Regulator, which has a circular, direct, and retrograde Motion; see Fig, 103, 104.

f. f. The strong Shoulder or Stud fixed to the Shaft close by the Wallower, which stops this loose Wallower, when the End of the Regulator comes against it, thereby confining it for 2 Revolutions; after which it quits

quits this Stud, and does the fame on the oppofite Side of the Wheel, and fo on alternately to reverfe the Motion of the Stems in the different Cylinders.

g. g. The Wheels with their Coggs, which alternately work the fixed Wallower lying between them.

h. The fixed Wallower fuppofed to be of 4 Feet in Diameter (on a very fhort Shaft) whofe Rounds muft be of caft foft Iron, and truly turned, to elevate and deprefs the Racks to the Height of 24 Feet by it's 2 Revolutions.

i. i. i. i. The 4 Lifters or Forcers, behind each of which muft be a fmall Leverage back Wheel, truly fitted to direct the fame to rife and fall eafily and exactly perpendicular, to avoid Friction and Lofs of Water in the Cylinders.

k. k. The large vertical Wheel, a fmall Segment of which comes through the Floor in the Dome for the 4 Horfes to ftand and Draw on.

l. m. The Arms, and the main Shaft of the fame.

n. The turned T Gudgeon, with it's Collar and Shoulder, both of which muft clafp the Rim of the under Leverage Wheel; to keep all firm and fteady when in working.

o. The Leverage Wheel of about 4 Feet in Diameter, with a Brafs or Iron Rim fuppofed to be truly turned, and to have a ftrong fhort Iron Spindle through it's Centre, and at each End a turned Steel Collar and Shoulder bearing on 2 caft Cap Braffes exactly level, and funk into a ftrong arched piece of Timber well braced and fupported for this purpofe.

p. p. Two fmall fide Leverage Wheels exactly fitted to the turned Part of the great Gudgeon, between the Collar and Shoulder: they are to be fo placed and keyed, that their Friction from the Gudgeon may be alike when at Work.

q. q. The Steps which the Horfes Feet prefs, about 8 or 9 Inches broad, 2 Inches thick behind, and declining to an Edge, being defigned to make level Ground and good footing for their hinder Legs when they draw.

r. r. Four Horfes only in view to avoid Confufion, all drawing horizontally in a ftrait Line, and at right Angles, whereby thefe ufeful Animals will foon be taught a new and pleafant way of working to themfelves, a more advantageous one to their Mafters, and of greater Utility to the Publick.

s. The faftening places behind the Horfes, fuppofed to be ftrong Arms below in the Supporter, and a Crofs-Bar above, at both of which you may place fmall Sheeves or Rollers; the upper Part of them to be level with each Horfe's Breaft (when drawing) and the Rope or Strap to come over the fame, in order to keep a Weight fufpended of 300 ℔ more or lefs one or two Inches from a Plank. By this Method you will be exactly informed of the Strength of each Horfe, how long it continues, and when to relieve him, as alſo when juſtly to correct the ſlothful one, whoſe Weight reſting on the Plank will always diſcover his Lazineſs.

t. The faſtening Places before, being deſigned to direct their Heads.

u. The Dome merely for Ornament; in the place of which, erect a Workloft, over that a horizontal Windmil; on the lower End of it's upright Shaft, fix a Spur-Wheel to work with the Coggs of the great Wheel, thereby to aſſiſt the Horſes, or when there is a ſufficient Force of Wind to do their whole Duty.

w. The Coupling Staples with their Braſſes.

x. The Strong Catch which confines the great Wheel to the Frame.

y. The Screw or Key-band to confine all cloſe and tight.

z. The Cylinders which are ſcrewed together at their Ends out of Sight.

&. All the ſame ſort of Work chiefly for Uniformity in the Draught.

N. B. A ſingle Shaft with the looſe and fixed Wallowers, will be of great Simplicity and Advantage to the Publick, as being erected for leſs Expence, and as it will work pleaſantly any Number of Racks for lifting or forcing, at either of it's Ends, or at both together: But chiefly, as it is eaſily adapted to the different ſorts of Windmils, Waterwheels, &c. of all Denominations already in Uſe. It alſo ſerves for ſmall Purpoſes, *Vid.* Fig. 103. The Pins 4, 4, and the Arms 5, 5, which claſp the Braſſes 6, 6, with the oval Figure 7 and it's 2 Teeth, make this Regulator, which is worked by the Stud in the main Shaft.

☞ In large Engines and Machines where the Motion is regular, every heavy Bearing ſhould have one of theſe Wheels, for they ſave Power by greatly abating Friction. Upon the Principle of theſe Leverage Wheels, Capt. *Rowe* has publiſhed what he calls his Friction-Wheels, tho' Subſequent to my Specification thereof.

CHAP. VII.

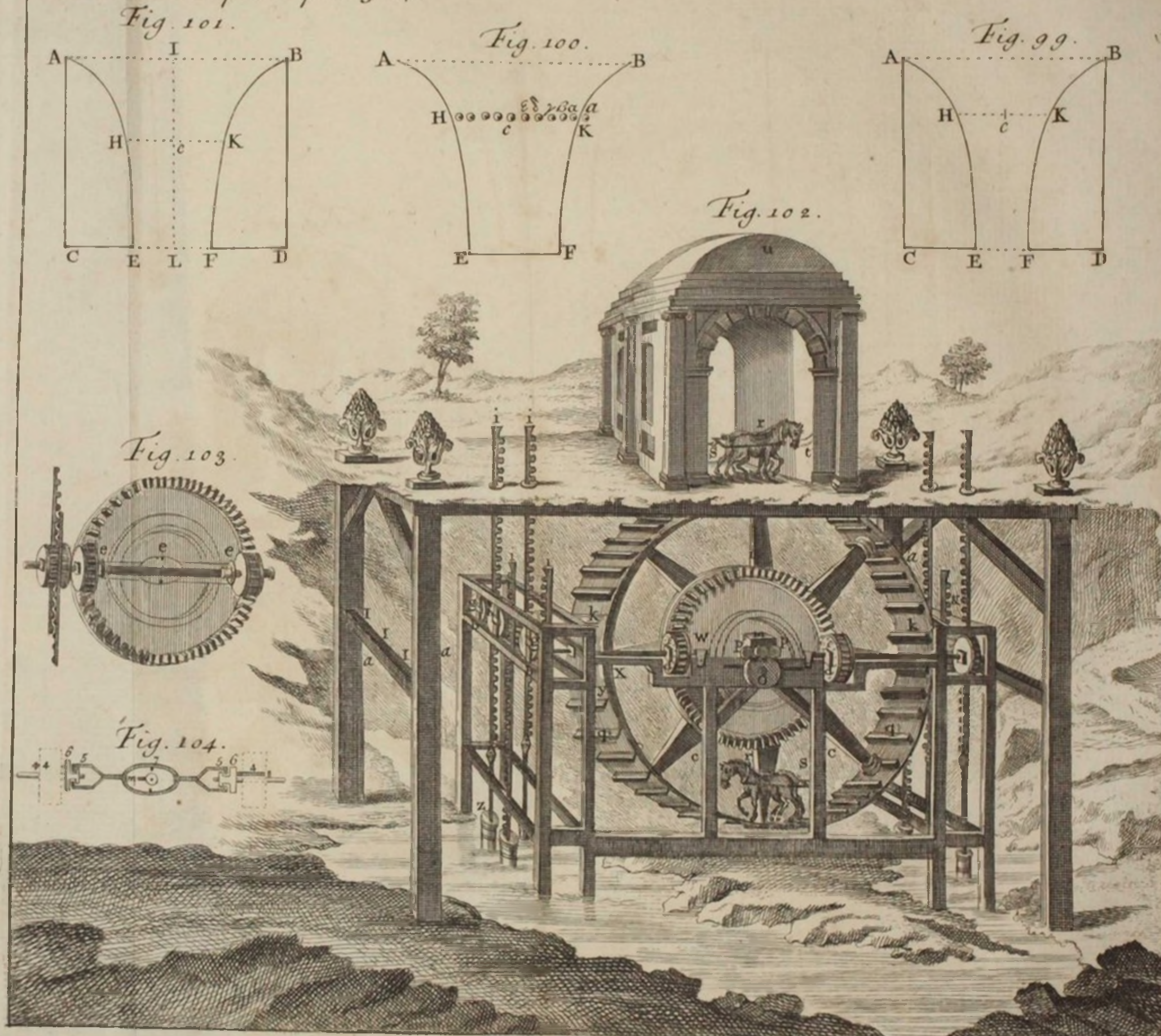
GEOGRAPHY and NAVIGATION.

Of the Figure of the Earth, and the Variation of Gravity on the Surface. By Mr James Stirling, *F. R. S.* No. 438. p. 98. July, *&c.* 1735.

I. THE Centrifugal Force, ariſing from the Diurnal Rotation of the Earth, depreſſeth it at the Poles, and renders it protuberant at the Equator; as has been lately advanced by Sir *I. Newton*, and long ago by *Polybius*, according to *Strabo* in the 2d Book of his *Geography.* But although it be of an oblate ſpheroidical Shape, yet the kind of that Spheroid is not yet diſcovered; and therefore I ſhall ſuppoſe it to be the common Spheroid generated by the Rotation of an Ellipſis about it's leſſer Axis; although I find by Computation, that it is only nearly, and not accurately ſuch. I ſhall alſo ſuppoſe the Denſity to be every

 where

Pl. XVIII. *Vol. VIII part 1. pa.* 324.



where the ſame, from the Center to the Surface, and the mutual Gravitation of the Particles towards one another, to decreaſe in the duplicate Ratio of their Diſtances: And then the following Rules will follow from the nature of the Spheroid.

1. Let A D B E be the Meridian of an oblate Spheroid, D E the Axis, A B the Diameter of the Equator, and C the Center. Take any Point on the Surface, as F, from which draw F C to the Center, F G, perpendicular to the Surface at F, meeting C B in G, and F H cutting the Line C G, ſo that C H may be to G H as 3 to 2. I ſay that a Body at F will gravitate in the Direction F H; and that the mean Force of Gravity on the Surface will be to the Exceſs of the Gravity at the Pole above that at F, as the mean Diameter multiplied into the Square of the Radius is to $\frac{1}{5}$ of the Difference of the longeſt and ſhorteſt Diameters multiplied into the Square of the Coſine of Latitude at F. Fig. 105.

2. The Decrement of Gravity from the Pole to the Equator is proportional to the Square of the Coſine of Latitude; or, which comes to the ſame, the Increment of Gravity from the Equator to the Pole is proportional to the Sine of Latitude. Hitherto I have conſidered the Variation of Gravity which ariſes from the ſpheroidical Figure, while it does not turn round it's Axis; but if it doth, the Direction of Gravity will be in the Line F G, perpendicular to the Surface; and it's Variation now ariſing from both the Figure and centrifugal Force, will be 5 times greater than what ariſes from the Figure alone; as will appear from the Proportion of the Lines F H and F G, the former being to the latter, as the whole Force of Gravity at F, while the Spheroid is at Reſt, to the Force with which a Body deſcends at F, while it turns round it's Axis.

3. From this laſt Article it appears, that $\frac{1}{5}$ of the Variation of Gravity is occaſioned by the Figure of the Spheroid, and the remaining $\frac{4}{5}$ by the centrifugal Force. And whereas the Earth could not be of an oblate ſpheroidical Figure, unleſs it turned round it's Axis; nor could it turn round it's Axis, without putting on that Figure: I ſay, that the Diminution of Gravity towards the Equator, known by the Experiments with Pendulums, prove both the Rotation and oblate ſpheroidical Figure of the Earth.

4. The mean Force of Gravity on the Surface is to the centrifugal Force at any Point F, as a Rectangle under the Radius and mean Diameter to a Rectangle under the Coſine of Latitude, and $\frac{4}{5}$ of the Difference of the longeſt and ſhorteſt Diameters. And at the Equator, where the Coſine of Latitude becomes equal to the Radius, the mean Force of Gravity is to the centrifugal Force, as the mean Diameter to $\frac{4}{5}$ of the longeſt and ſhorteſt Diameters. This Article is found from the Proportion of the Lines F H and G H; the former being to the latter as the Force of Gravity to the centrifugal Force.

5. The Proportion of the Diameters of the Earth will be found in the following manner: The Moon revolves about the Earth in 27^d, 7^h, $43'$,

on

or in 39343 Minutes: And her mean Diſtance is about 59 ½ Semidiameters of the Earth, according to *La Hire*'s and *Flamſtead*'s Tables; but near 60 ½ by *Halley*'s Tables. I ſhall therefore take 60 for the mean Diſtance, till it be better known: Then according to the Nature of Gravity, as the Cube of the Moon's Diſtance to the Semidiameter of the Earth, or as 216000 to Unity, ſo is 1547870000 the Square of the periodick Time of the Moon to 7166, the Square of the Number of Minutes in which another Moon would revolve about the Earth at the Diſtance of it's Semidiameter. And as this laſt Number to 2062096, the Square of 1436, the Number of Minutes in a Sydereal Day, ſo is Unity to 287.7; which would ſhew the Proportion of the centrifugal Force at the Equator to the mean Force of Gravity (by *Corol.* 2. *Prop.* 4. *Lib.* 1. *Princip.*) were it not for the Action of the Sun on the Moon. Therefore (by *Corol.* 17. *Prop.* 66. *Lib.* 1. *Princip.*) I ſay, As the Square of the Sydereal Year, to the Square of the periodick Time of the Moon, that is, as 179 to Unity, So is 287.7 to 1.6; which being added to 287.7, makes 289.3. And therefore, As Unity to 289, neglecting the Fraction which is uncertain, So is the centrifugal Force at the Equator to the mean Force of Gravity on the Surface. And thence (by Article 4.) As 289 to $\frac{5}{4}$, So is the mean Diameter to the Difference of the longeſt and ſhorteſt: And therefore, As the Axis is to the equatoreal Diameter, So is 2307 to 2317, or in ſmaller Numbers, As 231 to 232, the ſame as Sir *I. Newton* found in a different manner, for he makes it as 230 to 231, and as 230 to 231, So is 231 to 232.004.

6. In the ſame manner the Proportion of the Diameters of any Planet may be found, if it has a Satellite: For Inſtance, in *Jupiter*, he turns about his Axis in $9^h. 56'$, or in 596 Minutes, and his third Satellite revolves about him in $7^d, 3^h, 42', 36''$, or in 10302.6 Minutes, at the diſtance of 15.141 of his Semidiameters. Therefore, I ſay, As the Cube of 15.141 to Unity, So is the Square of 10302.6 to 30579, the Square of the Number of Minutes in which a Satellite would revolve about him at the diſtance of his Semidiameter: And as this laſt Number is to 355216, the Square of 596, ſo is Unity to 11 $\frac{5}{8}$, or the centrifugal Force at his Equator to the mean Force of Gravity on his Surface. There is no need of correcting this Number, as in the former Article, becauſe the periodick Time of *Jupiter* round the Sun is vaſtly greater than that of his third Satellite round him. I have choſen the third Satellite before any of the reſt, becauſe it's greateſt Elongation was obſerved by Dr *Pound*, with a Micrometer adapted to a Teleſcope 123 Feet long; and he alſo took the Diameter of *Jupiter* by the Tranſit of the Satellite, which is a much more exact Way than with a Micrometer. But as the Planes of *Jupiter*'s Satellites almoſt coincide with the Plane of his Equator, the Diameter, determined by the Tranſit of the Satellite, is his greateſt; and the Diſtance of the Satellite, which ought to have been given in his mean Diameters, is aſſigned in his greateſt: For which Reaſon the Force of Gravity already found, muſt be augmented in the

triplicate

triplicate Ratio of his greateſt Diameter to his mean one; that is, if a repreſents the mean Diameter, and d the Difference of the longeſt and ſhorteſt, in the Proportion of $2a + 3d$ to $2a$ very nearly. Hence, as the centrifugal Force at his Equator, to the mean Force of Gravity on his Surface, ſo is Unity to $11\frac{5}{8} \times \frac{2a+3d}{2a}$. And (by Article 4.)

$$11\tfrac{5}{8} \times \frac{2a+3d}{2a} : 1 :: a : \tfrac{4}{5}d, \text{ or } 20\,aa = 186\,ad + 279\,dd;$$

which makes a to d, as 108 to 10; and thence the Axis is to the equatoreal Diameter, as $108 - 5$ to $108 + 5$, or as 103 to 113; that is, as 12 to $13\frac{1}{6}$: Which agrees nicely with the Obſervations of both Dr *Pound* and Mr *Bradley*, made with *Huygens*'s Long Teleſcope; the former making it as 12 to 13, and the latter as 25 to 27, which is very nearly the ſame. And if this Theory agrees ſo well with Obſervations in *Jupiter*, there is no doubt but it will be more exact in the Earth, whoſe Diameters are much nearer to Equality.

7. By Experiments made at *Jamaica** in the Latitude of 18° with a very curious Clock, contrived by Mr *Graham*, it was found that the *London* Pendulum went ſlower there by 2′ 6″ in a Sydereal Day, than at *London*. But it was found by Experiments made with Thermometers, that 9″ were to be allowed for the lengthening of the Pendulum by Heat; and therefore it was retarded only 1′ 57″ by the Decrement of Gravity. So that while a Pendulum of *London* makes 86164 Vibrations, the Number of Seconds in a Sydereal Day, the ſame at *Jamaica* only gives 86047 Vibrations. Therefore the Force of Gravity at *London* is to that in the Latitude of 18°, as the Square of 86164 to the Square of 86047; that is, very nearly as 1106 to 1103. And (by Article 1, and 2.) if a denote the mean Diameter of the Earth, d the Difference of the greateſt and ſmalleſt;

$$a - \frac{ccd}{rr}$$ will denote the Force of Gravity in general in any Latitude, whoſe Coſine is to the Radius as c to r: Where, if in the Place of c there be ſubſtituted the Coſines of 51° : 32′ and 18° : 0′, that is of the Latitudes of *London* and *Jamaica*, we ſhall have the Force of Gravity at the former to that at the latter, as $a - ,3870\,d$ to $a - ,9045\,d$, that is as 1106 to 1103. Whence the mean Diameter of the Earth will be to the Difference of the Axis and equatoreal Diameter, as 191 to Unity; and thence (by Article 4.) as the mean Gravity on the Surface to the centrifugal Force at the Equator, ſo is 191 to $\frac{4}{5}$, or ſo is 239 to Unity. In order to ſhew that this cannot be, I ſhall obſerve, that when the Moon's Diſtance was ſuppoſed 60 Semidiameters of the Earth (as in Article 5.) it was found that the mean Force of Gravity was to the centrifugal Force at the Equator, as 289 to 1. But if the Proportion now found be true, the Moon's Diſtance of 60 Semidiameters muſt be augmented in the ſubtriplicate Proportion of 289 to 239, and then it will

* See Chap. V. §. III.

become

become 64 Semidiameters. In the like manner, if we compute the Ratio of the mean Force of Gravity to the centrifugal Force, by presupposing the Magnitude of the Earth, as Sir *I. Newton* and Mr *Huygens* did, we must suppose a Degree to be above 80 *English* Miles to bring it out 239 to Unity. Now whereas it is certain that the Distance of the Moon is about 60 Semidiameters of the Earth, and that a Degree is less than 70 *English* Miles; therefore, I say, that the Conclusion which seems to follow from the *Jamaica* Experiment, cannot be allowed to be true. And the Experiments made by *Richer*, in the Island of *Cayenna*, would still make a greater Difference betwixt the Diameters of the Earth, than those made in *Jamaica*. And the Lengths of the *Paris* and *London* Pendulums compared together, would make it greater than one 231 Part of the Whole, as it was found in Article 5.

8. From all the Experiments made with Pendulums, it appears that the Theory makes them longer in Islands, than they are found in Fact. The *London* Pendulum should be longer when compared to the *Paris* one, than it really is: The *Jamaica* Pendulum, when compared to the *London* one, which vibrates in a greater Island, should be longer than is found by Experience; and the Pendulum in *Cayenna* (a smaller Island than *Jamaica*) should still be longer. This Defect of Gravity in Islands is very probably occasioned by the Vicinity of a great Quantity of Water, which being specifically lighter than Land, attracts less in Proportion to it's Bulk. And I find by Computation, that the Odds in the Pendulums betwixt Theory and Practice is not greater than what may be accounted for on that Supposition. I shall also observe, that although the Matter of the Earth were entirely uniform, yet the Hypothesis of it's being a true Spheroid is not near enough the Truth to give the Number of Vibrations which a Pendulum makes in 24 Hours. And suppose the true Figure were known, the Inequalities of Mountains and Vallies, Land and Water, Heat and Cold, would never allow Theory and Experiments to agree. But after the *French* Gentlemen who are now about measuring a Degree, and making Experiments with Pendulums in the North and South, shall have finished their Design, we may expect new Light in this Matter.

Some Investigations, by which it is proved, that the Figure of the Earth must very nearly approach to an Ellipsis, according to the Laws of Attraction, in an inverse Ratio of the Square of the Distances,

II. According to Sir *I. Newton*'s *Principia (Cor. 3. Prop.* XCI. *Lib.* 1. and *Prop.* XIX. *Lib.* 3.) if an elliptic Spheroid, consisting of fluid and homogenous Particles mutually attracting each other, in an inverse *Ratio* of the Square of the Distances be revolved round it's *Axis* A *a*, that the Columns C E, C N, C A, of which that Spheroid is composed, may be placed *in Æquilibrio*, and so the Spheroid may always have the same Figure, the Gravity in any Point of the Surface N must necessarily be in an inverse *Ratio* of the *Radius* C N..

That we may know therefore, whether the Spheroid has this Property, let us now seek what Attraction is suffered by every Corpuscle N, of the whole Spheroid according to the Direction C N; and from that Attraction let us take that Part of the centrifugal Force, which proceeds

from

from the Rotation of the Spheroid acting according to C N, and let us seek whether the remaining Force is proportional to $\frac{1}{CN}$. Therefore we will first investigate the following; and as our Intention is to apply our Discoveries to the Spheroid of the Earth, which all agree to be very little different from a Sphere, our Computations must be adapted to those Spheroids, the greater *Axis* of which exceeds the lesser by the very smallest Quantity.

by M. Alexin Clairaut, F.R.S. and R. Acad. Sc. Paris. No. 445. p. 19. Jan. &c. 1737.

Prob. I. *To find the Attraction, which the Spheroid* A E *a e*, differing very little from a Sphere, exercises on a Corpuscle situated at the Pole A.

Fig. 106.

For the Solution of this Problem we should repeat; *Cor.* 2. *Prop.* 91. *Newt. Princip.* by which you may learn the manner of finding the Attraction of any Spheroid, if you substitute in the general Value for C E the Quantity which differs infinitely little from A C; but as in that case the Problem comes out much easier, we shall solve it after the following manner.

Let A M D *a d* be a Sphere, of which the *Radius* is A C: We will seek the Attraction of the Space which rises from the Revolution A D *a* E, which Attraction, being added to the Attraction of the Sphere, gives the Attraction sought.

To find the Attraction of the Space arising from the Revolution A N E *a* D M, let A C be r, D E, αr, A P, u, then from the Nature of the Ellipse N M $= \alpha \sqrt{2ru - uu}$; but from the Nature of the Circle A M $= \sqrt{2ru}$. But the Space arising from the Revolution N *n m* M will be $\frac{\alpha c}{r}\ 2\,\overline{ru - uu}\,.\,du$, for c is the Circumference, and r the *Radius.*

Because of the Smallness of N M, we may account all the Particles of Matter contained in that Space as equally attracting the Corpuscle in A: wherefore you will make but little account of the Attraction of that Space, if you multiply it's Solidity by the Attraction in M. But that Attraction in M ought to be $\frac{1}{AM^2} \times \frac{AP}{AM}$. You will therefore have analytically $\frac{u}{2ru\sqrt{2ru}} \cdot \frac{\alpha c}{r} \cdot 2\,\overline{ru - uu}\,.\,du = \frac{\alpha c}{2rr\sqrt{2r}}$ $(2rdu\sqrt{u} - udu\sqrt{u})$ of which the Integral $\frac{\alpha c}{2rr\sqrt{2r}}$ $\left(\frac{4}{3}ru\sqrt{u} - \frac{2}{5}uu\sqrt{u}\right)$ is the Attraction of the Space arising from the Revolution A N M. In which Value, if you make $u = 2r$, you will have by Reduction $\frac{8}{15}c\,\alpha$; whence the Attraction of the whole Space A E a C is expressed, and by adding afterwards $\frac{2}{3}c$ for the Attraction of the whole Sphere, you will have $\frac{2}{3}c + \frac{11}{15}c\,\alpha$, the Attraction of the Ellipsoid.

Coroll. If you would have an oblong Spheroid, α will be negative, but the Sum of the Attraction will be $\frac{2}{3}c - \frac{8}{15}c\,\alpha$.

Note. If the above Spheroid, instead of circular Elements arising in P N, consisted of other Elements, for Instance, Elliptical, which should differ from a Circle no more than the Ellipsis A E, and should have the same Surface as the Circles P N, the Attraction would manifestly be always the same, because in those Elements P N, whatsoever the remaining Force should be, the Circles P M being taken away, it would be as it were composed of Parts which would have the same Attraction as upon that of the Ellipsoid, having regard to the Smallness of N M, and the Quantity of equable Matter.

Lemma. Fig. 107. Let K L be a Circle, H the Centre of the Circle, V H a Perpendicular in the Area of the Circle, and N H a Line equal to the Perpendicular V H, which shall make therewith an Angle infinitely small or very small, I say that the Attraction of the Circle K L in N, may be taken without any sensible Error as the Attraction of the Circle in V, or, which is the same thing, that one Attraction does not differ from the other but by a Quantity infinitely less with respect to both, than V N is less in respect to H V.

To demonstrate which Proposition, it must be shewn, that, 2 Corpuscles being placed at the extremity of any Diameter K L, there is one attractive Force in N, and another Force in V, of which the Sum may be reckoned the same. But neglecting the Computation to have the Attraction of a Body placed in K to the Corpuscle N, you may easily see, that it will be the same with the Attraction in V, to which a small Quantity should be added, which N V should enter. In like manner also you may see, that the Attraction of the Body placed in L to the Corpuscle N will be the same with the Attraction in V, taking away the same small Quantity. Therefore the Sum of both these Attractions is one and the same.

Coroll. If instead of the Circle K L there was a certain *Ellipsis*, or any other curve Line, which should differ very little from a Circle, by the same Arguments, which were used in the *Note*, it is easily gathered that there would always be place for the foregoing Proposition.

Let A E a e be an Elliptic Spheroid, of which let A a be the *Axis* of Revolution. I ſay that the Attraction, which this Spheroid exerciſes to the Corpuſcle placed in N, is the ſame with that Attraction, which every Spheroid exerciſes, whoſe Pole ſhould be N, Axis of Revolution N n, and ſecond *Axis* the *Radius* of a Circle, which ſhould have the ſame *Superficies* as the *Ellipſis* F G, a Section of the *Ellipſoid* A E a e thro' a Plane erected perpendicularly on F G, it's conjugate Diameter. Theorem I. Fig. 108.

To Demonſtrate this, imagine innumerable Elements K L, parallel to the *Ellipſis* F G, that is, all erected upon Ordinates to the Diameter. It is evident, that the Spheroid A E a e will differ from the aforeſaid Spheroid only in this, that in the firſt all the Elements make an Angle with C N differing from a right Angle by an Angle infinitely ſmall, but in the ſecond all the Elements make a right Angle without any Difference, whereas in both Spheroids the Elements have the ſame *Superficies*. But, by the preceding Propoſition, the Attraction of every Element K L to N is thought in a manner the ſame in both Caſes; but as for the Thickneſs of the Elements, K k l L, we may take H b for the Perpendicular b i, becauſe of the Smallneſs of the Angle i b H; therefore the total Attraction of both Spheroids may be taken one in the Room of the other.

To find the Attraction of the Spheroid A E a e, *to a Corpuſcle placed in any Point* N. Prob. II.

Let A C $= a$, CE $= b$, C N $= r$, C G the conjugate Diameter, C N will be $\frac{ab}{r}$ (ſince a and b differ very little between themſelves) we muſt (by the preceding Propoſition) ſeek the Attraction of the Spheroid, whoſe greater *Axis* is r, and leſſer $\sqrt{\frac{abb}{r}}$, or $b\sqrt{\frac{a}{r}}$.

To this we muſt apply the *Formula* which we found in *Prob.* I. $\frac{2}{3} c - \frac{8}{15} c\,\alpha$, or $\frac{2}{3} p\,r - \frac{8}{15} p\,r\,\alpha$ (putting $p\,r$ for c) but inſtead of α in this Formula, we muſt ſubſtitute $\frac{r - b\sqrt{\frac{a}{r}}}{r} = 1 - \frac{b}{r}\sqrt{\frac{a}{r}}$, or $\frac{3}{2} n - m$, if you put $a + m\,a$ for b $a + n\,a$ for r, and in the Computation neglect the ſecond Degrees of the Magnitudes n and m.

If therefore you put $\frac{3}{2} n - m$ in the place of α, the aforesaid *Formula* will become $\frac{2}{3} p r - \frac{4}{5} p r n + \frac{8}{15} p r m$, or $\frac{2}{3} p a - \frac{2}{15} p a n + \frac{8}{15} p a m$; which is the Expression of the sought Attraction of the Spheroid in N.

If $n = 0$, then you may have $\frac{2}{3} p a + \frac{8}{15} p a m$ for the Attraction in a, that is, to the Pole.

But if $n = m$, then you may have $\frac{2}{3} p a + \frac{6}{15} p a m$ for the Attraction to the Equator.

Theorem II. Fig. 106. Let A E a e be a Spheroid as above, whose *Axis* differs by a very small Quantity, which, for the greater Perspicuity, I shall call infinitely small. If this Spheroid is conceived to be of a fluid and homogenous Matter, and turned about the *Axis* A a, in a congruent Time, that the Gravity of the Column C E may be equal to the Gravity of the Column A C, that is, by Sir *I. Newton*'s Principles, the Attraction in E, the Centrifugal Force being taken away, may be to the Attraction in A, as C A to C E: I say, that all the Columns C N, wanting an infinitely small of the second Order, will preserve an *Æqilibrium* with those 2 Columns; that is, the Attraction in N, taking away the centrifugal Force made simple according to C N, is to the Attraction in A as C A to C N.

Let the same Denominations be preserved for the Demonstration, which were used in the preceding Proposition; first let the centrifugal Force in E be sought, which may agree with the *Æquilibrium* of the Columns C E, C A.

Therefore say $\frac{2}{3} p a + \frac{6}{15} p a m - f : \frac{2}{3} p a + \frac{8}{15} p a m :: 1 : 1 + m$, whence is drawn $f = \frac{8}{15} p a m$.

Then to apply the Gravity in N compounded of the Attraction, taking away the centrifugal Force, the centrifugal Force in N is to be sought, or, which is the same thing, in M above the Sphere, because they ought to differ from each other only by an infinitely small of the second Order, if D E is supposed to express the centrifugal Force f in E, M N will express the centrifugal Force in N, but the centrifugal Forces are as *Radii*, when the Times of Revolutions are the same, but by the property of the *Ellipsis* it becomes as D E : N M :: C E : M P.

But if the centrifugal Force acts according to N P, it must be reduced according to N C, and N O will be the remaining Part. Therefore the

the centrifugal Force in N or in M is to the centrifugal Force in E or in D, as N O is to D E. Therefore the Expression of the centrifugal Force in N will be $\frac{8}{15} p a n$, and consequently the Expression of the Gravity will be $\frac{2}{3} p a - \frac{2}{15} p a n + \frac{8}{15} p a m - \frac{8}{15} p n a$, or $\frac{2}{3} p a - \frac{2}{3} p n a + \frac{8}{15} p a m$.

Now to find the centrifugal Force in N, which follows from the *Æquilibrium* of the Columns, the Gravity in A must be to the Gravity in N, as N C to A C, the Gravity in A is $\frac{2}{3} p a + \frac{8}{15} p a m$, which Expression being drawn into $\frac{1}{1 + n}$ or $1 - n$, after Reduction will become $\frac{2}{3} p a - \frac{2}{3} p n + \frac{8}{15} p a m$, and is the same Expression with that above. Thence we may see, that there can be but an infinitely small Difference between the Figure which the Earth ought to have by the Newtonian *Hypothesis*, and the Ellipsoid. For as the Quantity D E is about $\frac{1}{230}$ Part of A C, in the preceding Computation, we neglect only the Quantities of the same Order with $\frac{1}{230 \cdot 230^2}$.

An Account by John Eames, *F. R. S. of a Dissertation, containing Remarks upon the Observations made in* France, *in order to ascertain the Figure of the Earth, by Mr* Celsius, *intituled,* De Observationibus pro Figura Telluris determinanda, in *Gallia* habitis, Disquisitio. Auctore *Andrea Celsio,* in

III. 1. That the Figure of the Earth is Spheroidical is agreed upon by all: But whether it be an oblong or oblate Spheroid, *i. e.* whether the Axis be longer or shorter than a Diameter at the Equator, has been for some time a matter of Doubt. Three several Methods have been proposed to determine this Controversy by Experiments; as by the different Lengths of Pendulums vibrating Seconds, in different Latitudes; the Figure of the Earth's Shadow in Lunar Eclipses; and by the actual Measurement of the Lengths of a Degree on the Meridian in different Latitudes.

It is certain, if the Lengths of the Degrees of Latitude decrease as we go from the Equator toward the Poles, then the Axis is greater, and the Figure an oblong Spheroid; but, on the contrary, if these Lengths increase as you remove towards the Poles, the Axis is less than a Diameter at the Equator, and consequently an oblate Spheroid.

M. *Cassini* and others, judge the Earth to be of an oblong Spheroidical Figure; and the Observations made in *France*, if entirely to be depended upon, prove this *Hypothesis* to be a Matter of Fact. Our late illustrious President, Sir *Isaac Newton*, Mr *Huygens*, and others, make the Earth

to

Acad. *Upsal.* Astronom. Prof. Reg. &c. *Upsaliæ*, 1738. 4to. No. 457. p. 371. *July*, &c. 1740.

to be an oblate Spheroid, higher at the Equator than at the Poles; and this Figure of the Earth is undoubtedly the true one, if the Observations lately made near the Arctic Circle be admitted as certain and exact. So that since both Sets of Observations have been taken by Persons of known Skill, Dexterity, and Integrity, it is now become absolutely necessary to inquire into this Matter, in order to find out the Occasion of so great a Difference in their Conclusions.

Mr *Celsius*, in the Treatise before us, proposes to consider this Matter more closely, and begins with a Defence of the Observations made at *Tornea*, near the North Polar Circle; and then takes Notice of some Things, proper to be considered, relating to the Instruments, Astronomical Observations, and Trigonometrical Operations, performed in *France*; which, in his Judgment, render the Observations uncertain; at least so far as not to be accurate enough to be depended upon in determining the Matter in Question.

To begin with the Defence of the Observations made at *Tornea*: Perhaps it may not be improper to premise a short Account of them. They were undertaken at the Charge of the King of *France*, by 5 skilful Gentlemen; 3 of them Members of the *Royal Academy* at *Paris*, who were joined by Mr *Celsius*, and the Abbé *Authier*. The Trigonometrical Part of the Work was performed near the River of *Tornea*, whose Direction is the same with the Meridian of *Tornea*; the Coasts of the Gulph of *Bothnia* being found very inconvenient for that Purpose. By the favourable Situation of 5 Mountains they formed 8 Triangles, which took in Space enough for their Design. All the 5 Gentlemen observed, one after another, each Angle of these Triangles, setting them down in writing separately.

They afterwards determined the Distance between *Tornea* and Mount *Kittis*, under the same Meridian, by a Basis, measured on the River when frozen over, whose Length was 7406 Toises 5 Feet, by the first Measurement; and when measured again, was barely 4 Inches over. This Distance between them they found to be 55,234 Toises.

The first Part of their Work being thus finished, the next was to find the Difference of Latitude of these two Places: This they did by the Help of a Telescope, fixed to a Sector of 9 Foot, made at *London*, by the Care and Direction of Mr *George Graham*. The Star they observed at *Tornea* was α *Draconis*: They repeated their Observations 3 Times, and the greatest Difference between them was but $2''$: Removing to Mount *Kittis*, they took the same Number of Observations, of the same Star, without finding more than $1''$ Difference. The Result was, that the Amplitude of the Arch, in the Heavens, between *Tornea* and Mount *Kittis*, (allowing for the Precession of the Equinox, and the Time elapsed between the 2 Observations, according to Mr *Bradley*'s Theory) was $57' \, 26''$. Hence the Magnitude of a Degree, on the Earth, intersecting the Polar Circle, was found to be greater than a mean Degree of *France* 377 Toises; and to differ 900 Toises from

from what it ſhould have been, according to M. *Caſſini*'s Hypotheſis: And if the Correction, according to Mr *Bradley*'s Theory, were omitted, the Difference would have amounted to above 1000 Toiſes: The Conſequence of which, ſay the curious Obſervers, is, That the Earth is not only flatted towards the Poles, but that it is much more ſo than Sir *I. Newton* or M. *Huygens* thought it. This unexpected Difference being ſo very great, made them reſolve upon a careful as well as new kind of Verification of the Whole. In the firſt Place, they repeated their Aſtronomical Obſervations 3 ſeveral Times, at *Tornea* and *Kittis*, with the ſame Inſtrument, but on another Star, *viz.* δ *Draconis*: The Difference of Latitude between the 2 Places was found to be the ſame, within $3\frac{1}{2}''$, with the Firſt. They then not only examined the Truth of their Meridian Line, the Exactneſs of the Sector, in the different Diviſions upon the Limb, chiefly in the 2° imployed in obſerving α & δ *Draconis*, but ſuppoſed that, in their Trigonometrical Operations, they had erred in each Triangle, by $20''$ in each of the 2 Angles, and $40''$ in the Third; and that all theſe Errors tended to diminiſh the Length of the Arch; the Calculation, upon this Suppoſition, gives but $44\frac{1}{10}$ Toiſes for the greateſt Error that could be committed.

When a particular Relation of all theſe Obſervations was read before the *Royal Academy of Sciences* at *Paris*, and inquired into; the main Exception taken to them was, That the Obſervers, omitting to make a Proof of the Line of Collination, by Means of double Obſervations, with the Face of their Inſtrument turned contrary Ways, have thereby not duly aſcertained the Truth of their Obſervations. But this Objection was fully anſwered by M. *Maupertuis*, as Mr *Celſius* hopes and believes, to the entire Satisfaction of M. *Caſſini*, who made it. He allows M. *Caſſini* had very good Reaſon to mention this, as a Thing proper to be done in Inſtruments of common Uſe for this Purpoſe, which generally ſtand in Need of ſuch a Method of Verification: But it was not at all neceſſary in the Inſtrument uſed at *Tornea* and Mount *Kittis*: The very Make of it was ſuch, that no Alteration could eaſily be made in it, ſo as to create any perceptible Error in the Obſervations. The whole Apparatus of the Teleſcope and Sector is all framed together; the Object-glaſs and Croſs-wires, as well as the Limb, ſo firmly fixed to the Tube, as not to be diſlocated without great Violence. Notwithſtanding all this, the utmoſt Care was taken in tranſporting it from one Place to another; being placed in a Cheſt, that the *Laplanders*, to uſe his own Words, *in illa ciſta idolum quoddam ſervari facile ſibi perſuaderent.* He adds, the ſame Objection may be made to M. *Picard*'s Obſervations, who does not ſeem to have uſed this Precaution, as M. *Caſſini* himſelf acknowledges, who nevertheleſs approves and extols his Obſervations for their Accuracy: So that thoſe at the Arctic Circle may be very good, notwithſtanding the Want of this, ſuppoſed neceſſary, Operation. And indeed, that they were ſo, ſufficiently appears from this Fact: The Difference of Latitude between *Tornea* and Mount *Kittis*, found in

September, was obſerved again in *March* following, by the Help of the ſame Star δ *Draconis*, and did not differ from the former above $3\frac{1}{2}''$, though the Inſtrument had been twice carried from one Place to the other. This is a Degree of Exactneſs not eaſy to be met with; no not in M. *Caſſini*'s Obſervations, made on different Stars, which differ ſometimes $40''$, in determining the Amplitude of an Arc in the Heavens, though their Inſtrument was carefully examined in the Way abovementioned.

The Author then proceeds, in his Turn, to inquire into the Accuracy and Certainty of the two Sets of Obſervations made in the North and South Parts of *France*, in reſpect of the Royal Obſervatory at *Paris*.

As to the Meaſures of the Degrees in the Northern Parts of *France*, between *Paris* and *Dunkirk*, he owns they cannot be much out of the Way; being in ſome Meaſure confirmed by M. *De la Hire*, in the Year 1683, and M. *Caſſini* himſelf. Yet Mr *Celſius* obſerves, that the Baſis on the ſandy plain Shore, near *Dunkirk*, when meaſured again, differed 3 Feet from the former Meaſurement; which is a much greater Difference than that Mr *Celſius* and the other Gentlemen found, in meaſuring a much longer Line twice over, which was but 4 Inches.

As to the Aſtronomical Obſervations taken by the 6 Foot Sector, whoſe Limb of $12°$ was divided only at every $20''$; it is true, M. *Caſſini* examined the Inſtrument ſeveral Ways at *Paris*, after his Return thither: but that a Correction, owing to the Change of Centre, might be ſafely applied to the Obſervations at *Dunkirk*, the Examen of the Centre ſhould alſo have been taken at *Dunkirk*; it being uncertain, whether this Alteration or Aberration of the Centre was cauſed by the Journey to or from *Dunkirk*.

The Difference of $41''$ between the Obſervations taken to ſettle the true Meaſure of the Arc of the Heavens, ſeems to be enormous. Perhaps the Stars were not lucid enough to be well obſerved by the 3 Foot Tube; but might they not, for a due Degree of Accuracy, have been viewed through the 9 or 10 Foot Teleſcope?

Our Author prefers the Obſervations of 1719, made after the Return to *Paris*, to thoſe made before; becauſe made at the ſame Time of the Year with thoſe of *Dunkirk*, and ſo not ſtanding in Need of Mr *Bradley*'s Correction: Though this Caution, perhaps, may be thought not neceſſary here, where the Errors of the Obſervations are greater than the Correction itſelf. Mr *Celſius* remarks farther, if the Difference of Latitude between *Dunkirk* and *Paris* be ſuppoſed to be $2° \, 12' \, 12\frac{1}{2}''$, which is a Mean between 4 others he mentions, the Length of a Degree will amount to but 56,395 Toiſes. And if the Obſervations at *Malvoiſine* and *Amiens* be counted, according to Mr *Bradley*'s Theory, for the Interval of a Month between the Obſervations, the Length of a Degree will come out to be 56,926 Toiſes; which is 135 Toiſes leſs than the Length of a Degree, found by meaſuring the whole Length of of *France*; and 134 lefs than that of Mr *Picard*, fo highly approved of by M. *Caffini*, as confirming his own.

2. Mr *Celfius* having finifhed his Remarks upon the Obfervations made in the North Part of *France*, extending from *Paris* to *Dunkirk*, proceeds to examine thofe taken in the South, from *Paris* to *Collioure*, near the Borders of *Spain*, and the *Pyrenean* Mountains. By the former, a mean Degree was found to confift of 56.960 Toifes, by the latter 57,097; and confequently the Earth is an oblong Spheroid. *The fame continued.* Ibid. p. 378.

Mr *Celfius*, in examining thefe Obfervations, which were taken under the Conduct and Direction of the late M. *Caffini* in 1700, firft confiders the Structure and Goodnefs of the Inftruments ufed; then the Accuracy of the Aftronomical Obfervations for finding the Difference of Latitude; and, in the laft Place, the Trigonometrical Operations for determining the Diftances of Places; efpecially the two Extremes under the fame Meridian.

The principal Inftrument M. *Caffini* carried with him, was, a Limb of 12°, whofe Radius was indeed 10 Foot, but divided only into Degrees and Minutes; the other Parts were added to it at *Perpignan.* Here Mr *Celfius* obferves, that the finding the true Centre of this Limb was and ftill is a very difficult and troublefome Problem to a good Artift; that no Mention is made, whether the Pofition or Place of this Centre, and the Divifions of the Limb, were ever examined at *Paris* or *Collioure*, though the Carriage of the Inftrument through fo long and rough a Way, could not but make fome Alteration in the Place of the Centre.

It is true, the Zenith Diftance of *Capella*, taken by it at *Paris*, was confirmed to be right by another Inftrument; but it cannot be concluded, that the Zenith Diftance of the fame Star, taken at *Collioure* by this Inftrument, and not confirmed there by another Inftrument, muft be true alfo. For the Point of Divifion, anfwering to this Diftance in the Limb, was not examined; and a Centre wrong placed may by Accident give the true Zenith Diftance, *viz.* when the true and erroneous Centre happens to lie in the fame Perpendicular to the Horizon.

The Exceptions taken to the Aftronomical Obfervations for finding the Difference of Latitude between *Paris* and *Collioure*, are, in the firft Place, That though 5 Stars were obferved at *Collioure* and *Paris*, yet 1 only was made ufe of, *viz. Capella*: That the Difference of Latitude by *Capella* is 6° 18′ 57″: If *Lucida Lyra* had been ufed, the Difference would have been but 6° 17′ 7″; but by the Right Shoulder of *Auriga*, 6° 19′ 25″: Hence arifes the Uncertainty or Difference of 2′ 18″ between the greateft and leaft of their Obfervations: That the late M. *Caffini* makes the Difference 57″ lefs than M. *Caffini*, who accounts for this Difference from the Obfervations being taken by an ordinary Inftrument; but the Inftrument is the fame which was ufed to take the Altitude of the Pole of *Amiens*, which was very near that found by Mr *Picard*.

As to the Trigonometrical Operations for finding the Diſtance of Places, Mr *Celſius* thinks they labour under conſiderable Uncertainties; not only on the Account of the many Difficulties they met withal, *viz.* mountainous Countries, want of proper Signals, *&c.* ſo that convenient Triangles could not be formed; but add to all theſe, ſeveral of the Triangles had but Two Angles obſerved, and ſome of theſe Angles too acute; whence, as M. *Caſſini* himſelf very juſtly obſerves, in his Examination of *Snellius* and *Riccioli*'s Obſervations, great Errors may ariſe. M. *Picard* thinks all Angles leſs than 20 Degrees ought to be avoided; as alſo that the Triangles ſhould be contrived ſo as to have Sides of a due Length, neither too great nor too ſmall: Then follow 16 Triangles, wherein one or more of theſe Inconveniencies are to be found.

It may be ſaid, the Whole of theſe Obſervations and Meaſures of M. *Caſſini* ſeem to be ſufficiently confirmed, if not aſcertained; ſince the principal Baſe in *Rouſſillon* was found, when computed, to differ but Three Toiſes from the ſame as it was actually meaſured; and that, after ſome due Corrections, it was made to agree with the greateſt Exactneſs. Mr *Celſius* replies, Why are we not told what thoſe Corrections were, that we may ſee whether they were really neceſſary or no? Why were they not taken Notice of in the Calculations of each Triangle? Beſides, the real Length of the Baſe, or the fundamental Line, in *Rouſſillon*, is not fully aſcertained, it not being meaſured more than once; whereas that at *Dunkirk* and that of M. *Picard* were meaſured twice; and there was more Reaſon for doing ſo here than at *Dunkirk*, on account of the uneven and almoſt ever changing Shore in *Rouſſillon*, from the reſtleſs overflowing Sea.

The great Number of the Triangles, joined with the numerous ſmall Errors of the Angles, is another Ground of Uncertainty; for the Errors in the Angles, though ſmall, may make the Diſtance of the Parallels of the 2 extreme Places greater than it ought to be; and yet the principal Sides, that is, thoſe that are made Baſes to the following Triangles, continue the ſame. This made it neceſſary to verify the Sides, at leaſt at every ſecond Degree, by meaſuring the principal Baſe twice over with due Care; which might have been done, and therefore ſhould have been done, in a Matter of ſo much Nicety as an Attempt to find the Difference between Two Degrees ſo near one another, under the ſame Meridian.

To ſhew what bad Conſequences may ariſe from ſmall Errors committed in obſerving the Angles of ſeveral Triangles, Mr *Olavus Hiorter*, a curious and ingenious Friend of Mr *Celſius*, has taken the Pains to form the Triangles of M. *Caſſini* between *Bourges* and *Collioure*; ſo that the Diſtance between their Parallels ſhall be conſiderably leſſened; and yet the Baſe in *Rouſſillon*, found by Computation, ſhall not, after due Correction, differ ſenſibly, if at all, from the ſame actually meaſured. In conſequence of this, Mr *Celſius* concludes with obſerving, that

that the Diſtance between the Royal Obſervatory and the Perpendicular to the Meridian of *Collioure*, deduced from the Triangles of *Caſſini*, corrected after Mr *Hiorter*'s Method, *&c.* will amount to but 358,980 Toiſes. This, divided by the mean Difference of their Latitudes, 6° 19′ 11″, will give 56,803 Toiſes, for the Length of a Degree, one with another, between *Paris* and *Collioure*, which is leſs than the Length of a mean Degree found by M. *Picard*, and pretty near the Truth: So that the Degrees decreaſe as you go towards the Equator; and conſequently the Earth is higher at the Equator than at the Poles, as Sir *I. Newton* and Mr *Huygens* believed.

The Diſtance of the Parallels of *Paris* and *Collioure* by this Method is indeed leſs than that computed by M. *Caſſini*; but this cannot reaſonably be complained of, ſince theſe computed Meaſures of M. *Caſſini* ſeem very capable of being leſſened; and it is no more than what M. *Caſſini* himſelf hath done to the Meaſures publiſhed by his Father, which he has ſhortened by 325½ Toiſes. But however that Matter be, whether this particular Correction of M. *Caſſini*'s Diſtance, and, conſequently, Length of a mean Degree, be admitted or no, Mr *Celſius* is fully perſuaded, upon the Whole, that he hath made it plain to every unprejudiced Reader, that theſe Two Sets of Obſervations in *France* are not taken with ſuch a Degree of Exactneſs as to be depended upon, in determining ſo nice a Matter, in Diſpute for 50 Years, as the true Figure of the Earth; which was the Thing propoſed to be done by them.

Concerning a Place in New-York *for meaſuring a Degree of Latitude, by Mr* J. Alexander. *Ibid. p.* 453.

IV. The Mention of the *French* Endeavours to diſcover the Figure of the Earth by Obſervation, puts me in Mind, That a very exact Obſervation for that Purpoſe might be made here, becauſe *Hudſon*'s River here is frozen over from *New-York* up to *Albany*, and it's Courſe is very ſtrait, almoſt true North, and the Diſtance between *New-York* and *Albany* is above One hundred and Fifty Miles; *New-York* is in Latitude of 40° 40′, nearly; ſo that the Length of above 2 Degrees of Latitude on the Earth might be meaſured here, with much more Exactneſs than it was poſſible in *England* or *France*, becauſe of the Aſcents and Deſcents, and curved Lines, which, I think, they would continually be obliged to make Allowances for.

From all which Difficulties the Menſuration here on the Ice would be entirely clear.

A Propoſal for the Meaſurement of the Earth in Ruſſia, *read at a Meeting of the Academy of Sciences of* St Peterſbourg, Jan. 21, 1737, *by Mr* Joſ. Nic. de L'Iſle, *firſt Prof. Aſtron.*

V. Neceſſity, or the Exigencies of Geography and Navigation, put Mankind very early upon the Enterprize of meaſuring the Earth. For how is it poſſible to conſtruct the Charts of each Kingdom or Empire, without ſetting down all the Places in their true Diſtances, by the Meaſures made uſe of in each Country: Such as were the *Stadia* of the Ancients, and ſuch as are our Miles, Leagues, Werſts, *&c.* And how could different States be compared with one another, ſo as to come at the Knowledge of the Spaces they ſeverally occupy on the Earth's Surface, without knowing the Number of theſe common Meaſures contained in a Degree, or in the whole Extent of the Earth? Hence proceeded the *and F. R. S. Translated from the* French *printed at* St Petersbourgh, 1737. 4to. *By* T. S. *M. D. F. R. S.* No. 445. p. 27. Jan. &c. 1737.

the twofold Method of determining the Situation of the different Parts of the Earth, either by their mutual Distances set down in the Measures made use of in each Country, or expressed in Measures common to all, as Degrees, Minutes, and Seconds, by marking the Longitude and Latitude of each Place.

Upon the first Determination of the Magnitude of the Earth in Geographical Measures, as in *Stadia* and *Arabian* Miles, the Ancients did not employ any great Degree of Exactitude. They were content to set down the Circumference of the Earth, and of it's Parts, in round Numbers; probably, because they did not expect to be able to attain much Preciseness in a Research of this Nature. But according as their Desires of improving Geography increased, by entering into a Detail of it, they found it necessary to have a more exact Knowledge of the Magnitude of each Degree, not only in great Measures, as in Miles and Leagues, but also in Perches, Toises, and Feet; which could not be done otherwise than by Geometrical Operations and Astronomical Observations, more exact, and consequently more operose, than had been, or indeed could have been, undertaken before.

I shall not enter here upon a Detail of the immense Labours of modern Mathematicians on this Head, as those of *Fernel* in *France*; of *Snellius*, *Blaeu*, and *Musschenbroek* in *Holland*; *Norwood* in *England*; Father *Riccioli*, and lately *Monsignor Bianchini* in *Italy*; and the Gentlemen of the *Academy of Sciences* in *France*; to get only the precise Magnitude of a Degree in the Measures of their respective Countries. But I will answer an Objection which might be raised hereon, *viz.* That it was needless to undertake these same Operations in so many different Places, since the Magnitude of a Degree once determined in the Measures of any one Country, may be easily reduced to the Measures of any other, by the exact Knowledge we now have of the Proportions of modern Measures. Whence it might be inferred, that after all the Exactness which the Astronomers of the *Royal Academy of Sciences* of *Paris* have obtained by their Labours, in drawing their Meridian from one Sea to the other, it is unnecessary to enter upon a new Undertaking of the same Thing any where else: Since, in order to reap the Advantage of that Work for the Geography of each particular Country, nothing more is requisite than exactly to compare the Measures of those Countries, with those made use of by the *French* Astronomers in their Operations and Calculations.

Now, taking *Russia* for the Example, the Geographical Measures of which are *Wersts*, divided each into 500 *Sagenes*, and each *Sagene* supposed to be exactly seven Feet *English*; this Relation once known, as also the exact Relation of the *English* to the *French* Foot, or to the Toise of six Feet, which the *French* Astronomers employed in their Measurements, and of which they found a Degree of a great Circle contained 57060; what more is requisite for concluding that a Degree of a great Circle contains $104\frac{1}{2}$ *Wersts?* And what remains towards the Perfection

fection of the Geography of *Russia*, in the most minute Detail that can be entered upon, but to employ this Measure of *Wersts*, *Sagenes*, and *English* Feet, (if you please) in actual Measurements; and to construct the Charts by the most exact Methods of Geometry; taking care to set them down right, as to their true Bearings, and to regulate them by the most exact Astronomical Observations of Longitude and Latitude that can possibly be made.

It must be confessed, we should be very happy, if in the Geography of *Russia* we were arrived at this Pitch; not only in the general Map, but likewise in that of any particular District whatsoever, the nearest and of most Concern to us. But besides that we are as yet far from pretending to this; I will now make appear that it is not possible to attain it, without undertaking an equal and even a greater Work than all that has been hitherto done in *France* and elsewhere, towards the Measurement of the Earth. I am myself affrighted at the very Thought of what I propose, and am under Apprehensions that it will give the same Pain to those of the Company, who know, as well as I, the prodigious Labour in which this Work must engage the Undertakers. But what is not a Person capable of undertaking for the Glory and Interest of her Imperial Majesty, when excited by the Benefits she heaps on the Academy, and by the singular Protection her Ministers grant to this Body and the Sciences therein cultivated! Sufficient Motives for undertaking Matters of the utmost Difficulty.

When I said above, that an exact Knowledge of the Magnitude of a Degree of the Earth in any known Measures of one Country was sufficient for constructing exact Charts of all other Countries, only having a Regard to the different Proportion of the Measures; that is to be understood upon a Supposition of the Earth's being perfectly spherical: Seeing it is well known, that in a Sphere the Degrees of all the great Circles are every where equal; and that we likewise know, in a Sphere, the Proportion of the Degrees of the small Circles to their great Parallels, according to their Distance from them.

But if the Earth be not perfectly spherical, the Case is quite altered: All the Degrees of the great Circles will not be equal to one another; and those of the small Circles, taken at a certain Distance from their parallel great Circles, will not have the same Relation that the Degrees of the small Circles, taken at the same Distance, would have on a Sphere. In all this there might possibly arise an infinite Variety, according to the Figure the Earth might have; and as it is not yet decided what is the Earth's true Figure, and that there is no better Method of ascertaining it than by Observations made in so great an Extent as that of *Russia*: For these Reasons I have advanced, that the Perfection of the Geography of *Russia* stands in Need of this great Undertaking; which, besides the Usefulness of it, will acquire much Honour to the Academy of *Petersburgh*; if that Body can, by Means of this Work, contribute towards the deciding the celebrated Question of the Earth's

Figure,

Figure. But before I enter into a Detail of the great Advantages of this Research, and the Nature of the Operations I propose, it is necessary to explain in what Manner I mean that the Question of the Earth's Figure and Magnitude is not yet decided.

There have been some who have long since suspected, and even thought they were furnished with Proofs of the Earth's not being exactly spherical. I here entirely abstract from the Unevennesses of it's Surface, which are not sensible in regard of the Earth's whole Bulk; seeing the Tops of the highest Mountains, and those even few in Number, are scarce more than a League above the Level of the Seas. Wherefore, I suppose the Earth to be bounded by a Curve Surface, such as it would be by the Level of the Sea carried quite over all the Earth. It is in this Manner, the Earth being considered as covered with a Fluid, that Sir *Isaac Newton*, in the first Edition of his *Principia*, published in 1686, has demonstrated, that supposing this Fluid homogeneous, and the Earth to have been at Rest at the Time of it's Creation, it must have assumed the Figure of a perfect Sphere: But afterwards, supposing it to have a Motion on it's Axis, as is well known it has in 24 Hours; this spherical Figure must have been changed into that of a Spheroid, flatted at it's Poles, in which the Degrees on the Meridian must be greater drawing near the Poles, than near the Equator.

Sir *Isaac* confirms this Hypothesis of the Earth's Figure, by Observations of the Diminution of the simple Pendulum upon approaching the Equator: To which Dr *Pound* adds the Analogy the Earth has with some of the other Planets, as *Jupiter*, which sometimes appears oval, it's least Axis being that about which it makes it's Revolution.

This Opinion of Sir *Isaac* has likewise been maintained by Mr *Huygens*, though with some small Difference. But in 1691, Mr *Eisenschmid* * having compared the Measurements of the Earth made in different Latitudes, as that of Father *Riccioli* in *Italy*, of M. *Picart* in *France*, and of *Snellius* in *Holland*; and having found that the Degree, which resulted from those different Measurements, continued to grow less in drawing nearer the Poles, (which is quite the contrary of what follows from the Earth's Figure supposed by Sir *Isaac* and *Huygens*) Mr *Eisenschmid* was thereupon of Opinion, that the Earth was longer at the Poles.

This Opinion of Mr *Eisenschmid* was afterwards confirmed by the late M. *Cassini*, in the Observations of the Meridian of *Paris*. For in 1701, having carried on these Operations to the *Pyrenæan* Mountains, which is a Space of above $7\frac{1}{2}^{\circ}$, he found, that as he advanced to the South these increased $\frac{1}{800}$ Part, or 72 Toises each Degree.

* Jo. Casp. Eisenschmidii Diatribe de figura telluris Elliptico-Sphæroide; ubi unà exhibetur ejus magnitudo per singulas dimensiones, consensu omnium Observationum comprobata. *Argentorati, apud Joh. Frider. Spoor.* 1691. *4to. (pag. 54. cum fig.)*

Since

Since the Meridian of *Paris* was, in 1718, carried on Northward to the Sea, M. *Caſſini*, the Son, found, upon comparing more than 8°, which this Meridian contains from Sea to Sea, that the Increaſe, going Northward, was but from 60 to 61 Toiſes each Degree; as may be ſeen in the large Treatiſe publiſhed in a ſeparate Volume, as a Sequel to the Memoirs of the *Royal Academy of Sciences* of *Paris* for the Year 1718. Theſe Reaſons did not hinder Sir *Iſaac* from perſiſting in his firſt Opinion of the Figure of the Earth flatted at the Poles, as appears in the 2d and 3d Editions of his *Principia*, publiſhed in 1713 and 1726: And it is very ſurprizing, that by this very Figure of the Earth he demonſtrates a certain Motion it has, to explain in the *Copernican* Syſtem the Preceſſion of the Equinoxes, or the apparent Motion of the fixt Stars in Longitude. Sir *Iſaac* finds the Inequality of the Degrees on the Meridian, in ſo little an Extent as that of *France*, not ſenſible enough to be poſſibly determined by immediate Obſervations; and he is of Opinion, that we ought more to rely on the Obſervations of the ſimple Pendulum, and on the other Principles which he has built upon, to conclude the Earth flatted at the Poles.

In 1720, M. *Mairan* attempted to reconcile the two different Hypotheſes of Sir *Iſaac* and M. *Caſſini*, by imagining that the Earth, at it's Creation, being without Motion, was of a much more oblong Figure than that which *Caſſini* thinks it has at preſent; ſo that it might have been reduced to that which it now has, by the diurnal Motion on it's Axis, *&c.* But Dr *Deſaguliers*, who is of Sir *Iſaac*'s Opinion, has made appear, that M. *Mairan*'s Suppoſition is contrary to the Laws of Motion; and has moreover propoſed ſeveral conſiderable Doubts on the Obſervations and Suppoſitions employed by M. *Caſſini* in his Determination of the Earth's Figure in 1718.

As ſoon as the Meridian of *Paris* had been extended from one Sea to the other, and M. *Caſſini* had thence deduced a Confirmation of the Syſtem of the Earth's being longer at the Poles; I imagined a new Method of deciding the Queſtion, by the Obſervation of the Degrees of the Parallel compared with thoſe of the Meridian.

For that Purpoſe I conſidered, that as the Degrees of the Meridian and thoſe of the Parallel, at the ſame Elevation of the Pole, had different Relations, according to the different Figures aſcribed to the Earth; nothing more was requiſite for concluding which Hypotheſis was the true one, than to determine this Relation by immediate Obſervation.

Having ſuppoſed, that there had been obſerved on the Parallel of *Paris*, a Space nearly of the ſame Magnitude with that on the Meridian, that is, of about 13 Degrees, ſince that on the Meridian is about $8\frac{1}{2}°$; I found by an exact Calculation, that according to the Figure which M. *Caſſini* has given to the Earth, this Space ought to contain $13\frac{1}{2}'$ of the Parallel more than in the Hypotheſis of the Earth's being ſpherical; which appeared to me conſiderable enough to be able to decide between theſe two Hypotheſes, and by a ſtronger Reaſon between

the

the Hypotheses of *Newton* and *Cassini*; seeing the Difference ought to be still more considerable than that now specified.

I concluded, at least, that, independent of the Figure of the whole Earth, which could not be determined by the sole Observations made in *France*, without making Suppositions, and admitting Principles, which are still liable to be contested; it would be of great Consequence towards constructing exact Charts of the Kingdom, to ascertain this Relation by Observations, which consisted only in forming Triangles along the Parallel of *Paris*, and observing at the two Ends the Difference of the Meridians, by the most exact Methods.

The Difference, which I have now mentioned, seemed to me to be so considerable, that I was in hopes of being able to determine it by Means only of two Places within Sight of one another, and situated to the East and West; provided their Difference of Longitude were accurately observed, independently of Astronomical Observations, by Means of lighted Fires; after the Manner that M. *Picart* put in Practice in *Denmark*, for determining the Difference of Longitude of the Astronomical Tower at *Copenhagen*, and of *Uraniburg* in the Isle of *Huen*. With this Intent, in *April* 1720, I went some Distance from *Paris* Southward, to the Places which I judged most proper for my Purpose; but my Design was not then executed, for Want of Assistance, and for other Reasons, which I shall pass in Silence.

Since that Time, I saw with Pleasure, that the Marquis *Poleni* had hit upon the same Thought with me; as may be seen in his Letter to the Abbot *Grandi*, dated *Nov.* 1724.

The Decision of this famous Question of the Earth's Figure had stopped there, when in the Year 1733, the Minister of *France* having thought it necessary to construct an exact Map of the whole Kingdom; and being informed, that the Work could not be better carried on than by the Astronomers of the *Royal Academy of Sciences*, applied to M. *Cassini* on that Head; who was of Opinion, that, in order to execute it with the utmost Exactitude, the same Method ought to be employed as for the Meridian, by taking through the whole Extent of the Kingdom, Triangles linked together by Means of Objects seen successively one from another, &c. This Project of making a Map of *France* by such Triangles, had been already offered to M. *Colbert* by M. *Picart* in 1681, but was not then executed. However, M. *Cassini* proposed, that these Triangles should be begun in a Direction perpendicular to the Meridian; in order to render these Operations of Service towards the Decision of the Earth's Figure, pursuant to the Method which I spoke of above: And M. *Cassini*, having in Person undertaken these Operations, and having carried them that same Year, 1733, from *Paris* to *St Malo*, whose Longitude from *Paris* M. *Picart* had observed in 1681; the Relations of the Degrees on the Meridian and Parallel were found to be such as were required in the Hypothesis of the Earth lengthened at the Poles, and even more lengthened than *Cassini* had determined

determined in 1718. For instead of the Diminution of $\frac{1}{60}$ Part for each Degree of the Parallel, which I had found according to the Earth's Figure, as determined by *Cassini* in 1718, he deduced from his Operations in 1733, a Diminution of the 36th Part of each Degree.

True it is, that M. *Cassini*, in the Account he gave of this Determination at the publick Meeting of *Nov.* 14, 1733, does not give it as entirely sure; because the Longitude of *St Malo*, with regard to *Paris*, was collected but from one Observation only of *Jupiter*'s first Satellite, wherein there may possibly be some Error: But at least M. *Cassini* seems certain, that there is a very considerable Diminution in the Degrees of the Parallel of *Paris*, which confirms his Opinion of the Earth's being longest at the Poles. This we are likely to have a better Certitude of hereafter, seeing we are informed that this Measurement of the Parallel of *Paris*, is carrying on in *France* by M. *Cassini*'s Sons, M. *Maraldi*'s Nephew, and several other young Mathematicians, instructed by M. *Cassini* in this sort of Work.

I have already said, that all these Operations performed in *France*, for the Figure and Magnitude of the Earth, could not serve to determine the Earth's Figure out of *France*, without the Assistance of certain Hypotheses; unless the same thing were undertaken and carried on in the other Regions of the Earth, more Northern and Southern than *France*. 'Tis upon this Consideration, that the *Royal Academy of Sciences* took up the Resolution of sending some Astronomers to make the like Observations as near the Equator and the Poles as possible, which are the Places where the difference of the Degrees on the Meridian ought to be the greatest, according to the different Hypotheses.

In *April* 1735, set out from *France* 3 Mathematicians and Astronomers of the Academy, *viz.* Messieurs *Godin*, *Bougher*, and *De la Condamine*, for the Province of *Quito*, which is the most Northern part of *Peru* in *America*; in order to observe, just under the Equinoctial Line, the Magnitude of some Degres of the Meridian and Equator.

As to the other Mathematicians and Astronomers of the same Academy, *viz.* Messieurs *de Maupertuis*, *Camus*, *Clairaut* the Son, and *Monnier* the Son, who have been sent to the North, they departed from *France* in *April* 1736, with Mr *Celsius* Professor of Astronomy at *Upsal*, who accompanied them to *Sweden*, as far as the Bottom of the Gulph of *Botbnia*, where they might measure about a Degree on the Meridian at it's crossing the Polar Circle. But as, by the last News I received from them, they had not finished their Operations, 'tis not yet known whether the Magnitude of the Degree measured by them, favours the Opinion of M. *Cassini*, or that of Sir *I. Newton*. All we know is, that they have found the length of the simple Pendulum favourable to the latter, that is, longer under the Polar Circle than farther South. My Brother *De la Croyere*, had already found the same Thing: For being at *Archangel* in 1728, he there observed, in the most exact Manner he possibly could,

 the

the Length of the ſimple Pendulum, which he found to be $\frac{1}{10}$ Parts of a Line longer than at *Paris*.

We are likewiſe informed by the other Aſtronomers gone to *Peru*, that in their Way towards the Equator, being at *St Domingo*, in the Latitude of 18° 37′, they there found the Pendulum ſwinging Seconds, to be about two Lines ſhorter than at *Paris*. Thus, all we as yet know from thoſe Gentlemen, on the Expeditions to the North and the Line, confirms the Opinion of Sir *I. Newton* and his Adherents: And yet M. *Mairan*, whom I have already mentioned, pretends, that this ſhortening of the Pendulum in drawing nearer the Equator, is in one Senſe entirely independent of the Earth's Figure.

Thus it appears from the foregoing Account, that the Queſtion concerning the Earth's Figure is not yet at an end. Nay, 'tis not impoſſible, that after finiſhing all the Obſervations which are actually making, new Difficulties may ariſe, and new Objections be ſtarted, that may prevent it's being entirely decided. However, all this Work cannot fail giving great Light to this important Queſtion, and procuring conſiderable Advantages to Geography, Aſtronomy, and Natural Philoſophy.

'Tis with this View, and particularly to render ſuch important Service to the Geography of *Ruſſia*, that I think it neceſſary to undertake a Work of that Nature in *Ruſſia*; towards executing which we have great Advantages, which the other Nations have not. One of the principal of theſe Advantages is the great Extent of *Ruſſia* every way. For were the Meridian of the *Imperial Obſervatory* of *Petersbourg* to be determined, it might be carried to between 22 and 23 Degrees; which is a fourth Part of the Diſtance from the Pole to the Equator. The Meridians of *Moſco* and *Aſtracan* are not of leſs Extent; and conſequently we might, by the Meaſurement of ſome one of theſe Meridians, determine more exactly than could have hitherto been done, the Inequality that ſubſiſts between the Degrees of the Meridian.

This is what the great *Caſſini* wiſhed, when, after having, in 1701, determined this Inequality by the Extent of 7° obſerved in *France*, as has been mentioned above, he ſays, that this Fact might be verified by Menſurations of greater Extent, if the other Princes of the Earth did contribute as much as the *King of France* towards the perfecting of Sciences.

M. *Caſſini* was then ignorant of the Views which *Peter* the Great had formed in the Eſtabliſhment of the *Academy of Sciences* at *Petersbourg*; nor could he then foreſee that her preſent Imperial Majeſty, was deſtined not only to purſue the Deſigns projected by that great Monarch, but alſo to ripen them to Perfection, by granting ſuch Succours and Aſſiſtances for the Promotion of Science, as were never yet afforded from any of the greateſt Princes of the Earth.

In the great Extent which might be given to the Meridian of *Peterſbourg*, as aboveſaid, there would be the Advantage of knowing, by Operations linked together, or uninterrupted, the Magnitude of ſome Degrees equal to thoſe which have been meaſured in *France*, and to that which

which the *French* Aſtronomers have meaſured in *Sweden*; and not only all the Degrees between the two, which the *French* Aſtronomers have not had in their Power to obſerve, but alſo ſome Degrees farther Northward than that meaſured by them in *Sweden*.

As the Exigencies of Geography require the Triangles, taken for the Determination of the Meridian, to be continued on every Side, and principally in Directions perpendicular to the Meridian, or according to the Parallels; with how great Exactitude may we not then determine the Proportion of the Degrees on the Parallels to thoſe on the Meridian, by means of the vaſt Extent of the *Ruſſian* Empire, which on it's Weſtern Side extending as far as all the Dominions of *Europe* from the moſt Northern to the moſt Southern, has no other Bounds to the Eaſt than the Eaſt itſelf, if I may be indulged the Expreſſion; ſeeing it's Extent that Way contains near half the Earth?

Another great Advantage to be obtained by the Work I now propoſe to be made in *Ruſſia*, is, That we, coming after others, ſhall reap the Benefit of all their Knowledge and Experience in the like kind of Meaſurements: Whence we may expect to ſucceed and execute it better than could have been done elſewhere, by applying timely Remedies againſt the Difficulties that occurred in other Places.

Theſe Operations are to be founded on a Baſis of the greateſt Length poſſible; which muſt be actually meaſured, and with the greateſt Exactneſs that may be; as it is to ſerve for a Foundation to the Meaſurement of all the Triangles. And in this Point too we have a very great Conveniency near *Petersbourg*, ſeeing on the Ice here we may meaſure out a Baſis, greater than has been hitherto taken, namely, from the Coaſt of *Ingria* about *Peterhoff*, to the Coaſt of *Finland* toward *Syſterbeck*. There is not leſs than 20 Werſts Diſtance between theſe two Extremities, and this great Diſtance may be meaſured very exactly, this Year eſpecially, that the Ice is very even. Moreover, as this Baſis is ſituated between the Iſle of *Cronſtad* and *Petersbourg*, in a Direction nearly perpendicular to the Diſtance from *Petersbourg* to *Cronſtad*, there can be no better Method for inferring thence, by exact Obſervation of the Angles taken at the Extremities of this Baſis, the Diſtance from the Centre of the Imperial Obſervatory to the Steeple of the new Church of *Cronſtad*; which two Objects are ſeen reciprocally from each other, and are not leſs than 30 Werſts aſunder: And this Diſtance once known exactly, will ſerve as a Foundation for all the Triangles that are to be taken; of which each of the Sides may have not leſs than from 30 to 40 Werſts, according as we ſhall find Objects advantageouſly ſituated for that Purpoſe. We have, to begin with, the Mountain of *Douderhof*, which, with the Imperial Obſervatory, and the Steeple of *Cronſtad* Church, forms one of the moſt convenient Triangles imaginable for the Subject we propoſe.

In taking Obſervations at theſe three Places, we ſhall ſee if we can diſcover others of the ſame advantageous Situation; but when no remarkable Objects are found of the Situation and Distance sought for, they must be erected on purpose, in the same manner as was of necessity done in other Countries: And this may be done here with more Ease, seeing, in Places where the Woods intercept our Sight, small Towers may be raised, at very little Expence, out of these same Woods, with Signals placed on them, which may be seen as far as may be required. In open Places, where consequently Wood is not so common, Signals alone, without Towers, will suffice.

The most necessary Instruments for executing this undertaking, are, besides the ordinary Astronomical Instruments, a common Quadrant of between 2 and 3 Feet *Radius*, for observing the Angles of the Triangles that shall be taken; and a Portion of a Circle of the greatest Radius that can be conveniently had, for observing the Arches of the Heavens corresponding with the Distances measured on the Earth.

I say, the Quadrant ought not to have a Radius of more than between 2 and 3 Feet: For if it be bigger, it cannot for the most part be made use of in Steeples and other Places of considerable Height, where 'tis requisite to observe; but also if it be less than 2 Feet, it will not give the Value of the Angles with sufficient Exactness.

As to the other Instrument for observing the Arches of the Heavens, it's Radius ought not to be less than from 12 to 15 Feet: but 'tis not necessary that it should contain a large Portion of a Circle. 'Tis only requisite to have this Portion somewhat larger than the Arch of the Heavens intended to be measured. Thus, as the Meridians, which may be traced in *Russia*, can be extended but between 22° and 23°, as already mentioned, it will suffice, that the Instrument employed therein be a Portion of a Circle of 30°.

M. *Picart*, for his first Operation, got an Arch of a Circle made of 18° and of 10 Foot Radius, with which he thought himself sure within 2'' or 3'': And no other Instrument was made use of in the chief Observations for the Meridian of *Paris*. The Astronomers who are gone to *America*, carried with them an Instrument of 12 Feet Radius, and of a Portion of a Circle of 30°. But those come to *Sweden*, contented themselves with a Portion of a Circle of 5½°, and 9 Feet Radius: But this Instrument, made by Mr *George Graham*, a very able *English* Mechanician, is by it's Construction so exact, that the Astronomers who have used it, think themselves sure to 2''. The one we want for the Observations in *Russia* ought to be made by the same Artist, and of the same Construction.

'Tis with such an Instrument that Mr *Bradley*, a celebrated *English* Astronomer, has discovered, in the Meridian Altitudes of some fixt Stars, certain constant and annual Variations, which do not proceed either from the Variation of the Refractions, or from the Parallax of these Stars, or, in fine, from any Nutation or Wavering of the Earth's Axis; but which he accounts for by the successive Motion of Light.

Whatever be the Cauſe of theſe Variations, (which Cauſe, as well as it's Effect, are not as yet, perhaps, entirely cleared up), as they may poſſibly happen in the Space of Time requiſite to be ſpent in making the Obſervations for the Meridian, or in paſſing from one End of the Meridian to the other; it is neceſſary, with the ſame Inſtrument, or ſuch another, that is of pretty near the ſame Exactneſs, to examine the Variations of the Stars made uſe of: Wherefore it would be of conſiderable Advantage, not only for the Obſervations of the Meaſurement of the Earth, but alſo for all the other principal Reſearches in Aſtronomy, to have Orders given for procuring two mural Quadrants of Mr *Graham*'s Make, and of the ſame Conſtruction, as I have already ſpecified; for which there are Walls already raiſed at the Imperial Obſervatory, in the Plane of the Meridian.

With theſe two Quadrants, which might be of 7 Feet Radius, and the moveable Teleſcope 9 or 10 Feet long, we ſhould be in a Condition to make Obſervations of the utmoſt Accuracy, ſuch as the preſent State of Aſtronomy requires.

Beſides theſe Inſtruments now mentioned, which are of abſolute Neceſſity to a ſolid Eſtabliſhment of Aſtronomy and Geography in this Country, there are ſtill ſome other ſmaller Inſtruments, that may be of great Uſe in the Operations I propoſe, or may ſerve to make other curious and uſeful Obſervations at the ſame time that thoſe for the Meaſurement of the Earth are making.

When the Sides of the Triangles, taken for meaſuring the Earth, terminate at very elevated Places, as on the Tops of the higheſt Mountains, it is neceſſary to reduce theſe Triangles to what they would be, had they been obſerved in horizontal Plains ſituated upon a Level with the Sea. For this Purpoſe, we muſt know the Height of the Mountains above the Sea's Level, which cannot always be determined geometrically, or would at leaſt be too tedious to perform: Wherefore, in the Meridian of *Paris*, which croſſed very high Mountains, M. *Caſſini* was of Opinion, that he ought to fix their Height by a ſhorter Method, which is that of the Height of the ſimple Barometer, obſerved on the Top of each Mountain, and compared with that obſerved at the ſame Time in another Place, whoſe Elevation above the Sea's Level was known. But as that Method ſuppoſes the Knowledge of the Proportion which the different Fallings of the Mercury keep with the diffierent Heights to which the Barometer is carried; and as Natural Philoſophers are not as yet entirely agreed on this Head, for want of Obſervations of ſufficient Accuracy: Thence it happened, that Dr *Deſaguliers*, making appear that M. *Caſſini* has not employed the moſt exact Proportion, found Reaſons for correcting, or at leaſt for doubting, of ſome of M. *Caſſini*'s Calculations. Thus it muſt be by the Aſſiſtance of new Experiments, better circumſtanced than thoſe hitherto made, and purſuant to a Theory entirely agreeing with theſe Experiments, that this Method may be employed with Certainty, for determining the Height of Mountains by the Barometer, and reducing the

the Angles obſerved from the Tops of theſe high Places, to what they would be, if they had been obſerved on a Plane horizontal with the Level of the Sea. Now theſe new Obſervations can be made on our Way in tracing the Meridian; and for that Purpoſe I have begun to conſtruct compound Barometers, which, by their peculiar Make, being very nice, will ſerve to obſerve with Accuracy the Quantity of the Mercury's Fall, at the different Elevations to which they ſhall be carried, in order to fix with greater Certainty the Proportion of that Fall. I ſhall take particular Care in the Conſtruction and Uſe of theſe Inſtruments to provide a Remedy againſt the Effect of Heat, which, as it is different in the different Times and Places of making theſe Experiments, may poſſibly produce apparent Variations, of which 'tis neceſſary to keep an Account.

There is ſtill another Method of determining the Elevation above the Level of the Sea of all the Points, in which the Triangles terminate, that are made for the Meaſurement of the Earth. This may be done by beginning theſe Operations near the Sea, as I propoſe to do, and actually meaſuring how many Toiſes and Feet the Places of the firſt Stations are elevated above the Level of the Sea. For if the Angles of the apparent Elevations of the ſecond Stations ſeen from the firſt be afterwards obſerved, it will be an eaſy Matter, from the known Diſtances, to deduce the true Elevations of the latter above the former, and conſequently above the Sea's Level, making proper Allowances in the Calculations for the Difference of the apparent Level from the true one. In this Method nothing is to be apprehended but the Variation of Refractions; but for this a Remedy may be found, for the moſt part, by returning upon one's Steps, that is, by reciprocally obſerving the firſt Stations ſeen from the ſecond: For if it be found, that as much as the ſecond Station appears elevated above the firſt, ſo much the firſt is depreſſed below the ſecond, except the ſmall Difference which muſt ariſe according to the given Diſtance, it will be a Proof, that the Refraction has been of no Prejudice.

The Obſervations and Determinations of the true Heights of all the Places which are to be viſited, will not be the leaſt laborious of thoſe that are to be made in theſe Journies; but then their Uſefulneſs will be a ſufficient Recompenſe for the Trouble; ſeeing they will afford us the Means of knowing all the chief Unevenneſſes of the Ground traverſed by theſe great Triangles, which being compared with the Length of the Courſe of the Rivers, may give us room to judge of their Rapidity, of the Eaſe or Difficulty of their Communications, *&c.*

The other conſiderable Obſervations and Experiments to be made in the Journies undertaken for ſuch Enquiries, are, the Obſervations of the Magnetic Needle, both as to it's Dip and Variation: But chiefly the Obſervations of the Length of the ſimple Pendulum, which, at preſent, is become requiſite to be obſerved with as much Exactneſs, and in as many Places as is poſſible; but alſo for which there are new Methods invented

invented, that we are promiſed the Communication of, and which probably ſurpaſs thoſe hitherto made uſe of; in as much as, ſince thoſe Methods have been found by the *Royal Academy of Sciences* of *Paris*, it was thought proper to notify them to the Aſtronomers ſent to *Peru*, in order to put them in Practice in their Obſervations.

Whereas all theſe Operations and Obſervations, which I have here propoſed, however arduous and difficult they may prove, have no other End than the Benefit of Geography; thoſe who are to have the Management of this Enterprize muſt be attended by ſeveral Surveyors and other Mathematicians of this Nation, who are to be inſtructed on the Road, and employed at the ſame Time in leſſer Operations with ſmaller Inſtruments: By which Means the Maps of the Countries, taken in by theſe great Triangles, may be verified; and thus, according as this Work advances, the finiſhing Stroke may be given to the Charts of *Ruſſia*.

The actual Menſuration of the Baſis propoſed in the preceding Article, by M. de L'Iſle. *Tranſlated from the* French *by* T. S. *M. D. F. R. S. Ibid.* p. 50. *dated* Peterſbourg, May. 14. 1737.

VI. Since my laſt, I undertook to meaſure the Baſis ſpoken of; and had the good Fortune to meaſure very exactly on the Ice, by taking the preciſe Diſtance between her Imperial Majeſty's Caſtle at *Peterhoff*, and the Caſtle of *Doubki*, ſituated oppoſite to it, on the Coaſt of *Finland*. I found the Diſtance between the oppoſite Walls of theſe Caſtles to be 74,250 Feet *Engliſh*. This Baſis, being much greater than any of thoſe employed hitherto for this Purpoſe, gives room to expect great Exactneſs in the whole Work, when it ſhall be carried on in the ſame Manner. It will at once ſerve to make a very exact Map of the Bottom of the Gulph of *Finland*. 'Tis for the ſame Deſign, and for better ordering the Charts of the Coaſts of the *Baltick*, that I intend (as ſoon as my Project ſhall be approved here in it's full Extent) to begin to meaſure my Triangles along the Coaſts of *Ingria* and *Livonia*, to the Iſlands of *Dagho*, *Oeſel*, *&c.* And to the end that the Charts of the Places taken in by theſe Triangles may be finiſhed at the ſame Time, I ſhall take with me all the Charts of theſe Parts, which can be had, in order to verify and correct them in my Way. According as theſe Charts are thus finiſhed in the beſt Manner, they may be engraved. I likewiſe intend to publiſh, as ſoon as poſſible, all the Operations and Obſervations I ſhall have made in my Expedition; that thus early Benefit may be reaped from them, and that the Publick, at the ſame time the Charts come out, may be acquainted with the Foundation on which they are conſtructed. I once thought to have by this Time printed the whole Detail of my Operations in taking the Baſis, that is, of the Precautions I uſed in aſcertaining it; but as it was meaſured in *Engliſh* Feet, which I have a Deſire to reduce to this Country Meaſure, and that 'tis requiſite to conſult the original Standards here on this Head, which I have not as yet been able to procure; for theſe Reaſons, I am obliged to delay the Publication of theſe firſt Obſervations.

An Account of an Improvement on the Terreſtrial

VII. The Globe is juſtly reckoned very uſeful and inſtructive, both as a general Map, and alſo for explaining the firſt Principles of Geography, and the ſpherical Doctrine of Aſtronomy. By this Inſtrument it is eaſy

to

Globe, by Joseph Harris, *Gent.* N°. 456. p. 321. Jan. &c. 1740.

to find the Length of the Days, and their Increase and Decrease, in all Places, and at all Times of the Year. But this is not usually performed in such a manner as at the same Time to explain how these *Phænomena* arise from the Motion of the Earth, which is the principal thing Beginners especially should have in View: Nor can this be remedied, at least but in few Cases, as Globes are commonly fitted up; for the Axis and the horary Circle prevent the Brass Meridian from being moveable quite round in the Horizon, which it ought to be, and so indeed prevent the Globe from being universally useful, even in the common way of considering it.

It is now about 6 Years since I removed this Impediment, by placing two horary Circles under the Meridian, one at each Pole. These Circles are fixed tight between two Brass Collars placed about the Axis, but so that they may be easily turned by the Hand when the Globe is at Rest; and when the Globe is turned, they are carried round with it, the Meridian serving as an Index to cut the horary Divisions. The Globe, being thus fitted, serves readily for solving of Problems in South as well as in North Latitudes, as also in Places near the Equator. But the chief Advantage gained by this Alteration, is, that the Globe is now adapted for solving of Problems upon the Principles of the *Pythagorean* System, or to shew how the Vicissitudes of Days and Nights, and the Alterations of their Lengths, are really made by the Motions of the Earth. To expedite this, I had the Brass Meridian at one of the Poles divided into Months and Days, according to the Sun's Declination, reckoning from the Pole. This being done, if we bring the Day of the Month to the Horizon, and rectify the Globe according to the Time of the Day, the Horizon will represent the Circle separating Light and Darkness, and the upper Half of the Globe, the illuminated Hemisphere, the Sun being in the Zenith.

While we view the Globe in this Position, we see the Situations of all Places in the illuminated Hemisphere, with respect to the Horizon, Meridian, *&c.* and by observing the Angles which the Meridians, cutting any Parallels of Latitude in the Horizon, make with the Brass Meridian, we have the Semidiurnal Arches of these Parallels respectively: And at the same Time (if the Sun be not in the Equator) we see why the Diurnal Arches of the Parallels continually decrease from the Neighbourhood of the elevated Pole, till we come to the opposite Part of the Horizon. If we turn the Globe Easterly round it's Axis, we shall see how all Places change their Positions with respect to the general Horizon, the Meridian, *&c.* by the Motion of the Earth round her Axis.

It yet remains to be shewed, how the annual Motion of the Earth in her Orbit, causes the Change of the Sun's Declination: This cannot be done by the Globe simply taken, but is very well shewed by the Instruments called *Orreries*: But to these their Costliness is an Objection, not mentioning others from a want of due Proportion in the things they exhibit. I had therefore an Instrument made, which consisted only of a round Trencher of Wood, a Circle of Brafs upon the Face of it, and between thefe 3 Wheels of the fame Dimenfions and Number of Teeth: The innermoft Wheel was fixed to the Wood in the Centre, the third had it's Axis come through the brafs Plate, round which was a brafs Circle having a Socket making an Angle with it of 66½ Degrees; in this Socket was fixed the Axis of a little Globe, having a Horizon about it, to reprefent the Circle feparating Light from Darknefs, the Sun being fuppofed to be in the Middle of the Inftrument. While the brafs Plate is turned round through the Scale of Months and Days expreffed on the under Plate, the Axis of the *Terrella* is kept all the while parallel to itfelf, by means of the fecond Wheel placed between the two above-mentioned; and fo the Change of the Sun's Declination, or rather, which comes to the fame Purpofe, the different Pofition of the Equatorial Axis with refpect to the Circle feparating Light and Darknefs, is exhibited all the while the Earth is going round in her Orbit. By placing the Axis of an ivory Ball having one half blacked, upright in the middle of the Circle which carries the *Terrella*, this little Inftrument will ferve to explain the *Phænomena* of the Moon's *Phafes*.

Having thus learned the Caufe of the Sun's Change of Declination, we may now have recourfe to the larger Globe, and moving it according to the different Seafons, we may obferve the *Phænomena* thence arifing more diftinctly.

For a graduated Meridian, I had a flexible Slip of Brafs divided into Degrees, which I could fix occafionally in the two Hour Circles; and upon fuch another Slip I had a Scale of Months, anfwering to the Sun's Declination, reckoning both ways from the Equator. By means of this graduated Meridian, the Globe being rectified according to the Sun's Declination, if we gently turn it round it's Axis, we may prefently find the Time of the Sun's rifing or fetting in all Places, by obferving the Hour Circle, when the feveral Degrees of Latitudes refpectively come to the Horizon.

After the fame manner, if the Globe be elevated to any particular Latitude, and the Meridian having the Scale of Months be fixed in it's Place, we may foon find the Time of the Sun's rifing or fetting in that Latitude throughout the Year, by obferving the Hour Circle when the refpective Days come to the Horizon. This Method is not only ufeful on the Account of it's being expeditious, but alfo becaufe it intimates, why at the fame time the Days are of different Lengths in different Latitudes, and in the fame Latitude at different Times of the Year.

The Globe-makers might fave us the Trouble and Expence of having thefe graduated Slips of Brafs, by dividing fome Meridian, which goes over the leaft Land, into Degrees, which might be marked with round Dots, and every Tenth numbered. The Scale of Months might be engraven upon fome other Meridian. It would be of Ufe likewife, if the Parallels and Meridians of every Degree between the Tropics be drawn in faint Lines, which I think might be done without obſcuring the Map.

Parallel to the Horizon, and 18° below it, I had a Circle fixed for ſhewing the Limits of the Twilights: This is uſeful, as it ſhews at one View the State of the Twilights, and alſo why they do not lengthen or ſhorten, as the Days do. The Semi-Circle of Poſition is a thin narrow Plate of Braſs as uſual, but made ſo that it's Axis is moveable quite round the Horizon. I had alſo a narrow flexible Slip of Braſs, which might be girt round the Globe in any Poſition, and ſo be made to repreſent any great Circle whatſoever: This occaſional Circle may be inſtructive to Beginners on ſeveral Occaſions.

If the principal Horizon be of Wood, or made ſo as to obſcure the Globe below it, the Twilight Horizon had beſt have ſmall Feet of a proper Length, fixed ſo that it might ſtand in it's proper Place upon the other, occaſionally; then inverting the Poſition of the Globe, the ſame thing will be ſhewed as before.

The farther Uſe and Application of theſe Contrivances to different Projections of the Sphere, *&c.* will be obvious to thoſe who are acquainted with theſe things; and without dwelling any longer upon this Subject, it may ſeem, that I have already ſaid more than was needful in this Place. But the Globe being in every body's Hands, and in reality a very uſeful, entertaining, and inſtructve Inſtrument, I thought an Attempt to render it more ſo, would not be altogether uſeleſs, or yet unworthy the Notice of the Curious.

The Construction and Uſe of Spherical Maps, or ſuch as are delineated upon Portions of a Spherical Surface. By Mr John Colſon, M.A. *F. R. S.* No. 440. p. 204. Jan. *&c.* 1736.

VIII. Geographical Maps, and Hydrographical Charts, though they are Repreſentations of a Convex Spherical Surface, yet were firſt delineated upon Planes, as being the moſt eaſy and obvious, tho' not the moſt natural and accurate Repreſentations: And they will be ſufficiently near the Truth, when the Part of the Earth or Seas to be deſcribed is not of a very large Extent. Such as theſe have been uſually called Chorographical and Topographical Maps; but when the Map is any thing general, or is to contain any large Tract of the Earth or Seas, ſuppoſe (for Inſtance) one of the four Quarters of the World, as they are called; then, when they are projected, or repreſented upon a Plane, the Parts muſt neceſſarily be diſtorted, one way contracted beyond the Truth, another way dilated, ſo as to give no juſt Idea of the whole. Nor can this Diſtortion be poſſibly avoided, when any conſiderable Part of a Spherical Surface, by any Projection whatever, is to be repreſented upon a Plane. 'Tis true, this Diſtortion is always regular, and according to certain Laws; ſo that knowing the Nature of the Projection, it may tolerably well be allowed for. But to do this ſcientifically, and as it ought to be done, requires much Skill and Accuracy in the Maker, as well as good Proficience and Experience in the Peruſer; and therefore not ſo proper for an Introduction to Learners, in the Rudiments of Geography. Young Minds are apt to receive wrong Notions and Prejudices

judices from them, at leaſt cannot be rightly and eaſily inſtructed by them.

To obviate this Inconvenience, Geographers have contrived and conſtructed the Terreſtrial Globe, on which they endeavour to delineate all the Parts of the Earth's Surface in their natural State, as to Longitude, Latitude, Diſtance, Bearing, Magnitude, *&c.* which being a true and genuine Repreſentation of the whole Superficies of the Earth, as far as it is yet known, is the beſt adapted for conveying juſt Notions to young Minds, and for preventing all falſe Conceptions and Prepoſſeſſions. After the firſt Rudiments of Geography have been imbibed from hence, they will be then prepared for the Uſe of plain Maps ; and they will afterwards find, that large Projections of particular Countries, Kingdoms, and Provinces, *in plano* ; will be of excellent Service to them for their farther Improvement in this uſeful and neceſſary Science. Nor will they now be in any Danger of being miſled by ſuch Maps, tho' they are not ſo juſt and natural Repreſentations of the Earthly Globe.

Now the ſame Conveniencies that may be derived from the whole Globe, may, in Proportion, be had from any notable Portions of it ; as an Hemiſphere, a Quadrant, a Sectant, an Octant, or other Part. But with this Advantage beſides, that theſe partial Spherical Maps will not only be much leſs cumberſome, and more manageable than a whole Globe, but may be made much more accurate and particular, as being capable of being formed to a much larger Diameter than a Globe can conveniently be made to. The Maps may firſt be printed upon a Plane, as is uſual in the common Globes, and then paſted upon thin convex Shells of Paſteboard, formed to the intended Radius. The forming of theſe ſpherical Coats of Paſteboard will be a Matter of no great Difficulty, even to as large a Diameter as ſhall be deſired ; but the chief Art will be required in projecting the Maps *in plano*, after the ſimpleſt and exacteſt Manner, ſo as that they may adapt themſelves, with as little Error as poſſible, to a ſpherical Surface. For a plane Surface cannot be converted into a ſpherical Surface without ſome Error. The beſt Method of doing this, with the leaſt poſſible Error, I think will be as follows.

Inſtead of the uſual Slips or Guſſets, as is the manner of Globe-makers, which are comprehended between two Meridians at ſome Diſtance, and are formed only tentatively and mechanically, without the Help of any juſt Theory, we may divide the whole ſpherical Surface into parallel Portions, or Zones ; that is, into Parts terminated by two Parallels to the Equator, at the Diſtance (ſuppoſe) of ten Degrees. As if the firſt of theſe Portions, or Zones, were at the Equator itſelf, and extended to 5° of Latitude on each Side of that Circle, the ſecond Zone would be at the Parallel of 10° of Latitude, and would extend to 5° of Latitude on one Side, and to 15° of Latitude on the other Side of that Parallel, and ſo of the ſucceeding Zones.

Now we may conceive the firſt of theſe Portions, or Zones, to be converted from a ſpherical Surface to a plane Surface in this manner, without ſenſible Error. Let the middle Line of this Zone, that is the Equator, continue in it's Situation, and let the Segments of the Meridians on each Side be conceived to unbend themſelves gradually, 'till they are extended into right Lines perpendicular to the Equator: Then will that which was before a Zone, or Portion of a ſpherical Surface, with a ſmall Alteration become a Portion of a cylindrical Surface, circumſcribed about the Sphere; whoſe Breadth is every where equal to 10° of the Sphere, and whoſe Circumference is equal to the Equator. And thus every Parallel to the Equator, as far as that of 5° of Latitude on each Side, will be ſtretched and extended into a Circle as large as the Equator; but they will all keep the ſame Diſtance from one another, and from the Equator, that they had before. This Extenſion, or Alteration, will be every where regular and uniform, and will be but very little, even where it is moſt: For the leaſt of theſe Circles, which is the Parallel of 5° of Latitude, has the ſame Proportion to the Circle it is ſtretched to, or the Equator, as the Sine of 85° has to the Radius, or as 9961947 to 10000000; which approaches very near to a Ratio of Equality. And now it will be eaſily conceived, that without undergoing any other Alteration, or Diſtortion, this Portion of a cylindrical Surface may be rectified, or extended into a plane Parallellogram, whoſe Length will be equal to that of the Equator, and whoſe Breadth will be equal to an Arch of 10° of the ſame Equator.

And conſequently, by an Operation that will be juſt the Reverſe of this, if upon a Plane we delineate ſuch a Parallellogram as this, we may then lay down all the Places that are contained in it very exactly, in their proper Situation of Longitude and Latitude; and then apply it's middle Line, or Equator, to that of a Globe of a due Magnitude, which will then become a Portion of a cylindrical Surface, circumſcribed about the Globe. Then by preſſing it cloſe to the Body of the Globe, we ſhall cauſe it to contract itſelf a very little, but regularly, which Contraction will be only according to Longitude, and not at all according to Latitude; and then the cylindrical Surface will be changed into that of a Sphere, and will become the firſt ſpherical Zone before deſcribed, with all it's Delineations in their due Poſition, without ſenſible Error.

In like manner in the ſecond ſpherical Portion, or Zone, comprehended between the Parallels of five and fifteen Degrees, whoſe middle Line is the Parallel of ten Degrees, we may conceive the Segments of the Meridians to unbend gradually on each Side, and to extend themſelves into Tangent right Lines, which therefore will form a Segment of a conical Surface, ſtill touching the Globe in the Parallel of ten Degrees of Latitude. The Axis of this Cone will coincide with the prolonged Axis of the Globe, and the Side of the Cone, which is to be eſtimated from the Vertex to the Circle of Contact, will be the Co-tangent of the Latitude, or the Tangent of 80°. Now this Portion of a conical Surface may eaſily be conceived to be unrolled, or to be expanded into a plane Surface, without undergoing any other Alteration, and then it will become a Portion of a Sector of a Circle; which Portion will have for it's Length, or middle Line, an Arch of a Circle described with the aforesaid Tangent, as a Radius, whose Length will be the same as the Parallel of Contact, and it's Breadth will be equal to an Arch of the Equator of 10° as before. This Segment of a Sector of a Circle so produced, may therefore be easily described *in plano*, and within it may be inserted all the Places belonging to it, according to their Longitude and Latitude. Then it must be applied to the Globe, so as that it's middle Line shall coincide with the Parallel of 10°; then by pressing it may be bent to the Surface of the Globe, every Meridian to it's respective Representative, by which it will uniformly contract a little according to Longitude, but not at all according to Latitude. And thus the Globe will be covered as far as 15° of Latitude.

The next Zone, or that belonging to the Parallel of 20°, may be thus constructed *à priori*. Upon a plain Paper, with Radius equal to the Tangent of 70 Degrees, describe an Arch, whose Length is equal to that of the Parallel of 20°; as also two other concentrick Arches on each Side, at a Distance from the middle Arch equal to an Arch of 5°. This will be the required Segment of the circular Sector, in which are to be inserted all the Places belonging to it, according to their Longitude and Latitude. Then the middle Line or Arch is to be applyed to the Parallel of 20° upon the Globe, and the Segment of the conical Surface thence arising is to be duly contracted as before, or pressed close to the Globe; by which Means this Zone will also be compleated. And in the same manner we are to proceed to the succeeding Zones, 'till the whole Globe is covered. And the Method will not differ in any material Circumstance, if instead of a whole Globe, we are to construct any Part of it only, or what I here call a Spherical Map.

To reduce this Theory to Practice, and as a Specimen of Spherical Maps, I have constructed a Terrestrial Hemisphere to a Diameter of near 15 Inches; To which I have given the Name of the *British Hemisphere*, because it has *Great-Britain* in the Centre, or rather at it's Vertex. It is therefore adapted to the Meridian and Horizon of *London*, and exhibits one half of the Earth's Surface, as it lies round about this City; which is vastly the most considerable Part of the whole Earth's Superficies. The Longitude and Latitude of Places are here easily known by Inspection, and their Bearing and Distances may be nearly estimated: And all the Delineations are as accurate and particular as this small Radius would permit. I conceive therefore it may be no unfit Instrument for instructing Beginners, or for initiating young Minds in the first Rudiments of Geography.

A Spirit Level to be fixed to a Quadrant for taking a Meridional Altitude at Sea.

IX. The Necessity of seeing the Horizon, in order to find the Latitude of a Ship at Sea, has always been so great an Inconvenience, that any Method for determining it without the Help of the Horizon, will be of considerable Use, although it should be liable to an Error of a few Minutes: And as it is generally agreed by Seamen, that they are much oftener *when the Horizon is not visible. By* John Hadley, *Esq; V. Pr.* R S. No. 430. p. 167. Nov. &c. 1733. Fig. 109.

oftner ſenſible of this Inconvenience in calm Weather, than in rough; it is hoped that the following manner of conſtructing and uſing a Spirit Level, may, in that Caſe, be capable of ſo much Exactneſs, at leaſt, as may render it acceptable to the Publick.

This Level is compoſed of a Glaſs Tube A B, bent into an Arch of a Circle, and containing ſuch Number of Degrees as will be moſt ſuitable to the Degree of Exactneſs with which the Obſervation can be made. The Bore of it muſt not be wider than $\frac{1}{10}$ of an Inch in Diameter, that the Liquor in it may the better keep together, and the two Ends of it ſtand Perpendicular to the Tube in all Poſtures: Nor ſhould it be much leſs, leaſt the hanging of the Spirit to the Sides hinder it from ſettling ſo truly by it's Weight to the loweſt Part of the Tube. This Tube is cemented into another Braſs one C D E F, of the ſame Curvature, the outer Half of which is taken off, ſo as to ſhew the Glaſs, leaving only a ſmall Part in the Middle D F entire, in which a ſmall Stop-cock G is placed. The Glaſs Tube is divided in two in the Middle, to make room for this Stop-cock, the Key of which muſt be pierced through with a Hole of only about $\frac{1}{100}$ Part of an Inch, for the Paſſage of the Liquor. The outer Ends of the Glaſs Tube muſt have a Communication with one another round about by Means of two ſmall Pipes I and K, and the Tube H, the manner of which is ſufficiently ſhewn by the Figure.

Each half of the Glaſs Tube A B muſt have a Scale of Degrees anſwering the Curvature of the Tube, ſubdivided at Pleaſure. They may be numbered either as the upper or under Scale in the Figure; and obſerve that in the under Scale two Degrees are numbered as one; the Reaſon of which is, that the Motion of the Spirit in the Tube increaſing the Number on one Hand, and at the ſame Time as much diminiſhing that on the other, their Difference is altered thereby, ſo as to anſwer to double that Motion. The Diviſion of the Scales are cut on the Edge of the Braſs half Tube, or Trough, which is made thick for the greater Strength.

In one of the ſmall Pipes I or K, juſt againſt the Return of it, which enters the End of the firſt-mentioned Glaſs Tube at A or B, is a ſmall Hole, by which to introduce into it ſo much Spirit of Wine as may fill it from the Middle of the Scale on one Hand to the Middle of that on the other; this Hole may be afterwards ſtopped by a Skrew-pin.

The inner Ends of the two Halves of the Glaſs Tube A B ſhould be fixed into the entire Part of the Braſs Tube D F with a Cement made with old hard Bees-Wax, or ſome other Materials not diſſolvable by Spirit of Wine, as ſhould alſo the Ends of the ſmall Pipes I and K into this and the Tube H: Thoſe Halves, as to the remaining Part of their Lengths, may be faſtned down with any ſtrong Cement.

This Level may be ſet on to one of the Limbs of the Quadrant, fitted up for this Purpoſe, in the manner expreſſed in the Figure. It hath an Index moveable on the Centre, and a Spring at the other End to keep it ſteady, when it is directed to any of the Diviſions on the Arch, which needs

needs no other Divifion than into whole Degrees. The Index may be furnifhed either with plain Sights, or may carry a fhort Telefcope, with a Vane in it's Focus, to receive the Image of the Sun, when it is bright enough; but if the Sun be hazy, or the Moon, or a Star be obferved, a fliding Shutter may be drawn out to tranfmit the Rays of Light to the Eye-glafs. The Vane has alfo a Thread fixed on it perpendicular to the Plane of the Quadrant. The whole Inftrument (for the eafier managing it) may be fupported by a Staff, refting with one End on the Floor.

The manner of ufing it is thus: Holding the Quadrant in a vertical Pofture, with that Limb to which the Level is fixed parallel to the Horizon, raife the Index to fome Divifion of the Arch, as near as you can to the true Height of the Object; which is fuppofed to be near the Meridian, and confequently to alter it's Altitude but flowly; Then turning the Key of the Stop-cock, fo as to let the Spirit of Wine pafs through the fmall Hole in it, keep the Image of the Object as clofe to the Thread on the Vane as you can, endeavouring that the unavoidable Vibrations of it above and below the Thread, may be equal, both in refpect of their Length, and the Swiftnefs of their Motions, *&c.* Continue this 'till the Spirit feems quite fettled to fome Part of the Scale, and fomething longer. This it will do flowly, but without any fenfible Vibrations; for the Stop-cock allowing it no Paffage but through the fmall Hole in it's Key, will give fuch a Check to it's Motions, as not only to ftop thofe Vibrations, but alfo to hinder it's being thrown backwards and forwards in the Tube by any Shocks of the Inftrument; and yet as far as I have obferved will not prevent it's fettling (with fufficient Truth, though flowly) to the loweft Part of the Tube. About half a Minute of Time or more may be neceffary for this, according as the aforefaid fmall Hole is greater, or lefs in Proportion to the Bore of the Tube. When you judge the Spirit quite fettled, turn the Stop-cock again: It is of no Importance that the Image of the Object be exactly on the Thread at the Inftant that this is done. Obferve againft what Degree, and Part of a Degree, each End of the Spirit in the Tube ftands. If your Scale be numbered like the upper one in the Figure, and the Quantity of Spirit be exact, both Ends will agree, and the Degree and Parts marked muft be added to, or fubftracted from the Altitude fhewn by the Index, according to the Directions: If the Ends do not exactly agree, take the Mean between them. If you ufe the under Scale, fubftract the lefs Number from the greater, and add, or fubftract the Excefs, the Number refulting will fhew the mean Elevation of the Index during the latter Part of the Obfervation, and will differ from the true Altitude of the Object about half fo much as the Vibrations of it's Image above and below the aforementioned Thread on the Vane fail of compenfating one another during that Time. If either End of the Spirit leave the Scale, the Index muft be removed three or four Degrees, and the Obfervation repeated.

Inftead of the Curve Tubes A and B, two ftrait ones might be ufed, fet together fo as to make a very obtufe Angle in the Middle; but then it will be convenient to have the Quantity of Spirit more exactly fitted to the Scale, because the allowing for the Difference will be something more troublesome.

If the Observer have an Assistant to attend to the Level, while he himself observes the Object, the whole Apparatus of the Brass Tube, and Stop-cock, may be omitted, substituting in it's room only a Plug with a small Hole in it, which may be wrapped round with a very thin Slice of Cork, and so thrust down into the middle of the Glass Tube. The cutting the Glass Tube in half in the Middle may likewise be avoided, if instead of the Stop-cock at G, there be one fixed in one or both of the Pipes I and K, to open and stop the Passage of the Air, having a larger Hole in their Keys, there being also a Plug with a small Hole, thrust down into the Middle of the Tube, as before.

The Bore of the small Pipes I and K, and the Tube H, must not be so narrow as to make it difficult to reduce the Spirit into it's Place, if by any Accident either End of it should get into them.

I have been informed, that an Object may by kept in View without much Difficulty, even in pretty rough Weather, thro' a Telescope magnifying about ten times. Now as such Telescopes seldom comprehend an Area of much more than 1° in Diameter, or at most 1° 20′ it follows that the Axis of the Telescope is always kept within 40′ at most of the Object, and that is the greatest Vibration of the Image above and below the Thread on the Vane. If this be allowed, it seems reasonable to expect that the Medium of the Vibrations one Way should not exceed the Medium of those the other, more than by about $\frac{1}{5}$th or $\frac{1}{6}$th Part of the greatest Vibration; *i. e.* about 7 or 8′ the half of which will be the Error of the Observation. In still Weather it will probably be much less, if the Instrument be in the Hands of a Person moderately skilful in observing.

A Description of a Water-Level to be fixed to Davis's *Quadrant, whereby an Observation may be taken at Sea, in thick and hazy Weather, without seeing the Horizon; by* Charles Leigh, *Gent.* No. 451. p. 413. *dated* Nov. 3. 1737.

X. The Sea-Quadrant now in Use, called Captain *Davis*'s Quadrant, being invented by that ingenious Gentleman, for taking the Sun's Altitude, is an Instrument well known, universally approved, and sufficiently accurate; I say sufficiently, because it is well known to all Artists at Sea, that 5 or 10′ Error (which is generally the most, if the Instrument be good, though the Motion be great) is a Trifle scarce worth the noting, either in sailing near a Meridian, or parallel Circle. This, together with a long Use of this Instrument, has, to my Knowledge, (having had the Experience of 17 Years in the Royal Navy) occasioned such a Fondness to it, that it would be no easy matter to dissuade the Navigator from the Use of it, to any other.

It is true, that when the natural Horizon is obscured by thick and hazy Weather, (which is very frequently the Case, especially off of our Chanel, the Banks of *Newfoundland*, &c.) this Instrument, as it now stands, is of no Use; which too often occasions melancholy Consequences, such as the Loss of Ships and Cargoes, and, what is still more valuable, our Seamens Lives. If therefore, to this Instrument, an Apparatus were added,

such

Pl. XIX. *Vol. VIII. part 1 pa. 360.*

Fig. 106.

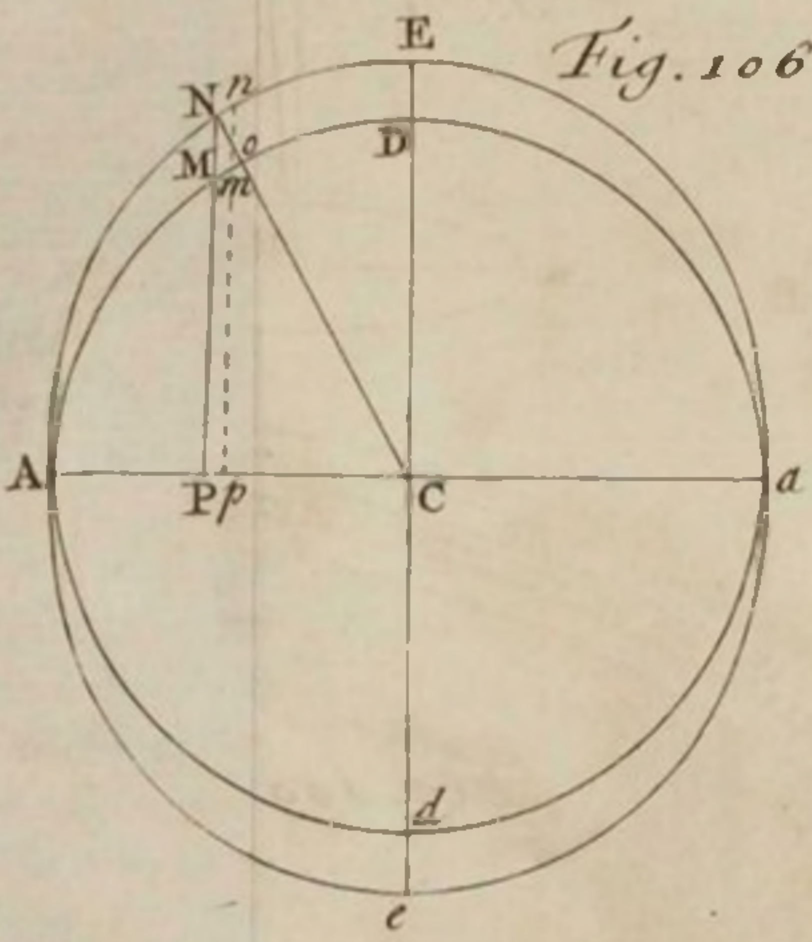


Fig. 105.

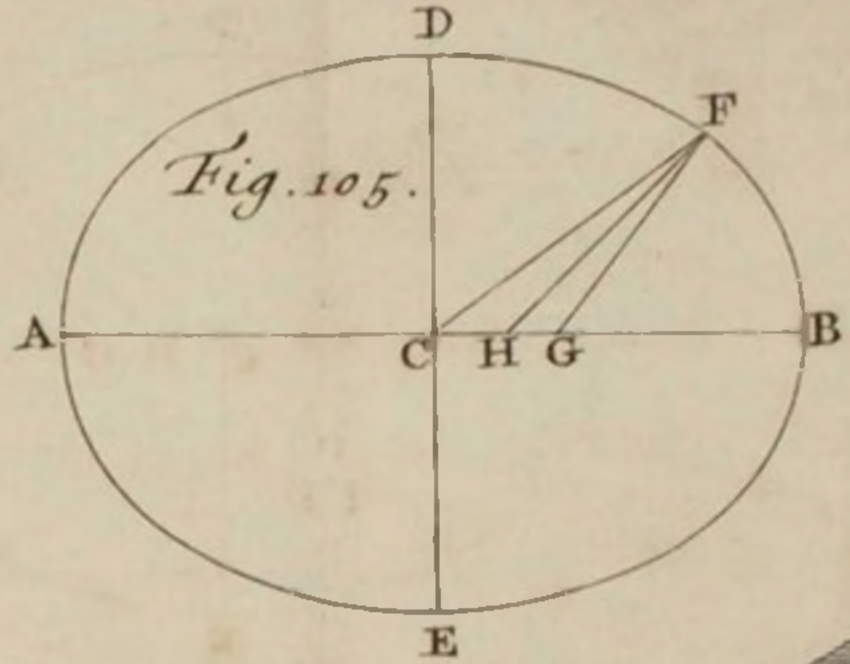


Fig. 107.

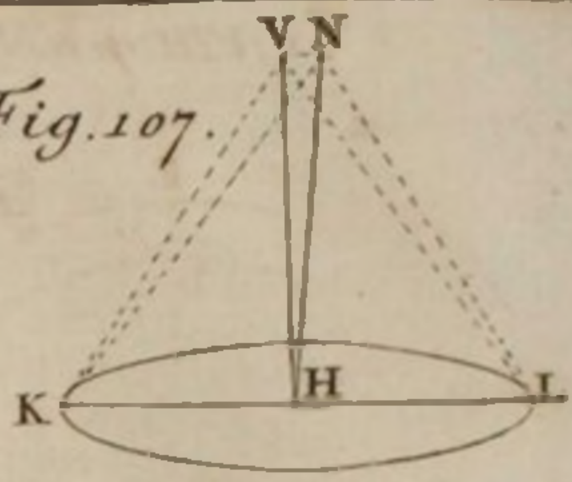


Fig. 109.

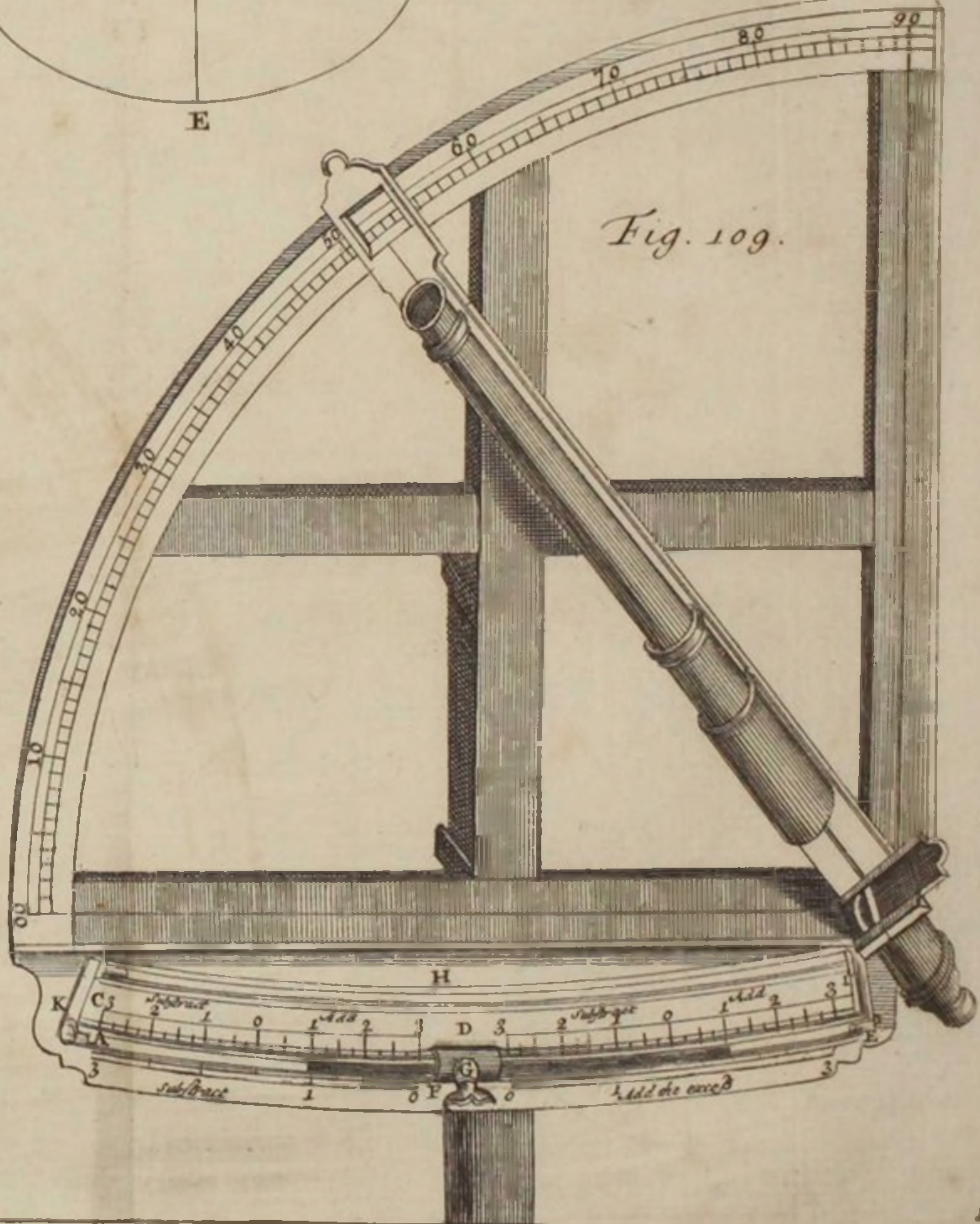
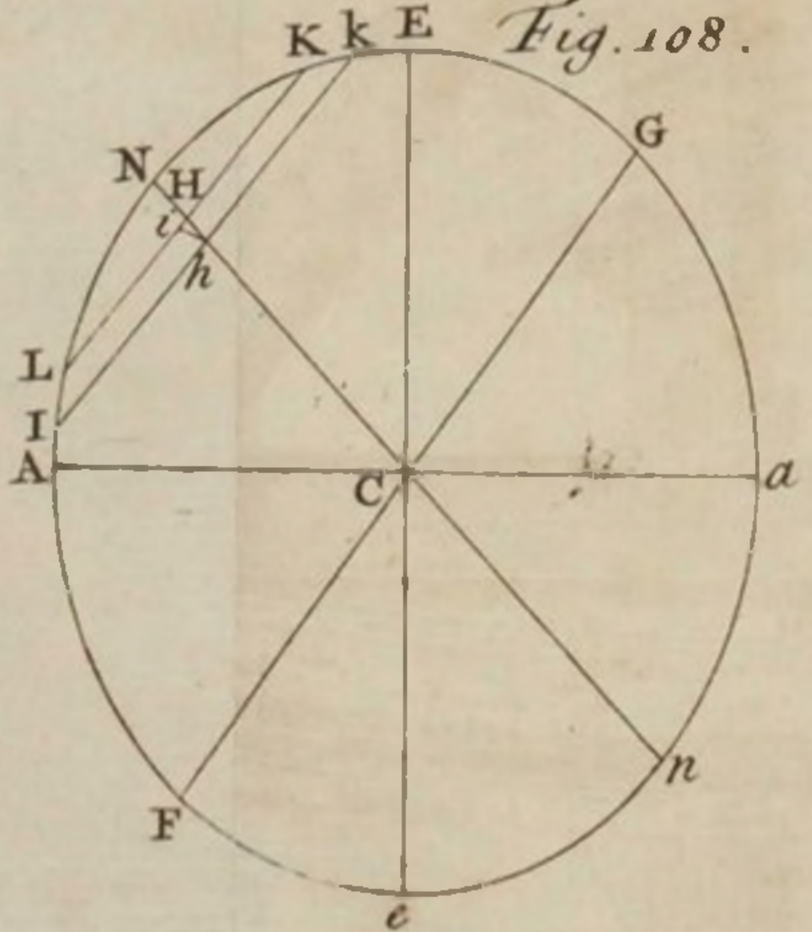


Fig. 108.



19

ſuch as an artificial or portable Horizon, that could be as effectually relied on, as that of the true or natural; and at the ſame Time plain, eaſy, and obvious; I am of Opinion, it would be needleſs to go about proving it's Uſefulneſs.

To this End, ſome ingenious Gentlemen have, within theſe few Years, very commendably employed their Talents this way; among which, I humbly offer my Mite.

I ſhall now proceed to the Principle on which this Apparatus is founded, *viz.*

That the Surface of all Liquids (when free from any external Cauſe) that have a Communication with each other, though divided and ſeperated in their Surfaces, will be truly in a horizontal Plain.

The Quadrant, and it's Conſtruction, being well known, there remains but little to be ſaid to it; the principal Parts that I ſhall take Notice of, are the two Sections of two different Circles that are concentrick, as A B, C D, on which the Degrees and Minutes are graduated; E, the common Centre, through which goes a braſs Pin fixed to the Apparatus E F, which is an Index or *Radius* to the Section C D, on which Index is fixed a braſs Tube 15 Inches long, in the Extremities of which are fixed perpendicularly two Glaſs Tubes E *b* and *d b*, 4 Inches long, with braſs Ferrels on the Tops. Fig. 110.

On the central Pin, which is fixed in the Index, is alſo fixed the braſs horizontal Vane E *z* obliquely, in which there is a Hole for the central glaſs Tube E *b*, to come through ½ of it's Length, cloſe to which, and from the common Centre, comes a white fine Thread, the End being fixed in the Vane E *z*; and in the ſame manner is a Thread fixed cloſe to the glaſs Tube *d b*.

Directions to prepare, and obſerve by this Inſtrument.

To prepare this Inſtrument for Obſervation, you muſt pour Water (for that is always to be had) into the Tube E *b*, till it's little Surface riſes to the central Thread; then to keep it fixed there, ſhut the Slide or Stop that is fixed on the Top of the central Tube, and there it will continue; then you may at Pleaſure pour or drop Water into the Tube *d b*, till it's Surface alſo riſes to the Thread fixed there; and if too much Water is dropped in, dip in a Wire with a ſmall bit of Spung or Cotton fixed to the End, till you exactly trim your Tubes; for in this lies the greateſt Nicety and Exactneſs, to trim your Surfaces true to the Threads.

This being done, you are prepared for Obſervation; and placing yourſelf conveniently, where there is the leaſt Motion, ſit down on a Stool or the Deck, and having the Quadrant in it's proper Poſition on your Lap, open the Slide on the Top of the Tube E *b*, that the Water may have it's natural Tendency, which will be truly horizontal, conformable to the above Principle; then keeping your Eye on the central Thread, bring that and the little Surface into one, which will be effected

 with

with the ſame Eaſe as if you obſerved by the natural Horizon; then keep moving the End of the Index F, till you bring the *Speculum* of the Sun in the little Hole on the Horizon-Vane that is cloſe to the Thread, ſo that you have, as it were, but one Object to look at during the Time of Obſervation: But if you uſe the Shadow-Vane, you muſt bring the upper Edge of the Shadow on the central Line, drawn on the Horizon-Vane, as uſual; remembering as often as you reſt, waiting the Sun's riſing, to cloſe the Slide, which prevents the Water's running out, it then remaining immoveable. And thus continuing to do, till the Sun is on your Meridian, caſt up the two Sums as is uſual, that is, the Degrees cut by the Shadow-Vane, and thoſe cut by the upper Edge of the Index on the greater Arch, which Sum will give what is required, *viz.* the Sun's Diſtance from the Zenith. On the End of the Index is fixed a Sight-Vane N, by which you may obſerve by the natural Horizon, the very ſame way as with the common Quadrant; ſo that the one will be the Proof of the other.

N. B. There are of late Invention, large Glaſs *Lens's*, very uſeful for collecting the weak and ſcattered Rays of the Sun into a *Speculum*; but if the Rays are even too weak to be collected by that, and that you have any Sight of the Sun, let another look through the little Hole on the Horizon-Vane above-mentioned, and the upper Edge of the Shade-Vane, to the Sun, and it will give what is required: The ſame Rule is to be obſerved in taking the Altitude of a Star.

The Deſcription and Uſe of an Apparatus added as an Improvement to Davis's *Quadrant, conſiſting of a Mercurial Level, for taking the Co-altitude of Sun or Star at Sea, without the uſual Aſſiſtance of the ſenſible Horizon, which frequently is obſcured. By the ſame Ibid.* p. 417.

XI. I had the Honour ſome time ago to communicate an Invention much upon the ſame Nature and Principle with this; ſince which I have made ſuch Alterations and Improvements thereto, as have rendered it complete and perfect for the Uſe intended, and have been confirmed by repeated Experiments, as well on board Ships, as on Shore. An Inſtrument of this Nature we greatly want at Sea, and it would be a great Satisfaction to me, if any Thoughts and Inventions of mine ſhould contribute to the removing of this grand Impediment, that ſo frequently happens.

To arrive to the utmoſt Perfection in Navigation, three things are abſolutely requiſite, *viz.* The Variation, the Latitude, and the Longitude; which laſt is, as yet, concealed from us. The two former indeed, we have a tolerable Certainty of, eſpecially the firſt which may be found by Obſervation, almoſt at any time the Sun ſhall be viſible in or above the Horizon, either by an Amplitude or Azimuth; but unhappily as yet, it is not ſo in regard to the Latitude, by any certain Method, but what is looked on as too abſtruſe for common Practice; for it is but once in 24 Hours that an Obſervation can be made from the Sun, and even that Space of Time ſo very ſhort, that if the Horizon ſhould then be obſcured, or a Cloud intercept the Rays of the Sun, the dead Reckoning

is

is then the only Guide, which, in Fact, is little better than groping in the Dark.

Since the Latitude then is our principal Guide at present, and liable to these Obstructions, it would be unnecessary to enlarge on the Advantages that would accrue to Navigation from Improvements tending to obviate them. As this Invention removes a very material Obstacle, *viz.* an obscure Horizon, there remains another, which, I hope and believe, is not altogether impracticable to remove; and that is, being confined but to one short Space of Time for Observation, as already mentioned; and doubtless it would be of great Advantage to Navigation, could an accurate Method be found for discovering the Latitude as frequently in the Day, as you may that of the Variation.

But to return to the Instrument under Consideration, which is founded on this obvious Principle, *viz.* "That the Surfaces of all Liquids, "that have a Communication with each other, though separated at any "Distance in their Surfaces, will be in a true horizontal Plane."

The first Instrument that I made conformable to this Principle, was with a Water-Level; but finding that Water was subject to some Inconveniencies, I altered the Apparatus, and changed the Fluid from Water to Mercury: This Alteration and Improvement will more intelligibly appear by the Figure of the Instrument, where A B, C D, represents Fig. 110. the Segments of two different Circles that are concentrick; E, the common Centre, in which moves the Pin or Axis fitted to the Index or Label E F; on which Label is also fixed the horizontal Tube G *g*, which has a Communication with the Two Glass vertical Tubes E *b*, *d b*, in which moves the Mercury. On each Top of the vertical Tubes are fixed a large hollow brass Cylinder *b b*, having in their Tops a Pin, by closing of which, the included Air is prevented from any Communication with the External; by which means this Advantage is obtained, that it prevents, in a great measure, that too quick and vibratory Motion, that is natural to the Fluidity joined to the Gravity of Mercury when moved, and at the same Time, by having a sufficient Space and Quantity of Air in the Cylinders at Top, does not in the least impede the true Level; but notwithstanding this Precaution, the Mercury still would be subject to a tremulous Motion, were it not that the Diameters of the vertical Tubes, to that of the horizontal, are as 2 to 1, and consequently the Area 4 to 1; by which means this Inconveniency is also removed, without any way affecting the horizontal Level.

The first trimming or preparing the Tubes with Mercury is sufficient, and when the two little convex Surfaces of the Mercury appear just visible above the level Rings E *e*, then is the Instrument correctly trimmed; if they appear much above or below the Rings, move the Tubes a little up or down, till the Surfaces are adjusted to the Rings; which is effected by means of the regulating Screw *l*, fixed at the End of the Base Tube.

As I well know the Fondneſs our Navigators have to *Davis*'s Quadrant, I adapted the Apparatus to this Inſtrument, which is ſo far from being perplexing, that it becomes obvious at firſt View, and by which an Obſervation can be made with great Facility; for the Obſerver may place himſelf in the moſt convenient Part of the Ship, where there is the leaſt Motion and Wind to diſturb him, and ſitting on a Stool or the Deck, holding the Inſtrument with his left Hand under the Horizon-Vane E *z*, and his Right at the End of the Label F, with his Thumb thereon, keeping the Label on the ſame Height or Level with his Eye, bring the left convex Surface of the Mercury to appear juſt viſible above the central Ring E, and the Shade or *Speculum* of the Sun from the Solar Vane *k*, to coincide therewith on the central Line E *z*; and the Sum of Degrees and Minutes cut on the two Arches by the Vane *k*, and the End of the Label F, will give, as uſual, the Angle of the Sun's Co-altitude. As the Sun riſes, the Shade will fall below the central Line (the Surface in it's proper Place); and when it paſſes the Meridian, and falls, it will appear above, ſo that the End of the Label muſt be moved in the ſame manner as the Sight-Vane uſually is.

To obſerve by a Star, another Perſon muſt look through the Slit on the Horizon-Vane, and over the upper Edge of the Shade-Vane, and bring the Star to coincide therewith, proceeding in the ſame manner as before, with the Sun.

There are two very oppoſite Cauſes of an obſcure Horizon; the one proceeds from thick hazy Weather, and the other from fine clear and calm Weather, as I have often experienced at Sea: I have been running with a Freſh of Wind, ſometimes five, ſix, and ſeven Days together, the Diſtance of 2 or 300 Leagues, without an Obſervation; and on the ſixth, ſeventh, or eighth Day, it has proved ſtark calm and clear Weather, but the Sea ſo ſmooth, and ſo like in Colour to the Sky, that the Edge or Circle of the ſenſible Horizon could not be diſtinguiſhed therefrom, and conſequently no Obſervation to be made by the Inſtruments then in Practice.

By this Improvement to *Davis*'s Quadrant, the above Obſtacles are entirely removed; ſo that an Obſervation can be made off of Headlands, in Harbours, on Shore, and, in ſhort, any where that a Sight of the Sun, *&c.* can be obtained, without any regard had to the Horizon; and, what is peculiar to it, is, that the true Level will be preſerved, as well on the Top of the higheſt Mountain, as cloſe to the Surface of the Horizon. The Apparatus is ſo contrived, that an Obſervation can be made with the ſenſible Horizon as uſual, by means of the Sight-Vane N, fixed near the End of the Label for that Purpoſe, ſo that the one will be a Proof to the other.

As the Succeſs of Inventions in all things of this kind muſt be confirmed by Experiments only, among many others, two were effectually made on board his Majeſty's Ship *the Oxford* at *Spithead*, in a high Wind when the Motion was ſhort and quick, and conſequently, a greater

Diſadvantage

Disadvantage than if on the high Sea, where the Motion is grave, flow and regular, occasioned by long Waves; but notwithstanding this quick Motion, the Observation made, exactly agreed with the Latitude of the Place; as will more evidently appear by the Report hereunto annexed, signed by all the Principal Officers that were then on board.

THE new Improvement made by Mr *Charles Leigh* to *Davis's* Quadrant, consisting of a Mercurial Level, for taking the Sun or Stars Altitude at Sea, when the sensible Horizon is obscured either by thick and hazy Weather, or in smooth Calms, when the Sky and Horizon are not distinguishable, was tried on board this Ship, when the Latitude by Observation made with the said Instrument agrees, as appears by the following Calculations; *viz.*

March the 9th, high Winds, and a quick Motion.	°	′	*March* 10th, ditto Weather.	°	′
Sun's Zenith Dist. - -	50	30	Zenith Distance - -	50	38
Sun's Declination - -		15 S.	Declination - - - -		9 N.
Latitude by Observ.	50	45	Latitude by Observ.	50	47

From which Experiment we judge this Instrument sufficiently accurate for discovering the Latitude, and removing that grand Impediment that frequently happens by an obscure Horizon, and consequently to be of great Use in Navigation.

From on board his Majesty's
Ship Oxford, *at* Spithead,
March 10. 1738.

Signed,

Thomas Strachey, *First Lieutenant.*
Thomas Griffin, *Lieutenant,*
James Irving, *Master.*
William Slanning, *second Master.*

Note, The Latitude of *Spithead* the nearest is about — — — — — — } 50 46 North.

THE Alteration made in this Instrument is greatly for the better, for the Level of Water required to be trimmed every time of Observation, besides the Hazard of spilling the Water from a great Motion; but *Directions concerning the Quadrant, &c.*

but in this Level of Mercury, the firſt Trimming ſerves always, and without hazard of ſpilling, being cloſe confined, as will be ſeen in the Inſtrument. ——The Cylinders are made large enough to receive the Air that will be condenſed and rarefied alternately by the vibratory Motion of the Quickſilver through the ſmall glaſs Tubes, without affecting the true Level Line, as will be found upon Trial: Notwithſtanding, the included Air has no Communication with the External, it's being cloſe confined gives this Advantage, that it prevents the Mercury, in it's vibratory Motion, from being quick and tremulous.

The Bottoms of the braſs Cylinder that the glaſs Tubes are fixed in, muſt in the Inſide be made Tunnel-wiſe, that the Mercury may not lodge behind. The Hole at the Top, and the Pin, is for taking out or putting in Mercury, if Occaſion; as alſo to clean the Tubes with a Wire. The perpendicular Tubes muſt at leaſt be twice the Diameter of the long Baſe Tube, for this Reaſon among others, that the dilating and condenſing of the Mercury, from Heat or Cold, may not be ſenſible in the perpendicular Tubes; and alſo that the Baſe Tube muſt be as long as the Index or Label will admit, and the Tube thereof to be as ſmall as can be, but ſo as to admit a Paſſage for the Mercury. This Paſſage ſhould be through a ſmall Glaſs Tube incloſed in Wood, *&c.* The Cylinders muſt not be ſoldered with ſoft Solder nor Silver: The Mercury will affect it.

Note, If the Mercury ſhould be ſeparated by an Air-Bubble in the Tube, incline the Inſtrument till the Mercury diſappers in the Tube below the Baſe, and it will take it out. The true Level is when the little convex Surfaces of the Mercury juſt appear above the Level Rings; then it is rightly trimmed; and when you obſerve, you look only at one of them, *viz.* that at the Centre, the Shade-Vane co-inciding at the ſame Time on the Horizon-Vane.

March 11. 1738.

An Account of Mr Thomas Godfrey's *Improvement of* Davis's *Quadrant, transferred to the Mariner's Bow, communicated to the* Royal Society, *by Mr* J. Logan. No 435. p. 441. *dated* Philadelpha June 28. 1734.

XII. Being informed that this Improvement, propoſed by *Thomas Godfrey* of this Place, for obſerving the Sun's Altitude at Sea, with more Eaſe and Expedition than is practicable by the common Inſtruments in uſe for that purpoſe, was laſt Winter laid before the *Royal Society*, in his own Deſcription of it; and that ſome Gentlemen wiſhed to ſee the Benefit intended by it more fully and clearly explained: I, who have here the Opportunity of knowing the Author's Thoughts on ſuch Subjects, being perſwaded in my Judgment that if the Inſtrument, as he propoſes it, be brought into Practice, it will in many Caſes be of great Service to Navigation, have therefore thought it proper to draw up a more full Account of it, than the Author himſelf has given, with the Advantages attending it; which if approved of by better Judgments, to whom what I offer is entirely ſubmitted, 'tis hoped the Uſe of it will be recommended ed and further encouraged, as alſo the Author. The Riſe of the Improvement with it's Conveniencies, as alſo a Deſcription of it, are as follows.

Tho. Godfrey, having under the greateſt Diſadvantages (as I obſerved in my firſt Letter to Dr *Halley*, giving an Account of his Invention of the Reflecting Inſtrument) made himſelf Maſter of the Principles of Aſtronomy and Optics, as well as other Parts of Mathematical Science, applied his Thoughts to conſider the Inſtruments uſed in that moſt momentous Part of Buſineſs, Navigation. He ſaw that on the Knowledge of the Latitude and Longitude of the Place a Ship is in, the Lives of thouſands of uſeful Subjects, as well as valuable Cargoes, continually depend; that for finding the firſt of theſe, certain and eaſy Methods are furniſhed by Nature, if Obſervations be duly made: But *Davis's Quadrant*, the Inſtrument generally uſed by Britiſh Navigators, (tho' ſeldom by Foreigners) he perceived was attended with this Inconveniency, that the Obſerver muſt bring the Shade or Spot of Light from the Sun, and the Rays from the Horizon, to coincide exactly on the fiducial Edge of the horizontal Vane; That tho' this can be done in moderate Weather and Seas with a clear Sky, and when the Sun is not too high, without any great Difficulty; yet in other Caſes it requires more Accuracy than can in ſome Junctures poſſibly be applied, and more Time than can be allowed for it. In *European* Latitudes, or to thoſe nearer the Northern Tropick, when the Sun is in the Southern Signs, and near the Meridian, he riſes and falls but ſlowly: Yet in Voyages to the *Eaſt* and *Weſt-Indies*, of which very many, eſpecially to the latter, are made, he is at Noon, often and for many Days together, in or near the Zenith, and when approaching to, or leaving it, he riſes and falls, when he has Declination faſter than even at the Horizon; for it is well known to Perſons acquainted with the Sphere, that when his diurnal Courſe takes the Zenith, he there riſes and falls a whole Degree or 60 Minutes, in the Space of $4'$ Time; ſo that the Obſerver has but $1'$, to come within $15'$ of the Truth in his Latitude: While in a middle Altitude, as 45° he is at Noon above $5'\frac{1}{2}$ in Time, in riſing or falling one ſingle Minute of Space, the Odds between which is more than 80 to 1. And yet, perhaps, no Parts of the World require more Exactneſs in taking the Latitude than is neceſſary in Voyages to the *Weſt-Indies*: For it is owing to the Difficulty of it, that Veſſels have ſo frequently miſſed the Iſland of *Barbadoes*, and when got to the Leeward of it have been obliged to run down a 1000 Miles further to *Jamaica*, from whence they can ſcarce work up again in the Space of many Weeks, againſt the conſtant Trade-Winds, and therefore generally decline to try for, or attempt it.

But farther, as the Latitude cannot be found by any other Method, that our Mariners are generally acquainted with, than by the Sun or a Star on the Meridian: In a cloudy Sky, when the Sun can but now and then be ſeen, and only between the Openings of the Clouds for very ſhort Intervals, which thoſe who uſe the Sea know frequently happens: As alſo in high tempeſtuous Seas, when tho' the Sun ſhould appear, the

 Obſerver

Observer can scarce by any Means hold his Feet; it would certainly be of vast Advantage to have an Instrument by which an Observation could also be, as it were, snatched or taken in much less Time, than is generally required in the Use of the common Quadrant.

Tho. Godfrey therefore considering this, applied himself to find out some Contrivance by which the Necessity of bringing the Rays from the Sun, and those from the Horizon to coincide (which is the most difficult part of the Work) on one particular Point or Line from the Centre, might be removed. In order to which he considered, that by the 21. 3^{d} *Elem. of Eucl.* all Angles at the Periphery of a Circle, subtended by the same Segment within it are equal, on whatever part of the Circumference the angular Point falls; and therefore, if instead of a Quadrant, a Semicircle were graduated into 90 Degrees only, accounting every two Degrees but one; this would effectually answer: For then, if an Arch of the same Circle were placed at the End of the Diameter of the Instrument, every Part of that opposite Arch would equally serve for taking the Coincidence of the Rays above-mentioned. But such an Instrument would manifestly be attended with great Inconveniencies; for it would in great Altitudes be much more unmanageable, and the Vanes could not be framed to stand, as they always ought, perpendicular to the Rays. He therefore further resolved to try whether a Curve could not be found to be placed at the Centre of a Quadrant, which would, at least for a Length sufficient to catch the Coincidence of the Rays, with Ease fully answer the Intention.

A Curve that in all the Parts of it would in Geometrical Strictness effect this, cannot be in Nature, any more than that one and the same Point can be found for a Centre to different Circles, which are not concentric. It is certain that every Arch on the Limb may have a Circle that will pass through the Centre, and be a Locus or geometrical Place for the Angle made by that Arch to fall on: but then every Arch has a different one from all others; as in the Figure. Let A B C be the Quadrant, and A B, E F, G H be taken as Arches of it: Circles drawn through each two of these respectively, and through the Centre C as a third Point, will manifestly be such Loci or Places: For every Pair of these Points stand in a Segment of their own Circle, as well as on a Segment of the Quadrant; and therefore by the cited 21. 3^{d} *Elem.* the Angles standing on these first Segments will every where be equal at the Periphery of their respective Circles, and their Radius will always be equal to half the Secant of half the Arch on the Quadrant. For in the Circle C E D F (for Instance) the Angle C E D is right, because 'tis in a Semicircle, C E is the Radius of the Quadrant, E D the Tangent of the Angle D C E = ½ the Arch E F, and C D is the Secant of the same = the Diameter of the Circle CEDF, and therefore it's Radius is half that Secant.

Fig. 111.

Now from the Figure 'tis plain, that in very small Arches the Radius of their circular Place will be half the Radius of the Quadrant; that is, putting this Radius = 10, the other will be 5. And the Radius for the

Arch of 90, the highest to be used on the Quadrant will be the Square Root of half the Square of the Radius = Sine of 45 Degrees = 7.071, and the Arches at the Centre drawn by these two *Radii* are the Extreams, the Medium of which is 6.0355. And if a circular Arch be drawn with this Radius $\frac{1}{10}$th Part of the Length of it, that is, in an Instrument of 20 Inches Radius, the Length of one Inch on each Side of the Centre affording 2 Inches in the whole, to catch the Coincidence of the Rays on, which must be owned is abundantly sufficient, the Error at the greatest Variation of the Arches, and at the Extremity of these 2 Inches, will not much exceed 1′.

But in fixing the Curvature or Radius of this Central Arch, something farther than a Medium between the Extreams in the Radius is to be considered: For in small Arches the Variation is very small, but in greater it equally increases, as in the Figure, where it appears the Difference between the Angles A B C and A D C is much greater than the Difference between E B C and E D C, though both are subtended by the same Line B D: for their Differences are the Angles B A D and B E D. Fig. 112. Therefore this Inequality was likewise to be considered; and compounding both together, *Tho. Godfrey* pitched on the Ratio of 7 to 11, for the Radius of the Curve to the Radius of the Instrument, which is 6.3636 to 10. But on further Advisement he now concludes on 6 $\frac{4}{10}$; and a Curve of this Radius of an Inch on each Side of the Centre to an Instrument of 20 Inches Radius or of $\frac{1}{20}$th of the Radius, whatever it be, will in no Case whatever, as he has himself carefully computed it, produce an Error of above 57″; and 'tis very well known that Navigators (as they very safely may) in their Voyages entirely slight a Difference of one Minute in Latitude.

This Radius is the true one for the circular Place to an Arch of 77° 15′, and the Variation from it is nearly as great at 90 Degrees as at any Arch below it, the greatest below being at about 44°, which is owing to the Differences expressed by the last Figure above, and not to those of the Curvatures or circular Places. Yet this Variation of 57″ arises only when the Spot or Coincidence falls at the Extremity of the horizontal Sight or Vane, or a whole Inch (in an Instrument of 20 Inches Radius) from the Center, and then only in the Altitudes or Arches of about 44 or 90°. And in these, at the Distance of ½ an Inch from the Center, the Variation is but ¼ so much, *viz.* about 14″; and at ¼ of an Inch, not 4″; at the Center 'tis precisely true. Therefore as an Observation may be taken with it in ⅓ of the Time, that *Davis*'s Quadrant, on which three Things must be brought to meet, in a general way requires: I say, considering this, and the vast Importance of such Dispatch, in the Case of great Altitudes, or of tempestuous Seas, or beclouded Skies, 'tis presumed the Instrument thus made will be judged preferable to all others of the kind yet known. Some Masters of Vessels, who sail from hence to the *West-Indies*, have got of them made as well as they can be done here; and have found ſo great an Advantage in the Facility and in the ready Uſe of them, in thoſe Southerly Latitudes, that they reject all others. And it can ſcarce be doubted, but when the Inſtrument becomes more generally known, it may, upon the *Royal Society*'s Approbation, if the Thing appear worthy of it, more univerſally obtain in Practice.

'Tis now 4 Years ſince *Tho. Godfrey* hit on this Improvement; for his Account of it, laid before the Society laſt Winter in which he mentions two Years, was written in 1732. And in the ſame Year, 1730, after he was ſatisfied in this, he applied himſelf to think of the other, *viz.* the reflecting Inſtrument by Speculums, for a help in the Caſe of Longitude, though 'tis alſo uſeful in taking Altitudes, and one of theſe, as has been abundantly proved by the Maker, and thoſe who had it with them, was taken to Sea and there uſed in obſerving the Latitude, the Winter of that Year, and brought back again hither before the End of *February*, $17\frac{30}{31}$, and was in my keeping for ſome Months immediately after. It was unhappy indeed, that having it in my Power, ſeeing he had no Acquaintance nor Knowledge of Perſons there, that I tranſmitted not an Account of it ſooner: But I had other Affairs of more Importance to me: And it was owing to an Accident which gave me ſome Uneaſineſs, *viz.* his attempting to publiſh ſome Account of it in Print here, that I did it at that Time, *viz.* in *May* 1732, when I tranſmitted it to Dr *Halley*; to whom I made not the leaſt Doubt but the Invention would appear entirely New. This, on my part, was all the Merit I had to claim, nor did I then, or now aſſume any other, in either of theſe Inſtruments. I only wiſh that the ingenious Inventor himſelf might by ſome means be taken Notice of, in a Manner that might be of real Advntage to him.

There needs not, I ſuppoſe, much more of a Deſcription of the Inſtrument than has been given: I ſhall only ſay, the Bow had beſt be an Arch of about 100 Degrees, well graduated, and numbered both ways; the Radius of 20 or 24 Inches; the Curve at the Centre to be $\frac{1}{20}$th of the Radius on each Side, that is, $\frac{1}{10}$th of it in the whole; the Radius of that Curve $\frac{64}{100}$ Parts of the Radius of the Inſtrument; that the Glaſs for the Solar Vane ſhould not be leſs, but rather larger, than a ſilver Shilling, with it's Vertex moſt exactly ſet. And that the utmoſt Care be taken to place the Middle of the Curve at the Centre exactly perpendicular to the Line or Radius of 45 Degrees. As the Obſerver muſt alſo take Care that the two Vanes on the Limb be kept nearly equi-diſtant from that Degree; to which I ſhall only add, that it may be beſt to give the horizontal Vane only one Aperture, and not two. The reſt I ſuppoſe may be left to the Workmen.

Fig. 113. *Note*, That the Radius of the Quadrant being divided into 20 equal Parts, the Centre X of the Curvature of the Horizon-Vane (A B) muſt be 12 $\frac{8}{10}$ of thoſe Parts from the Centre (C) of the

the Quadrant. The Breadth (A B or *g h*) of that Vane should be $\frac{1}{10}$ of the whole Radius, that is, $\frac{1}{20}$ on each Side of the Centre (C).

XIII. The necessity of finding the Latitude a Ship is in, is too well known to be insisted on: Frequent Opportunities of observing the Latitude must consequently be of very great Advantage to Navigation. The Method usually practised, is by taking the Sun or Star's Meridian Altitude or Zenith Distance: In this Case, if the Sun does not shine but for some small Time only, before Noon and after, though it be clear all the rest of the Day, it is of no use for this Purpose. Mr *Fatio*, *F. R. S.* (in the Year 1728) proposed a Method for finding the Latitude, from two or more Observations of the Sun (or Stars) at any Time, the Distance of the said Observations in Time, being given by a Watch; but as his Method requires a vast Number of Computations, and a great deal of Skill in Spherical Trigonometry, it has very seldom been made use of, and never but by good Mathematicians. The Instrument here described will answer the same End, and has these Advantages; *viz.*

The Description and Use of an Instrument for taking the Latitude of a Place at any time of the Day; by Mr Richard Graham, *F. R. S. Ibid.* p. 450.

1st, It may be very easily understood by Seamen.
2dly, It immediately shews the Latitude of the Place.
3dly, It gives the Time of Day at Sea when no other Instrument can.
4thly, It may be made as large, and consequently as accurate as is desired.

A B C represents part of the Hemisphere of a large Globe (half the Globe, and the Part below the Tropick are cut off, that it may take up the less room). A C, half the Equator, divided into 12 Hours above, and 180 Degrees below, and subdivided into Minutes, as is likewise the lower Tropick D D. E E, a moveable graduated Meridian, turning on the Axis F F. G an Index to fix it (by the means of the Screw H) to any Hour. I *i* I, a circular Beam-Compass, the Centre I *i* to be fixed on the Meridian to any Degree and Minute of Declination, by the Method commonly called *Nonius*'s Divisions: *k* the Point for drawing Arches, which is likewise fixed to any Degree and Minute by the same Method. As the Meridian is at some Distance from the Globe, L is a piece of Brass to fix on the Meridian, marked with *Nonius*'s Divisions, with a Point reaching down to the Intersection of the Arches, by which means the Distance of the said Intersection from the Equator, or it's Latitude is found. The Degrees and Minutes may likewise be shewn by diagonal Lines.

A Description of the Instrument. Fig. 114.

Prop. I. *From two Observations of the Height of the Sun, the Distance of the said Observations in Time, being given by a Watch, as likewise the Declination of the Sun; to find the Latitude of the Place, and Hour of the Day.*

The Use of the Instrument.

 I. *When*

I. *When the Ship is at Rest, that is, at Anchor, or in a Calm, so as to have little or no progressive Motion.*

Case 1. Suppose the Sun in the Equator, on the Day of Observation: Fix the Centre of the Beam-Compass at 0 Degree (or at the Equator,) and move the Point *k* to the Zenith Distance, (the Complement of the Altitude, taken by the usual Instruments,) and from any Hour, as from C, describe an Arch of a Circle with the said Point, as *b c* (*Ex.* 1.) Suppose eight Hours after, by your Watch, you have another Observation; move the Meridian 8 Hours farther, to *d*, and fix it there; and with the Zenith Distance then observed, describe another Arch as *e f*, the Point where it cuts the former is the Place of Observation, and it's Distance taken on the Meridian from the Equator shews it's Latitude; and the Minutes reckoned on the Equator from the Meridian to C and *d* (the Times of Observation) shew what those Hours were.

Case 2. When the Sun has Declination: Fix the Centre of the Beam-Compass on the Meridian, to the proper Degree of Declination for the Day of Observation, and proceed as before.

Case 3. If the Observations are at a greater Distance than twelve Hours, but in the same Day: Make use of the Complement to twenty-four Hours of the Distance in Time, and take the Declination on the contrary, or lower Side of the Equator; and instead of the Zenith Distances, take the Nadir Distances or Altitudes increased by 90°.

Thus you will find the Latitude, and Time of each Observation from Midnight. In this Case the Beam-Compass must extend to more than 90°.

Case 4. If the Observations are more than a Day asunder; as for Instance a Day and 2 Hours (26 Hours): Place the Centre of the Beam-Compass 2 Hours farther than it was the Day before; but in different Declinations, according to the Table of Declination for the several Days.

Case 5. When the Observations are made by a Star: The Centre of the Beam-Compass must be set to the Declination of the Star; then proceed as before. To find the Hour in this Case, the right Ascension must be likewise given.

Scholium. The same Method may be useful at Land, when no Meridian Observation offers.

II. *The Ship in Motion.*

Case 1. Suppose the Sun in the Equator: The Distance between the two Observations 8 Hours, as before, and the Arch *a a a* (*Ex.* 2.) described by the Zenith Distance of the first Observation, from the Centre C; and the Angle *c a b*, 40 Degrees, is the Angle between the Ship's way, and the Azimuth of the Sun continued, (given by the Azimuth Compass) and that during the eight Hours, the Ship has made 1°, or 60′ from *a* to *b*, or from the Sun; then, as Radius is to the Cosine of *c a b* 40°, so is *a b* 60′ to *a c* 46′; add 46′ to the Zenith Distance C *a*; and with *k*, the Point of the Beam-Compass set at that Distance, describe the Arch *c b e*; then with the Zenith Distance of the last Observation, whose Centre is *d*, draw the Arch *f f*; the Point where it cuts the Arch *c b e*, is the Place where the Ship was last; and it's Distance taken on the Meridian from the Equator shews it's Latitude; the Minutes reckoned on the Equator from the Meridian to *d* (the Time of the last Observation) shew the Hour, or it's Distance from 12 o' Clock.

If the Ship had sailed from *a* to β or towards the Sun: The Cosine of the Angle β *a* γ, or of the Angle between the Ship's Way and the Sun, must be substracted from the Zenith Distance of the first Observation. Case 2.

N. B. Only the two Arches *c b e*, *f f*, are to be drawn on the Globe, the rest being added here, to shew the Reason of the Construction.

To find the Latitude of the first Place: From the Equator, with a pair of Compasses, take the Distance sailed 60′, and with one Foot in the Intersection of the Arches *b e*, *f f*, the Place found before, put the other in the Arch *a a a*, the Zenith Distance of the first Observation, and in this Instance, on the left Hand of the Azimuth of the Sun, this is the Place sought; and it's Distance taken on the Meridian from the Equator, shews the Latitude; and the Minutes reckoned on the Equator from the Meridian to C, the Time of the first Observation, shew the Hour. Case 3.

The Interval in Time or Degree between the two Places, shewn by the Index G, is the Difference of Longitude.

N. B. Those Observations are best, whose Arches cross each other almost at right Angles.

Prop. II. *The Zenith Distances of two Stars, observed at the same Time, their Declination, and right Ascension being known; to find the Latitude of the Place of Observation.*

Fix the Centre of the Beam-Compass to the Declination of either of the Stars, and with the Zenith Distance of that Star describe an Arch; move the Meridian as many Hours farther as is the Difference of right Ascension of the other Star; and fix the Centre of the Beam-Compass to the Declination of it; and with it's Zenith Distance cross the first Arch: The Intersection shews the Latitude of the Place of Observation; and also the Distance of the right Ascension of the Zenith from that of either of the Stars, by which means the Hour may be known.

If a Celestial Globe is made use of, then place the Centre of the Beam-Compass over the several Stars.

The Latitude and Hour being given, the Variation of the Compass is easily known.

N. B. In order to draw Arches on the Globe; rub some Black-Lead powdered on a Piece of Paper; lay the Side which is blacked next the Globe, where you imagine the Intersection of the Arches will be: Then draw them on the clean Side with the Point of the Beam-Compass, and they will appear on the Globe; and if the Globe is well varnished, they may be rubbed out with Bread, or washed out with Water.

As Altitudes at Sea are now readily taken, with great Exactness, by the Quadrant invented by *John Hadley*, Esq; *V. P. R. S.* and as the said Altitudes are the Principles on which the Operations above described are founded; the previous Use of that Quadrant cannot but be of the utmost Importance to those who shall have Occasion for this Instrument.

The Description and Use of this Instrument was laid before the *Royal Society*, *Dec.* 9. 1731; but as I knew Mr *Reid* was contriving one for the same Purpose, I delayed making mine Publick. His Method not yet appearing in Print, I have thought proper to communicate my own (especially as 'tis now improved) conceiving it may be of some Advantage to Navigation.

The Use of a new Azimuth-Compass for finding the Variation of the Compass or Magnetic Needle at Sea, with greater Ease and Exactness than by any ever yet contrived for that Purpose; by Captain Christopher Middleton, *F. R. S.* No. 450. p. 395. Oct. &c. 1738.

XIV. To discover the Declination of the Magnetic Needle, or Variation of the Compass at Sea, with some tolerable Degree of Certainty and Exactness, is a thing of great Use and Importance in the Art of Navigation.

The Instruments and Methods hitherto used for this Purpose, (as we could easily demonstrate, if it were needful) are subject to several Inconveniencies, Errors, and Defects; to remedy which, this new *Azimuth-Compass* was contrived, and has by Experience been found effectual. It would be needless to give a Description to such as have the Instrument before them, and we shall therefore only shew the Manner of using it, and that as briefly as may be, which take as follows:

1st, The Instrument must be rectified, or fitted for Observation, by turning it about till the four Cardinal Points, that are hung upon the Centre-Pin, agree with the four Cardinal Points on the Chart, at the Bottom of the Box: Then will the Needle, that shews the Magnetic Meridian, stand at no Degrees, and the East and West Points at 90°, on the graduated Circle within the Box; and in this Situation it must be kept, as near as may be, during the whole Time of the Observation.

2dly, Let the Index of the Quadrant be placed to that Degree of the Arch, on the Rim of the Box, which the Obſerver judges to be nearly equal to the Height of the Sun or Star whoſe *Azimuth* is ſought; for by this means the Object will be more readily found.

3dly, Turn the Quadrant round towards the Sun or Star, till it appear upon the vertical Hair within the Teleſcope, to an Eye looking through the ſmall Hole or Sight; and then ſlide the Index a little upward or downward on the Arch, till the Object by this means be brought to coincide or touch the viſible Horizon.

Laſtly, The Degrees and Minutes then marked by the Index upon the Arch of the Quadrant, will ſhew the Altitude of the Object, which will always be the ſame, whether the Inſtrument is in Motion or at Reſt; at the ſame time the Degree cut by the Index on the horizontal Rim or Circumference of the Compaſs-Box, will give the magnetical *Azimuth* of the Sun or Star.

N. B. All this may be performed by one Perſon, whereas the old Compaſs requires ſeveral to manage it, which alſo makes it ſubject to many great Errors.

How the Variation of the Needle is found by means of Magnetical *Azimuth* and Altitude thus obtained, is taught in every Treatiſe of Navigation, and we have no need to repeat theſe Rules in this Place. But as the Reſolution of this Problem is ſomewhat troubleſome, and requires ſuch a Knowledge of the Doctrine of the Sphere, as every Seaman has not attained, we ſhall here exhibit an eaſy Method of diſcovering the Variation of the Compaſs without any manner of Calculation, which cannot fail to render this Inſtrument ſtill more acceptable: To this End,

1ſt, Let the Magnetic *Azimuth* of the Sun (or any Star, when it is near the prime Vertical, and conſiderably elevated above the Horizon) be found according to the Directions already given, before it arrive at the Meridian, and note well the Altitude, or let the Index remain fixed at ſame Point on the Arch.

2dly, Find the Magnetic *Azimuth* of the Sun or Star in like manner as before, when it is exactly at the ſame Degree of Altitude, after it has paſſed the Meridian: And,

3dly, If theſe two Magnetical *Azimuths* are equal, the Needle has no Variation: If unequal, add them together, and half their Sum will be the true *Azimuth*; or ſubtract the leſs from the greater, and half the Difference will be the Variation required. The Circumſtances of the Obſervation will the more readily diſcover whether the Declination is Eaſterly or Weſterly.

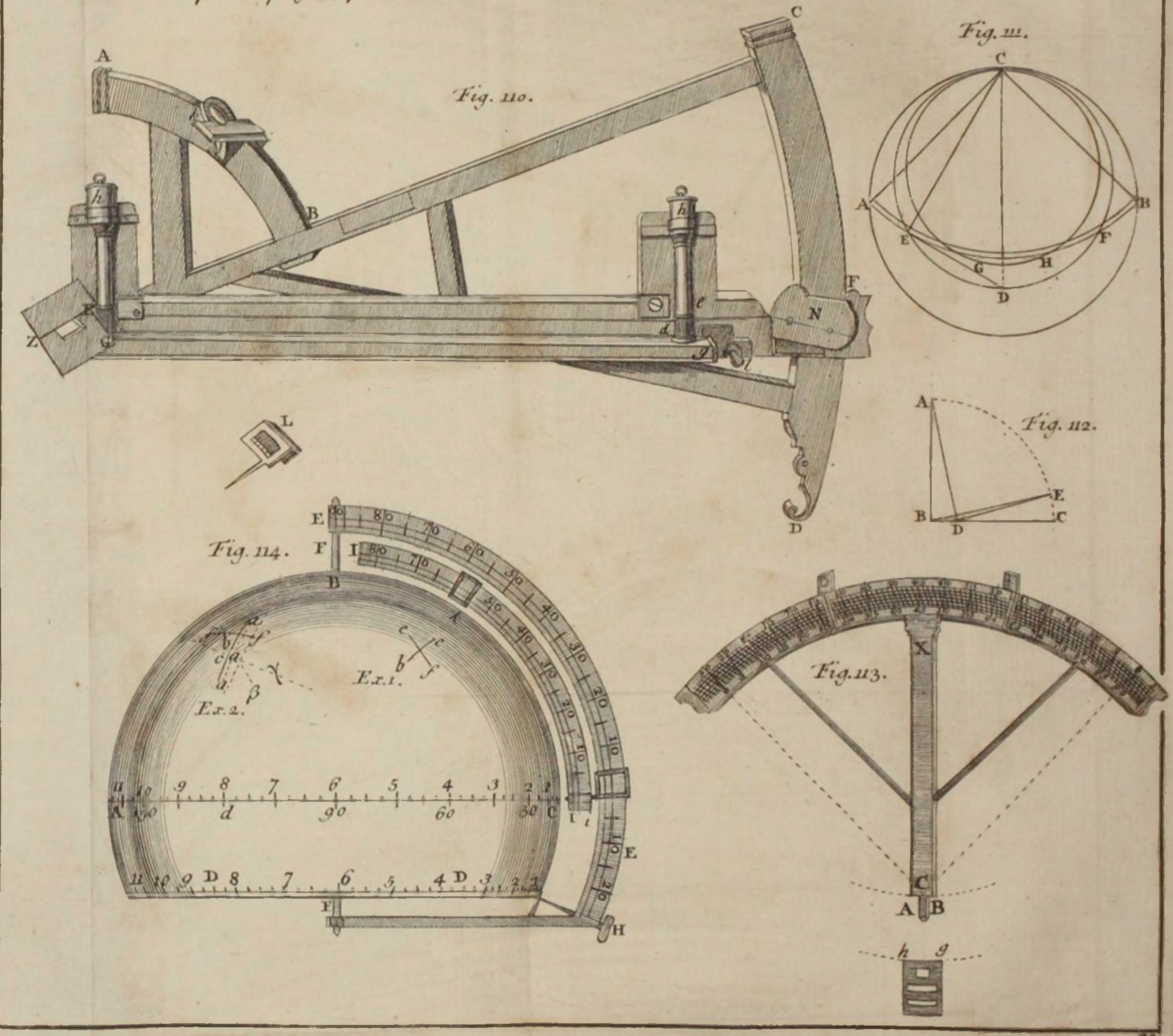
N. B.

N. B. Though it would be very commendable in Gentlemen who uſe the Sea, to learn the Names of moſt of the principal Fixed Stars, yet even that Knowledge is not neceſſary in the Uſe of this Inſtrument: Neither is it needful in this caſe to know exactly the Latitude of the Place of Obſervation, provided the Difference of Latitude between the Obſervations be not very great: It is ſufficient, that Care be taken to obſerve the ſelf-ſame Star, before it comes to the Meridian, and after it has paſſed it; and for the ſake of greater Exactneſs, the Caution before given ſhould be regarded, to wit, That the Star be at ſome conſiderable Height above the Horizon, and alſo near the prime Vertical.

XV. *(See the folded Sheet.)*

The End of the FIRST PART.

Pla. XX. Vol. VIII. part 1. page 376.



XV.

Observations made of the Latitude, Variation of the Magnetic Needle, and Weather, by Capt. Christopher Middleton, in a Voyage from London to Hudson's-Bay, Anno 1735. No. 442. p. 270. July, &c. 1736.

Month.	Day.	Hour.	Altitude of the Thermometer.	Altitude of the Barometer.	Latitude by Account.	Longitude from London by Account.	Variat. of the Comp.	Latitude observed by Smith's Quadrant.	Latitude observed by Hadley's Quadrant.	Latitude observed by Elton's Quadrant.	Latitude by a Seastaff of Ward and Smith.	Winds.	Remarks.
			° ′	° ′	° ′	° ′	W.	° ′	° ′	° ′	° ′		
May	31	6 12 9	27 24 29.7	29.9 29.6 29.7	60.0	19.11	23					S W to S E	Variable, with some Rain.
June	1	6 12 9	29.7 25 25	29.7 29.5 29.5	59.50	23.33	24					S E to S W	Variable the first two Parts, moderate the latter, fresh Gales and a great Sea.
	2	6 12 9	26 26 27	29.4 29.3 29.8	59.55	25.53	25	59.54	59.59			S W to S by E	Cloudy, and squally with Rain.
	3	6 12 9	28 26 27.9	29.9 28.8 30.1	60	28.3	26	59.57	60			S W	Frequent Showers of Rain, with Squalls.
	4	6 12 9	29 26 27.9	30.5 29.8 30.1	60.40	29.25	26	60.39	60.41			S W	Uncertain squally Weather, with a Western Swell.
	5	6 12 9	28 26 25	30 29.8 29.7	60.31	32.12	27					S W	Squally, with some Rain.
	6	6 12 9	28 25 24	29.7 29.5 29.5	58.44	35.46	27	58.47	58.50			S W to N N W	Variable, with Squalls of Rain.
	7	6 12 9	28 28 30	29.5 29.9 29.8	58.11	36.58	28	58.8	58.11			N N W to W N W	Variable fresh Gales, with Rain and Squalls.
	8	6 12 9	30 23 26	29.9 29.8 29.6	57.50	38.33	28	57.53	57.57			N N W to W by S	Moderate and fair Weather, with Clouds.
	9	6 12 9	26 25 26	29.9 28.9 29.6	58.28	42.16	28	58.28	58.38			W by S to S by E	Rain and fresh Gales.
	10	6 12 9	31 28 26	29.8 30 30.4	58.25	43.11	29	58.16	58.22			N W	Hard Gales, with a great Sea.
	11	6 12 9	30 27 28	29.8 29.8 29.9	58.48	44.16	29	58.40	58.45			N W to N E	Moderate, with some Rain.
	12	6 12 9	30 28 28	30 29.6 29.8	58.22	45.46	29					N N E to W by S	At 6 made Cape *Farewell*, from N b W to N b E 14 or 15 Leagues dist. Moderate with some Rain.
	13	6 12 9	27 27 32	29 29.7 29.9	58.12	45.37	29	58.10	58.13			N W	Much Ice in Sight. An hard Gale, with a great Sea.
	14	6 12 9	31 26 29	30.1 29.9 30.23	58.12	46.11	30	58.10	58.11			N N W to E S E	Most part hard Gales. Under Main Sail and Mizen.
	15	6 12 9	29 29 27	30.5 30.1 10	59.7	49.49	31					E S E to S E	Saw a large Isle of Ice. Pleasant Weather.
	16	6 12 9	26 27 24	30.1 30.1 29.6	60.14	55.3	33					S E by S	Fresh Gales and cloudy, with some Rain.
	17	6 12 9	[illegible] [illegible] [illegible]	[illegible] 29.9 29.9	61.7	59.43	36					S by E to S E by S	Moderate, but hazy, with Rains.
	18	6 12 9	31 23 27	29.9 28.5 29.7	61.23	63.7	39					S E	Hazy, with Rain and Fogs. Several Pieces of Ice.
	19	6 12 9	27 31 30	29.35 50 29.9	61.47	63.16	39	61.44	61.49	61.54		S E S S W S by E	Foggy, with Calms. Much Ice all round *Resolution*.
	20	6 12 9	28 29 30	30.1 30 29.8	61.40	64.8	39			61.46		W N W Wt S W b S	Moderate Weather, most part Foggy.
	21	6 12 9	29 29 29	29.8 30.6 30.8	61	64.34	39	61.4	61.5	61.12		Wt W N W N by W	Much Ice all round; fore Part Foggy, latter clear.
	22	6 12 9	32 27 28	29 29.4 29.9	61.11	65.40	40		61.	61.12	61.12	N by W	The most part foggy and hazy Weather.
	23	6 12 9	30 27 	29.9 29.8 	61.30	66.50	40			61.30	61.30	So S W	Fair, with much shattered Ice, and large Isles.
	24	6 12 9	 24 26	 29.8 29.6	61.37	67.19	41	61.36		61.38		W N W	Easy Breezes, fair and clear, heavy Ice all round.
	25	6 12 9	28.5 27 28	29.9 29.6 29.8	61.53	68.12	42					Ditto	Hazy middle Part, clear the latter.
	26	6 12 9	28 27 30	.9.7 29.8 39.9	61.41	68.42	41					S W b W	Fair pleasant Weather; much Ice.
	27	6 12 9	 29 29	 29.7 29.6	61.30	68.42	41					N N E W S W	Little Winds, and fair pleasant Weather.
	28	6 12 9	31 29 29	29.8 29.6 29.75	61.33	69.2	40						Most Part Foggy, with Calms.
	29	6 12 9	28 27 25	29.5 29.6 29.75	61.30	69.13	40	61.30		61.30	61.20	E S E	Several large Isles of Ice; Foggy.
	30	6 12 9	26 26.5 27	29.9 29.5 19.9	61.33	69.26	40					E S E to W S W	Little Winds and Calm; heavy Ice all round.
July	1	6 12 9	29 27.5 27.5	29.9 29.7 29.6	61.28	70.10	41					E S E	Fast in thick Ice, with Fogs and Rain.
	2	6 12 9	32 31 26.5	29.1 29 29.3	61.28	70.37	41					S E to N E	These 24 Hours the first and latter Parts fresh Gales, middle a Storm of Wind and Rain.
	3	6 12 9	29.5 28 27.5	29.5 29.8 29.9	61.28	70.22	41					N W	Hard Gales, with much Snow.
	4	6 12 9	32 29.5 25.5	29.9 29.6 29.3	61.28	70.26	40					N W N by W	Moderate and fair Weather with much Ice.
	5	6 12 9	29 27 29	29.8 29.9 29.7	61.39	70.36	40	61.33		61.44	61.43	W by S	Working to Windward in Ice, sometimes in a Clear.

Month.	Day.	Hour.	Altitude of the Thermometer.	Altitude of the Barometer.	Latitude by Account.	Longitude from London by Account.	Variat. of the Comp.	Latitude observed by Smith's Quadrant.	Latitude observed by Hadley's Quadrant.	Latitude observed by Elton's Quadrant.	Latitude by a Seastaff of Ward and Smith.	Winds.	Remarks.
			° ′	° ′	° ′	° ′	W.	° ′	° ′	° ′	° ′		
July	6	6 12 9	31 27.5 26	29.9 29.7 29.7	61.46	70.54	40		61.54		61.53	W S W to N E b N	Most Part fair and pleasant Weather, with much Ice.
	7	6 12 9	27 27 27	29.4 29.6 29.9	61.55	71.13	40					N E to N W b N	Foggy Weather, close Ice all round.
	8	6 12 9	29 26 25.5	29.7 29.75 29.9	61.46	70.21	40					Ditto.	Moderate, with Fogs.
	9	6 12 9	29 27 27	29.9 30 30	61.42	71.40	41			61.38		Ditto.	The first Part hazy, the latter clear.
	10	6 12 9	32.5 29.5 26	29.7 29.6 29.75	62.18	75.13	41					N W to S E	Clear Sea, fresh Gales and hazy.
	11	6 12 9	31 27 28	29.7 29.9 29.75	62.31	76	42					S by W	Foggy Weather.
	12	6 12 9	31.5 31.5 28	29.9 29.9 30	63.5	75.28						Ditto.	Ditto.
	13	6 12 9	30 29 27.5	30.2 30.3 29.9	63.15	75				63.20		S to N W	Working in loose Ice; hazy, sometimes clear.
	14	6 12 9	31 27 25	30 30 30	63.25	74.38						N W to West	Much Ice, fair and clear, with Calms.
	15	6 12 9	30 27 27	29.9 29.8 29.8	63.20	76						S S E to E S E	Much Ice, fresh Gales and hazy.
	16	6 12 9	27 26 26.5	29.8 29.7 29.7	63.10	77.20						S E	Loose Ice, large Clears in Sight of *Salisbury*.
	17	6 12 9	27.5 27.5 26	29.7 29.6 29.7	63.10	78.20	44					E S E	Foggy, much Ice and fresh Gales.
	18	6 12 9	29 26 26	29.6 29.8 29.8	63	78	43					Calm.	Loose Ice, East End of *Nottingham*, N b E seven Leagues.
	19	6 12 9	25 24 27	29.9 29.85 29.80	63.10	78.20	43					E S E	Much Ice, clear Weather. Cape *Walsingham* S W b W five or six Leagues.
	20	6 12 9	28 27.5 27	29.85 29.84 29.9	63.20	78.30	41					S W	Shattered Ice, foggy West; Island *Diggs*, W S W five Leagues.
	21	6 12 9	27.5 25.5 24	29.98 29.9 29.8	63.10	79.50	42					S S E S by W	Cape *Diggs* S E b S 6 Leagues. Fresh Gales, and Ice.
	22	6 12 9	27 25 23.5	29.9 29.9 30.1	63	80.13	42					S W b W to South.	Much large Ice; the North End of *Mansfield* S W b W four Miles.
	23	6 12 9	26.5 26 25	30.1 30 30	63.4	81.16	44			62.52		S W	North End of *Mansfield* S W fourteen Miles.
	24	6 12 9	29.5 29.5 26	29.9 29.9 29.85	61.34	82.52	40			61.26		E by S to S S E	Hazy; some Pieces of Ice.
	25	6 12 9	31 28 26	29.82 29.82 29.83	61.15	83.56	38			61.14	61.17	S S W S by E	Much Ice, and Rain.
	26	6 12 9	32 29 29.5	29.7 29.7 29.9	60.45	84.23				60.41		S by E to W by N	Thick foggy Weather, with Showers of Rain.
	27	6 12 9	30.5 26 26	29.9 29.9 30.1	59.39	84.11	30	59.36		59.34		N W to West.	Steering Ice with wet Fog.
	28	6 12 9	28 26 26	30.1 30.1 30.1	58.9	82.44	28	58.5	58.2			N by W	*Baker's Dozen* S b W four Leagues. Fair Weather.
	29	6 12 9	28 26 25	30 30 29.95	56.46	82.45	26			56.30		W by S	Moderate, with Fogs, Rain and Ice.
	30	6 12 9	27.5 25.5 23	29.85 29.9 30	55.51	82.39	25					N W b W	Fresh Gales. A great Sea from the Southward.
	31	6 12 9	26 25.5 24	29.9 29.5 30	54.44	82.47	24					N E	Thunder and Rain. *North Bear* S W by W five Miles.
August	1	6 12 9	26 25.5 23	30 29.98 29.85	53.3	81.20	22					N E	Moderate and fair Weather.
	2	6 12 9	24 23.5 24	29.8 29.9 29.9	52.20	82						South to North	The first Part moderate, the latter hard Gales, with Thunder and Lightning in with the W. Main.
	3	6 12 9	 26 	 30 	51.40	83							Arrived in *Moose River* Road.
	4	6 12 9											
Sept.	1	12	27	29.85	55.36	West. 00.42	22					West and W by N	Moderate and fair.
	2	9 12 9	29 27 26	29.85 30.5 30.8	53.50	from *Bear*. 1.16	26					N W to North.	Moderate Gales and Hazy, with small Rain.
	3	9 12 9	30 26 23	29.8 30.4 31.5	56.6	1.16	26					N by W to N by E	Ditto.
	4	9 12 9	30 26 23	29.8 30.6 31	58.6	2.58	30					South to East.	The first Part moderate, middle and latter very hard Gales, with Squalls of Rain.
	5	9 12 0	33 33 34	29.8 29.85 30	58.28	4.26	30					N E b E	Ditto, hard Gales and Squalls.
	6	9 12 9	33 32.5 32	30.1 30.2 30.2	58.40	3.23	31	58.42	58.49			E N E North.	Fresh Gales and squally, with a Head Sea.

Month.	Day.	Hour.	Altitude of the Thermometer.	Altitude of the Barometer.	Latitude by Account.	Longitude from *London* by Account.	Variat. of the Comp.	Latitude observed by *Smith's* Quadrant.	Latitude observed by *Hadley's* Quadrant.	Latitude observed by *Elton's* Quadrant.	Latitude by a Sextant of *Ward* and *Smith*.	Winds.	Remarks.
			° '	° '	° '	° '	W.	° '	° '	° '	° '		
Sept.	7	9 12 9	27 27 26	29. 9 29. 8 30. 1	60.45	1.15	31					N W	Ditto, Gales and Snow.
	8	9 12 9	37. 5 35 32	30. 3 30. 2 30. 1	61.37	East. 0. 4	32					S W to N W	Strong Gales and squally; great Sea from the North-West.
	9	9 12 9	34 32. 5 32	30. 3 30. 3 30. 5	61.55	East. 0. 4	33					N W to N N E	Moderate, and fair; four Leagues from the South End of *Mansfields* 76 Fathom, Mud.
	10	9 12 9	32 29 30	30. 5 30. 5 30. 5	61.58	West. 1. 0	34	61.43	61.45	61 46		N by E S W	Little Winds and fair Weather In Sight of *Mansfields*.
	11	9 12 9	36 30 28	30.25 30. 1 30	62.30	East. 2.21	43	61.30				S W	Fair and pleasant Weather. In Sight of *Mansfields*, 4 Miles.
	12	9 12 9	30 30 30	30. 1 30 30. 1	62.52	East. 2.28	42					W S W to W N W	At Noon *Mansfields* N W by W 5 Leagues; Sleet and Foggy. Cape *Walsingham* S W 6 Leagues.
	13	9 12 9	32 33 32	30. 3 30.25 30. 2	63. 9	*fr. Digs* 3.40 East.	43					N W to N by E	Frequent Showers of Hail and Snow.
	14	9 12 9	31. 5 29 31	30. 1 30. 5 30	62.50	East. 6.44	42					N N E Calm.	Moderate and fair Weather.
	15	9 12 9	30. 5 30 29	29. 5 29.46 29. 5	62.22	7.55	42	62.21	62.23			S W to W N W	Much Snow, with thick Weather.
	16	9 12 9	29 30 28	29. 8 30 30. 3	61.43	12	42					N W to N by E	Passed several large Isles of Ice; fresh Gales and Sleet; thick Weather with Snow.
	17	9 12 9	32. 5 33 31	30. 4 30. 5 30.55	61.43	15.10	40					North. to N N E	First 2 Parts hard Gales and Snow, latter moderate. At Noon Cape *Resolut.* NE6L. S. Pt. NWbN4L.
	18	9 12 9	31 34 31	30. 4 30. 3 30 4	60 40	West. 58.40	40					N E b N	Many large Isles of Ice; hard Gales; great Sea from the North-East.
	19	9 12 9	23 26 26	29 9 29. 5 29. 5	59.49	53 43	38					North. to S W	Moderate, but dark and cloudy, with Sleet and Rain.
	20	9 12 9	30 29 29	29 7 29 65 29. 6	58.42	48.52	32	58.37	58.38			N W	Fresh Gales, with Rain and Snow.
	21	9 12 9	31 30 29	29. 5 29. 6 27. 7	57 28	44.31	29					N N W to North.	Variable, with fresh Gales, and Rain.
	22	9 12 9	27. 5 27. 5 27	29. 5 29. 5 29. 6	56.49	41.39	27					North. to E S E	Fresh Gales and Squalls; variable Winds and cloudy.
Sept.	23	9 12 9	29 26. 5 26	29.65 29. 5 29. 7	55.52	36.50	25	55.44	55.47			N E b E to N by W	Hard Gales, with Rain and Fogs.
	24	9 12 9	31 27 26	29. 8 39. 9 29. 8	55. 3	31.21	24	55. 1	55. 4			N N W	Fresh Gales, and Rain.
	25	9 12 9	27. 5 26 26	30 30 30. 2	53.43	25.59	23					N W b N to S W b W	Hard Gales, with Squalls and small Rain.
	26	9 12 9	21 19 20	30. 2 30. 2 30 1	52.38	22.23	22					S W to S W b S	First Part hard Gales, middle and latter moderate and hazy.
	27	9 12 9	21 20. 5 24	30 31 30. 1	52.27	20.15	21					South to N N E	Fresh Gales, and much Rain; hazy Weather.
	28	9 12 9	25 25 24	30. 2 30.25 30. 2	50.30	16.33	20	51	51			N E	Fresh Gales, and much Rain; hazy Weather.
	29	9 12 9	23 23 24	30. 3 30. 2 30. 2	50.20	13. 7	19	50.24	50.25			N E to N by W	Fresh Gales and hazy, with some Rain the first two Parts, the latter moderate.
	30	9 12 9	23 21. 5 20	30. 2 30. 3 30. 2	50.37	11.41	18	50 30	50 35			N W to S by E	Moderate and fair; Winds variable.
Octob.	1	9 12 9	[illegible]2. 5 22 22	30. 3 30. 2 30. 3	50 35	11.22	18					S S E	Moderate, with a great Head-Sea from the Eastward.
	2	9 12 9	22 23 22	30. 3 30. 2 30	49.40	12.32	18					S E by S	Cloudy, with a great Sea from the Eastward.
	3	9 12 9	22 22. 5 22. 5	29 29 30 1	50. 7	11.39	18					S S E to S by E	Hard Storm of Wind, with a great Sea.
	4	9 12 9	20 20 20	29. 9 30. 1 30 25	50. 3	10.36	17	50. 4				S S W	Moderate Gales, with small Rain.
	5	9 12 9	20. 5 20. 5 20. 5	30.35 30. 3 30. 4	49 38	8,52	16	49 42				S S W to West	Ditto, fair and moderate.
	6	9 12 9	22 22 22	30. 2 30. 1 29. 9	50	7 20	15	50. 5				South Calm S S E	Saw several Ships, sounded 100 Fathom. Moderate.
	7	9 12 9	22 21 21	29. 9 30 29. 9	49.39	6.58	15	49.38				S S E W N W	Two first Parts hard Gales and heavy Squalls, the latter moderate.

An Explanation of the TABLES.

The first Column contains the Month; the second Column is the Day of the Month; the third the Hour of the Day, beginning at 6 in the Morning, to 12 at Noon, and 9 at Night; the fourth Column is that of the Thermoscope; the fifth Column is the Height of the Mercury in the Baroscope, the first Number is the Inches of it's Height, the second and third Number marks the tenths and hundredth Parts of an Inch; the sixth Column is the Latitude the Ship is in, by Account, every Day at Noon; the seventh Column is the Longitude the Ship is in every Day at Noon, by Account, from the Meridian of *London* (except where otherwise expressed). The Column Variation, is the Variation of the Needle; and the next four Columns are the Latitudes observed at Noon by four several new Instruments; the first is Mr *Smith's* Prismatic Quadrant, the second is Mr *Hadley's*, the third by Mr *John Elton*, and the fourth by Mr *Caleb Smith* and Mr *William Ward*; the next Column is the Wind for the most Part of the 24 Hours.

The Thermoscope which I made use of in the Voyage, was made by Mr *John Patrick*, together with the Baroscope; in his Thermoscope he places [°] at the Top, supposing it to be the Heat under the Line, and so the Figures increase downwards, with the Increase of Cold. Temperate is placed at 25.

This Prismatic Quadrant of Mr *Caleb Smith* I find to be of very great Use at Sea, in particular for the Stars, as I have experienced several times in my Voyage to *Hudson's Bay*, in the worst of Weather, when you can but see the Horizon; and his other is of great Use, in tolerable smooth Water, in foggy and hazy Weather, when there is no Horizon to be seen, yet have the Benefit of the Sun.

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 II.

CONTAINING THE

PHYSIOLOGICAL 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 II.

CONTAINING THE

PHYSIOLOGICAL PAPERS.

CHAP. I.

PHYSIOLOGY, METEOROLOGY, PNEUMATICKS.

I. IT may, perhaps, be needless now to add any thing in Confirmation of Dr *Wallis*'s Solution of the *Sun* and *Moon*'s appearing so much larger at rising or setting, than when in a greater Altitude; tho' some have very absurdly still gone on to account for it from Vapours, which I remember was given me in my Youth for the true Cause of it. 'Tis true, indeed, that 'tis these Vapours, or the Atmosphere, alone, that make those Bodies, when very near to the *Horizon*, appear in a spheroidal Form, by refracting, and thereby raising (to Sight), the lower Limb more than the upper, yet these can be no Cause of the other. The *Sun* and *Moon*, each subtending about half a Degree, appear in the Meridian of

Some Thoughts concerning the Sun *and* Moon, *when near the* Horizon, *appearing larger than when near the* Zenith; *by* James Logan, No. 444. p. 404 *dated* Philadelphia, Sept. 20 1735.

of the Breadth of 8 or 10 Inches, to ſome Eyes more, and to others leſs; and in the *Horizon* to be 2 or 3 Foot, more or leſs, according to the Extent of Ground they are ſeen over: But if one can have an Opportunity, as I have here frequently had, of ſeeing the *Sun* riſe or ſet over a ſmall Eminence at the Diſtance of a Mile or two with tall Trees on it ſtanding pretty cloſe, as is uſual in Woods without Underwood, his Body will then appear to be 10 or 12 Foot in Breadth, according to the Diſtance and Circumſtances of the Trees he is ſeen through; and where there has been ſome thin Underwood, or a few Saplings, I have obſerved that the *Sun* ſetting red, has appeared through them like a large extenſive Flame, as if ſome Houſe were on Fire beyond them. Now the Reaſon of this is obvious, *viz.* that being well acquainted with Trees, the Ideas of the Space they take up are in a Manner fixed, and as one of thoſe Trees ſubtends an Angle at the Eye, perhaps not exceeding 3 or 4 Seconds, and would ſcarce be diſtinguiſhable, were it not for the ſtrong Light behind them, the *Sun*'s Diameter of above 30′ takes in ſeveral of them, and therefore will naturally be judged vaſtly larger. Hence 'tis evident, that thoſe Bodies appear greater or leſs, according to the Objects interpoſed or taken in by the Eye on viewing them. And to this only is that Phænomenon to be imputed.

I am ſenſible this Method of arguing is not new, yet the Obſervations here given may probably tend to illuſtrate the Caſe beyond what had been advanced on the Subject.

A Phyſico mathematical Demonſtration of the Impoſſibility and Inſufficiency of Vortices: *By M.* de Sigorgne. *Tranſlated from the* French *by* T.S. *M.D. F.R.S.* No. 457. p. 409. July &c. 1740.

II. That natural Philoſophers of an inferior Claſs, who conſider only the Outſide of Things, are obſtinate in the Defence of *Vortices*, is, in my Opinion, not to be wondered at: The Idea of them ſtrikes the Mind very agreeably at firſt, and even ſeems to promiſe the true Mechaniſm. But that Perſons verſed in the moſt profound Geometry, and in the moſt ſublime Calculations, able Academicians, who inceſſantly apply themſelves to the Study of Nature, ſhould plunge headlong into theſe Notions, and ſuſtain the *Vortices pro aris & focis*, is to me Matter of unaccountable Surprize.

It has been long ſince ſaid, that according as *Vortices* ſhall be multiplied, they will degenerate into Littleneſs and Puerility: And theſe are the Sentiments even of the good *Carteſians* of our Days. But might it not be ſaid, that the great *Vortices* having the ſame Origin with the little, the latter ſhew the Meanneſs of Extraction of the former? As Matter is diviſible *in infinitum*; as to Vorticity, there is no Difference between the Great and the Small: And conſequently, we have a Right to reject the large *Vortices*, ſince *Carteſians* proſcribe the ſmall.

It is on this Conſideration that I am reſolved to attack the *Vortices*: For I muſt own, to the Shame of our Nation, that the Spirit of Party is ſo predominant therein, that ſeveral Perſons, who by a cloſe Study have found the Inſufficiency of *Vortices*, for explaining the *Phænomena* of the Heavens, yet have not dared to publiſh their Notions on that Subject.

But as at present the System of small *Vortices* is freely attacked, I think, that I have a Right to attack the large; and to this Purpose I hope to prove,

I. That the mechanical Formation of a *Vortex* is impossible.
II. That the *Vortex*, were it formed, cannot be of long Duration.
III. In fine, that it is not sufficient for explaining the *Phænomena*.

The mechanical Generation of the Vortex *is impossible.* PART. I.

In the Hypothesis of a perfect *Plenum*, GOD at first created Matter indefinite, uniform, homogeneous, and at Rest. This is allowed by all *Cartesians*, and follows in their Principles from this alone, that Matter was created at Rest. Now, from this perfect Homogeneity of Matter it evidently results, in my Opinion, that the *Vortex* cannot be mechanically formed. Suppose, say the *Cartesians*, that while Matter is as yet at Rest, GOD imprints a Motion in a strait Line on one of it's Particles: This Particle will every Instant meet with Obstacles to the rectilinear Motion in the encompassing Matter; this Motion must therefore be turned aside, and will by this means become circular. Demonstration.

But why should the encompassing Matter, which is at Rest, be an Obstacle to the rectilinear Motion? Because, say they, it happens to be in the Line described by the Particle, on which Motion is supposed to be imprinted. But this very Reason would also prove, that the Body supposed to be in Motion could not circulate round a Centre at a Distance from it; because it would constantly meet with Matter at Rest in the Sides of the Polygon which it was to have described.

In a Word, it is a received Principle, that a Body which moves in a homogeneous Medium, never quits the Line of it's first Direction: It does not refract, or deviate on one Side or the other of this Direction, except when it passes from an easier into a more difficult Medium, or from a denser into a less dense Medium: And even then it's Direction must be oblique on the Surface of this Medium.

Now, the Body in Question would move in a Medium entirely homogeneous; seeing all the created Matter is supposed to be so, and that all but one Particle of this Matter is at Rest. It is moreover evident, that as all the Matter is uniform, every Direction, of what kind soever, of a Body which moves in the midst of this Matter, will be perpendicular to the Surface which corresponds to it; as is demonstrated in Mechanics. The supposed *Mobile* will therefore always move in the Line of it's first Direction, until it has communicated all it's Force; or rather it will remain at Rest after the least Shock, if Regard be had to nothing more than what I have hitherto said.

But there still remains a very important Remark to be made on this Subject, to wit, that as it is universally agreed at this Day, that Rest is not a Force, all this Matter created at Rest will be infinitely soft: It's

Parts will have no Tenacity, no Connexion, no Viscosity; they will be but contiguous, and will not have more Adhesion to one another, than Two Globes which would touch out of the Bounds of the World without any reciprocal Attraction; since Tenacity, Viscosity, *&c.* are, in the *Cartesian* System, but the Effects of Compression every Way. Wherefore these Parts will be divided at the least Shock, in the same manner as if Quicksilver be thrown against a Wall, it is instantly seen to be divided into a Million of Parts, to be reflected on every Side, and be again divided as soon as it falls on the Floor. I know my Comparison is not exact, but the Advantage is on my Side; because Quicksilver is not without Viscosity, or a certain Tenacity between it's Parts; whether it proceeds from Attraction, which is my Opinion, or that it be the Effect of the Pressure of the ambient Fluid. Therefore the *Cartesian* Matter will have more Facility to divide than Quicksilver, and will not be susceptible of any regular Motion; which alone demonstrates, that the mechanical Generation of the *Vortex* is impossible.

There is however this Difference between the *Vortex* imagined by *Descartes*, composed of hard Globules; and that of the infinitely soft Matter of F. *Malebranche*, whose System is revived by his Disciple M. *de Molieres*; that if the *Cartesians* admitted Gravity as a Principle; besides that it would give the true Cause of Hardness, it's Combination with the strait or projectile Motion would produce a Motion in a Curve; as Sir *I. Newton* has demonstrated. But until they will return to this Idea of primitive Gravity, and further while they will make use of no other Matter than one infinitely soft, and really unintelligible, it will not be possible to conceive a single *Vortex* formed; far from having this infinite Number, which, by-the-bye, ought to be dissipated as Waves raised in the Water, upon account of their perfect Homogeneity.

The famous *Cartesians*, always refusing to allow this primitive Gravity, and at the same Time plainly seeing, that this first Manner of forming the *Vortex* was impossible, have had recourse, in order to it's Formation, to the Motion of Rotation of a solid Sphere at the Centre of a small Particle of Matter at Rest, *&c.* and they have pretended, that this Sphere in it's Circulation ought to carry along with it the circumambient Matter.

But this Notion is certainly as unsustainable as the First. For,

1st, They must explain to us the mechanical Formation of this Sphere; they must account for it's Solidity: But all this manifestly supposes the *Vortex* already formed; all this supposes a Pressure equal on every Side, uniform and concentric.

2dly, This Sphere would never imprint an equal Velocity on all the Points of the concave Surface which touches and incloses it, seeing itself has not an equal Velocity in every Point of it's last Surface; and therefore the *Vortex* would not have as much Force to defend itself towards the Poles, as towards the Equator; as we shall shew hereafter.

3dly, This Sphere, in ſtriking againſt the ambient Matter, would but divide it *ad infinitum*; becauſe it is infinitely ſoft, and that it's Parts have no Adherence with each other.

4thly, It is not ſufficient, that a Sphere turns round it's Centre, to draw into it's Circulation the ambient Matter: It is moreover requiſite, that to preſs on this Matter in a Direction from the Centre to the Circumference, (which a ſolid Globe either cannot do, or can hardly be conceived poſſible for it to do) and further ſtill, it is neceſſary there ſhould be Unevenneſſes on this Sphere, and on the concave Surface of the ambient Matter; becauſe otherwiſe, though the Sphere ſhould preſs this Surface by it's centrifugal Force, it would only raiſe it up, or tend to raiſe it, and it would ſlide along the Surface without dragging it away with it: On which Head there is this Particularity to be remarked, that, for the uniform Circulation and Conſervation of the *Vortex*, and ſtill more for the preſerving of *Kepler*'s Laws, the Spheres and Surfaces muſt be ſtrictly Mathematical, as we ſhall ſoon ſee; and for it's Formation they muſt be rough, and full of Unevenneſſes: But what can be more whimſical? And further, though theſe Surfaces were full of Prickles, yet could not the *Vortex* be formed in the Hypotheſis of F. *Malebranche*'s ſoft Matter; becauſe the Parts which would form theſe Eminences and Unevenneſſes on the concave Surface of the Matter ſurrounding the Sphere, not being connected with the other Parts of the ſame Matter, would be carried off without Difficulty by the Rotation of the Sphere; and the reſt of the Matter would remain at Reſt. And thoſe who would pretend, that theſe Unevenneſſes, theſe Parts which form the Hillocks we are ſpeaking of, could not, in conſequence of God's Decree, looſe themſelves from the other Parts of the Matter, would evidently abandon Mechaniſm, without reaping any Advantage: Becauſe, ſuppoſing it true, that by this Means the ambient Matter would be compelled to circulate, yet could it not form a fluid *Vortex*, wherein *Kepler*'s Laws could be obſerved; becauſe both the Sphere and theſe Surfaces being by theſe Unevenneſſes wedged into each other by ſolid hard and inflexible Parts, they would neceſſarily move all of a Piece, as the Parts of a Sphere do.

5thly, By means of this Sphere one could have but a great *Vortex* formed; and not that infinite Multitude of ſmall *Vortices*, with which the great ones are at this Day ſuppoſed to be filled, and in the Centre of all, or moſt Part of which, People will not allow that there are hard Globules, and ſo of the reſt: For I am perſuaded, that the Reader, by a little Meditation on this Subject, will find almoſt as many Reaſons againſt this Syſtem, as there are ſmall *Vortices* ſuppoſed to exiſt.

It may be objected, that we do not pretend to form a *Vortex*: We ſuppoſe that God formed it in the Beginning, and in Conſequence hereof we account for it's properties and Conſervation.

But, beſides that the Impoſſibility of the mechanical Generation of a *Vortex* is a ſtrong Prejudice againſt it's Conſervation; I pretend, in the Principles of our Adverſaries, God could not form a ſingle *Vortex*.

I deſire

I desire Attention may be given, that a circular Motion is a redoubled and forced Motion; and not, as Mr *Perault* thought, a natural Motion. Now the rectilinear Motion cannot be redoubled thus, as against it's Nature, in order to become circular, but upon a Supposition that it meets in the ambient Matter invincible Obstacles to it's Direction; or that by a primitive Law it is carried towards a Centre by a Motion of Gravitation, at the same Time that it receives a Motion in a strait Line. Therefore, since on one hand this universal and primordial Gravity is obstinately rejected; and on the other, as it is solidly proved above, that the ambient Matter is no Obstacle to the rectilinear Motion; it remains certain, that the Formation of the *Vortex* is impossible. *Q. E. D.*

PART II *and* III. *The* Vortex, *though once formed, cannot last, and it is not sufficient for explaining the celestial* Phænomena.

Postulatum. The cylindric *Vortex* cannot long subsist, and is not sufficient for explaining the celestial *Phænomena:* This Principle is allowed by all *Cartesians* in both it's Parts. It cannot subsist; because not having Force to defend itself towards the Poles, if it happened to hit on that Side against another cylindric *Vortex*, that presented it's Equator, it would soon be broke into, and burst to it's very Centre. If, on the contrary, it's same Side touched another cylindric *Vortex* by the Poles, they would both mix together, and would compose but one *Vortex*.

It is not sufficient for explaining the celestial *Phænomena*; because it is allowed, that the translative Velocities of it's Points cannot be in an inverted *Ratio* to the Roots of the Distances, and that it's centrifugal Force does not diminish in the inverted *Ratio* of the Squares of these Distances, &c.

Corollary. Therefore the spherical *Vortex*, in order to be of Use, must have other Properties than the cylindric: That is to say, it must have a relative Force to one and the same Centre; for it is by this Force alone that it can be different from the cylindric *Vortex*.

This Force, moreover, must be equal in all the Points of the same spherical Superficies; because otherwise it might be burst and broke into in it's weak Parts, as well as the cylindric, &c.

Theorem I. Even in the spherical *Vortex* there is no relative Force to one and the same Centre: That is to say, that it has properly but an axifugal Force.

Demonstration. The spherical *Vortex* is composed, as well as the cylindrical, of several parallel Circles, but with this Difference, that in the spherical *Vortex* the *Radii* of the parallel Circles are not all equal, but on the contrary diminish according as they recede from the Equator, and approach the Poles. Now it is manifest, that all the parallel Circles circulating round different Points of the Axis in the spherical *Vortex*, as well as in the cylindrical, tend to recede only from these different Points of the Axis, round which they circulate; because a Body cannot tend to recede from any Centre but

but that of it's Circulation. In a Word, in order to make a *Vortex* ſpherical, which was cylindrical, they have but proportionally ſhortened the parallel Circles. But let the *Radius* of a Circle be ever ſo much ſhortened or lengthened, that will not change the Direction of it's dilative Effort. I am miſtaken! An imaginary Line is going to change the Direction of the axifugal Force. This Force, as all agree, has for it's Direction the *Radius* I C, in the Circumference whereof it is the *Radius*; but the Direction I C is oblique to C E the Tangent to the Sphere; therefore it changes, according to the general Law of an oblique Shock, into the Determination I E or O C relative to the Centre O.

But if Lines may be imagined, and that nothing more is requiſite to realize them, than Points that correſpond to them; we ſhall have ſome of all ſorts in the *Vortex*: We ſhall have oblique Lines on the *Radius* O A, a perpendiclar one, and ſome more or leſs oblique, on the *Radius* I C, and by that means we ſhall be able to determine nothing. Let us grant however, that there is a Tangent to the Sphere C E, at the Point C, and let us ſee if it will be a ſufficient Reaſon for decompoſing the centrifugal Force I C into a central Force I E or O C. For that Purpoſe I aſk, What are the Points that compoſe this Tangent? It is evident that it can only be the Globules of the upper *Stratum* that anſwer thereto. The Line C E is therefore compoſed only of a certain Number of Points ſeparate one from the other, and which conſequently can move one without the other. Therefore if the Line I C is perpendicular to the Globule that occupies the Point C, and that it paſſes through it's Centre; there will be no Decompoſure, and the Force I C will not change into a Force that has the *Radius* O C for it's Direction. Fig. 1.

Now it is infinitely probable, that the *Radius* I C paſſes through the Centre of the Globule C; and it is eaſy to demonſtrate, that it is actually ſo even in the Principles of M. *Saurin*, who firſt invented this central Decompoſition. For what has been the cauſe of the Decompoſition of the circular Velocity into the centrifugal Force I C? It ſeems plain to me, that no other Cauſe can be aſſigned than the Point or Globule C; ſeeing there is but that one at the Point where it happened. The Line I C paſſes then through the Centre of the Globule C; ſince the Decompoſition is always made in a perpendicular Line to the Point that cauſed it.

And indeed, either the *Radius* I C paſſes through the Centre of the Globule C, or the Centre of this Globule is on one Side or the other of this *Radius*, but ſo as that this *Radius* cuts the Point C; or elſe, it is a Space intercepted between two Globules, which directly anſwers to the Point C. In the Firſt Caſe, there is no Decompoſition: In the Second, and in the Hypotheſis, that the Centre of the Globule C happens to be between the *Radius* I C and the Equator, there will be a Decompoſition; but it is manifeſt, that it will not be a central one: It will, on the contrary, be relative either to the very Pole, or to one of the polar Circles. In the Third Caſe, wherein it is ſuppoſed that it is a Space intercepted

between

between two Globules, which answers to the Point C; there may be a Decomposition, but it will be double, the one relative to the Centre O, and the other relative to the Pole Z,

Now the *Cartesians* can never draw from this Decomposition the Advantage they propose; because there will not be more Reason for heavy Bodies precipitating to the Centre of the Sphere by means of the central Force, than to the very Pole by the Assistance of the polifugal Force; or rather, the Complication of these two Forces will compel the *Mobile* to precipitate to the Centre I of the Parallel it happens to be in.

Wherefore, in order to defend the spherical *Vortex*, they must say, that the Centre of the Globule C is comprehended between the Poles and the *Radius* I C. But on what Foundation will they assure it? What are the Proofs they will give for it? One must certainly be a very bold Gamester, to hazard this Point; because besides the Appearance of Truth, the Adversaries of *Vortexes* may wager Three to One, that it does not so happen. But in case it be allowed, will they ever find in the soft Matter of F. *Malebranche* and M. *de Moliere*, a sufficient Cause of the Decomposition? There must be a Resistance to produce a Decomposition, and an infinitely soft Matter does not resist. And further, in the Hypothesis of the Decomposition of I C or O C, the *Vortex* would not be in Safety; because there would be a Remainder of the centrifugal Force I C, that would be parallel to the Tangent C E, and would evidently spread Confusion in the *Vortex*, by driving all the parallel Circles towards the Equator.

This seems to me sufficient to discredit, in the Minds of rational People free from Prejudice, this central Force, which is attempted by all means to be introduced. But let us not be tired of examining this Point thoroughly: It is of Consequence, and the *Cartesians* well deserve the Trouble of an abundant Refutation. Wherefore let us suppose, that God forms a *Vortex* cylindrical and fluid; it is a received and evident Principle, that it's Points will have but an axifugal Force. And if a Sphere be conceived to be inscribed in this Cylinder, the Points that compose it, will not in like manner have any central centrifugal Force, according to the Axiom: *Nostrum intelligere nihil ponit in re.* Now let us realize this spherical *Vortex*, which before we had but conceived; that is, let us suppose, that God has destroyed the translative Velocity of the Points that form the angular Spaces intercepted between the last Surface of the inscribed Sphere and that of the Cylinder; it is manifest, that no Change will happen in the Velocity and axifugal Force of the rest of the Points, which are not included in these; for this Reason, that the Points which fill the two kinds of Basons that mark the Excess of the Cylinder above the inscribed Sphere, remain in the same Order, Disposition, and Direction, with regard to the inferior Points, which they were in at the Time of their Motion. And there is no other Difference to be perceived herein, except that at present it is the same Point that constantly corresponds to the same Place; and that before this Place was successively occupied by

Points

Points entirely resembling each other, and that which remains or is supposed constantly to remain therein.

Now whether this Place be constantly occupied by one and the same Point, or successively by Points entirely alike and in the same Order, is what ought not to produce any Variation in the Effect which we are examining: And this appears to me at least as clear as Noon-day.

Wherefore, since these inferior Points had then but an axifugal Force, it follows that even now they have no other Tendency than to recede from the Centres of their Circulations, without having any Force relative to the Centre of the *Vortex*.

This is all that pure Reason dictates to me on this Point of the Nature of the *Vortex*, whether spherical or cylindrical: And I dare flatter myself, that whosoever will attentively examine my Reasonings, will find them as demonstrative as can be desired in Natural Philosophy.

In Effect, Experience agrees here with Reason. If a glass Globe filled with Water be rapidly turned on it's Axis, one sees little Foulnesses; the small Atoms which it never fails to contain, gather together along the Axis, and form a little Cylinder round it. ——Which very plainly shews, that in this spherical *Vortex* of Water there is but an axifugal Force. Q. E. D.

Therefore Gravity is inexplicable in the *Vortex*, and it has not Strength to defend itself towards the Poles. Corollary.

Supposing there was in the spherical *Vortex* a central Force according to the *Radius* O C, it could not by Reaction be changed into a centripetal Force according to the Radius C O. Theorem II. Fig. 1.

This Proposition is well known to all who are somewhat conversant in Mechanics.

It is therein demonstrated, that if the *Radius* I C, for Example, forms with the Tangent C E an Angle of 45 Degrees, the Line of Reflexion will be parallel to the Axis; and that from the Point C to the Pole Z, the Lines of Reflexion will be divergent to the Axis; and, in fine, that from the Point C to the Equator, these same Lines of Reflexion will be indeed convergent to the Axis, but will never terminate at the Centre O: In a word, that because the Angle of Reflexion is always equal to the Angle of Incidence, it is only at the Equator that the centrifugal Force can be changed into a centripetal Force. Q. E. D.

Therefore the modern *Cartesians* are strangely mistaken, when they pretend to account for Gravity by the Reverse of the central centrifugal Force. Corollary I.

And they can never, *à fortiori*, in their Principles, explain the Figure of the Earth and of *Jupiter*, which are flatted Spheroids made by the Conversion of an Ellipsis upon it's small Axis. Corollary II.

If the centrifugal Force represented by I C, be decomposed on the spherical Tangent into a Force, that for it's Direction has the Centre of the Sphere; the central Force, which results from this Decomposition, will be to the centrifugal Force, as the *Radius* I C to the *Radius* O C. Lemma I.

For the centrifugal Force I C, being decomposed into C on the Tangent of the Sphere, will strike this Tangent with a Force that will be represented by I E. But on account of the similar Triangles I E C, I O C: I E. I C : : I C. O C.

Lemma II. A Body which describes a Curve, strikes this Curve every time it passes from one Side to the other, with an infinitely small Force of the first kind with regard to it's Velocity.

To the best of my Remembrance, this Proposition is demonstrated in Dr *Clark*'s Notes on *Rohault*'s *Physica*, and in M. *de Moliere*'s Lectures: And it is evident from this alone, that it can only be by a Force represented by the Sine of the Angle of Contact that this moveable Body strikes the Tangent of it's Curve.

Theorem III. Let us put Complaisance on the Stretch, and grant that *Vortices* have a central and centripetal Force relative to one Centre O: I say, that the spherical *Vortex* will not have as much of this central Force, to defend itself towards the Poles, as towards the Equator.

Construction. Fig. 1. Let us take, in the same Superficies X two Points at Pleasure, the Point A in the Circumference of the Equator, and the Point C in the Circumference of a subduple parallel Circle; we will give in the Demostration an equal Velocity to the Globules which circulate in these two Circumferences; which is the most favourable Concession imaginable for the Patrons of *Vortexes*.

Demonstration. It is manifest, that if the Point A is in an equal Space of Time struck an equal Number of Times as the Point C, and that each Stroke against the Point A be double each Stroke against the Point C; it is manifest, I say, that there is more Force at the Equator than at the parallel Circle. Now the Supposition is very certain in both it's Parts: For,

1. Since the Circumference of the Equator is the double of that of the parallel Circle, and that being at an equal Distance from the Centre O, the Globules they contain are equal to each other; if there be a thousand Globules in the Circumference of the Parallel, there will be 2000 in the Circumference of the Equator. And as these Gobules are supposed to have in both an equal Velocity, they will make (but) One Revolution in the Equator, while those of the subduple Circumference will make two. Therefore, in both, there will be 2000 Strokes employed in the same Space of Time, against the Points A and C.

2. Each central Stroke is double at the Equator: Because, as there is in both an equal Velocity, and that (LEM. II.) each centrifugal Stroke in every Circumference is a Fluxion of the first kind, with regard to the Velocity of the Globule which is in Motion; it follows that the centrifugal Strokes both in the Equator, and in the parallel Circle, are equal to each other. But the central Effort (which is the only one by which a *Vortex* can defend itself towards the Poles) is at the Point C (LEM. I.) but half the centrifugal Effort, since it is represented by I E subduple of I C; whereas at the Equator the central Effort is the same with the centrifugal

1 Effort

Effort, because the *Radius* O A is perpendicular on the spirical Tangent, which corresponds to it. Therefore, *&c.* Q. E. D.

Therefore if a *Vortex* be in *Æquilibrium* with another *Vortex*, and that the Equator of one happens to answer to the Poles or Tropics of the other; the latter will be burst and penetrated to the Centre: And I do not think, that the *Cartesians* can find their Account in this Consequence. Corollary I.

Therefore if the *Vortex* was the mechanical Cause of Gravity, Gravity ought to be greater at the Equator than at the Poles; and the Earth would be an oblong Spheroid; which is contrary to Observations. Corollary II.

I have said, that it was making a large Concession to the *Cartesians*, to suppose that the Globules of both the Circumferences have an equal Velocity. For if a Sphere full of Water be made to turn on it's Centre, Experience teaches, that the Velocity is greater at the Equator than in the parallel Circles; since it is observed, that the Times of their periodical Revolutions are equal. Whence it follows, that I have, in my Demonstration, made the most favourable Supposition for the *Cartesians* that was possible, Remark.

In order to determine the Tendency of a Layer towards the Upper Part of the *Vortex*, regard must be had not only to that which results from it's own Circulation, but also to that which it receives from the other lower Layers, unless it be the Layer next the Centre. Theorem IV.

While a Layer is in Circulation, it visibly makes a continual Effort towards dilating itself, by reason of the centrifugal Force, with which all it's Parts endeavour to recede from the Centre of Circulation: But it's actual Dilatation being impeded by the Layer next above it, this last will be naturally pressed by it. And thus it is that the first or lowest Layer, being put into Circulation, presses the Second; and the Second, assisted by the First, presses the Third; this, assisted by the two preceding, presses the Fourth; and so on from Layer to Layer, through the whole Extent of the *Vortex*, Whence it follows, that in order to estimate the Quantity of Force with which a Layer tends towards the Surface of the *Vortex*, one must take the centrifugal Force proper to this Layer and that, which all the Matter of the Fluid contained under it acquires by Circulation. Q. E. D. Demonstration.

Therefore the dilatative Effort of the Layers increases with the Layers in a greater Proportion than these Layers. Corollary I.

Therefore it is impossible to explain in the *Vortex*, how Gravity decreases in an inverted *Ratio* of the Squares of the Distances; and consequently there will be nothing found in the *Vortex* to answer to Attraction, whose Existence Sir *I. Newton* has so demonstratively established. Corollary II.

Thus we have re-established in it's full Light the Difficulty, which M. *de Fontenelle* proposed to M. *Villemot* in the Memoirs of the Academy Corollary III.

for the Year 1705 *. This learned Academician pretends, that as in the *Vortex* the lower Points ought to move faſter than the upper, in order to preſerve *Kepler*'s Aſtronomical Law; they ought alſo to have a greater centrifugal Force, and conſequently compel them to deſcend, particularly in Proportion to their Fluidity. The Objection made a great Noiſe, and the only Method found of getting rid of it, was by ſaying, that although each lower Point had more centrifugal Force than each upper; yet as the *Vortex* was in *Æquilibrium*, and the Sums of the Force of each of the two Layers were equal, there was no Reaſon why the lower *Stratum* ſhould get the better of the upper; becauſe this was as prevalent by the Number of it's Points, as that was by the Force of each of it's own.

But it is manifeſt, after what has been demonſtrated above, that the ſecond Layer, being aſſiſted by the firſt, muſt have a greater Force than the third, and conſequently compel it to deſcend, purſuant to the Principle then granted to Monſieur *Fontenelle*.

But if it be aſked, How could the upper Layer deſcend, ſeeing Matter is impenetrable?

I ſhall aſk in my turn, How, in an entire *Plenum*, do heavy Bodies fall to the Centre? And I reaſon on the Principle granted to M. *Fontenelle*.

But yet, becauſe what is allowed by one *Carteſian* is not always always allowed by all; let us ſuppoſe, that the upper Layer cannot deſcend; this, at leaſt, will follow from my Demonſtration, that, according to the Principles of all theſe Gentlemen, an upper Layer being preſſed by all the under ones, it muſt haſten it's Circulation, as long as it is ſlower than that of theſe under Layers; by reaſon that the Exceſs of their Velocities will act upon it, as if it had been at Reſt.

Corollary IV. Therefore the Layers of a *Vortex* will move all of a Piece, as do thoſe of a ſolid Sphere; and *Kepler*'s Law cannot poſſibly be preſerved. We ſhall now give other Proofs upon other Principles.

Theorem V. The Motion of the Points of the Equator is abſolutely independent of the Motion of the parallel Circles; and conſequently, in order to determine the *Æquilibrium* of the Points of the Equator, we muſt attend to nothing but it's Motion.

Demonſtration. The Plane of the Equator is parallel to the Planes of the other parallel Circles, that turn round the ſame Axe with it: It's centrifugal Force is perpendicular to the Tangent to the Sphere, which anſwers to it: It has not then any lateral Tendency towards theſe parallel Circles, and by a neceſſary Conſequence it's Motion is abſolutely independent of theirs.

And indeed, if it be ſuppoſed, that the Motion of the other parallel Circles ſtops, there is ſtill ſome Motion conceived in the Equator, juſt

* He afterwards publiſhed a Book intituled *Nouveau Syſteme, ou nouvelle Explication du Mouvement des* Planetes, *par M.* Philippe Villemot, *Prêtre, Doctеur en Theologie*, &c. Lyon, 1707. in 12°.

as

as in the Case of the cylindrical *Vortex* : It is likewise conceivable, that the Velocity may be greater at the Equator than in the parallel Circles, as the Experiment already cited shews us : And if no Regard be had to the lateral Frictions, as the *Cartesians* would have it, who suppose them none or insensible, and as indeed they are obliged to say, that the *Vortex*, by the lateral Friction of the Equator, may not become cylindrical ; this Equator will always continue to circulate uniformly, without communicating any of it's Velocity to the Points that laterally surround it. Therefore, *&c.* Q. E. D.

Therefore for the *Æquilibrium* of the Points of the Equator, it is necessary, at least, that an upper Circumference should have as much Tendency towards the Superficies of the *Vortex*, as another under concentric Circumference ; because, if it had less, there would be no *Æquilibrum*, even in the Principles of the *Cartesians* ; and the under Circumference, pressing the upper, would either make it descend, or communicate to it a Force equal to it's own. Wherefore, calling F the proper centrifugal Force of a Point of the upper Circumference, and f that of a Point of the under one ; if S, s mark the different Sums of the Points contained in these two Circumferences, we shall have $FS = fs$. Corollary I.

Therefore the centrifugal Force does not diminish in the Plane of the Equator in the inverted *Ratio* of the Squares of the Distances from the Centre ; for since $FS = fs$; $F . f :: s . S$. But the Points being supposed equal on both Sides, their Sums are as the Circumferences, and one has $s . S :: d . D$; which gives $F . f :: d . D$. instead of $:: dd . DD$. Corollary II.

Therefore *Kepler*'s Rules cannot be observed in the *Vortex*, or at least in the Plane of it's Equator ; for since $F . f :: d . D$; by putting in the Place of F, f, their Values, we shall have $\frac{VV}{D} . \frac{uu}{d} :: d . D$, and therefore $V = u$ and $D^2 . d^2 :: TT . tt$. whereas we ought to have $V . u :: \sqrt[2]{d} . \sqrt[2]{D}$ and $D^3 . d^3 :: TT . tt$. Corollary III.

There is here a *Finesse* of the *Cartesians* to be observed. These Gentlemen consider only the *Æquilibrium* of the spherical Layers of the *Vortex*, and from the Equality of their central Forces they deduce *Kepler*'s Laws, as well as they can. Remark.

But it is manifest, that whatever becomes of the Equality of Force in different spherical Superficies of the *Vortex*, there must be an *Æquilibrium* in the Plane of the Equator ; because it is in this Plane that the Planets move ; and if there had not actually been an *Æquilibrium* between it's Points, they would soon place themselves there, by reason that Fluids always tend to the Side where they are less pressed ; and it is by an actual *Æquilibrium* alone that they are kept in their Places ; which entirely overturns the Theory of these Gentlemen.

Let us however grant to the *Cartesians*, that the Sums of the Forces of the two spherical Surfaces are equal ; I cannot see, that they can thence infer, as they do, that the central Force diminishes in a reciprocal *Ratio*

of

of the Square of the Distance from the Centre. Let us examine their Argument:

$FS = fs$, say they; therefore $F.f :: s.S$; but s, S mark the Sums of the Points contained in the two Surfaces; therefore they are as these Surfaces, which, being as the Squares of their Distances, give $F.f :: dd\, DD$.

But it must be remarked, that the Surfaces of the *Vortex* are not Mathematical, they are Surfaces which have some Thickness: They cannot then be proportional to the Squares of their Distances from the Centre, except in the Case when their Thickness is equal. Now according to the *Cartesians*, the Points or Globules, which compose the *Vortex*, increase in Bulk according as they recede from the Centre; and, besides, they are homogeneous, or of an equal specific Density, at least in their common System. And consequently it is certain, that the different natural or real *Strata* of the *Vortex* are not of an equal Thickness, and that the Matter contained therein is not proportionate to the Squares of the *Radii* of these Surfaces, but only to the Squares of these *Radii* multiplied by the Thickness of the *Strata*, Therefore, *&c.* Q. E. D.

Corollary IV. Therefore, even allowing the *Cartesians*, what one has a Right (*Cor.* I. *Theor.* IV.) to refuse them, they will never be able to explain *Kepler*'s Rules in the *Vortex*; for it is only by the Proportion, which I have just now annulled, that they pretend to do it. See M. *de Moliere*'s *Leçons de Physique.*

And if it be objected, that I have not, in the preceding Corollaries, had any Regard to the Thickness of the Circumferences; I answer, that it was by way of pure Concession that I have not done it; and if any Person will be at the Pains of doing it, he will easily find, that *Kepler*'s Rules will only be the more disturbed thereby.

Conclusion. Therefore the *Vortex* is every way impossible, and insufficient in Natural Philosophy. It's mechanical Generation is impossible (*Part* I.); it has only an axifugal Force, and not a centrifugal and centripetal Force, as it should have (*Theor.* I. and II.); and even if it had, it cannot (*Theor.* III.) defend itself equally on all Sides. It is not sufficient for explaining Gravity, and it's Properties; it destroys *Kepler*'s Astronomical Laws. (*Coroll.* III. *Theor.* IV. and V.) What more can be desired, in order to conclude with Sir *Isaac Newton*? "Itaque hypothesis Vorticum (est impossibile &) "cum phænomenis astronomicis omnino pugnat, & non tam ad expli-"candos quam ad perturbandos motus cœlestes conducit." Q. E. D.

A short Account by James Parsons, *M. D. F. R. S. of a Book intituled,* Traité des Sens, *&c. by* M. le Cat, *M. D. F. R. S. printed at* Rouen, 1740. 8vo. No. 466. p. 264. *Read* Dec. 16. 1742.

III. This Treatise appears, by the Advertisement prefixed to it, to be a Part of a Physiological Work, which the Author says is not likely to be soon published; and that he has therefore exhibited this Part for the Use of the Curious, and Lovers of Philosophy, who might not be so agreeably entertained by the rest of the Work, as treating chiefly of the Human Body, and therefore calculated rather for those of the Faculty of Medicine.

He

He begins the Book with Page 201. and says he has, before, establish-ed certain general Principles of *Sensation*, and that now he proceeds to recount the particular Parts with which Nature has furnished the animal Oeconomy, serving to our different Senses; and then expatiates a little upon the general Utility of them.

Chap. I. treats of the Sense of *Feeling*, wherein he has compiled all the different *Phænomena* that regard this Sense, as those of Heat, Cold, and other Objects of Feeling, with the Structure of the Skin, to which he thinks fit to subjoin two known Histories, one of a blind Orga-nist in *Holland*, who distinguished all kinds of Coins, and played at Cards, by Feeling; and the other of the famous Statuary *Ganibasius*, who, though stone-blind, could by Feeling make a Statue in Clay, perfectly like what he felt. Our Author adds something of *Tickling*, and endea-vours to prove, that Imagination has a great Share in the Cause of this Sensation, as well as the others; and thence he falls upon an Account of another Sense, which he brings under this Head; which he calls, *la Chatouilment de l'Amour*, of which he gives a florid Definition.

Tasting is his next Subject, wherein, as in the foregoing Chapter, he has drawn together the several Sections relating to it, as, an Account of the Organs of Taste, the Mechanism of *Savours*, and the manner of their being varied into compound Tastes. His Comparison here is new; he says, Since the Principles of Savours are Salts, both fixed and volatile, that Water, Earth, and Sulphur, serve to make the great Variety, and different Kinds, that are in Taste, just as Shadows variously mingled with Light form different Appearances; not that the Shadow is capable of making an Impression upon our Organs of Sight, but the Light alone; as the Salts alone are, upon our Organs of Taste. He has also some Reasoning upon the Difference that is in Mens Appetites to some Eatables, which were before disagreeable. His Reason is, not that the Organs differ at any time from what they always were, but because the Soul sometimes changes her Ideas, even from the same Impressions, and that therefore there can be no Ideas essential to any Impressions; or at least, that there are none which the Soul cannot change: He also says, that Imagination is much concerned in the Variation of Tastes.

The Sense of *Smelling* is discussed in his Third Chapter, wherein he observes the same Method as in the two former, in describing the Me-chanism of the Organs serving to that Sense, and accounting for the Con-veyance of Odours to those Organs; and for the *Stimulus* of some odori-ferous Particles causing Tears to flow, as well as Sneezing caused by a glaring Light; and, after making some Reflections on the many Effects of Smells upon the Human Body, and the exquisite Sense of Smelling in some Animals, he recites the Story told by Sir *K. Digby*, of the Boy brought up in a Forest, whose Smell was so exquisite as to perceive the Approach of Enemies, and warn his Parents of them. Our Author found this Story elegantly told and reasoned upon, in M. *Verduc*'s Book called, *Usage des Parties*. He also mentions the Perfection of Smelling in the

Inhabitants

Inhabitants of the *Antibes*, who can run a Man upon the Nose like a Hound; and concludes this Section with a Relation of a *Frier* of *Prague*, from the *Journals des Sçavans*, who could not only distinguish different Persons from each other by smelling, but also an incontinent Woman from a chaste one; and adds, in a joking Strain, that this Man had begun a Treatise of Odours before he died, which the Journalists much regretted the Loss of: But, says M. *le Cat*, for my part, I do not know but a Person so exquisite in this kind of Knowledge would be dangerous in Society.

He proceeds next to treat of *Hearing*, and brings under that Head the whole Mechanism and Doctrine of Sounds; the Vibrations of all sounding Bodies: And from the Experiment of holding a Candle near any vibrating or sounding Body, without the Flame's being moved or otherways affected, he argues that the common Air does not produce the Sound, but a more subtile Fluid better proportioned to the Organs of Hearing: Here he runs into a Detail of the Principles of the Chords and Tones of Music, and makes a new and curious Comparison between the principal Colours in the Rays of Light, and the foresaid Fluid, which is more or less subtile in the Air, some Particles of which are only capable of being moved to express low Tones, others higher, and so on successively, as far as the Compass of Music reaches; just as the Light is composed of certain kinds of Rays, some of which produce *Red*, some *Green*, *&c.* This being supposed, says he, it may be conceived, that every Tone will move the Fluid that is proper to itself; and by that means the Ear may receive at once the Impressions of every Fluid, as the Eye receives the Impulsions of several coloured Rays at the same Instant. He adds to this, by way of Reasoning, that when a single String of an Instrument is touched, though the generality of Mankind can distinguish but one Tone, which he calls the *fundamental Sound*, yet People accustomed to Harmony can distinguish, besides, an *Octave*, a *Fifth*, and a *Third*, covered by this *fundamental Tone*; for the *Octave* is half that Sound, or the Produce of half the String; the *Fifth* is the Produce of $\frac{2}{3}$, and the *Third* is the Produce of $\frac{4}{5}$ of the same String.

He proceeds to reason upon this in an agreeable Manner, and concludes his above-mentioned Comparison to this purpose: Thus there are in the vibrated String all the Harmonies or Chords at once which compose the *fundamental Sound*, by vibrating each it's particular proper Fluid at the same Time; just as the Assemblage of all the different primitive coloured Rays meeting together, makes the white Colour or Light: And so the Ear of a good Musician is a kind of *Prism*, which can separate and distinguish the Sounds or Tones from each other in the *fundamental Sound*. He gives an anatomical Description of the Organs of Hearing; and has added some good Figures of the external and internal Parts of the Ear, with the *Eustachian* Tube, much after the manner of *Du Verney*.

He

He has alſo the Figure of an Inſtrument, Page 292, to help thoſe that are hard of Hearing, which he claims the Invention of. The particular *Form* of this Inſtrument may be new to the Author; yet we have had of this Kind in Uſe many Years in *England* for the ſame Purpoſe. He finiſhes this Section with ſome Reflections upon a young Man of a Town called *Chartres*, who was born deaf and dumb, and whoſe Hearing ſuddenly came to him, and who ſpoke ſome Months after. In this Place he has a very good Figure of the *Baſis Cerebri*, by a tranſverſe Section through the *Frontal Sinuſes* a little above the Eyes, and continued through the *temporal Bones*; demonſtrating the *Originations* and *Exit* of the Nerves, with the Conjunction of the *vertebral* and *carotid Arteries*, according to the Diſſection of the famous *Willis*; and then proceeds to his laſt Section, which treats of *Seeing*.

This Section, in a Word, is on the Structure of the Eye, and all the *Phænomena* of Viſion. He begins it with the Doctrine of *Lights* and *Colours*, making uſe of many Experiments and Explanations of the great Sir *Iſaac Newton*; having alſo added ſeveral of his own, beſides ſome little Cavils, a mere *Jeu des Mots*, againſt that great Man's Doctrine of *Attraction*, to which he prefers the *Impulſion* of *Carteſius*. He quotes againſt Sir *Iſaac*, *M. de Fontaine*, *M. Bannier*, and *M. Voltaire*; and as our young Author had a Mind to oppoſe the Opinions of one of the greateſt Abilities in the Sciences, common Prudence ſhould have informed him, that the Name *Newton* beſpeaks the greateſt Modeſty and Diffidence in the Attempt. Our Author amuſes himſelf thus againſt that *Prince* of Philoſophers, which is the more ſtrange; ſince if he had wrote nothing on the Subject, *M. le Cat* would have wanted a great Part of his Furniture for this Section.

The principal Authors beſides, regarding Anatomy and Phyſiology, which our Author ſeems to have had in his View, are *Du Verney*, *Willis*, *Synac* upon *Heiſter*, and *Verduc*'s excellent Book *L'Uſage des Parties*. However, this *Treatiſe* of the Senſes is judiciouſly compiled; nor does it want ſeveral ingenious Embelliſhments from the Author, beſides the Opinions of ſeveral others; we may therefore conclude it to be a very uſeful Book.

Concerning Electricity, *by M.* Du Fay, *F. R. S. and R. Acad. Sc.* Paris, *tranſlated from the* French, *by* T. S. *M. D.* No. 431. p. 258. *dated* Paris, *Dec.* 27, 1733.

IV. 1. Firſt, I have found that all Bodies (metallick, ſoft, or fluid ones excepted) may be made Electrick, by firſt heating them more or leſs, and then rubbing them on any ſort of Cloth. So that all kinds of Stones, as well precious as common, all ſorts of Wood, and, in general, every thing that I have made Trial of, became Electrick, by heating and rubbing; except ſuch Bodies as grow ſoft by Heat, as the Gums, which diſſolve in Water, Glue, and ſuch other Subſtances. 'Tis alſo to be remarked, that the hardeſt Stones and Marbles require more chafing or heating than others, and that the ſame Rule obtains with regard to the Woods; ſo that Box, *Lignum Vitæ*, and ſuch others muſt be chafed almoſt to the Degree of burning, whereas *Fir*, *Lime-Tree*, and *Cork*, require but a moderate Heat.

Secondly, Having read in one of Mr *Gray*'s Letters *, that Water may be made Electrical by holding the excited Glass Tube near it (a Dish of Water being first fixed to a Stand, and that set on a Plate of Glass, or on the Brim of a Drinking-Glass, previously chafed, or otherwise warmed) I have found upon Trial, that the same thing happened to all Bodies without Exception, whether solid or fluid; and that for that Purpose it was sufficient to set them on a Glass-Stand slightly warmed, or only dried; and then by bringing the Tube near them, they immediately became Electrical. I made this Experiment with Ice, with a lighted Wood-coal, and with every thing that came into my Mind; and I constantly remarked, that such Bodies as of themselves were least Electrical, had the greatest Degree of Electricity communicated to them at the Approach of the Glass Tube.

Thirdly, Mr *Gray* says, towards the End of one of his Letters †, that Bodies attract more or less according to their Colours. This led me to make several very singular Experiments. I took 9 silk Ribbons of equal Size, one white, one black, and the other 7 of the 7 primitive Colours, and having hung them all in Order on the same Line, and then bringing the Tube near them, the black one was first attracted, the white one next, and the others in Order successively to the red one, which was attracted least, and the last of them all. I afterwards cut out 9 square Pieces of Gause, of the same Colours with the Ribbons, and having put them one after another on a Hoop of Wood with Leaf-Gold under them, the Leaf-Gold was attracted thro' all the coloured Pieces of Gause, but not thro' the white or black. This inclined me at first to think, that the Colours contributed much to Electricity. But 3 Experiments convinced me of the contrary: The first, that by warming the Pieces of Gause, neither the black nor white Pieces obstructed the Action of the electrical Tube more than those of the other Colours. In like manner, the Ribbons being warmed, the black and white are not more strongly attracted than the rest. The second is, the Gauses and Ribbons being wetted, the Ribbons are all attracted equally, and all the Pieces of Gause equally intercept the Action of electrick Bodies. The third is, that the Colours of a Prism being thrown on a Piece of white Gause, there appear no Differences of Attraction. Whence it follows, that this Difference proceeds not from the Colour, as a Colour, but from the Substances that are employed in the dying. For when I coloured Ribbons, by rubbing them with Charcoal, Carmine, and such other Substances, the Differences no longer proved the same.

Fourthly, Having communicated the Electricity of the Tube by means of a Packthread, after Mr *Gray*'s manner, I observed, that the Experiment succeeded the better for wetting the Line; and that it may be supported on Glass-Tubes instead of Silk Lines. And I made this Experiment at 1256 Feet Distance, in a Garden, tho' the Wind was high,

* Vol. VI. Part ii. Chap. i. §. iii. 3. † Ibid. Art. 2.

and that the Line made 8 Returns, and passed thro' 2 different Walks. By means of 2 Silk Loops I adjusted 2 Lines in such a manner, that their Ends were but a Foot distance from one another, and I remarked that the Electrick Virtue was still communicated. I have since that seen *, that Mr *Gray* had the same Thought, and that he had done the same with Rods. This Experiment put me upon placing several different Bodies between the two Lines, in order to examine which diminished or intercepted the Electricity, and which gave no Obstruction to it; I have given the *Academy* an Account of the Particulars, which I now omit for the Sake of Brevity.

Fifthly, I suspended a Child on Silk Lines, and made all the surprising Experiments described by Mr *Gray* †. But having tried the Experiment upon my own Body in the same manner, I observed several things very remarkable. First, when I take the Paste-board or Stand, on which the Leaf-Gold is laid, into my Hand, neither my other Hand nor my Face has any Attraction. But if another Person, who is in the Chamber, comes near me, he will attract it with his Face, his Hand, or even with a Stick. Secondly, while I am suspended on the Lines, if the electrick Tube be put near one of my Hands, or my Legs, and then if another Person approach me, and pass his Hand within an Inch or thereabouts of my Face, Legs, Hand, or Cloaths, there immediately issues from my Body one or more pricking Shoots, with a crackling Noise, that causes to that Person as well as to my self, a little Pain resembling that from the sudden Prick of a Pin, or the burning from a Spark of Fire, which is as sensibly felt thro' one's Cloaths, as on the (bare) Hand or Face. And in the Dark, these Snappings are, as may be easily imagined, so many Sparks of Fire. These Snappings, or Sparks, are not excited, if a Bit of Wood, Cloth, or any other Substance than a living Body be passed over the Person suspended on the Lines, unless it be a Piece of Metal, which produces very nearly the same Effect. Any other living Animal doth the same, if put on the Lines, and that first the Tube, and then the Hand be applied near it: But it is otherwise, if the Experiment be made with the Carcass of an Animal; for then one perceives only, if it be in the Dark, a still uniform Light, without Snappings or Sparks. I omit many other Circumstances of less Importance, though curious, to avoid running into too great a Length.

Sixthly, On making the Experiment related by *Otho de Guerik*, in his Collection of Experiments *de Spatio Vacuo*, which consists in making a Ball of Sulphur rendered Electrical, to repel a Down-Feather, I perceived that the same Effects were produced not only by the Tube, but by all electrick Bodies whatsoever; and I discovered a very simple Principle, which accounts for a great Part of the Irregularities, and if I may use the Term of the Caprices that seem to accompany most of the Ex-

* Vol. VII. Part. iv. Chap. iv. † Vol. VI. Part ii. Chap. i. § iii. 2.

periments on Electricity. This Principle is, that electrick Bodies attract all those that are not so, and repel them as soon as they are become electrick, by the Vicinity or Contact of the electrick Body. Thus Leaf-Gold is first attracted by the Tube, and acquires an Electricity by approaching it; and of consequence is immediately repelled by it. Nor is it re-attracted, while it retains it's electrick Quality. But if, while it is thus sustained in the Air, it chance to light on some other Body, it straightways loses it's Electricity; and consequently is re-attracted by the Tube, which, after having given it a new Electricity, repels it a second time; which continues as long as the Tube keeps it's Electricity. Upon applying this Principle to the various Experiments of Electricity, one will be surprized at the Number of obscure and puzzling Facts it clears up. For Mr *Hauksbee's* famous Experiment of the Glass Globe, in which Silk Threads are put, is a necessary Consequence of it. When these Threads are ranged in Form of Rays by the Electricity of the Sides of the Globe, if the Finger be put near the Outside of the Globe, the Silk Threads within fly from it, as is well known; which happens only because the Finger, or any other Body applied near the Glass Globe, is thereby rendered electrical, and consequently repels the Silk Threads, which are endowed with the like Quality. With a little Reflection one may in the same manner account for most of the other *Phænomena*, and which seem inexplicable, without attending to this Principle.

Seventhly, Chance has thrown in my Way another Principle, more universal and remarkable than the preceding one, and which casts a new Light on the Subject of Electricity. This Principle is, that there are two distinct Electricities, very different from one another; one of which I call *vitreous Electricity*, and the other *resinous Electricity*. The first is that of Glass, Rock-Crystal, Precious Stones, Hair of Animals, Wool, and many other Bodies: The second is that of Amber, Copal, Gum-Lack, Silk, Thread, Paper, and a vast Number of other Substances. The Characteristick of these two Electricities is, that a Body of the *vitreous Electricity*, for Example, repels all such as are of the same Electricity; and, on the contrary, attracts all those of the *resinous Electricity*; so that the Tube, made electrical, will repel Glass, Crystal, Hair of Animals, *&c.* when rendered electrick and will attract Silk, Thread, Paper, *&c.* though rendered electrical likewise. Amber, on the contrary, will attract electrick Glass, and other Substances of the same Class, and will repel Gum-Lac, Copal, Silk, Thread, *&c.* Two Silk Ribbons rendered electrical, will repel each other; two Woollen Threads will do the like; but a Woollen Thread and a Silk Thread will mutually attract one another. This Principle very naturally explains, why the Ends of Threads, of Silk, or Wool, recede from one another in Form of a Pencil or Broom, when they have acquired an electrick Quality. From this Principle one may with the same Ease deduce the Explanation of a great Number of other *Phænomena*. And 'tis probable, that this Truth will lead us to the further Discovery of many other things.

In Order to know immediately, to which of the two Claſſes of Electricity belongs any Body whatſoever, one need only render Electrical a Silk Thread, which is known to be of the *reſinous Electricity*, and ſee whether that Body, rendered Electrical, attracts or repels it. If it attracts, 'tis certainly of that kind of Electricity which I call *vitreous*; if on the contrary it repels, 'tis of the ſame kind of Electricity with the Silk, that is, of the *reſinous*. I have likewiſe obſerved that communicated Electricity retains the ſame Properties: For if a Ball of Ivory, or Wood, be ſet on a Glaſs Stand, and this Ball be rendered electrick by the Tube, it will repel all ſuch Subſtances as the Tube repels; but if it be rendered electrick by applying a Cylinder of Gum-Lac near it, it will produce quite contrary Effects, *viz.* preciſely the ſame as Gum-Lac would produce. In order to ſucceed in theſe Experiments, 'tis requiſite that the two Bodies, which are put near one another, to find out the Nature of their Electricity, be rendered as electrical as poſſible; for if one of them was not at all, or but weakly electrical, it would be attracted by the other, though it be of that Sort, that ſhould naturally be repelled by it. But the Experiment will always ſucceed perfectly well, if both the Bodies are ſufficiently electrical.

Experiments and Obſervations upon the Light that is produced by communicating Electrical Attraction to animal or inanimate Bodies, together with ſome of it's moſt ſurpriſing Effects, by Mr Stephen Gray. *F.R.S.* No 436. p. 16. *dated* Charter-Houſe, Jan. 28th, 1734-5.

2. It is no ſmall Satisfaction to me, that my Electrical Diſcoveries have not only been confirmed by ſo judicious a Philoſopher as Mr *Dufay*; but that he has made ſeveral new ones of his own, more eſpecially that important luciferous one, which put me upon making the Experiments I am now going to relate.

I ſhall firſt give ſome Account of the Experiments made the laſt *Spring*, ſoon after I received the Tranſlation of Mr *Dufay*'s Letter; then of thoſe we made at my honoured Friend's, *Granvill Wheler*, Eſq; *F.R.S.* in the Months of *July* and *Aug.* and laſtly proceed to thoſe I have made ſince my Return to *London*, which was in *Sept.* laſt.

As I had not any ſilk Lines by me ſtrong enough to bear the Boy, I cauſed him to ſtand on ſome of the Electric Bodies; and, as I concluded, found the Effect the ſame as mentioned by Mr *Dufay*. I ſhall not need to mention the Particulars of the Experiment, but proceed to thoſe that were ſuggeſted to me upon Mr *Dufay*'s ſaying, that theſe Snappings or Sparks are not excited, if a Piece of Wood, or any other Subſtance than a living Body, be paſſed over the Perſon ſuſpended on the Lines, unleſs it be a Piece of Metal: from thence I concluded, that if I ſuſpended the Metal upon ſilk Lines, or laid it upon any of the Electric Bodies, the Effect muſt be the ſame, when the Metal had been made Electrical by the Tube, and the Hand of any one was held near it, and found it ſucceeded accordingly. I began firſt with ſome common Utenſils that were at Hand, ſuch as the Iron Poker, Tongs, and Fire Shovel; any of theſe being ſuſpended upon Lines of the largeſt ſewing Silk, then the excited Tube, being applied firſt to the Knob of the Poker, and after it the Hand, there was the Snap and Pricking felt, as I expected; and the Effect was the ſame, when the Tube was firſt applied to the other End

of

of the Poker. I had by me a three pronged Iron Inſtrument, which was made many Years ago; it's Uſe was deſigned for propping up the Obſervatory Table, when I obſerved the Spots in the Sun; the Prongs were about half an Inch Diameter, two of them about 22 Inches, and the third about 8 Inches long; they were tapering towards the Ends, and pointed: This being laid either upon Cylinders of Glaſs, Cakes of Roſin and Bees Wax, or on a Cake of Sulphur, the Tube being applied to the End of any of the Legs, the Hand or Cheek being applied near the other, both the other Legs had the ſame Effect as that to which the Tube had been applied; but by holding my Cheek near any of the Points of the Legs, the pricking or burning Pain was much more ſenſibly felt, and was ſometimes felt for ſeveral Minutes after. I was not ſo inquiſitive at that Time about making the Experiment in the Dark, that I might ſee the Light proceeding from the Iron, not thinking the Electricity communicated to the Metals would have produced ſo ſurpriſing Phænomena, as by the following Account of the Experiments will be deſcribed.

1. I come now to give ſome Account of the Experiments we made at Mr *Wheler*'s, beginning firſt with the Succeſs we had in repeating Mr *Dufay*'s Experiment. Mr *Wheler*, ſoon after my coming to him, procured ſilk Lines ſtrong enough to bear the Weight of his Footboy, a good ſtout Lad; then having ſuſpended him upon the Lines, the Tube being applied to his Feet or Hands, and the Finger of any one that ſtood by held near his Hands or Face, he found himſelf pricked or burnt, as it were by a Spark of Fire, as Mr *Dufay* had related, and the ſnapping Noiſe was heard at the ſame Time; but it did not ſucceed with us, when we applied our Hands to any Part of his Body through his Cloaths, except upon his Legs, upon which he felt the Pain through his Stockings, although they were very thick ones.

2. Being deſirous to make the Experiment upon another Species of Animals, we took a large white Cock and ſuſpended him upon the Lines firſt alive, and the Effect was the ſame as on the Boy, whether we applied our Fingers to any Part of his Body, or our Cheek to his Beak, Comb or Claws; then the Cock was killed, and put on the Lines again, and we found very little, if any, Difference, from the Effect it had on us when the Cock was living: We then cauſed the Cock to be ſtripped of his Feathers, and the Difference from what has been ſaid before was not very great.

3. We took a large Sirloin of Beef, that came from an Ox that had been killed two Days before, and ſuſpended it on the ſilk Lines; then the Fingers held near any Part of it, there was a Snapping, and the Fingers were puſhed or pricked; but the Snapping was thought not to be quite ſo loud as when the Experiment was made on the Cock.

4. We cauſed to be made an Iron Rod, 4 Foot long, and about half an Inch Diameter, pointed at each End, but not ſharp, being left about the Bigneſs of a Pin's Head, this being ſuſpended on the Lines; then the Tube being rubbed, and held near one End of the Rod, and then the

Finger

Finger or Cheek being put near either End of the Rod, the Effect was the ſame as when an Animal had been ſuſpended on the Lines, with reſpect to the pricking Pain we felt.

5. At Night we made the luminous Part of the Experiment, ſuſpending the Iron Rod on the Silk Lines; then applying one End of the Tube to one End of the Rod, not only that End had a Light upon it, but there proceeded a Light at the ſame Time from the other, extending in Form of a Cone, whoſe Vertex was at the End of the Rod, and we could plainly ſee that it conſiſted of Threads, or Rays of Light, diverging from the Point of the Rod, and the exterior Rays being incurvated. This Light is attended with a ſmall hiſſing Noiſe; every Stroke we give the Tube, cauſes the Light to appear: The Hiſſing ſeems to begin at that End of the Rod next the Tube, and as it comes, increaſes in it's Loudneſs, but it is ſo ſmall as not to be heard without good Attention, and by thoſe only that ſtand at that End of the Rod from whence the ſaid Light proceeds.

Mr *Godfrey* being deſirous to ſee theſe Experiments, I repeated them, by laying a Rod of Iron upon a Cake of Shell-Lack, which was laid upon a Glaſs Veſſel; but the Effects being much the ſame with what has been above-mentioned, I ſhall not need to mention any other Particulars.

1. I ſhall now proceed to give ſome Account of the Experiments I have made ſince my Return to *London*, which was in *Sept.* laſt. I cauſed 3 Iron Rods to be made, one of 4 Feet long, two, each 3 Feet in Length; one of theſe was made tapering toward the Ends, and pointed as that of 4 Feet was; the other pointed at one End, and the other End not pointed, the Diameter of the Rods about half an Inch; they were firſt forged, then filed and burniſhed. With theſe I made the following Experiments: When any of them were laid either upon the Brims of hollow Cylinders of Glaſs well warmed, or upon Cakes of Roſin and Bees Wax, or upon thoſe of Sulphur, the Phænomenon was the ſame as when they had been ſuſpended on Silk Lines: But now I diſcovered another very ſurpriſing one, *viz.* that after the Tube had been applied, and the Light ſeen at both Ends, upon my going to the other end of the Rod, when there was no Light to be ſeen, upon holding my Hand at ſome diſtance from it, then moving my Hand towards it with a pretty ſwift Motion, there iſſued from that Point of the Rod a Cone of Light, as when the Tube had been applied to the other End; and upon repeating this Motion of my Hand, the ſame Phænomenon appeared for five or ſix times ſucceſſively, only the Rays were each time ſhorter than the other; theſe Lights are alſo attended with a hiſſing Noiſe: That Light which appears upon that End next the Tube, when it is held obliquely to the Axis of the Rod, has it's Rays tending towards it: All the Time I am rubbing the Tube, theſe Flaſhes of Light appear upon every Motion of my Hand up or down the Tube, but the largeſt Flaſhes are produced by the Motion of my Hand downwards.

2. When

2. When 2 or 3 Rods are laid either in a right Line, or making any Angle with each other, or either touch, or are at a ſmall Diſtance from one another, the Tube being applied to one of their Ends, the furtheſt End of the further Rod, exhibits the ſame Phænomena as one ſingle.

3. An Experiment with the Rod that was pointed at but one of it's Ends. When the Tube is applied to the other End of the Rod, the Point gives the ſame Appearance and a like Effect with the Rods, that are pointed at each End; but the great End of the Rod, when the Hand or Cheek is applied near it, gives but one ſingle Snap; but this is much louder than the greateſt of thoſe from the Point of the Rod, and one feels a little more Pain by it.

4. I cauſed an Iron Ball to be forged, and then turned and burniſhed; 'twas 2 Inches Diameter, which being placed on a wooden Stand, that had a ſmall Concave at the Top, in which the Ball was placed; the Stand being ſet upon a Cylindrick Glaſs, then the excited Tube being applied near the Ball, there proceeded a Stream of Light from it, with a ſmall hiſſing Noiſe; then putting my Finger or Cheek near the Ball, there was no Snapping, nor any Pain felt, yet there appeared a very bright Light.

5. The Rod of 4 Feet long, being placed upon a Stand, that had a croſs Arm with a Groove in it to receive the Rod; then the Stand being placed on the Glaſs Cylinder, they were ſet at ſuch a Diſtance, as that one of the Points of the Rod might juſt touch the Ball over againſt it's Centre; then going to the other End of the Rod with the prepared Tube, and applying it as uſual, when I came to the Ball, the Hand or Cheek being near it, cauſed a loud Snap, compared to thoſe made by the Points of the Rods, and the Pain of pricking or burning was more ſtrongly felt, the Light alſo was brighter and more contracted: I then placed the Rod with it's Point at an Inch diſtance from the Ball, and applying the Rod as before, I came to the Ball, and touching it with my Hand or Finger, there not only appeared a Light on the Ball, but there alſo proceeded a Bruſh of Light from the Point of the Rod after the ſame manner as when the Experiments had been made with the Rods only.

6. An Experiment made with the 4 Feet Rod, and a Braſs Plate 4 Feet ſquare. This was placed upon a Stand, ſo that the Plate ſtood perpendicular, the Stand being ſet on the Cylindrick Glaſs; then the Rod with it's Stand and Glaſs was ſet ſo as that one Point of it was about an Inch from the Centre of the Plate; then the Tube being applied to the other End of the Rod, and after going to the Plate, on ſtriking it gently with my Finger on the back Side, a Light appeared upon the Plate, and at the ſame Time the Bruſh of Light came out from the Point of the Rod; and when my Hand or Cheek was held near any of the Angles of the Plate, there was a Light came from thence with a ſmall hiſſing Noiſe, and the Pricking was felt as when the Experiments were made with the pointed Rods.

7. A

7. A Pewter Plate being laid upon the Stand, which had been ſet upon a Glaſs Cylinder, the Tube firſt, and then the Finger applied, there appeared a Light upon the Plate, and the End of the Finger was puſhed; and when the Cheek was held near the Edge of the Plate, there was a Snapping heard, but not ſo loud as when Iron Rods were uſed. I then filled the Plate with Water, and applying the Tube and Finger as before, there was the ſame Light, puſhing of the Finger, and ſnapping, as when the Experiment was made with the empty Plate. When the Experiment is made with Water by Day-light, by applying the End of the Finger near the Surface of the Water, it appears to riſe in a little Hill, and upon the ſnapping Noiſe falls down again, putting the Water into a waving Motion near the Place where the Water had riſen.

8. I then took a wooden Diſh, and placed it upon the Stand, firſt empty; then applying the Tube, and the Finger held near the Diſh, there appeared a Light, but no puſhing of the Finger nor ſnapping: I then filled the Diſh with Water, and the Tube being held over the Surface of the Water, there appeared a greater Light than when the Finger had been applied to the empty Diſh, but no ſnapping, till by holding the Tube after it had been well rubbed, within two or three Inches of the Finger that was held near the Surface of the Water, and then the Finger was puſhed, and a ſnapping Noiſe heard, as when the Experiment was made with the Pewter Plate.

By theſe Experiments we ſee, that an actual Flame of Fire, together with an Exploſion, and an Ebullition of cold Water, may be produced by communicative Electricity; and altho' theſe Effects are at preſent but *in minimis*, it is probable, in Time there may be found out a Way to collect a greater Quantity of it; and conſequently, to increaſe the Force of this electrick Fire, which, by ſeveral of theſe Experiments (*Si licet magnis componere parva*) ſeems to be of the ſame Nature with that of Thunder and Lightning.

Some Experiments relating to Electricity, by the ſame. No. 439. p. 166. *Dated* June 12, 1735.

3. *Feb.* 18, I tried what Effect would be produced on ſeveral Sorts of Wood with reſpect to the luminous Part of Electricity; the Wood was made into Rods of the ſame Form with thoſe Iron ones mentioned in my former Letter; the Woods made uſe of were Fir, Aſh, and Holly; theſe being ſucceſſively diſpoſed upon electrick Bodies, after the ſame manner as the Iron Rods had been, the Tube being applied to one End, there appeared a Light on it, but not with ſo great a Force, nor did the Light extend to ſo great a length; neither was the Form of it conical, but rather cylindrical; but the Extremity of it ſeemed to conſiſt of a ſhort Fringe of Light; when the Light, that was given to the Rod by the Application of the Tube, did ceaſe, upon a Motion of my Hand towards the Point of the Rod, the Light came out again, as has been mentioned of the Iron Rods; but when the Hand or Finger was held near the Point of theſe wooden Rods, there was no pricking or puſhing of the Finger felt, as when the Iron Rods were made uſe of. I had ſome of theſe Rods made much bigger at one End than the other,

and now applying my Finger to the larger End, there not only appeared a Light, but the Finger was pushed, more especially when the Holly Rod was made Use of, and the Cheek was a little pricked, but the Smart was not near so great, as when the Iron Rods were used; the great End of the Rod was pointed with a much larger Angle than the lesser one, yet there was very little, if any Difference, in the Form, or Bigness of the Light that proceeded from either End.

Having procured me 2 Pair of Lines made of Worsted Yarn, one of them of a Mazareen blue, the other of a scarlet Colour; on the 3d of *April*, I suspended the Boy first on the blue Lines, and found that all the Effects were the same, as when he was suspended on Lines of blue Silk. I then suspended him upon the scarlet Lines, but now though the Tube were as well excited, and the Experiment often repeated, yet there was no Effect produced on him, either of Attraction of a pendulous Thread, nor of pricking or burning, by applying one's Hand near him; I then laid one of the Iron Rods first upon the blue Lines, and all the same Effects were exhibited, as when the same Rod had been laid on Silk Lines of that Colour; but upon laying the same Rod upon the Scarlet Lines, no Manner of Attraction, *&c.* was perceived.

In *Philos. Transact.* No. 422 *, I gave an Account of the Experiments I made upon the communicative Electricity of Water, and that Water is attracted by the Tube, together with several remarkable Circumstances with which this Attraction is attended; but I have now found, that when the Stand with those little Ivory Cups there mentioned, be set upon any electrick Body, the same Phænomena are produced, not only by holding the Tube near the Water, but when that is removed, and the Tip of the Finger placed over the Water, *viz.* there is a little Hill, or Protuberance of Water of a conical Form, from the Vertex of which proceeded a Light and a small snapping.

May 6, we made the following Experiment. The Boy being suspended on the Silk Lines, and the Tube being applied near his Feet as usual; upon his holding the End of his Finger near a Gentleman's Hand, that stood on a Cake made of Shell Lack and black Rosin; at the same Time another Gentleman stood at the other Side of the Boy with the pendulous Thread; then the Boy was bid to hold his Finger near the first Gentleman's Hand, upon which it was pricked, and the snapping Noise was heard; and at the same Time, the Thread which was by it's Attraction going towards the Boy fell back, the Boy having lost a great Part of his Attraction, upon a second moving his Finger to the Gentleman's Hand, the Attraction ceased; then the Thread being held near that Gentleman, he was found to attract very strongly; but having since repeated this Experiment, I find that though the Attraction of the Boy is much diminished, yet he does not quite lose it, till 2, 3, and sometimes 4 Applications of his Finger to the Hand of him that stands on the elec-

* Vol. VI. Part ii. Chap. i. § iii. 3.

trick

trick Body, but without touching him. At another Time I caused 3 Persons to stand, one of them upon a Cake of Shell Lack, *&c.* the other upon one of Sulphur, the third upon a Cake of Bees-Wax and Rosin; the Persons all holding Hands, the Boy applying his Finger near the first Man's Hand, they all three became electrical, as appeared by the Attraction of the Thread, when held near to any of them.

P. S. *A Repetition of some of these Experiments, and an Addition of some others made* June 10.

June 10. in the Morning, I repeated the Experiments with the wooden Rods, the most material ones of which were made with the Holly Rod: This being laid on the Glass Cylinder, and a Fir Board about a Foot square and three tenths of an Inch thick being placed erect upon a Stand, that was set on another Glass Cylinder, so that the Center of the Board was placed near the Point of the Rod, but not to touch it by near half an Inch; then the Tube being held near the great End of the Rod, there issued out a Light from the little End of the Rod, which was that next the Board; and, as the Boy told me, it came along with a hissing Noise, and struck against the Board; when he touched the Board, there was a Light; and at the same time, another on the End of the Rod, but he heard no snapping nor pricking of his Finger, as when the Brass Plate and Iron Rod were made use of.

Experiments with the Scarlet and Blue Worsted Yarn repeated.

When the Boy was suspended upon the scarlet Lines, he attracted the white Thread at a very small Distance, but the Attraction ceased in about 6 or 7 Seconds of Time. Then the Boy being taken off, an Iron Rod was laid on the Lines, but there was no Attraction of the Thread by the Body of the Rod; but when the Thread was held near either of the pointed Ends of it, there was a small Repulsion of it, and in the Dark a very small Light was seen at each End of the Rod.

When the Boy was suspended upon the blue Lines, he attracted the Thread to him when it was held at least a Foot distance from him, and continued his Attraction to near 75 Seconds, the Iron Rod continued it's Attraction not more than 36 Seconds.

Experiments made in the Afternoon upon the Boy, when he was suspended upon Silk Lines of several Colours.

When he was suspended on the blue Lines, he continued his Attraction 50 Minutes, on the Scarlet Lines 25 Minutes, on the Orange coloured Lines 21 Minutes.

By these Experiments we see the Efficacy of Electricity upon Bodies suspended upon Lines of the same Substance, but of different Colours, and also that the Attraction continues much longer upon Silk than upon Yarn, and consequently Silk is the properest Body we can make Use of to suspend those Bodies upon, to which we would communicate an Electricity.

Concerning the Revolutions which small pendulous Bodies will, by Electricity, make round larger ones from West to East, as the

4. I have lately made several new Experiments upon the projectile and pendulous Motion of small Bodies by Electricity, by which small Bodies may be made to move about larger ones, either in Circles or Ellipses, and that either concentrical or excentrical to the Center of the larger Bodies about which they move, so as to make many Revolutions about them; and this Motion will be constantly the same Way that the Planets move about the Sun, *viz.* from the Right to the Left, or from West to East:

But

Planets do round the Sun, by Mr Stephen Gray, *F.R.S.* No. 441. p. 220. *Dated* Feb. 6, 173⅚.

But these little *Planets*, if I may so call them, move much faster in their *Apogeon*, than in the *Perigeon* Parts of their Orbits; which is, as you very well know, directly contrary to the Motion of the Planets about the Sun.

Feb. 6, 173⅚.

Electrical Experiments by the same, taken from his Mouth by C. Mortimer, *M. D. R. S. Secr.* Feb. 14, 173⅚. *being the Day before he died.* No. 444. p. 400.

Exper. I.

5. Take a small Iron Globe of an Inch or Inch and half Diamenter, which set on the Middle of a Cake of Rosin of about 7 or 8 Inches Diameter, having first excited the Cake by gently rubbing it, clapping it 3 or 4 times with the Hands, or warming it a little before the Fire; then fasten a light Body, as a small Piece of Cork, or Pith of Elder, to an exceeding fine Thread, 5 or 6 Inches long, which hold between your Finger and Thumb, exactly over the Globe, at such an Height, that the Cork, or other light Body, may hang down about the Middle of the Globe: This light Body will of itself begin to move round the Iron Globe, and that constantly from West to East, being the same Direction which the Planets have in their Orbits round the Sun. If the Cake of Rosin be circular, and the Iron Globe placed exactly in the Centre of it, then the light Body will describe an Orbit round the Iron Globe, which will be a Circle; but if the Iron Globe be placed at any Distance from the Centre of the circular Cake, then the light Body will describe an [Elliptical] Orbit, which will have the same Excentricity as the Distance of the Globe from the Centre of the Cake.

If the Cake of Rosin be of an Elliptic Form, and the Iron Globe be placed in the Centre of it, the light Body will describe an Elliptical Orbit of the same Excentricity as the Form of the Cake.

If the Iron Globe be placed in or near one of the *Focus*'s of the elliptick Cake, the light Body will move much swifter in the *Apogee* Part of the Orbit, than in the *Perigee* Part, contrary to what is observed of the Planets.

Exper. II.

Take the same or such another Iron Globe, and having fastened it on an Iron Pedestal about one Inch high, set it on a Table, then set round it a Glass Hoop or Portion of an hollow Glass Cylinder of seven or eight Inches Diameter, and two or three Inches high: This Hoop must be first excited by warming and gently rubbing it, then hold the light Body suspended as in the first Experiment, and it will of itself move round the Iron Globe from West to East in a circular Orbit, if the Hoop be circular and the Globe stand over the Centre of it, but in an Elliptic Orbit with the same Excentricity, if the Globe does not stand in the Centre of the Hoop, as in the first Experiment, when the Globe does not stand on the Centre of the Cake.

[What will happen if the Hoop be Elliptic, he did not mention; I suppose, he had not an oval Glass Hoop by him.]

Exper. III.

This same Iron Globe being set on the bare Table, without either the Cake of Rosin or Glass Hoop, the small light Body being suspended as in Exp. I, II. will make Revolutions round it, but slower and nearer to it than when it is placed on a Cake of Rosin, or within a Glass-Hoop.

He

He had not yet found that theſe Experiments would ſucceed, if the Thread, by which the light Body was ſuſpended, was ſupported by any other Thing than an human Hand; but he imagined it might happen the ſame, if the Thread ſhould be ſupported or faſtened to any animal Subſtance whatever; and he intended to have tried the Foot of a Chicken, a Piece of raw Fleſh, or the like. REMARKS.

He imagined to explain the foregoing Particular, by the following odd Phænomenon, of which, he aſſured me, he was very certain, having often obſerved it, *viz.* If a man reſting his Elbows on his Knees, places his Hands at ſome ſmall Diſtance from each other, they will gradually accede to each other, without any Will or Intention of the Man to bring them together; and they will again recede of themſelves. In the like manner, the Hand will be attracted by the Body; or the Face of a Man, if he ſtand near a Wall, will be attracted to the Wall, and be again repelled by it.

He told me, he had thought of theſe Experiments only a very ſhort Time before his falling ſick, that he had not yet tried them with Variety of Bodies, but that from what he had already ſeen of them, which ſtruck him with new Surprize every Time he repeated them, he hoped, if God would ſpare his Life but a little longer, he ſhould, from what theſe Phænomena point out, bring his Electrical Experiments to the greateſt Perfection; and he did not doubt but in a ſhort Time to be able to aſtoniſh the World with a new Sort of ***Planetarium*** never before thought of, and that from theſe Experiments might be eſtabliſhed a certain Theory for accounting for the Motions of the Grand ***Planetarium*** of the Univerſe.

In trying theſe Experiments ſince his Death, I have found that the ſmall light Body will make Revolutions round a Body of various Shapes and Subſtances, as well as round the Iron Globe, if ſet on the Cake of Roſin; thus I tried with a Globe of black Marble, a Silver Sand-diſh, a ſmall Chip-box, and a large Cork. I obſerved that the Cake, if nothing ſtood upon it, would in any Part ſtrongly attract the light Body, as held ſuſpended by the Thread; but when the Globe, or other Body, was ſet upon it, the Edges of the Cake attracted the ſtrongeſt, and ſo gradually the Attraction ſeemed as it approached the Centre to grow leſs, till at a certain Diſtance it was changed into a Repulſion, which proceeded from the Globe, or other Body placed upon the Cake, which very ſtrongly repels the light Body, unleſs it be held very near it, and then it attracts it ſtrongly. While the light Body is ſuſpended, as in the forgoing Experiments, if you approach the Finger of the other Hand near it, it will fly from the Finger, or be repelled by it with great Vigour.

6. The following Experiments I made in the Autumn of the Year 1732, and repeated them to Mr *Gray* the following Summer, when he came into the Country. I had then Thoughts of communicating them to you through his Hands, to whom they owe their Being, and drew them *Some Electrical Experiments, chiefly regarding the Repulſive Force*

up

of Electrical Bodies; by Granvile Wheler, *Esq;* *F. R. S.* No. 453. p. 98. April, &c. 1739.

up with a Letter to him prefixed: But, unwilling to be an Author, I deferred the Communication from time to time, till the second Summer came, when I was informed, that Mr *Dufay* had taken Notice of the same Solution of the repulsive Force. All Thoughts of publishing them were then laid aside, but meeting some Time since with a fuller Account of them in the *Memoirs of the Academy of Sciences* for the Year 1735; by which it appeared Mr *Dufay*'s Experiments were not the same with my own, and having since received the Commands of our worthy President to communicate them, I take the Liberty at last of sending them, as I intended they should have passed through Mr *Gray*'s Hands, if Mr *Dufay* had not appeared upon the same Subject, in 3 Propositions, and a few Corollaries.

Ottesden-Place,
Jan. 17, 173⅞.

GRANV. WHELER.

PROP. I. *Bodies made Electrical, by communicating with an electrical Body excited by Friction, are in a State of Repulsion with regard to such excited Bodies.*

EXPER. I. I hung a fine white Thread by a Loop, to an horizontal blue Silk Line, about four Feet long, tied at each End, and at about a Foot distance from it, placed a Glass Tube two Feet and a half long nearly, and one Inch and quarter Diameter, fixed in the Centre of a circular Piece of Wood supported upon three brass Skrews, so that the Tube and pendulous Thread were parallel to each other. The Tube being rubbed, the Thread was attracted and repelled 7 or 8 times (in very good Weather, I have observed it to move to and from the Tube 12 times, at above one Foot distance). I then tied a Piece of new smooth Packthread to the Top of the Tube, and to the Loop of the Thread hanging down as before, and again excited the Tube: The Thread, without coming once towards the Tube, went into and continued in a State of Repulsion; but if I only touched the communicating Packthread with my Finger, the white Thread immediately hastened to the Tube: And upon hanging another long Piece of Packthread, which reached the Ground, to the communicating Packthread, and again rubbing the Tube, the pendulous white Thread was so far from going into a State of Repulsion, that it became attracted to the Tube, and continued so, without shewing the least Tendency to a State of Repulsion, as long as the Virtue of the Tube lasted.

EXPER. II. I tied a Piece of small Cane about 16 Inches long, and ¼ of an Inch Diameter at one End, and a little more at the other, at right Angles to the Top of my Tube, fixed in the same Pedestal as before, and making unequal Arms with it; and at the End of the larger Arm, a Piece of Stick transversly, about 6 Inches long, so as it might slide backwards and forwards to and from the Tube: This moveable short Stick at one End supported a very fine white Thread, at the other a very fine blue Silk, by which

which means we had now a Silk and a Thread at the ſame time hanging parallel to the Tube. The Thread, after the Tube was rubbed, firſt was attracted, but then immediately repelled, and continued a conſiderable Time in a State of Repulſion; but upon tying to the End of the ſhorter Arm of Cane, a Piece of long Packthread, which reached down upon the Table, and rubbing the Tube again, the Thread continued in a State of Attraction, without being once repelled during the whole Virtue of the Tube, as in the preceding Experiment: Yet the Silk, whether the long Packthread was added or not, to the ſhorter Arm of the Cane, continued conſtantly attracted towards the Tube; but upon putting a ſhort Silk only ſix Inches long, in the ſame Circumſtances, it would, after ſome Time rubbing the Tube, turn into a State of Repulſion, the upper Part firſt bending from the Tube, and the lower Part towards it, the upper Bending ſtill increaſing till the whole was repelled; and, which is remarkable, the upper Part or bending, upon the Approach of the Finger, or any Body not impregnated with electrical *Effluvia*, flying towards it, and the under Part or Bending rather ſeeming to fly from it, till the whole was ſaturated, and in a State of Repulſion with regard to the Tube, and then any Part of it would come to the Finger, or any other Body, not made Electrical. It is proper to add here one more Difference remarkable between the Thread and Silk: The Thread in a State of Repulſion touched with the Finger, would immediately fly towards the Tube; but the Silk in the ſame State, after touching ſeveral times, ſtill continued in a State of Repulſion, and would not be attracted till ſqueezed from Top to Bottom between the Finger and Thumb, once, and ſometimes two or three times. And farther, the Thread would immediately turn again into a State of Repulſion, whereas the Silk, after the Violence committed by the Thumb and Finger, being attracted to the Tube, would not without a good deal of rubbing the Tube, be repelled again.

N. B. The Silk was untwiſted, and about ¼ Part of the Thickneſs made uſe of.

From the different State of the pendulous Silk and Threads at the ſame time under the ſame Circumſtances, the former being attracted while the latter is repelled, it follows, that a mere Vibration of the Parts of the Tube is not ſufficient to account for the Electrical *Phænomena*; which appears farther from the two contrary States continuing ſome time, and from the ſame Piece of Silk being at once Part in a State of Repulſion, Part in a State of Attraction. Corollary I.

That ſome Bodies immediately receive and immediately part with the Electrick *Effluvia*, but that others are ſome time before they receive it, or receive enough of it; and when they have received enough of it, part with it more unwillingly. Corollary II.

That any light Body, as a Feather, after touching or nearly approaching the Tube, muſt fly from it: Upon Contact or a near Approach, it ſaturates itſelf with the Electrick *Effluvia*, and by this means becomes Corollary III.

itſelf

itself electrical (as is plain from it's coming to all other Bodies too large to come to it); and consequently, from the foregoing Experiments, is in a State of Repulsion with regard to the Tube. As soon as it touches any other Body, it loses it's acquired Electricity, and therefore may be attracted as at first.

PROP. II. *Two or more Bodies made electrical by communicating with an electrical Body, excited by Friction, are in a State of Repulsion with regard to one another; or Bodies made electrical by Communication, repel one another.*

EXPER. I. I suspended 2 Pieces of white Thread, each about one Foot long, by Loops, upon an horizontal blue Silk Line 4 Feet long, about ½ an Inch asunder from each other; and upon holding the excited Tube over them at a little Distance, the two Threads immediately receded from each other considerably at the Bottom. I then removed one of the Threads, and held the Tube over the other, in the same manner as before. The single Thread was not observed to move to either Side; consequently the moving of the Threads side-ways was occasioned neither by the Attraction of the cross Line, nor that of the Tube, nor by the Frame of Wood, to which the cross Line was tied at each End, but only by their Action upon each other.

I then added a third String, at the same Distance from the second, that the second was from the first, and upon holding the excited Tube over the middle one, at the same Distance from the cross Sik I did before, if the Strings continued in the same Plane, the middle one stood still, and the String on each Side of it receded considerably at the bottom Part, which in this Case must necessarily happen upon a Supposition, that they repel one another equally; for the 2 contrary Forces of the outer Threads destroy each other, and consequently the middle one must remain quiet; but there was nothing to hinder the middle one from repelling the 2 outer on each Hand sideways. If, as it often happened, the 3 pendulous Threads did not remain in the same Plane, they then all receded from one another equally, and formed nearly a triangular Prism; the 3 Threads being the 3 Edges, or rather a triangular Pyramid with the Top cut off.

Upon suspending four Threads at the same Distance as before from one another, if they continued in the same Plane, they all parted, but the 2 outermost more from their Neighbours, than the 2 in the middle from each other.

If they moved out of the Plane they were first in, they formed two Prisms, each extreme with the two in the middle forming one, or rather a Parallellopepid, less at Top than at Bottom.

When five Strings were suspended, either the middlemost continued stationary when the Plane was not altered, or if it was, they formed 3 Prisms.

I after-

I afterwards placed two croſs blue Silks, of the ſame Length as before, about half an Inch aſunder from one another horizontally, and tied at each End; and upon each of theſe, at different times, hung 2, 3, 4, and 5 Threads, at the ſame Diſtances as before, when every thing ſucceeded, as it ought to have done, upon a Suppoſition of their mutually repelling one another. Exper. 2.

To each of the Ends of 2 Threads, ſuſpended as at firſt, a Feather being tied, the two Feathers manifeſtly receded from each other: And when 3 Threads had each a Feather at their Extremities, the middlemoſt became ſtationary, and the 2 outer went off on each hand. Exper. 3.

I ſuſpended afterwards 2, 3, 4, and 5 blue ſilk Strings by Loops, upon one croſs blue Silk, and found the ſeveral Experiments ſucceed in the ſame manner as in Threads; except that they remained a longer Time before they appeared in a State of Repulſion, receded from one another more ſlowly, and continued much longer in the repulſive State, after the Tube was removed. Exper. 4.

This done. I made ſeveral Experiments, by mixing Silks of different Colours, and Silks and Threads of different Colours, and ſuſpended them by Turns upon Silks of different Colours, whence aroſe ſeveral different *Phænomena*, which I ſhall not take Notice of here; but I muſt not omit mentioning, that upon ſuſpending two black Silks at the before-mentioned Diſtances from each other, upon a ſcarlet croſs Silk, they did not only open and recede from each other at the Bottom conſiderably, but when the Tube was held under, ran or jumped away from each other, to the very Ends of the croſs red Silk that ſupported them, taking 2, 3, or more Jumps from each other. I obſerved the ſame of two white Silks ſuſpended upon red Silk, but think they did not move away ſo briſkly as the Black. Exper. 5.

I tried whether Threads hanging parallel, as above, from a croſs blue ſilk Line, and joined with one or more tranſverſe Threads, ſo that the perpendicular Threads remained nearly parallel, would mutually repel when the Tube was held over them; they ſeemed to repel each other full as ſtrongly as before. When they were joined by only one croſs Thread towards the Top, the lower Parts ſeparated conſiderably; when joined by 2 croſs Threads, one towards the Top, and one towards the Bottom, they ſeparated both in the middle Parts between the 2 croſs Threads, and at their lower Ends under the ſecond or loweſt croſs Thread. When ſeveral were tied together at the Top and Bottom, and about a Foot long, not by tranſverſe Threads, but in a Knot at each End, they all bellied out from one another, deſcribing a Figure generated by an Ellipſis, revolved about it's greater Axis; approaching nearer to a Sphere, the ſtronger the repulſive Force was. And, though it was only a neceſſary Conſequence, I could not without ſome Pleaſure obſerve the Knot at the Bottom, as the Strings ſwelled out, ſenſibly riſing up. I could ſcarce forbear imagining my Bundle of Silks, a Bundle of muſcular Fibres. Exper. 6.

 I ſuſpended

Exper. 7. I ſuſpended two Braſs and afterwards two Iron Wires upon a croſs blue Silk, in the ſame manner as the Threads and Silks before-mentioned, and found the Experiments ſucceed as in Threads of the ſame Number, except that they did not recede ſo far from one another, which muſt neceſſarily follow from their greater Weight.

N. B. Theſe Experiments were made ſometimes with the Tube held over, ſometimes held under the croſs Line; but they generally ſucceeded beſt when the Tube was held under the Extremities of the pendulous Wires, which in this Caſe ſeparated much farther, and kept their repulſive Force much longer.

Exper. 8. I muſt not omit here, that I hung up 2 Fragments of Barometer Tubes, each about a Foot long, by blue ſilk Lines going through each, ſo that they hung parallel, horizontal, at equal Heights, and about ¼ of an Inch aſunder; upon holding the excited Tube above and under them, they manifeſtly receded from each other.

I ſuſpended the ſame Fragments of Tubes by blue ſilk Lines of equal Length, from a croſs blue Silk in a perpendicular Poſture, each having a little red Sealing-wax at the upper End, to hinder the Strings from ſlipping off: The excited Tube being brought near them, they receded manifeſtly, eſpecially at the lower Ends; the Diſtance from one another, when at Reſt, being about ¼ of an Inch.

Corollary I. From the repulſive State of the Pendulous Threads tied tranſverſly with two or more Threads, and bending out from each other, where at Liberty, it follows that all the Threads of a Table-cloth, or other large Piece of Linen, when made Electrical, (as has been often done all over) have a Deſire to fly from each other; and conſequently, was the repulſive Force ſtrong enough, the whole would be diſſolved, or torn in Pieces. A ſhort Thread of black Silk, by repeated Applications of the Tube, I have ſeparated into it's ſmalleſt Fibres: Whence is ſuggeſted more plainly, than from any other known Experiment, a Reaſon for the Diſſolution of Bodies in their reſpective Menſtruums, *viz.* That the Particles of the Solvend having imbibed the Particles of the Menſtruum, ſo as to be ſaturated with them, the ſaturated Particles become repulſive of each other, ſeparate, and fly to Pieces.

And hence, perhaps, ariſes a Reaſon, why Particles of Bodies ſpecifically heavier than the Menſtruums in which they are diſſolved, are, after the Diſſolution and Diſperſion, ſuſpended all over the Menſtruum, *viz.* That they repel each other. Attraction is inſufficient; for Parts attracted equally in all Directions, are, in Effect, not attracted at all, and the Imperfection of the Fluid will not do; for if this occaſioned the Suſpenſion, ſtriking or joggling the Veſſel would make them ſubſide.

Corollary II. Hence we plainly ſee how Heat may divide the Particles of Water with greater or leſs Force, in Proportion to the Degree of Saturation, and throw them into the Air; where they may continue to aſcend, if at the ſame time they are divided, they are expanded into little Shells or Bubbles, of a Diameter large enough to be ſpecifically lighter than the lower Air, as the Great *Halley* has ſagaciouſly conjectured. Or if the upper Parts of the Air, as being leſs ſaturated than the lower Parts, may be able to draw them upwards, till the Exceſs of Weight, which is conſtantly increaſing, is equal to the Exceſs of Attraction.

Bodies made Electrical by rubbing, do themſelves repel one another, or the electrical excited Bodies themſelves repel one another. PROPOSITION III.

The two Fragments of Tubes before-mentioned *, being ſuſpended horizontally, and in a Poſture parallel to each other, I held in one Hand, and with the other rubbed ſome time; then gently letting them go ſo as to be at Reſt, I could plainly perceive them recede from each other towards that End which had not been taken hold of. Exper. 1.

But as upon repeated Trials I found it difficult to make this Experiment ſucceed unexceptionably, the Tubes generally having ſome reciprocating Motion of their own, after quitting the Hand I made uſe of the following Method.

I ſuſpended a ſingle little Tube about a Foot long, by a long blue ſilk Line, perpendicularly, and upon a Table placed my great Tube fixed in a Stand as before, excited each alternately, two or three times; then gently moved the Tube with the Stand it was fixed in, near the ſuſpended little one: The little Tube manifeſtly receded ſo much, that a croſs blue ſilk Line ſtretched horizontally at about an Inch Diſtance on the oppoſite Side, would ſometimes, upon the firſt Approach of the great Tube, be touched by it.

Three ſcarlet Silks, pendulous each by Loops from a croſs ſilk Line, and cloſe together, being rubbed downwards two or three times, between the Finger and Thumb, ſhewed a conſiderable repulſive Force with Regard to each other, forming themſelves immediately into a triangular Pyramid and continuing in this State of Separation ſome time, and which ſhews their Attraction at the ſame time, with Regard to other Bodies not excited, coming to them when brought near them. Exper. 2.

I obſerved the ſame repulſive Force in 3 yellow and 3 green Silks, under the ſame Circumſtances, and excited in the ſame manner, but not in ſo great a Degree as in Scarlet. In Blue the repulſive Force was ſcarce diſcernible after ſeveral times rubbing.

The Rev. Dr *Stephen Hales* †, obſerves, " That if a Piece of " one of the *Bronchiæ*, or Gills, of the *Muſcle Shell-Fiſh*, be cut " off, and put into a ſmall concave Glaſs, with 3 or 4 Drops of " it's Liquor, and be then placed under a double Microſcope, " the Blood may be ſeen greatly agitated in the fine Veſſels; and at the " cut Edge of the Piece of Gill, may, with great Pleaſure, be ſeen many Scholium.

* Prop. II. Exper. VIII. † Statical Eſſays Vol. II. Exp. XI. Art 12.

" Blood-Globules, repelled from the cut Orifices of the Blood-Veſſels, " and attracted by other adjoining Veſſels; alſo other Globules rolling " round their Centre, and repelling each other; whence (as he ſays) it " is plain, that Bodies, by briſk rubbing and twirling about, may ac- " quire, in a watery Fluid, both attractive and repulſive Virtue or " Electricity."

From our laſt Experiments we are led to think, that the Globules of the Blood, if by Friction they acquire an electrical attractive Virtue, muſt of neceſſity repel one another; and that Electricity is not ſo properly called an attractive and repulſive Virtue, as a Virtue attractive of thoſe Bodies that are not attractive themſelves, and repulſive of thoſe that are; and that this repulſive Force of the electrical Blood-Globules, excited by Friction, as they flow in their Channels (and particularly in the ſmall ones, and perhaps more ſo in thoſe of the Lungs, where the refrigerating Power of the Air may aſſiſt, as Dr *Hales* has obſerved); this repulſive Force of the Blood Globules, I ſay, may be the great Cauſe that hinders the Blood from coagulating as it circulates; may be the great Cauſe of the conſtant Perſpiration in an healthy State, and of the Increaſe of it, *cæteris paribus*, in Proportion as the Velocity and Friction of the Blood increaſes.

If theſe things are ſo, the Neceſſity of Exerciſe appears more plainly than ever, in order to keep the Body in an healthy State, as we may obſerve here the very Steps that Nature makes uſe of to free herſelf from her Suppreſſions.

An Account of ſome of the Electrical Experiments made by Granvile Wheler, *Eſq; at the Royal Society's Houſe,* May 11. 1737. *drawn up by* C. Mortimer, *M. D. R. S. Secr. Ibid.* p. 112.

Exper. 1.

7. A large Octavo Book was placed horizontally upon Silk Lines, and the upper Surface ſtrewed with ſeveral Pieces of leaf Braſs, all or the greateſt Part of which flew upwards, from one another, and off the Book, upon holding an excited Tube at a little Diſtance underneath the Book.

Exper. 2.

Two Lines were extended horizontally the whole Length of the Library being between 30 and 40 Feet, diſtant from one another about 2 Feet at one End, and meeting together in a Knot at their other Ends, the whole Lines being Packthread, except 5 Feet of ſilk Line tied at each of the ſeparated Extremities, as well as at the Knot where the other Ends united, in order to ſtop the Current of the *Effluvia*. Upon the united Extremities was placed horizontally a Piece of Card about 2 Inches ſquare, on which were ſtrewed Pieces of leaf Braſs: The excited Tube being held at a little Diſtance under the ſeparated Extremities of the Packthread, the leaf Braſs on the Card at the other End flew upwards, and off the Card.

Exper. 3.

Five Glaſs Receivers placed one within another upon an electrical Cement of Bees-wax and *Venice* Turpentine, were all exhauſted: In the innermoſt a fine white Thread about 5 Inches long, was ſuſpended from the Crown of it, by the Aſſiſtance of a little Cement made of Bees-wax and Oil. Upon moving the excited Tube up and down near the Side of, and horizontally to and from, the outward Receiver, the ſuſpended Thread

Thread manifeſtly made many Vibrations correſponding to the Motions of the Tube.

An electrical circular Cake of Bees-wax and Roſin, ten Inches in Diameter, was placed horizontally upon a tall Glaſs Receiver near 3 Feet high, ſuch as is made uſe of for the dropping the Feather and Guinea. This Cake being, the preceding Evening about Eight o'Clock, warmed with an hot Iron held over it, and then ſtruck perpendicularly all over it's Surface with the Hands in parallel Directions, and ſo left covered with a thin Paſteboard, was about 12 next Day at Noon gently uncovered, and an Ivory Ball about 1 ½ Inch Diameter placed in the Centre, a fine white Thread about 10 Inches long, with a ſmall Piece of Cork, the Size of a Pin's Head, at the End of it, being held between the Finger and Thumb, was gently let down upon the Vertex of the Ball; it firſt flew off at ſome Diſtance, and then made ſeveral pretty regular Revolutions from Weſt to Eaſt about it, in the Form of a Circle. Exper. 4.

The Ball was removed, and the Cake again warmed and excited as before; after which the Ball was replaced at a little Diſtance from the Centre, nearer to Mr *Wheler*; the Conſequence of which was, that the pendulous little Body moved with a direct Motion as before, but in an Orbit that reſembled an Ellipſe, having the Ball in one of it's *Foci*. Exper. 5.

Two Bullets fixed on little Stands of Cork about ¼ of an Inch high, were placed upon the Cake, each about an Inch diſtant from the Centre of it, and in a Line with the Centre and Mr *Wheler*; the pendulous Body deſcribed an Orbit reſembling an Ellipſe, having the two Bullets for it's *Foci*, and the Motion was direct from Weſt to Eaſt. Exper. 6.

Inſtead of the Cork, another pendulous Body of a cylindrical Form was made uſe of, tied to a fine white Thread about 20 Inches long; the Cylinder conſiſted of two circular Baſes of Paper ½ an Inch Diameter, but all cut away except a Ring and a ſmall Bar croſs the Middle, through which Baſis 6 equal fine Threads paſſed at equal Diſtances from one another, knotted at the lower Baſe ſeparately, and joined together in one Knot at about ½ an Inch Diſtance from the upper Baſe, from which Knot proceeded the long Thread. This Body moved from Weſt to Eaſt about the central Ball, and at the ſame time diſcovered a Motion about it's own Axis in the ſame Direction; but after two or three Turns generally ſtopt, and turned the contrary Way, which ſeemed to ariſe from the untwiſting of the Thread. Exper. 7.

A Thread about a Foot long, was ſuſpended from a horizontal Line of Packthread, parallel to it an excited Tube placed erect in a Stand, the Thread approached the Tube, and continued in a State of Attraction: A Thread of the ſame Length, ſuſpended from a Silk Line, vibrated backward and forwards 2 or 3 times, being firſt attracted, and then repelled, and continuing ſome time repelled; but upon joining the Top of the Tube, by a Packthread going round it, to the Loop of the Thread, the Thread continued conſtantly in a State of Repulſion, ſhewing no Tendency to Attraction. Exper. 8.

Two

Exper. 9. Two black Silks, about the same Length with the Thread in the preceding Experiment, were suspended by Loops from an horizontal red Silk Line, at the Distance of about ½ an Inch from each other; upon holding the excited Tube under them, the Silks swelled out from one another, and then jumped away on each Hand to the Distance of 2 Feet.

Exper. 10. A circular Board of nearly the same Diameter with the electric Cake, was suspended horizontally by 6 Silk Lines, tied to one Silk Line which was brought over a Pulley at the Top of a Frame of Wood, so as to be moved up and down. From the Board hung 6 fine white Threads about 18 Inches long, fixed by a little Cement at equal Distances from each other. The Board being let down till the Ends of the Threads were about an Inch distant from the electric Cake, which was directly under, and had the Ivory Ball on it's Centre; the Threads all approached towards the Centre of the Cake, both when the Ball was in the Centre, and when taken away, keeping an equal Distance from the Centre, and from one another, as long as a Packthread joined the Circle of Board and the Frame to keep it steady; and upon removing the Ball out of the Centre towards the Circumference, the Figure lengthened, the Threads next the Ball advancing nearer the Circumference; when the Ball was placed at about an Inch Distance from the Circumference, the Thread that was before nearest the Circumference, whipped between the Ball and the Centre, so as to be almost in the same Plane with it's two neighbouring Threads, the Figure formed by the Extremities resembling an Ellipse with one End cut off: But when instead of the Packthread that joined the Board to the Frame, a blue Silk Line was tied in the same manner in all respects, the Threads, instead of coming towards the Centre, all flew away at a great Distance from the Cake, and from one another.

It ought to be observed in the Experiments of the circular Motion of the pendulous Body, that Mr *Wheler*'s Hand seemed as steady as possible, except in the first Experiment, when a litttle Trembling appeared; Mr *George Graham* taking a very good Method to observe it, by keeping his Eye fixed upon a Point at a considerable Distance, in the same Line with the End of Mr *Wheler*'s Finger, and his own Eye.

Yet when Mr *Wheler* had finished the Experiments to the Satisfaction of all present, Mr *Hawskbee*, Mr *George Graham*, and Dr *Mortimer*, held the Thread with the pendulous Body over the Cake with the Ball on it's Centre, after the Cake had been excited by Mr *Wheler*; but they had no regular Revolutions at all, though several very manifest Motions were made with the Hand, to try if a projectile Motion might by that means be given to the pendulous Body. Mr *Wheler* had tried the same thing with his Servant; from whence it is reasonable to conclude, that it is necessary, that the same Person who excited the Cake should likewise hold the Thread; as if there were some Analogy between the *Effluvia* excited by the clapping of the Hand on the Cake, and the *Effluvia* which

may

may be communicated along from the Hand which holds the Thread to the Piece of Cork at the End of it. And this ſeems to be the Reaſon of what the late Mr *Gray* told me, *viz.* That there was ſomething in the human Hand eſſential to the Experiment, which he had not yet found in any other Supporter of the Thread.

Some Remarks on the late Mr Gray's *Electrical Circular Experiment, by* Granvile Wheler, *Eſq;* *Ibid. p.* 118. *dated* Feb. 20. 1737-8.

8. Some odd Circumſtances led me to make Mr *Gray*'s circular Experiment in the following Manner. While I excited a Cake of Roſin and Bees-wax 10 Inches Diameter, by clapping with my Hand, I let my Ivory Ball continue in a Baſin of Water; then ſhaking off the Drops, placed it in the Centre, and with my right Hand held a fine Thread, about 8 or 9 Inches long, having one End rolled up into a little Ball, and the other, for about an Inch, reduced to it's greateſt Fineneſs, to only one Fibre, myſelf and Hand being ſupported on the Back of a Chair. The Succeſs was, I had a great many Revolutions, to the Number of 50, from Weſt to Eaſt; but at firſt not ſo regular as towards the laſt, at firſt deſcribing only about ⅓ of the Circumference at a Time, and after ſtanding ſtill a little, deſcribing another third Part. I might probably have had a great many more Revolutions, but being tired, I was forced to reſt myſelf, which I did for 10′, then took up the Thread again. The Thread ſtood repelled at ſome Diſtance, without making any Revolutions, and at laſt only made half an one the contrary way to what it did before; but upon wetting it, by drawing it 2 or 3 times over the Surface of the Water, it made again 20 more Revolutions from Weſt to Eaſt, only at a ſmaller Diſtance from the Ball, (for the Water muſt make it heavier) but full as regular as before, and rather quicker: The Virtue of the Cake muſt now have laſted about three quarters of an Hour. After reſting about 6′, I tried again with the String freſh wetted, the Ball and Cake continuing as before; and had, to my great Surpize, 100 Revolutions in the Space of about 12′, the Revolutions being ſtill quicker, and more regular, and nearer the Ball; and at the ſixth Revolution of this laſt Hundred, the Thread was attracted to the Surface of the Ball, and, being wet, did not diſengage itſelf, till pulled away; yet after this, deſcribed the remaining Ninety-four Revolutions of the Hundred, and ſeemed inclined to deſcribe a great many more, but that I was forced to reſt my Arm again, which I did for about 8′, then tried again, the Thread being freſh wetted, and had 70 Revolutions at nearly the ſame Diſtance from the Ball in leſs than 9′, all very regular, and without any Attraction of the Thread to the Ball. I reſted again 16′, wet the Thread again, and held it as uſual; it was repelled at about ½ Inch Diſtance from the Ball, but ſeemed to have no Tendency to a circular Motion; yet after continuing ſtationary about 1′, I perceived a Motion about it's Axis, about which it took ſeveral Turns; but ſtill had little or no progreſſive Motion, till about 1′ longer, when it began to move forward, and continued doing ſo from Weſt to Eaſt, for about 33 Revolutions, very regular, but ſlower than in the 2 laſt Caſes, the String having been held about 10′, and the Revolutions performed.

performed in about 7 or 8 of them. I obſerved each of theſe 3 laſt times, it was rather longer before the progreſſive Motion began than uſual; and in all the Trials of this Experiment, I frequently perceived a Motion about the Axis, which was generally from Weſt to Eaſt, though now-and-then the contrary Way. The Virtue of the Cake muſt now have laſted near 2^h; about ¼ of an Hour after, I tried again, and had 60 Revolutions from Weſt to Eaſt, in about 10′, the Diſtance from the Ball being ſtill leſs than before, hardly ⅙ of an Inch, ſcarce any Revolution about the Axis appeared, and at the Beginning the Thread was twice attracted to the Ball. About an Hour and half after, the Virtue of the Ball was not quite gone, the wet Thread being repelled, and making 3 or 4 Revolutions from Weſt to Eaſt, as well as moving a little about it's Axis the ſame Way. But as it was reaſonable to ſuppoſe the Ball itſelf in the Centre of the Cake was now dry, with a Feather dipped in Water I wet it's Surface; yet found no Increaſe of Virtue, rather a Diminution of it, the pendulous Body ſeeming ſcarce at all repelled; but it is to be obſerved, that the Ball, as it was wetting, twice tumbled over, and rolled upon the Surface of the Cake; by which means the Virtue of the Cake might be much diminiſhed.

It is not improper too to take Notice here, that during the Revolutions of the wet String, I have frequently obſerved a kind of Oſcillatory Motion, as if there was an alternate Intention and Remiſſion of the repulſive Force. As alſo that I have often taken Notice of little Plucks, and convulſive Motions, in the pendulous Body, and ſometimes thought I have felt ſomething like it in my Arm that held it, though at no other time have I ever been ſenſible of any ſuch thing.

I have ſeveral times ſince repeated this Experiment with the Thread and Ball both wet, and found it ſucceed much better than when they were both dry; and once I find by my Notes, I had two hundred and twenty Revolutions before I reſted my Arm. I have tried too with the Ball dry, and the String only made wet; but the Virtue did not continue ſo long, as when both were wet.

I now flattered myſelf with Hopes of Succeſs, if the Thread was ſuſpended from an undoubted fixed Point, which therefore I proceeded again to try with the greateſt Care and Caution, but in vain; the Revolutions were uncertain.

This Difference naturally led me to reflect upon the Cauſe of it. The Tremor of the Hand would not account for it; for this being both ways backward as well as forward, muſt as often hinder as promote a continual Motion one way: And though in two oppoſite Parts of a Circle, the Motion is really in contrary Directions, and therefore the contrary Impulſes of a Tremor may promote a Revolution applied at oppoſite Places of the Orbit; yet as theſe Tremors are irregular, and ſucceed much quicker than the Revolutions are performed, they ſeem inſufficient to account for the Motions of the pendulous Body, performed with any Degree of Regularity.

A Stream

A Stream of Air in my Room might impel along the Tangent the pendulous Body, kept at a Diſtance from the Ball by it's repulſive Force; and then Gravity, taking place, might with the firſt Motion compound a Curve; but ſtill the Reſiſtance of the Air would ſoon deſtroy the original Impulſe, could a few Revolutions be performed; and beſides, one Revolution could not be performed, becauſe the ſame Stream of Air that began the Motion, muſt be contrary to it in it's Return.

A Finger held on the right Hand near the pendulous Body, when ſuſpended from a fixed Point, will make it revolve from Weſt to Eaſt; but then it muſt be applied and removed alternately: The repulſive Force therefore which the Arm may acquire, by being held in the Sphere of the *Effluvia*, is inſufficient; for, as it is in one Place, it muſt impel only one way, and conſtantly the ſame way; and therefore, like a Stream of Air in the Room, though it might create the Beginning, it muſt hinder the Completion of a Revolution.

Sometimes I have doubted, whether the Pulſe of the Arm might not be aſſiſting in giving a projectile Motion. When one Leg is laid over the Knee of the other, a Motion and Heaving of the Leg that lies over, anſwering to every Stroke of the Pulſe, is very apparent at a Diſtance: The Arm therefore in ſome Poſtures, in which it's great Artery meets with a proportionable Preſſure or Reſiſtance, may have a conſtant Motion, though leſs perceivable.

What ſeemed the moſt probable Solution, was this: When the Arm is extended, the Poſture being uneaſy, there muſt be a Reaction of the Muſcles, or a continual pulling of the Arm towards the Body. When therefore the right Arm is made uſe of, the pulling will be from Right to Left; and conſequently the Motion produced in the Body held by it in the ſame Direction, or from Weſt to Eaſt. When the left Arm is made uſe of, the Reaction of the Muſcles will be from Left to Right, and therefore the Motion of the pendulous Body from Eaſt to Weſt. And, agreeably to this, I have obſerved, (as I formerly took Notice, though this Reaſon did not then occur to me) when I uſed my left Hand, all other Circumſtances continuing the ſame, the Motion of the pendulous Body was from Left to Right, or from Eaſt to Weſt, contrary to what was obſerved when held by the right Hand.

Yet ſtill neither of theſe Solutions would account for the Variety of Oddneſſes I have met with under various Circumſtances.

I proceeded therefore to try with Reſts for my Arm of different Heights, having an Arm of Wood, about 2 Feet long, fixed to a Reſt for my Teleſcopes, which I could raiſe to any Height I wanted; and I found the Experiment ſucceed only well, when the Reſt was lower than the electric *Area*, and the Arm was ſupported upon it's Elbow, which was the Poſture conſtantly made uſe of, when reſted upon a Chair, the Chair being lower than the electric *Area*, that it might leſs affect the *Effluvia*, as was then thought.

I began now to think with myself, whether it was not possible, that an Inclination to a Motion one way in the Person that holds the Body, might not have such an Influence upon the Arm, and consequently the String and pendulous Body, as to determine them the same way by some Pressure or Byass put upon it, though no Motion sensible even to himself, was produced in the Hand. If so, I might, by a contrary Inclination, produce a Motion the contrary way. Having therefore a fine Day, and my circular Cake being well excited, I tried if I could not produce a regular Motion from East to West, about the Ball in the Centre, having my Hand supported, as usual, upon the Back of a Chair. I found I could produce a very regular one from East to West for many Revolutions, and change from one Motion to another, without being sensible I moved my Hand at all.

I then wet the Ball and String, as in the Experiment before-mentioned, and found I could tire myself with a Motion either from East to West, or from West to East, as I pleased, without giving any Motion, that I could perceive, to my Hand or Fingers. Hence many odd Experiments that please, may, when repeated, succeed.

Since therefore the Motion of the pendulous Body from a Point undoubtedly fixed, is irregular, as I have found by many different Experiments, repeated with the greatest Care and Caution; and since I am convinced from these last-mentioned Trials, the Motion from West to East, and from East to West, must generally have been determined by myself; I am inclined to think, that a Desire of producing a Motion from West to East, was the secret Cause that determined the pendulous Body to that Direction, by some Impression from Mr *Gray*'s Hand, as well as my own, though I am persuaded at the same time, he was not sensible of giving any Motion to his Hand himself: And I the rather think this was the Case, from the Instance Mr *Gray* gives, by way of Explanation, of a Man resting his Elbows upon his Knees, this implying that he rested his Arm upon his Elbow, as I did myself.

But though upon the whole it does at last appear, that this Motion from West to East in a pendulous Body, applied to another in the Centre of an electric *Area*, is to be ascribed to the Hand that holds it, and not solely to the Nature of the electric *Effluvia*, or the Figure of the central Body; yet still, perhaps, it may not be improper for Astronomers to consider, whether or no a Medium with this Property, that all Bodies immersed in it, are repulsive of one another, ought not to be joined with Gravity to explain the heavenly *Phænomena*; especially since the *Phænomena* of Fire, and our electric *Effluvia*, have a great Affinity to each other; and since many of the heavenly *Phænomena* are to be accounted for, upon this Supposition, with great Simplicity; and some of them, that have not yet perhaps been fully accounted for, seem necessarily to follow.

Some Thoughts and Experi-

9. The *Phænomena* of Electricity are so odd, that though we have a great many Experiments upon that Subject; we have not yet been able

from

from their Comparison to settle such a Theory as to lead us to the Cause of that Property of Bodies, or even to judge of all it's Effects, or find out what useful Influence Electricity has in Nature: Though certainly, from what we have seen of it, we may conjecture, that it must be of great Use, because it is so extensive.

ments concerning the Electricity, by J.T. Desaguliers, *LL.D. F.R.S.* No 454. p. 186. July, &c. 1739.

Though some Persons have been too hasty in their Conjectures, and too apt to run into Hypotheses not sufficiently supported by Experiments, yet it would be of great Use to settle some general Propositions concerning Electricity from the Light we have already, and what we may further discover by future Experiments; provided we have a sufficient Number of them to settle a general Rule. For Example; I now propose some general Assertions to be considered, and to be rejected or allowed of as a Number of Experiments shall determine; but to stand only as *Queries* till they are settled.

I have hitherto avoided entertaining the *Society* upon this Subject, or pursuing it so far as I might have done, (considering that I can excite as strong an Electricity in Glass, by rubbing it with my Hand, as any body can) because I was unwilling to interfere with the late Mr *Stephen Gray*, who had wholly turned his Thoughts that Way; but was of a Temper to give it entirely over, if he imagined, that any Thing was done in Opposition to him. But now I intend not only to go on myself in making electrical Experiments, but shall always be ready to make such as shall be proposed by any Member of the *Society*. The *Queries* which I have already examined, are the following:

Whether all Bodies in general are not capable of receiving the Electricity which has been given to a Tube by Friction, though there be a great many Bodies, such as Metals and Vegetables, &c. in which we have not hitherto been able to excite any Electricity by Heat, or Friction, or any other Operation on the Bodies themselves? Query 1.

Whether when a String is stretched out at Length, with a Body hanging at one End of it, to which Body we would communicate the Electricity of the Tube rubbed at the other End, the Supporters of the String ought not to be of such Bodies as are capable of having Electricity excited in them by Friction, Heating, Beating, or Patting, or some immediate Operation on the Bodies themselves? Query 2.

Whether these Supporters of the String (mentioned in the last *Query*) which stops the electrical Virtue from passing any farther, are not of such a Kind as are incapable of having the electrical Virtue excited in them immediately by any Operation yet known; though they are all capable of receiving it from a rubbed Tube, even at a great Distance, by the Communication of a String made of vegetable Substances? Query 3.

Whether the Reason, that some Supporters transmit the Electricity running from the rubbed Tube along the String to Bodies beyond them, be not as follows, *viz.* That having received some of the electrical Stream, they soon become saturated with it, and so receiving no more of it, let the rest pass on without disturbing it? Query 4.

Query 5. Whether the Reafon, that Supporters made of vegetable Subftances, Metals, and fuch others, as ftop the Electricity above-mentioned from running any farther along the String than the Place where it refts upon them, be not this? *viz.* That they are never faturated with the electrical Stream, but continually receive it, and tranfmit it to the next contiguous Body, provided that contiguous Body be of the fame Kind with themfelves, and alfo contiguous to other Bodies of the fame Sort: I mean fuch as would ftop the Electricity, if the String was fupported by them. For even thefe Supporters will tranfmit the Electricity, if terminated at each End by Bodies that tranfmit the Electricity, when they fupport the String.

Query 6. Whether we may not diftinguifh all Bodies in general, in refpect of Electricity, into fuch as may be excited to Electricity, and fuch as cannot be excited to Electricity? The two Kind of Bodies receiving the Electricity from other Bodies into which it has been excited differently; the firft alfo tranfmitting the Electricity, while the others do not.

Thefe *Queries* are fuch as arife from a Confideration of Experiments made by others, and fuch as I have made myfelf.

As I go on in making other Experiments, other *Queries* may arife, and I fhall communicate them.

Here follow the Experiments I have already made, and am ready to repeat as the *Society* may defire.

Experiments relating to Query 1. I ftretched a Cat-gut about 5 Feet in Length, and faftened it to the Top of 2 Chairs in an horizontal Situation, and fuch another Cat-gut String to 2 other Chairs parallel to the firft, and at the Diftance of 15 or 20 Feet from the former. Then I fufpended one End of a Packthread to the Middle of the firft Cat-gut, and carried it on fo as to lay it over the Middle of the other Cat-gut, and leave the other End of the Packthread hanging down about a Foot below the Cat-gut, with a Loop to hang feveral Bodies to it, fucceffively to receive the Electricity excited by the Tub, and applied to the other End of the Packthread.

All the Bodies I tried received the Electricity communicated from the rubbed Tube along the String, which appeared by holding a Thread faftened to a Stick, the Thread being attracted towards the fufpended Body.

1. A Gold Medal. 2. A Silver Medal. 3. A Copper Medal. 4. A Brafs Ball. 5. A Steel Ball. 6. A Tin Ball. 7. A Leaden Ball. 8. Sulphur. 9. Sealing-Wax. 10. Pumice-Stone. 11. Bees-Wax. 12. Refin. 13. *Sal Ammoniac.* 14. Ivory. 15. Human Bone. 16. Fifh-Skin. 17. Load-ftone. 18. Flefh. 19. Cotton. 20. Wax-Candle. 21. Tallow-Candle. 22. A Leak. 23. Celeri. 24. Tobacco-Pipe. 25. A Glafs Ball. 26. A Rufh rolled up.

Experiments relating to Query 2. Retaining the firft fupporting String of Cat-gut, inftead of the laft Cat-gut Supporter, I made the Packthread pafs over the following Subftances fucceffively, all which tranfmitted the Electricity to the Body fufpended at the End of the Packthread; *viz.* 1. A Silk String.

2. Hair

2. Hair Rope. 3. Parchment. 4. A Thong of Sheep-ſkin, but it ſtopped the Electricity till it was dry and warm. 5. A Liſt of Woollen Cloth. 6. A Liſt of Flanel. 7. Cadis, or a Kind of Worſted Tape. 8. Quills. 9. Whalebone. 10. A Man's Thigh-Bone. 11. A Bladder. 12. A Cat, held between two. 13. A Tallow-Candle. 14. A Wax-Candle (the String was alſo laid over the unburned Cotton Wick at the End of the Candle). 15. A Tallow-Candle and it's Wick. 16. Tobacco-Pipe, with a Cat-gut or a Packthread through it, or without, that is, a Packthread String being faſtened at each End of it. 17. A Sword-Belt. 18. A Piece of a white Hat. 19. A Piece of a black Hat. 20. A Glaſs Tube. 21. The ſame with Water in it. 22. With Spirit of Wine. 23. The ſame with Mercury in it. 24. Sealing-Wax. 25. Crape.

N. B. All theſe Subſtances, except the Sheep-ſkin, the Tobacco-Pipe, the Quills, the Candles, and the Bone, not only tranſmitted the Electricity, but became ſo far electrical, as to attract the Thread a little Way on each Side of the ſupported Packthread.

There are more Experiments required to be made, before this *Query* can be turned into an Aſſertion.

Inſtead of the laſt Supporter of Cat-gut near the ſuſpended Body, I made uſe of the following Subſtances ſtretched from Chair to Chair; and then the Thread hanging on the Stick was not at all attracted by the ſuſpended Ivory Ball, which I made uſe of in all the Experiments to try the Supporters. *Experiments relating to Query 3.*

1. A Hempen Rope. 2. A ſmall Packthread. 3. A drawn Sword. 4. A Sword in the Scabbard. 5. The Scabbard without the Sword. 6. A twiſted Cotton Thread. 7. Tape made of Thread. 8. Bars, Tubes, and Wires of Copper, Braſs, Iron, and Lead. 9. White Paper and brown. 10. A moiſt Thong of Sheep-ſkin. 11. Celeri. 12. Leeks. 13. Fir-wood. 14. A Cane. 15. A Piece of black Thorn. 16. The ſame Ruſhes that had before received the Electricity when ſuſpended. 17. A Spunge dry. 18. White Thread. 19. Hay. 20. A Marble Slab.

N. B. Such Bodies as were too ſhort to reach from Chair to Chair, were lengthened out by Pieces of Packthread at each End.

The Cat-gut Supporters, and all the others mentioned in the Experiments to *Query* 3, which tranſmitted the Electricity, attracted the Thread of the Stick near the conducting Packthread, but not ſo far as the Chairs to which the ſaid Supporters were faſtened. *Experiments relating to Query 4.*

All the Supporters which did not tranſmit the Electricity, when they reached from Chair to Chair, were made to tranſmit, when they were lengthened out with Cat-gut at each End, and then they became electrical *Experiments relating to Query 5.*

cal themſelves from one End to the other, as becoming Part of the ſuſpended Body; and becoming ſo ſaturated, as not to be able to carry the Electricity on either Side any farther than the Cat-gut to which they were faſtened.

Experiments relating to Query 6. The late Mr *Stephen Gray* has, by rubbing, excited Electricity in ſeveral of thoſe Bodies which I have made Supporters of to tranſmit the Electricity: I have done the ſame with ſeveral others, but not with all of them, though I ſhall try them all: But as it is more difficult to excite that Virtue in ſome than others, and all the Experiments in general ſucceed better in dry and cold Weather than in moiſt and warm, I muſt wait for proper Opportunities to make the Experiments, and then I ſhall communicate them.

Experiments concerning mixed Subſtances. 1. Cadis (or Woollen Tape) laid on Thread-Tape, when made a Supporter, tranſmitted the Electricity.

2. When the Thread-Tape was uppermoſt, the Electricity was ſtopped.

3. When they were twiſted together, the Electricity was tranſmitted, but moſt weakly when the Packthread going to the Ball was laid over that Part of the Twiſt which had the Thread-Tape.

N. B. The two Paper Supporters which did not tranſmit the Electricity, ought to have done it according to *Query* 2, becauſe, by Mr *Gray*'s Experiments, Electricity is to be excited in the Paper by rubbing: Therefore, perhaps, the Papers wanted to be drier or warmer, ſo that I ſhall try them again. Theſe are the only two Experiments that do not agree with the ſecond *Query*; but I would not omit mentioning them, becauſe it is the Part of an impartial Philoſopher to mention as well thoſe Things which favour, as thoſe that diſagree with his Hypotheſes and Conjectures.

Experiments made before the Royal Society, *Feb.* 2, 1737-8, *by the ſame. Ibid.* p. 193.

10. *N. B.* In the following Account, which is the Sequel of former Experiments, I call *Conductors* thoſe Strings, to one End of which the rubbed Tube is applied; and *Supporters* ſuch horizontal Bodies as the *Conductor* reſts upon.

Experiment 1. Old Packthread Supporters tranſmitted Electricity but weakly, though more ſtrongly when twiſted with Cat-gut; but new Packthread did better.

N. B. *Where it is not mentioned otherwiſe, an Ivory Ball hangs at the End of the* Conductor; *and it's Electricity is tried by a Thread applied near it.*

Experiment 2. A Conducting String of Cat-gut received the Electricity a little Way; but did not carry it quite to the Tube.

Two

Two conducting Strings, one of Cat-gut, and one of Packthread, compared, the first attracted less and less, as the Distance from the Tube increased; and the other more and more, till it was strongest at the suspended Body: But both ceased immediately after the Removal of the Tube. Exper. 3.

A Sealing-Wax Supporter transmitted the Electricity, but did receive little or none when suspended. If it was but just rubbed with the Hand, it attracted the Thread when first suspended; and strongly, if much rubbed; but that Virtue was soon lost, if the Tube was applyed to the conducting String, and then it would receive no more Electricity from the Tube. If the Stick of Wax was wet, then it would strongly receive the Electricity. Exper. 4.

A Wax Supporter wet, and Silk String wet, did not transmit the Electricity.

Dried Ox-Guts did not transmit Electricity when held in Hand; but when tied to Cat-gut, transmitted it; and, when suspended, received it plentifully. Exper. 5.

The same with a small Cord. Exper. 6.

The same with a Rod of Iron, and Tube of Brass. Exper. 7.

A Glass Tube, made Conductor, received the Electricity but a little way. Exper. 8.

Dry Sheep-Skin transmitted the Electricity, but not when wet, though it received it then when suspended. Exper. 9.

A middle Supporter of Packthread was again supported on one Side by a Glass Tube, and on the other by Sealing-Wax, and had at each End an Ivory Ball hanging. Those Balls became electrical in the same manner, and at the same time, as the Ball at the End of the conducting String. Exper. 10.

When a Bar of Oak was made use of instead of the Tube, or a small Iron Bar instead of the Wax, the Electricity was stopped: But if the Bar was thrust a little way into a Glass Tube, the Electricity was communicated as before. Exper. 11.

11. I fixed 6 Iron *Radii* of twisted Iron Wire to a Brass Ring of two Foot and an half Diameter, and half an Inch wide, which had a Socket in the Centre, whereby to set it either on an upright Glass Tube, or on a wooden Pillar: Then I hung upon the End of the six *Radii*, next to the Circumference, the following Substances. 1. A Piece of Resin. 2. A Stick of Wax. 3. An Apple. 4. An Ivory Ball. 5. A Steel Ball. 6. A Glass Ball. *Experiments made at the* Royal Society, Feb. 9. 173⅞. *by the same.* *Ibid.* p. 196.

I rubbed the Tube, and applied it to the Centre of this Machine, as it stood on a Glass Tube; and the Electricity was communicated to all the suspended Bodies, and the Ring also; but none of them received it, when the Machine stood upon a wooden Pillar, whose Foot was on the Floor. Exper. 1. and 2.

I tied to the Ends of the 6 *Radii* as many Cat-gut Strings, but so long as to unite together about a Foot higher than the Centre of the Ring, Exper. 3.

 where

where I suspended them by another Cat-gut String 3 Foot long, the Top of which was fastened to an hempen Rope. Then applying the rubbed Tube very near the Place where all the Cat-gut Strings joined over the Ring, (at which Ring the same Bodies were suspended as before) neither the Bodies nor Ring received any Electricity.

N. B. This was done in foul Weather, when the Electricity does not extend itself far from the Tube: But in fair Weather, the Electrical Virtue, at the same Distance, reached the Iron *Radii* of the Ring; and consequently the Ring and Bodies suspended, though the Virtue was not propagated along the Cat-gut: For if the Tube was applied a little higher to the single Cat-gut, so as the *Effluvia*, or Virtue darted directly from the Tube, did not reach the Ring, or it's Iron *Radii*, then no Virtue was communicated to the Ring, or the suspended Bodies, *&c.*

Exper. 4. I suspended the Ring by six Packthreads, just in the same manner as the Cat-gut Strings before; but still all those Strings were suspended by the perpendicular Cat-gut of three Foot in Length. Then all the Bodies received the Electricity from the rubbed Tube applied to the Top of the Pyramid of Packthreads.

Exper. 5. Instead of the perpendicular Cat-gut between the Pyramid of Packthread and the upper hempen String, I substituted a Packthread; and then no Virtue was communicated to the Ring, but all went up the hempen String, and was lost; except the Tube was held very near the Ring, and then it gave a small Degree of electrical Attraction to the Ring, and the Bodies suspended at it.

Exper. 6. Having again suspended the Ring with the Bodies and Pyramid of Packthreads to the perpendicular Cat-gut, I tied a Packthread to the Ring, and carried it horizontally about 20 Feet from the Ring; and having fastened a Cat-gut String three Foot long to it, I gave it an Assistant to hold: Then applying the rubbed Tube to the End joining that Cat-gut, the Electricity was communicated to the Ring, and all the suspended Bodies, as appeared by applying the white Thread near them, which was attracted by every Part of the Ring, and all the Bodies.

Experiments made before the Royal Society, Feb. 16. 173$\frac{2}{3}$. *by the same.* 12. I applied the rubbed Tube to a burning Candle, and it had no manner of Effect on the Flame; but as soon as the Candle was blown out, it attracted the Smoak at four or five Inches Distance.

Ibid. p. 198. Exper. 1. Exper. 2. An horizontal Packthread, of about 18 Feet in Length, being terminated by the Cat-gut Strings, of three Foot long each, I hung (towards one of the Ends of the Packthread) upon it a Candlestick with a lighted Candle in it; then applying the rubbed Tube to the other End of the Packthread, the Candlestick attracted the Thread, and it was also attracted by the Candle, but not within 2 or 3 Inches of the Flame; but as soon as the Candle was blown out, the Thread was attracted by every Part of it; nay, even the Wick, when it was quite extinguished.

I suf-

I ſuſpended a Wax Candle in the ſame manner, and the Experiment ſucceeded in the ſame manner; only the Electricity came not ſo near the Flame in the Wax as in the Tallow Candle. Exper. 3.

I hung an Iron Wire 16 Foot long horizontally by two Cat-gut Strings at it's Ends about 3 Foot long each, and bent down the Wire from the Place joined to the Cat-gut, ſo as to hang down a Foot at one End; then applying the rubbed Tube at the other End, this Conductor carried the Electricity along to the Ball; but not ſo well as the Packthread Conductor; but it did ſomething better when it was wet. Exper. 4.

The ſame happened when the Conductor was Braſs Wire of the ſame Length.

N. B. The Packthread Conductor alſo carried the *Effluvia* ſtronger when it was wet.

13. I took the Glaſs Tube A B of 2 Inches Diameter, which had at one End A, a Braſs Ferril with a Brim cemented to it, and at the other End B, a Braſs Cap cloſe at Top, the Braſs-work being joined to it, in order to exhauſt it of it's Air upon Occaſion. When this Tube was very dry, it would become electrical by rubbing, ſo as to ſnap by paſſing the Ends of the Fingers near it; but that Virtue could not be excited in the Tube nearer the Braſs at the Ends than from *a* to *b*, and not unleſs the Tube was very dry within. *An Account of ſome Electrical Experiments made before the Royal Society, Feb. 16. 173⅞. by the ſame. Ibid.* p. 200. Exper. 1.

The Tube being thus prepared, and having an Ivory Ball C, of about two Inches Diameter, tied to it at the End B by a ſhort String, I paſſed the Tube through the horizontally ſuſpended Plate D D, till it was ſtopped by the Brim at A; and as it hung perpendicularly, the Ball C was within a Foot and an half of the Floor. The Plate D D was about 10 Inches in Diameter, and ſuſpended by three ſmall Cat-gut Strings as E, *e*, of about two Feet in Length, all which were tied together at E, to an hempen String hanging from the Cieling at F. Fig. 2.

By reaſon of the Diſtance of the Ends of the Cat-gut Strings cloſe to the Plate at *e e e*, I was able to thruſt in between them one End of an open Tube G G, after I had rubbed it ſo as to make it electrical, to ſee whether I could make the aforeſaid ſuſpended Tube A B the Conductor of Electricity to the Ball C; but the firſt Trial was in vain.

Then laying horizontally over the Plate D D an Iron Bar a quarter of an Inch thick, and a Yard long, I hung at the Ends of it two Ivory Balls *c c*, of the ſame Size as C, by Packthreads of the ſame Length as the Tube A B. Exper. 2.

Having again made the Tube G G electrical, I applied it over A, as before, and immediately the two Balls *c c* received the Electricity, ſo as to attract the *Thread of Trial* T hanging at the End of the Stick S T, when applied near them; though it received no Motion when applied to C. But if the Strings H *c*, inſtead of Packthread, were Cat-gut, then

then the Balls *c c* received no Electricity from the Tube G G rubbed and applied over A.

N. B. To be ſure that the rubbed Tube is made electrical, I paſs my Fingers near it after rubbing, to hear whether it ſnaps; but always rub again before I apply it; becauſe by ſnapping it loſes it's Electricity at the Place where it ſnaps.

Exper. 3. When I rubbed the Tube A B, it would then attract the Thread of Trial T between *a* and *b*; but not at all above *a* or below *b*, unleſs when I applied the Tube G G above A: Then the Thread of Trial would be attracted by the Plate D D, and the Top of the great Tube from A to *a*, but no lower. It would alſo be attracted by all the Bar H H, and only three or four Inches below H.

Exper. 4. Having filled the Tube A B with Water, the Electricity of the rubbed Tube G G, applied at A, ran ſtrongly down the Tube A B, and impregnated the Ball C, ſo as to make it ſtrongly attract the Thread of Trial, whilſt the Balls *c c* received no Virtue at all. But upon wetting the Cat-gut Strings H *c* with a Spunge, all the three Balls *c* C and *c* ſtrongly received the electrical Virtue.

Exper. 5. I took away the Bar H H, and it's Balls and Strings; and having well dried the Tube, I rubbed it, and hung it up as before; ſo that it would ſnap, or attract the Thread from *a* to *b*, but no-where elſe.

Then putting the ſmall Bar H H into the Middle of the Tube A B in it's Axis repreſented by the pricked Line, upon Application of the rubbed Tube G G at A, the Virtue was immediately communicated to the Ball C. The ſame thing happened, when, inſtead of the Bar, a Braſs Wire, a Walking-Cane, a ſmall green Stick, or ſmall Packthread was placed in the Axis of the Tube.

Exper. 6. I took a Barometer Tube empty, and very dry, and placed it in the Axis of the great Tube A B; but it would conduct no Electricity to the Ball C; though it carried it down very readily when full of Water, though quite dry on the Outſide.

Another ſmall Tube open at both Ends, which conducted no Virtue to C when dry, being only moiſtened a little by the Breath in blowing through it, carried down the Virtue from A to C very ſtrongly.

N. B. All this while the Cat-gut Strings E *e* received no electrical Virtue.

As I deſign to purſue theſe Inquiries much further, I beg Leave to be allowed to make uſe of ſome Terms, (which I ſhall here define) in order to ſave uſing many Words in giving an Account of ſome electrical Experiments, which I have made, and ſhall hereafter make.

Definition I. A Body *electrical per ſe* is ſuch a Body in which one may excite Electricity by Rubbing, Patting, Hammering, Melting, Warming or or any other Action on the Body itself, as Amber, Sealing-Wax, Glass, Resin, Sulphur, *&c.* besides many, if not all, animal Substances.

Definition II. A *Non-electrical* is such a Body as cannot be made electrical by any Action upon the Body itself immediately; though it is capable of receiving that Virtue from an *Electrical per se.*

Observations. 1. When the Air is full of moist Vapours, *Electricals per se* are excited to Electricity with very great Difficulty, requiring to be often warmed, and much rubbed; as appears in exciting that Virtue in Glass, Amber, Wax, *&c.*

2. In dry Weather, especially in frosty Weather, the *Electricals per se* will have their Virtue excited with very little Action upon them; as appears by warming a Glass Receiver, which, without any rubbing, will cause the Threads of a Down Feather, tied to an upright Skewer, to extend themselves as soon as it is put over the Feather. Sometimes Resin and Wax exert their Electricity by only being exposed to the open Air.

3. *Electricals per se* retain the Virtue longest when kept near to, or inclosed by, other *Electricals per se.* Thus the rubbed Tube will retain it's Virtue pretty long in dry Air, as appears by chasing a Feather about the Room very long without new rubbing; as also by Lumps of Resin and Sulphur, *&c.* which have been melted and poured into dry Drinking-glasses, keeping their Virtue long, if kept in those Glasses, and wrapped in dry Silk, or such Sort of Paper as will become electrical by rubbing; for as often as they are exposed to the Air, they will attract.

4. *Electricals per se* communicate their Virtue to any of the *Non-electrical*, when brought near them; in which Case the *Non-electricals* attract and repel like the *Electricals per se.* Thus an Iron Bar suspended by a silken Thread, an Hair Rope, or a dry Cat-gut, when an excited *Electric per se* is brought near it, will both attack and send out it's *Effluvia* to a *Non-electric* held near it; as appears in the Dark by the Light coming out at the End of the Bar.

5. An *Electrical per se* loses it's excited Virtue in communicating to the *Non-electrical*; and the sooner, the more of those Bodies are near it. Thus in moist Weather the rubbed Tube holds it's Virtue but a little while, because it acts upon the moist Vapours that float in the Air; and if the rubbed Tube be applied to Leaf-Gold or Brass, laid upon a Stand, it will act upon it much longer, and more strongly, than if the same Quantity of Leaf-Gold is laid upon a Table, which has more *Non-electrical* Surface than the Stand.

6. When a *Non-electrical* is suspended by, or only touches an *Electrical per se*, it receives the Properties of an *Electrical per se* from a rubbed Tube or Wax, *&c.* This appears by the Fire that flashes from the Fingers of a Man suspended by Hair-Ropes, or who stands upon a Cake of Resin, when he has received Virtue from the rubbed Tube.

7. The Virtue which a *Non-electrical* receives from a rubbed Tube, runs on to the moſt diſtant Part of the ſuſpended Body from the Place where the Tube is applied, and ſeems to be collected there, from whence it flaſhes in the Dark, ſnaps and exerts it's Attraction upon the Thread of Trial; though as the Virtue runs along, it ſometimes ſhews itſelf in other Parts of the ſuſpended *Non-electrical.*

8. If a *Non-electrical*, whilſt it is receiving the Virtue from the rubbed Tube, be made to communicate with the Floor of the Room, or any other great *Non-electrical* Body by a *Non-electrical* String, how ſmall ſoever, (though but a Thread) the Virtue will not ſhew itſelf, as it did before, at the Extremities, where the Flaſh of Light was ſeen.

9. If a *Non-electrical* be ever ſo big, when ſuſpended, it will receive Electricity from the rubbed Tube. And if five or ſix hundred Foot long, when the rubbed Tube is applied at one End, the Bodies hanging at the other End will become electrical. This has been tried by ſeveral People as well as myſelf.

10. If a long *Non-electrical* String be faſtened to an *Electrical per ſe*, and extended to a great Diſtance, being ſupported by *Electricals per ſe* to keep it from touching the Ground, all Bodies faſtened at the End of it will become electrical when the rubbed Tube is applied at the other End, though the Tube does not touch it, but is only brought within two or three Inches of it.

N. B. This String we have before called the *Conductor of Electricity*, and the Cat-gut or ſilken Strings, Glaſs Tubes, or whatever kept the long String from touching the Ground, *Supporters.*

11. If any of the *Supporters*, mentioned in the laſt Obſervation, be changed for a *Non-electrical* Supporter, the Virtue will there be ſtopped and taken away by that *Supporter*: But if that *Supporter* be again ſupported by *Electricals per ſe*, it will only receive ſo much Electricity as will impregnate it, and then the Virtue will go on to the End of the String, and impregnate the Bodies faſtened to it.

12. The *Non-electricals* receive the greateſt Virtue at the End of the String, and moſt of all, if they are wet. But the *Electricals per ſe*, if long Bodies, as long Sticks of Wax, and Glaſs Tubes, only become electrical at the End next to the String.

13. *Electricals per ſe* will become *Non-electricals* if they be wet, or only moiſtened. Thus Supporters that tranſmit the Electricity immediately, ſtop it when wet with a Spunge, or when blown through, if open Tubes. And if the long *Electricals per ſe*, hanging at the End of the Conductor, be made wet, they will become *Non-electricals*, and ſtrongly receptive of the Virtue given by the rubbed Tube at the other End of the String.

N. B. All the ſix Experiments mentioned in the Beginning of this Paper, confirm this Obſervation.

14. A

14. A *Non-electrical* having been impregnated with Electricity by the rubbed Tube, is repelled by it, till it has lost it's Electricity by communicating it to another *Non-electrical*. Then being in it's first State, it is again attracted by the Tube, which holds it till it has fully impregnated it; then it repels it again. This is evident, by attracting a Down Feather by the Tube in the Air, and then repelling it, so as to make it dance backwards and forwards to and from a Finger held up at a Foot or two from the Tube. But the Thing appears more plainly from the following.

Having rubbed the Tube T *t*, and with it attracted a Feather, the Feather at *t* was repelled from the Tube, whenever it was brought near it; but suddenly dipping the End T of the Tube in Water, the Feather floating in the Air came to it again, and stuck to the End of the Tube at T, or near F. Exper. 7. Fig. 3.

N. B. In fair Weather this Experiment will not succeed, unless the Tube be thrust pretty deep into Water (a Foot at least); but in moist Weather an Inch or two will do.

P. S. Though animal Substances be generally thought to be *Electrical per se*, yet it is only when they are very dry: This is the Reason why a living Man suspended by a Hair Rope, or standing upon a Cake of Resin, to receive Electricity from the Tube, must be considered as a *Non-electrical*, by reason of the Fluids of his Body.

An Account of some electrical Experiments made at his Royal Highness the Prince of Wales's *House at* Cliefden, April 15, 1738, *where the Electricity was conveyed 420 Feet in a direct Line. By the same. Ibid.* p. 209.

14. Having heard that Electricity had been carried along an hempen String 5 or 600 Foot long, but having only seen it when the String was carried backwards and forwards in a Room by Silk *Supporters*, I was willing to try it with a Packthread String stretched out at full Length; for which Purpose having joined a Cat-gut String of 6 Foot long, I fastened it to the Inside of a Door in the Suit of Rooms at *Cliefden*; and having also tied another Cat-gut, like the first, to the other End of the String, I tied it up to the Inside of the Door at the other End of the House; but at the Place where the Packthread was joined to the Cat-gut, I left a Foot and an half of Packthread hanging down, and fastened to it a *Lignum Vitæ* Handle of a Burning-Glass; then applying a rubbed Tube at the other End of the String, I made the Electricity run to the *Lignum Vitæ*, but with some Difficulty, which I attributed to the Size, being an animal Substance that still stuck to the Packthread as it was new; therefore I caused the Packthread to be wet with a Spunge from one End to the other, to wash off the Size: Then was the Electricity from the Tube communicated very soon, and very strongly; for the Thread of Trial (mentioned in my former Papers) was drawn by the *Lignum Vitæ* at the Distance of a Foot.

Afterwards, having joined more Packthread together, I made a String of 420 Foot long, one End of which I fastened (by the Interposition of Cat-gut as before) to the Iron Gates in the Garden, before the House, and the End which had the *Lignum Vitæ* Handle, to the

upper

upper Part of the Door next to the Back-side of the House in a large Drawing Room, taking Care that the String came through the Middle of the opened Doors through which it passed; and to prevent this String dragging upon the Ground, three Pieces of Cat-gut held across by two Men, at equal Distances from the Ends, and from each other, supported it. The String was altogether dipped in a Pail of Water, before the Experiment; but great Care taken, that the Cat-gut should not be wet.

Then I applied the rubbed Tube at the End in the Garden, whilst my Assistant held the Thread of Trial near the Handle above-mentioned, which Thread was strongly attracted, though the Wind was very high, and blowed in the contrary Direction to that in which the Electricity ran along.

I first tried the Experiment with the Packthread dry, but then it would not do at that Distance.

N. B. The Weather was moist when I made the Experiment.

Some Things concerning Electricity. By the same, No. 459. p. 634. Jan. &c. 1741.

15. About a Year or two ago, in a Paper I gave in to the *Royal Society*, I endeavoured to establish some general Principles concerning Electricity, from the Consideration of many Experiments, which have been tried by others, as well as some new Experiments by myself, an Account of which I then gave. Therefore I shall only now repeat my Distinction of all Bodies into two Classes, in respect of Electricity, and make good the Definitions that I gave by some further Experiments; and though I do not pretend to know the Cause of Electricity in general, yet I hope from a few Laws of Electricity, deduced from known *Phænomena*, to solve most other *Phænomena*, (though seeming quite unaccountable) so far as to shew what Law of Electricity they depend upon; and to be able to foretel what will happen to most Bodies before the Experiments are tried upon them in an electrical Way.

1. Bodies electric *per se* are such in whom a Virtue of attracting and repelling small Bodies at a Distance is inherent, though it is not always in Action, so as to produce that Effect. But by rubbing, patting with the Hand, hammering, warming, and sometimes only exposing to dry Air, such Bodies exert the Virtue above-mentioned; otherwise they are in a Non-electric State.

2. Non-electric Bodies are such in which no electrical Virtue can be excited by any Action upon the Bodies themselves, such as rubbing, warming, &c. But an Electric *per se*, when excited, can communicate it's Virtue to a Non-electric, and that Virtue will be received by all the Parts of the Non-electric, (be the Body ever so long or large) and be strongest, being, as it were, collected at that End of the Non-electric, which is farthest from the Place where the Electricity is first received.

3. A Non-electric, having received Electricity, will communicate to nother Body brought to touch it, or only brought pretty near, and that often

often with a snapping Noise, and a small Flash of Light, losing by that Means all it's own Electricity.

4. An Electric *per se* will become a Non-electric for a time, if it be made wet or moist, and become receptive of Electricity, which it will receive at one End, and carry to the other, where the Electricity will go off with a small Explosion, to impregnate any other Non-electric, which is brought near.

5. An Electric *per se*, in which Electricity has been excited, may become Non-electric by being exposed to moist Air, whose humid Vapours it attracts; and then, brought to the Fire, or into very dry Air, recover it's Electricity when the Moisture is exhaled again.

6. An Electric *per se* may be made strongly electric in Part of it's Length, whilst the other Part remains in a Non-electric State.

7. A Body in a State of Electricity (whether a Non-electric having received Electricity, or an Electric *per se*, excited to Electricity) will attract all Non-electrics, and repel other Bodies that are in a State of Electricity, provided the Electricity be of the same kind.

8. A Non electric Body will not retain the Electricity which it receives from an Electric *per se*, unless it be free from touching any other Non-electric Body; but must be suspended or supported by Electrics *per se* touching only them and the Air.

9. An Electric *per se*, when it is not reduced to a Non-electric State, will not receive Electricity from another Electric *per se*, whose Electricity is excited, so as to run along it's whole Length; but will only receive it a little Way, being (as it were) saturated with it.

10. An Electric *per se* will not lose all it's Electricity at once, but only the Electricity of such Parts of the Body as have communicated it to other Bodies, or near which Non-electrics have been brought.

11. When a Non-electric, which has received Electricity, communicates it's Electricity to another, it loses all it's Electricity at once; and the *Effluvia*, in coming out, strike the new Body brought near, as well as the Body first made electric.

12. Excited Electricity exerts itself in a Sphere round the Electric *per se*; or rather a Cylinder if the Body be cylindric.

13. The Electricity which a Non-electric of great Length (for Example, a hempen String 800 or 900 Feet long) receives, runs from one End to the other in a Sphere of electrical *Effluvia*. But all the Supports of this String must be Electrics *per se*.

14. If this String be branched out into many Strings, the Electricity will run to all their Ends.

15. If the Non-electric String, which is to receive and carry on the electric *Effluvia*, be not continuous, but has between it's Ends some Electrics *per se*, the *Effluvia* will stop at the first of them, unless the Interruption or Discontinuation of the Non-electric be short; because in that Case the Electricity jumps from the End of the first Non-electric to the Beginning of the next, especially if the Air be very dry, even though the

the Ends of the String should be about a Foot distant, and no Body but the Air between. Sometimes indeed the Distance must not be above an Inch or two.

There are Two Sorts of Electrics *per se*, known by what follows: A Body impregnated with Electricity from one Sort will repel all Bodies that have that Sort of Electricity, till they have lost their own Electricity by coming to some Non-electric. But an Electric *per se* of the other Sort, though excited, will attract all those Bodies, though in a State of Repulsion, on account of the other Electricity; and so *vice versa*.

An Account of some Electrical Experiments made before the Royal Society, Jan 22 1740 1 *by the same. Ibid.* p. 637.

16. It being a Matter in Dispute, whether there is any Difference between the Electricity of Glass, and that of Gums and Resins, I made the following Experiments, in order to settle that Point:

I fastened a String of dry Cat-gut (which, when dry, is an Electric *per se*) from one Pillar to the other, at the End of the Table in the Meeting-Room of the *Royal Society*, about 7 Feet from the Floor; and to the Middle of that Cat-gut fastened a silken Thread about 2 Foot long, which hung down, and at it's lower End had a Down Feather. Then rubbing the End of a Stick of Wax pretty quick and strongly against my Waistcoat, which was made of Cloth, the Wax became electrical, and attracted the Feather, which stuck to it awhile, and then was repelled from it, as long as it retained the Electricity it had received from the Wax: But, having touched the Feather with my Finger, it lost it's Electricity; and, becoming a Non electric, was again attracted by the Wax, which gave it fresh Electricity; and then it was repelled from it, and so *toties quoties*. When the Feather was in it's electric State, I applied to it another Stick of Wax, which I first rubbed; and it repelled the Feather, though it had not touched it before, and did the same as the other Stick of Wax had done.

After that I rubbed a Glass Tube, which first attracted and then repelled the Feather, as the Wax had done: And another Tube being rubbed, repelled the Feather, when it was put into an electric State by the first Tube, without first attracting it. But Non-electrics, such as the Finger, or a Stick, attracted the Feather, when it had first been made electric; and not only so, but Electrics *per se*, when they were become Non-electric, as the Tube unrubbed, or the Wax unrubbed; nay, the rubbed Tube also, when it's End was moistened, or that End of it turned to the Feather, which had been held in the Hand.

Then I made the Feather electric, by the Application of the excited Tube; and, having rubbed the Wax to give it Electricity, I brought it near the Feather, which it attracted strongly, though it had repelled it before, when the Feather had been made electric by Wax.

Afterwards I made the Feather electric by the Wax, which first attracted and then repelled it: And, having applied the rubbed Tube to the Feather, it attracted it strongly, though it repelled it when the Feather was made electric by another Glass Tube.

Having

17. Having shewn lately, by some plain Experiments, that the Electricity of Glass is different from that of Sealing-Wax, because the Wax attracted a Feather suspended in the Air by a fine Silk, when the rubbed Glass Tube repelled it, (as described in the Account of those Experiments) I made the Experiment with a Cake of Rosin instead of Sealing-Wax; and it appeared to have the same Kind of Electricity as the Sealing-Wax. Then considering that the Supporters of any Non-electric Conductors of Electricity must themselves be electric, I had a Mind to try whether Bodies, endued with either Kind of Electricity, were in any-wise different in that Case; which I did by the following Experiments:

Electrical Experiments made before the Royal Society, March 15, 1740-1. *By the same. Ibid.* p. 639.

I laid a Piece of Wood, 4 Foot long, on 2 Glass Plates, whose Ends stood one Foot beyond the Side of the Table on which they were laid: Then, applying the rubbed Tube to one End of the Wood, the other attracted Leaf-brass, or a Thread hanging down from a Stick. Then, instead of the Glass Plates, I laid the long Piece of Wood on two Cakes of Rosin, and applied the rubbed Tube to the End of the said Wood, which conducted the Electricity to the other End, where Leaf-brass and the Thread were attracted in the same Manner.

This shews that, in order to conduct Electricity along any Non-electric Body, it is indifferent what Kind of Electricity it's Supporters are endowed with, provided they are electric.

18. I mentioned in one of my former Papers concerning Electricity, that Electrics *per se* would not receive the Electricity of a rubbed Tube, so as to carry on to a Distance; but that, if those Bodies were changed into Non-electrics, they would then receive and convey the Electricity of the rubbed Tube in the same Manner as all other Conductors of Electricity do. The Experiments which I made to prove this plainly, are as follow:

An Account of some Experiments made before the Royal Society, May 14, 1741. *By the same.* No. 460, p. 661.

Exper. 1. I suspended a long small Glass Tube at about the Distance of 14 or 15 Inches from a horizontal extended Cat-gut in the same Position, or parallel to it, by two small silken Threads; and, with a small Packthread, hung an Ivory Ball on the End of the suspended Tube; and, applying the rubbed Tube to the other End, (though lightly excited, as appeared by it's snapping) no Electricity was communicated to the Ball: Though, when a very small Packthread was hung from one End of the Tube to the other, the Ivory Ball became very electric, as appeared by it's attracting a Thread hung on a Stick. Then taking off the Packthread, and wetting the Tube from one End to the other with a Spunge, it became a Non-electric, and conveyed the Electricity to the Ball as strongly as the Packthread had done.

Exper. 2. The Tube being well dried, I applied a Silk in the same Manner as I had done the Packthread; but no Electricity could be conveyed to the Ball by applying the rubbed Tube to the contrary End of the Silk: But afterwards, having wetted the Silk, it became a Non-electric, and received the Electricity, which it communicated to the Ball.

N. B. I chose a Glass Tube for this Experiment, because Mr *Du Fay* had made use of Glass Tubes for the Supporters of his Conductors of Electricity; and Silk, because Mr *Gray* had supported his Conductors of Electricity upon Silk.

An Account of some Experiments made before the Royal Society, May 28, 1741. *By the same. Ibid.* p 662. Exper. 1. Fig. 4.

19. That it is not the Quantity of Matter in Bodies, that makes them more or less receptive of Electricity, and conductive of it, but entirely their Quality, appears by the following Experiments.

From a Cat-gut String A B, about 12 Foot long, stretched horizontally 6 Foot above the Floor of the Room, I suspended two Iron Bars C D, E F, of about 40 Inches long, and a Quarter of an Inch square, by the silken Strings C *c*, D *d*, E *e*, F *f*, which at first touched at their Ends D, E; and from the End F of the Bar E F there hung, by a Packthread, the Ivory Ball G. Then having rubbed a large Tube I K to excite it's Electricity, I applied it near the End C of the first Bar; and the electrical Virtue ran along the two Bars, and impregnated the Ball G, as appeared by it's attracting the Thread hanging from the Stick H, at about 3 Inches Distance. Afterwards I separated the Tubes in the Manner that they appear in the Figure, and the Electricity was communicated to the Ball but faintly when the Bars were 1 ½ Inch asunder, and not at all when they were 2 ½ Inches asunder. But this was owing to the moist State of the Air; for, when the Air is very dry, the Virtue will jump 6 or 9 Inches; but when the moist Particles, that float in the Air, are attracted by the Bars, the Virtue will reach but a little Way; though, if that Moisture be fixed upon any Body, which (being an Electric *per se*) would not conduct the Electricity applied to it's End, the Virtue will be carried from one Bar to another as well as if they had touched, as will farther appear by the next Experiment.

Exper. 2. Fig. 5.

Having separated the hanging Bars so far asunder, that the Electricity could not jump from the one to the other, (*viz.* about 3 Foot) I laid upon their End the small Tube D E, having wiped it very dry: Then applying the rubbed Tube to C, the Virtue stopped at D; and neither the Bar E F, nor the Ball G, received any Electricity, the Thread H being attracted by neither of them. But, having blown through the Tube, the Moisture of the Breath changes it from an Electric to a Non-electric; and then the Virtue of the rubbed Tube runs along freely from C quite to the Ball at G, which then strongly attracts the suspended Thread.

N. B. When the Air is very moist, the Tube D E is turned to a Non-electric without blowing, only by the watery Particles adhering to it.

Exper. 3. Fig. 6.

The Bars remaining in their Situation, I took off the Tube, and stretched a very fine white flaxen Thread from D to E, which touched both

both Bars: Then applying the rubbed Tube to C, the Virtue was carried from Bar to Bar, and the Ball G attracted the Thread H at 2 Inches Diſtance. Afterwards wetting the Thread, the Attraction became much ſtronger at G, ſo as to attract the Thread H at 4 or 5 Inches Diſtance.

Having joined together the Ends of the ſuſpended Bars, I ſuffered the Thread D *d* to hang down, but touching no other Body; then the Electricity was freely communicated (by applying the rubbed Tube to C) from C to the Ball at G. But if *d*, the lower End of the Thread, touched the Ground, or a Chair, or was taken hold of by any body's Hand, or lifted up by a walking Cane; then the Electricity advanced no farther on the Bars than D, but was interrupted by the Thread D *d*, and carried to the Bodies contiguous to *d*, when they are Non-electric. If the End *d* of the Thread was laid upon a dry Glaſs Tube, held in the Hand of an Aſſiſtant, then the Electricity ran as before along the Bars to the Ball G. The ſame Thing alſo happened when the End *d* of the Thread was thrown up upon the Cat-gut String; for in that Caſe the Electricity, having impregnated the String, did afterwards run along the Bars, *&c.* Exper. 4. Fig. 7.

20. Having found by ſeveral of Mr *Gray*'s Experiments, as well as ſome of my own, that Water is receptive of Electricity, ſo as to be raiſed up in a little Cup, to emit a Vapour towards the rubbed Tube, to ſnap, and to give Light; having alſo found, (as I ſhewed the Experiment before the *Royal Society*) that when a dry Tube, ſuſpended horizontally, will not conduct the Electricity of the rubbed Tube applied to one of it's Ends; and yet, when blown into, will conduct it ſtrongly all it's Length, becauſe the Electricity runs along from one moiſt Particle to another, though thoſe Particles are not contiguous. I thought that Electricity might impregnate a whole Jet of Water, whether perpendicular, oblique, or horizontal: And ſuppoſed alſo, that if at any Time there be electrical *Effluvia* in or above a Cloud, that Virtue may be communicated by the falling Rain, to any thing that the Rain falls upon. How far my Conjecture is true, will appear by the following Experiment:

An Account of ſome new Electrical Experiments, performed before the Royal Society, Aug. 29, 1741. *By the ſame. Ibid.* p. 666.

Having properly ſuſpended (that is, ſuſpended by ſome electric Body, here Cat-gut) a Copper Fountain with the Spout downwards, I opened the Cock, and let the Water ſpout into a Veſſel underneath: Then, having excited a great Tube to Electricity, I held it over the Copper Fountain, whilſt an Aſſiſtant held the Thread of Trial (that is, a Thread hanging from a Stick) near ſeveral Parts of the Jet, which attracted it ſenſibly: Then I applied the rubbed Tube near to the falling Jet, which attracted it ſtrongly, ſo as to bend it into a Curve, and ſometimes cauſe it to fall out of the Veſſel below.

21. Electrics *per ſe* (which I have heretofore defined, *Bodies in which an electrical Virtue may be raiſed by ſome Action on them*, ſuch as Rubbing, Patting, Warming, *&c.*) are reduced to a Non-electric State by being

Some further Obſervations concerning Electricity;

by the same. No. 462. p. 14. *Read* Jan. 14, 174½.

being in Contact with Non-electric Bodies, especially Water, which is the greatest Non-electric, even when it becomes Vapour.

A Non-electric (which though it cannot be made electrical by any Action upon it) receives Electricity from an excited electrical Body; but does not retain it whilst it touches any other Non-electrical Body. An Electric *per se*, when it is become Non-electrical, differs from the Non-electric *per se* in this; that it may be so restored to Electricity, by applying a rubbed Tube to it, as to repel all other Electrics of the same Kind of Electricity as the Tube; till it meets with some Non-electric Body, which brings it back to Non-electricity, or at least to such a languid State, that it's Electricity is scarce perceptible.

The Electricity may be also restored in the same Manner by Wax, &c. But in both Cases, an electric Body, in a languid State, cannot be restored to Electricity whilst it adheres to a Non-electric *per se*.

Experiments to illustrate these Assertions.

From an horizontal Cat-gut (which is an Electric *per se*, as most animal Substances are) I suspended 2 Feathers, the one by a Thread, and the other by a Silk, about 2 Foot long each: Then applying the rubbed Tube to the Feather hanging by the Silk, (which Silk is an Electric *per se*) the Feather came to the Tube, and stuck to it, as all Non-electric Bodies do, till it was so impregnated with the Virtue from the Tube, as to come out of it's languid State, and become strongly electrical; which appeared by it's flying from the Tube, and being repelled as often as the Tube was brought near it; till it had touched some Non-electric Body, or was left so long as to imbibe the moist Particles floating in the Air, by which it became Non-electric, and was again attracted by the Tube.

When I applied the Tube to the other Feather that hung by the Thread, (which, like most vegetable Substances, is generally Non-electric *per se*) the Feather was constantly attracted, and never repelled; because the Virtue communicated from the Tube to the Feather, lost itself along the Thread; which would have been retained by the Feather, if it had floated in dry Air, or been suspended by an electrical Body.

These Properties of electric Bodies shew the Reason of that *Phænomenon*, whereby a rubbed Tube, after having attracted a Feather, repels and chases it about a Room in the Air, and does not attract it a second Time, till the Feather has touched some other Body; and also shews the Reason why the Experiment does not succeed in moist Weather.

Pure Air, that is dry, may be ranked among the Electrics *per se*, because it repels all Bodies in a State of Electricity, whether they have been excited to it by Wax or Glass; that is, by either of the two Sorts of Electricity.

Watery Vapours, that float in the Air, are Non-electric; from which Mixture the Air becomes more languid in it's Electricity, when most impregnated with Vapours; so that dry Air is more electric than moist;

moiſt; but cold Air in froſty Weather, when Vapours riſe leaſt of all, is more electric than Air in Summer, when the Heat raiſes Vapours; which renders that State of the Air more fit for making electrical Experiments.

The rubbed Tube retains it's Electricity a long Time, becauſe it repels, and is repelled by the dry Air; and the Feather, which has been attracted by the Tube, after adhering to it a-while, is raiſed out of it's languid State to a ſtrong Electricity; whereby it flies from the Tube, repels and is repelled by the Air, where meeting with very few Vapours, it retains it's Electricity a long Time; till touching a Non-electric, that is brought to it, it loſes it's own Electricity by communicating it, becomes a Non-electric, and is re-attracted by the Tube, to which adhering ſome Time, it receives ſo much Virtue from the Tube, as to be reſtored to it's Electricity, and again repelled.

In a moiſt State of the Air, the Feather, after it has been made electrical, and repelled by the Tube, it attracts to it the moiſt Vapours floating in the Air; whereby, loſing it's Electricity, it is attracted by the Tube, without touching any other Body firſt.

Sometimes when the Feather flies off from one Part of the Tube, it immediately returns to another Part, generally the Top of the Tube, becauſe the Top of the Tube has attracted the moiſt Vapours, and is become a Non-electric, and therefore attracts the Feather; which being become electric, flew off from the electric Part of the Tube.

That this is true, appears from an Experiment to be made in dry Weather.

At that Time, when every Part of the Tube repels the Feather ſtrongly, after having attracted it, if you wet 2 or 3 Inches of the upper End of the Tube, the Feather will come to that End.

Wetting the Silk by which the Feather hangs from the Cat-gut, the Feather will be always attracted, and not repelled.

When the Silk is dry, the Feather once made electrical, ſo as to be repelled by the Tube, retained that Virtue above two Hours in froſty Weather; but in moiſt Weather loſt it in half a Minute.

Some Conjectures concerning Electricity, and the Riſe of Vapours. By the ſame. No. 464. *Read* June 24, 1742.

22. It is proper firſt to mention, by way of Preliminary, that M. *Du Faye*'s Aſſertion of two Sorts of Electricity is found to be true by Obſervations and Experiments, *viz.* that Bodies endowed with the vitreous Electricity repel one another, and attract thoſe that have the reſinous Electricity; on the contrary, thoſe that are endowed with the reſinous Electricity, repel one another, but attract thoſe that have the vitreous Electricity.

I ſuppoſe Particles of pure Air to be electric Bodies, always in a State of Electricity, and that vitreous Electricity.

1*ſt*, Becauſe Particles of Air repel one another without touching, as has been deduced from Experiments and Obſervations.

2*dly*, Becauſe when the Air is dry, the Glaſs Tube rubbed (or only warmed) throws out it's *Effluvia*, which the Air drives back to the

Tube, from whence they dart out anew, and so move backwards and forwards with a vibratory Motion, which continues their Electricity.

3dly, Because the Feather made electric by the Tube, and darted from it, keeps it's Electricity a long Time in dry Air; whereas, when the Air is moist, the moist Particles, which are Non-electric, being attracted by the Feather, soon make it lose this Electricity, which also happens even to the Tube in a little Time.

From this Consideration it will be easy to account for a famous Experiment of the late Mr *Hawksbee*, which is this:

Having pumped out all the Air from a Glass Globe, he caused it to turn on it's Axis very swiftly, by Means of a Rope with a Wheel and Pully; then rubbing the Glass with his Hand during it's Motion, there appeared a great deal of Light of a purple Colour within the Globe, without any Light or Attraction observed on the Outside of the Glass, which is observed when the Air has not been pumped out. Then turning the Cock so as to re-admit the Air gently into the Globe during it's Motion, the Light was broken and interrupted, diminishing gradually, till at last it appeared only on the Outside of the Glass, where it was accompanied with Attraction. Does it not appear to be, that at first the external Air by it's Resistance drives back the electric *Effluvia*, which go then to the Inside of the Globe, where there is the least Resistance? For we observe, that as the Air comes in, it repels the electric *Effluvia*, that go inwards no longer, when all the Air is come in. If the Fact be so, as the Experiment shews, is not my Conjecture proved, *viz.* that the Air is electrical?

In Dr *Hales*'s *Vegetable Statics*, several of his Experiments shew, that Air is absorbed, and loses it's Elasticity by the Mixture of sulphureous Vapours, so that four Quarts of Air in a Glass Vessel will be reduced to three. Will not this *Phænomenon* be explained by the different Electricity of Sulphur and Air? The *Effluvia* of Sulphur, being electric, repel one another; and the Particles of Air, being also electric, do likewise repel each other. But the Air being electrical of a vitreous Electricity, and Sulphur of a resinous Electricity, the Particles of Air attract those of Sulphur, and the *Moleculæ* compounded of them, becoming Non-electric, lose their repulsive Force.

It has for a great while been thought, that watery Vapours, that rise in Air to form Clouds, used to rise, because the Water which is of itself specifically heavier than Air, (being formed into little hollow Spherules or Bubbles filled with an *Aura*, or thinner Air than the ambient Air) in this new State made a Fluid of little Shells, specifically lighter than the ambient Air in which it must rise. But Philosophers are come off of that Opinion; and such as have implicitly come into it, may find it refuted in the *Philosophical Transactions* *.

* Vol. VI. Part ii. Chap. I. § xix.

Now

Now may not this *Phænomenon* of the Riſe of Vapours depend upon Electricity in the following Manner?

The Air which flows at Top of the Surface of the Waters is electrical, and ſo much the more as the Weather is hotter. Now in the ſame Manner as ſmall Particles of Water jump towards the electric Tube, may not thoſe Particles jump towards the Particles of Air, which have much more ſpecific Gravity than very ſmall Particles of Water, and adhere to them? Then the Air in Motion having carried off the Particles of Water, and driving them away as ſoon as it has made them electrical, they repel one another, and alſo the Particles of Air. This is the Reaſon that a cubic Inch of Vapour is lighter than a cubic Inch of Air; which would not happen, if the Particles of Vapour were only carried off in the Interſtices of Air, becauſe then a cubic Inch of Air, loaded with Vapour, would be made ſpecifically heavier than an Inch of dry Air; which is contrary to Experiments, which ſhew us by the Barometer, that Air which is moiſt, or full of Vapours, is always lighter than dry Air.

Some Thoughts and Conjectures concerning the Cauſe of Elaſticity. By the ſame, No. 454. p. 175. July, &c. 1739.

V. Attraction and Repulſion ſeem to be ſettled by the great *Creator* as firſt Principles in Nature; that is, as the firſt of ſecond Cauſes; ſo that we are not ſollicitous about their Cauſes, and think it enough to deduce other Things from them. If Elaſticity was admitted as a firſt Cauſe, (as it is by ſome) it is thought we ſhould admit of too many principal Cauſes in Nature, which is contrary to the Rules of good Philoſophy. Philoſophers therefore have endeavoured to deduce Elaſticity from Attraction, or from Repulſion, or from both. It is obſerved, that the ſame Particles that repel each other ſtrongly, will attract other Particles very ſtrongly, as appears by many Chemical Diſſolutions, eſpecially by the alternate Diſſolution and Precipitation of Metals in acid *Menſtruums*. The reverend and learned Dr *Hales* has proved this many Ways, in his *Vegetable Statics* and *Hæmaſtatics*. The Elaſticity of Air ſeems to conſiſt wholly in the repulſive Power of it's Particles, which do not touch one another while the Air is in it's elaſtic State; and if thoſe Particles be brought nearer and nearer together, the Effect of their repulſive Force will increaſe, the Air's Elaſticity being always proportionable to it's Denſity by Compreſſion, which Property will be preſerved, though compreſſed Air be kept a Year or two; notwithſtanding Mr *Hawksbee*, in his *Phyſico-mechanical Experiments*, ſays, that Air will loſe Part of it's Spring by being very much compreſſed: But the Air with which he tried it, muſt have been filled with moiſt Vapours; and it is well known, that the Steam of Liquors will loſe it's Elaſticity, eſpecially where it's Heat decays. I have kept ſeveral Wind-Guns, ſtrongly charged, for half a Year together, in which the Air had loſt none of it's Elaſticity: Others have found the Air as ſtrong after a Year; and I have been told by a Perſon of Credit, that a Wind-Gun having been laid by and forgotten for ſeven Years, when it was found, diſcharged it's Air as many Times, and with as much Force, as it uſed

to

to do. Now, though Air, compreſſed by any external Force, does always increaſe in Elaſticity, as it diminiſhes in Bulk; yet it may, by Fermentation, diminiſh it's Bulk very much, without gaining any more Elaſticity: For if another Fluid, whoſe Parts repel one another, but attract the Parts of Air, be mixed with Air, the Repulſion of any two Particles of Air will be diminiſhed, in Proportion as a Particle of the other Fluid, inſinuating itſelf between them, attracts them towards itſelf on either Side. The ſame Thing will happen to the other Fluid, in reſpect of the Particles of Air, which mixing with it's Particles, do in the ſame Manner deſtroy their Repulſion. Thus, if we allow an Attraction ſtrong enough between the Parts of two elaſtic Fluids, it is poſſible, that by Fermentation a Solid may be made out of two elaſtic Fluids, which would have ſtill continued fluid without ſuch a Mixture. We are taught by Chemiſtry to mix Fluids together, which immediately coaleſce into a Solid. When Brimſtone Matches are burning, the *Effluvia* of the Sulphur repel each other to great Diſtances, as may be known by the ſulphureous Smell upon ſuch an Occaſion. Now, though theſe Particles repel each other, they attract the Air very ſtrongly, as appears by the following Experiment:

Take a tall Glaſs Receiver cloſed at Top, holding about 4 Quarts of Air, and having put it's open End over a Bundle of Brimſtone Matches on Fire, ſtanding up in the Middle of a large Diſh with Water in it, (to keep the Air from coming in at the Bottom of the ſaid Receiver) you will obſerve, that not only as ſoon as the Matches are burnt out, but a good while before, the Air, inſtead of being expanded by the Flame of the Brimſtone, will retire into leſs Compaſs, the Water beginning to riſe from the Diſh up into the Receiver, and continuing ſo to do till ſome Time after the Matches are burnt out; ſo that there will be in the Receiver only 3 Quarts of Air, inſtead of 4 (more or leſs, in Proportion to the Quantity of Brimſtone burnt): And this plainly happens by ſome of the *Effluvia*, or little Parts of the Sulphur, attracting ſome of the Particles of Air, ſo as to make an unelaſtic Compound, that precipitates into the Water. If the Elaſticity of the Air is quite loſt when the Repulſion of it's Particles is taken off, or ſufficiently counteracted, it muſt follow, that it's Elaſticity depends upon Repulſion: And that this is often the Caſe, appears by a great Number of Dr *Hales*'s Experiments, of which I will mention but a few. The Doctor took a cubic Inch of Mutton-Bone, and having put it into his Gun-Barrel Retort, he diſtilled out of it 2 or 300 cubic Inches of Air into a large Glaſs Bottle, the Weight of which Air, together with the Aſhes of the Bone left, weighed as much as the whole Quantity of Bone did at firſt. Now the Air had been confined in that Bone, together with many ſulphureous Particles, in ſuch Manner, that the mutual Attraction of the Sulphur and the Air had alternately deſtroyed each other's repulſive Force, and brought thoſe Subſtances into a little Compaſs; but the Fire in the Diſtillation ſeparated them from each other,

ſo

ſo as to reſtore them, in a great Meaſure, to their uſual Elaſticity. This appeared by bringing a Candle near the Mouth of the Bottle that held this revived Air; for every time the Candle was brought near, the Air took Fire, and flaſhed out of the Bottle with a ſulphureous Smell. The Air may be conſolidated in many hard Bodies, ſo as to be there quite void of Elaſticity, and there do the Office of a Cement, till by the Action of Fire, or ſome particular Fermentations, it is again reſtored to it's perfectly elaſtic State. This is the Meaning of the Doctor's Words, when he ſays, that ſome Bodies *abſorb*, and others *generate* Air; and the ſame Bodies do ſometimes abſorb, and at other times generate Air. He found more or leſs Air in almoſt every ſolid Subſtance that he tryed; but, what was moſt remarkable, he found that the *Calculus humanus* (or Stone taken out of a Man's Bladder) was made up of above half it's Weight of Air.

Some have endeavoured to ſolve Elaſticity by Attraction only; as for Example: If the String A B be conſidered as made up of Particles lying over one another in the manner repreſented at A D B; it is plain, that if the Point D be forcibly brought to C, the Parts will be pulled from each other; and when the Force, that ſtretched the String, ceaſes to act, the Attraction of Coheſion (which was hindered before) will take place, and bring back the String to it's former Length and Situation after ſeveral Vibrations. Now, though this ſeems to agree pretty well with the *Phænomena* of a String in Motion, it will by no means ſolve the Elaſticity of a Spring faſtened at one End, and bent either way at the other, like a Knife or Sword-blade. For if ſuch a Spring be bent from A to *a*, the Particles on the Side C, which now become convex, will be farther aſunder at F, while the Particles at D, carried to the concave Part E, will come cloſer together: So that the Attraction, inſtead of making the Spring reſtore itſelf, will keep it in the Situation in which it is, as it happens in Bodies that have no Elaſticity, where perhaps only Attraction obtains. Thus a Plate of Lead, a Plate of Copper, and a Plate of ſoft Iron, ſtands bent.

Fig. 8. Fig. 9.

But the moſt probable way of ſolving the Elaſticity of Springs, is to conſider both a repulſive and an attractive Property in the Particles, after the manner of the black Sand, which is attracted by the Loadſtone, and has been ſhewn by the learned and ingenious Profeſſor *Petrus van Muſchenbrook*, to be nothing elſe but a great Number of little Loadſtones.

Let us ſuppoſe a Row of round Particles touching one another only in the Points *c* in a Line from A to B. It is plain, from what Philoſophers have ſhewn, concerning the Attraction of Coheſion, that upon the leaſt Shake, or Alteration of the Poſition of a ſtrait Line, theſe Particles will run together, and form a Sphere, in which the Globules will have more Points of Contact. But if theſe Particles have Poles like Magnets in the oppoſite Places marked *n*, *s*, ſo that all the Poles *n*, *n*, *n*, &c. repel one another; and all the Poles *s s s*, &c. do likewiſe repel Fig. 10.

 one

one another, the Line A B will continue ſtrait; for if by any Force the ſaid Line B A be put into another Poſition, as into the Curve *ba*, then the Poles *nn*, &c. being brought nearer together, (while the Poles *s*, *s*, &c. are farther aſunder) will repel one another more ſtrongly, and thereby hinder the Globules from running together towards the concave Part; and the Spring, left to itſelf, (all this while ſuppoſing one End, as *b*, B, or β, fixed) will reſtore itſelf, throwing it's End *a* back to A, and ſo on to α, by the firſt Law: Then being in the Poſition α β, the Poles *s*, *s*, &c. are brought nearer together, whoſe Repulſion, thus increaſed, throws back α to A, and ſo on forward, the Line of Particles performing ſeveral Vibrations round B.

May not a Spring of Steel, or other Springs, conſiſt of ſeveral Series of ſuch Particles, whoſe Polarity and Attraction acting at the ſame time, will ſhew why ſuch Bodies, when they have been bent, vibrate, and reſtore themſelves?

If we take a Plate of Steel, and make it ſo hot till it looks white, and then immediately quench it, we thereby fix the Metal in a State very near Fluidity, ſo that the Particles which the Fire had almoſt brought to Roundneſs, have but a very ſmall Contact; as appears by the Fragility of the Steel thus hardned, which breaks like Glaſs, and has a ſhort Grain. Steel, thus hardened, is highly elaſtic; for what Workmen call *hard*, is the moſt *elaſtic*: As appears by the Congreſs of high-hardened Steel Balls, which return, in their Rebound, the neareſt to the Place we let them fall from; and, next to Glaſs have the quickeſt Elaſticity of any thing we know.

That we may not be thought to have given an imperfect Account of the Elaſticity of a Steel Spring, becauſe ſuch an one as we have deſcribed wants Toughneſs, and will immediately fly when bent to any Degree; we muſt beg Leave to conſider farther the Properties of the round Particles, or little Spheres, of Steel, in which we have ſuppoſed a Polarity.

Fig. 11. Let us ſuppoſe A B to be two little Spheres or component Particles of Steel, in which, at firſt we will ſuppoſe no Polarity, but only an Attraction of Coheſion. Then, whether the Particles have their Contact at *c*, *d*, *e*, *n*, or at δ, ε, *s*, their Coheſion will be the ſame; and the leaſt Force imaginable will change their Contact from one of thoſe Points to another; becauſe in the rolling of theſe little Spheres, they do not come into more or leſs Contact in one Situation than another. But if we ſuppoſe the Point *n* in each Spherule to be a Pole with a Force to repel all the other Points *n* in any other Spherule, and likewiſe *s* another Pole, repelling the other Points *s*; the Spherules will cohere beſt, and be at Reſt in that Poſition where the Points *c*, *c*, are in Contact, and *n* and *s* at equal Diſtances on either Side. For if the Spherules be turned a little, Fig. 12. ſo as to bring the Points *d*, *d*, into Contact, the Poles *n*, *n*, being brought nearer, act againſt each other with more Force than the Points *s*, *s*, which are now farther off, and conſequently drive back the Spherules to the Contact at *c*, *c*, beyond which continuing their Motion, they

will

will go to δ δ, and so backwards and forwards, till at last they rest at *c*, *c*, which we may call *the Point of Æquilibrium for Rest* in a Spring. Now there are besides this, two other *Points of Æquilibrium*, beyond which the Spring may break, which are the Points *e*, *e* towards *n*, and ϵ, ϵ towards *s*; that is, when the Spherules have their Poles *n*, *n* brought very near together, the mutual Repulsion increases so, that the Attraction at the Contact is not able to hold them, and then they must fly asunder, the Spring breaking. We suppose the Points *e*, *e*, to be the Points of Contact, beyond which this must happen; but that if the Contact be ever so little short of it, as between *e* and *d*, the Spherules will return to their Contact at *c*, after some Vibrations beyond it, as has been already said. This is the Reason why I call *e*, (in one of the Spherules) and it's correspondent Point ϵ on the other Side *c*, *the Points of Æquilibrium*; for if the Spring be bent towards *a* (Fig. 10.) so that the Spherules, like A and B, (Fig. 14.) touch beyond *e*, the Spring will break: Likewise if the Spring be bent the other way, till the Spherules touch beyond ϵ, then it will break the other way. Now when the Spherules touch at *e*, *e*, or at ϵ, ϵ, the Spring is as likely to return to it's first Position as to break; for which Reason I have called the Points *e* and ϵ, *Points of Æquilibrium*, as also having known by Experience, that a Spring left bent to a certain Degree, has, after some time, broken of itself. Fig. 13. Fig. 14.

From all this it appears, that Spherical Particles will never make a tough Spring; therefore the Figure of the Particles must be altered, in order to render it useful; and this is what is done in bringing down the Temper of the hard Steel, and *letting down a Spring*, as it is called. What Change ought to be made in the Particles, we shall first shew; and then consider how far that is done by those who make Springs.

If the Parts supposed Globules, as in Fig. 10. are now flattened at *c*, where the Contact is, so as to put on the Shape *n e d c* δ ϵ *s*, the Contact will be much increased, and reach from *d* to δ, so that in bending the Spring there will still remain a great Contact in the Particles, and the Points of *Æquilibrium* for breaking (*viz.* *e*, *e* above, and ϵ, ϵ below) will be removed nearer to the Poles *n*, or *s*, than when the Particles are round; the Consequence of which will be, that the Spring must be bent much farther, to be in Danger of breaking, than in the former Supposition; as may be seen in Fig. 16. where two Particles being opened about the Point *d* as a Centre, the attracting Points *c*, *c*, and δ δ, have still some Force to help to bring back the Particles to their whole Contact; because in this Shape of the Particle the attracting Points *c*, *c*, δ, δ, are removed but in Proportion to their Distance from the angular Point *d*; whereas if the Particles had been spherical, and the Line *d* δ an Arc of a Circle, the attracting Points *c*, *c*, and δ, δ, would have removed from one another farther than in Proportion to twice the Square of the Distance from *d*, as in Fig. 12. and so have afforded very little Help for bringing back the Particles to their Contact. A Row of Particles in the Fig. 15. Fig. 16.

Fig. 17. Spring thus conditioned, is to be seen in the natural State at B A, Fig. 17. and bent at *b a* in the same Figure. Here it is to be observed, that if in this Figure of the Particles you would bend the Spring to bring the Particles to touch at their Point of *breaking Æquilibrium*, you must open them so much on the contrary Side, that the Spring will be bent far beyond any Uses intended to be made of it, as appears by Fig. 18, where two Particles are brought to touch at the equilibrating Point *e*; and by Fig. 19. where many Particles being put into that Condition, the Spring is brought round quite into a Circle.

Fig. 18.

Fig. 19.

Now the common Practice in making Springs is the most likely to produce this Effect required in the Particles; for the hard Spring, whose Particles were round, or nearly so, is heated anew, and whilst it is cooling gently, the mutual Attraction increases the Contact, so that the Particles grow flatter in those Places where before they had but a small Contact; and lest this Contact should become too great, the Spring's softening is stopped by quenching it in Water, or Oil, or Grease. Another way of making Springs, is to begin and shape them in cold unelastic Steel, and then having heated them to a small Degree, for Example, to a Blood red Heat, immediately to cool them in some proper Liquors. This also settles the Particles in their oblong Figure, through which they must pass before they become round, or nearly so, in a white Heat. That Particles of Steel are fixed in the Figures which they have at the Instant of dipping, will not appear strange, when we consider, that dipping red-hot Steel in cold Liquors, in a particular Position, makes it magnetical. If it be asked, how we account for making Springs only with hammering, it is easily answered, That we can make Iron and Steel magnetical only with hammering; and if we can give and destroy Poles in the whole Piece, there is no Improbability to think we can give Poles to little Parts; or rather bring into a particular Situation the Poles which they have; for if the Poles that we have considered be placed quite irregularly, there will be no Elasticity at all. Agreeable to this, Springs may be made of other Metals than Iron or Steel, though not so perfect, by Hammering; for it will be sufficient for the little Particles to have Poles that attract and repel one another, driven by the Hammering into a regular Order.

N. B. *This applied to the Vibration of a String, will better solve it's several Cases than Attraction alone*; and the Elasticity of Glass is just the same as that of a very brittle *Steel-Spring*.

A Description of a Barometer, wherein the Scale of Variation may be increased at Pleasure. By

VI. A B C D, is a Cylindrical Vessel filled with a Fluid to the Height W, in which is immerged the Barometer S V, consisting of the following Parts; the principal of which is the Glass Tube T P (represented separately at *t p*) whose upper End T is hermetically sealed: This End does not appear to the Eye, being received by the lower End of a Tin Pipe G H, which in it's other End G receives a Cylindrical Rod,

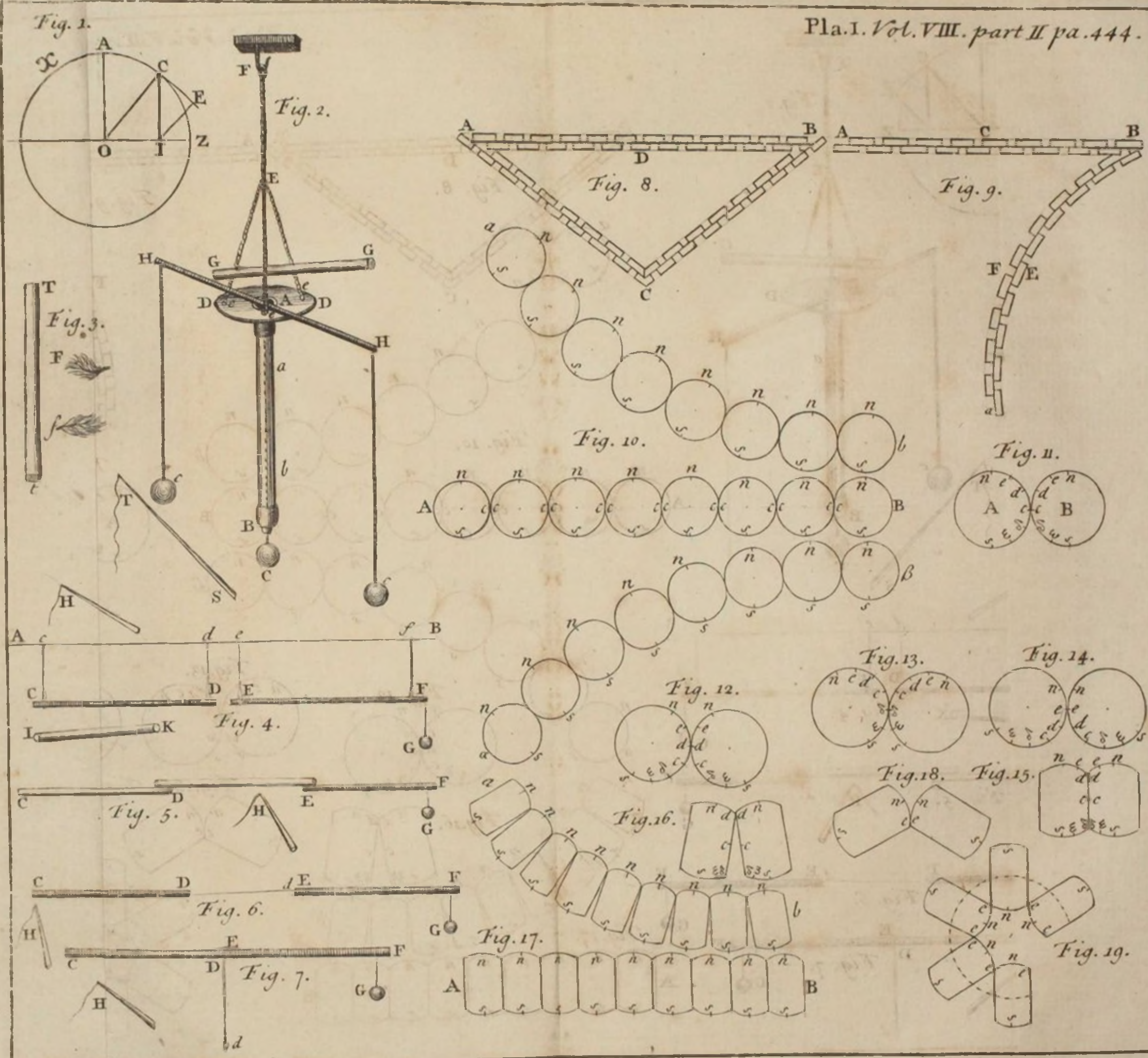


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.

Rod, or Tube S T, either hollow or solid, made of any Materials whatsoever, thereby fixing it to the Tube T P. The Rod S T may be taken off, in order to put in it's stead a larger or lesser, as Occasion requires. S is a Star at the Top of the Rod ST, which serves as an Index, pointing to the graduated Scale L A, which is fixed to the Cover of the Vessel A B C D. M N is a large Cylindrical Tube made of Tin (represented separately at *m n*) which receives in it's Cavity the smaller Part of the Tube T P, and is well cemented to it at both Ends, that none of the Fluid can get in.

the Rev. Mr John Rowning, *Fellow of* Magdalen-*College in* Cambridge. No. 427. p. 39. Jan. &c. 1733. Fig. 20.

The Tube T P, with this *Apparatus*, being filled with *Mercury*, and plunged into the Bason V, which hangs by two or more Wires upon the lower End of the Tube M N, must be so poised as to float in the Liquor contained in the Vessel A B C D, and then it will rise when the Atmosphere becomes lighter, and *è contrà*.

Let the specifick Gravity of Quicksilver be to that of Water, or to the Liquor the Barometer floats in, as s to 1 : And if it be proposed that the Variations of this Compound Barometer shall be to the contemporary Variations of the common Barometer in the given Ratio of n to 1, this Effect will be obtained by making the Diameter of the Rod S T to the Diameter of the Cavity of the Tube H I, as $\sqrt{\frac{n+s}{n\ s}}$ to 1, which may be thus demonstrated.

Let us suppose that the Variation of the Height of the Quicksilver in the common Barometer, called v, is such, that a Cubic Inch of Quicksilver shall rise into the Vacuum X T ; in order to which a Cubic Inch of Quicksilver must rise from the Vessel V, that is, the Surface P must subside so far, that a Cubic Inch of Water (if that be the Fluid made use of) shall enter the Vessel V, by which Means the Barometer with the Parts annexed will be heavier by a Cubic Inch of the Fluid.

Now this additional Weight of a Cubic Inch of Fluid will make the whole Barometer subside (according to the Laws of Hydrostaticks) 'till a Cubic Inch of the Rod H S, immediately extant above the Surface at W, shall come under it ; but the Length of such a Magnitude of H S will exceed the Length of an equal Magnitude of Quicksilver in the larger Tube X, as much as the Square of the Diameter at X exceeds the Square of the Diameter at H (the Lengths of equal Cylinders being reciprocal to their Bases). That is, the perpendicular Descent of the compound Barometer will be to v, the perpendicular Ascent of the Mercury in the common Barometer, as d to 1 (supposing this the Ratio of the Bases) and consequently will be equal to $d\ v$.

But by this Descent, the Distance P W between the Surface of the stagnant Quicksilver and the Top of the Fluid will be augmented by a Column, whose Height is $d\ v$, the Descent of the Compound Barometer ; and consequently the Weight of the whole Column of the Fluid pressing on the lower Surface of the Quicksilver (to which the Height X

is

is partly owing) will be increased by a Column of that Length, and this Increase, would produce a second Ascent of the Mercury at X equal to itself, namely dv, were the Fluid as heavy as Quicksilver; but since it is supposed to be lighter in the Ratio of s to 1, the Ascent of the Mercury on this Account will only be $\frac{dv}{s}$.

But now, as in the former Case, when the Ascent of the Mercury was v, the Descent of the Compound Barometer was shown to be dv, so here the Ascent of the Mercury being $\frac{dv}{s}$, the Descent of the Compound Barometer will be $\frac{ddv}{s}$, and the next Descent $\frac{dddv}{ss}$ and the next $\frac{d^4v}{s^3}$, and so on to Infinity. Therefore the whole Descent of the Compound Barometer, is to the Ascent of the Mercury in the common Barometer, that is, n is to 1, as $d + \frac{dd}{s} + \frac{ddd}{ss} + \frac{d^4}{s^3} +$, &c. to 1, or as $\frac{ds}{s-d}$ to 1; because the Terms of the Series being in Geometrical Progression, the Sum of them all is $\frac{ds}{s-d}$. Hence we have $n = \frac{ds}{s-d}$ and $ns = ds + dn$; that is, $1 : d :: n + s : ns :: \frac{n+s}{ns} : 1$ and $1 : \sqrt{d}$, that is, the Diameter of S T to the Diameter of H I, as $\sqrt{\frac{n+s}{ns}}$ to 1. Q. E. D.

Example I. Putting $s = 14$ and $n = 1$, the Variations in each Barometer will be equal, by taking the Diameter of S T to the Diameter of H I as $\sqrt{\frac{15}{14}} : 1$, that is, as 30 to 29 nearly.

Example II. If n be put infinite, the Diameter of S T will be to the Diameter of H I as $\sqrt{\frac{1}{s}}$ to 1, or 1 to $\sqrt{14}$; that is, as 1 to $3\frac{3}{4}$ nearly.

The Bottom of the Vessel V, and the Ends of the Tubes, ought to be made rather round than flat for their more easy Motion up and down in the Fluid.

It will be convenient to have a ſmall Baſon fixed upon the Star, to contain Shot for the more eaſy poiſing the Barometer in the Fluid.

VII. The Eſſay conſiſts of 3 Chapters; the firſt is wholly Mathematical, containing a new Theory concerning the Propagation of tremulous Vibrations along a Series of contiguous elaſtic Bodies. The ſecond applies this Theory to the Solution of the chief Appearances of the Baroſcope, and the laſt explains the ſeveral States or Conſtitutions of the Air and Weather connected with them.

An Account of a Book intituled Chriſtiani Ludov. Gerſten Tentamina Syſtematis novi ad mutationes *Barometri* ex natura elateris aërei demonſtrandas, cui adjecta ſub finem, Diſſertatio Roris decidui errorem antiquum & vulgarem per obſervationes & experimenta nova excutiens. Francofurti 1733. in 8*vo*. *Ibid.* p. 43.

To give a clearer and fuller Apprehenſion of this Matter, our Author thinks it neceſſary to alter the common Definition of Elaſticity, and uſe the following new Terms: The natural Equilibrium of an Elaſtic Body; it's violent Equilibrium; and the Line of a tremulous Vibration.

By the *natural Equilibrium* of an Elaſtic Body, Mr *Gerſten* means the external Figure and Extenſion, which an Elaſtic Body naturally has, when free from all external Preſſure. By a *violent Equilibrium*, he underſtands that State or Degree of Expanſion which an Elaſtick Body is kept in by ſome external compreſſing Force; and he calls that the *Line of a tremulous Vibration*, which a Point taken at Pleaſure in an Elaſtic Body deſcribes during the Vibration.

I paſs over, for Brevity ſake, the Corollaries drawn from his Definition of Elaſticity concerning the Acceleration and Retardation of a Vibration, as alſo the Deſcription and Uſe of an Inſtrument contrived to prove the Truth of what he had advanced; nor ſhall I take any particular Notice of *Prop.* III, IV, V, and VI, wherein he treats of the Velocity of the Elaſtic Bodies in their tremulous Vibration, of the Motion of ſtretched Strings, and their iſochronal Vibrations.

In *Prop.* X he demonſtrates, that ſuppoſing three Elaſtic Bodies to be detained in a *violent Equilibrium* in the ſame Line of tremulous Vibration, if the middle be farther compreſſed on all Sides, ſo as ſtill to remain contiguous to the neighbouring Bodies, it may be reſtored, by tremulous Vibrations, to a greater Extenſion than it had before. He then ſhews the ſame to be true, ſuppoſing the elaſtic Body be placed between ſeveral other homogeneous elaſtic Bodies, in the State of a *violent Equilibrium*. For the Uſe and Application of all this, he refers us to the third Chapter.

The XI and XII *Propoſitions* deſerve particular Regard: In the former he deſcribes a *Machine*, by which any curious Perſon may enquire into the Phænomena and Laws of the Propagation of a tremulous Motion: In the latter he explains and demonſtrates the Theory of thoſe Propagations, found out by the help of this Machine. The Subſtance of them is this.

Suppoſe a Series of elaſtic Globules or Rings, *a*, *b*, *c*, *&c.* in the Line of a *tremulous Vibration* A B, to be kept in a *violent Equilibrium*, an Obſtacle being placed at A and B. Let us ſuppoſe alſo, the laſt Fig. 22.

Ring

Ring *a* to be compressed farther towards A, *&c.* so as that the Space left in the Line of Vibration may be taken up by the others.

These Things being supposed, the Author asserts, that by the Restitution of the compressed Ring *a*, the Force impressed will be propagated through the other Rings by an individual Action, *i. e.* the Ring *a* in restoring itself will first act only upon *b*, and compress it by the Difference of the Forces. Then *b* being compressed, will transfer the Force it receives, not backwards, but into the Ring *c*, by an individual Action, and compress it likewise in Proportion to the Difference of the Forces, and so on in the other Rings. So that the Compression of the last Ring or Body is to be looked upon as it were like a Substance or Body put into Motion, which continues in Motion, 'till it meets with something else, that makes an equal or sufficient Resistance.

From the Whole Mr *Gersten* draws this material Proposition, that the Body *a* may after this manner acquire, by repeated Vibrations, a greater Expansion in the Line A B, than it had before, provided the Series be long enough, so that the Force impressed may not be soon reflected. This Matter is treated of more at large in the following Propositions, to the end of the Chapter.

The *second Chapter* is an *Application* of the *Theory* delivered, to the Solution of the Phænomena of the Barometer, after the following manner. The Particles of Air, says Mr *Gersten*, however unknown in other respects, are very well known to be capable of receiving and propagating tremulous Vibrations: From hence it follows, as also from some Principles of Sir *I. Newton*, that the Air (*as in Prop.* V) may be dilated by repeated tremulous Vibrations; and by *Prop.* VI, these Vibrations may be generated or produced by a confused Motion of the Particles of the Air, or by the Agitation of a Wind.

The Author in *Prop.* VII and VIII, undertakes to demonstrate, that the Dilatation produced by the Motion of a Wind, is less when the ambient Air has a Motion the same Way, than if the Wind moved with the same sensible Velocity against the quiescent Atmosphere; but that this Dilatation would be greater, if the Atmosphere had a Flux or Current in a Direction contrary to that of the Wind.

Mr *Gersten* demonstrates in the next *Proposition*, that a perpetual *Easterly Wind* will reign in all Places within the Tropics, arising from the diurnal Heat; and that this Wind will diffuse itself to the other Regions without the Tropics, and have a Direction declining from the East towards the North or South, according to the Situation of the Region on the terrestrial Globe; that it's Motion will be more remiss, the nearer the Places are to either Pole, and that the Angle of Declination from the East will be greater for the same Reason.

The preliminary Propositions being thus settled, he proceeds in *Prop.* XIX, to account for the rising and falling of the Mercury in the Barometer thus. The Air of the Atmosphere in our Regions has a natural Motion or Current, whose Direction is situated between the East and

and North Points of the Compaſs. If therefore a ſpecial Wind ſhould ſpring up and blow in a contrary Direction, it will produce tremulous Vibrations, and conſequently Dilatations of the Air; then equal Bulks of the dilated Air dilated, will have a leſs Quantity of Matter than before: Therefore the Gravity of the Air will be leſſened, and by Conſequence the Quickſilver in the Weather-Glaſs will fall. And this Decreaſe of Gravity in the Air, and of the Height of the Mercury in the Baroſcope, will be proportional to the Greatneſs of the Force of the Wind and Degree of Oppoſition of it's Direction to that of the Flux of the Atmoſphere conjunctly.

This, ſays Mr *Gerſten*, is the Reaſon why the Mercury falls when Southerly or Weſterly Winds blow, and why the Quickſilver ſinks ſo very low when theſe Winds blow Storm. On the contrary, ſince the Effect ceaſes when the Cauſe is removed, the Height of the Mercury will be greater, the fewer ſpecial Winds there are blowing in a contrary Direction: So that the gentle Winds that blow from the Points of the Compaſs, which lie between the North and the Eaſt, are, as the Author believes, nothing but the natural and univerſal Motion, Current, or Flux of the Atmoſphere impeded by or meeting with very few ſpecial Fluxes. In order to illuſtrate and confirm the Truth of the Demonſtration of this Experiment, he hath in *Schol.* 1. quoted the Experiment of Mr *Hawksbee*, in his *Phyſico-Mechanical Experim. Sect.* V. *Exp.* 5. *pag.* 114. *Edit.* 2.

The Deſign of *Prop.* XX is to prove, that a ſpecial Wind blowing parallel to the Direction of the univerſal, will permit the Mercury to ſtand at a greater Height, than if it had blown in a contrary Direction with the ſame Force. This he confirms by three Obſervations of his own in the *Scholium* annexed.

Prop. XXI ſhows why the Deſcents of the Quickſilver are ſucceſſive, and do uſually, as well as regularly, precede the Arrival of the Winds that cauſe them.

Prop. XXII aſſigns the Reaſons why very conſiderable Alterations in the Riſe or Fall of the Mercury are obſerved at the ſame Time in different Places, though they are at a great Diſtance from each other.

In *Prop.* XXIII, he takes into Conſideration what Influence the Heat has on the Variations of the *Baroſcope*, and denies that it cauſes any ſenſible Changes: However, in the *ſecond Scholium* of this Propoſition, he explains by it, why the Limits of the Variations of the Mercury leſſen, as the Places are nearer the *Tropics*.

The *third or laſt Chapter* is taken up in accounting for the various Changes of the Weather connected with, or conſequent upon the Riſe and Fall of the Mercury in the Weather-Glaſs. The ingenious Author beginning with the Original and Manner of forming Vapours, undertakes to ſettle and confirm, upon ſolid Principles, that which Dr *Halley* had long ago communicated to the learned World, upon this Argument.

In *Prop.* VI he makes use of the Principle mentioned before, concerning an elastic Body, that it restores itself to a greater Degree of Expansion than it was in before it's tremulous Vibrations; and endeavours from thence to explain more particularly the Way, that Nature takes in forming and distending the Cavities of the vapoury Bubbles, and afterwards emitting or detaching them from the Surface of a Fluid.

Prop. VIII gives an Account of what will happen to Vapours, according to this Theory, when the surrounding Air is condensed, or rather compressed by an external Cause. Mr *Gersten* affirms, that in this Case it is not possible for them to descend. In the *Scholium* subjoined, he enlarges upon this Subject, and maintains, that the same will happen if the Air be condensed by any internal Cause, for Instance, Cold.

It was this *Proposition*, as the Author informs us, that put him upon inquiring more exactly into the Nature of Dews, which by their Fall in a cold or condensed Air seemed to contradict this Part of his Theory. The Result of his repeated and laboured Enquiries is a *Dissertation*, wherein he undertakes to prove, by a Variety of Experiments, that Dews do not fall, as both the Vulgar and Learned believed before, but rise out of the Earth. Of this we shall give a brief Account, as soon as we have observed, that in *Prop.* IX and X, he considers what will happen to Vapours in any external Dilatation of the Air; and in *Prop.* XI, shows, that in that Case the Clouds are resolved into Rain; and upon this he deduces, from the two last Propositions, the Reason of the Descent of the Mercury in the Barometer in rainy Weather, and, on the contrary, of it's Ascent in fine Weather.

The Design of the Dissertation annexed, is to enquire into the Nature of Dew, explain it's Original and Kinds.

All Dews, according to our Author's Philosophy, owe their Original either to Vegetables, or terrestrial ascending Exhalations. Such as derive their Origin from Vegetables, he takes to be only Exudations of their Leaves, *&c.* congealed by the Air. Before he enters upon the Proof of these Positions, he gives us three general Observations regarding the Circumstances that are requisite, in order to have a plentiful Dew in any Place. As first, the Place in the Day-time must be exposed to the Rays of the Sun for a considerable Time; for in shady Places, or where the Sun shines but little, little or no Dew is to be found. There must also be a considerable Difference between the Heat of the Day, and the Cool of the Night; and in the last Place, a sufficient Moisture in the Earth.

In treating of that Kind of Dew, which is a Secretion or Exudation of a Juice in Vegetables, he observes, that some Plants furnish the Spectator with a very entertaining Sight, the little Drops of Dew being disposed after a very regular, not fortuitous Manner, upon the Surfaces or Edges of their Leaves. He gives us the Figures of ſome of them in a Plate.

To determine whether this beautiful Diſpoſition of the dewy Particles is owing to a Deſcent from the chilled Air over the Plant, or a Secretion made from the Juices of the Plant itſelf, he covered ſeveral with Glaſſes, or earthen Veſſels, having their Mouths downwards; and yet the next Day Plenty of this Kind of Dew appeared in it's uſual regular Form.

As to the next Species, or common Dew, he produces ſo many, and ſo differently made Experiments, againſt the vulgar Opinion of it's Deſcent, that if they be all true, it ſeems difficult to ſupport it againſt them. I ſhall mention ſome of the principal.

For two Months together, *viz. June* and *July*, in 1728, every Night, ſeveral ſmooth Plates of Braſs were laid upon the bare Ground; and during theſe Experiments, he never obſerved the leaſt Impreſſions or Traces of Dew on the upper Surfaces, whereas the lower were always covered with it. He repeated the ſame Experiments laſt Year, and with the ſame Succeſs, except in one Caſe, where a Plate lying too near ſome *Lavender*, was bedewed a little on that Part of it's Surface, which was next the Plant, the other Part however remaining dry. He alſo ſuſpended theſe Plates by Threads, in an horizontal Situation, and found the Dew ſpread almoſt equally over both Surfaces, at the Height of three, four, or five Feet, at the Diſtance of one Foot and an half, the lower was more bedewed; but at the Heights of one, two, or three Inches, the lower was overſpread with Dew, while the upper had none.

He is ſo impartial as to mention, in *Section* XVI, ſome Experiments which he made, and at firſt View ſeemed to contradict his Theory: For Inſtance, when he uſed convex Bodies, whether round or cylindrical, he found the upper Surface covered with Dew, and that, whether they were laid upon the Ground, or ſuſpended at any Height from it.

This Obſervation is general, and extends to Bodies of this Kind, that are only contiguous, as Heaps of Straw, Hay, or Wool. It is to Obſervations of this Kind, the vulgar Opinion of Dews falling, owes it's Birth and main Support. Mr *Gerſten* therefore propoſes to conſider theſe diſtinctly in another Eſſay: But leaſt any Argument ſhould in the mean time be drawn from them againſt this Hypotheſis of the Aſcent of Dews from the Earth, he oppoſes Experiment to Experiment.

Thus in *Section* XVII he lays upon the Ground a Board two Foot and a half long, eleven Inches broad, and two Inches and a half thick, with a Quarter of a Sheet of Writing-Paper upon it, having about an Inch hanging over one Edge of it.

To ſecure the Paper from being removed out of it's Place by the Wind, he faſtens it with an Awl ſtuck perpendicularly, having a round wooden Handle, and lays a Knife with a cylindric wooden Handle, as an additional Weight. Upon his Return to ſee what Effect the Dew had upon them, he found that the ſmall Part of the Paper, which was extended over the Edge, was moiſtened with Dew, while the reſt of the Paper, as alſo the upper Surface of the Board, were dry, but the upper Parts of the Handles of the Awl and Knife all wet.

An Experiment ſomething a-kin to this was made with a Glaſs Tube laid horizontally upon a Braſs Plate fourteen Inches ſquare, having about two Inches and a half reaching beyond the Side of the Plate. The Tube was kept from rolling by two Parallelopipeds of Lead. The Event was, the whole Surface of the Tube was bedewed, while the upper Surface of the Plate remained dry.

In *Section* XXI, XXII, we have a Set of Experiments made with concave Veſſels, having their Mouths upwards, and placed at different Elevations above the Earth. In theſe Caſes alſo he found no Dew at the Bottom of their Cavities, nor on the Sides, except within about an Inch near the Brim.

Since Hoar-Froſt is only common Dew congealed, he applied himſelf to make ſome of the ſame Kind of Experiments upon that, with Braſs Plates laid upon the Ground as before. Theſe likewiſe he found covered with this Kind of Froſt below, but free on the upper Superficies, agreeably to his Hypotheſis.

The Author cloſes the Diſſertation with a curious Inquiry into the Nature and Original of *Honey-Dew*. This he takes to be nothing but the Excrements of ſome Inſects which are to be met with, adhering to the lower Superficies of the Leaves of Plants; and appeals to the Evidence of Senſe for a Demonſtration.

Let any inquiſitive Perſon, ſays he, lay a Piece of Paper under any of the Leaves abounding with theſe *Animalcula*, and in a ſmall Space of Time he will find a Liquor, or ſoft Subſtance, the very ſame with what we call *Ros Mellis*, gathered together in good Plenty upon it.

Of the Differences of the Heights of Barometers, by Sam. Chriſtian Hollman. In Regia Georgia Auguſta. Leg. Mat. & Theol. Nat. P. P. O. No. 464. p. 116. *Read* May 20, 1742.

VIII. In *July* 1741, being to take a Journey with our famous *Haller* to the Mountains of our famous *Hercynia*, I prepared among other Things a Barometer, with which I intended to make Experiments as we went along. I had carefully divided the Scale of Aſcent and Deſcent of the Barometer into *Rhinland* Inches and Twelfths, or Lines, from the 20th to the 32d Inch. When I went to apply it to this Barometer, which was newly made, and compared it with 6 others, which I had by me, I found unexpectedly, that none of them exactly agreed in Height, there being a Difference from 2 to 12 Lines between them. I was ſatisfied that there was no Fault in any of them; and yet the new made one, which I had hitherto found to be exceeding good, and very ſenſible, roſe 2 Lines above them. On my Return I began to inquire diligently into the Differences that I had obſerved, and as I found them to be the ſame again, I conſtructed new Barometers, with upright Tubes, but different Apertures. I found again the Differences between theſe to be from 1 to 4 Lines; and thoſe, which roſe the higheſt, exceeded that which I have called my beſt by full 6 Lines. *Aug.* 12. I repeated the ſame Experiments with theſe 15 Barometers, and

and again obſerved nearly the ſame Differences. Therefore I prepared 10 new ones, with upright Tubes, Part of them having bent Glaſs Ciſterns to receive the Quickſilver deſcending from the Tubes, and Part without. Here I obſerved 10 Heights of theſe Barometers to differ from 1 to 1 ½ Line, and to exceed the Height of my beſt Barometer 4 Lines.

It is not neceſſary to mention with how much Circumſpection and Care I conſtructed all theſe Barometers. They were all made after the ſame Manner, and great Care was taken that no Air ſhould remain in the Extremity of the Tubes, or between the Particles of Quickſilver, or ſtick to the Glaſs. The laſt 18 were made with the ſame Quickſilver perfectly purified. But there was ſome Difference among the Glaſs Tubes, which with ſome other Circumſtances muſt not be paſſed over in Silence: For my beſt Barometer, in which the Quickſilver has always the leaſt Height, has a Tube made of green Glaſs, and a ſeparate Ciſtern made of the ſame Glaſs, with ſome Quickſilver in it. I ſhall diſtinguiſh it in Claſs I. under N°. 1; and that which I uſed in the Mountains of *Hercynia*, under N°. 2. But that Tube, in which the Quickſilver roſe higheſt, and often a full *Rhinland* Inch higher than on N°. 1, is made of the whiteſt Glaſs. It is inſerted into a wooden Ciſtern, and has a remarkable *Phoſphorus*; but changes it's Heights more ſlowly than any of the reſt: For I ſuppoſe every body knows, that the Quickſilver does not riſe in all Barometers in the ſame Manner, and with equal Readineſs. I have known it myſelf above theſe 7 Years. I ſhall diſtinguiſh this Barometer under N°. 7: That under N°. 3 is a diagonal one, with a ſingle Bend, and a bent Glaſs Ciſtern adhering underneath. N°. 4 is *Bernoulli*'s, the Tube of which is to the Cylinder faſtened above as 1 to 8. N°. 5 is *Huygens*'s, the Diſagreement of which, as well as of the diagonal ones with the reſt, is not to be wondered at; for the Cauſes of their not agreeing are evident. N°. 6 is another diagonal one, but with a double Bend, one of which is received by that Part of the Tube to which the Scale is applied, intercepting an Angle of about 25°, with the perpendicular Part of the Tube, in the double Angle of which, becauſe of the greater Narrowneſs of the Tube, the Quickſilver muſt of Neceſſity be wonderfully retarded in it's Aſcent and Deſcent.

The Barometers, which I have referred to the ſecond Claſs, have all of them Glaſs Tubes of a different Kind; but the Glaſs is ſubject to one Fault, that when melted at a Lamp, the Surface becomes covered with very ſmall Scales, and loſes Part of it's Tranſparency. And theſe are the Tubes, in which the Quickſilver riſes to the greateſt Height in the upright and ſimple Barometers, excepting only that which produces the *Phoſphorus*.

To the *third* Claſs I have referred thoſe Barometers, which conſiſt of very white Tubes, but ſuffer no Alteration by the Fire, and were prepared in the ſame Glaſs-Houſe at the ſame Time. The Barometers

compoſed

composed of these were all rectilineal and simple. And among these I found no greater a difference of Height, than from $\frac{1}{2}$ to 1 $\frac{1}{2}$ Line; and the greatest Height in these did not exceed the least in the others by above 4 Lines, as I mentioned before.

Are we therefore to seek the Cause of this Difference in the Diversity of the Glass Tubes? Is not the Surface of one more rough and uneven than the Surface of another, and does it not therefore more or less resist the Ascent of the Quicksilver by it's Friction? or is it from any other Cause?

I shall now give a short Account of my Observations.

Class I.

Barometers of which the different Heights were observed *July* 27. and *Aug.* 12.

	Aperture of the Tube.	Height of the Quicksilver. *Rhinland Foot* ″	‴
N°. 1	$\frac{1}{3}$ Line	27	11
2	$\frac{1}{2}$	28	1
3	$\frac{1}{2}$	27	11
4		28	4
5		28	5
6	$\frac{1}{3}$	28	7 $\frac{1}{2}$
7	$\frac{1}{2}$	28	9

Class II.

8 Barometers new made *July* 27.

	Aperture of the Tube.	Height of the Quicksilver. ″	‴
N°. 1	2 $\frac{1}{2}$ Line	28	1 $\frac{3}{4}$
2	2	28	4
3	1 $\frac{2}{3}$	28	4
4	1 $\frac{1}{2}$	28	2 $\frac{1}{4}$
5	1 $\frac{1}{3}$	28	2
6	1 $\frac{1}{4}$	28	2
7	1	28	2
8	$\frac{2}{3}$	28	4

Class III.

10 Barometers newly constructed *Aug.* 12. of which the 5 first had no Cisterns annexed, and the 5 last had bent Glass Cisterns annexed underneath, to receive the Quicksilver descending from the Tube.

	Aperture of the Tube.	Height of the Quicksilver. ″	‴
N°. 1	$\frac{1}{3}$ Line	27	11 $\frac{1}{2}$
2	$\frac{1}{3}$	27	11 $\frac{1}{2}$
3	1 $\frac{3}{4}$	27	11 $\frac{1}{2}$
4	2 $\frac{1}{4}$	28	0
5	1 $\frac{1}{2}$	27	11 $\frac{1}{2}$
6	1 $\frac{2}{3}$	28	0
7	$\frac{1}{2}$	28	0
8	1	28	1
9	1 $\frac{1}{2}$	27	11 $\frac{1}{2}$
10	$\frac{1}{4}$	27	11 $\frac{1}{2}$

Therefore since different Barometers made at the same Time, and in the same Place, have different Heights; ought we not to think about *harmonic* Barometers with the same Earnestness as about Thermometers, before we can with sufficient Security collect from the annual Observations of Barometers, their mean Heights in different Places,

Places, and thence, among other things, the Elevations of thoſe Places above the Surface of the Sea? According to the Barometer, which I have now uſed for 7 Years, and put in Claſs I. *N.* 1. the mean Height of the Quickſilver here will be = 27'' 10''' *Rhinland* Foot; but according to the 2*d*, which I uſed in the Mountains of *Hercynia*, 28'' 0''', to which the reſt may be eaſily referred.

But to conclude, I ſhall add only one thing more, which I obſerved with the Barometer on the Metal-bearing Mountains of our *Hercynia*, and on the loftieſt Summit of them, called in German *Der Brocken*, or *Der Brockeſberg*, I went thither *July* 9. when the Barometer juſt now mentioned ſtood here at *Gottinghen* at 28'' 3''', but the next Day on the Summit of *Blockſberg* it ſtood at 25'' 2''', when in the mean Time the Height was altered only 2 Lines here. Therefore the difference of Height between this Place and the Top of *Blockſberg*, which is the higheſt of all the Mountains in this Neighbourhood, is = 2'' 11''', which, according to *Scheuchzer*'s Computation, would anſwer to 2550 *Paris* Feet, or in round Numbers to about 2500: Which Height, tho' it ſeems conſiderable enough to the Inhabitants of this Mountain, yet can by no means be ſet in Competition with the Mountains of *Switzerland*, *France*, and other Countries.

The Imperfections of the Common Barometers, and the Improvement made in them, by Mr Cha. Orme *of* Aſhby-de-la-Zouche *in* Leiceſterſhire, *where they are perfected and rectified; with ſome Obſervations, Remarks, and Rules for their Uſe, by* Hen. Beighton, *F. R. S.* No. 448. p. 248. June *&c.* 1738.

IX. The Barometer I am about to deſcribe, is not different in Form from ſome uſually made, it being of the Diagonal Kind from whence the more minute Alterations are more readily diſcovered: Of this Form many have been made by the late curious Operator Mr *Patrick*, who though he had done ſo much towards the proving the Weight of the Atmoſphere by which the Mercury in the Tube was ſuſtained, he himſelf did not believe it, but run into that Abſurdity of the Funicular Hypotheſis.

There is an Inconveniency or Imperfection in moſt, if not all, of thoſe Diagonal Barometers; for after ſome time, the various riſing and falling, and Changes of the Weather, of Heat and Cold, the ſmall Particles of Air that have been interſperſed in the Mercury, have got together in a larger Maſs, as they will incline by Attraction, which will ſeparate the Mercury; and that Quantity of Air will be dilated by Heat, and contracted by Cold, ſo as to ſpoil the Deſign thereof.

Beſides, there is ſuch a Coheſion or Attrition of the Mercury to the Tube, (eſpecially in the ſmall ones) that after ſome Time, the Mercury that is not truly cleanſed from it's Droſs, and purged of all it's Air, in remarkable Changes of the Weather will neither riſe nor fall. All which Embarraſſment is taken off, and the Difficulties ſurmounted, in

Mr Cha. Orme*'s Improvements of the* Barometer, *by the Method following.*

Firſt, The Quickſilver is all purified from it's Droſs and earthy Particles by Diſtillation; and when the Tube is filled by a Pound and half,

two,

two, or three Pounds of Mercury, and all the Air got out by the Methods used in filling Tubes, then the remaining Air is got out by such an intense Heat of Fire as makes the Mercury boil; by which Ebullition an innumerable Quantity of small Particles are emitted, and blow with a great Velocity at the open End of the Tube, till all the Air is quite cleared out; which curious as well as fatiguing Operation is continued for the Space of four Hours: And when no more Bubbles would rise in the Tube, it remained whole, with it's Mercury of a most lively sparkling Brightness, with this Difference only, that the Mercury, so purged from it's Air, did not fill the Tube so high as when first put in by about two Inches; which is a plain Demonstration, that in that Tube, which was 49 Inches long, there was interspersed in the Mercury at first filling it, so much Air as would fill two Inches of the said Tube, which was a 24th Part of the said Space.

The whole Operation I myself attended the 20th of *January*, 1734-5.

And further I can affirm, that every Part of the Mercury boiled for a long Time, and the Tube was *gradatim* so red-hot, that with a warm Knife I could make Impressions in any Part of it.

And this I the rather mention, by reason I have heard several Persons, and those not incurious, affirm it was impossible.

And that this is the most sure and certain (if not the only) Method for getting out all the Air, may be judged by the boiling of Water, which in it's Ebullition does emit a great Quantity of Air for a long Space of Time.

The Perfection *of these* Barometers, *which exceed all others, I have ever observed in the following Particulars:*

1. They are sensible of the most minute Changes of the Air whatsoever.

2. They foretel the Weather by a much longer Space of Time than others, as mostly 20 Hours, sometimes 36 or 48 Hours: Nay, before great Tempests, and such Rains as cause great Floods, for a much longer Time before they happen.

3. Although they are so sensible of such minute Changes of the Air, yet the most intense Heat will not raise them a Hair's Breadth, nor the greatest Cold make them fall. This shews they are perfect Barometers, and not in any Degree Thermometers.

4. You may by them distinguish whether, if they shew for Rain, it will be little or much.

5. As by other Barometers you cannot tell the Weather, but by a past and a present Observation; these tell you the Instant of Time you come to them, what the Weather is going to be: For by rapping the Case with your Finger, if it is going to be fair, or very fair Weather, the

the Mercury will riſe that Moment $\frac{1}{10}$ of an Inch, or more: But if for foul, it will ſcarce make any ſenſible Riſe.

I have had one of the Glaſſes by me for 10 Years, and have conſtantly obſerved it's Motions, which has very ſeldom failed me in foretelling any conſiderable Change of the Weather.

But as ſome People have ſuch ſtrange Notions, as not judging afterward whether they were told true or falſe, and others may miſs in their Expectations of perfect Certainties, which none can attain to, it will not be improper to make the following REMARKS.

1. Though you can foretel it will rain on the Morrow, it is impoſſible to tell where that Rain will fall; for as every Shower has Space, *i. e.* Length and Breadth, if it rains in that particular Field, yet it may be fair in the next adjoining: And if in Harveſt, or on a Journey, you proclaim it will rain on the Morrow, ſome will, if it does not fall on their Land, or on his Coat, be ſo ſilly as to ſay the Prediction was falſe. Remarks.

2. The Barometer does only ſhew the Preſſure or Weight of the Atmoſphere, and Inclination of the Air, in and about the Country where it ſtands, and not always in a particular Spot; ſo that in foretelling of great Rains, People are apt to ſay the Indication is falſe, becauſe they have not ſeen or heard of it; when perhaps in a Day or two you will hear, that it did then fall three, four, or may be 10 Miles off: For though the Rain ſhould be over us when the Glaſs fell, yet the Wind carries the Clouds and Rain with it.

3. It is very hard to diſtinguiſh on the Mercury's falling, whether it will be Rain or high Winds, they equally cauſing the Mercury to ſubſide.

4. Of all thoſe who gueſs at the Weather from the Whims of their own Brains, it is obſervable it is not true one Time in ten, nor do any two of them agree about it.

But from Obſervations on this Barometer, it will ſeldom fail you once in 20; ſo it is above 100 to 1 preferable.

5. If from the State of the Mercury Yeſterday and this Morning, it be pronounced the next Day will be no Rain, and I look at the Glaſs no more To-day; perhaps Winds may ariſe, and ſo alter the Atmoſphere's Weight, and the Glaſs falls much, it will rain on the Morrow, contrary to what I at firſt expected. Here it is plain, had I ſeen the Glaſs again in the Afternoon, I might have alſo foreſeen the Rain.

Hence it is evident from theſe Remarks, that Judgments are taken on the Weather from Barometers, which do not prove ſo; and this begets Opinions in the Vulgar and Ignorant, that there is no Judgment at all to be had from them.

If they could conſider, nothing in Nature is certain, permanent, and perfect, neither in ourſelves, or what we do or think; then why do we expect it in the Air? Is it not ſubject to as many Chances, Variations, and Mutations? Or why ſhould we expect a Foreknowledge of it abſolutely from the Barometer, and that it ſhould force us to underſtand it's Meaning infallibly?

If the Barometer could only foretel very great and remarkable Changes of the Weather; for Inſtance, in Harveſt-time, that a very great Rain, or perhaps Floods, were coming, the Huſbandman would ſtop cutting down his Grain, and ſave ſome of it being ſpoiled by the Wet: Or on a Journey, if I know that if I do not get Home by ſuch a Time, or paſs ſuch Rivers, the Floods will be ſo great as not only to prevent me, but endanger my Life: And may be here is a Man's Fortune ſaved, nay his Life, merely from the Indications of the Barometer; and who reckons this nothing, deſerves neither.

Do not we reckon a Memory, or a Hiſtory, good, that calls to Mind, or notes every valuable or remarkable Event, though not every Tale or trifling Story?

The greateſt Storm that has been in our Days, was *Jan.* 8, 1734-5. On the 5th the Mercury began to fall, and on the 8th was $\frac{1}{10}$ below 28 Inches, which has not been ſeen in this Age, or perhaps ſince *Torricellius*'s Time; thence I could plainly indicate, that it would be the greateſt Flood we ever heard of, or the greateſt Storm we ever felt; the latter of which it proved.

Some Rules and Obſervations for foreknowing the Weather, by the riſing and falling of the Mercury.

Though riſing always preſages fair, and falling foul Weather, yet there are ſeveral Difficulties and Niceties in making a true Judgment from them, and herein conſiſts the chief Part of the Art.

I ſhall not trouble you with the ſeveral Obſervations made by Dr *Halley*, Dr *Beal*, Dr *Derham*, Mr *Patrick*, and others, though they are moſt of them applicable to this improved Diagonal Barometer, by reaſon their Eſteem has cauſed them to be in ſo many Hands, and in moſt Authors on the Subject, and becauſe I have collected them in order to be made public, at the Requeſt of the Improver of the Barometer, Mr *Orme*, and for his Uſe; which ſome Time ſince were put into the Hands of Dr *Deſaguliers*, who is acquainted with Mr *Orme* and his Glaſſes. I ſhall only inſert here ſome few Obſervations, which I believe may be called Rules, as I have deduced them from Time to Time, in uſing Mr *Orme*'s Glaſſes, and keeping a Regiſter of the Weather; and ſhall at the End of this Account inſert ſeveral more Obſervations on the Diary of the Weather, now ſent with this, which are not yet digeſted into certain Rules, but may in Time, I preſume, by ſome more ſkilful Perſons; or by a longer Series of Obſervations and Regiſters of the Weather, which I deſign to purſue, if Health continue.

Rules and Obſervations for the improved Diagonal Barometer.

1. This Barometer very rarely foretels Thunder, ſeldom falling at all before it, which Mr *Patrick* obſerves others do.

2. In ſerene and hot Weather, when the Mercury is high and riſing, and you have all the poſſible Certainty of fair Weather the next Day, and if there happen to fall great Showers, you may conclude they have been driven upon you by Thunder, though you have heard nothing of it.

3. When

3. When the Mercury is pretty high, and has fallen to foretel Rain, and it riſes again before the Rain cometh, it indicates there will be but little of it.

4. If the Mercury continues falling whilſt it does rain, it ſhews it will rain the next Day.

5. In fair Weather, when the Mercury has continued high or riſing, if it falls a little To-day about Noon, and towards the Evening riſes again, you muſt expect a ſingle Shower the latter Part of the next Day, (or perchance by Noon) and then fair Weather again forward.

6. When the Mercury riſes gradually, (about half a 10th perpendicular) and continues ſo to do for many Days together; you may reaſonably expect a fair Seaſon for as long a Time as it was riſing, unleſs ſome Gales of Wind intervene, and eſpecially the S W by S, or thereabouts.

7. When the Mercury riſes very faſt, or falls very faſt, neither the fair nor foul Weather it forebodes will continue long.

8. Without knowing how the Mercury has ſtood ſome little Time before, a true Judgment cannot be given at all Times: For ſuppoſe I find it in a riſing Condition, I am apt to think it will be fair; but if it had been higher ſome Hours ago, and fell, there muſt happen a Shower.

That the Mercury in the Diagonal Barometer, (if it be for fair Weather) on rapping the Caſe ſeveral Times, which jars and makes the Tube tremble, will riſe at every Stroke for ſeveral Strokes together, and in all ſometimes $\frac{1}{10}$ of an Inch, or more, in the perpendicular, may, I preſume, be thus accounted for:

1. There is a Coheſion of the Mercury to the Tube, which hinders it's riſing, and ſuch rapping releaſes that.

2. But it is obſervable, that it will riſe a little at all Times, even when it is in a ſtanding or even in a falling Condition. This may be accounted for thus:

The Mercury and Atmoſphere are in an *Equilibrium*, and rapping ſtarts and raiſes the Mercury a little in a boiling Manner, eſpecially the upper Surface of it, which is ſeen to leap, or be in a ſwimming Poſture; then the Preſſure of the Atmoſphere over-balances the Remainder of the Mercury, and it muſt riſe a little.

Or ſuch violent Jarring puts the Mercury in a lateral and upward Motion, (for downward it cannot go) which takes off it's Gravity, as the Winds leſſen the Preſſure of the Air; therefore it muſt riſe a little.

But then it is obſervable alſo, that if the Mercury was in a ſtanding Condition, or falling, ſuch riſing as above will in a Minute come to the ſame Place again; and even when the Mercury is in a riſing Condition, it will, in that Space of Time, fall a little Part of that it roſe by ſuch rapping.

This Barometer has the Coruſcations, as they were obſerved in Mr *Patrick*'s pendant one; for by rapping the Caſe with the Finger in a dark Place, it will emit ſeveral bright Flaſhes along the empty Part of the Tube.

This I take to be an Argument that the *Vacuum* is very pure, and the Mercury truly purged.

Collections from the Diary of the Weather and Barometer, in order to ſettle Rules for foretelling the Weather by the Barometer.

Before great Storms the Mercury falls 3 or 4 Days, and is exceeding low.

1734-5, *Jan.* 4. at Night the Mercury at 29.92 Inches.
5. Night — — — — 29.66
6. Night — — — — 29.2
7. Night — — — — 28.1
8. Noon — — — — 27.9 Lower than has been known by $\frac{1}{10}$, and the greateſt Storm of Wind ever heard of in this Age, in the South of *England*, as alſo in *France* and *Holland*.

1736. *Jan.* 31. — — — — 29.47
Feb. 1. — — — — 29.15
2. — — — — 28.39 Rain and ſtormy.
1734. *Aug.* 11. — — — — — — Stormy.

Before great Floods the Mercury falls very much.

1735. *Sept.* 4. — — — — — 29.7
5. — — — — — 29.6
6. Night — — — — 29.6
7. — — — — — 29.25 The greateſt Flood that has been (at *Coventry*, being about the Middle of *England*) theſe 40 Years, and yet the Mercury fell but little.

1735. *Oct.* 23. — — — — — 29.55
24. Night — — — 28.8
25. Night — — — — 28.78
26. — — — — — 28.85
27. — — — — — 28.26 A great Flood.
1735. *Aug.* 19. — — — — 29.3
20. — — — — — 29.28
21. — — — — 29.3
22. — — — — — 29.2
23. — — — — — 29.2 Stormy, great Rain.
24. — — — — 29.38 Floods.
1735. *Dec.* 2. — — — — 29.32 Rain.
3. — — — — — 29.5 Fair.
4. — — — — — 28.8 Rain.
5. — — — — — 28.9 Rain.
6. — — — — — 29.5 Fair.
7. — — — — — 29.52 Great Rains and Floods.

The

The Mercury seldom falls for Rains that come by Thunder. See Diary, *June* 2, 1735.

When the Mercury did rise.

1733. *June*	21.	29.16	29.56
	22.	29.56	29.56
	23.	29.62	29.65 Hot.
	24.	29.65	29.57 Sultry.
	25.	29.54	29.52 Sultry.
	26.	29.51	29.59 Great Thunder.
	27.	29.57	29.56 A very violent Thunder, from ten in the Morning to one in the Afternoon, doing great Damages.

1735. *June*	1.	29.3	29.8
	2.	29.4	29.55 Thunder and great Rains.

The Mercury fell before Thunder.

1733. *July*	27.	29.44 Hot, fair.
	28.	29.87 Wind, Rain.
	29.	29.09 Violent Thunder.
1734. *Aug.*	7.	29.59 Sultry.
	8.	29.46 Fair.
	9.	29.25 Thunder.
	10.	28.87 Rain, Thunder.

A Frost, when the Mercury is high, brings Rain.

1731, *March.* The Mercury was high all the Month, and no Rain, but what followed the Frost on the 17th and 29th.

Dry Season in *June* 1729, and the Mercury scarce ever above changeable.

In *Aug.* 1730, the Mercury never lower than 29.37.

1731, from the 1st to the 10th, and Rain came the 16th, though the Mercury was rising.

A great Frost, although the Mercury fell; but it was attended with a great Snow, which might occasion it to subside.

1731, *Jan.*	1.		29.56 Rain.
	2.	29.46	29.12 Rain.
	3.	28.78	28.72 Wind.
	4.	28.72	28.81 Frost, great Snow.
	5.	28.93	29.12 Snow, Frost.

Great

Great Rains, although the Mercury was rising.

1732. *May*	1.	29.28	29.25 Wind.
	2.	29.21	29.25 Rain all Day, Snow hard from 8 to 11.
	3.	29.34	29.0 Rain.
	4.	29.09	29.09 Rain.
	5.	29.12	29.34 Wind.
	6.	29.44	29.46 Fair.
	7.	29.52	29.39 Rain and great Floods.

Great Rain, though the Mercury fell but little.

1733.	24.	29.6	29.54 Wind.
	25.	29.51	29.54 Fair.
	26.	29.52	29.54 Fair.
	27.	29.5	29.39 Violent Rain for more than 11 Hours.

Great Rains, the Mercury falling very much.

1734. *July*	10.	29.65	29.67 Fair, hot.
	11.	29.63	29.62 Fair, hot.
	12.	29.59	29.4 Rain.
	13.	29.29	29.13 Great Rains.

——The Mercury falling a great while before the Rain came, and the Rain continued as long.

1736. *May* 19.	29.75 Fair, Wind,	29.8
20.	29.8 Cold Wind, fair,	29.7
21.	29.65 Cold Wind,	29.52
22.	29.39 Wind, Clouds, Rain,	29.31
☉ 23.	29.28 Cloudy, fair	29.27
24.	29.32 Fair,	29.35
25.	29.32 Cloudy, Wind, Rain	29.24
26.	29.15 Rain,	29.15
27.	29.12 Rain,	29.2
28.	29.28 Rain,	29.23
29.	29.37 Wind, cloudy, Rain.	

1735. *Feb.*	22.	29.43
	23.	28.82
	24.	28.9
	25.	28.76 Great Rain.

Just after hot or sultry Weather, the Mercury generally falls.
See 16 *Sept.* 1731.
8 *Aug.* 1734.

After the *Aurora Borealis*, there generally follow high Winds.

27 *Oct.* 1733. a large *Aurora Borealis*, and the 28th, 29th and 30th high Winds.
See 23 *Jan.* 1734.

The Mercury falling pretty much, and neither Wind nor Rain succeeded.
1733, from the 18th to the 21st it fell 41, and no Wind or Rain at all till the 25th.

Sultry Weather generally makes the Mercury fall soon after.
1734. *Aug.* 8.

After a great Storm the Mercury rises very fast.
1734. *Aug.* 11.
1736. *Feb.* 6.

Before great Winds the Mercury falls very soon.
1734. *Aug.* 26.
1736. *Feb.* 8.

The Mercury below 28 Inches.
1734. *Dec.* 15. at 27.9
1735. *Jan.* 8. 27.9

In Winter, before Frosts, the Mercury generally rises pretty fast.
1735. *Dec.* 12.

Before a Thaw the Mercury falls.
1735. *Dec.* 13.
17.
1736. *Feb.* 9.

The Mercury falls suddenly before a great Snow.
1731. *Jan.* 4.
1736. *Feb.* 8.
21.

When the Mercury falls for high Winds, and it continues to fall when that Wind is come, it is likely to be tempestuous, or continue some time, unless Rain succeeds.

1736.	22. *Nov.*	29.62	fair, warm	29.62	
	23.	29.49	windy, warm	29.32	Wind.
	24.	29.1	high Wind	28.88	28.73 Stormy.

Some of these Collections are quite contradictory to any settled Rules, and such will happen, and others confirm them; but I have collected so very few of a Sort, though the Diary furnishes a great many, that till more are in this manner collected, it will be very doubtful to form any

Rules

Rules from them: As Opportunity gives leave, I intend to collect many more.

The Description and Manner of using an Instrument for measuring the Degrees of the Expansion of Metals by Heat. By Mr John Ellicott. No. 443. p. 297. Oct. 1736.

Fig. 22.

X. A A is a flat Plate of Brass, which, for farther Strength, is screwed down to a thick Piece of Mahogany: Upon this Plate are screwed 3 Pieces of Brass, 2 of which, marked B B, serve as Supports for the flat Iron Bar C; and which, on account of it's Use, I shall call the *Standard Bar.* The upper Part of the third Piece of Brass is a Circle about 3 Inches Diameter, divided into 360 equal Parts or Degrees: Within this Circle is a moveable Plate, divided likewise into 360 Parts, and a small Steel Index. The Brass Circle is marked D, and the moveable Plate *d.* Upon the Standard Bar the Bar of Metal is laid, on which the Experiment is to be made, as E.

F is a Leaver 2 $\frac{1}{2}$ Inches in length, fastened to an Axis, which turns in 2 Pieces of Brass screwed to one of the Supports marked B: To the End of this Leaver is fastened a Chain, or Silk Line, which, after being wound round a small Cylinder, to which the Index in the Brass Circle D is fastened, passes over a Pulley, and has a Weight hung to the End of it: Upon the Axis, to which the Leaver is fixed, is a Pulley, $\frac{1}{4}$ of an Inch Diameter, to which a Piece of Watch-chain is fastened; the other End of this Chain is hooked to a strong Spring, marked G, which Spring bears against one End of the Metal E.

H is a Leaver exactly of the same Form and Dimensions with the former; but the Chain fastened to the Pulley on it's Axis is hooked to the Standard Bar*. The Line fastened to the End of this Leaver, after being wound round a Cylinder, to which the moveable Plate is fixed, passes over a small Pulley, and has a Weight hung to the End of it; or rather the same Line passing under a Pulley, to which the Weight is hung, has it's other End fastened to the Leaver F: Thus one Weight serves for both Leavers, as in the *Figure.*

From this Description it is plain, that whenever the Bar E is lengthened, it gives Liberty to the Weight to draw the Leaver F upwards by it's Action on the Spring G; and the Index will, at the same time, by Means of the Silk Line, be carried forward in the Circle; and as the Bar shortens, it will return back again; the same Motion will be communicated to the Standard Bar.

The Lengthening the Bar the $\frac{1}{20}$ of an Inch, will carry the Index once round the Brass Circle, which is divided into 360 Degrees; therefore, if the Metal lengthens the 7200th Part of an Inch, the Index will move one Degree.

To make an Experiment with this Instrument, lay a Bar of any kind of Metal, as E, on the Standard Bar; then heat the Standard Bar to any Degree of Heat with a Lamp, and mark the Degree of it's Expansion as marked by the moveable Plate: Observe also the Degree of Ex-

* *N. B.* The Chain to the former Pulley being fastened to a Spring, and not directly to the Metal E, is only for the more easy shifting the Metals.

pansion

panſion of the Metal E, by the Heat communicated to it from the Standard Bar, as marked on the Braſs Circle by the Index: Let the Inſtrument ſtand, till the whole is thoroughly cold; then removing the Bar E, lay a Bar of any other Metal in it's Place, and heat the Standard Bar to the ſame Degree of Heat as before, which is ſeen by the moveable Plates marking the ſame Degree of Expanſion. Then the Index will ſhew the Degree of Expanſion of the ſecond Metal, as it did of the firſt; and, by this Means, the Degrees of Expanſion of different Metals by the ſame Degree of Heat, may be exactly eſtimated.

An Experiment concerning the nitrous Particles in the Air; by the late Rev. John Clayton *D. D.* No. 452. p. 62. Jan. &c. 1739.

XI. I took a ſmall Gally-pot, ſuch as the Apothecaries in the North of *England* make uſe of, where I was when I made this Experiment, and ground the Top of it very ſmooth and true, and adapted thereto a Cover of blue Slate, which I had likewiſe ground with much Care. Into this Gally-pot I put equal Quantities of Nitre and Flour of Sulphur, about a Dram of each. I then fixed on the Cover, putting it into a new Digeſter; but the Height which I raiſed the Heat to, and how long I continued it, I do not exactly remember, but believe it was three or four Seconds. When I opened it the Day following, I perceived ſomething had tranſpired betwixt the Top of the Gally-pot and the Cover; the top Edges of the Gally-pot, where the Glazing was ground off, being diſcoloured, though the Nitre and Sulphur were very little diminiſhed as to their Weight; only they were melted into one Lump, which I took out of the Gally-pot.

And having ſet the empty Gally-pot upon a Shelf, upon looking at it the next Day, I found long hoary Hairs, very bright and brittle, all around the ground Edges of the Pot, very ſpecious to behold. After I had admired them a while, I gathered them, and, taſting them, found them to be pure Nitre. I then ſet the Pot upon the Shelf again, and in 3 or 4 Days, ſtill finding there were freſh Shoots made, as large and ſpecious as at the firſt, I gathered them a ſecond and third time; ſo that I ſuppoſe the Pot would have continued to have ſhot freſh Nitre much longer, if I had not had urgent Uſe for it, to make other Experiments in. However, it is to be obſerved, that I had already gathered more Nitre than I put into the Pot at firſt; though, as I ſaid before, for what I could perceive, I had taken all or near all the Nitre that I firſt put in together with the Sulphur, out of the Pot in a Lump. Hence we may have ſome Conceptions of the Nature of mineral Earths, and how they grow and increaſe, when once impregnated with the Seeds of a Mineral. This likewiſe is a Proof of the Quantity of nitrous Particles with which the Air abounds, ſince the large Quantity of Nitre which I collected out of the Pot, when left empty upon the Shelf, could be ſupplied by the Air only.

An Experiment to prove, that Water, when agitated by Fire, is in-

XII. Sir *Thomas Proly* having heard of a new Digeſter, which I contrived, had a Deſire to ſee it, and ſome Experiments made therein. I had a ſmall one, which I deſigned only for an inward Cylinder; this I could eaſily put in my Pocket: Wherefore, going to pay him a Viſit at *Elton* *finitely more elastic than Air in the same Circumstances. By the same.* No. 454. p. 162.

Elton in *Huntingdonshire*, I took it along with me; and having softened a Bone therein in a very short Space, he was desirous to know the shortest Time it was possible to soften a Bone in: I told him, I thought I could soften the Marrow-bone of an Ox in a very few Minutes, but that that Vessel was very weak, and I feared would not endure the Pressure of so violent a Heat; yet seeming desirous to have the Experiment tried, I said I was ready to venture my Vessel: Then having fixed all things right, and included about a Pint of Water, and, I believe, about ℥ij of a Marrow-bone, we placed the Vessel horizontally betwixt the Bars of the Iron Grate into the Fire about half way; and in three Minutes time I found it raised to a great Heat; whereupon I had a mind to have taken it out of the Fire, least it should have burst; telling Sir *Thomas* of the Danger that I apprehended: For I remembered, that the Screws of a Digester, made after Mr *Papine*'s Method, giving way, the Head flew one way and the Screwe and Irons another, with such Violence, that the Head, having hit against a Brick, cut a Piece clearly out of it; which was one Reason and Motive to my contriving a Digester this way, that the Screws cannot possibly start, but that the Vessel would sooner break in any other Part: But in this (I added) I thought the Bottom would first burst, it being only soldered in. Scarce had I done speaking, and Sir *Thomas* thereupon moved his Chair to avoid Danger; but seeing the Heat become more raging, I stepped to the Side-table for the Iron wherewith I managed the Digester, in order to take it out of the Fire, when, on a sudden, it burst as if a Musquet had gone off. A Maid that was gone a milking, heard it at a considerable Distance; the Servants said it shook the House. As I had foretold, the Bottom of the Vessel, that was in the Fire, gave way; the Blast of the expanded Water blew all the Coals out of the Fire all over the Room; for the Back of the Fire-range was made just like an Oven, so that circulating therein, it brought forth all the Coals at the Mouth thereof. All the Vessel together flew in a direct Line cross the Room, and hitting the Leaf on a Table made of an inch Oak plank, broke it all in Pieces, and rebounded half way of the Room back again. What surprised me in this Event, was, that the Noise it made at it's bursting was by no means like the successive evaporating of an Æolipile, but like the firing off of Gunpowder. Nor could I perceive any where in the Room the least Sign of Water, though I looked carefully for it, and, as I said before, I had put a Pint into the Digester, save only that the Fire was quite extinguished, and every Coal belonging to it was black in an Instant.

But to confirm the Elasticity of Water, or to shew, at least, that there is a much stronger elastic Force in Water and Air, when jointly included in a Vessel, than when Air alone is inclosed therein, I made the following Experiment: I took two ℥vj Phials, into the one I put about ℥v of Water, or better, and so corked it as well as I possibly could; the other I corked in the same Manner, without putting any thing into it. I inclosed them both in my new Digester, ⅔ being filled with Water; when

when the Heat was raiſed to about five Seconds, I heard a conſiderable Exploſion, and a jingling of Glaſs within the Veſſel, and ſhortly after another Exploſion, but not ſo loud as the former; whence I concluded, that both the Phials were broken. I then let the Digeſter cool leiſurely, and the next Day I opened it; both the Corks were ſwimming on the Top of the Water, but only one of the Phials was broken, *viz.* that one into which I had not put any Water. At firſt, indeed, I concluded, that the Preſſure or Dilatation of the Air in the empty Phial being ſtronger than the ambient Preſſure, forced forth the Cork, whereupon the Water, ruſhing in with Violence, might break the Phial; and therefore that this was the Cauſe alſo of the Loudneſs of the Exploſion; whereas the other being moſtly filled with Water, there being but a ſmall Quantity of Air therein, juſt enough to ſorce out the Cork, the Phial was not broken, but was preſerved by the Force of the Water incloſed therein. But I have had Reaſon ſince to change my Opinion; for having had very ſtrong Phials made, on Purpoſe to make ſome peculiar Experiments therewith, I took one of them, and having filled it about $\frac{5}{6}$ full with Water, and corked it very well, I ſet it in a ſquare Iron Frame, with a Screw to ſcrew down the Cork, and keep it from flying forth. I then put it into a Digeſter, $\frac{4}{5}$ filled with Water; which being heated to a due Height, when I opened it, I found the Cork forced into the Phial, though the Cork was ſo very large, that it amazed ſeveral who ſaw it, to conceive how it was poſſible for ſo large a Cork to be forced into the Bottle. Hence it manifeſtly appears, that the Preſſure in the Digeſter, wherein was proportionately more Water, and leſs Air, was ſtronger than the Preſſure within the Phial, wherein was proportionately more Air, and leſs Water. Then I reaſoned thus alſo of the two former Phials: That the Air in the Phial, wherein was no Water included, making not a proportionate Reſiſtance to the ambient Preſſure in the Digeſter, wherein was a conſiderable Quantity of Water, the Cork was forced inward with ſuch Violence, that it, together with the Water, daſhed the Phial in pieces; but that in the other Phial, wherein there were $\frac{5}{6}$ of Water, the inward Preſſure in the Phial being greater than the ambient Preſſure in the Digeſter, wherein were but $\frac{4}{5}$ of Water, the Cork was thereby forced outward; and that the ſmall Difference between the proportionate Quantity of Water and Air in the Phial and in the Digeſter, being only as $\frac{4}{5}$ to $\frac{5}{6}$, was the Reaſon not only why the Bottle was not broken, but alſo of the Faintneſs of the Exploſion.

The Conſtruction of a Quickſilver Thermometer, by Mr Joſ. Nic. De l'Iſle, *F. R. S. Dated* Peterſburg, $\frac{4}{15}$ Feb. 1732-3, *&c.*

XIII. In order to have more ſure Grounds for my Experiments of Natural Philoſophy in this Country, and that they might be compared with thoſe of other Countries, I applied myſelf this Winter to the Conſtruction of Thermometers of Mercury, regulated by the Expanſion of that Fluid proportionably to it's Bulk. This Expanſion is indeed not very perceptible, conſidering that Dr *Halley* in the Experiments made by him upon it above 40 Years ago*, found that the ſaid Expanſion, by

* See Vol. II. Chap. I. §. xv.

the

taining feveral Literary Communications. Tranflated from the French *by* Phil. Henry Zollman. *F. R. S.* No. 441. p. 221.

the Heat of boiling Water, was no more than $\frac{1}{74}$ Part of the Bulk of Mercury, the Experiment having been tried in the Months of *February* and *March*, when the Weather was cold enough, though it did not freeze.

M. *Amontons*, a Member of the Academy of Sciences at *Paris*, alfo relates in the Memoirs of that Academy of the Year 1704, that this Expanfion of the Mercury, is but $\frac{1}{115}$ Part of it's Bulk from the greateft Heat to the greateft Cold that is felt at *Paris*. For my own part I found in the great Cold, we had here this Winter on the $\frac{16}{27}$ *January* laft in the Morning, that the Bulk of the Mercury was condenfed almoft $\frac{1}{50}$ Part of the Extent it had in boiling Water. The Cold we had that Day, the Wind being at Eaft, was one of the fevereft that ever was felt here. I fhall give you a more exact Account of it hereafter, when I have compared my new Thermometers with thofe ordinary Ones I made ufe of for thefe four or five Years paft. My new Thermometers of Mercury I had made of a good large Size, and in fuch Manner that, having divided in each the whole Quantity of Mercury it contains into 100,000 Parts; and having marked the Extent of the Bulk of that Mercury in boiling Water, I can at any time fee on the Divifions of thefe Thermometers, by how many Parts the Bulk of the Mercury is condenfed through the prefent Temperature of the Air. And though I have made four of thefe Thermometers, which differ very much as to their Size, and the Quantity of Mercury they contain, yet they agree within a very few of thefe Parts. As pure Mercury is of the fame Nature every where, nor is liable to any Alteration from being inclofed in a Tube; and as it is probable, that taking it equally purified, it will in different Countries be fubject to the fame Expanfion, if expofed to the fame Degree of Heat; for this Reafon I am perfuaded thefe Thermometers may very well ferve to compare the Temperature of different Countries; the rather, as I found by Experience, that thefe Thermometers may be rendered fit enough to mark fenfibly the Increafe, or Diminution of the Bulk of the Mercury, within one or two Parts out of the 100,000 continued in the whole Bulk. This Sort of Thermometers has alfo this advantage, that as they mark the proper Expanfion of the Mercury in each Temperature of Air, they may ferve to fhew every Moment the Correction that is to be made in the Height of the Mercury in fimple Barometers; which will ferve for reducing them to the Height they would have in an equal Temperature of Air: And one might, for this End, chufe and agree upon the Heat of boiling Water, as a fixed Term, which, in all appearance, will be the fame all over the World. If the Royal Society fhould approve this new Conftruction of Thermometers, and fhould order fome of their Members to make the like, we might hereafter be able exactly to compare the Temperature of *England* with that of this Country, and other Places where the like Thermometers fhould be made. In order to reap this Advantage from my Experiments, I fhall communicate to the *Royal Society* all the Obfervations I have made here for thefe four or five Years

paft,

To fold in between pages 468 and 469.

Observations of the Variations of the Needle *and* Weather, *made in a Voyage to* Hudson's-Bay 1731, *by Capt.* Christopher Middleton.

No. 429. p. 127. *July, &c.* 1733.

Months and Days.	Hours.	Barom. Alt.	Therm. Alt.	Lat. p. Davis or Acc.	Lat. per Elton.	Obs.	Long.	Need. Variat.	Obs.	Winds.	Weather.
1731	9									E N E	
June 14	12	30	26	59.17			5°.52	17.30		S E	Fair and Close.
	9									S E b E	Ditto.
15	9	32	25½								Fair and Close.
	12	31	25½		59 24	Obs.	8°.58	18		E S E	Ditto.
	9	32	24¾							Dit.	Ditto.
16	9	32	24¾							E S E	Ditto.
	12	32	24.3	59.30			11.50	19		Dit.	Ditto.
	9	32	24½							Dit.	Ditto.
17	9	31	24							S E b S	Ditto.
	12	31	24½	59 35			15.10	20		Dit.	Ditto,——at 2 small Rain.
	9	31½	24							S b W	Ditto.
18	9	31	24							S W	Cloudy with small Mist.
	12	30	24	59.29			18.45	21		S W b S	Foggy, with small Rain.
	9	29	23							N N W	Fair and clear, but cloudy.
19	9	31	25							Dit.	Clear, with Clouds.
	12	31	25	58.54	58.52	Obs.	21 15	21.30		Dit.	Hazy, with small Rain.
	6	30	24							N b E	Fair and Clear.
20	9	30	24	Acc.						S E	Clear, with small Rain.
	12	30	24¼	58 47	58.55	Obs.	24 11	22		Dit.	Fair and cloudy.
	9	29	24½							S E b S	Foggy, fresh Gales and Rain.
21	9	27	24½							S E	Fresh Gales, with much Rain.
	12	26	24	58.47			29.13	24		Dit.	Ditto.
	9	25	24							Dit.	Little Wind and foggy.
22	9	25	24	Acc.						E S E	Ditto,——with small Rain.
	12	24	23½	58.42	58 47	Obs.	32 54	26	Obs.	S E b E	Foggy.
	9	24	23½							E S E	Ditto.
23	9	26½	25							N E b E	Fresh Gales, hazy and misty.
	12	28	25	58.38			36.55	26		E N E	Ditto.
	9	30	26½							Dit.	Fresh Gales, with Clouds.
24	9	30	26¼							E S E	Fair, with little Winds.
	12	29	25½	58.37		Obs.	40.56	27 30		Dit.	Ditto.
	9	28	25½							Dit.	Ditto.
25	9	28	25¼							E b S	Light Winds and fair Weather.
	12	27	25	58 32		Obs.	43	30	Obs.	Dit.	Fair and clear.
	9	28	25½							N N W	Cloudy.
26	9	32	28							W N W	Close.
	12	34	29	57.39			43.17	29		W b N	Hazy.
	9	31	27							S W	Fresh Gales and hazy
27	9	33	28							N W	Foggy.
	12	33	28	58 6			45.10	30		N W b N	Thick and foggy.
	9	31	27							S E	Hazy, with small Rain.
28	9	29	28							S S E	Foggy, with much Rain.
	12	29	29	58.18			47.16	31		S W	Hazy, with small Rain.
	9	32½	29½							W S W	Fresh Gales, with Fogs.
29	9	33	29½							W S W	Little Wind, and hazy.
	12	33	29	58.29			47.41	31		S W	Fair and clear; some Rain.
	9	32½	28½							S S W	Foggy and wet.
30	9	30	28½							S b W	Squally
	12	29	28¾	58.53			51.16	33		Dit.	Fresh Gales, and hazy.
	9	28	29½							S W	Fresh Gales, Fogs, and Rain.
July 1	9	27½	30½							W b S	☿ 2½ Deg. above chang. stormy.
	12	31	30½	59.49			54.56	34		West	—fell 1 Deg. below chang. mod.
	6	32	30½								—fell. Small Rain, and Hazy.
2	9	32	30							Calm	Calm. Somewhat hazy.
	12	32	28½	60.16	60.16	Obs.	55.4	34		Calm	—at a Stand, fair and calm.
	9	32½	29							Dit.	—somew. fall. light, and clear.
3	9	33	30							North	Close and grey Weather.
	12	33½	31	60.08			56.22	34		N b E	—continues to fall, clear and cold.
	9	33	30½							North	—rising a little, fair and clear.
4	9	31	29½							N N W	—rising, fair with flying Clouds.
	12	30	29	59 33	59 33	Obs.	58.10	37	Obs.	N W	—rising, little Wind, & small Rain.
	9	29	28½							North	Close, little Winds & flying Clouds.
5	9	30	29							N W b N	Light Air, and variable,
	12	31	28½	59 33			58.28	37		Calm	—fallen, fair and clear.
	9	31	28½							Dit.	—at a stand, fair Weather.
6	9	31	28½							N N W	—at a stand, close and little Winds.
	12	31	29	60.2			58 40	37		N N W	—at a stand, little Winds.
	9	31	29							N E	—at a stand, close, and cloudy.
7	9	31½	30½							N N E	—some. fall. clear white Clouds.
	12	31	30	60.9			60 22	37.40		Calm	Close and grey Weather.
	9	30½	29						Obs.	S W	—rising, little Wind and hazy.
8	9	31	29½							S E	Close and foggy.
	12	31	30	60 29	60.30	Obs.	61 23	38		N E	Ditto.
	9	31	29							W b N	Ditto.
9	9	34	34							S W b S	—fallen 4 Deg. clear and cold.
	12	34	33½	61.16	61.19	Obs.	63.57	38	Obs.	Dit.	Fair, clear, much Ice in sight.
	9	33	32								—rising, fair and clear.
10	9	32	32	Acc.						E S E	—rising, fair and clear.
	12	32	32	61.35	61 25	Obs.	65.17	40		Dit.	—at a stand, fair and clear.
	9	32	32								—stands, fresh Gale.
11	9	34	33½	Acc.						S E b S	—fallen, fresh Gales with Squalls.
	12	34	33½	62	62 4	Obs.	69 2	41		S E b S	—at a stand, fresh Gales.
	9	34	33							Dit.	—at a stand, hazy.
12	9	37	32½							Dit.	—fallen, fair.
	12	36	30½		62.33	Obs.	71.2	43		W S W	—somewhat rising, fair.
	9	35	30							Dit.	—rising, little Winds, and fair.
13	9	35	29½	Acc.						Dit.	—at a stand, fair with calm.
	12	35	30	62.46	62 40	Obs.	72.6	43		N W	Fair and moderate.
	9	35	30½							N W	Fair and clear.
14	9	35	32							Dit.	—at a stand, fair and clear.
	12	35	32	62 30		Obs.	73 33	41		North	—at a stand, fair and clear.
	9	35	32							N W b N	Fair, serene Weather.
15	9	34½	32	Acc.						South	—somewhat fallen, foggy.
	12	35	33½	63.13	63.14	Obs.	75.9	41		S b E	Very foggy.
	9	35	33½							S E	—at a stand, foggy.
16	9	35	33	Acc.						N W	—at a stand, fair and clear.
	12	35	32	63.21	62 58	Obs.	77.3	42		N N W	Fair, serene Weather.
	9	35½	31½							Dit.	—falling, clear.
17	9	34½	31							N W	—rising, clear.
	12	33½	30½	63.17			78.29	41		Dit.	—continues rising, clear.
	9	33	30½							North	—rising, serene Weather.
18	9	33½	31	Acc.						N W	Foggy.
	12	34	31½	63.9	63.6	Obs.	79.53	40		Dit.	—a little fallen, thick Fog.
	9	34	31½							South	—at a stand, hazy.
19	9	33½	31½							East	—rising, fresh Gale.
	12	33	30½	62.14		Obs.	80 44			E b N	—continues rising, fair and clear.
	9	30½	29							Dit.	—continues rising, serene.
20	9	28	27							West	—rising, grey close Weather.
	12	28	26½	61.18			81.26	37		Dit.	—stands, fresh Gales and foggy.
	9	30	29							N W	—fallen, hazy.
21	9	32	30							North	—falls, fair and clear.
	12	32	29	60 5		Obs.	83.2	34		N W	—stands, fresh Gales, and fair.
	9	32	29½							Dit.	—at a stand, squally.
22	9	34	31							N W	—fallen, moderate and fair.
	12	34	31	58.4		Obs.	84.20	30		West	—at a stand, fair.
	9	33	31							W S W	—rises, dark and cloudy.
23	9	32	31							W b N	—rising, dirty and Rain.
	12	32½	31½	57.35			84.20	29		W b S	—stands, foggy, and small Rain.
	9	33	32							N W	—fallen, fresh breeze and cold
24	9	37	34	Acc.						N W	—fallen 4 Deg. is close white Ice
	12	37	33½	56.13	56.20	Obs.	85.27	25		West	—stands, foggy and much Ice.
	9	36½	32½							Calm	—rising, serene.

Months and Days.	Hours.	Barom. Alt.	Therm. Alt.	Lat. p. Davis or Acc.	Lat. per Elton.	Obs.	Long.	Variat.	Obs.	Winds.	Weather.
1731	9	36	32	Acc.						S S E	☿ rising, hazy.
July 25	12	35	31½	56.24	56.24	Obs.	85.27	24		East	—continues rising, fair.
	9	35	32½	Lat p.		p. El				S E b E	—stands, hazy, and cold Air.
	9	35	33½	Acct.		ton.				S S E	—somewhat fallen, hazy, & cold.
26	12	36	33½	55.39		55.33	85.40	24		N b E	—falling, Rain, and fresh Gales.
	9	36	34							N N W	—stands, thick and rainy.
27	9	37	35							E N E	—continues falling, fair and cold.
	12	37	34½	54.23			85.48	24		E b N	—stands, cold, and much Ice.
	9	37	35							N b E	—as above, fresh Gale, cold.
28	9	37	34½							N b W	—as above, moderate and hazy.
	12	37	34½	53.57				24		N N W	—stands, moderate and fair.
	9	35	30							S W	—rising, fair pleasant Weather.
29	9	35	30½							S W b W	—at a stand, cloudy with Rain.
	12	34	31	53.29				25		S W	—rising. Thunder and Rain.
	9	33	31								—rising. fresh Gales with Rain.
30	9	37	32							N N W	—much fallen, moderate.
	12	36	29½								—rising, fair and Moderate.
	9	35	29½								—continues rising, fair and settled.
31	9	31	27								—rising, squally, with Rain.
	12	31	27								—as above, fair and moderate.
	9	29	25								—rising quick, fair.
August 1	9	26	22								—continues rising, warm & fair.
August 20	9	30	35							W N W	Hazy and moderate.
	12	33	27	52.40		52.42			Obs	N N W	☿ stands, hazy
	9	35	30		p. Da					Dit.	—stands, fresh Gales.
21	9	36½	30		vis.				Obs.	North	—somewhat fallen, fair.
	12	37	31	52.54	52.55					North	—fallen.
	9	37	30½							Dit.	—falls, clear, serene, and cold.
22	9	36	28							S W b W	—rises, fair light Breezes.
	12	36	29	52.57	52.59				Obs.	Dit.	—stands, clear, and calm,
	9	35	29								—somewhat risen, calm.
23	9	34½	29							S S E	—rises, hazy.
	12	34	29	53.54						Dit.	—continues rising, fresh Gales.
	9	33	30							S b W	—rising, hazy, drops of Rain.
24	9	32	28½							S E	Fair and moderate.
	12	32	29	55.16				25		S E	Hazy.
	9	33	30							N E	Hazy and close.
25	9	34	30½							N N E	☿ falls, foggy, and cold.
	12	35	31	55 48				26		Dit.	—continues fallen, cold.
	9	34	31							Dit	—little rising.
26	9	36	33							E S E	—fallen, moderate, and fair.
	12	36	33	56.55	56.56	56 56		28	Obs.	Dit.	—stands, fresh Gales.
	9	37	34							E b N	Hard Gales.
27	9	40	34							E N E	—fallen, fresh Gales.
	12	40	34	58.14	58.13			29	Obs.	Dit.	Hard Gales.
	9	40	34							E N E	—stands, moderate.
28	9	38	33							N N E	—somewhat risen, fair.
	12	38	32	58.35	58.41			30	Obs.	North	—stands, fair and moderate.
	9	38	32							North	Fair and calm.
29	9	37	31							Dit.	Fair and clear.
	12	37	30	59.24	59.33			31	Obs.	N b W	☿ little risen, fair.
	9	36	30							West	—stands, fair Weather.
30	9	36	31							Calm	—stands, variable.
	12	37	30	59.55	60.5			31	Obs.	E S E	—somewhat fallen.
	9	37	30							E b N	—stands, squally.
31	9	39	35							E N E	—fallen, fresh Gales.
	12	40	37	59 55				32		Dit.	—cont. fallen, fr. Gales & cloudy.
	9	40	37							Dit.	—stands, squally.
Sept. 1	9	40	36							N E	—stands, great Sea.
	12	40	36½	59 25				31		Dit.	
	9	41	36								
2	9	42	36½							E N E	—fallen, flying Clouds.
	12	43	36½	59.43	59.36			32	Obs.	Dit.	—continues falling, moderate.
	9	42	34							N N W	Moderate.
3	9	41	35							S W	—rising, fresh Gales.
	12	40	35½	60.40				33		Dit.	—continues rising, hard Gales.
	9	35	35							Dit.	—rising fast, very hard Gales.
4	9	32½	32½							Dit.	—still rising, blowing very hard.
	12	31	33	61.33				35		Dit.	—continues rising, foggy.
	9	30	33							W S W	Thick Fog.
5	9	34	33							N b E	☿ fallen, fresh Gales.
	12	37	35	62.12				38		Dit.	Fresh Gales.
	9	38	35½							Dit.	—continues fallen, moderate.
6	9	42	37							N E b N	—stands, hazy, Snow,
	12	42	36½	62.37		62.49		42	Obs.	Dit.	—stands, thick Weather.
	9	42	36½							S E	—the same.
7	9	42	37								Cont. hazy, white Ledges of Ice.
	12	42	37	63.10		63.21		42	Obs.	S E	Ditto.
	9	42	36½							Dit.	The same, Fogs and freezing.
8	9	41	36							E S E	☿ somewhat risen, foggy.
	12	41	35½	63.25				41		Dit.	—stands.
	9	41	35½							Dit.	—the same, fair.
9	9	41	35							Calm	—the same.
	12	40	34½	63.19		63.13		40	Obs.	N W b W	—somewhat risen, fair.
	9	41	35½							Dit.	—fallen, fair.
10	9	42	37							N b W	—continues fallen, fair.
	12	42	38	62.48	62.30			41	Obs.	Dit.	—the same
	9	43	38							Dit.	—fallen, fair.
11	9	44	38							Dit.	—fallen, fair.
	12	45	38½	61.43				40		Dit.	—continues fallen, fair and cold
	9	45	38							North	—at a stand, fair.
12	9	44	37½							W N W	—risen, fair.
	12	42	36½	61.31	61.29			40	Obs.	West	—risen, hazy weather, much Ice.
	9	41	36							Dit.	—rising, fair and clear.
13	9	40½	36							N W	Fair.
	12	40	36	61.16	61.5		60.18	38	Obs.	Dit.	Fair and moderate.
	9	40	36								—the same, cloudy.
14	9	40	35½							N W	—the same, fair.
	12	39	35	60.36			57.12	38		Dit.	Fair and cloudy.
	9	38	35							N W b N	—rising, hazy, with Snow.
15	9	37	35½							N N E	—risen, foggy.
	12	37½	30	59 41	59 28		52.29	35	Obs.	Dit.	Fair.
	9	37½	35							N W	
16	9	37½	34½							West	—stands, cloudy.
	12	37½	33½	58.20	58.10		47.18	33	Obs.	N W b W	—the same, cloudy.
	9	37½	33½								—stands, variable.
17	9	37	33							N N W	—the same, cloudy.
	12	37½	33½	57.15			43.44	29		Dit.	—somewhat fallen, cloudy.
	9	37½	33							Dit.	Calm.
18	9	27	28½							S W	—rising quick, fresh Gales.
	12	26	28½	56.35	56.25		42.32	27	Obs.	Dit.	—little rising, cloudy.
	9	26	28½							West	—stands, squally, with Rain.
19	9	34	29							N W	Clear, with fresh Gales.
	12	35	30	55.23	55.29		38.48	25	Obs	Dit.	—still falling, fair.
	9	35	31							Dit.	—stands, fair, squally.
20	9	34	30½							W N W	—rising, squally with Hail.
	12	33½	31	54.23			34.19	22		Dit.	Hard Squalls.
	9	33½	31								—at a stand, hard Squalls.
21	9	33	30							N W	Squally with fresh Gales
	12	33	29½	53.9			29.51	20		Dit.	—stands, clear fresh Gales.
	9	34	29							Dit.	—somewhat fallen.

Months and Days.	Hours.	Barom. Alt.	Therm. Alt.	Lat. Acct.	Lat p. *Davis*	Lat. *per Elton.*	Long.	Ned. Variat.	Obs.	Winds.	Weather.
1731	9	34	28½							N b W	☿ ſtands, fair.
Sept. 22	12	35	28	52.7	52.14		26 25	19	Obſ.	N W b W	—fallen, clear.
	9	30	25							S W	—riſing quick, cloudy.
23	9	28	25							S b E	—ſtill riſing, hard Gales.
	12	24	24							Dit.	—riſing quick, thick and dirty.
	9	21	21								—continues riſing.
24	9	22	22							Eaſt	—ſtands, cloudy and Rain.
	12	22½	22½	52.30			23 12	17		Dit.	Ditto, freſh Gales.
	9	22½	22½							Dit.	—at a ſtand, cloudy.
25	9	21½	22							South	—riſing, ſqually and Rain.
	12	21½	22	51.50	51.54		20.24	16	Obſ.	Dit.	—the ſame, freſh Gales.
	9	21	22							W S W	—the ſame, ſqually with Rain.
26	9	21	21½							S W	Fair, freſh Breeze.
	12	21	21½	50.38	50.46		17.12	16	Obſ.	S b W	Fair and clear.
	9	21	21							Dit.	—the ſame, freſh Gales.
27	9	22	21							Weſt	—little fallen, fair and cloudy.
	12	23	20½	49.28	49.28		13.26	15	Obſ.	Dit.	Fair.
	9	22	20							S W	A great Sea, ſqually.

Months and Days.	Hours.	Barom. Alt.	Therm. Alt.	Lat. Acct.	Lat p. *Davis*	Lat. *p. Elton*	Long	Variat.	Obſ.	Winds.	Weather.
1731	9	20	19							S W	☿ riſing, freſh Gales and Rain.
Sept. 28	12	19	18	49 15	49.2		9.21	14	Obſ.	Dit.	—continues riſing, freſh Gales.
	9	21	18½							W S W	—little fallen, fair Weather.
29	9	23	18½							S b W	—fallen, hazy, freſh Gales.
	12	22	18	49.32			5.18	14		Dit.	
	9	22	18							S W b S	—at a ſtand, freſh Gales.
30	9	22	18							Dit.	—the ſame, hazy.
	12	22	18	49 18	49.30			14	Obſ.	S W	—at a ſtand, hazy
	9	22	18								—continues the ſame, ſqually.
Oct. 1	9	24	17							Weſt	—fallen, fair and clear.
	12	26	19	50. 2				14		W S W	—continues fallen, fair.
	9	28	21							Dit.	—fallen, fair and clear.
2	9	29	21							Dit.	
	12	29	21							Dit.	—continues, pleaſant.
	9	30	22							Dit.	—the ſame, fair and clear.

January 28, 1731-2.

THESE will inform you of my Sentiments concerning Mr *Patrick*'s Marine-Barometer, which I have made uſe of for two Voyages to *Hudſon*'s-*Bay* in *North America*, and by the ſtricteſt Obſervations, I have always found it to give me timely Notice of all bad Weather, and likewiſe of veerable Winds; as alſo, certain Intelligence of our coming nigh any Ice, with the Quantity we had to go through. It is an Inſtrument of excellent Uſe, I having continually found myſelf obliged to conform to it's more certain Information before all other ocular Appearances in the Horizon whatever. I muſt likewiſe inform you, that when we come in or near Ice we are obliged to keep one of our Compaſſes continually moving, there being either ſome magnetick Particles in the Air, or ſome other Quality that hinders them from traverſing, which makes our Courſe very difficult to direct; this happens generally in our entring *Hudſon*'s-*Streights* and *Bay*, but never ſo without being near or amongſt Ice. I have enquired of the Commanders, and others that uſe *Greenland* and *Davis*'s-*Streights*, and find great Complaints from them of their Compaſſes not traverſing. I have tried the Needle of the Azimuth Compaſs without the Chart, and find it to traverſe much better, ſo that I deſign next Voyage to have Iſing-Glaſs Charts, as being lighter.

paſt, on the Barometer and Thermometer, as ſoon as I ſhall have adapted them to the Effect which I juſt now ſaid that Heat and Cold produce upon the Mercury. I am informed, that four or five Years ago, the *Royal Society* ſent to M. *Abraham Vater* at *Wittenberg*, large Thermometers of Spirit of Wine, made and regulated by an Inſtrument-Maker of the *Royal Society*, in order to compare the Obſervations to be made in *Germany*, by the Means of thoſe Thermometers, with the Obſervations made in *England* by the like Thermometers, the one being regulated by the others. M. *Weidler*, Profeſſor of Mathematicks at *Wittenberg*, mentions in the Account which he gave of his Meteorological Obſervations for the Year 1729, that he has furniſhed himſelf with one, which he intends to make uſe of hereafter for his Meteorological Obſervations. He alſo ſays, that the Obſervers of the *Royal Society* of *Berlin*, make uſe of a like Thermometer; and I have myſelf received from thence, Obſervations on the Heights of the Thermometer of Spirit of Wine, made probably with that Inſtrument, for the whole Year 1729, and for the firſt three Months of 1731. Thoſe Obſervations are engraven on Copper-Plates, where the Heights of the Spirit of Wine are expreſſed in Parts of the *French*, *Engliſh*, and *Rhinland* Foot. If the *Royal Society* approve of this Sort of Thermometers, and are deſirous I ſhould compare them with mine; if they alſo deſire that Meteorological Obſervations with thoſe Thermometers of Spirit of Wine ſhould be made in *Ruſſia*, I beg you would ſend me ſeveral of them; but then I beg that thoſe you ſend me, be well regulated, and exactly compared with thoſe the Obſervers of the *Royal Socie y* make uſe of; ſuppoſing that ſome Perſon or other of their Body is appointed to keep Journals of theſe Obſervations. I ſhall ſend in exchange to the *Royal Society*, if they deſire it, ſome Thermometers of Mercury regulated by and compared with the four large ones which I made here.

An Obſervation of extraordinary Warmth of the Air in Jan. 1741-2. *by the Rev. Mr* H. Miles, No. 462. p. 20. *Read* Jan. 21. 1741-2.

XIV. My Mercurial Thermometer abroad, was laſt night, at 10 o'Clock, 20 Degrees above the freezing Point; which is higher than it was ſixteen Mornings of the one-and-thirty in *May* laſt, and higher than in any Morning in *April* one excepted. *Tooting, Surry,* 20 *Jan.* 1741-2.

XV. *(See the folded Sheet.)*

The Effects of Cold; together with Obſervations of the Longitude, Latitude, and Declination of the Magnetic Needle, at Prince of

XVI. I Obſerved that the *Hares*, *Rabbets*, *Foxes*, and *Partridges*, in *September*, and the Beginning of *October*, changed their native Colour to a ſnowy White; and that for ſix Months, in the ſevereſt Part of the Winter, I never ſaw any but what were all white, except ſome *Foxes* of a different Sort, which were grizzled, and ſome half red, half white.

That Lakes and ſtanding Waters, which are not above 10 or 12 Feet deep, are frozen to the Ground in Winter, and the Fiſhes therein all periſh.

Yet in Rivers near the Sea, and Lakes of a greater Depth than 10 or 12 Feet, Fiſhes are caught all the Winter, by cutting Holes through the

Wales's Fort, *upon* Churchchill River *in* Hudson's Bay, North America: *by Capt.* Christopher Middleton, *F.R.S. Commander of His* MAJESTY's *Ship* Furnace, 1741-2. No. 465. p. 157. *Read* Oct. 28. 1742.

the Ice down to the Water, and therein putting Lines and Hooks. But if they are to be taken with Nets, they cut several Holes in a strait Line the Length of the Net, and pass the Net, with a Stick fastened to the Head-Line, from Hole to Hole, till it reaches the utmost Extent; and what Fishes come to these Holes for Air, are thereby entangled in the Net; and these Fish, as soon as brought into the open Air, are instantaneously frozen as stiff as Stock-fish. The Seamen likewise freshen their salt Provisions, by cutting a large Hole through the Ice in the Stream or Tide of the River, which they do at the Beginning of the Winter, and keep it open all that Season. In this Hole they put their salt Meat, and the Minute it is immersed under Water, it becomes pliable and soft, though before it's Immersion it was hard frozen.

Beef, *Pork*, *Mutton*, and *Venison*, that are killed at the Beginning of the Winter, are preserved by the Frost, for Six or Seven Months, intirely free from Putrefaction, and prove tolerable good eating. Likewise *Geese*, *Partridges*, and other Fowl, that are killed at the same time, and kept with their Feathers on, and Guts in, require no other Preservative but the Frost to make them good wholsome eating, as long as the Winter continues. All kinds of Fish are preserved in the like manner.

In large Lakes and Rivers, the Ice is sometimes broken by imprisoned Vapours; and the Rocks, Trees, Joists, and Rafters of our Buildings, are burst with a Noise not less terrible than the firing off a great many Guns together. The Rocks which are split by the Frost, are heaved up in great Heaps, leaving large Cavities behind; which I take to be caused by imprisoned watery Vapours, that require more Room, when frozen, than they occupy in their fluid State. Neither do I think it unaccountable, that the Frost should be able to tear up Rocks and Trees, and split the Beams of our Houses, when I consider the great Force and Elasticity thereof. If Beer or Water is left in Mugs, Cans, Bottles, nay in Copper Pots, though they were put by our Bed-sides, in a severe Night they are surely split to pieces before Morning, not being able to withstand the expansive Force of the inclosed Ice.

The Air is filled with innumerable Particles of Ice, very sharp and angular, and plainly preceptible to the naked Eye. I have several times this Winter tried to make Observations of some celestial Bodies, particularly the Emersions of the Satellites of *Jupiter*, with Reflecting and Refracting Telescopes; but the Metals and Glasses, by that Time I could fix them to the Object, were covered a quarter of an Inch thick with Ice, and thereby the Object rendered indistinct, so that it is not without great Difficulties that any Observations can be taken.

Bottles of *strong Beer*, *Brandy*, *strong Brine*, *Spirits of Wine*, set out in the open Air for Three or Four Hours, freeze to solid Ice. I have tried to get the Sun's Refraction here to every Degree above the Horizon, with *Elton*'s Quadrant, but to no purpose, for the Spirits froze almost as soon as brought into open Air.

The

The Froſt is never out of the Ground, how deep we cannot be certain. We have dug down 10 or 12 Feet, and found the Earth hard frozen in the Two Summer Months; and what Moiſture we find five or ſix Feet down, is white like Ice.

The Waters or Rivers near the Sea, where the Current of the Tide flows ſtrong, do not freeze above 9 or 10 Feet deep.

All the Water we uſe for Cooking, Brewing, *&c.* is melted Snow and Ice; no Spring is yet found free from freezing, though dug never ſo deep down. All Waters in-land are frozen faſt by the Beginning of *October*, and continue ſo till the Middle of *May*.

The Walls of the Houſe we live in, are of Stone, 2 Feet thick, the Windows very ſmall, with thick wooden Shutters, which are cloſe ſhut 18 Hours every Day in the Winter. There are Cellars under the Houſe, wherein we put our *Wines*, *Brandy*, *ſtrong Beer*, *Butter*, *Cheeſe*, &c. Four large Fires are made in great Stoves, built on purpoſe, every Day: As ſoon as the Wood is burnt down to a Coal, the Tops of the Chimneys are cloſe ſtopped with an Iron Cover: This keeps the Heat within the Houſe (though at the ſame time the Smoke makes our Heads ake, and is very offenſive and unwholſome); notwithſtanding which, in 4 or 5 Hours after the Fire is out, the Inſide of the Walls of our Houſe and Bed-places will be 2 or 3 Inches thick with Ice, which is every Morning cut away with a Hatchet. Three or Four times a Day we make Iron Shot of 24 Pounds Weight red-hot, and hang them up in the Windows of our Apartments. I have a good Fire in my Room the major Part of the 24 Hours, yet all this will not preſerve my *Beer*, *Wine*, *Ink*, &c. from freezing.

For our Winter Dreſs we make uſe of 3 Pair of Socks of coarſe Blanketting or Duffield for the Feet, with a Pair of *Deer-Skin* Shoes over them; two Pair of thick *Engliſh* Stockings, and a Pair of Cloth Stockings upon them; Breeches lined with Flannel; two or three *Engliſh* Jackets, and a Fur or Leather Gown over them; a large Beaver Cap, double, to come over the Face and Shoulders, and a Cloth of Blanketting under the Chin; with Yarn Gloves, and a large Pair of Beaver Mittings hanging down from the Shoulders before, to put our Hands in, which reach up as high as our Elbows; yet notwithſtanding this warm Cloathing, almoſt every Day, ſome of the Men that ſtir abroad, if any Wind blows from the Northward, are dreadfully frozen; ſome have their Arms, Hands, and Face bliſtered and frozen in a terrible manner, the Skin coming off ſoon after they enter a warm Houſe, and ſome have loſt their Toes. Now their lying-in for the Cure of theſe frozen Parts, brings on the Scurvy in a lamentable manner. Many have died of it, and few are free from that Diſtemper. I have procured them all the Helps I could, from the Diet this Country affords in Winter, ſuch as freſh Fiſh, Partridges, Broths, *&c.* and the Doctors have uſed their utmoſt Skill in vain; for I find nothing will prevent that Diſtemper from being mortal, but Exerciſe and ſtirring abroad.

Corona

Coronæ and *Parhelia*, commonly called *Halo*'s, and *Mock-Suns*, appear frequently about the Sun and Moon here. They are ſeen once or twice a Week about the Sun, and once or twice a Month about the Moon, for 4 or 5 Months in the Winter, ſeveral *Coronæ* of different Diameters appearing at the ſame time.

I have ſeen 5 or 6 parallel *Coronæ* concentric with the Sun ſeveral times in Winter, being for the moſt part very bright, and always attended with *Parhelia* or *Mock-Suns*. The *Parhelia* are always accompanied with *Coronæ*, if the Weather is clear; and continue for ſeveral Days together, from the Sun's Riſing to his Setting. Theſe Rings are of various Colours, and about 40 or 50 Degrees in Diameter.

The frequent Appearance of theſe *Phænomena* in this frozen Clime ſeems to confirm *Deſcartes*'s Hypotheſis, who ſuppoſes them to proceed from Ice ſuſpended in the Air.

The *Aurora Borealis* is much oftener ſeen here than in *England*; ſeldom a Night paſſes in the Winter free from their Appearance. They ſhine with a ſurpriſing Brightneſs, darkening all the Stars and Planets, and covering the whole Hemiſphere: Their tremulous Motion from all Parts, the Beauty and Luſtre, are much the ſame as in the Northern Parts of *Scotland* and *Denmark*, &c.

The dreadful long Winters here may almoſt be compared to the Polar Parts, where the Abſence of the Sun continues for ſix Months; the Air being perpetually chilled and frozen by the Northerly Winds in Winter, and the cold Fogs and Miſts obſtructing the Sun's Beams in the ſhort Summer we have here; for notwithſtanding the Snow and Ice is then diſſolved in the Low-lands and Plains, yet the Mountains are perpetually covered with Snow, and incredible large Bodies of Ice continue in the adjacent Seas. If the Air blows from the Southern Parts, the Air is tolerably warm, but very cold when it comes from the Northward, and it ſeldom blows otherwiſe than between the North-eaſt and North-weſt, except in the two Summer Months, when we have, for the major Part, light Gales between the Eaſt and the North, and Calms.

The Northerly Winds being ſo extremely cold, is owing to the Neighbourhood of high Mountains, whoſe Tops are perpetually covered with Snow, which exceedingly chills the Air paſſing over them. The Fogs and Miſts that are brought here from the Polar Parts, in Winter, appear viſible to the naked Eye in Icicles innumerable, as ſmall as fine Hairs or threads, and pointed as ſharp as Needles. Theſe Icicles lodge in our Cloaths, and, and if our Faces or Hands be uncovered, they preſently raiſe Bliſters as white as a Linnen Cloth, and as hard as Horn. Yet if we immediately turn our Backs to the Weather, and can bear our Hand out of our Mitten, and with it rub the bliſtered Part for a ſmall time, we ſometimes bring the Skin to it's former State: If not, we make the beſt of our Way to a Fire, and get warm Water, wherewith we bathe it, and thereby diſſipate the Humours raiſed by the frozen Air; otherwiſe the Skin would be off in a ſhort time, with much hot, ſerous, watry

watry Matter coming from under along with the Skin; and this happens to ſome almoſt every time they go abroad for five or ſix Months in the Winter, ſo extreme cold is the Air when the Wind blows any thing ſtrong.

Now I have obſerved, that when it has been extreme hard Froſt by the Thermometer, and little or no Wind that Day, the Cold has not near ſo ſenſibly affected us, as when the Thermometer has ſhewed much leſs freezing, having a briſk Gale of Northerly Wind at the ſame time. This difference may perhaps be occaſioned by thoſe ſharp-pointed Icicles before-mentioned ſtriking more forcibly in a windy Day, than in calm Weather, thereby penetrating the naked Skin, or Parts but thinly covered, and cauſing an acute Senſation of Pain or Cold: And the ſame Reaſon, I think, will hold good in other Places; for ſhould the Wind blow Northerly any thing hard for many Days together in *England*, the Icicles that would be brought from the Polar Parts by the Continuance of ſuch a Wind, though imperceptible to the naked Eye, would more ſenſibly affect the naked Skin, or Parts but ſlightly covered, than when the Thermometer has ſhewn a greater Degree of freezing, and there has been little or no Wind at the ſame time.

It is not a little ſurprizing to many, that ſuch extreme Cold ſhould be felt in theſe Parts of *America*, more than in Places of the ſame Latitude on the Coaſt of *Norway*; but the Difference I take to be occaſioned by Wind blowing conſtantly here, for 7 Months in the 12, between the N E and N W, and paſſing over a large Tract of Land, and exceeding high Mountains, *&c.* as before-mentioned. Whereas at *Drunton* in *Norway*, as I obſerved ſome Years ago in wintering there, the Wind all the Winter comes from the N and N N W, and croſſes a great Part of the Ocean clear of thoſe large Bodies of Ice we find here perpetually. At this Place we have conſtantly every Year 9 Months Froſt and Snow, and unſufferable Cold from *October* till the Beginning of *May*. In the long Winter, as the Air becomes leſs ponderous towards the Polar Parts, and nearer to an *Æquilibrium*, as it happens about one Day in a Week, we then have Calms and light Airs all round the Compaſs, continuing ſometimes 24 Hours, and then back to it's old Place again, in the ſame manner as it happens every Night in the *Weſt-Indies*, near ſome of the Iſlands.

The Snow that falls here is as fine as Duſt, but never any Hail, except at the Beginning and End of Winter. Almoſt every Full and Change of the Moon, very hard Gales from the North.

The conſtant Trade Winds in theſe Northern Parts I think undoubtedly to proceed from the ſame Principle, which Dr *Halley* conceives to be the Cauſe of the Trade Winds near the Equator, and their Variations.

" Wind, ſays he, is moſt properly defined to be the Stream or Cur-
" rent of the Air; and where ſuch a Current is perpetual and fixed in
" it's Courſe, it is neceſſary, that it proceed from a permanent and un-

 intermitting

" intermitting Cauſe, capable of producing a like conſtant Effect, and
" agreeable to the known Properties of Air and Water, and the Laws
" of Motion of fluid Bodies. Such an one is, I conceive, the Action
" of the Sun's Beams upon the Air and Water, as he paſſes every Day
" over the Oceans, conſidered together with the Nature of the Soil and
" Situation of the adjoining Continents. I ſay, therefore, firſt, That
" according to the Laws of *Statics*, the Air which is leſs rarefied or ex-
" panded by Heat, and conſequently more ponderous, muſt have a
" Motion towards thoſe Parts thereof which are more rarefied, and leſs
" ponderous, to bring it to an *Æquilibrium*, &c."

Now, that the cold denſe Air, by reaſon of it's great Gravity, continually preſſes from the Polar Parts towards the Equator, where the Air is more rarefied, to preſerve an *Æquilibrium* or Balance of the Atmoſphere, I think, is very evident from the Wind in thoſe frozen Regions blowing from the N and N W, from the Beginning of *October* until *May*; for we find, that when the Sun, at the Beginning of *June*, has warmed thoſe Countries to the Northward, then the South-eaſt, Eaſt, and variable Winds continue till *October* again; and I do not doubt but the Trade Winds and hard Gales may be found in the Southern Polar Parts to blow towards the Equator, when the Sun is in the Northern Signs, from the ſame Principle.

The Limit of theſe Winds from the Polar Parts, towards the Equator, is ſeldom known to reach beyond the 30th Degree of Latitude; and the nearer they approach to that Limit, the ſhorter is the Continuance of thoſe Winds. In *New-England* it blows from the North near 4 Months in the Winter; at *Canada*, about 5 Months; at the *Danes* Settlement in *Streights Davis*, in the 63d Degree of Latitude, near 7 Months; on the Coaſt of *Norway*, in 64, not above 5 ½ Months, by Reaſon of blowing over a great Part of the Ocean, as was before-mentioned; for thoſe Northerly Winds continue a longer or ſhorter Space of Time, according to the Air's being more or leſs rarefied, which may very probably be altered ſeveral Degreees, by the Nature of the Soil, and the Situation of the adjoining Continents.

The vaſt Bodies of Ice we meet with in our Paſſage from *England* to *Hudſon*'s-*Bay*, are very ſurpriſing, not only as to Quantity, but Magnitude, and as unaccountable how they are formed of ſo great a Bulk, ſome of them being immerſed 100 Fathom or more under the Surface of the Ocean; and ⅕ or ⅙ Part above, and 3 or 4 Miles in Circumference. Some Hundreds of theſe we ſometimes ſee in our Voyage here, all in Sight at once, if the Weather is clear. Some of them are frequently ſeen on the Coaſts and Banks of *Newfoundland* and *New-England*, though much diminiſhed.

When I have been becalmed in *Hudſon*'s-*Streights* for 3 or 4 Tides together, I have taken my Boat, and laid cloſe to the Side of one of them, ſounded, and found 100 Fathom Water all round it. The Tide floweth here above 4 Fathom; and I have obſerved, by Marks upon a Body of

Ice,

Ice, the Tide to riſe and fall that Difference, which was a Certainty of it's being aground. Likewiſe, in a Harbout in the Iſland of *Reſolution*, where I continued 4 Days, 3 of theſe Iſles of Ice (as we call them) came aground. I ſounded along by the Side of one of them, quite round it, and found 32 Fathom Water, and the Height above the Surface but 10 Yards; another was 28 Fathom under, and the perpendicular Height but 9 Yards above the Water.

I can in no other manner account for the Aggregation of ſuch large Bodies of Ice but this: All along the Coaſts of *Streights Davis*, both Sides of *Baffin's-Bay*, *Hudſon's-Streights*. *Anticoſh*, or *Labradore*, the Land is very high and bold, and 100 Fathoms, or more, cloſe to the Shore. Theſe Shores have many Inlets or Fuirs, the Cavities of which are filled up with Ice and Snow, by the almoſt perpetual Winters there, and frozen to the Ground, increaſing for 4, 5, or 7 Years, till a kind of Deluge or Land-flood, which commonly happens in that Space of Time throughout thoſe Parts, breaks them looſe, and launches them into the Streights or Ocean, where they are driven about by the variable Winds and Currents in *June*, *July*, and *Auguſt*, rather increaſing than diminiſhing in Bulk, being ſurrounded (except in 4 or 5 Points of the Compaſs) with ſmaller Ice for many Hundred Leagues, and Land covered all the Year with Snow, the Weather being extreme cold, for the moſt part, in thoſe Summer Months. The ſmaller Ice that almoſt fills the Streights and Bays, and covers many Leagues out into the Ocean along the Coaſt, is from 4 to 10 Fathom thick, and chills the Air to that Degree, that there is a conſtant Increaſe to the large Iſles by the Sea's waſhing againſt them, and the perpetual wet Fogs, like ſmall Rain, freezing as they ſettle upon the Ice; and their being ſo deeply immerſed under Water, and ſuch a ſmall Part above, prevents the Wind's having much Power to move them: For though it blows from the N W Quarter near 9 Months in 12, and conſequently thoſe Iſles are driven towards a warmer Climate, yet the progreſſive Motion is ſo ſlow, that it muſt take up many Years before they can get 500 or 600 Leagues to the Southward; I am of Opinion ſome Hundreds of Years are required; for they cannot, I think, diſſolve before they come between the 50th and 40th Degree of Latitude, where the Heat of the Sun conſuming the upper Parts, they lighten and waſte in Time: Yet there is a perpetual Supply from the Northern Parts, which will ſo continue as long as it pleaſes the AUTHOR of all Beings to keep things in their preſent State.

Obſervations of the Longitude, Latitude, and the Declination of the Magnetic Needle, at Prince *of* Wales's Fort, Churchill-River.

	h	′	″
Having obſerved the apparent Time of an Emerſion of *Jupiter*'s firſt *Satellite* at *Fort-Churchill*, on *Saturday* the 20th of *March* laſt 1741-2, at — — — —	11	55	50
I find the ſame Emerſion happened at *London*, by Mr *Pound*'s Tables, compared with ſome Emerſions actually obſerved in *England* near the ſame, at — — —	18	15	10

Whence

	h	′	″
Whence the horary Difference of Meridians, between *Fort-Churchill* and *London*, comes out — — —	6	19	20
	°	′	″
Which converted into Degrees of the Equator, gives for the Diſtance of the ſame Meridians — — — —	94	50	00

Wherefore, ſince the Time at *London* was later in Denomination than that at *Churchill*, it follows that, according to this Obſervation, *Churchill* is 94 Degrees 50 Minutes, in Longitude Weſt of *London*.

I took ſeveral other Obſervations, which agreed one with another to leſs than a Minute, but this I look upon as the moſt diſtinct and beſt.

The Obſervation was made with a good 15 Foot Refracting Teleſcope, and a Two Foot Reflector of *Gregory*'s Kind, having a good Watch of Mr *Graham*'s that I could depend upon; for I have frequent Opportunities of diſcovering how much it's Variation amounted to, and conſtantly found it's daily Deviation or Error to be 15 Seconds too ſlow; by which means it was as uſeful to me for all Purpoſes, as if it had gone moſt conſtantly true without any Change. This Watch I kept in my Fob in the Day and in Bed in the Night, to preſerve it from the Severity of the Weather; for I obſerved, that all other Watches were ſpoiled by the extreme Cold.

I have found, from repeated Obſervations, a Method of obtaining the true Time of the Day at Sea, by taking 8 or 10 different Altitudes of the Sun or Stars, when near the Prime Vertical, by Mr *Smith*'s or Mr *Hadley*'s Quadrant, which I have practiſed theſe 3 or 4 Years paſt, and never found from the Calculations, that they differed one from another more than 10 or 15 Seconds of Time. This Certainty of the true Time at Sea is of greater Uſe in the Practice of Navigation, than may appear at firſt Sight; for you thereby not only get the Variation of the Compaſs without the Help of Altitudes, but likewiſe the Variation of the Needle from the true Meridian, every time the Sun or Star is ſeen to tranſit the ſame. Alſo having the true Time of Day or Night, you may be ſure of the Meridian Altitude of the Sun or Star, if you get a Sight 15 or 20 Minutes before or after it paſſes the Meridian; and the Latitude may be obtained to leſs than Five Minutes, with ſeveral other Uſes in Aſtronomical Obſervations; as the Refraction of the Atmoſphere, and to allow for it, by getting the Sun's apparent Riſing and Setting, which any body is capable of doing, and from thence you will have the Refraction.

If we had ſuch a Teleſcope contrived as Mr *Smith* recommends to be uſed on Shipboard at Sea, now we can have an exact Knowledge of the true Time of the Day or Night from the above Inſtruments and a good Watch, I hope we ſhould be able to obſerve the Eclipſes of the firſt *Satellite* of *Jupiter*, or any other *Phænomenon* of the like Kind, and thereby find the Diſtance of Meridians, or Longitude at Sea.

The Variation of the Magnetical Needle, or Sea-Compaſs, obſerved by me at *Churchill* in 1725. (as in N° 393, of the *Philoſophical Tranſactions* for the Months of *March* and *April* 1726.) was at that Time North 21 Degrees Westerly, and this Winter I have carefully observed it at the same Place, and find it no more than 17 Degrees, so that it has differed about One Degree in Four Years; for in 1738, I observed it here, and found it's Declination 18 Degrees Westerly. I have carefully observed, and made proper Allowance for the Sun's Declination and Refraction, and find the Latitude here to be 58 Degrees 56 Minutes N: But in most Parts of the World, where the Latitudes are fixed by Seamen, they are for the most part falsely laid down, for want of having Regard to the Variation of the Sun's Declination, which, computed at a distant Meridian, when the Sun is near the Equator, may make a great Error in the Sun's rising and setting *Azimuths*, &c.

These things I thought proper to take Notice of, as they may be of Service to Navigators, and the Curious in Natural Inquiries.

An Account of a Book intituled, Observationes de Aëre & Morbis epidemicis, ab Anno 1728, ad finem Anni 1737, Plymuthi factæ. Auctore Joanne Huxham, *M.D. R.S.S.* Londini, apud S. Austen, 1739, 8vo. *Drawn up by* Thomas Stack, *M. D. F. R. S.* No. 451. p. 429. Dec. 1738.

XVII. This Treatise of Epidemicks is ushered in by a large and learned Discourse by way of *Prolegomena*; wherein the Author considers the various Properties of the Air, with it's Effects on living Bodies both in Health and Sickness; and then describes the Method and Instruments he made use of in his Observations.

His Method of observing the Weather, is that published by Dr *Jurin**.

His Instruments, a Barometer with a pretty large Tube, and a very wide Bowl, filled with Quicksilver well purged by Distillation. And this Barometer, to *June* 1733, stood about 46 Feet above the Level of the Sea at Low Water; but after that Time at 30 Feet only above the said Level.

A Thermometer made by Mr *Hauksbee*.

The chief of his Hygroscopes was made after that of Mr *Molyneux* †.

A round Funnel for collecting the Rain, 25 Inches in Diameter, and placed so as to be equally exposed to all Winds.

And he closes the *Prolegomena* with an Account of the Situation of *Plymouth*; and with earnestly expressing his Desire of a general accurate History of the Atmosphere, towards which he contributes this his Mite.

In the Body of this useful Treatise, the ingenious Author gives for every separate Month curious Abstracts of his Meteorological Diary, *viz.* The Quantities of Rain for the respective Days; the Days on which there fell Hail, Snow, *&c.* The *Auroræ Boreales*, and other like Meteors: The Winds, with their Degrees of Force: The considerable Tides: The highest and lowest Stations of the Barometer and Thermometer: The warmest and coldest Days, with the middle Temper of the Air. To these he subjoins the reigning or most epidemic Diseases, and their Methods of Cure; with excellent medicinal Observations both Theore-

* See Vol. VI. Part II. Chap. I. §. xxxvii. † See Vol. II. Chap. I. §. xvi. 3.

tical

tical and Practical, thereon, as often as any thing new or uncommon occurred.

As a Specimen of the Work I beg Leave to exhibit ſome few of his Obſervations, both Philoſophical and Medical, for each reſpective Year.

1723. *July* 2, at 11 at Night, no Wind ſtirring, our Author obſerved a very conſiderable *Aurora Borealis*, whoſe Beams ſhot upward, and terminated in a bright Canopy about 8 or 10 Degrees to the South of the Zenith; with it's Centre about the ſame Diſtance to the Eaſt of the Meridian. It was attended with a very plentiful Dew.

July 22. at 9. *p. m.* The Wind at North, with one Degree of Force; there was a ſmall but uncommon *Aurora Borealis*, whoſe pyramidal Rays darted in an inverted Order; for their Points tended to the Centre, and ran below the Horizon at North.

	Inches.	Deci.
The whole Quantity of Rain collected this Year, was -	36.	364.
Of which fell in *January*, the wetteſt Month, — —	6.	108.
in *September*, the dryeſt, — — — —	1.	526.

	Inches.	
The higheſt Station of the Barometer was — — — — —	30. 3.	on *Feb.* 2. and *Dec.* 15.
loweſt — — — — —	28. 5.	on *Jan.* 18. and *Sept.* 24.

In his general Obſervations on this Year, among other things, he obſerves, that the higheſt Tides generally happened when the Barometer was loweſt: And he thinks, their Cauſe, in part at leaſt, may be attributed to the Air being lighter on our Seas than on the main Ocean. He alſo ſuſpects, that *Spouts* and *Boars* may derive their Origin from the ſame Cauſe, but acting with extraordinary Violence: As a Clap of Thunder, which cauſes a ſort of *Vacuum* for an Inſtant.

This having been a wet Year, our Author takes occaſion to account how a wet and cold Temper of the Air creates Heavineſs, Colds, and other Diſeaſes, from ſuperabundant Seroſity: How Fevers are produced by a moiſt and unelaſtic Air; and gives the manner of treating intermitting Fevers, with the Method and Uſe of Vomits; the different ſorts of Aſthma's with the Cure; and Cautions for avoiding the Contagion of the Air.

1729. *Jan.* 14. at 9. *p. m.* He obſerved a bight Cloud between *Orion*, the *Bull*, and the *Whale*'s Mouth, ſhooting forth very bright Rays: Though there was no Sign of an *Aurora Borealis* in any other Part of the Heavens all that Night.

	Inches.	Deci.
The Total of Rain this Year — — — — —	33.	055.
Of which *Sept.* the wetteſt Month, afforded — — —	6.	498.
January, the dryeſt. — — — — —	0.	900.

On

On Occaſion of his Thermometrical Obſervations made in *Feb.* he ſays, he has frequently obſerved Froſt in Spring, when Mr *Hauksbee*'s Thermometer was but at 55 ; whereas in Winter he has ſeen it at 65, and lower, without the leaſt appearance of Froſt : Wherefore he thinks, the Degree of Cold which produces Froſt, is not yet brought to a due Certainty ; and that Congelation does not depend barely on a Privation of Motion or of Heat.

A ſlow putrid Fever was very epidemical part of this Summer and Autumn; which he aſcribes to the moiſt and hot Temper of the Air, and gives it's Deſcription and Cure.

1730. In the Beginning of *October* there was a violent Eaſtedy Wind, which ſo diſturbed the Sea, that at 3 or 4 Miles from the Shore he found the Leaves of the Trees very ſalt to the Taſte.

	Inches.	Deci.
The Total of Rain this Year was — — — —	25.	698.
Whereof in *November* fell — — — — — —	4.	480.
in *January* but — — — — — —	0.	666.
Barometer higheſt on *Jan.* 10. — — — — —	30.	6.
loweſt *March* 7. — — — — —	28.	5.

On account of a particular Colic which reigned part of this Year, the Author remarks the pernicious Conſequences of giving general Names to Diſtempers ; which often leads the Unſkilful into irreparable Errors in Practice.

	Inches.	Deci.
1731. This was a dry Year, the Total of Rain being but — — — — — — — — — — —	17.	266.
Whereof in *Nov.* the wetteſt Month, fell — — —	2.	356.
in *March*, the dryeſt, — — — — —	0.	140.
Mercury higheſt on *March* 2. — — — — —	30.	4.
loweſt *November* 8. — — — — —	28.	7.

A very remarkable Fall of the Barometer happened between *Feb.* the 2d and the 9th, when the Mercury deſcended from 30. 3, to 28. 9.

Our Author has very often obſerved a very ſudden fall of the Mercury, without any conſiderable Change in the Face of the Atmoſphere ; but upon a more ſtrict Inquiry, he found there had been either great Rain or Thunder ſomewhere in the Neighbourhood ; to which Place the ambient Air ruſhed ſuddenly, in order to reſtore the *Æquilibrium.*

As inflammatory Diſeaſes of the Breaſt were fatal this Year, the Author takes Occaſion, in treating of them, to give the diſtinctive Characteriſtics of the true Pleuriſy and Peripneumony, and conſequently of the frequent Combination of both, or *Pleuro-Peripneumony* ; with their different Methods of Cure.

	Inches.	Deci.
1732. The Total of Rain was — — — — —	33.	096.
Whereof in *October* fell — — — — — —	6.	342.
in *Auguſt* but — — — — — — —	0.	362.
Mercury higheſt *November* 25. — — — — —	30.	4.
loweſt *October* 14. — — — — —	28.	7.

This Year contains excellent practical Obſervations on the Whooping Cough of Children.

	Inches.	Deci.
1733. Total of Rain — — — — — — —	29.	884.
Of which in *December* alone — — — — — —	4.	688.
in *July* but — — — — — — —	0.	772.

In *Dec.* though a very wet Month, the Barometer was high; which he attributes chiefly to the great Quantity of Vapours with which the Air was loaded.

	Inches.	Deci.
The higheſt Station of the Mercury was *Jan.* 24. *Mar.* 3. *Oct.* 18. *Nov.* 5. — — — — — — — —	30.	2.
June 18. and *Oct.* 25. loweſt — — — — — —	20.	8.

And 'tis very remarkable, that whereas the Mercury was at it's higheſt Station of 30. 2. on *Oct.* 18; it was fallen to it's loweſt of 28. 8. on *Oct.* 25. and riſen again to the higheſt on *Nov.* 5.

Here the Reader will find an accurate Deſcription of the Epidemical Colds of this Year, with their Cure.

	Inches.	Deci.
1734. Was a very wet Year, the Total of Rain being	37.	114.
Of which fell in *December* — — — — — —	6.	192.
in *January* but — — — — —	1.	484.

The Mercury's higheſt Station 30. 4. on *Jan.* 29. *Feb.* 1. and *Nov.* 27.
loweſt — 28. 2. *Dec.* 14.

The moſt ſudden Change was between the 23d and 27th of *November*, when the Mercury roſe from 28. 8. to 30. 4.

Quinzeys were very rife this Year among young Folks; for which Reaſon they are here carefully deſcribed, with their various Changes and Cure.

	Inches.	Deci.
1735. Total of Rain fallen this Year — — — —	30.	974.
Whereof in *November* — — — — — —	4.	922.
in *May* — — — — — — —	1.	646.
Higheſt Station of the Mercury on *Jan.* 4. and *Feb.* 2. — — — — — — — — —	30.	5.
Loweſt — — — — — — *Jan.* 11. —	28.	1.

Where

Where it is obſervable, that between the 4th and 11th of *Jan.* the Mercury fell from it's higheſt Station of this Year, *viz.* 30. 5. to it's loweſt (and indeed a very low one) 28. 1.

Nov. 27. though the Moon was in one of the Quarters, the Tides were higher than they generally are at the New and Full.

This was a very moiſt Year as to the Frequency, though not the Quantity of Rain.

This Year a malignant Fever, with Spots, was brought to *Plymouth* by the Fleet, and became very epidemical and deſtructive; wherefore our Author takes great Pains to inveſtigate it's Nature, and gives the Method of Cure which beſt ſucceeded with him.

1736. *Feb.* 6. There was a conſiderable *Aurora Borealis*, wherein the Streamings darted from the very South; and the lucid Canopy was more to the Eaſt than the Author had ever obſerved before.

May 9. A large Halo round the Moon at 10 at Night, and at the ſame Hour on the 11th, another very large one, remarkable for it's fiery Colour.

Aug. 25. Wind W. 1 Deg. Between the Hours of 9 and 11, there appeared in the Heavens a narrow, but very bright Band, which extended entirely from Weſt to Eaſt, and was like a great Rainbow.

	Inches.	Deci.
The Total of Rain — — — — — — —	36.	706.
Of which in *October* — — — — — — — —	6.	534.
in *November* — — — — — — —	1.	150.
And on *July* 10, the Rain was ſo exceſſive, that from 3 *p. m.* to 5 the next Morning it amounted to — —	1.	686.
The Mercury's higheſt Station was on *Dec.* 24. — —	30.	4.
loweſt — — — *Oct.* 9. — —	28.	4.

1737. In *Auguſt* there were *Auroræ Boreales* for 4 ſucceſſive Nights, *viz.* from the 9th to the 12th. The firſt and laſt ſeem to have no particular Circumſtances attending them. That on the 10th, ſeen at 9 o'Clock, was very great. It's Rays were of various Colours, though all very bright and vivid; and formed a beautiful Canopy from the Zenith to about 12° Eaſtward, and a little to the South.

That on the 11th, about 10, was alſo conſiderable. The Canopy appeared in the ſame Place with that of the foregoing Night, and of the Colour of red-hot Iron.

Dec. 5. Our Author obſerved the remarkable red Lights*, and ſays, that in the Evening the Sky ſeemed overcaſt with a thin Cloud or Vapour, but looked as red as from the Reflexion of a great Fire; and it caſt as much Light as the Full Moon on a cloudy Night. This ſurpriſing *Phænomenon* laſted till near Midnight, but it's greateſt Brightneſs was

* See §. XLVI. of this Chapter.

between the Hours of 5 and 7. It cauſed great terrors among the Vulgar ſome apprehending a vaſt Fire, others thinking the Sky overſpread with Blood. For the greateſt Part of the Day the Air was cloudy and warm, with a gentle Rain falling often, eſpecially in the Afternoon, and ſcarce any Wind. In the Evening the Vapour emitted a diſagreeable Smell; and the Doctor happening to ride in the Rain, he perceived the Drops were of a maukiſh ſweet Taſte. This ſame *Phænomenon* was of great Extent in the Northern Parts of *Europe*; and at *Kilkenny* in *Ireland*, was ſeen ſomewhat like a Globe of Fire ſuſpended in the Air for near the Space of an Hour; which then burſting, ſpread Flames around on every Side.

	Inches.	Deci.
The total Quantity of Rain this Year was — — —	27.	364*.
Whereof fell in *March* alone — — — — —	4.	328.
in *May* but — — — — — —	0.	332.
The Mercury's higheſt Station in the Barometer was on *Jan.* 19. — — — — — — — —	30.	6.
loweſt *Sept.* 22. — — — — —	28.	5.

Towards the latter End of this Year, a catarrhal Fever broke out, and became very epidemic. It was indeed ſomewhat like the epidemic Colds of 1733, but much more ſevere. Here the Reader will find the Differences in their Symptoms and Cure well deſcribed: As alſo two valuable Diſſertations, one on the various Species of Jaundice, the other on nervous Fevers, ſo common of late Years: Which I think very worthy of a careful Peruſal by all Orders of Men, who have any Pretence to the Practice of Phyſic.

An Inquiry into the Cauſes of a dry and wet Summer. By— No. 458. p. 519. Sept. &c. 1740.

XVIII. The wet Weather which we had in *March* 1734, (the Year beginning with *Jan.*) ſet me on conſidering what might be the Cauſes of it. The Wind was then, generally, S W, the Weather rainy. Sometimes it veered to S E, which, commonly, brought much Rain: But the Wind ſeldom ſtood at that Point 24 Hours, before it returned to S W again. A ſtrong Gale at S W, with Rain, would be ſucceeded by as ſtrong at N W, ſtill raining; but if the N W continued 24 Hours, it cleared the Sky. The Summer following was cold and wet; the Wind on the ſame Points. The preceding Winter was mild, and eſpecially *Dec.* in which Month, from the 10th incluſive, the Wind blew, generally, S W, ſometimes ſtrong, attended with much Rain. At the End of *Dec.* the Birds ſang, and the Graſs did grow as at other Years in the Spring.

The Winter of 1734 was as mild as that of 1733, the Birds as joyful, and the Graſs as green at the End of *December*, the ſame Winds ſtill

* *N. B.* The Reader is deſired to take Notice of an Error in the Total of Rain for this Year, as it ſtands in the Book; where the Total of the preceding Year has by ſome Miſtake or other been repeated in this: And he is therefore intreated to correct it by the Total ſet down in this Abſtract.

prevailing;

prevailing; but the S W was more stormy. *Dec.* 29, there blew a Storm, first from S W, and then from N W: But the Storm of *Jan.* 8. was much stronger, which blew on the same Points. The Summer of 1735 was colder, and wetter than the preceding Summer.

This put me on recollecting what sort of Winter went before a dry Summer. In the Year 1731, the Summer was remarkably dry. I had not begun to keep a Journal of the Weather in 1730. But I took so much Notice of the unusual Cold in *April* 1731, that I made the following Remarks. *April* 1. begins with peircing cold Winds at N E, black Clouds, stormy, very dry. 4th, 5th, some Wind, Ice. 6th, 7th, 8th, 9th, same Wind. 9th, Snow. 10th, The Harbour frozen over. If my Memory doth not fail me very much, it was in the Winter of the Year 1730, or the Beginning of 1731, that a *Horse* was frozen to Death in *Moscow*, as he stood in the Street.

From hence I conclude, that a frosty Winter produces a dry Summer; and a mild Winter a wet Summer. I am sensible, that these Conclusions are drawn from short and imperfect Observations: But, supposing them to be true, I would be glad to know why these things are so.

I find from these and some other Observations, which I have casually made, that the Weather depends very much on the Wind. I shall therefore begin with inquiring what is the Cause of Winds, and then proceed to find out, as well as I can, why the Wind doth influence the Weather.

Wind is a Stream of Air; Air an unmixed Fluid encompassing our Globe, with a Shell of at least 60 Miles thick. Every Particle of Air gravitates equally towards the Centre of the Earth. Air is capable of being compressed and expanded: The more Air is compressed, the heavier it is; the more it is expanded, the lighter. Cold and Heat, whatever they be, or however they act, produce these contrary Effects in the Air: That is, Cold doth compress the Air, and Heat expands it: Therefore Cold and Heat, in different Parts of the Air, will make it flow: For Cold making the Air heavy, and Heat making it light, the lighter must, of course, give Way to the heavier; as, in a Balance, a greater Weight makes a smaller rise. We daily see a Proof of this in a Stove.

The Sea and Land-breezes, and the Trade-Wind, owe their Original to these Causes. The Sea-breeze, when regular, begins at 9 in the Morning, approaches the Shoar gently, at first; increases till 12; retains it's full Strength till 3; then gradually decreases till 5, when it dies away. At 6 in the Evening the Land-breeze begins, and continues till 8 next Morning: The Interval between these two Breezes, at Morning and Evening, are the hottest Parts of the Day. It is said, that these Winds vary in their Periods; which not being to my Purpose, I take no Notice of.

The way of accounting for this Vicissitude of Sea and Land-wind, is thus: The Sun, as it ascends, sheds it's Heat equally on the Land and

Sea; but the Earth receives the Heat sooner than the Water, or else reflects it stronger. For one or both of these Reasons, the Air that hangs over the Land, is heated more than the Sea-air, it becomes thereby more rarefied and consequently lighter; and therefore the Sea-air, with it's superior Weight, flows in upon it every Way. The Intervals between are owing to the Air of both Places being in an equal Degree of Heat, and consequently of equal Weight.

The Trade-wind never varies, which is thus accounted for: The Air just under the Sun is the hottest: The cold Air presseth upon the hot, as the hot Air follows the Sun; and therefore it makes a perpetual Flow of Wind between the Tropics from *Africa* to *America*, and from thence to the *East-Indies*.

With regard to the Wind influencing the Weather; I find that though Air be an unmixed Fluid, yet it is capable of receiving many Vapours, which float in it, as we see other Bodies float in Water. Sometimes the Vapour ascends, and sometimes it falls to the Ground. All which I take to be effected by Heat and Cold in this Manner: Heat separates Water into small Particles, and the incorporated Air, rarefied by the same Heat, blows up those Particles into Bubbles; by which means the swoln Vapour becoming specifically lighter than a like Space of ambient Air, ascends, swift, at first, (which affords a pleasant Sight in a warm Summer's Day) and then gradually slower, till it gets up to that Part of the Air which is of equal Lightness with itself; and there it remains, as long as the Air continues in the same State: But whenever the Air cools, in which these watery Bladders float, the Cold contracts the Bladder, which becoming thereby specifically heavier than the Air, down it falls in Dew, or Rain. A common Alembic sufficiently shews the Operation of Heat and Cold on the ascending and descending Vapour.

Thus in a calm Evening, when there is no Wind to waft the Air, as the Heat of the Sun declines, the Cold arrests some few of the last ascending Vapours, and, by it's own Force, without any other Change in the State of the Air, compels them to return, in Dew, to the very Spot from whence they arose; whilst their Brethren escape, who go out of the Reach of the Cold a little before the Approach of Night.

Since therefore the same Air, in different States of Heat and Cold, affects Vapour in this Manner, it thence follows, that Vapour, wafted from Air of one Temperament to another, must be affected in the same Manner also: So that Vapour, carried from a colder to a warmer Air, will ascend; and, on the contrary, Vapour carried from a warmer to a colder Air, will descend.

Now if Cold condenses the Air, and thereby makes it press upon the warmer; and if Vapour, carried by a Stream of Air from a colder to a warmer Region, ascends; we have the Reason why the N E blows, and why it blows dry.

Let us fix upon some Spot in the Continent of *North-Europe*, whence this Wind comes to us: Suppose *Archangel*, which lies on our N E

 Point,

Point and is in 65° N Lat. When the Froſt is intenſe, the incumbent Air there muſt needs be very heavy; that Air will preſs every Way: *Quà data porta, ruit.* Let us conſider which Way this condenſed Air can burſt out from thence: It cannot go to the N, where the Cold is greater; nor to the E, for the Air over the large Continent of *Tartary* is at leaſt of equal Coldneſs with itſelf. I make no doubt but they complain at *Archangel*, in their Turn, of Cold N and N E, and even E Winds, as much as we do here. The great Continent to their S muſt be ſo cold as to make a ſtrong Reſiſtance: To the W, the Air might find a free Paſſage over the Ocean, were not the Colds of *North-America* too near. The main Outlet is between both, towards the *Atlantic Ocean*: The warm Air over which being able, of itſelf, to make but a feeble Reſiſtance, yields to the ſuperior Force; the Conqueror eagerly purſues his Victory, and we, happening to lie directly in the Way, feel then a cold dry N E Wind: This is the Wind that brings us Froſt in the Winter. When the Winter is ſevere, it continues to blow all the Spring, and it's Influence reaches to the End of the Summer.

This, I think, ſufficiently proves, that Air, flowing from a cold to a warmer Quarter, will blow dry: But, like a willing Witneſs, it proves too much; for, if Wind proceeds only from cold Air preſſing upon hot, and if Heat makes the Vapour aſcend, it follows from thence, that Wind can never bring Rain; whereas we find the contrary by ſad Experience; the S W Wind hath ruled theſe two Years, and ſtill doth rule.

How can this be accounted for, upon the Principles commonly received? That Vapour, wafted from a warmer to a colder Region of Air, ſhould precipitate, is what I have already ſhewn. But the Queſtion is, which I have not as yet ſeen anſwered, Why does the S W blow? What is the Cauſe why a Stream of Air ſhould be carried, for ſo long a Time, and with ſo great Violence, as we have often felt, from a warmer to a colder, from a rarer to a denſer, from a lighter to a heavier Quarter? To the N E of us lies the Continent of *North-Europe*, great Part of which is, in the Winter, deprived of the Sun's Heat, and conſequently very cold; on the other Side, to the S W, lies the vaſt *Atlantic Ocean*. We find by Experience, that the Sea ſhore is warmer than the Inland; that the Sea is warmer than the Shore; and that the Ocean is ſtill warmer than the Sea. Beſides, the more you go from hence towards the S, the nearer you go to the Sun; and the more N, the farther from it: This muſt make the S W Ocean much warmer than the Continent, that lies at an equal Diſtance, on the oppoſite Point: From this very warm Place, the Wind blows to a Place much colder; and yet there muſt be a natural Cauſe of all this apparent Contradiction to the Laws of Nature: Whether we can find it out or not, I ſhall attempt it as well as I can.

It will be in vain to ſeek for the Cauſe of this Wind in this Ocean itſelf, or in the Air over it, influenced only by the Sun, and the Surface of the Sea. But there may be *Tornados* in thoſe Seas: Our Seamen often meet them between the Tropics, ſeldom, as I am told, in the Ocean I am

now

now speaking of, which is to the N of the Northern Tropic. But were they more frequent and violent than they really are, yet they are not lasting, and therefore cannot produce a long steady Course of S W Winds with us.

My Conjecture is, that our S W Wind is no other than an Eddy of the Trade-wind, reflected from *America* to us. Though we cannot see the Eddy of Air, as we do that of Water; yet we must be otherwise very sensible, that it makes a strong Recoil, when it meets with lofty Buildings, Woods, Hills, *&c.* The more elastic any Body is, it rebounds with the more Agility; and the Experiments that have frequently been made, sufficiently shew the vast Elasticity of the Air. There can, I think, be no Difficulty in conceiving, that there may be an Eddy of Wind from that Part of *America* which lies under the Equinoctial Line, even to us, provided there be a sufficient impelling Force, and due Resistance, and a proper Direction.

The impelling Force is a steady brisk Stream of Air flowing perpetually from *Africa* to *America:* The Strength of this Wind may be in some measure judged of, from what Sailors observe, and express in their Language, thus: It commonly blows a good Top-sail Gale, as we sail large; and if we were to sail on a Wind, our lower Sails would be enough. I am sensible of what every Map shews us now, that the Trade-wind does not blow exactly from E to W: But though the Arrows are placed as if shot obliquely towards the Equinoctial, or rather towards a Line parallel to it, and distant from it between 4 and 12 Degrees N Lat. yet they are all pointed Westward; and that, I presume, will be as much to the Purpose I am upon, as if the whole Stream went due W.

In order to guess at the Momentum of this repelling Force, we should consider the Breadth and Height of that Part of the Trade-wind, which I suppose to be turned this Way.

With Regard to the Breadth, I read in *Dampier*, that they meet the Trade-wind at about 30° on this Side the Line; as many Degrees on the other Side will make the Whole extend to 60° broad. Methinks I do not want such a Breadth, nor indeed can I fairly expect it. For so much of this Wind as blows to the S of the most Eastern Point of S *America*, which I think, is called *Cape St Augustin*, should turn off southward; the rest, which blows to the N of that *Cape*, I may lay Claim to. This *Cape* is in about 8° S Lat. so that I may demand a Breadth of 38°; but I will make an Abatement: For though the Trade-wind, to the N of the Line, be sometimes 30° broad, yet sometimes it is not above 24°; which Variation depends, as I suppose, on the Sun's Place in the *Zodiac:* So that it is narrowest in the Winter, and widest in the Summer. Taking it then at the narrowest, when the Sun is in the Winter Solstice, we shall have a Breadth of 32°: But I allow 2°, to make Amends for the slack Wind, to the North of the Tropic of *Cancer*, and for the Calms near the Equator; and insist on 30° only for the Breadth of that Trade-wind, which is to be reflected back to us.

How

How high ſoever that Column of Air be, which is carried through this wide Space, no more of it can affect us, than what is repelled by the Hills it ſtrikes againſt, and by the cold Air which hangs over them.

I take theſe high Lands, and their incumbent Air, to be a Reſiſtance ſufficient to repel the Trade-wind: The Land muſt needs be ſo to it's Height; and the Air over it, being many Degrees colder than the Trade-wind, will make a Reſiſtance in Proportion to it's ſuperior Weight. How high this Reſiſtance may be, I cannot pretend to determine: If I require no more than 3 Miles from the Surface of the Sea to the Top of the higheſt Ridge of Hills, within the Tract I am now ſpeaking of, and to the cold Air above them, I think I make but a modeſt Demand. Here, then, we have a Gale of Wind of the Breadth of 30°, 3 Miles high, carried with a great Velocity from *Africa* to *America*, a Momentum more than ſufficient to drive the Air from *America* to us, if there be but a proper Direction.

Were the whole Stream of the Trade-wind like a Mathematical Line, mere Length, without Breadth, and were this ſtrait Line to ſtrike on a ſmooth Surface of a given Inclination, we could know it's Direction exactly. For it is a Rule in Geometry, that the Angle of Reflection is equal to the Angle of Incidence. Suppoſe, for Example, that the Line of Trade-wind blew juſt S E, as it is ſaid to do, S of the Equator; that the Surface it ſtruck againſt ran exactly from S to N, as the Hills of *Peru* do; and that the Point of Incidence were under the Equator; in this Caſe the Angle of Incidence will be half a Right Angle, or an Angle of 45°, and conſequently the Angle of Reflexion will be 45°: Now, as theſe Degrees, when the reflected Line ſhall have run 90° in Length, will be equal to Degrees of a great Circle, and as we are about 90° E of this ſuppoſed Place of Contact, therefore this reflected Line will, in our Longitude, reach to 45° of N Lat. which is about *Bordeaux*. If we ſhould ſuppoſe the whole Breadth of the Trade-wind to conſiſt of an infinite Number of parallel Lines, falling on a Surface of the ſame Inclination, then the reflected Lines will be all parallel, and conſequently the Angles will be all equal; but they will reach wider, according to the Diſtance of one Point of Contact from the other; ſo that if that Line, which fell on the ſuppoſed Surface under the Equator, be reflected to 45° N Lat. that which fell on the ſame Surface to the N of the Equator, ſuppoſe in 23° S Lat. will reach to 68° N Lat. which is to the N of the *Orcades*, and almoſt to the N Cape of *Norway*. Or if we ſuppoſe the Trade-wind to the N of the Equator, to flow directly N E as it is alſo ſaid to do, and to ſtrike againſt a Surface inclining from S E to N W, which is pretty near the Bearing of the *Iſthmus* that joins N and S *America*; in both theſe Caſes the Reflexion will be towards the N E.

But there is no depending on this way of calculating: Not that God does not act according to the exacteſt Rules of Geometry, in the Motion of the Winds, as well as in all other Parts of the Creation: But we do not know, at leaſt I am far from pretending to know, all the infinite

Variety

Variety of Reverberations that the Wind must have, from the uneven Surfaces it strikes against, between Cape *St Augustin* and the Bottom of the Gulph of *Mexico.* I make no doubt but that different Parts of this Air are reflected a thousand different Ways; and yet that the whole afterwards unite, or the far greater Part, and flow this Way. I find myself under a Necessity of supposing what I cannot demonstrate Mathematically, since I can assign no other Cause why the S W Wind blows so long with us.

But there are some other Facts which strongly support my Hypothesis; *viz.* Currents of the Sea, and the Wind in the *Atlantic* Ocean, to the N of the Trade-wind.

With Regard to the Currents, *Dampier* tells us, it is generally observed by Seamen, that, in all Places where the Trade-wind blows, the Current moves the same Way with the Wind; and that though it be perceived most near the Shore, yet it makes no sensible Rising in the Water, as the Tides do. He says, there is always a strong Current setting from Cape *St Augustin* Westward, occasioned, as he remarks, by the S E Trade-wind driving the Surface slanting on the Coast of *Brasil*; which, being there stopped by the Land, bends it's Course Northerly, towards Cape *St Augustin*; and, after it has doubled that Promontory, it falls away towards the *West-Indies*, down along the Coast Westward, till it comes to Cape *Gratia de Dios*; from thence N W towards Cape *Catoch* in *Jucatan*, thence to the Northward between *Jucatan* and *Cuba.* He says, that in the Chanel, between *Jucatan* and *Cuba*, he has found the Currents extraordinary strong; that it is probable, that the Current which sets to Leeward, on all the Coast from Cape *St Augustin* to Cape *Catoch*, never enters the Bay of *Mexico*, but bends still to the Northward, till it is checked by the *Florida* Shore; and then wheels about to the E, till it comes near the Gulph's Mouth, and passes with great Strength through the Gulph of *Florida*, which is the most remarkable Gulph in the World for it's Currents, because it always sets very strong to the N.

Thus far this Pilot: And, if too great a Fondness for my own Conjecture does not prejudice me very much, I may venture to say, that these Observations strongly confirm it. He takes notice of the first Current which the Trade-wind makes near the Shore at Cape *St Augustin*, where it is strong; thence he traces it from one Cape to another, as it winds about by different Directions, yet still gathering Strength at every Turning: It is extraordinary strong between *Jucatan* and *Cuba*, but strongest at the End of it's Course in the Gulph of *Florida*. This Acquisition of Strength upon a new Direction, is contrary to the Laws of Motion; therefore it must be owing to a fresh Supply, which the rest of the Current, caused by the Trade-wind, gives it, till at length the whole Power, joined together, rushes out into the *Atlantic* Ocean.

Let us then suppose the Wind, which drives this Water before it, to follow it much in the same Course; and that, instead of striking against one plain Surface, with such an Inclination as would direct it to us, it strikes

ſtrikes againſt a Million, yet ſtill bending this Way: Let this natural Suppoſition be admitted, and we have the very Thing ſought for, *viz.* a proper Direction.

The other Fact is this: That when our Ships return from the *Weſt-Indies* through the Gulph of *Florida*, and are got into the wide Ocean, they have a regular Wind at S W, or near that Point, which ſometimes attends them to their very Port. This Wind cannot have it's Riſe in that Ocean, nor can it come from any Continent that lies to the North, or even Weſt of it; therefore I conclude, that it muſt be an Eddy of the Trade-wind.

But to all this it may be objected, that the Sea-current ſets out of the Gulph of *Florida* towards the N; whereas, I ſay, the Wind comes towards the N E. Sailors, it ſeems, take no farther Notice of theſe Currents, than while they are near Head-lands, where they are ſtrongeſt, and affect their Navigation moſt. But there ſeems to me to be a Neceſſity of the Continuance of this Current much farther than the Gulph of *Florida*, and of it's taking new Directions from the N towards the N E, and thence even towards the S, before it be quite ſpent. For it muſt be a vaſt Quantity of Water that is driven by the whole Breadth of the Trade-wind, from *Africa* to the Shores of *America*; the far greater Part of which, as *Dampier* ſuppoſes, doth flow by the Promontory of Cape *St Auguſtin* Weſtward. This great Flux of Water has found a Paſſage out towards the N, between *Florida* and *Cuba*; which is the Reaſon, that, notwithſtanding the Current ſets Weſtward, the Sea in the *Weſt-Indies* never riſes. Here we ſee, that the Middle Ocean is at a great and conſtant Expence of Water; it muſt therefore want a Supply, and no Supply ſo natural, as for it to have it's own Water again; which, after it hath paſſed the Gulph of *Florida*, meets with one Check ſtill after another, till it returns to the Place from whence it came.

For the ſame Reaſon we may ſuppoſe, that though the Eddy of the Trade-wind ſhould be reflected due North, from the Land it firſt ſtrikes againſt; or even though it ſhould undergo as many Turnings as the Surface of the Sea it drives before it; yet it may take a new Direction in the Ocean, cauſed by the Winds that blow from the Continent of *North America*.

Another Objection may be made againſt the S W Wind being an Eddy of the Trade-wind, from what I myſelf have advanced, *viz.* that Cold is the Cauſe of Wind: That the *Atlantic* Ocean is too warm to produce this Wind; and yet that it comes from the Trade-wind, which blows between the Tropics, a Place much warmer: So that, according to this, here is a very warm Wind making it's Way againſt the Cold of the North.

That Wind will blow from a warmer to a colder Quarter, is confirmed not only from the S W raging with us in Winter, which muſt be confeſſed to come from a much warmer Climate, whatever Cauſe it be owing to; but from the almoſt daily Obſervation of thoſe who live in the Country, and will look a little about them. Whoever is within the Sight of Hills, and there are few Places where there are not some in View, will find, if he takes the least Notice, that it rains in the Hills before it rains in the Vales: What can be the Cause of this Rain? Nothing, doubtless, but a Wind blowing from the Vales towards the Hills; that is, from a warmer to a colder Region, where the Vapour, which is brought thither, falls. Suppose the Air over the Hills be cold in 20°, and the Air over the Vales but in 10°, 10 cannot outweigh 20; but, if it gets an additional Force of 30, it will then be double the Strength of 20; and, consequently, blow from a warmer to a colder Quarter.

The *Momentum* of every Thing that moves, is made up of two Powers, Weight and Velocity, multiplyed by one another. This is fully shewn in the Butcher's Stiliard, which, with a Weight of 5 Pound, will weigh 100 Pound, by placing the 5 Pound Weight 20 times farther from the Centre, than the Thing to be weighed is placed: For 5 Pound the Weight multiplied by 20, the Velocity is equal to 100 Pound. Supposing then that the Cold to the N E of us be 100, and the Cold which drives the Trade-wind, only 5; that is, that the Cold of the one Place be 20 times greater than the Cold of the other: But supposing the Air 5° cold, to move 20 times faster than that which is 100°; upon this Supposition the *Momentum* will be equal: And since they move in direct Opposition to one another, they will meet exactly half way, which I take to be sometimes near the Case. But if the Northern Power should lose $\frac{1}{2}$ it's Weight, *i. e.* be milder, by $\frac{1}{2}$, one Winter than it is another, the other Power still continuing the same, then the S W will blow one Half farther.

I am aware of but one Objection more, which is, that in the Gulph of *Florida*, through which I suppose the Trade-wind to flow towards us, there are variable Winds, which must interrupt this Stream, if there be any; since the same Air cannot flow different Ways at the same time.

It is no new thing to have variable Winds near the Land, even in the midst of the Way of the Trade-wind. The Westerly Winds, which, as *Dampier* says, blow on the Coast, between Cape *Gratia de Dios*, and Cape *La Vela*, are a Proof of it. The common Trade-wind on this Coast is between N E and E; but from *Oct.* till *March*, and chiefly in *Dec.* and *Jan.* the Winds blow W; and yet when they are longest and strongest on the Coast, the Easterly Trade-wind blows off at Sea, as at other times. Near Cape *La Vela*, the true Trade blows within 8 or 10 Leagues off the Shore, when the Westerly Winds blow on the Coast. This shews that these Land-winds reach but a little Way, and therefore can have but a small, if any, Influence on the main Stream of the Trade-wind.

In smaller Navigations here in *Europe*, they find the Wind out at Sea different from what it is near the Shore, and especially near Head-lands, where it generally blows hardest, and which helped to make the Navi-

gation

gation of the Ancients, in the *Mediterranean*, so tedious and dangerous. These variable Coast-winds may be owing to great Snows, or Rains that fall upon High-lands, when there is none, or little, at Sea, or to some Storms of Thunder that burst over them, or to their natural Coldness, or even to the Repercussion of the Air. I take the variable Winds they meet on the Coast of *Florida*, to be owing to the like Causes, which have their Influence but a little Way.

But it may be said, that these variable Winds on the Coast of *Florida* are found so near the Trade-wind, that there is no Room between them for the Eddy of the Trade-wind, I am speaking of, to pass out. It may be Fact, for ought I know: I will suppose it to be Fact. But I desire the Objector to consider, that, when he is sailing, he is on the Top of the Water, and at the Bottom of the Air; he perceives the Current of Water run very fast at Top, but does not know how it runs at the Bottom. It is very certain, that there are Under-currents in Water: In Rivers that ebb and flow, it is perceived every Tide; for the Current will run up after it hath begun to ebb. By Experiments that have been made, it appears, that in some Places, where the Current on the Surface is very strong, the Under-current, running quite the contrary Way, shall be much stronger, and carry away a Boat against the Force of the upper Current.

And why may there not be contrary Currents in the Air? An Element much more subtile than Water, and therefore capable of being put into a greater Variety of Motions. The Sailor concerns himself no farther with the Wind, than as it fills his Sails, the Height of which can bear but a small Proportion with that Column of Air I am now speaking of. The Land-breezes about Islands, in the Torrid Zone, shew different Currents in the Air. For, in the Night, the Wind shall blow from the Centre of the Island, every Way, into the Sea, and even in direct Opposition to the Trade-wind, and yet give no Interruption to the Progress of it, except just in that little Spot, and for a small Height too; which is evident from hence, because in sailing to the Westward of *Barbadoes*, suppose, or *Jamaica*, without the Reach of the Land-breeze, you feel no Interruption in the Strength of the Trade-wind, by Night as well as by Day.

I, who am one of no great Observation, have frequently seen different Currents in the Air, at the same Time, and in the same Quarter, under one another. For Example: When the under Current has been E, the upper Current has been S W, and the middle S E. I shall appear ridiculous, if I say I see the Wind; the Vulgar think, that Swine only are endowed with that Quickness of Sight. I do not say, that I can see the Wind; but I have often seen Clouds, Weather-cocks, Smoke, and such-like Things, that are either carried or turned by the Wind. Smoke and Vanes are so near, that they can hardly cause any Deception; some Clouds may, unless properly observed: For when there are two Tire of Clouds, both carried the same Way, and with the same Velocity, the upper Tire ſhall appear to move directly contrary to the lower; which Deception is owing to the different Angles that Objects of the ſame Magnitude, at different Diſtances, make on the *Retina.*

The way to obſerve the Motion of the Clouds, is by looking at them and a fixed Object at the ſame time, as the Sun and Stars, ſometimes: The beſt ſuperior fixed Object is the Moon in her Quarters, which may be then ſeen by Day light, without offending the Eye. The fixed Objects below the Clouds are, a Ridge of Hills, lofty Buildings, or, for want of them, a Tree. By obſerving a Cloud with any fixed Object, you will not only ſee on what Point of the Compaſs the Clouds paſs; but you will ſee alſo the Motion of the upper and under Current: By this means you will find, as the Caſe happens, either both Currents going the ſame Way, though with different apparent Velocity; or the upper Current going one Way, and the lower another, and perhaps you will ſee the Smoke going a Third. This ſufficiently ſhews, that there are different Currents in the Air.

From all my little Obſervation I find, that the upper Current generally prevails. For though the under Currents from, ſuppoſe, the E, or even N E, be briſk at firſt, and the briſker they are at firſt, the longer they continue; yet they die away by degrees, as their Strength ſpends itſelf; the Air becomes near calm, and then the S W, which before blowed aloft, deſcends to the Earth, and commands the whole Sky.

That the Diſorders of the lower Air do not affect the Stream above, appears alſo from the Trade-wind paſſing over the very Continent, from the E to the W Side of *America.* I make no doubt but the high Hills of *Peru* cauſe a greater Variety of Winds and Weather, than we have here. Their Weſtern Sea ſhews, that the lower Part of the Trade-wind meets with great Obſtructions in paſſing over the Continent. For, as *Dampier* obſerves, you do not meet with the true Trade-wind, till you are got 150 or 200 Leagues from Shore; and then it blows in it's uſual manner. If all the Diſturbance that the high Hills of *Peru*, ſaid to be the Higheſt in the World, give to the Trade-wind blowing over them, cannot intercept the upper Stream, which, after ſurmounting all thoſe Heights, and the Diſorder that their Cold occaſions, ſtoops down again, till it comes to touch once more the Surface of the Ocean; we may eaſily ſuppoſe, that that Part of the Trade-wind, which is reflected from theſe Hills towards the N E, may diſengage themſelves, in like manner, from all inferior Obſtructions, and fly over all the little low Diſorders of the *Floridan* Coaſt.

Upon the Whole, then, though I cannot pretend to find out the Angle of Incidence, yet I muſt conclude, that the Trade-wind is reflected in ſuch a manner as to cauſe our S W Wind.

And I conceive, that this new Direction is ſo far from checking it's Current, that it the rather increaſes it. For a great Part of the cold Air, that hangs over the Continent it ſtrikes againſt, having no other Vent, flies

off

off with the Eddy, and thereby makes more than Amends for the Stop it gave.

From *America* to the Weſt of *England* this Wind glides over the Ocean, a plain Field, that gives no Oppoſition, and which, with it's natural Warmth, encourages the Waft, by making the Air over it more ready to yield to the impelled Force.

Having thus opened a Paſſage for the Trade-wind to flow even to us, with a back Stream. if my Conjecture hath opened it; what I have ſaid may ſerve as a Hint to thoſe who have better Materials, and can make a better Uſe of them: But, admitting that my Conjecture is right, we have the Cauſe why the S W Wind blows with us; and then there can be no great Difficulty in finding out the Reaſon why it brings ſo much Rain.

For this Wind blowing over a warm Ocean, which ſends up many Vapours, by the time it reaches us, comes charged with an infinite Swarm of watery Bladders, which the Cold of this Climate condenſes, and then down they fall in Showers of Rain.

From hence it appears, that the two great Rulers of the Weather with us are the N E and S W Winds. Like two neighbouring potent Monarchs, they are engaged in eternal Wars: Sometimes the one puſhes his Conqueſt with great Rapidity; and ſometimes the vanquiſhed Power not only recovers it's loſt Dominions, but carries on the War into his Enemy's Territories with great Succeſs. As we happen to lie near their Frontiers, we feel, by turns, the different Effects of their fierce Contention: Some Years we have a Run of N E Winds, froſty Winters, and dry Summers; and ſome Years the Reverſe of all this.

But if I have hit upon the true Cauſes of theſe Winds, yet the Queſtion will be, On which Side lies the Redundancy, or Failure, that makes all this irregular Variation? For, between two Antagoniſts, the Advantage will be the ſame to the Conqueror, whether his Superiority be owing to his own Strength, or the Weakneſs of his Adverſary, I would be glad to find this out, but I doubt that all my little Search will not be able to do it. I will proceed as far as I can.

Let us ſuppoſe, in the firſt Place, the N to be entirely paſſive, and that all the Variation of Cold and Heat is owing wholly to a Defect, or Exceſs, in the S W Wind: So that, when the S W blows, it ſhall be always warm; and, when it ceaſes to blow, it ſhall be ever cold. If this be Fact, then it will follow, that whilſt the S W blows with the ſame ſteady Gale, the Weather ſhall be of the ſame Degree of Heat: But we find it otherwiſe; for the Nights, in a mild Winter, are colder than the Days, the ſame S W ſtill blowing; therefore Cold, with us, is not wholly owing to a Slackneſs of the S W Winds.

Let us now ſuppoſe the Privation of the Sun's Heat to be the only Cauſe of Cold: The Conſequence will be, that all Places equally diſtant from the Sun, will be equally cold. But it is well known, that, in the ſame N Lat. in *Europe*, Cold is greater on the Continent than in Iſlands:

Islands: Therefore Privation of the Sun's Heat is not the only Cause of Cold. The Sun's Absence, like other negative Causes, can amount only to the Removal of an Obstruction which hindered the efficient Cause of Cold, whatever it be, from acting.

Since the larger the Tract of Earth, the greater the Cold, the efficient Cause of Cold seems to be in the Earth; and yet, when we descend a little Way under-ground, not only in Mines, but in some Cellars, we find an even Temperament: We must therefore confine this efficient Cause of Cold to the Earth's Surface.

But if the Earth's Surface be the sole efficient Cause of Cold, since the Surface of the Earth still continues the same, the Cold should be the same on that Surface every Winter; whereas we find it otherwise. We must, therefore, seek for some concurrent Cause, between whom and the Earth's Surface this Cold is generated; and that, I think, can be no other than what is carried on the Wings of the Wind.

Dampier observes, that, after a *Tornado* at Land in *Jamaica*, the Land-wind will begin by 4 or 3 in the Afternoon. The Materials of this *Tornado* must be carried thither by the Wind; where the *Tornado* bursts, it cools the Air; which makes the Land-breeze begin some Hours sooner than it's usual Course.

That the S W Wind, warm as it is, carries with it the Seeds of Cold, is evident from those violent Storms of Thunder, attended with great Rains, and large Hailstones, several of which happened this last Summer.

The 8th of last *Sept.* was a cold Winter's Day at the Place where I dwell. In the Morning, when I awoke, I perceived a great Dew on the Inside of the Glass of my Chamber-window: When I went out, I observed the Wind to be N E, strong, black Clouds, and little Rain early, rest dry. 9th in the Morning, the Wind was N E, brisk, dry. I began to think, that the Winter was going to set in very severe; but I was in a little time undeceived. The Afternoon of the 9th was overcast. On the 10th, I saw Colts-tails, as the Sailors call them: I take them to be *Virgil's Tenuia lanæ vellera:* Marks of Rain, that seldom deceive those who are used to observe them. On the 11th, the Wind returned again to it's old Point of S W, with Rain. Some time after, I did read in the News-papers, that on the 7th a violent Storm fell about *Worcester*, which is distant from hence about 2 Degrees, and bears, nearly, on the N E Point. Then I found out the Cause of that little Winter.

I could mention more Facts of this Kind, but these, I believe, are enough to satisfy us, that the Seeds of Cold are carried on the Wings of the Wind. It will be needless to take notice, that the Wind carries the Cold back again: Every one who feels his Hands tingle in a frosty Morning, and looks at the Weather-cock, must be sensible of it.

Since, therefore, a large Surface of Earth to the N of us, assisted only with a Privation of the Sun's Heat, cannot produce Cold to so great a Degree, as to affect the Weather with us; and since it appears, that that which

which is to help these two Causes to produce such a Cold, is brought by the Winds, and carried off again; I must conclude, that there are frigorific Particles floating in the Air, whether they be Nitre, or by whatever Name the Chymists will call them; that they are always acting, unless obstructed by other Causes; and that, when they find a proper Recipient, and all Obstructions be removed, they act with Vigour.

When I speak of the Seeds of Cold, I do not mean, that Cold acts as a Vegetative: Though whoever considers the Order that Frost observes in building it's Ice upon the Water, will be apt to think, that if it be not the Effect of Vegetation, it is something that resembles it very near.

It first shoots out a small strait Twig; then, from the same Centre, one on each Side; from these main Beams dart out smaller Sprigs on each Side, to form the Contignation; then these Rafters sending forth their Sprays, the whole Floor is laid, weak at first; but as they gather Strength, they make a Plancher, strong enough, sometimes, to bear the Weight of whole Armies passing over the *Baltic*.

I do not expect, that the Ladies will expose themselves so much to the Cold, as to see all this: But if they please to give themselves only the Trouble to look on their Chamber-windows in a Frosty Morning, if they rise soon enough, they will see there such Embroidery made by Ice, as their own Fingers, were they used to work, and the finest Needles, could not equal.

All this, I say, would tempt one to imagine, that there is something vegetative in what I call the Seeds of Frost. But that is not what I am about at present. All I contend for now is, that that which co-operates with the Earth's Surface, to produce Cold, which way soever it produces it, is carried to and fro by the Air.

Instead of their acting like Seeds, let us suppose them to act like inanimate Bodies: That each Particle acts with a determined Force; and that, consequently, the more of them act together, the greater their Effect. Upon this Supposition we can easily account for the different Temperament of the Air in the same Seasons. For a Continuance of N E Winds for some Years will carry off many of these Seeds, or Grains of Cold; and an equal Continuance of S W Winds will bring them back again; and these Periods will be longer or shorter, according to the Strength or Weakness of the Blast.

And thus, at length, I have satisfied myself, till I can find out a better Reason, why a cold frosty Winter produces a dry Summer; and a mild Winter a wet Summer. For these Seeds of Cold being the chief Cause of Frost, and their Strength being in Proportion to their Number, when the Winter is severe, there is so vast a Quantity of these frigorific Particles in *North-Europe*, that their Strength will not soon be exhausted; and, consequently, that the N E Winds will blow long, and make the Summers dry.

But,

But, on the contrary, when the Winter is mild, there are but a few of thoſe Particles in *North-Europe*, not enough to cool the Air there to ſuch a Weight, as to enable it to hinder the S W from reaching us, even in Winter; and therefore, when once the Sun's Heat comes to deſtroy thoſe few, the South-weſt, which is always acting with equal Force, prevails, and brings Rain in Summer.

I make no doubt, but that a Courſe of Obſervations, kept for ſome Years, in ſeveral Places, would reduce the Knowledge of theſe Viciſſitudes of Wind and Weather to ſome Certainty.

I have taken notice only of two Winds, the North-eaſt and South-weſt, as the Producers of a long Run of dry or wet Weather: But if I have hit upon the true Cauſes of thoſe Winds, the ſmaller Variations may be eaſily accounted for. I ſhall mention a few.

Next to thoſe Two, the N W Wind blows longeſt here, and with the greateſt Force, but with various Effects. Sometimes it conſpires with the South-weſt, to blow a mere Storm, with hard Rain; and ſometimes it takes part with the North-eaſt, blows dry, and freezes. We are, in a great meaſure, beholding to this Wind, for the little dry Weather we have in a mild Winter.

I take this Wind to proceed from the Continent of *North-America*, where the Cold muſt needs be very intenſe, that can drive the Air from thence hither, with ſuch ſtrong Guſts. It is well known, that Places of the ſame N Lat. are much colder in *America* than in *Europe*. Upon Suppoſition that the N W Wind blows from *N America*, I can, methinks, eaſily account for all theſe contrary Effects produced by the ſame Wind.

Though it blows from a cold to a warmer Quarter, yet it brings Rain at firſt, for this Reaſon, becauſe the Air over the Ocean about us is warmer than that over us. When the N W begins to blow, it muſt drive the Air before it; and then the Vapour that floated in warm Air will fall down with us. Even the N E, the drieſt and coldeſt Wind we have, will bring Rain, and for many Hours, when it ſets in after a South-weſt.

Hence alſo it is that the S E and S Winds bring much Rain, and for many Hours together. I take the S E to come from the *Alps*, and the South from the *Pyrenees*.

I ſhall, at preſent, run no farther into Particulars; my Deſign being only to inquire into the Cauſes of a long Continuance of dry or wet Weather. It would be endleſs to enter into all the Predictions of Weather, that may be collected from Books, and private Obſervations: Moſt of them pretend to foretel the Weather no farther than a few Days. If thoſe Predictions and my Hypotheſis be founded on Nature, they will all admit of the ſame, or of a conſiſtent Explanation.

N. F. Dec. 31. 1735.

XIX. The

XIX. The Place from which I write, and where I reside, is 14 Miles South of *Durham*, Lat. 54° 46′. The Evening before the 8th, my Barometer stood at 29 Inches, but had been gradually falling for two Days. The Wind was then S W high in the second Degree; which increased towards Midnight a Degree more. Most of the Day was attended with Snow or Sleet. *Concerning the Storm* Jan. 8th 1734-5, *by* Mr Henry Forth, *dated* Darlington, Jan. 18, 1734-5.

The 8th in the Morning I found my Glass fallen to 28 Inches, 38 Parts, and at 4 *p. m.* down to 28 Inches, 5 Parts, and by 10 *p. m.* risen again to 28 Inches 45 Parts. All this while the Wind with us was in the N E, with only a moderate Gale, tho' attended all Day with Snow, which at Night was 2 ¼ Inches deep; and about 8 it began to freeze. As the Wind in the South Parts was all that while in the opposite Quarter, I should have expected an Accumulation of the Air, and, as a Consequence, the rising of the Barometer at the time of it's falling the lowest. Had the Storm been the Night before, when our Wind was in the same Direction, and had afterwards fallen, I should then have imputed the Fall to the quick Return of the Current of Air to restore the Æquilibrium: But as it is, the small Progress I have made in *Natural Philosophy*, leaves me in Ignorance.

P. S. As we have been fortunate in escaping the last Storm, we have been no less so in Regard to the melancholy Effects which the great Rains have produced in the more Southerly Parts; for tho' we had more than usual with us the last Month (for I find by my Register that 13 ℔ 85 Pts. fell through my Funnel, whose Area is just 100 Inches, in *Dec.* last) yet the almost constant intermitting Frosts we had, kept it from going off in any considerable Quantity at a time.

		Inches.
Barometer,	greatest Altitude — —	30 : 10
	least ditto — — ——	29 : 13

XX. Yesterday was the most violent Hurricane of Wind in these Parts, that ever was known since the Memory of Man. *Cambridge* was not in the midst of the Hurricane, so that it has escaped very well. I happened to be paying a Visit to Dr *Knight*, at *Bluntsham* in *Huntingtonshire*, about 10 Miles N W of *Cambridge*. We were in the midst of the Hurricane; but, by getting into the strongest Part of the House, we escaped without any great Danger. The Morning, till half an Hour after 11, was still, with very hard Showers of Rain: At half an Hour after 11 it began to clear up in the S, with a brisk Air, so that we expected a fine Afternoon: The S W cleared up too, and the Sun shining warm drew us out into the Garden. We had not been out above 10′, before we saw the Storm coming from the S W: It seemed not to be 30 Yards high from the Ground, bringing along with it a Mist, which rolled along with such incredible Swiftness, that as near as we could guess, it ran a Mile and an half in half a Minute: It began exactly at 12 o'Clock, *Concerning a violent Hurricane in* Huntingtonshire, Sept. 8. 1741. *By* Mr Stephen Fuller, *Fellow of* Trin. Coll. Cambridge. No. 461. p. 851. *dated* Cambridge, Sept. 9. 1741.

and laſted about 13′, eight Minutes in full Violence: It preſently untiled the Houſe we were in, and ſome of the Tiles, falling down to Windward, were blown in at the Saſhes, and againſt the Wainſcot on the other Side of the Room; the broken Glaſs was blown all the Room over, the Chimnies all eſcaped; but the Statues, which were on the Top of the Houſe, and the Baluſtrades from one End to the other, were all blown down. The Stabling was all blown down, except two little Stalls. All the Barns in the Pariſh, except thoſe that were full of Corn quite up to the Top, were blown flat upon the Ground, to the Number of about 60. The Dwelling-houſes eſcaped to a Miracle; there were not above a Dozen blown down out of near 100. The Alehouſe was levelled with the Ground; but by good Luck not a Soul in it. If the Storm had laſted 5′ longer, almoſt every Houſe in the Town muſt have been down; for they were all, in a manner, rocked quite off from their Underpinnings. The People all left their Houſes, and carried their Children out to the Windward Side, and laid them down upon the Ground, and laid themſelves down by them; and by that means all eſcaped, but one poor Miller, who went into his Mill to ſecure it againſt the Storm, and was blown over, and cruſhed to Death betwixt the Stones and one of the large Beams: I ſaw him taken out. All the Mills in the Country are blown down: I do not hear of any more bodily Miſchief; only one Miller at *Willingham*, ſo much bruiſed, that they hardly expect his Life. Hay-ſtacks and Corn-ſtacks are ſome quite blown away, ſome into the next Corner of the Field. The Pigeons, that were catched in it, were blown down upon the Ground, and daſhed to Pieces; one of which I found, myſelf, above half a Mile from either Houſe or Hedge. Wherever it met with any boarded Houſes, it ſeemed to exert more than ordinary Violence upon them, and ſcattered the Wrecks of them for above a quarter of a Mile to the N E, in a Line: I followed one of theſe Wrecks myſelf; and, about 150 Yards from the Building, I found a Piece of a Rafter, about Feet long, and about 6 Inches by 4, ſtuck upright 2 Feet deep in the Ground; and at the Diſtance of 400 Paces of my Horſe, from the ſame Building, was an Inch Board, 9 Inches broad, 14 Feet long: I am convinced, that theſe Boards were carried up into the Air; for I ſaw ſome, that were carried over a Pond above 30 Yards; and I ſaw a Row of Pales, as much as two Men could lift, carried 2 Rods from their Places, and ſet upright againſt an Apple-tree. Pales, in general, were all blown down, ſome Poſts broke off ſhort by the Ground, others torn up by the Stumps. The whole Air was full of Straw: Gravel-ſtones, as big as the Top of my little Finger, were blown off the Ground in at the Windows; and the very Graſs was blown quite flat upon the Ground. After the Storm was over, we went out into the Town, and ſuch a miſerable Sight I never ſaw: The Havock I have deſcribed; the Women and Children crying, the Farmers all dejected; ſome bleſſing God for the Narrowneſs of their Eſcape, others wondering how ſo much Miſchief could be done with one Blaſt of Wind, which

hardly

hardly laſted long enough for People to get out of their Houſes. I talked to two People, that were out in it all the Time, who ſaid, that they heard it coming about half a Minute before they ſaw it; and that it made a Noiſe ſomething reſembling Thunder, more continued, and continually increaſing. I ſaw a Man in the Afternoon, who came from *St Ives*, who ſays, the Spire of the Steeple, which is one of the fineſt in *England*, is blown down, as is the Spire of *Hemmingford*, the Towns having received as much Damage as *Bluntſham*. There was neither Thunder nor Lightning with it, as there was at *Cambridge*, where it laſted above half an Hour, and conſequently was not ſo violent. Some few Booths in *Sturbridge-Fair* were blown down. The Courſe of the Storm was from *Huntington* to *St Ives*, *Erith*, between *Wiſbich* and *Downham* to *Lynn*, and ſo on to *Suetſham*: We have heard nothing of it farther to the S W than *Huntington*, nor farther N E than *Downham*. Very few Trees eſcaped: The Barns that ſtood the Storm, had all their Roofs more damaged to the Leeward Side than to the Windward. We are in great Hopes the Storm was not general; I am apt to think it was much ſuch a Storm as ran through *Suſſex* about 10 Years ago. The Storm was ſucceeded by a profound Calm, which laſted about an Hour; after which the Wind continued pretty high, till 10 o'Clock at Night.

Concerning a terrible Whirl-wind, which happened at Corne Abbas *in* Dorſetſhire, Oct. 30. 1731. *by Mr* J. Derby. No. 454. p. 229. July, *&c.* 1739.

XXI. About a Quarter before one in the Night, there happened a very ſudden and terrible Wind Whirl-puff, as I call it: Some ſay it was a Water-ſpout, and others a Vapour or Exhalation from the Earth; but be it of what Name it will, it began on the S W Side of the Town, carrying a direct Line to the N E, croſſing the Middle of the Town in Breadth 200 Yards. It ſtripped and uncovered tiled and thatched Houſes, rooted Trees out of the Ground, broke others in the midſt of at leaſt a Foot ſquare, and carried the Tops a conſiderable way. The Sign of the new Inn, a Sign of 5 Foot by 4, was broken off 6 Foot in the Pole, and carried croſs a Street of 40 Foot Breadth, and over an oppoſite Houſe, and dropped in the Backſide thereof. It took off and threw down the Pinacles and Battlements of one Side of the Tower; by the Fall of which, the Leads and Timber of great Part of the North Alley of the Church was broken in. The Houſes of all the Town were ſo ſhocked, as to raiſe the Inhabitants; no hurt was done but only acroſs the Middle of the Town in a Line. No Life loſt, but three had a very providential Eſcape. 'Tis computed by judicious Workemen, that the Damage ſuſtained by this Accident amounts to 258 Pounds, and upwards. It is very remarkable, it only affected, as I have related: no other Parts of the Neighbourhood or Country ſo much as felt or heard it. It is ſuppoſed by the moſt Judicious, that it began and ended within the Space of 2′. It was ſo remarkably calm a Quarter after 12, that the Exciſeman walked through 2 Streets, and turned a Corner, with a naked lighted Candle in his Hand, unmoleſted and undiſturbed by the Air; and as ſoon as over, a mighty Calm, but ſoon followed by a prodigious violent Rain. *Mintern Magna, Nov.* 13. 1731.

Concerning the Cause of the General Trade-Winds: By Geo. Hadley, *Esq; F. R. S.* No. 437. p. 58. April &c. 1735.

XXII. I think the Causes of the General Trade-winds have not been fully explained by any of those who have written on that Subject, for want of more particularly and distinctly considering the Share the diurnal Motion of the Earth has in the Production of them: For although this has been mentioned by some amongst the Causes of those Winds, yet they have not proceeded to shew how it contributes to their Production; or else have applied it to the Explication of these Phænomena, upon such Principles as will appear upon Examination not to be sufficient.

That the Action of the Sun is the original Cause of these Winds, I think all are agreed; and that it does it by causing a greater Rarefaction of the Air in those Parts upon which it's Rays falling perpendicularly, or nearly so, produce a greater Degree of Heat there than in other Places; by which means the Air there becoming specifically lighter than the rest round about, the cooler Air will by it's greater Density and Gravity, remove it out of it's Place to succeed into it itself, and make it rise upwards. But it seems, this Rarefaction will have no other Effect than to cause the Air to rush in from all Parts into the Part where 'tis most rarefied, especially from the N and S, where the Air is coolest, and not more from the E than the W, as is commonly supposed: So that, setting aside the diurnal Motion of the Earth, the Tendency of the Air would be from every Side towards that Part where the Sun's Action is most intense at the Time, and so a N W Wind be produced in the Morning, and a N E in the Afternoon, by Turns, on this Side of the Parallel of the Sun's Declination, and a S W and S E on the other.

That the perpetual Motion of the Air towards the W, cannot be derived meerly from the Action of the Sun upon it, appears more evidently from this: If the Earth be supposed at Rest, that Motion of the Air will be communicated to the superficial Parts, and by little and little produce a Revolution of the Whole the same Way, except there be the same Quantity of Motion given the Air in a contrary Direction in other Parts at the same Time, which is hard to suppose. But if the Globe of the Earth had before a Revolution towards the E, this by the same means must be continually retarded: And if this Motion of the Air be supposed to arise from any action of the Parts of it on one another, the Consequence will be the same. For this reason it seems necessary to shew how these Phænomena of the Trade-Winds may be caused, without the Production of any real general Motion of the Air westwards. This will readily be done by taking in the Consideration of the diurnal Motion of the Earth: For, let us suppose the Air in every Part to keep an equal Pace with the Earth in it's diurnal Motion; in which Case there will be no relative Motion of the Surface of the Earth and Air, and consequently no Wind; then by the Action of the Sun on the Parts about the Equator, and the Rarefaction of the Air proceeding therefrom, let the Air be drawn down thither from the N and S Parts. The Parallels are each of them bigger than the other, as they approach to the Equator, and the Equator is bigger

bigger than the Tropicks, nearly in the Proportion of 1000 to 917, and consequently their Difference in Circuit about 2083 Miles, and the Surface of the Earth at the Equator moves so much faster than the Surface of the Earth with it's Air at the Tropicks. From which it follows, that the Air, as it moves from the Tropicks towards the Equator, having a less Velocity than the Parts of the Earth it arrives at, will have a relative Motion contrary to that of the diurnal Motion of the Earth in those Parts, which being combined with the Motion towards the Equator, a N E Wind will be produced on this Side of the Equator, and a S E on the other. These, as the Air comes nearer to the Equator, will become stronger, and more and more Easterly, and be due East at the Equator itself, according to Experience, by reason of the Concourse of both Currents from the N and S where it's Velocity will be at the rate of 2083 Miles in the Space of one Revolution of the Earth or Natural Day, and above 1 Mile and $\frac{1}{3}$ in a Minute of Time; which, is greater than the Velocity of the Wind is supposed to be in the greatest Storm, which, according to Dr *Derham*'s Observations, is not above 1 Mile in a Minute. But it is to be considered, that before the Air from the Tropicks can arrive at the Equator, it must have gained some Motion Eastward from the Surface of the Earth or Sea, whereby it's relative Motion will be diminished, and in several successive Circulations, may be supposed to be reduced to the Strength it is found to be of.

Thus I think the N E Winds on this Side of the Equator, and the S E on the other Side, are fully accounted for. The same Principle as necessarily extends to the Production of the West Trade-winds without the Tropicks; the Air rarefied by the Heat of the Sun about the Equatorial Parts, being removed to make room for the Air from the cooler Parts, must rise upwards from the Earth, and as it is a Fluid, will then spread itself abroad over the other Air, and so it's Motion in the upper Regions must be to the N and S from the Equator. Being got up at a Distance from the Surface of the Earth, it will soon lose great Part of it's Heat, and thereby acquire Density and Gravity sufficient to make it approach it's Surface again, which may be supposed to be by that Time 'tis arrived at those Parts beyond the Tropicks where the westerly Winds are found. Being supposed at first to have the Velocity of the Surface of the Earth at the Equator, it will have a greater Velocity than the Parts it now arrives at; and thereby become a westerly Wind, with Strength proportionable to the Difference of Velocity, which in several Revolutions will be reduced to a certain Degree, as is said before, of the Easterly Winds, at the Equator: And thus the Air will continue to circulate, and gain and lose Velocity by Turns from the Surface of the Earth or Sea, as it approaches to, or recedes from, the Equator. I do not think it necessary to apply these Principles to solve the Phænomena of the Variations of these Winds at different Times of the Year, and different Parts of the Earth; and to do it would draw this Paper into greater Length than I propose. From what has been said it follows:

First,

First, That without the Assistance of the diurnal Motion of the Earth, Navigation, especially Easterly and Westerly, would be very tedious, and to make the whole Circuit of the Earth perhaps impracticable.

Secondly, That the N E and S E Winds within the Tropicks must be compensated by as much N W and S W in other Parts, and generally all Winds from any one Quarter must be compensated by a contrary Wind some where or other; otherwise some Change must be produced in the Motion of the Earth round it's Axis.

Observations on falling Dew, made at Middleburg *in* Zeeland *on a leaden Platform, in the Night between* July 25 *and* 26. 1741. *N. S. with Figures of the flakes of Snow observed* Jan. 1742. *By* Leonard Stocks, *M. D.* No. 464. p. 112. *Read* May 20. 1742.

XXIII. *July* 25. at Noon, the Height of the Barometer was 29 *Rhynland* Inches and 2 ¼ Lines. The Height of *Fahrenheit*'s Thermometer was 70 Degrees. The Sky was clear, and the Wind blew gently from the westward. But at the Time of the Observations, from 10 to the first Hour of the Night, the Height of the Barometer was 29 Inches 2 Lines, of the Barometer about 60 Degrees, there being hardly any Wind stirring, and the Sky also being clear.

There fell much Dew upon Glass of various Kinds, so that it was wet all over.

On polished Brass but little and only a thin Vapour.

On rough unpolished Brass a little more.

On Iron tinned (in *Dutch Blick*) a little: On the same of a blue Colour, much: On the same rough very much: On the same smooth hardly any: On the same rusty, none.

On pure Quicksilver none.

On smooth Tin none.

On rough Lead much: On polished Lead a little.

On white Silver none: On polished Silver none: On Silver gilded none.

On blue Porcelaine none.

On a stone Slab much.

On a Basket made of Indian Cane finely woven, a little.

On a smooth Plank of Oak, of a white Colour, very much: On the same of a black Colour, much less.

A smooth Plank of Firr was only just moist.

A smooth Plank of white Deal had very little Dew.

On moving those Bodies, which received much Dew, a little higher, 2 or 3 Inches above the leaden Platform, the Lead dried, and the Bodies themselves were wet underneath as well as above; but the Tin and Silver, being placed in like manner, continued dry, tho' the Place, which was bedewed before, dried up.

Fig. 23. *Jan.* 2. 1742. *N. S.* There fell Flakes of Snow early in the Morning partly formed like Fig. 23. Their Diameters from the extremities of their Points were ¼ of a Line.

Fig. 24. *Jan.* 10. before Noon Fig. 24. ¾ of a Line in Diameter, in the middle of which was a hexagonal Rose, as in Fig. 23. The oval Figures were empty.

Jan.

Jan. 20. About Noon Fig. 25. 1 Line in Diameter, and Fig. 26. 1 ¼ Line in Diameter. The laſt ſhone like *Muſcovy* Glaſs. Fig. 25. Fig. 26.

New Experiments upon Ice; taken from Abbé Nolet, *F. R. S. at* Paris, *and communicated by* J. T. Deſaguliers, *F.R.S.* No. 449. p. 307. Aug. &c. 1738.

XXIV. 1. Ice that begins to melt, and Water that begins to freeze, have always the ſame Degree of Cold.

2. That Cold may be increaſed by a Mixture of Salts.

3. It has been thought for a long time, that *Salt-petre* was moſt fit to increaſe the Cold of *Ice*; but Experiments have ſhewn, that few Salts increaſe Cold ſo little as that Salt. Mix one Part of fine *Salt-petre* with two Parts of beaten *Ice*, and M. *Reaumur*'s Thermometer will deſcend in it but 3 ½ Degrees below the freezing Point.

What had cauſed this Miſtake, is, that People generally made uſe of *Salt-petre* of the firſt or ſecond melting, as being the cheapeſt; but that *Salt-petre* not being purified, contains a great deal of *Sea-Salt*; and it was in Proportion to the Quantity of the *Sea-Salt* that the Effect was the greater.

From this laſt Obſervation, one may deduce an advantageous Method for trying *Gunpowder*; for as of the three Ingredients of which it is made up, *Salt-petre* is the only one that can increaſe the Cold of *Ice*; if one Part of *Gunpowder*, or a little more, be mixed with two Parts of *Ice*, and it increaſes it's Cold more than 3 ½ Degrees, it is a Sign that the *Salt-petre* contained in it is not well purified; and the beſt Powder will be that which does leaſt increaſe the Cold of *Ice*.

4. *Sea-Salt*, that is the *Bay-Salt*, which is commonly uſed at Table in *France*, and that which is immediately taken from the Mines, called *Sal gemmæ*, give the greateſt Degree of Cold, for the moſt part; for *Pot-aſh* gives ſometimes a little more, but generally leſs. *Sea-Salt* mixed with *Ice* in the aboveſaid Proportion, gives 15 Degrees of Cold on M. *Reaumur*'s Thermometer, and *Sal-gem.* 17.

		Degrees.
5.	*Aſhes* of *green Wood* — — — — — — — —	3
6.	of *Sea-Coal* — — — — — — — — —	
7.	of *Vitriol* — — — — — — — — —	2
8.	*Tartar* — — — — — — — — —	10
9.	Common *Pot-aſh* (in *French* called *Soude ordinaire*)	3
10.	*Pot-aſh* made of *Vreck* or *Sea-weed* — — —	11

This laſt *Pot-aſh* may be ſubſtituted inſtead of *Sea-Salt*, for making *Ice-Creams*, in Places where *Salt* is dear, as in *France*, where it is ſold for 10 Sols a Pound.

1*ſt*, Becauſe in *France* this *Pot-aſh* is ſold only for 2 ½ Sols a Pound.

2*dly*, Becauſe, not freezing ſo faſt, it does not ſpoil the Creams by reducing them to Icicles.

3*dly*, Becauſe *Ice-Creams* made this way, will keep longer in a Condition fit to ſerve at Table.

		Degrees.
11.	*Sugar* — — — — — — — — — — —	4
12.	*Allom* — — — — — — — — — — —	1 ½

13. *Salt*

	Degrees.
13. *Salt of Glass*	10
14. *Sal Ammoniac*	12 $\frac{1}{3}$
15. *Quick-Lime*	1 $\frac{1}{4}$
16. *Sal Glauberi*	2

17. The Cold of *Ice* may still be considerably increased by a Mixture of *Spirit of Wine*; about a Drinking-glass full of *Spirit of Wine* to a Pound of beaten *Ice*.

18. The Cold of *Ice* will not increase, unless the *Ice* melts.

Experiments. Put into one Vessel ℥iv of *Ice* beaten very small, and into another Vessel ℥ij of *Sea-Salt*; set the two Vessels in a Mixture of *Ice* and *Salt*, which is to be renewed still, till by Means of the Thermometer you find, that the *Salt* and the *Ice* of the two first Vessels have acquired each of them 10 or 12 Degrees of Cold; then mix your *Salt* with your *Ice*, and this Mixture will not increase the Degree of Cold that the Ingredients had acquired, because the Mixture does not melt.

But if instead of *Salt* you mixed with your *Ice Spirit of Nitre*, cooled to the same Degree as the *Ice*, as this last is liquid, it will melt the *Ice*, and considerably increase it's Cold.

19. Salt mixed with Water, increases it's Cold.

20. Of all Salts, *Sal Ammoniac* gives the greatest Degree of Cold; so that if that Salt has been cooled in *Ice*, and then one Part of it be thrown into two Parts of Water cooled to the same Degree in *Ice*, that Water will become colder than *Ice*, and will freeze other Water thrown into it in a small Quantity.

This last Observation may be applied to the cooling of Liquors where no *Ice* is to be had; for there is hardly any Place but what has Wells: Now the Water of a Well moderately deep, wants about 8 or 10 Degrees of the Cold of *Ice*; and *Sal Ammoniac* being cooled beforehand in the Well, will, by mixing with some of the Water of that Well, come very near to the Cold of *Ice*.

An Account of an extraordinary Effect of Lightning in communicating Magnetism. By Dr Cookson *of* Wakefield *in* Yorkshire. No. 437. p. 74. Apr. *&c.* 1735.

XXV. 1. A Tradesman in this Place having put up a great Number of Knives and Forks in a large Box, some in Cases or Sheaths, and others not, of different Sizes, and of different Persons making, in order to be sent beyond Sea; and having placed the Box in the Corner of a large Room, there happened a sudden Storm of Thunder, Lightning, *&c.* by which the Corner of the Room was damaged, the Box split, and a good many Knives and Forks melted, the Sheaths being untouched. The Owner emptying the Box upon a Counter where some Nails lay, the Persons who took up the Knives that lay upon the Nails, observed that the Knives took up the Nails. Upon this the whole Number was tried, and found to do the same, nay, to such a Degree as to take up large Nails, Packing-Needles, and other Iron Things of considerable Weight. Needles or other Things placed upon a Pewter-Dish, would follow the Knife or Fork, though held under the Dish, and would move along as the Knife or Fork was moved; with several other odd

odd Appearances, which I won't now trouble you with, only this, that though you heat the Knives red-hot, yet their Power is ſtill the ſame when cold.

You may be aſſured of the Truth of this, having myſelf made a good many Trials of the Knives and the Forks: How they came by this magnetick Power, or how Lightning ſhould be capable of communicating ſuch a Power, is the *Query*.

Decem. 6, 1732.

A further Account of the extraordinary Effects of the ſame Lightning at Wakefield. *By the ſame.* Ibid. p. 75.

2. This Storm of Thunder and Lightning happened the latter End of *July*, 1731, and not only broke the Glaſs and Iron Frames of the Croſs-Chamber Windows, but at the ſame Time ſplit ſome Studds in the Corner of a Wood-Houſe; and paſſing into a Room, ſplit likewiſe a large Deal Box, which ſtood in the S. Corner of the Room, where the Lightning entered, and diſperſed a great many Dozen of Knives and Forks, which were put up in the Box, all over the Room.

Upon gathering up theſe Knives and Forks, ſome of them were melted, others ſnapped in ſunder; others had their Hafts burnt; others their Sheaths either ſinged or burnt; others not: But what was moſt remarkable, upon laying them on a Counter where there were Iron Nails, Rings, *&c.* it was obſerved, that when any of them were taken up, there hung a Nail or Ring at the End of each of them: Moſt of them were tried, and found to do the ſame; but little further Notice being taken of them at that Time, they were thrown aſide as damaged Goods.

Some Diſcourſe concerning the Effects of Thunder and Lightning happening to be the Subject of Converſation in a Company, where the Owner of theſe Knives was not long ago, he told them what had happened at his Houſe, and particularly to the Knives and Forks; and being aſked whether he had any of them left, ſaid that he had; and upon Trial, it was found that a good many of them were poſſeſſed of this magnetick Virtue.

Hearing of this, I went and found what was related, and what I ſent you an Account of before, to be Fact, and have now ſent you a couple of Knives and Forks, one for yourſelf, another for the *Royal Society*.

The Whittle-Knife, with the Box-Handle, is that which I would have you preſent to the Society; it is an excellent one, and one of the beſt: I had Thought to have kept it myſelf; but if it will be acceptable to that famous Body, it will be much more agreeable to me. They will perceive that it has been uſed pretty much; and the Owner's Son, who gave it to me, told me, that he has made uſe of it for almoſt a Year and an half to all Manner of Purpoſes; notwithſtanding which it ſtill retains the magnetick Virtue to an extraordinary Degree.

The Situation of the Room, Poſition of the Box and Knives, and Direction of the Lightning, may poſſibly contribute to a fuller Idea of the Matter: I ſhall therefore preſume to give a Plan of them, an Horizontal one, and ſubmit it and the following *Queries* to the Conſidera- of that learned Body.

Fig. 27. A. The South Angle of the Room where the Lightning entered.

B. The Direction of the Lightning.

C. The polar or magnetick Line.

D. The Box with the Knives lying in a Direction parallel to the longer Sides of the Box.

Query, Whether the Knives and Forks lying in ſuch a Direction as either to coincide, or make but an acute Angle with the magnetick Line, might any Ways contribute to their imbibing this magnetick Virtue; ſince a Bar of Iron placed in ſuch a Direction, ſhall in a ſmall Time receive a tranſient Polarity, and if it continue a long Time in that Poſition, a fixed and permanent one?

Query, Whether the Knives and Forks lying in ſuch a Poſition, and being violently heated by the Lightning, might not, as they cooled, ſtrongly imbibe this magnetick Virtue; ſince a Bar of Iron heated and placed in a certain Direction to cool, will ſooner imbibe this Power than in the ſame Direction cold?

Query, The Polarity of the Compaſs has been altered by Lightning, as it is to be ſeen in the *Philoſophical Tranſactions*: Now how ſhould Lightning be capable of communicating ſuch a Power in this Caſe, ſince it is plain that it has taken it away in another?

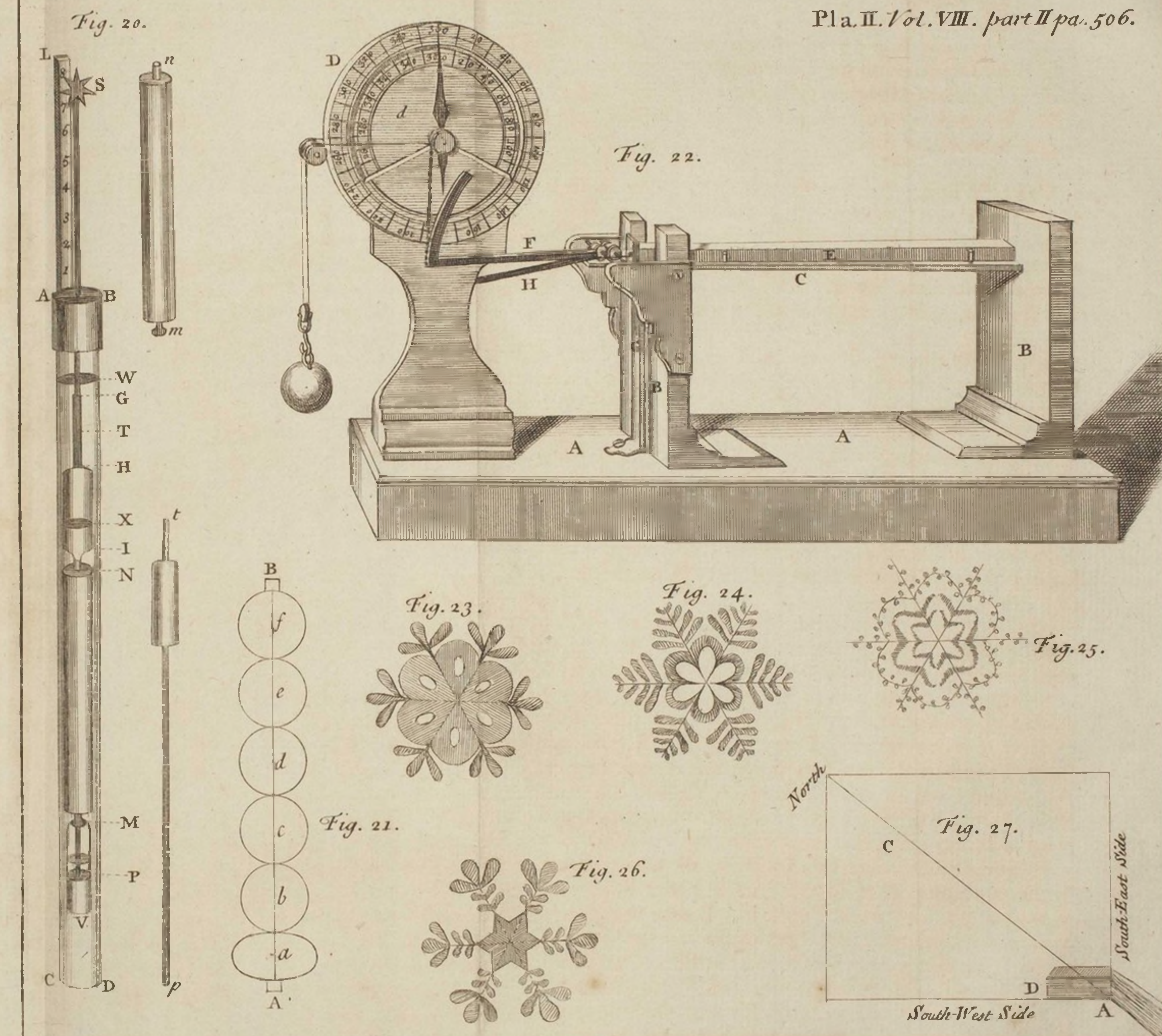
Concerning a File rendered magnetical by Lightning. By M. de Bremond, *M. D. Tranſlated from the* French *by* T. S. *M. D. F. R. S.* No. 459. p. 614. Jan. &c. 1741.

3. I have received a Letter from the Coaſt of *St André* in *Dauphiné*, dated *Sept.* 7, 1739, giving an Account of a Fact of the ſame Nature, with the preceding, which I here ſend you as I received it.

" Three Weeks ago the Lightning fell 30 Paces from my Houſe on " that of a Clock-maker. I ſhall not enter into the Particulars of the " Ravage it committed. Every Body knows how ſurpriſing the Effects " of Thunder are: But here is one that is very ſingular. The Thun- " der broke one of the Clock-maker's Files, 4 Inches from the End; " ſo that there ſtill remained 7 Inches of it in the Handle; and the Piece " of 4 Inches long, that was broken off, remained on the Shop-board.

" The next Day after the Accident, the Clock-maker, obſerving that " the remaining Part of this File might ſtill be of Service to him, " took it up, and worked with it. But he was much ſurpriſed to ſee, " that Iron followed the End of his broken File. He applied this " End to a Punch, (or Drill) and the Punch was immediately attracted " to the File. He called to me, and I made ſeveral Trials of this at- " tractive Quality. I took the Piece of the File that had been broken " off, and applied it to an Iron Ring for hanging Keys; which it

" lifted


Pl a. II. Vol. VIII. part II pa. 506.
Fig. 20.
Fig. 21.
Fig. 22.
Fig. 23.
Fig. 24.
Fig. 25.
Fig. 26.
Fig. 27.
North
South-East Side
South-West Side

" lifted up perfectly well, and held suspended as long as I thought " proper. I doubted not but it was the Lightning that had communi- " cated a magnetick Quality to this File; and I found, upon Trial, " that this Quality was given only to the Inside of the File, and the " broken Piece; for I applied Bits of Iron to every Side of it, with- " out any Effect, the Virtue residing no-where but in the Place that " was broken. I broke in two the same Piece of 4 Inches; and one " of the two Pieces attracted Iron at both Ends, the other only at it's " broken End. I rubbed the Point of my Knife on one of these two " Bits of the File, and it communicated to my Knife a Degree of " Magnetism sufficient to raise Needles, and hold them suspended."

Concerning the crooked and angular Appearance of the Streaks or Darts of Lightning in Thunder-Storms. By James Logan, *Esq; Dated* Philadelphia, Sept. 20, 1735. No. 441. p. 240.

XXVI. Looking into the second Volume of that ingenious Gentleman *Stephen Hales*'s Staticks, of which I have lately happened to have a cursory View, I observed him to mention that Phænomenon of the Streaks or Darts of Lightning in Thunder-Storms appearing crooked and angular, (I do not remember his Words) as a Thing not yet accounted for; and therefore he guessed at a Solution of it *.

The Clouds are generally distinct Collections of Vapours like Fleeces, and therefore the Rays of Light through them must pass through very different Densities, and accordingly suffer very great Refractions: From thence therefore, undoubtedly, that Appearance must arise: For it is most highly absurd to imagine, that Fire, darted with such a Rapidity, can from any assignable Cause deviate in Fact from a right Line, in the Manner it appears to us. And this, if duely considered, may probably be found a plenary Solution.

Extraordinary Effects of Lightning, by Sir John Clark, *one of the Barons of the Exchequer in* Scotland, *and F. R. S. Dated* Nov. 6. 1731. No. 454. p. 235.

XXVII. I was lately in *Cumberland*, where I observed in *Whinfield-Park*, belonging to the Earl of *Thanet*, a huge Oak, at least 60 Foot high, and four in Diameter, upon which the last great Thunder had made a very odd Impression; for a Piece was cut out of the Tree about 3 Inches broad, and 2 Inches thick, in a strait Line from Top to Bottom. In another Tree of the same Heighth, the Thunder had cut out a Piece of the same Breadth and Thickness, from Top to Bottom, in a spiral Line, making 3 Turns about the Tree, and entering into the Ground above 6 Foot deep.

Concerning some extraordinary Effects of Lightning, by the Right Hon. Robert James *Lord* Petre, *F. R. S.* No. 464. p. 136. *Dated* June 24, 1742.

XXVIII. On *Tuesday* Morning, between 3 and 4, we had at *Thorndon* some of the most terrible Thunder I ever heard; and, indeed, by the Effects of it, I have Reason to conclude, that it was very near us, as well as by the Noise, to which I really think no other Thunder I ever yet had any Notion of, could be compared. It has beat down a Chimney at a Farm-house just by, and the Lightning has also struck two large Oaks in my Park, which stand about 40 or 50 Feet apart. In one of them I do not observe any thing much different from other Trees which I have before seen struck with Lightning; the only Thing that seems remarkable, is, that the greatest Damage appears to be done

* See *Steph. Hales*'s Statical Essays, Vol. 2, p. 291.

to the Eaſt Side of the Tree, although it is certain, that the Storm all came from the S W. This Tree is extremely ſhattered, and ſplit from the Top to the Bottom; and on the S W Side, juſt by the Root, there is a large Hole made in the Ground, about 6 or 7 Inches Diameter, and about a Foot or 15 Inches deep. But in the other Tree, I think, there is ſomething more particular; for there, without ſhattering or ſplitting the Tree in the leaſt, or ſo much as diſturbing a ſingle Branch, although there are a great many upon it, the Lightning has taken off the Bark about 5 Inches wide, in a complete ſpiral Line, from about 40 Feet high, down to within about a Foot of the Ground, where the Width diminiſhes to about 2 Inches, and ſo goes quite off: In the Centre of theſe 5 Inches, it has entered the Wood about $\frac{3}{4}$ of an Inch deep, and about 1 and $\frac{1}{2}$ Inch wide: This Hollow it has in great Part cleared out entirely, and the reſt is left hanging like Pieces of broken or untwiſted Ropes; this Hollow alſo diminiſhes near the Ground, and dies quite out exactly at the Ground: The ſpiral Line is exactly regular, and goes juſt once round the Tree, or but very little more, and, as near as I can obſerve, is exactly of an equal Width all the Way. The Surface of the Bark of both the Trees is remarkably touched for about 10 Feet from the Ground, as if it were ſhot all over with ſmall Shot, each of which ſeems to have taken off little Scales or outſide Pieces of the Bark, from 1 $\frac{1}{2}$ Inch or 2 Inches broad and long, to $\frac{1}{4}$ of an Inch.

A Halo obſerved at Rome, Aug. 11, 1732, *by the Abbot* Didacus de Revillas. No. 438. p. 118. July, *&c.* 1735.

XXIX. A ſimple Halo, terminated on all Sides, and exactly circular encompaſſed the Sun from 9 in the Morning till 2 in the Afternoon. The Breadth of the Zone ſeemed to equal the apparent Diameter of the Sun. The inmoſt Colour was red, the others paler, and analogous to thoſe of the Rainbow, but ending in white, and ſomewhat altered about Noon. The Sky was a little hazy, and there was a gently N Wind. The Miſt thickened afterwards into whitiſh Clouds, whilſt the Halo diſappeared, the Diameter of which, from the inner Edge of the Zone, was 45°.

Height of the Barometer	*Paris* Inches.	
Eight in the Morning — — — — — —	27	11
Noon — — — — — — — — —	27	10 $\frac{1}{2}$
Two in the Afternoon — — — — — —	27	10 $\frac{1}{2}$

Obſervations of two Parhelia, or Mock-Suns, ſeen Dec. 30, 1735, *by the Rev. Mr* Timothy Neve, *Secretary of the Gentlemen's Society at* Peterbo-

XXX. On *Tueſday, Dec.* 30, 1735, as I was riding betwixt *Cherry Orton* and *Alwalton* in the County of *Huntingdon*, I obſerved two *Parhelia*, the firſt of which ſhone ſo bright, that at firſt Sight I took it for the real Sun, till looking a little farther on my Left-hand, I was convinced of my Miſtake, by ſeeing the true Sun much the brighteſt in the Middle, and a *Mock-Sun* on each Side, in a Line exactly parallel to the Horizon. I gueſſed their Diſtance to be about 40 Diameters of the Sun, or, as they uſually appear, 23 Degrees. That on the Left-hand of the Sun, when I ſaw it firſt, was ſmall and faint, but in about 2′

Time

Time became as large and bright as the other, and appeared at once as two white lucid Spots on each Side the Sun, Eaſt and Weſt, ſeemingly as big, but not ſo well defined: In about 3′ they loſt both their Colour and Form, and put on thoſe of the Rainbow; the Red and Yellow in both very beautiful and ſtrong neareſt to the Sun, the other Colours fainter. They became as two Parts of an Arch, or Segment of a Circle, with the Concave towards the Sun, only round at Top, the Light and Colours ſtreaming downwards and tending towards a Point below. This continued for about 4 or 5′, when the Colours gradually diſappearing, they became, as before, two lucid Spots, without any Diſtinction of Colours. They laſted a full Hour, ſometimes one brighter, and ſometimes the other, according to the Variation of the Clouds and Air, as I ſuppoſe. When I firſt ſaw it, it was exactly a Quarter after 11. There had been a Froſt in the Morning, which went away pretty ſoon with a thick Miſt, and between 10 and 11 cleared up, leaving only a Hazineſs in the Air behind it: The Weather quite calm, Wind, as I thought, N W.

rough. No. 445. p. 52. Jan. &c. 1737.

Theſe *Parhelia* commonly are ſeen with a Circle or Halo round the Sun, concentrical to it, and paſſing through the Diſks of the ſpurious or *Mock-Suns*. But there was not the leaſt appearance of ſuch a Circle here, it having only a Tendency towards one, when it was ſeen with the Rainbow Colours.

An Obſervation of two Parhelia, or Mock-Suns, ſeen at Wittemberg *in* Saxony, Dec. 31, 1735. *O.S. By* John Frid. Weidler, *F. R. S. &c. Tranſlated from the Latin by* T. S. *M.D. F. R. S. Ibid* p. 54. Fig. 28.

XXXI. This Day, a little after 10 in the Morning, a Friend told me, that ſeveral Suns were to be ſeen in the Heavens: Whereupon I went directly into the Garden adjoining to my Houſe, and immediately ſaw near the Sun S, on it's Left or Weſtern Side (1) the *Parhelion* B, as big as the true Sun. This *Mock-Sun* (2) was amidſt little, round, white Clouds, ſet thick, and cloſe to one another. (3) The Part of the *Parhelion* which faced the Weſt was not round, but broken, having about a third Part of it's Circumference open, and ſhooting out the long bright Stream or Tail BH. (4) To this, both above and below, adhered another Stream F G (5) ſomewhat curved, (6) with it's Horns turned from the Sun Weſtward. (7) The Middle of this *Mock-Sun* ſhone with ſo great a Light, that the naked Eye could not bear it; wherefore I viewed it attentively through a Glaſs darkened with the Smoak of a Wax-candle. (8) The Light of the *Parhelion* B appeared much weaker than that of the true Sun. (9) It's Circumference which faced the Sun, was red: Likewiſe (10) that Part of the Stream FG, which was towards the Sun, was purple. Within the red Border appeared the other Colours of the Rainbow, as yellow, green, and azure. And the Stream BH was likewiſe embelliſhed with red and yellow. Both Edges of this were reddiſh, and it's Middle yellowiſh. (11) The Sun S, was 15½° above the Horizon; and it's Image B was near the ſame Altitude, for I then found it to be 14°. (12) I meaſured the Diſtance from S to B, more than once, and found it to be 20°. (13) The Arch F G was near 6° in Length. (14) Moſt of the South part of the Hemiſphere was

overſpread

overſpread with white Clouds, interſperſed here and there with ſome darker ones. There were ſome thin Clouds before the true Sun, through which it's Rays eaſily paſſed. (15) When thicker Clouds ſurrounded the Sun, the Brightneſs of the *Parhelion* was leſſened. (16) The *Parhelion* was now and then hid by dark Clouds. (17) The thin white Clouds, with which the Northern Part of the Sky was overſpread, reached up to the *Zenith*. (18) Soon after my firſt obſerving the *Parhelion* B, as I looked up to the *Zenith*, I ſaw the beautiful Rainbow C D E parallel to the Horizon, with it's Horns turned to the North. It had the uſual Colours of the Rainbow, all very diſtinct. The Purple was on the Side facing the Sun; next to it was the Yellow, then the Green, and laſt the Azure. (19) A Line drawn from the Sun's Centre to the Middle D of the *Iris* tended to the *Zenith*, and was a Portion of that vertical Circle, in which the Sun then was. (20) The Point D was 61° diſtant from the Horizon; wherefore the Diameter of the Rainbow was 58°: (21) However, there was but Part of the Rainbow C D E ſeen, the Ends of which were ſometimes but 38° from one another: For more or leſs of it appeared at different Times, but ſcarcely above $\frac{1}{4}$ Part of it's Circumference at any Time. (22) It was ſometimes ſeen among ſmall white Clouds, which were about the *Zenith*, and ſometimes in a clear Sky. It laſted till the Sun and moſt part of the Sky was overcaſt by thick Clouds. (23) The Thickneſs of the Rainbow C K, as well as I could eſtimate by the bare Eye, was one Degree of a great Circle.

But as the neighbouring Houſes prevented my having a free Proſpect Eaſtward from my Garden, I went to another Place, whence I had a full View of the Hemiſphere. And having reached thither a little before 11, I immediately ſaw another *Parhelion* A to the Eaſt, (24) 20° from the Sun, as the foregoing was, and raiſed 15° above the Horizon. (25) This *Mock-Sun* was not inferior to the other B, in Brightneſs, for the naked Eye could no more bear it than that. (26) It's Light was white; (27) it's Figure round, and it's Size equal to that of the Sun S. (28) This *Parhelion* A, ſhot out the Stream I L, which was rectilinear, white and reſplendent, 8° long, and, as far as I could poſſibly diſcover, void of Colours; (29) for it appeared among ſmall white broken Clouds, and laſted ſomewhat longer than the former, without changing it's Figure. (30) Upon the Sun's being hid by thick Clouds about half an Hour after 11, both theſe *Mock-Suns* diſappeared, but became viſible again, upon the Sun's ſhining bright.

The Whole of the *Phænomena* obſerved in theſe *Parhelia* comes to this: That the true Sun, S, was accompanied by two *Parhelia*, both 20° diſtant from the Sun, one on each Side, and having nearly the ſame Altitude with the Sun from the *Horizon*. Above the *Parhelia*, Part of a Rainbow ſurrounded the *Zenith*; and each of the *Parhelia* ſent forth a bright luminous Stream or Tail, one rectilinear and white, the other ſomewhat curved and coloured. Moreover, from the weſtern *Parhelion*, a Stream parallel to the *Horizon*, and ſomewhat pointed, extended itſelf

on

on the Side opposite to the Sun; and this Scene lasted the two Hours of 10 and 11 before Noon, until thick Clouds put an end to it. There was no Appearance of an entire Crown, such as usually accompanies *Parhelia*, and encircles the Sun; although I observed the Track of the Sky near the Sun, both with the naked Eye and through Glasses.

As to the State of the Heavens on the 11th of *January*, when the *Parhelia* were observed; early in the Morning a thick Fog overspread the *Horizon*; about nine this Fog condensed into small Drops of Rain, which fell slowly: Soon after, the Vapours were collected into thin Clouds, particularly in that Part about the Sun. Then the Sky became clear about the North, and there blew a gentle Wind a little to the South of the East.

After Noon, Clouds gathered to the West; about 30 Minutes after 12, the whole Hemisphere was overcast, but in the Evening it became clear and serene on all Sides. On the following Days, from the 12th to the 17th of *January*, N. S. the Sky was constantly cloudy or dark, and the Sun seldom seen thro' the breaks of the Clouds. On the 18th Day, the Weather cleared up, which lasted three Days. On the 21st, that I am writing these Observations, the whole Surface of our Hemisphere is overcast with Clouds; and therefore this Appearance of *Parhelia* has not been attended with any uncommon Weather.

N. B. The Publisher having sent Mr Professor *Weidler* an Account of Mr *Neve's* Observation of the *Mock Suns*, seen by him in *England*, which seem to agree in so many Circumstances with those seen by the other in *Germany*: The Professor saith in his Answer, "That it seems to him very worthy of Remark, that *Parhelia*, so "very much alike, should appear two subsequent Days in Places so distant from each "other; which indicates a similar State of the Air or Atmosphere in both." This extraordinary Incident put the Professor upon writing an Essay on the Cause of *Parhelia*, and accordingly he hath published a small Pamphlet in Quarto, intituled, *Jo. Friderici Weidleri* Commentatio de *Parheliis* mense *Januarii* Anno M.DCC.XXXVI. prope *Petroburgum Angliæ*, & *Vitemburgæ Saxonum* visis. Accedit de *Rubore Cæli* igneo mense *Decembri* Anno 1737. observato *Corollarium*. *Vitembergiæ*, 1738. in 4to. *See* §. 33. *of this* Chap.

XXXII. As I was reading this Morning a little after 7, in a Room looking towards the N E, I accidentally took Notice of an odd Stream of coloured Light, shooting upwards from the Sun, as I then thought, shining through a thin waterish Cloud; but recollecting the Appearance was several Degrees more northerly than the Sun's true Place at that Time, I immediately went to the Window, and found what I had taken for the Sun was a *Parhelion* shooting out a short horizontal Stream or Tail towards the North; the Sun itself shining pretty bright and clear at the same Time. I also observed, the Stream I had at first seen, was part of an Arch concentric to the Sun, and passing through the *Parhelion*: This Arch was for a good Way tolerably defined, and tinged with Red on the Inside, and a blueish White on the other. I then cast my Eye to the other Side the Sun, where I perceived a second *Parhelion*, at the same Distance from him, towards the South, tho' not yet so bright as the

An Observation of three Mock-Suns *seen in* London, Sept. 17. 1736. *by* Martin Folks, *Esq; V. Pr.* R. S. *Ibid.* p. 59. Fig. 29.

first

firſt. I then went up to my Leads, my Proſpect being too confined below: Where ſoon after I was come, I found the *Phænomenon* conſiderably to improve, the Arch round the Sun forming itſelf into more than a Semicircle, reaching almoſt to the *Horizon* Northward, and with very little Diſcontinuance beyond the ſecond *Parhelion* towards the South. I then began to perceive a third *Parhelion* where the Circle ſurrounding the Sun would have been cut by the Vertical paſſing through him; and in the ſame Place his Circle was touched by the Arch of another, in ſome ſort confounding itſelf with it in the Place where the third *Parhelion* appeared: This was fainter a good deal than the other two, and the laſt Arch I have been ſpeaking of extended but a little way, ſo as for it to be difficult to determine where it's Centre lay; this Arch was coloured alſo, but with Red on it's convex Part. I had ſome Time before this began to ſee alſo another Circle, ſurrounding the Sun at the Diſtance of about 45°, which appeared to be about twice the Diſtance of the firſt; and this alſo increaſing whilſt I was conſidering it, became little leſs than a Semicircle, being alſo tinged with Red like the other on the inner Side. When this Circle had thus pretty well formed itſelf, I alſo diſcovered the Arch of a fourth, touching this, or rather confounding itſelf with it, in it's higheſt Part, and ſurrounding, as it ſeemed, the *Zenith*. Of this laſt Circle I ſaw, when it was moſt complete, better than half, and it was much ſtronger coloured than any of the others, being of a bright Red on it's convex Part, and a good Blue on the Concave. In the Part where this Circle confounded itſelf with the larger of thoſe that were concentric to the Sun, their common Part was nearly white, and brighter than the reſt, though hardly enough to call it a fourth *Parhelion*. The principal *Mock-Suns* continued tolerably bright till near 8, the ſouthern Part of the *Phænomenon* improving as the northern decayed; and the ſouthern *Parhelion* was once ſo bright, that, taking the Advantage of a place where a Chimney ſhaded the true Sun, it caſt a very viſible Shadow: The white and luminous horizontal Tail alſo, that went from this *Parhelion*, was much longer than that of the other, reaching at one time beyond the outer of the 2 concentric Circles. The *Parhelia* themſelves, tho' very luminous, were however never defined with any Exactneſs as to their Diſcs, but looked as we ſometimes ſee the Sun through a thin whitiſh Cloud, and they were themſelves of a reddiſh Colour on that Side next the true Sun. About 8 the *Phænomenon* was ſenſibly decreaſed, and had entirely diſappeared by 20′ after.

An Account of a Tract intituled, Jo. Friderici Weidleri Commentatio de Parheliis Menſe Januario Anni

XXXIII. This Tract is divided into 17 Sections. In the *1ſt* and *2d* the Author deſcribes his own Obſervation of two *Mock-Suns* at *Wittemberg*, on *Jan.* 11. *1735-6, N. S.* In the *3d* he gives a Meteorological Diary from *Jan.* 1. to 18. and in the *4th* the Rev. Mr *Neve*'s Obſervation on $\frac{Dec.\ 31.}{Jan.\ 11.}$ of two *Parhelia* near *Peterborough*. But theſe Deſcriptions have been already communicated to the *Royal Society* *. In the *5th*

* See §. xxx. and xxxi. of this Chap.

he

1736. prope Petroburgum Angliæ & Vitembergæ Saxonum viſis, Accedit de rubore cœli igneo Menſe Decembri Anni 1737. obſervato Corollarium. Vitembergæ, 1738. 4to. *Drawn up by* Tho. Stack, *M.D. F.R.S.* No. 458. p. 459. Sept. *&c.* 1740.

he compares the two Obſervations. In the *6th* and *7th*, he mentions ſeveral *Parhelii* taken notice of by the Ancients and Moderns; and in the *8th* enumerates the different Obſervables of this *Phænomenon*, for the better inveſtigating it's Cauſes.

The *9th* gives the Opinions of ſeveral of the Ancients concerning the Preſages taken from Mock-Suns.

From the *10th* to the *13th* incluſive, he relates divers Manners of accounting for them, by the chief of the Ancients and Moderns.

In the *14th*, preparatory to his own Opinion, he lays down the Doctrine of the Riſe of Vapours in ſmall globular Bubbles of Air, with a watery Coat to each,

In the *15th*, he refutes, by ſeveral Reaſons and Experiments, *Huygens*'s Manner of accounting for *Haloes*, which is by a vaſt Number of very ſmall Vapours, each with a ſnowy *Nucleus*, coated round with a tranſparent Covering: And ſays, that when the Sun depicts it's Image in the Atmoſphere, and by the Force of it's Rays puts the Vapours in Motion, and drives them towards the Surface, till they are collected in ſuch a Quantity, and at ſuch a Diſtance from the Sun on each Side, that it's Rays are twice refracted, and twice reflected, by the time they reach the Eye; they exhibit the appearance of a *Halo*, adorned with the Colours of the Rainbow: Which may happen in globular pellucid Vapours without ſnowy *Nuclei*, as appears by the Experiment of hollow glaſs Spheres filled with Water. Therefore, whenever thoſe ſpherical Vapours are ſituated, as before, the Refractions and Reflexions will happen every where alike, and the Figure of a circular Crown, with the uſual Order of Colours, will be the Conſequence.

As to the *Halo*, that attends *Parhelia*, being 44 or 45° in Diameter, he adopts *Gaſſendi*'s Opinion as probable, who applies to it the Geometrical Theorem: *De Angulo ad Centrum, duplo Anguli ad Peripheriam.* For when a *Halo* ſurrounds the Sun, the Sun is in the Centre, and the Eye out of it, as it were on the Surface of the *Phænomenon*; whereas, when the Rainbow appears, the Eye is placed in a Line drawn from the Sun to the Centre of the Rainbow: And thus the Eye ſerves for a Centre, from which the Diameter of the *Iris* is beheld, the Sun being placed on the Circumference. Yet he ſays, it ſtill remains to be accounted for, Why, when two *Haloes* appear at once, the Greater is double the Diameter of the Leſs, *i. e.* about 90°?

16th, But as *Haloes* often appear about the Sun and Moon, without *Parhelia* or *Paraſelinæ*, there muſt be a peculiar Diſpoſition of Vapours requiſite for forming *Parhelii*.

Parhelii, he ſays, are ſituated either in the Interſection of a vertical *Halo*, and the horizontal *Annulus*, which paſſes through the Sun; or in the Section of ſome horizontal Bands and the *Corona*: And the angular Figure of *Parhelia* leaves us no room to doubt, that it is produced by Planes of the *Annulus* or Bands running into the *Corona*. Now *Newton*'s Theory of Colours, and the Experiments it is built upon, ſhew, that

Whitenefs, which is a heterogeneous Light, is reftored by blending or collecting the couloured Lights: And this will likewife happen, when the white heterogeneous Light of the bright Ring or Band does penetrate and confound the Rays of the *Halo*, now fomewhat weakened. It is plain, that in order to a genuine Explication of Mock-Suns, it is chiefly requifite to have a clear Notion of the Origin of the horizontal Ring, or Bands. And from *Huygens*'s Experiment with a cylindric Glafs full of Water expofed to the Sun, which produces a white horizontal Ring by Reflexion alone, without an opake *Nucleus*; he afferts that the horizontal Bands, that interfect the folar *Coronæ*, are formed, when cylindric frozen Vapours are fufpended about the Sun, chiefly where the *Halo* is depicted, in a Situation perpendicular to the Horizon; which being rectilinear, each of them exhibits by Reflexion a lucid Line equal to the Sun's Diameter; and feveral of thefe optic Lines joining, compofe the Plane of the Ring or Band.

His laft Section is fpent in explaining the Appearance of Part of an inverted *Iris*, which accompanied his Mock-Suns: In order to which, he thus accounts for a (common or) primary Rainbow.

A primary *Iris* is formed, when the Sun's Rays falling on Drops of Rain, after fuffering two Refractions, and one Reflexion, tend to the Eye in fuch a Direction, that the Axis of the *Iris*, coming directly from it's Centre, and paffing by the Eye to the Sun, makes with thefe Rays returning from the Drops, an Angle of 40° below, and of 42° above; whereby the Width of the *Iris* is 2° 15′, and it's Diameter 42° 17′.

But as this inverted *Iris* was but one Degree broad, and the Diameter of the Arch probably but half that of the primary *Iris*; he is of Opinion, that the Sun's Rays, refracted and reflected as above, entered the Eye at half the aforefaid Angle, by the Eye's being placed beyond the Point, where the Rays met with the Axis. For thus the Order of the Colours is preferved, and this *Iris* is but half the Size of the common one.

See Sect. xlvi. of this Chap. As an Appendix to this Tract, Profeffor *Weidler* adds the following Account of the remarkable red Lights on the $\frac{16}{5}$ *December* 1737, feen not only by him at *Wittemberg*, but here at *London*, and in moft Parts of *Europe*.

December 9, 1737, the Barometer was remarkably low; *viz.* 28 Inches 8 Lines *Englifh* Meafure. It rained all that Day very plentifully; and from thence to the End of the Month the Sky was much loaded with thick Vapours. But on the 16th, the little Wind there was being at N W, and the Barometer at 30 Inches 2 Lines ¼ *London* Meafure, foon after Sun-fet, (the Moon in it's laft Quarter) the Sky began to appear very red; and, from Seven to Nine, gave a Light as ftrong as that of the Full-Moon behind a thin Cloud. The whole Sky was of that Colour, which is occafioned by a Fire feen at a Diftance in the Night. Such an uncommon Sight put the Inhabitants of this Town in great Terror.

Terror. The greateſt Brightneſs here was about $8^h\ 45'$; from which Time it gradually decreaſed; and at a Quarter after nine it ſeemed almoſt diſſipated. But it returned now and then, and continued, by Intervals, all Night. Now though the whole Face of the Heavens was remarkably red, yet the greateſt Brightneſs was in the N, and a little to the W. There were neither Pyramids, nor luminous Streamings, ſo common in *Auroræ Boreales*; nor even the leaſt Appearance of the horizontal black Cloud at North. The following Day was equally dark with the preceding, yet without the leaſt Remains of the Redneſs. Such was the Face of the Heavens at *Wittemberg*, and in the Neighbourhood. And, ſoon after, the publick News gave an Account of the like *Phænomenon* being ſeen at *Vienna*, *Venice*, *Mantua*, *Florence*, *Rome*, and ſome other Places. At *Vienna* the greateſt Brightneſs was obſerved at $9^h\ 15'$. The moſt enlightened Parts were the N W, and S S E; and there were ſome Returns of the Brightneſs on the 17th and 18th: But in *Italy*, at *Mantua*, *Florence*, and *Rome*, the Redneſs was accompanied with lucid Columns and Pyramids. And from *Rome*, in particular, they write, that this *Aurora Borealis* exceeded in Brightneſs all thoſe that had been hitherto obſerved. From theſe Obſervations it is no difficult Matter to deduce the Cauſes of theſe red Lights.

That this *Aurora Borealis*, on the 16th, was a very conſiderable one, appears both by the great Expanſion of the luminous Matter from it's Riſe in the N towards the S, and by the Return of the Brightneſs ſeen at *Vienna* on the ſubſequent Days. At *Mantua* the Northern Light reached the *Zenith*, and it is more than probable it did ſo in our more Northern Horizon: Wherefore, as the Matter was collected at the *Zenith*, the Light was reflected thence to all Parts of the Sky. But as the lower Region of our Atmoſphere was at the ſame Time overſpread with Vapours of a certain uniform Denſity, and entirely proper for ſeparating an homogeneous Light; thoſe Rays of the heterogeneous Light, which are the leaſt refrangible, or which produce the red Colour, were accordingly ſeparated by Reflexion and Refraction in great Quantities, and coloured the whole Sky with a fiery Redneſs. And where the Light was brighteſt, *viz.* between the N and W, which is generally the Focus of *Auroræ Boreales*, there likewiſe the Redneſs was ſtrongeſt.

A Repreſentation of the Parhelia *ſeen in* Kent, Dec. 19, 1741, *by the Rev. Mr* H. Miles. No. 462. p. 46. *Read* Feb. 25, 1741-2. Fig. 20.

XXXIV. 1. I have incloſed a Draught of the *Parhelia* ſeen in *Kent* the 19th of *December* laſt, as I took it from a private Letter ſent from thence to a Gentleman in this Town: The Writer of the Letter is not ſo particular in his Account of it as could be wiſhed: His Words were to this Purpoſe: " As to the Appearance of the Mock-Suns on the " 19th of *December*, I have incloſed a Scheme, ſuch as I could draw, " in which you may obſerve *S* is the Sun, *Z* the *Zenith* — *α α* an in- " verted Rainbow of the moſt lively Colours; the Mock-Suns *d d* were " ſometimes almoſt too bright to look upon, and then they ſeemed " round, but often were fringed (as drawn) with the priſmatic Colours;

" the Arch *bb* was but faint, and a whitiſh Light in the inner Part de-
" ſcribed at *c*. The Appearance ended about Noon, or rather a little
" before twelve; how early the whole was to be ſeen, I do not know."
The two largeſt Semicircles, I find no Notice taken of.——

On *Tueſday* the 19th Inſtant, at 10 at Night, being the Time I generally regiſter the Account of the Barometer and Thermometer, I found the Mercurial Thermometer abroad, at 20 Degrees above 0, or freezing Point: This I thought extraordinary, and for that Reaſon I conſulted my Regiſter of laſt Year, and found 16 Mornings and 13 Evening in *May* colder Air. And in *April* there were two Mornings and three Evenings only, a warmer Air.

Wind was Weſt all Day 19th, and began to riſe when I made the Obſervation, at going to Bed.

Of the ſame, by Mrs Tenniſon, *Ibid.* p. 48.

2. " Dr *Stukeley* likewiſe gave in a Scheme of the ſame Appearance, " as it was ſeen at *Canterbury* $10^h\ 12'$, *Dec.* 19, 1741, in which the " Light at *c* was not taken Notice of. He copied it from a Drawing " made by Mrs *Tenniſon*, who ſent it in a Letter to his Grace the Arch- " biſhop of *Canterbury*." *C. M.*

An Obſervation of an Anthelion ſeen at Wittemberg, *by* J. Fred. Weidler, *Prof. Math. and F. R. S.* No. 454. p. 221. July, &c. 1739. Fig. 31.

XXXV. *Jan.* 1/12, 1738. One of my Auditors, as he was meaſuring the Fields in a Plain near *Wittemberg* before Noon, happened to ſee an *Anthelion*, or Image of the Sun oppoſite to the true Sun, ſituated in the North. About 8 in the Morning, the Sky was very clear. About 9 ſome Clouds aroſe in the North, and there condenſed by Degrees, and about Noon extended themſelves farther. At $9^h\ 30'$, when dark Clouds had almoſt touched the *Vertex*, there appeared in them a Sun oppoſite to the Sun, of equal Magnitude, round, very bright, ſo that the Eye could not bear it's Splendor, encompaſſed with an oval Crown, or Halo. The greater Diameter of the Crown was about 5 Diameters of the Sun, and the leſſer about 3. The Crown itſelf was adorned with red and yellow Colours, the red being turned toward the Anthelion. The Tract of Clouds within the Crown was yellow, and here and there red. In the Anthelion 2 Portions of an *Iris* croſſed each other under an Angle of about 60°, and turned both to E and W, and were continued to the Circumference of the oval Crown; as *Hevelius* alſo obſerved *Sept.* 6, 1661. The *Phænomenon* continued a Quarter of an Hour; for when the Clouds were extended farther to the S. and hid the true Sun, the Anthelion diſappeared. It ſnowed a little at 11. In the Morning the Wind blew gently from the S W.

An Account of a Meteor ſeen in the Air in the Day-time, Dec. 8, 1733, *by* Mr Crocker, *at* Fleet *in* Dorſetſhire.

XXXVI. The Sun ſhining bright, the Weather warm, and Wind at South-Eaſt, ſome ſmall Clouds paſſing, I ſaw ſomething (between 11 and 12) in the Sky, which reſembled a Boy's Paper Kite, which appeared towards the North, and ſoon vaniſhed from my Sight, being intercepted by the Trees which were near the Valley where I was ſtanding. The Colour of it was of a pale Brightneſs, like that of burniſhed

or

or new-washed Silver. It darted out of my Sight with a seeming Coruscation, like that of Star-shooting in the Night; but had a Body much larger, and a Train much longer, than any Thing of that Kind I had ever seen before. At my coming home, one *Brown* assured me, he had seen the same Thing, for the Continuance of a Minute; and that the Body and Train appeared to him to be about 20 Foot long, and seemed to him to fall to the Ground somewhere about the Kennel-garden, whither I accompanied him in Expectation of finding some of those Gellies which are supposed to owe their Beings to such Meteors: But we might have searched long enough, as I understood the next Day, when Mr *Edgcombe* informed me, that he and another Gentleman had seen the same Appearance at the same Time about 15 Miles from us, steering the same Course from E to W, and vanished from them between *Walkhampton* and *Oakhampton:* They gave the same Account of it's Figure, Length, and Colour.

No. 456. p. 346. Jan. &c. 1740.

An Account of a luminous Appearance in the Sky, seen at London, March 13, 1734-5, *by* John Bevis, *M D.* Ibid. p. 347.

XXXVII. As I was observing *Mars* near a small, fixed Star, then in the West, on the Top of my House in *Buckingham-street*, about 5′ after 8, equal Time; happening to turn my Face Southward, I was surprized with an uncommon bright Glade of Light. It was strait, about 2 ½ Degrees broad, and 110, or 120 Degrees long, ill defined at either End, but pretty well at the Sides, that is, much as the common Rainbow, or one of those Pyramids which are used to dart up from the Horizon in an *Aurora Borealis*, which Light it resembled in all Respects, except in it's Place and Position, and that this was steady, and altogether without that tremulous Kind of Motion, which usually accompanies that. Besides *Saturn*, *Mars*, *Venus*, and the fixed Stars, there was then no other Light in the Sky, nor the least Cloud, nor any of that horizontal Blackness which we see Northward in the *Aurora*. The Stars were as discernible through it, as if nothing had been there. A Gentleman who was with me, fansied it to be the Tail of a Comet; but as neither he nor myself had ever seen one, I gave but little Heed to that Conjecture: However, I carefully directed a 17 Foot Glass to all Parts of it's Western Extremity, but could discern nothing like a *Nucleus*. When I first saw it, it extended itself from about the Midway between *Aldebaran*, and *Orion*'s left Shoulder, through *Gemini* a little under β, and so on through *Cancer* and *Leo*, just above *Cauda Leonis*, till it arrived between *Vindemiatrix* and *Coma Berenices*, where it ended very dilutedly. In about half an Hour it grew dim about the Middle, where in a short Time it separated in two, or rather became quite dark there; but then methought the disjoined Parts were more luminous than before; but they too in a little while after grew dimmer, and shortened away, on to their remote Extremities, which remained visible the longest; the Western one about 9, the Time of it's Extinction, being near *Orion*'s right Shoulder, and the other near the left Knee of *Bootes*; so that this Meteor seems pretty nearly to have accompanied the Earth in it's diurnal Motion, and to have had little or no Motion

besides.

beſides. I have looked for this Light ſince, but could find nothing like it.

The Day was exceeding fine, and by my Journal I find, that,

At Noon, the Barometer was	— — — — —	29.98.
Thermometer	— — — — —	57.
Wind	— — — — — —	Eaſt.
Decl. of the Needle	— — — —	14° 10′.
At 10 at Night, Barometer	— — — — —	29.86.
Thermometer	— — — — —	57.5.
Decl. of the Needle	— — — —	13° 50′.

Meteors obſerved at Philadelphia *in* North America *by* Joſeph Breintnal. Ibid. p. 359. *Dated* Philadelphia, May 9, 1738.

XXXVIII. The remarkable *Aurora Borealis*, that was ſeen in *Europe* the Beginning of laſt *Dec.* was not ſeen here.

But we had a viſible *Aurora Borealis*, *Dec.* 29, 1736. The Day was clear, with a briſk cold Wind N W, the Evening calm and ſerene, and about 7 we had a red *Aurora Borealis*.

Nov. 19, 1737, about Sun-ſet, many People in this Town ſaw a fiery *Meteor* in the Air, large and bright; it ſeemed in the *Zenith*, and ſo it ſeemed to them ſome Miles from Town; it was obſerved to be higher than the lower Clouds.

Dec. 7, 1737, a Minute or two before 11 at Night, we had two Shocks of an Earthquake, greater than ever felt here before. The ſecond Evening after, and for ſeveral Evenings in this Month, a red Vapour appeared to the S and S W, like the *Aurora Borealis*.

An Account of ſeveral Meteors, by Thomas Short, *M. D.* No. 459. p. 625. *Dated* Sheffield, March 18, 1740-1.

XXXIX. The whole of 1737, having been the moſt irregularly conſtituted Year of any in my Time; not one Month but what had the Weather of all the Seaſons in it, and that not by gradual Tranſitions, but by ſudden Jerks; Summer was dry, *Auguſt* was as cold as Winter, *September* full of great Changes; hence that ſudden and general *Catarrh* in *Octob.* ſucceeded in the latter End of the Month, and all *Nov.* by a fatal *Diarrhœa* among the Poor. From *Nov.* 29, to *Dec.* 5, was mild and warm, cloudy and clear mixed, like Spring Weather; the Wind daily veering from S to N W, and every Night falling back to S W or S. *Dec.* 5, at 5 at Night, the Sky round the Horizon was very cloudy, and clear in the *Zenith*; the Weſt Quarter was all of a deep Blood-red Colour, with Streamers of a very beautiful light Red, not running or dancing with ſudden Occurſions and Mixtures, like the *Auroræ Boreales*, but waving like Vapours, toward the *Zenith*, by N W to N: All the Clouds in the Interim were of a very dark red Colour, except that in the W, which was of a deep Blood-red. After it had continued ſome Time there, the ſame appeared in the N. Under the Clouds, from whence theſe Streamers came, was a Brightneſs ſuperior to that of a full Moon. Then both N and W ſent forth their Blood-like Streamers, one toward the other, which paſſed one another, and came to their oppoſite Funds before they were quite ſpent. Between 7 and 8 at Night, the

Scene ſhifted E; then that in the W was exhauſted, and that in the N weakened: None of them ſent their Streamers beyond the *Zenith* to the S; only the Clouds in the S. were of a very opaque Red. Laſtly, it removed S E, where the Remainder was ſpent: All was over about half an Hour after ten. I had no Inſtrument to take it's Altitude.

The chief Remarkables of this Meteor were,

1. From whatever Quarter theſe Streamers came, they iſſued out of a thick, deep-red Cloud, under which was hid ſo luminous a Body, that I could have eaſily read on a large Church Bible.

2. Theſe Streamers differed from thoſe of all preceding *Auroræ Boreales:* 1. That they were not white and clear, but a bright Red, like the Surface of arterial or pulmonary Blood. 2. They were not ſmall or narrow, but broad like the milky Way in a froſty Night. 3. They did not dart or fly ſwiftly from the Fund, or luminous Cloud, but moved ſlowly; then ſtood ſtill ſome Space of Time; then ſent out thin red Vapours, through which the Sky and Stars were viſible; theſe quickly ſpent themſelves, and vaniſhed. 4. Not only were their Funds red, but the whole Clouds were thick, and of a deep fiery Red.

3. They were above the Region of the Winds; for, though the laſt was S W, yet they moved from N to W, as quickly as from W to N.

4. Whilſt the Sky on the *Zenith* was of the common azure Blue, that in the S, on the opening of the Clouds, was a deep blueiſh Green, like Graſs.

5. The whole Time was attended with an extraordinary Heat of the Air for the Seaſon; for I was obliged to ſtrip to the Shirt, though abroad in the Air all the Time.

6. This Meteor was ſeen at *Venice* at the ſame Time; and, over *Kilkenny* in *Ireland*, it appeared like a great Ball of Fire, which burſt with an Exploſion, that ſhook great Part of the Iſland, and ſet the whole Hemiſphere on Fire; which burnt moſt furiouſly, till all the ſulphureous Matter was ſpent.

7. This Meteor put an End to the Remains of both the *Catarrh*, and watery *Diarrhœa*; and reſtored general Health, till the next epidemic *Catarrh* among the Infants in *February* 1738, two Months after.

The next Meteor was on *Aug.* , 1733, a clear, calm, exceſſive hot Day, at 9 at Night, a frightful Glade of Fire, or *Draco Volans*, from E to W.

Oct. 1, 1736, Day cloudy, Wind S W, clear Evening, 6 at Night, fell a great Ball of Fire out of the Air to the Earth, no Rain 15 Days before, and only a few Drops two Days after.

Aug. 28, 1738, five *p. m.* Wind S W, Sky clear, the Sun bright ſhining, a fiery Meteor appeared N E, ran N, like a Spear of Fire, with a great round Head, which burſt like a Rocket, ſpread about in a large Fire, and vaniſhed ſuddenly. This was a great Drought, which continued without Rain to *September* 7.

The

The next was *Dec.* 2, 1739, six at Night, Wind N, Sky clear, a white Frost, a great *Halo* about the Moon. This Meteor appeared like a large round Body of Fire, of about a Foot and an half Diameter; seemed very low, therefore could not be observed far, though it went all over this Country from N to S, pretty sharply, but nothing near so quick as a Glade of Lightning, was succeeded instantly by a most dismal Sound in the Air, like Carts, Drums, and Groans mixt: It kept the Track of the Meteor, but in an opposite Course, *viz.* from S to W. This was a most frightful Time of Rains, Snow, Storms, *&c.*

As to the *Auroræ Boreales*, the most remarkable were, 1. That of *Sept.* 14, 1736, Wind N W, Sky clear, next Day very rainy. This exactly resembled a Crown nobly adorned with the richest Jewels; it's concave Side facing the W, and it's Convex reaching near the *Zenith.*

2. *September* 3, 1737, Wind N W, the Day was very rainy, and the Night a clear Frost. About one in the Morning, was another *Aurora Borealis*, like a Crown, it's concave Side full of Streamers, several Times red, had very swift Motions; but the splendid Crowns stood steady and fair 2 or 3 Hours. We find an Instance of the like over *Bohemia* in the *Philosophical Transactions.*

The common late *Auroræ* were, 1736, *October* 16, 17, 18, Wind S, all three Days showery, the Nights bright and clear. *March* 10, 1737, Wind W a Day and Rain, S at Night and clear. *September* 16, Wind W, clear Night. 17, Wind W, a Shower in the Day, and clear at Night. 19, 20, 21, Wind W, all fair, some little Frost. *October* 13, 14, Wind N, clear Days, frosty Nights; the 15th was much Rain. *March* 7, 1738, Wind S W, Streamers reddish, Day cold and cloudy; the 8th was rainy; 30th, Air temperate, Wind N W, Day drisling, next Day fair and clear. *February* 4, 1739, Wind N W, Day and Night clear; next Day snowy; 23, 24, both Days clear, Wind W; the next Day good. *March* 1, Wind W; that Night frosty, the next after clear and good. *Sept.* 13, 14, 15, Wind N W in the Morning, and S W by S at Night; all 3 Days showery or drisling, Nights clear; 18, 19, Wind E, cloudy fair Days. *Oct.* 22, at Night, Wind North, cloudy; appeared a frightful, fiery Dragon, seen over all *England.* This Month was the only good Weather from the 6th Day to the End, that this Country had that Harvest. *Nov.* 25, Wind N W, cloudy Day, with a Shower, clear frosty Night, with *Auroræ Boreales.* The next I saw was on *October* 6, 1740, Wind W N W by W; Day clear, a small Shower, a frosty Night. *February* 28, 1741, Wind N W. *March* 5, Wind S. 6th, Wind W, then N. 9th, Wind S: All droughty Weather, with small Frosts.

Our Northern Lights have been much seldomer, and fainter, both in Appearance and Motion, than formerly; and whether they will dwindle away and vanish wholly for some Years, or whether they have had their former periodic Returns, is not certain: Nor is it less dubious, whether they affect our Weather, Seasons, and Animal Bodies, or not.

XL. *Dec.*

XL. *December* 11, 1741, at ſeven Minutes paſt one in the Afternoon by the common Clocks, a Globe of Light, ſomewhat larger than the horizontal Full-Moon, and as bright as the Moon appears at any Time while the Sun is above the Horizon, inſtantaneouſly appeared, in a blue clear Sky, about the S S E, moving towards the E. with a continual equable Motion, and leaving behind it a narrow Streak of Light, whiter than the Globe itſelf, throughout it's whole Courſe. Towards the End it appeared leſs than at the Beginning of it's Motion; and within 3, or at moſt 4″, it ſuddenly vaniſhed. It's apparent Velocity was nearly equal to half the Velocity of thoſe uſual Meteors commonly called falling or ſhooting Stars: This may be thought an indeterminate Way of expreſſing it's Velocity, as thoſe falling Stars vary in the Swiftneſs of their Motions; but if ſuch be underſtood as have a mean Velocity, between the ſwifteſt and the floweſt, it expreſſes, in the beſt Manner I can think of, the apparent Velocity of it's Motion.

An Account of a Meteor ſeen at Peckham, Dec. 11, 1741, *by* Tho. Milner, *M.D.* No. 464. p. 138. *Read* June 24, 1742.

The narrow luminous Streak remained very diſtinct after the Globe was gone; and give a fair Opportunity for taking the Elevation of this *Phænomenon* above the Horizon, at the Beginning and End of it's Motion, *&c.* had there been proper Inſtruments ready at hand: This not being the Caſe, I gueſſed the Elevation of the Globe, when it firſt appeared, was near 30°. But ſome Days after, being exactly in the ſame Situation as when I ſaw this Meteor, I took the Elevation of a ſmall Cloud, which appeared to be in the ſame Place, with a Quadrant of two Feet Radius, and found it to be but 20°. This luminous Track, or Path, ſeemed a right Line, not quite parallel, but a little inclined to the Plane of the Horizon, *viz.* higheſt towards the Eaſt. It was at firſt very narrow, and pointed at each Extremity; but ſoon grew broader, and within 20′ after the Appearance, which was the laſt Time I ſaw any thing of this Affair, it appeared exactly like a long, bright, rare Cloud, diſcontinued in two Places, above three Times it's firſt Breadth, and a little more inclined to, and elevated above, the Horizon, than it was immediately after the Motion of the Globe.

XLI. *Thomas Savory*, *John Walker*, and others of Lord *Lovell*'s Ploughmen, being at Plough about the Middle of *Auguſt* 1741, on a fair Day, at 10 in the Morning, ſaw on a Heath, about a Quarter of a Mile from them, a Wind like a Whirlwind, come gradually towards them, in a ſtrait Line from E. to W. It paſſed through the Field where they were at Plough, tore up the Stubble in the ploughed Ground, and alſo the Graſs beſides the ſame, for two Miles in Length, and 30 Yards in Breadth. When it came to ſome Cloſes at the Top of a riſing Ground, called *Ferrybuſh-Cloſes*, *Philip Henning*, and others, who were houghing *Turneps*, ſaw it appear like a great Flaſh or Ball of Fire. After having ſeen the Wind come into the Cloſes, *Robert May* was in a Cottage where he lives by a Road-ſide, at the Bottom of the Park, about a Furlong down-hill from the Cloſe, when one of his Children about 6 Years old, who was playing at the Door, cried out, That *Ferry-*

An Account of a Meteor ſeen near Holkam *in* Norfolk, Aug. 1741, *by the Right Hon.* Thomas *Lord* Lovell, *F. R. S.* No. 465. p. 183. *Read* Nov. 4. 1742.

buſh-Cloſes were on Fire; on which he went out to look, but ſaw no Fire, only a terrible Smoak, and heard ſuch a Noiſe as Fire makes when a Barn is burning. He then ſaw the Wind come from the Cloſes in the ſame Manner as before-mentioned, making a terrible Noiſe, like that of a violent Fire; alſo like Carts over ſtony Ground, which paſſed by his Houſe, tearing up the Stones in the Road, and tore up a Rank of Pales, and ſprung ſeveral of the Poſts out of their Places, and carried a Pewter Plate that ſtood on the Outſide of the Window, about 40 Yards from the Houſe; and a large Box-cover about an Inch and half thick, and 4 Feet ſquare, and croſs-barred, which he covers his Birds with, was carried away much further, and torn all to Pieces; and the Gravel flew about, and alſo the Flint-ſtones like Feathers. It alſo broke down ſome of Mr *Knotts*'s Fences, and frightened the Cattle in a terrible Manner. And, what is moſt remarkable, that every where elſe but in this Place the Weather was clear and fine, and no Sign of any Storm or Diſturbance whatſoever. About a Quarter of an Hour after, *Philip Henning*, and two of his Partners, *Turnep-houghers*, who were at Work about two Furlongs off, came to the ſaid *Robert May*, and told him, they were glad to ſee him alive; for they expected, that he and his Family, Houſe and all, had been deſtroyed, having ſeen the Fire go that Way, and heard a Noiſe, as if the Houſe had been demoliſhed. *Robert May* ſmelled a moſt terrible Smell of Sulphur, both before and after the Wind paſſed him, and heard the Noiſe a great while after ſeeing the Smoak, before he ſaw the Wind, an Hedge intercepting his Sight. He ſays it moved ſo ſlowly forward, as to be near 10′ in coming from the Cloſes to the Houſe.

Concerning a Ball of Sulphur ſuppoſed to be generated in the Air, by Benjamin Cook, *F. R. S.* No. 451. p. 427. *Dated* Newport (Iſle of Wight) July 9, 1733.

XLII. The great Heats we have lately ſuffered, were uſhered in by a very gloomy Night of almoſt continual Lightning, accompanied with very loud Claps of Thunder, which, as uſual, were towards the Morning followed by very heavy Showers of Rain. Early next Day, in a Meadow near the Sea-ſhore, far from any Houſe, and where it has not been known that any Improvement has been carried on, a Huſbandman found a beautiful yellow Ball lying on the Turf, which he gladly took up, in Hopes it would well reward him for ſtooping.

But it proved to be of Sulphur, of which it ſmelt uncommonly ſtrong. It was froſted, as it were, all over with an Efloreſcence of fine, ſhining, yellowiſh Cryſtals, which ſoon fell off with the lighteſt Touch.

Fig. 32.

It has on one Side a deep Hole *A*, admitting the End of a middle-ſized Knitting-needle, and on the oppoſite Side a deep Depreſſion *B*; which would induce one almoſt to think it's Form had been at firſt nearly ſpheroidal, formed by a Revolution round a ſuppoſed Axis drawn from *A* to *B*. It has ſeveral other Holes ſcattered irregularly up and down it's whole Surface, ſome fit to admit a Hog's Briſtle, others a Hair; as if it had been made of a fine Powder, and ſome thin Liquid, and after mixing had ſuffered ſome Fermentation; but thoſe Parts of it which

which are solid, seemed more compact than those of the common roll Brimstone of the Shops, and the Powder of it burns with a whiter Flame, and less acid Fumes. It's longest Diameter is betwixt eight and nine, and it's shortest betwixt $\frac{6}{10}$ and $\frac{7}{10}$ of an Inch; it's Weight is 108 Grains. To save more Words, I have roughly described two Sides, one of which has the Hole *A*, the other the Depression *B*.

We find frequent Mention in the Description of Thunder-Storms in hot Climates, that there falls often a flaming bituminous Matter to the Ground, which sometimes burns not to be soon extinguished, but more frequently spatters into an infinite Number of fiery Sparks, doing incredible Damage where they strike, always attended with a sulphureous, suffocating Smell, commonly compared to that of Gunpowder.

Whether this sulphureous Ball was intended for one of these, but by some Accident missed firing, it is now Time to consider.

Had it been formed in the Earth, how should it get to the Surface, without losing that most elegant, frosty Covering of fine shining Crystals, and appear not in the least sullied, or it's Pores filled with Earth, or other terrestrial Matter; on the contrary, not the least Adhesion of any thing of that Kind can be observed: Besides, Brimstone made the ordinary Way, seems to have a different Texture of it's internal Parts from this Ball. From these Observations I am ready to conclude it not formed in the Earth; but however submit it to the Determination of the Curious.

An Account of the Fire-ball seen in the Air, and of the Explosion heard, on Dec. 11, 1741, *by the R. Hon. the Lord* Beauchamp, *near* London. No. 461. p. 870. Aug. &c. 1741.

XLIII. 1. Being on the Mount in *Kensington* Gardens at a Quarter past 10, the Sun shining bright, in a serene Sky, I saw towards the South a Ball of Fire, of about 8 Inches Diameter, and somewhat oval, which grew to the Size of about 1 ½ Yard Diameter. It seemed to descend from above, and at the Distance of about ½ a Mile from the Earth, took it's Course to the East, and seemed to drop over *Westminster*. In it's Course it assumed a Tail of 80 Yards in Length; and before it disappeared, it divided into 2 Heads. It left a Train of Smoke all the Way as it went; and from the Place where it seemed to drop, there arose a Smoke which continued ascending for 20′ (as another Gentleman and I observed by our Watches); and at length formed into a Cloud, which assumed different Colours.

Concerning the same Meteor in Sussex, *by* John Fuller, *Esq; jun. F. R. S.* Ibid. p. 871.

2. In the Afternoon, between twelve and one, all this Part of the Country was alarmed with a most terrible Clap of Thunder, as it is generally imagined. The Sound came from the N, where the Weather appeared very black and dark all the Morning. The Sound was double, as if 2 very large Cannons had been discharged at the Distance of about 1″ from one another. Most People thought, just at the first hearing, that it was the Discharge of Cannons, till by the rolling and ecchoing of the Sound afterwards, they were convinced it was not. Our Neighbours thought some Powder-Mills had been blown up; and I look upon them to be no bad Judges in such Kind of Blasts, having been more than once alarmed with them, by the Powder-Mills in the Neighbour-

hood.

hood. I have it by Report, that a Countryman, at Work in the Fields about 7 Miles N. of us, saw a Flash of Lightning before he heard the Noise, but I cannot answer for the Truth of it: It is very easy to imagine, that Fancy and Fear in a poor Countryman upon such an uncommon Occasion, might conjure up the Idea of Lightning. If it was Thunder and Lightning, the Effects of it must be very terrible somewhere; for it gave the same Report, and shook all the Houses just in the same Manner, that were above 20 Miles distant from one another N. and S; which I think is an Argument, that it was more general than Thunder can possibly be.

Concerning the same Meteor in Kent, *by the Rev. Mr* William Gossling, *Minor-Canon of the Cathedral Church of* Canterbury. *Ibid.* p. 872.

3. About one in the Afternoon I found my House violently shaken for some Seconds of Time, as if several loaded Carriages had been driving against my Walls; and heard a Noise, which at first my Family took for Thunder, but of an uncommon Sound. For my own Part, (as I thought Thunder which would shake us at that Rate, would have been much louder) I concluded it an Earthquake: And, going immediately to the Top of my House, found the Sky cloudy, but nothing like a Thunder-cloud in View; only there was a Shower of Rain from the Eastward presently after, and the coldest that I have felt. I thought it the Shock an Earthquake, as I told you before; but since find it was attended (and I suppose caused) by a Ball of Fire, which passed with great Rapidity over our Country, from Westward to Eastward, for how long a Journey I cannot tell. It began with 2 great Blows, like the Reports of Cannon (which the jumbling of my Sashes prevented my distinguishing); and then rolled away till it was heard no more. The Appearance, I hear, was as that of a very large shooting Star; and it left a Train of Light, which soon disappeared, it being Noon-day. I met a Pilot To-day coming from *Deal*, whom I asked about it; and he told me he saw no Fire-ball, but heard the Noise, and that it made the Ship shake he was in, going from *Gravesend* to the *Nore*.

Canterbury, Dec. 13

——*In* Sussex, *by Mr* Christ. Mason. No. 462. p. 1. *Read* Jan. 7, 1741-2.

4. At *Buckfteep* in *Warbleton* Parish, in the County of *Sussex*, about ¼ before 1 in the Afternoon, I observed a very dark, uncommon Appearance in the N, and at the same Time the Sun shone bright at my Back; when, on a sudden, there was an Explosion, as violent as the Report of a Mortar-piece, attended with a rumbling Eccho, which run Eastward; and as near as I could conjecture, it came from about 40° of Elevation. Several People saw a Ball of Fire, which ran nearly Eastward, leaving a Train of Light, which continued some Time. The Ball of Fire was seen, and the Report heard very loud, at *Sompting*, beyond *Shoreham*. Although I had been gazing upon the black Cloud for some Minutes, yet I saw no Fire nor Lightning.

——*At* Newport, *in the* Isle of Wight, *by Mr* Benj.

5. I did not see the *Phænomenon*, but a Gentleman of my Acquaintance was on a Hill about 3 Miles W. of this Town, and had a very advantageous View of it. He says, that at that Time the Brightness

ness of the Sun was a little obscured by the Interposition of some thin Clouds, when he saw it pass by to the Eastward, at about the Distance of something more than a Quarter of a Mile, and apparent Height of 30 Feet above the Level of the Place where he stood. It's Colour was that of a burning Coal; it's Figure a Cone, whose Length might be 8 Feet, and Diameter at the Base 18 Inches. From about it's Apex, which was it's hinder Part, issued several bright Streams sparkling with fiery Drops, to the Length of about 4 or 5 Feet. It's Motion was nearly parallel to the Plane of the Horizon, and it's Direction (as near as we can find by comparing the Places it passed over) from S W by S to N E by N, without any Noise, Wind, or Motion of the Earth attending it. The Time of it's Appearance did not happen to be taken Notice of with the desired Exactness; but by the best Observation we can make, must be about a Quarter before 1 at Noon. There were a few others who saw it, to whom it appeared different in Shape, according to the Point it was seen from; and perhaps it's Shape might change as it became nearer consuming, and only it's Head, in the Form of a Bell, remain at last.

Cooke. *Ibid.* p. 25. *Read* Jan. 28, 1741-2.

Fig. 33.

6. About one, *P. M.* coming by Water from the City to *Whitehall*, and near to *Hungerford-stairs*, there appeared to me, between *Vauxhall* and *Lambeth*, a Body of Fire: It sprung upwards in it's Ascent almost perpendicular to the Horizon, to the Height, as near as I could guess by my Eye, of 35°, in the Space of a few Seconds, and nearly in Form of a large Boy's Kite, projecting a long Tail towards the North-West, not unlike those of Slips of Paper set on Fire: In this State it continued so long, that I made the Waterman lay his Oars in, that I might the more easily observe whether it was the Work of Art or Nature, for I was in some Doubt. It had from it's first Appearance expanded itself considerably, so that the extreme Breadth was seemingly equal to the Diameter of a Full-Moon, arising from a dusky Horizon. In this Form it continued ascending for the Space of 2′, gently shooting withal to the N E, till it arose to about 45°; then suddenly quitting it's Tail, which vanished, colouring the neighbouring Clouds with a Yellow on their Separation, it formed itself first into a Ball of Fire; then shooting quickly to the South-East in a Stream of Light, disappeared, making a Noise like a Clap of Thunder at some Distance, and leaving behind it a smoaky Substance in it's Track.

——*At London, by Capt.* William Gordon. No 463. p. 59. *Read* March 4, 1741-2.

The Weather moderate and cloudy, Wind, as nigh as I can remember, W S W. It continued in Sight upwards of 5′.

7. As the Fire-Ball appeared at Noon-day, and the Sun shining, few People saw it, and they could only guess at the Course. The best Account I have had is at Second-hand, from two Farmers who saw it together, and make it's Course from N W by N, to S E by S, and right over *Littleborn* (which is the first Village in the Road from *Canterbury* to *Deal*). Their Way of telling it's Course was by saying, it went from *Westbere* toward *Ratling*, near which Place Lord *Cowper* was then hunting,

A farther Account of the same, by the Rev. Mr William Gostling. *Ibid* p. 60. *Read* March 18, 1741-2.

ing, and heard but one Explosion, which seemed to be within a few Rods of him: The other, I suppose, happened at such a Distance, as to be in one with that so near him.

Two Observations of Explosions in the Air; one heard at Halsted *in* Essex, *by the Rev. Mr.* A. Vievar, *Minister of that Place; the other by* Sam. Shepheard, *Esq; of* Springfield *in the same County.* No. 455. p. 288. Nov. &c. 1739.

XLIV. *Sunday March* 12, 1731-2, between 1 and 2 in the Afternoon, walking in my Garden by the Side of a Canal, I heard as it had been a large Clap of Thunder from the N E, being a very clear Day, and no Clouds appearing. While I was looking into the Air, the Noise was repeated very loud, but seemed more like the violent Fall of a House, insomuch that I expected every Moment an Out-cry from the Town: But I was soon undeceived, when it began again, and I found it made towards me, with a different Noise from what I had heard, that is, like the Grinding of Flint-stones, but very loud: The Dimensions of it seemed to be about 3 Foot wide. I found it sink in the Air, and as it seemed to point directly at my Head, I laid myself down upon a Grass-slope, to let it pass over me. However, at the upper End of the Walk I found it fell to the Ground, and came rolling down the Grass-walk; and I can compare it to nothing better than to that of a violent Grinding of Flint-stones, or a Coach and six upon the full Speed upon a Causway of loose Stones. I lay attentive, expecting to see something, and saw a Piece of Wood came running before it. When the *Phænomenon* came to the Water-side, it twisted up a large Stake that stood in it's Way, and tossed it towards me with much Violence, and immediately fell into the Water with the Violence and the Noise of a red-hot Mill-stone. I have seen the Seas break against a Rock in a Storm, but never saw a greater Ferment caused by the boiling of the Waters. It staid about ¼ of a Minute in the Water, and then mounted again into the Air, and went rattling away, but with much less Violence: I heard it for about ¼ of a Mile, and lost it. *N. B.* It came against the Wind, and not faster than a Man may walk. The Froth and Foam upon the Water remained 30 Hours after, when I shewed it to some Friends.

Halsted *in* Essex,
1731-2.

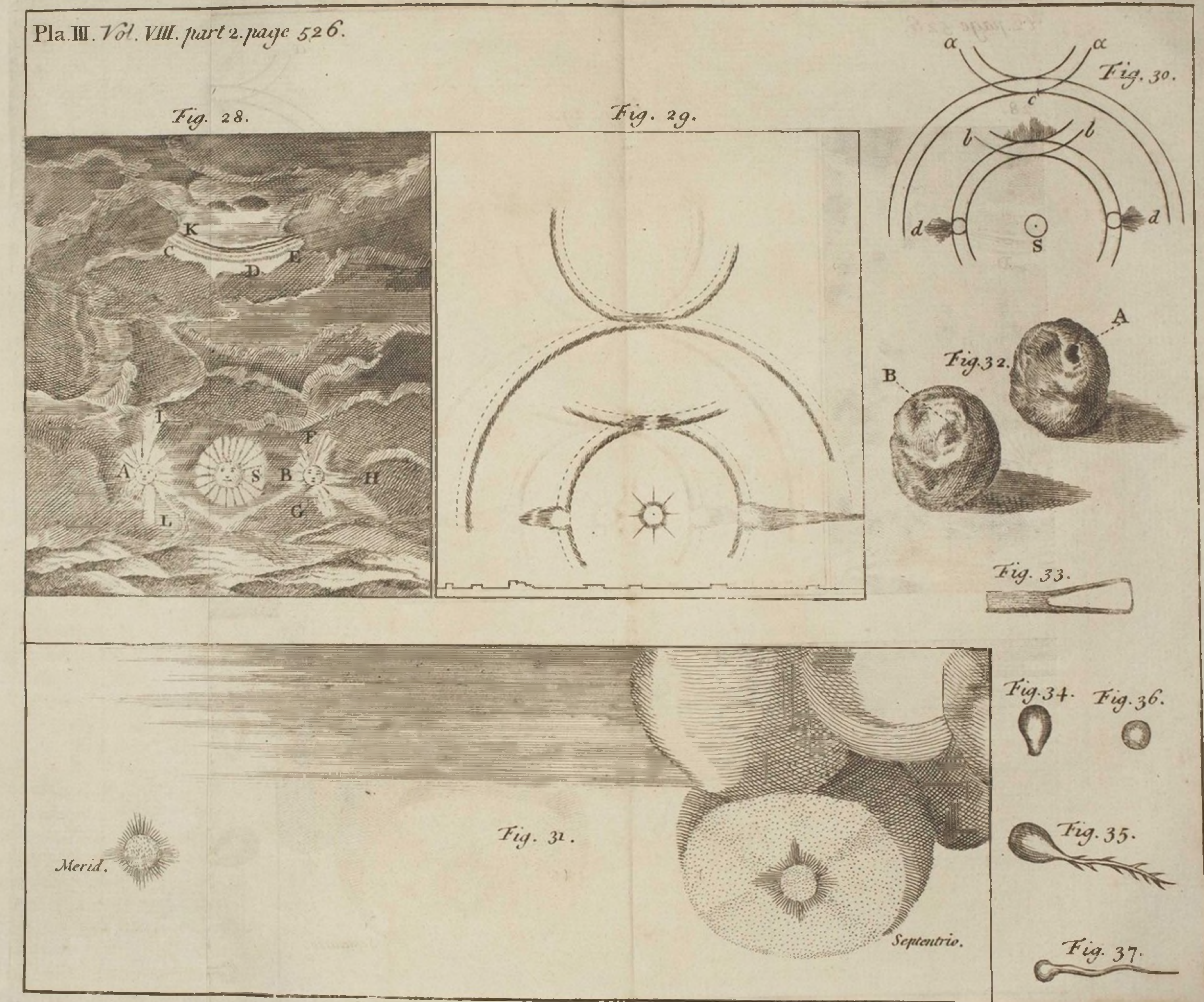
A. Vievar.

Tuesday *, *March* 15, 1731-2, between 11 and 12, the Sun shining very bright and hot, without the least Cloud, the Wind so calm, that the Water was as smooth as Glass, I was dressing in my little Room next the Garden, about 40 Yards from the Canal, when I heard a very surprising Noise of Fire, resembling, as I told you at *London*, as if a very large Quantity of Oil had been thrown into a great Bonfire, burning in it's greatest Rage. I stepped immediately to the Window which was open, where I saw the Middle of the Canal, which this dry Season has sunk about six Inches, in extreme Agitation, as rough as the *Thames* in a Storm, foaming and smoaking, and forced up, to my Appearance,

* *March* 15 was that Year on a *Wednesday*. *J. M.*

full

Pla. III. Vol. VIII. part 2. page 526.



full 2 Foot above the Surface, but it might be much more, my Window being greatly higher than the Canal; and the Fellow who was at Work, whom I examined again this Morning, protests he saw the Water, like the Spray of the Sea, above the Dwarf-Trees, which must necessarily be 5 or 6 Foot. I wish I had seen the Beginning of this uncommon *Phænomenon*, the Duration of which, I think, might be ½ a Minute, and made the House stink, as if a Gun had been fired in it.

My Canal bears E. and W, and the Fellow says he heard it coming from the W, bringing the Leaves of some tall Trees from an adjacent Field in it's Passage; but could not discover any material or substantial Body to fall in the Water, where the Hissing, as I observed above, was very loud and violent; neither was there any Lightning or Thunder before or after, but the Day remained bright, still, and hot. I forgot to say, the Space of the Canal that was affected by it, might be 12 or 15 Yards.

Springfield *in* Essex,
Aug. 22, 1732.

Sam. Shepheard.

XLV. *Octob.* 22, 1725, about 2 in the Afternoon, the Sky being very serene and clear, Capt. *Richard Smith* heard, as he then thought, the Noise of a Gun, of a Minion Size, about 12 Miles Eastward from him, which Noise was repeated at least 20 Times, but at unequal Distances of Time, and soon afterwards followed a very loud Explosion, as if a Ship had been blown up: Upon Enquiry, he was told by several Persons who lived about 12 Miles distant from his House, that they were greatly amazed with the Appearance of an extraordinary Brightness in the *Zenith*, resembling Flame, which continued for about 5′, and then the imaginary Guns were fired 20 or 30 Times, which so disturbed the Atmosphere, that the Birds lost the Use of their Wings, and fell to the Ground in great Disorder. This Noise was heard about 50 Miles each Way, from the bright Appearance aforesaid.——*Thus far the Captain.*

An Account of an Explosion in the Air at Maryland, *by Mr* Richard Lewis. No. 479. p. 120. July, &c. 1733.

I heard the Noise (as most People did) but saw not the Brightness at *Patapsko*, being about 60 Miles from the Captain's House, I was told that the Shock, occasioned by the Noise, threw down Pewter that was set to dry against the Side of a House.

XLVI. 1. *Dec.* 16, 1737, (*N. S.*) in the Evening, the Sun being about 25° below the Horizon, a Light was observed in the N, as if the Air was on Fire, and flashing; the Intenseness of which gradually increasing, at the third Hour of the Night it spread Westward in such a Manner, that if a Perpendicular was let fall from the Polar Star, and afterwards a Parallel to the Horizon supposed, and divided into 6 equal Parts, which Parallel should pass through the whole Extent of the aforesaid Light, it is certain, that 5 Parts of the 6 would be towards the W, and only one toward to E.

An Account of the red Lights, on Dec. 1/16, 1737, *as observed (at* Naples) *by the Prince of* Cassano, *F. R. S. Translated from the* Italian, *by* T. S. *M. D. F.R.S.* No. 459. p. 583. Jan. &c. 1741.

The greatest Height of this Light was about 65°; for it occupied the whole Extent of both the *Bears*, and the *Polar Star*: Yet at the Sides it was

was not so high; for in some Places near the N. it arose only to 50°, and gradually diminished, so as to become insensible at the true Horizon.

The above-mentioned Light at it's Extremities was unequally jagged, and scattered, and followed the Course of the Westerly Wind; so that for a few Hours it spread considerably wider, yet without ever reaching the *Zenith*.

The greatest Redness and Inflammation appeared half Way, between the visible Pole and the Northern Point of the Horizon; and in the Middle of this inflamed Part there appeared some Streaks less inflamed, and mostly perpendicular to the Horizon; some of which flashed from Time to Time, while others successively vanished. About the sixth Hour of the Night the Intenseness of the Colour disappeared; some small Traces of the Inflammation still remaining towards the N E and W, which were all vanished at $7^h \frac{1}{2}$ [of the Night.]

During the greatest Vigour of the Inflammation, some small dark Clouds often crossed the Light parallel to the Horizon: But the Sky was very clear, except in some Parts near the Horizon, where it was much overcast with Clouds.

The inflamed Matter, in the greatest Part of it's Extent, gave a free Passage to the Rays of the Stars, even of the third and fourth Magnitude, situated behind it. About the fourth Hour of the Night, a very regular Arch of a parabolic Figure was seen to rise gently, to 2° of rectangular Elevation, and to 20° of horizontal Amplitude.

This *Phænomenon* was seen all over *Italy*, as appears by several Accounts of it, though with some Disagreement between them.

But how bright soever and distinct it appeared, yet it's Cause has been deemed by many to be very obscure: For some call it an *Aurora Borealis*, therein following the Opinion of *Gassendus*, and deducing all the Appearances from the Laws of simple Refraction of the solar Rays. Others think it an Irradiation of some luminous Comet, placed below our Horizon. Others more politely say, it was a new celestial Body descended from it's upper Habitation down to us, and courteously received by the Earth's *Vertex*. Others, in Love with Authority, and *French* Names, have endeavoured to establish the Meteor as a Mixture of the two Atmospheres of the Sun and Earth; therein tenaciously adhering to the new Opinion of M. *de Mairan*, of the *Academy of Sciences* at *Paris*. In fine, others more accurately deduce the Whole from the simple firing of a bituminous and sulphureous Matter, upon account of it's very little specific Gravity, raised to the upper Parts of the Atmosphere, and there, by the clashing of contrary Winds, broken, comminuted, and at last set on Fire. This Opinion has been defended with strong Arguments, in the *Petersburg* Commentaries, by *Mayerus*, upon Occasion of the Appearance of a similar *Phænomenon* in those Northern Countries.

And,

And, indeed, the preceding Eruption of *Vesuvius*, the Contrariety of the moving Forces, the Readiness of the Matter to take Fire, the unequal Intenseness of the Light, the Streaks, and all the other Circumstances observed in this Meteor, are plain Arguments of a genuine and real Accension. And *Wolfius*, on the Appearance of a *Phænomenon* much like this, which was seen all over *Germany* on the 17th of *March*, 1717, is of Opinion, that it should be called imperfect Lightning, as being produced by the inflammable Matter of Lightning: And possibly we shall see the subsequent Rains fall quietly, without Lightning or Thunder.

1*st*, That it could be a Refraction, happens to be diametrically contrary to the Laws of Refraction; because the Sun was then in the opposite Tropic.

2*dly*, The Light ought to have been most intense in the East, and weak in it's Elevation; whereas quite the contrary was seen to happen. Thus the Whole is accounted for, not by Dioptrics, but by the sole Laws of direct or reflex Vision; and the Streaks already taken Notice of, were Spaces containing less of the inflammable Matter; whereby the luminous Rays of the neighbouring kindled Matter, being weakly reflected, made the Appearance of a fainter Colour.

3*dly*, The uneven Appearance of the Light at it's Extremities cannot be accounted for by Refraction, but perfectly well by Accension: Wherefore I think it rather deserves the Name of a *Northern Light*, or *Fire*, than that of an *Aurora*: But I leave the further Consideration thereof to better Heads.

2. The Sky was entirely clear, not only in the Beginning, but during the whole Night. The Wind was at N; which was rather known by the Weather-cock, than sensibly felt, the Air being very still. The Quicksilver in the Barometer stood at 30 Dig. 24 Dec. (*English* Measure) an extraordinary Height; since in the Space of 14 Years, that I have applied with great Care to Meteorological Observations, I have but once observed the Quicksilver at 30. 48. which I have hitherto looked upon as the greatest Height.

——*At* Padua, *by* [*the Marquis*] Poleni, *F. R. S. Translated from the* Latin *by* T. S. *M.D. F. R. S. Ibid.* p. 587.

In my Thermometer of M. *Amonton*'s Make, the Height of the Quicksilver was 48 Dig. 78 Dec. And in Monsieur *de l'Isle*'s Thermometer, which he sent me from *Petersburg*, (in which the Heights are changed by the greater or lesser Density of the Mercury, and the Measure is taken behind the vacant Space at Top) I reckoned 142.

But before I treat of the Observation, it becomes necessary to remark two Things, *viz.* that I suppose, that the Divisions of the Horizon into Degrees Eastward and Westward begin from that Point, where the Meridian intersects the Horizon in the N.: And besides, when I mention the Degrees of the Horizon, or Degrees only, I mean those Distances which can be defined by the vertical Circles reaching to the Degrees mentioned.

In ſetting down my Obſervations, I made uſe of apparent Time (*p. m.*) Afternoon.

At $5^h \frac{1}{4}$, there appeared near the Horizon a blackiſh Zone, with it's upper Limb of a Sky-colour, ſomewhat obſcure. Above this Zone was another very luminous, reſembling the Dawn pretty far advanced. The higheſt Zone was of a red, fiery Colour. The Altitudes of the Zones ſeemed to bear ſuch Proportion, that the ſecond was double the firſt, and the third triple: And, at the ſame Time, they in many Places roſe ſomewhat above the 40th Degree of Altitude. Eaſtward they extended to the 55th Degree on the Horizon, and Weſtward to the 70th. They had 3 perpendicular ſlender Diviſions, like Slits; but they were parallel to the Horizon, excepting that the third had ſome Parts of it's upper Limb unequal in Height, with ſome Aſperities upon it; and from the firſt to the ſixth Degree Weſtward, a Sort of Beam wider than the reſt was obſerved. The Stars of Part of the *Great Bear*, the *Dragon*, *Hercules*, and others, appeared more or leſs through the *Phænomenon* (and others afterwards, according as the Appearances varied). But through the lower Zone they appeared more obſcurely, and in ſome Places not at all: Through the middle Zone, they ſhone bright; but through the higheſt, they were leſs diſtinct.

I cannot determine with Certainty the firſt Moment of the Appearance of this *Aurora*: Nor indeed does it ſeem feaſible, to define the Riſe of ſuch *Phænomena* with ſufficient Accuracy. But it is worthy of Remark, that after Sun-ſet on the preceding Days, as well as this, there appeared in the W. a remarkable Redneſs expanded on each Side: And moreover, on the enſuing Evening, the ſame bright red Colour, appearing near the Horizon, deceived the common People into a Belief, that a new *Phænomenon*, like the foregoing, was breaking out of the Horizon. Wherefore I am of Opinion, that in this Caſe there is a conſiderable Difference between the *Aurora Borealis*, and the Redneſs occaſioned by the Sun's ſetting.

About $\frac{1}{4}$ of an Hour after, the Length of the Zones was contracted, their Extremities having receded about 10° from the E. and W. The white lucid Part was not now ſo diſtinguiſhable from the red as before: And this laſt Colour grew fainter almoſt every-where elſe but at the Weſtern Limit, where it was more vivid: But in that Weſtern Space, from which the *Aurora* was withdrawn, there remained a brighter Space of 3 or 4°, ſurrounded by a ſmall black Cloud, ſo that it ſeemed to be a Kind of *Hiatus*. Near our *Zenith* there appeared ſome thin lucid Clouds, partly of a whitiſh Red, in ſuch a Manner, that they ſeemed as if occaſioned by the burning of Houſes at ſome Diſtance to the N. Others of this Sort had happened before, and ſome were ſeen afterwards.

A little after ſix, the upper Parts began to emit red Streamings, or Rays, in Plenty; but in theſe, the Red was now-and-then intermixed with whitiſh and darkiſh Colours.

In

In a few Seconds after, there issued forth from the very Æquinoctial W, a red and very bright Column, which ascended to the third Part of the Heavens; and a little after, it became curved in the Shape of the Rainbow.

At ¼ after six, the red Colour appeared fainter, and the Zones were not so distinct from one another; the *Phænomenon* reached only to the 20th Degree E, but to the W. it retained it's Length, as before.

At 7 the *Phænomenon* appeared interrupted, and divided into two Parts, the intermediate Space becoming almost invisible. The red Part of it's Western Extremity was curved into an Arch terminating near the Horizon. Not far from the 84th Degree to the W, there appeared a Sort of *Hiatus*, not unlike that in the E. already described, and which had vanished by this Time.

Seven Hours 20′ the whole *Aurora* was become paler, so that the red Colour was scarce discernible, except at the Western End, where it was of the Colour of Fire.

A little after eight, the lowermost of the Zones, as they now stood, was blackish; and above this another whitish bright one was seen: And some Parts of these seemed to fluctuate, and be agitated (as it often happened before and after); and, if any of them disappeared, they were soon succeeded by others.

At ½ an Hour after eight, almost in an Instant of Time, the bright Zone, from the 8th Degree W. to the 50th E, became more vivid, and rose higher; and above this appeared a new large one, of a red fiery Colour, with several successive Streamings tending upward, and passing 60° of Altitude: The Western Part had assumed the Form of a thin Cloud.

A little before nine, at 16° Eastward, a curved red Beam, (or Bow) though irregular in some of it's Parts, rose up to the *Zenith*; and at the same Time such another, commencing at the Horizon beyond the 80th Degree W, arose to the same Height, and joined the Eastern Arch in the *Zenith*.

At 9, after these Beams had been up to the *Zenith* a very short Time, they parted, and began to fall considerably lower: But in that Place where they were in Contact, there remained a certain reddish Cloud, which gradually changed in Magnitude and Figure: However, I never observed it to assume that Figure which might properly be called a *Corona*. In some Time it vanished, as the other Appearances did from the *Zenith*: Nay, the whole *Phænomenon* grew less, and fainter; and was reduced to the irregular Form of bright Clouds and Beams, whose Light still diminished.

At ¼ after nine, the Western Part was transformed into the Appearance of one Cloud, of a very red Colour, with very little Roughnesses on it's Edges; but it was somewhat more contracted than before.

A little after 10, the Heavens became brighter from the 84th Degree W, to the 18th Degree Eastward, and to 50° high, or better.

At $10^h\ 36'$ the *Phænomenon* was contracted, being now about 10° in Longitude shorter on each Side. But it's upper Part was very red, as if on Fire, with several Rods, or narrow Beams, shooting from it. In a Word, the Disposition and Brightness of it's Parts came very near the Shape and Vigour the *Phænomenon* had at the Beginning.

At 11 the red Part did not afford the Sight of these Rods and Dartings; and the Colour being now fainter and pale, the whole *Aurora* was divided into 2 Parts, and the Light was weaker.

In 10′ after, the intermediate Scissure was larger, being now near 20°; and the Part on the Right-hand ran somewhat E.

About $10^h\ \frac{1}{2}$, the Redness became stronger, but more so to the W. than to the opposite Part.

In $\frac{1}{4}$ of an Hour, both the Light and Redness diminished; so that the only Space that retained a vivid Light was that of 6° to the W.

At 12 the Light of the *Aurora* was nearly extinct, there appearing only a very weak Light along the Tops of the Mountains.

Twenty Minutes after, there appeared a white brightish Beam, at 30° W, and 60° high; but it soon became invisible.

In $\frac{1}{2}$ an Hour after, a very weak Light remained in the West, near the Horizon; which had not been observable, if the Brightness of the preceding *Phænomenon* had not invited me to continue the Observation.

At $\frac{1}{4}$ after one, that weak Light was much contracted.

The Tranquillity of the Air continued the same, or nearly such, as in the Beginning; and yet there was not the least Report, or even hissing Noise, heard to issue from so much Matter.

At $1^h\ 30'$, that Part of the Heavens where the *Aurora Borealis* had shone forth, was no Ways different from the rest; and the only Light in the Sky proceeded from the Stars, and the Moon, which was now up.

I had at other Times observed some luminous Appearances in the Heavens, which may be referred, in some Measure, to the Class of the *Phænomenon* above described; but I was of Opinion, that the Memory of this ought to be preserved with the greater Diligence, as it far surpassed all that preceded it in Magnitude, Light, Figure, Colours, and Duration.

—*At the Observatory of the* Institute *of* Bononia. *By Dr* Eustachio Zanotti, *Deputy-Prof. Astron. Translated from the* Italian, *by* T. S. *M. D. F. R. S. Ibid.* p. 593.

3. The *Aurora Borealis*, which was formerly a rare *Phænomenon*, and almost unknown in this our Climate, is now become very frequent. In *Bononia* a great Number have been observed for some Years past, as appears by the Register of the Observations made in this *Institute*. This Time it was so very remarkable, that I do not think any one remembers to have ever seen the like. As to it's Extent, it spread so as to occupy about 140 Degrees of the Heavens: And, as to it's Light, it was so vivid, as by it to distinguish Houses at a great Distance; which seemed of a red Colour, and made some People attribute this Light to a Fire in the Neighbourhood. But when they were assured what it was, they remained no less frighted, superstitiously believing it impossible,

poſſible, that ſuch an uncommon Light, and of a red Colour too, like Blood, ſhould appear in the Sky without preſaging ſome unhappy Accident. While the whole City was intent upon viewing this new Appearance, I and ſome young Gentlemen were employed in calculating the *Ephemerides*; and, being apprized thereof, we jointly began to take Obſervations of it. This uncommon Light drew to the Obſervatory ſeveral others, that were uſed to come at other Times: But I ſhall only relate what is entered upon the Regiſter of Aſtronomical Obſervations, leaving to thoſe who are fond of philoſophical Hypotheſes, to inveſtigate it's Cauſe according to their Fancy.

$7^h\ 9'$, *p. m.* when we firſt perceived the *Aurora Borealis*, it's Centre was near the N. Pole. The Brightneſs extended along the Horizon about 70°, and it's Height was judged 20°. The Sky was almoſt totally overcaſt with Clouds, but the Light was viſible in ſeveral Parts, where the Sky was clear. The two Stars, ζ and ε, of the *Great Bear*, ſhone bright in the Midſt of the reddiſh Light of the *Aurora*.

$7^h\ 34'$, no Change having happened for ſome Time, the Light now appeared ſomewhat weaker, and removed from it's Place; for it's Centre was no longer in the North, but paſſed Weſtward. [N. W.] The Stars, ζ and ε, were ſtill viſible, but more Eaſtward, with reſpect to that Part where the Light was brighteſt.

$7^h\ 39'$, the Light continued diminiſhing. To the W, the Sky was quite overſpread with Clouds; ſo that it was not poſſible to diſtinguiſh it's Limits.

$7^h\ 42'$, the *Phænomenon* on a ſudden re-aſſumed new Strength, and became more vivid, and of a Colour as red as Fire.

$7^h\ 44'$, it again became languid, but was ſpreading at the ſame Time. To the E. it was not poſſible to determine it's Limits, by reaſon of the Weakneſs of the Light, which diſappeared by Degrees. About the Pole, and to the Weſt, it was loſt behind the Clouds.

$7^h\ 49'$, it continued to ſpread wider, and had already taken in the two Stars, β and γ, of the *Dragon's-Head*, and *Lucida Lyræ*.

$7^h\ 52'$, the Expanſion of the Light ſtill increaſed, which took in a great Part of the *Swan*, ſurrounded by a Miſt. At this Time the Height of the *Aurora* was 40°, and it's brighteſt Part was a little under *Lucida Lyræ*.

$7^h\ 54'$, on the other Side towards the N, the two Stars, δ and γ, of the *Great Bear* were immerſed in the Light.

$7^h\ 59'$, the *Aurora* formed itſelf into a concave Arch towards the Horizon. The Polar Star was near the Top of it's Convexity, and ſome Stars ſhone bright in the Midſt of the Light; and, among theſe, δ and γ, of *Urſa major*. The concave Part was terminated by a Baſis ſomewhat dark; which ſeparated the red Light of the Arch from a white and very bright Light, that remained within it. The Arch, which was 15° broad, was of a deeper Colour towards the Horizon than towards the Pole. The Weſtern Limit, which was interrupted by Clouds,

Fig. 38. Clouds, was wider and more irregular than the Eastern Limit. Fig. 38 exhibits the *Phænomenon* conformable to the Description now given.

$8^h\ 9'$, to the W, the Limit of the Arch remained confused, though of a red Colour, somewhat vivid: But to the E. it became more faint, and changed rather into a whitish Colour.

$8^h\ 19'$, the red Light spread to the Constellation of the *Dolphin.*

$8^h\ 22'$, the Arch, which was still distinct, grew bigger, passing Eastward by the two Stars, κ and ι, of *Ursa major*, and Westward by the Stars of the *Swan's Tail.*

$8^h\ 29'$, *Lucida Lyræ* remained clear of the red Light, which moved higher, and was immersed in the bright Light.

$8^h\ 30'$, at the Eastern Limit of the *Aurora*, that is, at 54° from the N. Pole, there was suddenly seen to rise vertically up, a Beam of Fire, at first of a very bright Light; but, in Process of Time becoming more resplendent, it changed into a red Colour, like that of the Moon in the Horizon.

$8^h\ 31'$, the Light still increased in Vigour, and was now entirely like the red Rays, which are separated by the Prism. It's Figure was changed; for it resembled a Pyramid, with it's Basis on the Horizon, 4° wide, and it's Height was about 20°. Near the Top of the Pyramid, the Redness was less than at the Basis, and it's Limits were not very distinct.

$8^h\ 34'$, the red Light continued spreading, and made, as it were, a Basis of a weaker Redness, for the aforesaid Pyramid. At this Time the *Aurora* appeared unsettled and curious, as in Fig. 39. At it's
Fig. 39. Eastern Limit the Pyramid continued visible, but of a more intense Colour towards the North, and from it's Middle there shot up vertically a Streak of Light, between a white and a yellow Colour. A very dark narrow Cloud crossed the whole *Phænomenon*, and went to terminate in the Pyramid. At the upper Part, a considerable Track of the Heavens was enlightened with a very vivid red Light, which was interrupted by several Streaks or Columns of a bright yellowish Light. The said Streamings shot up vertically, and parallel to each other; and the narrow Cloud seemed to serve them for a Basis. Under the Cloud there issued forth two Tails of a whitish Light, hanging downward on a Basis of a weak Red, and it seemed as if they kindled and darted the Light downward. There was likewise seen a white Streak, which passed across these two Tails, and extended from one End of the *Phænomenon* to the other, in a Position almost parallel to the above-mentioned Cloud. Westward the Sky was all cloudy, so as to suffer nothing to be observed. At this Time some of the Company perceived other little Shootings, like those which are frequently seen in Summer, and are commonly called *falling Stars.* More than one of these were observed in that Part of the Heavens that was free from the *Phænomenon*, at about 45 Degrees of Altitude, not far from the East.

$8^h\ 36'$,

$8^h\ 36'$, there re-appeared a Portion of the Arch, which was ſeen at firſt. The Pyramid was ſpreading, and loſing it's Figure.

$8^h\ 38'$, the very bright red Light, which firſt formed the Pyramid, ſpread Northward on the Tracks of the Arch; which nevertheleſs contained within it a bright Light extending to the Horizon, excepting that it was covered here and there by Clouds.

$8^h\ 39'$, the Stars, ζ, ϵ, of *Urſa major*, ſhone through the red Light, which contained ſeveral white luminous Streaks.

$8^h\ 44'$, the red Light, now very vivid, was all interſperſed with white luminous Streams, which darted out of the Baſis or lower Extremity of the Arch. To the W, the Northern Light terminated exactly in a white Streak, and Eaſtward it ſpread as far as the Horizon. The N. Pole began again to become red, yet there ſtill remained ſomewhat of the uſual bright Light between the Red of the Pole, and that of the Arch.

$8^h\ 51'$, the red Arch began alſo to appear to the Weſt, and reached to the Stars of the *Swan*, which at firſt were hid by the Clouds.

$8^h\ 54'$, the red Light began to ſpread on every Side, but ſtill contained within it ſomewhat of the Brightneſs. The *Zenith* was now all red, and with it that Part of the Sky which takes in 70° on each Side. Fig. 40 exhibits the *Phænomenon* as it was obſerved at that Time. The Circle deſcribed by the Figure denotes a Parallel to the Horizon at the Altitude of 45°; on which is a Portion of the Arch, ſo often made mention of. Fig. 40.

$8^h\ 56'$, there appeared ſeveral white Streaks to the Eaſt, where the Light of the *Aurora* was ſtrongeſt; which Light was riſing higher, and ſeemed to have entirely quitted that Part of the Sky near the Horizon.

$9^h\ 4'$, there now remained but a little reddiſh Light at the N. Pole; all the reſt was collected near the *Zenith*, not extending lower than the Star α of *Urſa major*. In the South, where the Sky was clear, there were ſeen ſome of thoſe Stars which we have called *falling Stars*.

$9^h\ 6'$, about the *Zenith* the Light continued red and vivid, but deſcended lower. The *Aurora* abandoned the E, and took Poſſeſſion of the N. W. It appeared as if the Coruſcations had almoſt conſtantly taken their Riſe from the Eaſtern Quarter, and afterwards extended to the W.

$9^h\ 9'$, a conſiderable Streak, or Track of red Light, more vivid than the reſt, croſſed the Stars of the *Swan* almoſt horizontally.

$9^h\ 12'$, in the Eaſt, where the *Aurora* ſeemed to have entirely diſappeared, it began again to make it's Appearance; but to this Time the Light was but faint, in Compariſon of that which was ſeen in the Beginning.

$9^h\ 19'$, the Light was become pretty faint, and confined within a ſmall Space, at the Height of about 40° above the N W. Many little Changes that occurred, are not ſet down, it being impoſſible to keep an

Account

Account of them all, inaſmuch as they ſucceeded one another very quick.

$9^h\ 34'$, the *Aurora* ſeemed entirely extinguiſhed. In ſome Minutes after, it began to revive; but the Clouds, which were in great Numbers, and ſpread round on every Side, left but a few little Spaces free. The greateſt Brightneſs was in the *Zenith*, which appeared like a red Veil, declining to the N. where it loſt itſelf behind the Clouds.

$11^h\ 6'$, the Light gathered new Strength, and was all at N. up to 20° of Altitude, the *Zenith* being quite clear of it. The Brightneſs was greateſt about the Pole, and grew weaker as it receded from it, taking in, upon the Whole, 90 Degrees of the Horizon. The Clouds continued to increaſe, and prevented ſeeing the *Phænomenon* but now-and-then; and in this manner the Light laſted to the 13th [1ſt] Hour. Some ſay they have ſeen Foot-ſteps of it at the 16th [4th] Hour; but our Company parted long before from the Obſervatory, thinking it entirely at an End; and the rather, becauſe the Clouds had deprived us of all Hopes of being able to purſue the Obſervation.

I ſhall add ſome things which have been communicated to me by the celebrated Dr *Beccari*, and are of his own Obſervation.

The Day of the *Aurora*, the Barometer was very high, *viz.* at 28 Degrees 5 ½ Lines. The preceding Day, the Winds were different, in different Regions of the Air. Near us [the Earth], the Wind was W. N. W. and pretty cold. Higher up, the Clouds came from the E. and moved Weſtward; which Clouds were globular Collections of Miſts. Above that Region the Wind blew at South-weſt by South, as appeared by ſome ſmall Fleaks of Clouds coming from that Quarter. The 16th Day, the Wind that reigned in the Region of the Clouds was *Greco-tramontana*, and was in the ſecond Degree of Strength.

Several Perſons have poſitively aſſured us, that, in the Evening of the 16th Day, they perceived a certain Stench in the Air, like that which is ſometimes occaſioned by a Fog. The ſame has been taken notice of at other times, when ſuch *Phænomena* have appeared.

There was a very thin Fog in the Air not only on the 16th Day, but alſo on the preceding and enſuing Days. The Mornings of the 17th and 18th, before and a little after Sun-riſe, the Air appeared of an uncommon fiery Colour. The Evening of the 17th, the Crepuſculum was of an extraordinary Height. Between the N. and W. there was ſeen a very thin red Vapour, which laſted almoſt till Night.

The various Appearances of the *Aurora*, obſerved by that Gentleman, are here omitted, becauſe they very well agree with thoſe above deſcribed.

—*At* Rome *by the Abbot* Didacus de Revillas *Pub Prof. Math. and F. R. S. Ibid.* p. 601.

4. Tho' I was not at leiſure to obſerve a very bright Light in the Heavens till $7^h\ 30'$, yet I had the following Informations from Eye-witneſſes of good Credit. 1. When the Twilight was ſcarce ended, the N. Part of the Heavens was ſtained with a red and fiery Colour, intermixt afterwards with ſome whitiſh Streaks. 2. A little afterwards the

the Light declined from the N. toward the W. the Streaks sometimes vanishing and then appearing again, resting for some time upon 2 whitish concentric Arches, very near the Horizon. 3. Near their Edges a more vivid Light darted from the Horizon. 4. About 7^h the Conflagration travelled again toward the N. and the Arches at the same Time disappeared. Thus much I had from others: The rest is from my own Observation.

At 7^h $30'$ a red and fiery Light illuminated the N. Part of the Sky, which was then serene: It rose to the Height of about 8° and extended 10° to the E. and 35° to the W. and the fixed Stars appeared thro' it. Near the Horizon there was an unusual whiteness in the Sky, resembling the true *Aurora*.

At $40'$ a blackish Streak rose toward the Polar Star, and in the mean time the red Colour was gradually propagated.

At 7^h $45'$ a great Conflagration was collected between 26 and 30° from the N. toward the W, and two whitish Streaks arose converging a little in the under Part: A little afterwards the Conflagration rose to the Height of 40° the N. Part of the Heavens being scarce sensibly red.

At 8^h the Inflammation was abated, where it had been very bright a little before. But at the N. and at E. N. E. as it went higher, it was kindled again.

At $15'$ the horizontal white Light appeared in all the N. Part under an Angle of Elevation of almost 7°, but the Conflagration which occupied the upper Parts tended a little to the Horizon near the N. E. Perhaps this Whiteness had the Figure of an Arch, but the Buildings hindered the Sight of it. A little afterwards at 32° to the Westward of the N. the red Colour almost disappeared. At the N. and N. E. it was more intense. In the mean time, about *Lucida Lyræ*, there appeared an almost elliptic *Area*, of a bright red Colour, it's greater *Axis* being perpendicular to the Horizon, and white Streaks were sent from it toward the Top of the Sky. Another equally white extended from the Bottom of the Ellipse to the Head of *Ursa Major*. The greater *Axis* of the *Area* occupied about 10°.

At $20'$ this new elliptical Conflagration was raised higher, and declining a little from the W. receded from *Lucida Lyræ*, assuming a trilateral Figure, or rather that of the Sector of a Circle, the Arch being turned to the Horizon. In the Centre of this Sector was a bright Star in the Breast of the *Swan*, much about the same time a bright Streak inclining from the burning N. Part toward the Centre of the Sector terminated the almost darkened intermediate *Trapezium*.

At $45'$ the lucid Sector disappeared. A blackish Cloud between 28 and 30° from the N. toward the W. almost the whole Conflagration was extinguished, except about the Polar Star.

At $50'$ a Light of a blood-like red Colour was again kindled between the Polar Star and the Head of *Ursa Major*, and was extended above the Height of 60° : In the mean time another Portion of the Heaven was kindled, separate from the former; toward the W. A little afterwards the intermediate Space was also kindled above, a whitish and bright Interval being left near the Horizon at N. N. E.

At 9^h a greater Conflagration about the Head of *Ursa Major*, a red Light was diffused almost to the Zenith, and again toward the W. many blackish Streaks converging a little below rose up within the Conflagration, and the largest of them reached to the Polar Star.

At 9^h 10′ the Streaks disappeared, a brighter Conflagration continuing above the Head of *Ursa Major*, and extending itself quite to the Horizon, which was whitish a little before.

At 13′ a greater Conflagration about the Polar Star. But the whole northern Hemisphere was very red.

At 20′ a large whitish Streak rose up to 12° from the N. to the W. and the Inflammation about *Ursa Major* was diminished. Between 30 and 34° it continued; where the Horizon, being whitish toward the W. was still tinged with red.

At 30′ it grew very faint, and deeper again near the N.

At 40′ it disappeared again, a faint Light continuing toward the W. which was extinguished by slow Degrees.

At 10^h an Inflammation again at the N. which was extended a little toward the E. but between each Stream of Light a Space of about 15° was interposed, into which the Inflammation on each Side faded, and collected itself again.

At 15′ the Sky was cloudy toward the W. the Horizon only was clear. The Conflagration was now extinguished, scarce any Redness remaining at the N.

At 11^h it revived again at the N. N. E. and continued till Midnight, then it was gradually diminished. But the doubtful Light and Redness of the Air which remained, scarce disappeared in 2 Hours afterwards.

On the 16th	at 9^h *a. m.*	the Barometer was at	28. 1
	9 *p. m.*	— — — —	28. 1 $\frac{1}{16}$
17	7 *a. m.*	— — — —	28. 1

—*by Mr* James Short, *dated at* Edinburgh, Dec. 6. 1737.

5. Yesternight we were surprized upon looking out at the Windows, about Six, to find the Sky, as it were, all in a Flame; but upon further Inquiry, it was nothing but the *Aurora Borealis*, composed of red Light. There was an Arch of this red Light reached from the West, over the *Zenith*, to the E. the N. Border of this Light was tinged with somewhat of a blue Colour. This *Aurora*, as far as I saw, did not first form in the N. and after forming an Arch there, rise towards the *Zenith*, as they commonly use to do; neither did the Light shiver, and by sudden Jirks spread itself over the Hemisphere, as is common, but gradually and gently stole along the Face of the Sky, till it had covered the whole Hemisphere; which alarmed the Vulgar and was indeed a strange

Sight: In fome Places we faw the Clouds pafs betwixt us and it. During the whole Time, which was from 5 till 8, there was a moft violent Wind from the S. W. I looked at *Jupiter* with a 15½ Inch Telefcope, but the Air was in fuch an Agitation I could not fee him diftinctly. *Lucida Lyræ* appeared through the red Light very dim to the naked Eye. About 8 this red Light formed a *Corona*, a little to the S. of the *Zenith*; and inftead of a dark Fund in the Middle, as ufual in fuch Occafions, it was of a deep Red. There was always a great Circle of this Light came from the W. to the *Zenith*, which feemed to be the Magazine whence all the reft were fupplied. It is but about a Year fince I firft obferved this red Light in the *Aurora Borealis*, and only then in very fmall Quantities.

6. It was a ftrong and very fteady Light, as near as can be of the Colour of red *Okre*; it did not feem to dart or flafh at all, but continued going on in a fteady Courfe againft the Wind, which blew frefh from the S. W. It began about N. N. W. in Form of a Pillar of Light, at about $6^h\ 15'$ in the Evening; in about $10'$, ¼ Part of it divided from the reft, and never joined again; in $10'$ more it defcribed an Arch, but did not join at Top; exactly at 7, it formed a Bow, and foon after quite difappeared; it was all the while lighteft and reddeft at the Horizon: It gave as much Light as a Full Moon. ——*by* John Fuller, *jun. Efq*;

At 8^h it began again exactly N. it was very light then, but not near fo light as before; in half an Hour it made an Arch from E. to W. and went quite away to the S. when it ended much with the fame Appearance as it began in the N. but not quite fo red.

Rofehill, [*Suffex*]
Dec. 20. 1737.

An Account, by Mr John Eames, *F.R.S. of a Book entitled,* TRAITE' PHYSIQUE ET HISTORIQUE DE L'AURORE BOREALE, Par Mr DE MAIRAN. Suite des Memoires de l'Academie Royale des Sciences, Année MDCCXXXI. No. 431. p. 243. Jan. &c. 1734.

XLVII. The frequent Appearances of the *Northern Lights* in feveral Parts of *Europe* and *America*, and the furprifingly beautiful *Phænomena* that have been obferved in fome of them, fuch as the Rainbow-Colours, Canopy, *&c.* have very juftly engaged the Philofophers of the prefent Age in a Search after the Caufes of them. Several Hypothefes have been invented and propofed by the Learned, in order to explain thefe things. Moft of them fuppofe thefe *Phofphorus*-like Appearances to proceed from certain *Effluvia*, either perfpired out of our Earth, or at leaft paffing through it. But our ingenious Author has thought of a Caufe very diftant, as well as very different from all thefe, *viz.* the Atmofphere of the Sun, which at fome times fhews itfelf under the Appearance of a Light, which he calls the Zodiacal Light, but at other times produces an *Aurora Borealis*. The Zodiacal Light is the purer unmixed Atmofphere of the Sun: But an *Aurora Borealis* is the Effect of the Solar Atmofphere, confequent upon it's making a Defcent into, and blending itfelf with the Atmofphere of our Earth, at certain Times and Seafons of the Year. But a more particular Account of this Matter will be given hereafter.

The learned Author of this Work has taken a great deal of Pains in compiling it. He has looked over the Accounts of Meteors, from the fifth Century down to the preſent Time, in the Hiſtorical Part; and has ranged them in very good Order, in Regard of the ſeveral Returns of this *Phænomenon*, making ſuch Remarks by the way, as ſerve to ſupport his Solution of it in the Philoſophical Part.

By a Return he does not mean barely a ſingle Appearance, but a Series of them after a Ceſſation or Non-appearance for ſeveral Years. Thus he makes but twenty-two Returns from the Year 400 to 1716, while the ſeveral Appearances of theſe Lights from 1707 to 1710, after a ceaſing to be ſeen for 20 Years, are reckoned but one Return.

Mr *Mairan* hopes the learned World will take the whole Performance under their Conſideration, and give their free Thoughts upon it.

The Work conſiſts of 5 Sections; the firſt gives a ſhort Hiſtory of the Zodiacal Light. In the ſecond he treats at large of the Atmoſphere of the Earth; it's Altitude, and the Height of the *Aurora Borealis* in it, and the Excluſion this Circumſtance gives to ſome of the Cauſes, which have been already aſſigned, of this *Phænomenon*. In the third he propoſes the Cauſe, and accounts for the Formation of this Appearance in general, and then deſcends to a Detail of the ſeveral Particulars, adding the Solution of each. The next Section is employed in relating the Hiſtorical Proofs of his Hypotheſis concerning the *Northern Lights*, taken from the Records we have of ſeveral Appearances of thoſe Lights, to be met with in ancient Authors, compared with thoſe of the Zodiacal Light, their ſuppoſed Cauſe, and the Situation of the Earth in her annual Orbit at thoſe times. The laſt Section conſiſts of 28 curious Queſtions concerning ſeveral other *Phænomena* of Nature, which the ingenious Theoriſt believes to have a Dependance upon his new Hypotheſis, and explicable by it.

But a more particular Account of theſe Matters may juſtly be expected.

Mr *Mairan* begins the whole with laying before the Reader a ſhort View of his Hypotheſis concerning the Nature of an *Aurora Borealis*, defining his Terms as he goes along.

The *Aurora Borealis*, ſays he, is a luminous *Phænomenon*, ſo called from the Place of it's Appearance, uſually in the Northern Parts of the Heavens, and with a Light near the Horizon, reſembling that of the Morning Dawn. This Name is ſuppoſed to be firſt given it by Mr *Gaſſendi*, but it appears otherwiſe, from a Place in his Animadverſions on *Diogenes*, quoted by Mr *Mairan*.

The Cauſe of an *Aurora Borealis*, in general, he takes to be a Light called the Zodiacal Light, which is in reality nothing elſe but the Atmoſphere of the Sun ſpread on each Side of him along the Zodiack, in the Form of a Pyramid. This ſometimes is extended to ſuch a Length as to reach beyond the annual Orbit of our Earth, and in theſe Circumſtances ſometimes to blend itſelf with our Atmoſphere, and being of an Hete-

Heterogeneous Nature, produces the ſeveral Appearances which are obſerved in, and uſually compoſe, the *Northern Lights.* This he undertakes to explain, and prove more largely, in the Sequel of the Work.

A Diſcourſe upon the Nature of the *Zodiacal Light*, or Sun's Atmoſpere, and the Matter of which it conſiſts, is the Subject of Chap. 3. That it is very different from the ambient Æther, he ſays is evident, in that the Æther reflects none of the Light of the Sun, is extremely rare, and altogether imperceptible. Whether the *Zodiacal Light* of the Solar Atmoſphere be any Emanation from the Body of the Sun, a Species of Effervеſcence, or Depuration of it's groſſer Parts, an Amaſs of Heterogeneous Parts diffuſed in the Æther, that meeting from all Parts, tend towards the Sun, *&c.* he will not undertake to determine.

It is enough for his Purpoſe, that it is of a luminous Nature, whether in itſelf, or becauſe ſtrongly illuminated by the Rays of the Sun, whoſe Body it environs. He does not deny but that it may be alſo of an inflammable Nature, nay actual Flame or Fire, though very fine and rare.

He obſerves, that the Form in which the Atmoſphere of the Sun is commonly ſeen in total Eclipſes of the Sun is round, though ſometimes conical, of which he gives us a Figure.

At all other times it moſt uſually preſents itſelf to us in the Form of a lucid Pyramid, or Lance, lying oblique to the Horizon, along the Zodiac, and for that Reaſon called by the late Mr *Caſſini* the *Zodiacal Light.* Mr *J. Childrey* in his Hiſtory of the Natural and Artificial Rarities of *England*, deſcribes it thus: There is another thing which I muſt needs recommend to the Obſervation of Mathematical Men, which is, that in *February*, and for a little before, and a little after that Month, as I have obſerved ſeveral Years together about ſix in the Evening, when the Twilight has almoſt departed the Horizon, you ſhall ſee a plainly diſcoverable Way of the Twilight, ſtriking up towards the *Pleiades*, and ſeeming almoſt to touch them. It is to be obſerved any clear Night, but it is beſt ſeen *illuni Nocte.* There is no ſuch Way to be obſerved at any other Time of the Year, that I can perceive, nor any other Way at that time to be perceived darting up elſewhere; and I believe it has been, and will be, conſtantly viſible at that Time of the Year. But what the Cauſe of it in Nature ſhould be, I cannot yet imagine, but leave it to farther Enquiry.

Upon a farther and cloſer Enquiry, and Conſideration of this Matter, the ingenious Author, Mr *Mairan*, tells us, he takes it to be the Solar Atmoſphere, and therefore treats at large of the Reality, Viſibility, and Antiquity of this Light.

I beg leave to tranſcribe the Accounts of the ſame, given in by the Reverend Dr *Derham*, Canon of *Windſor.* He informs us, that about ¼ of an Hour after Sun-ſet, *April* 3, 1707, he perceived in the Weſtern Parts of the Heavens a long ſlender Pyramidal Appearance, perpendicular to the Horizon. The Baſe of this Pyramid he judged to be the

Sun,

Sun then below the Horizon. It's *Apex* reached 15 or 20° above the Horizon: It was throughout of a rufty red Colour, at firft pretty vivid and ftrong, but the Top part much fainter than the Bottom nearer the Horizon. He did not remember he ever faw any thing like it, except the white Pyramidal Glade, which is now entitled by the Name of the *Aurora Borealis*, that being like it except in Colour and Length. Again, on the 20th of *March*, 1715-16, in the Evening, he efpied a very odd Sort of Light in the Conftellation of *Taurus*. This Glade of Light had the fame Motion that the Heavens had, and was much like the Tail of a Comet, but pointed at the upper End. This Light, I doubt not, is fuch as Dr *Childrey* firft obferved in *England*, and *Caffini* and others afterwards in *France*.

Mr *Mairan* proceeds to give an Account of the true Figure, Extent, Situation, *&c.* of this Light, or Atmofphere of the Sun. It's true Figure he judges, with Mr *Fatio*, to be lenticular, and gives a Projection of it upon the Plane of the Sun's Equator, the Eye being fuppofed in the Axis of the Sun produced through his South Pole at fuch a Diftance as makes the Solar Atmofphere appear under the Angle of 45°. In it you have a View of the Nodes, Poles, Limits, Declination, and Extent, paffing through and beyond the Orbits of *Mercury* and *Venus*, and in fome Parts beyond the *Orbis Magnus*. This laft Article of it's Extent he demonftrates from feveral Obfervations of the Elongations of the Apex of this Pyramid from the Centre of the Sun. This has been found to be fometimes double that of *Venus*, and other times 90° and once or twice above 100, whereas an Elongation of 90° gives the Diftance of the *Apex* from the Sun equal to that of the Earth at the time of Obfervation.

This Section is clofed with an account of the Changes, both real and apparent, to which the *Zodiacal Light*, or Solar Atmofphere is liable. It's Length has been for fome time upon the Increafe, afterwards in a diminifhing Condition, and has been altered fo much in the Compafs of 37 Months, as to have been 30° longer at one time than at another. The Changes in Luminoufnefs, Denfity, and Tranfparency, has likewife been found to be very confiderable. Sometimes the *Zodiacal Light* has been fo rare and weak as to be but juft vifible, afterwards for a long time not vifible at all.

Hereupon our ingenious Author thinks proper to obferve, that thefe Confiderations may ferve in fome Meafure to account for the Inconftancy of the *Aurora Borealis*, as alfo for their Non-appearance for fome Years; fince they owe their Original to, and have fo clofe a Connexion with, the *Zodiacal Light*, whofe Appearance is fo uncertain. Add to this the *Zodiacal Light*, as he afterwards fhews, muft not only be of a fufficient Length and Denfity, but the Earth muft be in or near the Nodes, formed by the Interfection of the Plane of the Sun's Equator with the Plane of the Ecliptick.

The

The ſecond Section treats at large of the Altitude of our Atmoſphere, and of that of the Region in it uſually poſſeſſed by the *Aurora Borealis.* Under this Head he diſcourſes of the ſeveral Methods the Mathematicians have uſed to find the greateſt Heights of the Air, ſuch as the Duration of the Twilight, the Altitude of the *Mercury* in the Barometer, and rejects them as inſufficient for that Purpoſe; the Atmoſphere being much higher than what has been ever found by them, and conſiſting of a Fluid much finer than the groſs or common Air, the Height of which laſt only is meaſurable by theſe ways.

Mr *Mairan* therefore goes on to ſettle the Altitude of the *Northern Lights*, after another manner, founded upon ſeveral Obſervations made at very diſtant Places at the ſame time, and fixes ſome *Auroræ Boreales* to be but 100 Leagues, though others are no leſs than 300, and the far greater Number of them about 200 Leagues above the Surface of the Earth.

Mr *Cramer*, Profeſſor of the Mathematicks at *Geneva*, computes the Height of the *Aurora Borealis*, ſeen at the ſame time at *Geneva* and *Montpellier*, *Feb.* 15th, 1730, to be $\frac{113}{1000}$ of a Semidiameter of the Earth, *i. e.* about 160 Leagues.

Mr *Meyer* has propoſed in the Memoirs of the Academy of *Peterſburgh*, a very ingenious Method of finding the Height and Diſtance of a *Boreal Arc*, from any Obſerver, by a ſingle Obſervation. Mr *Mairan* applies this Method to ſuch *Auroræ Boreales* as were capable of it, and finds that the *Boreal Arcs* of ſeveral were no leſs than 100 Leagues high.

It is on this account that in the next Chap. our Author conſiders ſome Solutions that have been offered to ſolve theſe Appearances of the *Northern Lights*, and ſets them aſide as inſufficient, becauſe they ſuppoſe Cauſes which have no Exiſtence, or at leaſt no Efficacy at ſo great a Height in the Atmoſphere.

The next Section is the principal, and is engaged in explaining the ſeveral particular Appearances of the *Lumen Boreale*, ſuch as it's Situation, ordinarily towards the N. declining a little towards the W. it's dark, duſky, circular Baſis, ſurmounted ſometimes with one or more luminous Arcs; from behind which Columns, or Streams of Light, ſeem to iſſue either perpendicularly or concentric with the Arcs; add to theſe the Rainbow-Colours, Flaſhes, Vibrations, and in the laſt Place, a Glory, Canopy, or *Corona*, formed by a Concourſe of the Rays of the Matter of this *Phænomenon*, near the Zenith of the Place.

Mr *Mairan* premiſes an Inveſtigation of the *Locus*, or Limit of the Attractive Forces of the Sun and Earth, ſo that a Particle of Matter placed any where in it, will be equally attracted by both, or tend as much towards the Earth as it does towards the Sun. He finds, that if in a Line connecting the Centres of the Sun and Earth, a Point be taken at the Diſtance of about 43 Semidiameters of the Earth from her Centre, that Point will be in this Limit, ſo that a Particle placed there, will not gravitate either towards the Sun or Earth, but remain in *Æquilibrio*,

the

the equal and contrary Forces of the Sun and Earth destroying each other. The Use he makes of this is to shew, that an *Aurora Borealis* may possibly be formed by a Descent of the *Zodiacal Matter*, lying between this Point of *Æquilibrium* and the Earth; though it does not reach so far as to involve the Earth itself. But the *Aurora* in this Case will be an incompleat and particular one.

The *Lumen Boreale* ordinarily appears in the Northern Parts of the Heavens, because tho' the whole Atmosphere of the Earth be involved in the *Zodiacal Matter* (or Solar Atmosphere) yet it is thrown off both Ways, from the Equatorial towards the Polar Regions.

This is owing to a double Cause, the first is the centrifugal Force, arising from the diurnal Motion of the Earth, which being greatest at the Equator (and gradually lessening as you approach the Poles, where it vanishes) makes greatest Opposition there, and not only hinders the Entrance of the *Zodiacal Matter* into the Earth's Atmosphere, near the Equatorial Region, but turns it aside into a Course towards each Pole; and the Author does not question but an *Aurora Australis* might be seen at proper Times in the Southern temperate Zone, just as an *Aurora Borealis* is in ours, which is Northern, attended with similar *Phænomena*, were there but attentive Observers

The second Cause is the progressive Motion of the Earth in it's annual Orbit near one half of the Year with the *North Pole* foremost, and in the other half with the *South Pole*, moving through the *Zodiacal Matter*.

The natural Consequence of which will be a heaping up of Matter, more on the Polar Regions than the Equatorial or Temperate, and this accounts in Part for the Declination of the Centre of the luminous Arcs, sometimes near 10° from the *Pole*; the Direction of this Motion of the Earth not coinciding with the Direction of the Axis of the Earth at those Times.

The dark arcular Segment next the Horizon appearing like a heavy black Cloud, or Mist, is formed out of the densest and specifically heaviest Parts of the *Zodiacal Matter*, which in their Descent must sink deepest into the Earth's Atmosphere, and are least inflammable in their Nature, while the rarer and lighter Parts, which are more inflammable and luminous, if not actually inflamed, form the Arc or Arcs that lie above the dark Segment. The ingenious Author speaks of a *Fort de l'incendie*, a Place where the *Zodiacal Matter* collected together, and moving or passing through it, is actually turned into Flame. Thus long Trains of descending *Zodiacal Matter* arriving in their Descent at this Place, being kindled, or at least reflecting the Light of that *Incendium*, produce the several Columns or Streams of Light that appear above, or behind the obscure circular Base, or luminous Arches.

The Breaks that are sometimes visible in these Arches, are occasioned by the Descent and Passage of several discontinued Trains and Flakes of of the denser and least inflammable Parts of the *Zodiacal Matter*, between the Eye of the Spectator and the luminous Arch.

The various Colours arise from a Separation of the Rays of Light from each other, either by a Sort of Filtration in passing through Mediums of different Densities, or by the Divergence of the differently refrangible and coloured Rays (or rather from the different Celerities of those Rays, as the Author says he has explained more at large in another Place) after the Manner that the Colours are formed in Clouds near the Horizon about the rising or setting Sun.

To conclude, the Canopy in a compleat *Aurora Borealis* he looks upon to be an Object purely optical, a simple Appearance arising from a singular Distribution of several perpendicular Columns, or Trains of *Zodiacal Matter*, as he explains more at large in two Figures. This Exactness and Regularity in the Distribution makes it an uncommon *Phænomenon*; so that among an hundred *Auroræ Boreales* that have been observed, he has met with but three attended with a *Corona*.

What remains, is only to take Notice of some of the Queries which relate to several Appearances in Nature, that seem to be explicable by our Author's Hypotheses of a solar Atmosphere, such as the *Nebulæ*, or lucid Spots observed among the fixed Stars, the Spots in the Sun, the Atmosphere and Tails of Comets, *&c.*

The *Nebulæ* are certain luminary Spots or Patches, which discover themselves only by the Telescope, and appear to the naked Eye like small fixed Stars. They are six in Number, and are accurately described in *Philosoph. Transact.* N° 347 *. Some of them have no Sign of a Star in the middle of them, and are properly *Nebulæ*, others have, and then are called *Nebulosæ*. They are looked upon by some to be in Reality nothing else but the Light coming from an immense great Space in the Æther, through which a lucid Medium is diffused, that shines with it's own proper Lustre, making a perpetual uninterrupted Day, by no Means owing to the Illumination of a central Body, or Star.

But Mr *Mairan* seems to be of another Mind, and queries thus: Since the fixed Stars are Bodies of the same Nature with our Sun, may not some of them have Atmospheres surrounding them so luminary and extending, as to become visible to us by a Light easily distinguishable from that of the central Body, and may not Atmospheres of others be so dense as well as luminous, and extended, as may suffice to obfuscate (to use the Author's Expression) the Light of the Star involved in it? Are not the *Nebulosæ* of the former Sort, and the *Nebulæ* of the latter? The lucid Spot in *Cingulo Andromedæ*, which, after *Hevelius*, our Author continues to call a *Nebulosa*, has been found by the late Mr *Cassini* to resemble the *Zodiacal Light* in some Circumstances, and by Mr *Kirch* to have suffered some Changes appearing and disappearing by Turns.

* See Vol. IV. Chap. III. §. vii.

Mr *Mairan* obferves by the Way, that this Spot was firft difcovered, not by Mr *Bullialdus* in 1660, as is commonly believed, but by Mr *Simon Marius* in 1612, who fully defcribes it in the Preface to his *Mundus Jovialis.*

The luminous Space round the *Nebulofæ* of *Orion's Sword*, difcovered and defcribed by Mr *Huygens*, he takes to be an Affemblage, or Sum total of the feveral Atmofpheres of the Stars, plainly vifible within that Space, and it may be of fome others that are concealed from our View. The Irregularity of the Shape is no Difficulty to him, it arifing from the different, and to us feemingly irregular, Pofitions of their Atmofpheres. He adds, as a Confirmation of his Hypothefis, that the Brightnefs and very Figure of this Space has fuffered fome Alterations fince Mr *Huygens*'s Time. That one of the Stars delineated by Mr *Huygens* without any furrounding Light, has fince been found to have a pale Light like an Atmofphere furrounding it.

Query 2. Is not the folar Atmofphere liable to frequent Fermentations, and fubfequent Precipitations of it's groffer Parts towards the Surface of the Sun? And are not the different Degrees of Brightnefs and Tranfparency owing hereunto? fince the Changes in our Air, or Atmofphere, are not fufficient to account for the Non-appearance of the *Zodiacal Light* in fome convenient Seafons, and clear Nights.

Query 3. May not the Spots, fo often of late obferved in the Surface of the Sun, be owing to thefe Precipitations of the groffer Parts of the *Zodiacal Light*, fince there feems to be fome Analogy or Correfpondence between the Frequency, Ceffation, and Returns of thefe Spots, with the Ceffation, Returns, and Apparitions of the *Zodiacal Light?*

Query 4. Are not the inferior Planets, *Mercury* and *Venus*, almoft always immerfed in the *Zodiacal Matter?* And may not that be one Reafon why it is fo difficult to obferve Spots in them? May not a Change, the Denfity, or Magnitude of the folar Atmofphere, be one Reafon why the Aftronomers at *Paris* have not been able to obferve thofe Spots in the Difk of *Venus* that have been taken Notice of, and defcribed by Mr *Bianchini* at *Rome*, a little before, fince the Telefcopes at *Paris* were of equal Length and Goodnefs?

Query 20. May not the Augmentation of the Quantity of Matter in the Earth and i ferior Planets, by the continued Accumulation of the *Zodiacal Matter* on their Surfaces during a long Courfe of feveral Ages, produce, among other Things, fome Alteration in their periodical Motions?

Query 21, *&c.* May not the Atmofphere and Tail of a Comet be owing to the *Zodiacal Matter*, which the Comet, during it's Paffage through

through the Atmosphere of the Sun, intercepts, and afterwards carries away with it in it's Ascent from the Sun?

Query 28. Is not the Earth safe enough from all Danger of any Inundation, much more of an universal Deluge, tho' it should pass through the Atmosphere, or Tail of a Comet? since the Effects of such a Passage can only be an *Aurora Borealis*, whose Matter is not at all of a watery vaporous Nature? A Conflagration rather than an Inundation might have been imagined to be the natural Consequence, but Experience informs us, that if this Hypothesis be admitted as genuine, that our Earth has been entirely plunged in this *Zodiacal Matter* without any sensible Heat attending it.

A Description of the Northern Lights seen at Wittemberg, *in* 1732, *by* Jo. Fred. Weidler, *L. L. D. P. P. Math. and F. R. S.* No. 432. p. 291. Apr. &c. 1734.

XLVIII. *Feb.* 18, 1732, *O. S.* about 9 at Night, the Sky being clear, there was an *Aurora Borealis*. In the N. there was a dark Arch elevated 20° in it's middle Part, whereas a little before the Sky had been observed to be very clear in the same Part. The Region over this dark Arch was white, and Radiations or luminous Pyramids broke out of it as usual, and thin white Vapours ran swiftly toward the *Vertex*, like little Clouds.

At 10 the Motion of the luminous Matter seemed to cease a little; and presently white Vapours like Waves proceeded again from that white Region of the Heavens: But there was no Resemblance of a Canopy seen toward the *Vertex*.

At $10^h\ 30'$, the white Border of the dark Arch was extended; but only a few lucid Vapours proceeded from it. Lucid Pyramids arose on both Sides near the N. Pole: But the fluctuating Vapours were more frequent toward the W. The Air was continually calm.

Oct. 12, 1732, *O. S.* presently after 6 in the Evening there was another *Aurora Borealis*. A dark Arch was extended between N N W and N E. Above it there was a bright Region of the Heavens, about 10° broad, not exactly expressing the Figure of an Arch. A broader Portion declined about 10° from the N. Pole to the W, and from this, as from a Fountain of lucid Matter, at $6^h\ 30'$ there went several white Pyramids, which reached almost to the Zenith. Some of them were reddish, and presently disappeared, one of them, which was extended between the Northern Crown and *Hercules* to the Zenith, lasted longer; and I observed but one Radiation only toward the N E. Within a Quarter of an Hour the Appearance was over. The Clouds, which before hung about the W, were dispersed by a S W Wind, and driven toward the E. But under them, toward the E, there remained a lucid Region, and the dark Arch was hidden below the Horizon, the white Arch, which lay over the dark one, descended below the Horizon with the Clouds, and the Darkness and thin Clouds being dispersed, there was an universal Brightness at 7^h: But a thin Light possessed the N. Part of the Horizon all Night long.

Besides these Observations, which I made myself, other Northern Lights are said to have appeared, tho' less bright, on *March* 10, *Apr.* 11, *Aug.* 11, and 30, *O. S.* but I have no certain Knowledge of the particular *Phænomena* of them. But from these and other Observations, which I have made in former Years of that Light, I am more and more persuaded that it has it's Seat entirely about the magnetical Pole, or at least that it's Motion is thereby in some Measure ruled and determined, which was first of all apprehended by the great Sagacity of the illustrious *Halley*.

We have not yet any certain Knowledge of the Effects of the *Aurora Borealis*. I have only observed, that some serene Days have always followed that Deflagration. The *Suedes* and *Norwegians*, to whom this *Phænomenon* more frequently appears, are said to have learnt from long Experience, that the Northern Light, when it shines frequently about the Beginning of Autumn, promises a more temperate Air, and plentiful Harvest, for which Reason they call the *Aurora Borealis*, **Rormmod**, or *ripening of Corn.* They also look upon the frequent Returns of them in Winter as a Token or Presage of a sharper Cold, as M. *Leopold* relates in his Letter to Dr *Woodward*. The Experiments made in our Climate in 1731 agree with this *Hypothesis*; for the Northern Light was very frequent and bright in that Year on *Oct.* 4, 7, 8, 10, and 23, S. N. and was followed by so fruitful a Season, that we had a great Plenty both from the Fields and Gardens in 1732.

Observations of the Aurora Borealis *made in* England, *by* Andr. Celsius, *F. R. S. and Sec. R. S. of* Upsal *in* Sweden. No. 441. p. 241. Apr. *&c.* 1736.

XLIX. *Sept.* 13, 1735, at *Woodford*, 6 Miles to the N E of *London*, at $11\frac{1}{2}^{h}$ at Night, there appeared a bright Band, almost parallel to the Horizon; and it's Middle was judged to be under η of the Great Bear. At Times another Light shot along the Great Bear; but almost constantly covered the Stars γ and δ.

Octob. 4, in *King-street*, *Bloomsbury.*

	h	'	"	
At	9	22	0	*p. m.* A Ray or Stream of Light appeared under the Polar Star perpendicular to the Horizon.
	9	24	13	That Ray disappeared.
	9	27	6	Two perpendicular Rays shot forth 5 or 6° from the N. towards the E.
	9	28	30	A whitish Ray again exactly under the Polar Star.
	9	30	0	That Ray moved Westward.
	9	31	0	'Twas seen under η of the Great Bear.
	9	31	27	It entirely disappeared.
	9	36	8	A Ray ascending perpendicularly by the Polar Star, and α and β of the Great Bear.
	9	40	0	The Ray seemed to move gradually under ξ of the Great Bear.
	9	44	0	No more Rays appeared. But whether there were any Remains of Light near the Horizon, or in the West,

I could

I could not ſee, upon account of the neighbouring Houſes.

Octob. 11, in *London.*

	h	*′*	
At	10	37	*p.m.* There were two bright Rays under ζ and ε of the Great Bear.
	10	39	A Ray between ε of the Great Bear and the Polar Star.
	10	39 ½	A Ray in Form of a Pyramid above η of the Great Bear.
	10	43	Theſe Rays had not any Motion parallel to the Horizon; but they entirely diſappeared.

Jan. 11, 173$\frac{5}{6}$, in *London.*

At	10	0	*p.m.* An indifferently bright Arch, pale towards the Edges, appeared 16 Degrees high: One of the Ends of which deſcended Eaſtward under η of the Great Bear: And lucid Streaks appeared now and then over this Arch.
	11	13	Under this Arch was another very bright Track parallel to it, 5° above the Horizon; in which Track there were Rays, that ſhot from W towards the E.
	11	15	This Arch was very faint.
	11	17	The firſt Arch became brighter, and the lower Arch was almoſt blended with the upper, and broken in the middle.
	11	20	The whole Arch was beſet with faint Rays.
	11	21	One of the Rays under the Polar Star.
	11	27	No Arch, nor Rays, but bright Tracks diſperſed here and there.
	11	36	The Light reached up to the Polar Star, and ſomewhat higher.
	11	44	A lucid Ray under the Polar Star.
	12	11	The Sky was overcaſt with Clouds, except one lucid Streak, which appeared three or four Degrees to the Eaſt of the North.

February 16, in *Clare-Hall, Cambridge.*

At a Quarter paſt eight in the Evening, the Moon ſhining very bright, there appeared two perpendicular Streams betweeen the great and little Bear.

April 3, in *London.*

At 8 46 I obſerved a lucid Arch one Degree broad, which extended along the Northern Crown, the *Cingulum Bootis*,

tis, the *Coma Berenices*, the leſſer Lion and Cancer, as far as the ſmaller Dog.

h ′ ″ At 8 49 36	This Arch quite diſappeared: But at the ſame Time I ſaw another broader and brighter Arch under *Caſſiopea*, ſeven or eight Degrees high.

In the Obſervations of *Oct.* 4, and in the laſt, I am certain as to the Time of the Clock: So that if it has happened that others have obſerved the ſame *Phænomena*, the Longitudes of Places may be determined by them with greater Exactneſs than by the Satellites of *Jupiter*, which I take to be the principal Uſe that may be made of theſe Obſervations, eſpecially in making Maps of the Northern Countries, where theſe Lights more frequently occur.

Auroræ Boreales *obſerved at* Wittemberg *in* 1733. *by* J. Fred. Weidler, *Prof. Math. &c.* Ibid. p. 238.

L. *May* 3, 1733, an *Aurora Borealis* is ſaid to have appeared.

June 26, an *Aurora Borealis* appeared in the Evening, and darted out the uſual Pyramids about Midnight.

Oct. 27, about 7^h in the Evening a dark Arch appeared in the N, covered with a white Border. At 8^h $30'$ the white Border was expanded higher to the Height of about 40°, and this lucid Region remained ſtill to paſt 10. After 10 pyramidal Eradiations darted with great Quickneſs from the white Track, and ſome waving Clouds moved along the Sky, and the whole white Border was ſeen to riſe toward the Pole, like a lucid Wave. This Undulation of ſhining Matter, with ſome pyramidal Rays mixt here and there, much more bright than that lucid Region, laſted till 11, the whole lucid Matter was gradually moved toward the E, and in it's Place ſome black, ſcattered Clouds ſucceeded toward the W. A little before 11, near the N W, there remained a white Cloud, which ſometimes grew red, and the Matter of it alſo waved. The Appearance ceaſed after 11: The Air was calm during the Appearance, and the Stars ſhone through the white Region. I thought it remarkable, that the Sky was dark and rainy the next Day, and that in the Night after the 29th, the Wind was very impetuous and ſtormy, whereas calm Weather commonly follows remarkable *Auroræ Boreales*.

The ſame *Phænomenon* was obſerved alſo at *Stockholm*.

——*In* 1734, *by the ſame.* No. 442. p. 266. July, &c. 1736.

LI. *Jan.* 23, 1734, a very bright *Aurora Borealis* appeared at 7^h $6'$, *p. m.* Under the N. was ſeen a dark Arch, with a double white Border over it, ſeparated by a dark Region in the middle. The laſt white Arch aroſe to the Height of 25°. In the dark Part ſome lucid Pyramids now and then ſhot forth; and a ſimilar white Arch was produced by the Light reflected near the Zenith. At 7^h $30'$ the white Track was diffuſed further to the W, and in the middle reached to the Height of 50°. At 7^h $35'$ the bright Region was drawn up quite to the *Vertex*; ſome intermitting Clouds tended toward the W, which very ſeldom

happens

happens in an *Aurora Borealis.* I wondered alſo at ſome Clouds that hung about the N E, and moved before the lucid Region, the Light of the *Aurora* being diſtinctly ſeen beyond them; whence it appears, that this Light is far above the Clouds. At $7^h\ 38'$ the lucid Region of the *Aurora* ſunk below the Horizon, eſpecially toward the W; and it is worthy of Obſervation, that tho' this *Aurora* was very bright, it ſhewed but very few lucid Pyramids. At $8^h\ 30'$ almoſt the whole Light was hid below the Horizon, but appeared near the W. to the Height of 10°. A Track of the dark Arch under the Pole, with a ſmall white Border over it, was yet viſible at 10^h.

March 19, $6^h\ 45'$, *p. m.* about the N N W there appeared under the Pole a dark Arch, covered with a broad white Border; the Light of the white Border was brighter toward the W, than toward the E, but was not followed by lucid Pyramids or Waves, ſuch as uſe to accompany the *Aurora Borealis:* Therefore there are ſome *Auroræ Boreales*, which diſcover only a white Region in the North, but without any Motion of Light.

March 29, there was a remarkable *Aurora Borealis*, which I did not ſee till 9. The Moon was then near ſetting, and ſhone only through Clouds: The whole Sky was almoſt covered with Clouds, and yet about the Pole there was a bright Region behind the ſcattered Clouds, from which ſome lucid Pyramids now and then proceeded, a ſure Mark of an *Aurora Borealis.* I perceived alſo behind the Clouds a lucid Track of an *Aurora*, that was formed into an Arch. The lucid Pyramids continued till 11; a rare Spectacle in a cloudy Sky, agitated by Winds; for the *Aurora Borealis* is ſeldom ſeen but in very calm, ſtill Weather.

Aug. 9, at 11^h ſome thin Clouds about the N N E ſeemed to burn. But a few Radiations being emitted toward the *Vertex*, the Inflammation ceaſed, and the little Clouds were driven toward the Eaſt by a gentle Wind.

From theſe and other Obſervations we may plainly perceive how variable a *Phænomenon* is the Light of the *Aurora Borealis.*

An Aurora Borealis *obſerved at* Peterborough, Dec. 11, 1735, *by Mr* Timothy Neve. No. 445. p. 53. Jan. &c. 1737.

LII. *Dec.* 11, 1735, a little after five o'Clock, I obſerved the Northern Hemiſphere to be obſcured by a duſky red Vapour, in which, by Degrees, appeared ſeveral very ſmall black Clouds near the Horizon. I thought it ſeemed to be a Preparation for thoſe Lights which afterwards were ſeen; the firſt Eruption of which was within a Quarter of an Hour, full E, from behind one of the ſmall dark Clouds, and ſoon after ſeveral others full N. Theſe Streams of Light were of the ſame duſky red Colour as the Vapour, juſt appeared, and vaniſhed inſtantly. I ſaw 8 or 10 of theſe at once, about the Breadth of the Rainbow, of different Heights, ſeveral Degrees above the Horizon, and looked like ſo many red Pillars in the Air; and no ſooner did they diſappear, but others ſhewed themſelves in different Places. In about half an Hour, this Colour of the Vapour gradually changed itſelf towards the uſual

White,

White, and spread itself much wider and higher; and after that, appeared as common.

—At Edinburgh, *by Mr* James Short, *of the College at* Edinburgh. No. 456. p. 368. Jan. &c. 1740.

LIII. I came here on *Sat.* last: That Evening, about 6, there was one of the most remarkable *Auroræ Boreales* that ever I saw. At first there appeared the ordinary luminous Arch, the Vertex of which was about 30° above the Horizon, and had it's Centre somewhere in the Meridian Circle. After this was perfectly well formed, there appeared little or none of the purple and red Colours which are usually in that Arch; but immediately there broke out, from the most Western Extremity, a great deal of that Northern Light which formed this Arch, and, rushing along with Rays directed to the *Zenith*, formed another *Aurora Borealis* above the first, the Centre of which was to the East of the Meridian: After this was formed, there followed from the same Extremity a great deal of purple and red-coloured Light, quivering and shaking towards the *Zenith*, with a flapping Noise in rushing along, till it formed a third *Aurora Borealis*, above the second, the Centre of which was somewhere on the East-side of the Meridian. When I was pleasing myself with this remarkable *Phænomenon*, looking again to the Western Source of these Arches, I perceived, as it were, a huge Pillar of a dull red coloured Light, rising out of the same Place whence the Arches took their Beginning, extending itself in a Direction towards the *Zenith*, till it rose almost 60° high. These Arches and Pillar lasted very near an Hour; the two uppermost Arches were continually quivering and shaking, and the Pillar always turning to a paler Red.

I forgot to tell you, that the Night before the *Aurora Borealis*, there was a prodigious Hurricane of Wind, which lasted till the *Saturday* Morning; but all that Day it continued to blow, though not so hard. The Arch from whence the Wind blew, was from the N W, the same Quarter from whence the Arches took their Rise. To this Day, ever since the Hurrican of Wind, there has been a most intense Frost: It froze so hard, that in less than 24 Hours after it began, the Lake on the North-side of this City was so strong as to bear People on it. Just now the Wind has changed, so that we expect a Thaw.

An Aurora Australis, *seen* March 18, 1738-9, *at* Chelsey, *near* London, *by* John Martyn, *F. R. S.* Prof. Botan. Cantab. No. 461. p. 841. Aug. &c. 1741.

LIV. 1. *March* 18, 1738-9, at $8^h\ 30'$, being informed, that there was a great Fire towards *London*, I made Haste towards an upper Window that looked to the N N E: I found an extraordinary Redness in the Air, but of too determined a Figure to arise from the burning of a House: A broad red Band extended itself to the Northward of the E; in the middle of which I very plainly saw *Arcturus*, then about 25° high; and it's Northern Edge touched *Cor Caroli*. It seemed to be fixed and permanent; not radiating, or fading, as in a common *Aurora Borealis*. This red Band, or Arch, was bounded on the N. by Streams of a greenish Blue, in the same Direction; the most Northern Edge of of which touched the Star marked η in the Tip of the Tail of *Ursa Major*. After I had considered this Phænomenon for some little Time, I retired into my Garden, where I saw a great Brightness almost in the Zenith,