THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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* Errata—Page 16, line 13, *after* the points A and B, *add* fig. 16.

† ——331, —— 11, *for* fig. 2, *read* 9.

‡ ——394, —— 23, *after* &c. *add* fig. 13.

THE

PHILOSOPHICAL TRANSACTIONS

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ABRIDGED.

The Newtonian Solution of Kepler's Problem, of finding the True Motion of the Planets, describing Areas proportional to the Times, in Elliptic Orbits, about one of the Foci; Demonstrated and Illustrated with Examples. By Mr. John Keill, Savil. Profes. of Astr. Oxford, and F. R. S. N° 337, p. 1, *Art.* 1, *Vol. XXVIII. Translated from the Latin.*

KEPLER was the first who demonstrated that the planets do not revolve in circular orbits, but in elliptical ones; and that they go round the sun placed in one of the foci of the ellipsis, in such a manner, that a radius extended from the planet to the sun's centre, always describes elliptical areas, which are protional to the times of description. This divine discovery of the sagacious Kepler, was owing to the accurate observations of Tycho Brahe; and is so much the more to be esteemed, that by help of it Newton has perfectly explained the laws of motion, and the philosophy of the system of the universe. Since therefore the planets revolve about the sun by such a law, that their places in their own orbits may be determined to any given time, it is necessary that the following problem should be solved, viz.

To find the Position of a Right Line, which passing through either focus of an ellipse, may cut off an area described by its Motion, which may be to the whole area of the ellipse, in a given ratio.—Let the ellipse be APB, fig. 1, pl. 1, a focus of of which is S: there is to be found the position of the right line SP, which may cut off the trilinear area ASP, to which the area of the whole ellipse has the same ratio, as the periodic time of the planet describing the ellipse, has to any other given time: which being found, the point P will be given, where the planet will be found at that given time. Or, let AQB be a semicircle described on the greater axis of the ellipse; a line SQ is to be drawn through S, cutting off the area ASQ, to which the area of the whole circle is in the same ratio: for if from Q the perpendicular QH be drawn, meeting the ellipse in P, drawing SP, it will give the elliptic area required, and the point P will be the place of the planet at the given time. For the elliptic semisegment APH is to the circular semisegment AQH, as HP to HQ, that is, as the area of the whole ellipse is to the area of the whole circle: but the triangle SPH is to the triangle SQH also in the same ratio of PH to QH: therefore the area ASP is to the area of the whole ellipse, as the area ASQ is to the area of the whole circle. So that if we had a method of cutting the area of the circle in a given ratio, by a line drawn through the given point S, it would be easy to cut the elliptic area in the same ratio.

Kepler himself, who first proposed the problem, had no direct method of computing the planets places, from the time being given: but he was obliged to proceed through the several degrees of the semicircle AQB, from the given arc AQ, called the excentric anomaly, and both to calculate the time by the area ASQ, which is proportional to the mean anomaly, and the angle ASP, that is the planet's place, or the coequate anomaly corresponding to this time.

Since then the solution of this problem was difficult, astronomers had recourse to other hypotheses, assuming some point for that about which the motion is equable, or proportional to the time, and thence the mean anomaly being given, they determined the coequate anomaly. But computations founded on these hypotheses were found not to agree with the observations. Therefore geometers had recourse to various approximations, by which, from the given area ASQ, which is analogous to the time, the angle ASP, or the place of the planet, may be had very nearly. Now the easiest of all these, and most ready for practice, seems to be that method which is taught by Mr. Newton in his Principia, p. 111 and 112, of the first edition, which is very much like that method, by which analysts extract the roots of affected equations; and indeed is so much the more to be esteemed, as that it not only exhibits the places of the planets, whose orbits approach very nearly to the form of circles, but almost with the same facility may be applied to comets, which move in orbits that are very excentric. Therefore I thought it not amiss to explain that method here, for the sake of such artists as are desirous of constructing astronomical tables, according to the true laws of motion, and not by any fictitious hypotheses.

Therefore let AQB, fig. 2, be a semicircle described on the greater axis of an ellipsis, whose centre is C, and S the focus in which the sun is placed. Let CQ be drawn, on which, produced if necessary, let fall the perpendicular SF. The area ASQ is equal to the sector ACQ, added to the triangle CSQ $= \frac{1}{2}$CQ $\times$ AQ $+$ $\frac{1}{2}$CQ $\times$ SF; and therefore, because of $\frac{1}{2}$CQ being given, the area ASQ will always be proportional to the arch AQ added to the right line SF, when the motion is from the aphelion towards the perihelion. But when the planet tends from the perihelion towards the aphelion, as in fig. 4, the area BSQ = sector BCQ — triangle CSQ, and therefore it will be proportional to the arch BQ — right line SF. Hence if there be taken the arch AN, in fig. 2, 3, and BN in fig. 4, proportional to the times, it will be AQ + SF = AN, and BQ — SF = BN. Whence SF will be equal to QN, if AN or BN are proportional to the times in which the areas ASQ or BSQ are described. Now that the measure of the arcs in the periphery AQB, which arc is equal to the right line SF, may be found in degrees and parts of a degree: let it be made, as CQ to CS, so is the arch of 57.29578 degrees, (which is equal to the radius CQ) to a fourth arc, which will be equal to CS. Let that arc be B. But it is CS to SF, so is radius to the sine of the angle SCF or ACQ. Therefore let it be made, as radius to the sine of the angle ACQ, or the arc AQ, so is the arc B to another D; that arc D will be equal to the right line SF; therefore if, at a given time, the area ASQ were proportional to the time, the arc D would be equal to NQ; and taking the arc NP = D, the point P would fall on Q. But if the area ASQ should not exactly answer to the time, the point P will fall above or below Q, according as the area ASQ is greater or less than the true area which answers to the time. Let it be ASq, and on Cq let fall the perpendicular SH; then, by what has been demonstrated, it will be SH = Nq. But it is SF = NP, whence it will be SH — SF or SF — SH, that is nearly HE = qP = QP — Qq or Qq — QP. And if the angle QCq be small, it will be CH : CQ :: HE : Qq :: QP — Qq : Qq; whence CQ + CH : CQ :: QP : Qq, when the arc AQ is less than a quadrant. But when it is greater than a quadrant, it will be CQ — CH : CQ :: QP : Qq. And in like manner, when the arc BQ is less than a quadrant, it will be CQ — CH : CQ :: QP : Qq.

When the angle ACQ or BCQ is small, that is, when the planet is near the apses, it will be CA $\pm$ CS : CA :: QP : Qq.

Make as CS to CQ so radius R to a certain length L, then will CQ $= \frac{CS \times L}{R}$. But radius is to the cosine of the angle ACQ, as SC : CF or CH (for CH and CF are nearly equal); therefore CH $= \frac{SC \times \cos.ACQ}{R}$, and therefore QP : Q$q$ :: $\frac{CS \times L + CS \times \cos : ACQ}{R} : \frac{CS \times L}{R}$ :: L + $\cos$.ACQ : L, when the arc AQ is less than a quadrant. But if AQ be greater than a quadrant, then will QP : Qq :: L — $\cos$.ACQ : L.

And in this manner, if the arc AQ be any how taken, which is somewhat less or greater than the truth, there will thence be found an arc Qq, to be added to it, or taken from it, which will make the area ASq very nearly proportional to the time. And if, instead of AQ, there be taken an arc Aq, and a process like the former be carried on, there will be found another Aq; which in like manner, by repeating the same process, will give another Aq; and thus we may approach as near as we please to the truth.

The angle ACq being found, we shall easily have the angle ASq, since in the triangle qCS are given the sides Cq and CS, and the angle qCS. Thence will be given the angle CSq, whose tangent is to be lessened in the ratio of the less axis of the ellipse to the greater, that at length may be had the tangent of the angle ASP. Or perhaps the angle ASP may be found more easily thus: let F be the number expressing the length CS in such parts as CQ is 100000: from the point q draw qr perpendicular to the axis, which will be the sine of the arc Aq, and Cr will be the cosine of the same, and Sr will be equal to the sum or difference of the right lines Cr, CS, that is, Sr = F $\pm$ *cos.*ACq: therefore in the right angled triangle rSq, Sr and rq being given, there may be found the angle rSq. Hence, if there be added into one sum the log. sine of the angle ACq, and the arith. complement of the log. of Sr, and the log. of the ratio of the less axis of the ellipse to the greater, there will be obtained the tangent of the angle ASP.

But the facility of this method is such, that it requires rather to be illustrated by examples, than any further explained. Therefore we may try it in the motion of the planet Mars, in whose orbit, according to the Caroline tables, the excentricity is to the mean distance, as 14100 to 152369, and therefore the log. of the arc B, which is equal to the right line SC, will be 0.7244451. Also in this example L will be 1080631 of such parts as the radius is 100000: find the angle ACQ, where the mean motion, or the arc proportional to the time computed from the aphelion, is 1 degree. Because CS is here nearly one tenth part of CA, I suppose the arc AQ to be 0.9 degrees, that is, one tenth part less than the mean motion. Let there be added the log. sine of the arc AQ to the log. of B, and the sum 8.9205471 is equal to the log. of the number 0.083281, which number expresses an arc equal to the right line SF = NP. And if the arc AQ had been rightly assumed, it would be AN − NP = AQ, and QP = 0. But here it is QP = 0.016719, from whence if we take away its 11th part, since AS exceeds AC by about the 11th part of itself, there will remain Qq = 0.0152; which being added to AQ, gives AQ = 0.9152, which does not differ from the true Aq by a thousandth part of a degree. Secondly, let the arc AN or the mean motion be 2 degrees. I make AQ = 1.83, almost double the former Aq, and to its log. sine let be added the log. of B. The sum will be 9.2286997, which is equal to the log. of the number 0.16931. Whence it will be QP = 0.00063, and Aq = 1.83063, which does not differ from the true Aq by the ten thousandth part of a degree. After the same manner let the motion, or the arc proportional to the time, be 3 degrees. Make the arc AQ 2.745 = 1.83 + 0.915, and to its log. sine adding the log. of B, there will be had the log. of the number 0.25392 = NP, and AN — NP = 2.74608, and therefore QP = 0.00108. Whence Qq = 0.001 nearly, and Aq = 2.746. Thus, by one addition of two logs, the arc Aq will be found which will be true to the thousandth part of a degree.

Now if the angle ACq is to be found, not by proceeding gradually, but per saltum, when the mean motion is 45 degrees: I make the arc AQ to be 40 degrees, and to its log. sine adding the log. of B, the sum is 0.5325125, which is the log. of the number 3.4081. This number subtracted from 45, leaves AN — NP = 41.5919, whose excess above the arc AQ is 1.5919. Whence if it be made, as L + cosin. ACQ to L, so is 1.5919 to another, the arc Qq will be found to be 1.4865 degrees. Therefore Aq = 41.4865, which differs from the truth not much above the thousandth part of a degree. But without this proportion Aq may be found, by taking a new arc AQ, which is a little less than AN — NP, yet nearly equal to it. For instance, make AQ = 41.50, and adding the given log. of B to its log. sine, there will be had another NP = 3.35131, which subtracted from AN, gives 41.4869 for a new Aq. And this arc is derived with less trouble, and comes nearer the truth than the former Aq.

After Aq is found, corresponding to the mean motion 45 degrees, proceeding again by steps, by one addition of two logs. will be had Aq to all the subsequent degrees of the mean motion. For instance, when the mean motion is 46 degrees, I make AQ = 42.4249; and adding its log. sine to the constant log. of B, it will be AN — NP = 42.4249; to which arc if a new AQ be put equal, there will be had Aq, which will not differ from the true Aq by the thousandth part of a degree. So when the mean motion is 47 degrees, I make AQ = 43.36, equal to the former Aq added to the increment of that arc for one degree of mean motion, and adding its log. sine to the log. of B, the sum will be the log. of the number 3.6402, which subtracted from AN, leaves AN — NP = 43.3593, equal to the new Aq, which differs from the true Aq about the ten thousandth part of a degree.

If, omitting the intermediate degrees, the arc Aq is to be found when the mean motion is 100 degrees; make AQ 96°, and adding its log. sine to the log. of B, the sum will be equal to the log. of the number 5.273, whence AN — NP = 94.727. Therefore, secondly, make AQ = 94.72, and adding its sine to log. B, there will arise the log. of 5.285, which subtracted from AN leaves AN — NP = 94.715 = Aq very nearly. In like manner, if the mean motion be 101°, make AQ = 95.71, whose log. sine added to the log. of B, gives the log. of the number 5.2756, which number taken from 101, there will remain

AN — NP = 95.7244 = Aq. And in this manner, the mean motion being given, by a gradual process the angle at the centre will be had, by the addition only of two logs. one of which, being constant, may be preserved on the paper, to spare the labour of writing it down too often.

Now let us proceed to an orbit of the other species, such as the distance of the aphelion may be to the distance of the perihelion, as 70 to 1. Such nearly was the orbit of that comet, which completes its period in 75½ years; as was first found by that sagacious astronomer and geometrician Dr. Edmund Halley. In this orbit AC or CQ, will be 35.5 and CS 34.4 of such parts as SB is one. And the arc Bq is to be found, when the mean motion is one 100th part of a degree. Since the middle distance exceeds the least distance about 35 times, I make BQ = 0.35, when the mean motion is 0.01. In this orbit the constant log. of B is found 1.7457133. Therefore this log. being added to the log. sine of the arc 0.35, gives the log. of the number 0.34013, which added to the arc 0.01, will make 0.35013. If this sum had been equal to 0.35, the arc BQ would have been rightly assumed; but the difference is 0.00013. Whence because CB is to SB as 35.5 to 1, let the difference 0.00013 be multiplied by 35.5, and there will arise Qq = 0.004615; whence it will be arc Bq = 0.354615, which hardly differs from the truth by 3 parts of ten thousand.

Secondly, let the mean motion be 0.02, and suppose BQ to be 0.71. To its log. sine adding the log. of B, the sum will be the log. of the number 0.68998; whence BN + NP = 0.70998, and therefore the assumed arc BQ = 0.71 was too much, and the difference is 0.00002. Which if it be multiplied by 35.5, and the product subtracted from BQ, there will remain Bq = 0.7092, deviating from the truth hardly the ten thousandth part of a degree.

Let the mean motion be 0.03. Suppose BQ to be 1.06 degrees, adding its log. sine to the log. of B, the sum will be the log. of the number 1.03008. To which if BN = 0.03 be added, the sum will be 1.06008, which number is greater than BQ; wherefore if the difference 0.00008 be multiplied by 35.5, and added to BQ, it will be Bq = 1.06284. In like manner, when the mean motion is 0.04, I suppose BQ = 1.40 degrees, and find NP = 1.3604; to which number adding BN = 0.04, the sum is 1.4004, which exceeds 1.40 by 0.004. Let this difference be multiplied by 35.5, and the product 0.01420 will be equal to Qq; whence Bq = 1.41420. In all these instances the errors are very small, and seldom go beyond the thousandth part of a degree.

Now let the arc Bq be to be found, when the mean motion is equal to one degree. Suppose BQ = 20°, and adding its log. sine to the log. of B, there will be had the log. of the number 19.045; to which adding BN = 1°, the sum 20.045 exceeds 20 by 0.045. And since in this case L — cosin. BQ is to L, as

1 to 11.5 nearly, I multiply the difference 0.045 by 11.5, and the product 0.5175 added to BQ, makes 20.5175. Therefore, I suppose secondly BQ = 20.51, and there will arise, in the same manner as in the foregoing, NP = 19.5092; to which adding BN, the sum is 20.5092, which is less than BQ. Wherefore if the difference 0.0008 be multiplied by 11.5, and the product 0.0092 be subtracted from BQ, there will remain Bq = 20.5008.

Lastly, let the mean motion be equal to 2°. I suppose BQ 30°, and there is found NP = 27.84; to which adding 2°, the sum 29.84 is less than 30. And if the difference 0.16 be multiplied by 6.3 (for L — cosin. BQ is to L, as 1 to 6.3 nearly) it will be 1.008 = Qq. Therefore this arc subtracted from BQ gives Bq = 28.982. Now that Bq may be corrected, I assume secondly BQ = 29 degrees; and by a like process we find Bq = 28.9672.

Of finding the Centre of Oscillation. By Brook Taylor, Esq. F. R. S. N° 337, *Art.* 2, p. 11. *Translated from the Latin.*

Definition. The centre of oscillation is a certain point in a pendulous body, whose single vibrations are performed after the same manner, and in the same time, as if that point only was suspended by a thread at the same distance from the point of suspension.

Of itself it is hardly sufficiently clear that there is such a point in a body, as that its acceleration ought, by this definition, to be the same in all inclinations of the pendulous body to the horizon, as if it were actuated by its own gravity; the other particles of the whole body giving no disturbance to its motion. Therefore, in order to the investigation of this centre, a proposition or two must be premised, whence it may appear that there is such a point.

Prop. 1. *Prob.* 1. In any given inclination, of a vibrating body, to the horizon, to find a point, whose acceleration shall be the same, as if it were urged by its own gravity only.

Let ABD (fig. 5, pl. 1) be a section of the proposed body in a plane perpendicular to the horizon, in which the centre of gravity G is moved, C being the centre of suspension. Let the body be distinguished into prismatical elements perpendicular to the plane ABD, and therefore always parallel to the horizon; as will easily appear from the motion of the centre of gravity G in that plane ABD. And because of that situation, any such element may be considered as a physical point p placed in the same plane ABD at the point z. Therefore let the body proposed be reduced to the physical plane ABD, consisting of such particles p.

To find in this plane a point O, whose proper acceleration is not changed by the action of the other particles, we must attend to the force of every single particle p, situated in the point z: for from these forces jointly, arises the absolute motion of the whole plane. By means of this, is given the motion of every point proposed; whence in its turn is found that point whose motion is given.

But the particle p will be urged by the force of its own gravity, which, if the cohesion of the particles were dissolved, in a given very small time, would produce a given acceleration of motion, in the perpendicular to the horizon zy. Draw xy perpendicular to EZ, and the acceleration zy will be resolved into the parts zx and xy. Because of the rigidity of the body, the force zx will be taken away by the resistance of the point C. But by the remaining force xy the space ABD is turned about the point C. And drawing the horizontal line Co, and a perpendicular zs, it will be as $\frac{\text{C}s}{\text{C}z}$: viz. because of the given force of gravity, and the similar triangles xyz and sCz. Therefore the force of the particle p, to move the space ABD, will be as $\frac{\text{C}s}{\text{C}z} \times p$.

To collect these forces together, let O be an invariable point, in a line drawn at pleasure, and at a distance CO as yet unknown. Then the force of the particle p to move the point O, will be as $\frac{\text{C}z}{\text{CO}} \times \frac{\text{C}s}{\text{C}z} \times p$, that is, as $\frac{\text{C}s}{\text{CO}} \times p$. And the acceleration which p contributes to the same point O, will be as $\frac{\text{CO}}{\text{C}z} \times \frac{\text{C}s}{\text{C}z}$. Therefore the force $\frac{\text{C}s}{\text{CO}} \times p$ being applied to the acceleration $\frac{\text{CO} \times \text{C}s}{\text{C}z^2}$, the quotient will be $\frac{\text{C}z^2}{\text{CO}^2} \times p$, which, if it be supposed to move in the point O with the same acceleration $\frac{\text{CO} \times \text{C}s}{\text{C}z^2}$, would produce just the same motion, as the particle p produces in the same point O. Thus finally the problem is reduced to a well-known theorem of motion: for the sum of the forces $\frac{\text{C}s}{\text{CO}} \times p$ being applied to the sum of the particles $\frac{\text{C}z^2}{\text{CO}^2} \times p$, the quotient will give the absolute acceleration of the point O. Then drawing the perpendicular Oo, and supposing this acceleration to be equal to the given acceleration $\frac{\text{C}o}{\text{CO}}$ of the point O, the distance CO will be given. For let $\frac{\text{C}o}{\text{CO}} = d$, and by the method of fluxions it is $\text{C}s \times p = \dot{\text{M}}$, and $\text{C}z^2 \times p = \dot{\text{C}}$. Then because of CO being variable, the sum of all the forces will be $\frac{\text{C}s}{\text{CO}} \times p = \frac{\text{M}}{\text{CO}}$, and the sum of all the particles $\frac{\text{C}z^2}{\text{CO}^2} \times p = \frac{\text{C}}{\text{CO}^2}$. Hence, applying the sum of the moments to the sum of the bodies, it will be $\frac{\text{M}}{\text{C}} \times \text{CO}d$, and therefore $\text{CO} = \frac{d\text{C}}{\text{M}}$. Therefore, C and M being found, CO will be given by the inverse method of fluxions. Q. E. I.

Corol. From the centre of gravity G draw Gg perpendicular to the horizontal line CO, and put the body ABC = A. Then, from the known property of the centre of gravity, it will be $M = cg \times A$. Hence $CO = \frac{dc}{cg \times A}$.

Prop. 2. *Theor.* 1. The same things being supposed, let the point O be found in the right line CG passing through the centre of gravity G (fig. 6). Then will the point O be the centre of oscillation of the body A.

For in this case it is $\frac{co}{CO} = \frac{cg}{CG} = d$; hence $CO = (\frac{dc}{cg \times A}$ by Cor. of prop. 1 $=)$ $\frac{C}{CG \times A}$. But A is given, and the point C being given, CG and the quantity C are given. Hence CO is given, whatever be the inclination of the vibrating body to the horizon. Therefore, by the definition and problem 1, O is the centre of oscillation of the body A. Q.E.D.

Prop. 3. *Theor.* 2. The same things being supposed, let D be the aggregate of all the $GZ^2 \times p$. Then it will be $CO = CG + \frac{D}{CG \times A}$.

For to CG draw the perpendicular ZF, fig. 7, and it will be $CZ^2 = CG^2 + GZ^2 - 2CG \times GF$, when F falls between C and G. But when F falls in CG produced, it will be $CZ^2 = CG^2 + GZ^2 + 2CG \times Gf$. Therefore C = (aggregate of all the $CZ^2 \times p =$) aggregate of all the $CG^2 \times p + GZ^2 \times p - 2CG \times GF \times p + 2CG \times Gf \times p$. But, G being the centre of gravity, the aggregate of all the $2CG \times GF \times p =$ the aggregate of all the $2CG \times Gf \times p$. Therefore C = the aggregate of all the $CG^2 \times p + GZ^2 \times p = CG^2 \times (A + D)$. But, by theor. 1, it is $CO = \frac{C}{CG \times A}$. Therefore $CO = CG + \frac{D}{CG \times A}$. Q.E.D.

Corol. Hence the parallelogram $CG \times GO$ is given. For $GO = \frac{D}{CG \times A}$. But A and D are given. Therefore $CG \times GO = \frac{D}{A}$ is given.

Prop. 4. *Theor.* 3. The same things being supposed, and having constituted the physical particle $\frac{CG \times A}{CO}$, which being actuated by its own gravity shall vibrate about the point C; the motion of the space ABC shall be just the same, as if it were actuated by the oscillation of the body A.

This appears both from the nature of the centre of gravity, and by prob. 1. For $\frac{CG \times A}{CO}$ is the aggregate of all the $\frac{CZ^2 \times p}{CO^2} = \frac{C}{CO^2}$.

Prop. 5. *Prob.* 2. Having given the magnitude of any body A, with the centre of gravity G, and the point of suspension C : to find its centre of oscillation O.

This is done either by theor. 1, by finding the quantity C; or by theor. 2, by seeking the quantity D.

Scholium. For performing the calculation in a particular case, the quantity C or D is to be chosen, according as may be suggested by the nature of the proposed figure. Then either of these being given, the other will be also given by the equation C $= CG^2 \times (A + D)$ in prop. 3. Hence also will be given the parallelogram CG $\times$ GO $= \frac{D}{A}$ (corol. prop. 3) $= \frac{C}{A} - CG^2$. By help of which, from the centre of gravity and the point of suspension being given, the centre of oscillation is given by division only. Therefore, in any example, it will always be most convenient to find this parallelogram first, by computing either the quantity D, or C, by a proper assumption of the centre of suspension.

It remains that we now illustrate this by some examples.

Example 1. Let the figure proposed be the pyramid ADC, fig. 8, whose base is the parallelogram AD; and let the motion of its centre of gravity be in the plane passing through the vertex C, and the diameter EF of the base parallel to the side AB.

To perform the calculation most conveniently, let the vertex itself C be the point of suspension. Then, after the manner of prob. 1, let the figure be reduced to the physical plane of the isosceles triangle CEF, fig. 9, in which *ef*, parallel to EF, represents a physical line composed of particles *p*. Put CH $= a$, HF $= b$, and $ch = x$. Then, from the nature of the figure it will be $eh = \frac{bx}{a}$, and the particle *p*, situated at the point z, will be as x. Or rather, making $hz = v$, then $\dot{v}\dot{x}$ will be the base of the elementary prism, and p will be as $\dot{v}\dot{x}x$. Hence it will be $\dot{c} = cz^2 \times \dot{v}\dot{x}x = \dot{v}\dot{x}x^3 + \dot{v}\dot{x}v^2x$. Therefore the sum of all the $cz^2 \times p$ in the line hz, will be $v\dot{x}x^3 + \frac{1}{3}\dot{x}xv^3$; and in the line *ef* (putting $\frac{bx}{a}$ for v) that sum will be $\frac{6ba^2+2b^3}{3a^3} \times \dot{x}x^4$. Hence again taking the fluent, and writing a for x, it will be C $= (6ba^2 + 2b^3) \times \frac{1}{15}a^2$. But the pyramid itself is A $= \frac{2}{3}ba^2$, and the distance of the centre of gravity G from the vertex C, is CG $= \frac{3}{4}a$. Hence

$\frac{C}{A} - CG^2 = \frac{D}{A} = CG \times GO = \frac{3a^2 + 16b^2}{80}$.

Exam. 2. Let the figure proposed be a right cone, described by the rotation of the isosceles triangle ECF about the perpendicular CH.

Here again taking the vertex C for the centre of suspension, and making CH $= a$, HE $= b$, $ch = x$, $hz = v$, as above, it will be

$p = 2\dot{v}\dot{x}\sqrt{(\frac{bb}{aa}x^2 - v^2)}$; hence $\dot{c} = 2\dot{v}\dot{x}\sqrt{(\frac{bb}{aa}x^2 - v^2)} \times (x^2 + v^2)$. Let B be the segment of the circle described on the diameter *ef*, which is adjacent to the absciss $hz = v$, and ordinate $\sqrt{(\frac{bb}{aa}x^2 - v^2)}$: then the sum of all the $cz^2 \times p$, in the right line hz, will be $\frac{4a^2+b^2}{2a^2}x^2\dot{x}$B $- \frac{1}{2}v\dot{x}(\frac{bb}{aa}x^2 - v^2)^{\frac{3}{2}}$. And when $v = eh$, this sum will be $\frac{4a^2+b^2}{2a^2}x^2\dot{x}$B; whose double $\frac{4a^2+b^2}{a^2}x^2\dot{x}$B is a part of C in the right line *ef*. But the area B is as x^2; put therefore B $= cx^2$; and then that part of C will be $\frac{4a^2+b^2}{a^2} \times cx^4\dot{x}$. Hence, taking the fluent, it will be $C = (4a^2 + b^2) \times \frac{1}{5}ca^3$. But the cone itself is $A = \frac{4}{3}ca^3$, and $CG = \frac{3}{4}a$: Therefore $\frac{C}{A} - CG^2 = \frac{D}{A} = \frac{3a^2 + 12b^2}{80}$.

And after the same manner the calculation proceeds in other figures, when the ratios of ch to he, and of hz to p are more compounded.

Exam. 3. To show the manner of calculating the quantity D, let the figure proposed be a parallelopipedon, whose face perpendicular to the horizon, and parallel to the plane of the motion of the centre of gravity, is ABD, fig. 10. Draw the diameters EF and HI, and let the altitude of the elements be p, and make tr parallel to HI; putting $GF = a$, $GH = b$, $Gs = x$, and $sz = v$. Then will $\dot{D} = v\dot{x}xx + \dot{x}vvv$. Hence the part of D in the right line tr will be $2b\dot{x}x^2 + 2b^3\dot{x}$; and again taking the double of the fluent, it will be $D = \frac{4}{3}(ba^3 + b^3a)$. But $A = 4ab$; hence $\frac{D}{A} = \frac{aa + bb}{3} = \frac{1}{12}DB^2$.

Exam. 4. Let the last example be in the sphere, a great circle of which is Btr, its diameter AB, and centre G, fig. 11. Then drawing the lines as appear in the scheme, it will be $\dot{D} = Gs^2 \times p + Gm^2 \times p$. But the sum of all the $Gs^2 \times p$ in the right line tr, is Gs^2 drawn into the area of the circle on the diameter tr. Also the sum of all the $Gm^2 \times p$ in the right line ki, is $Gm^2 \times$ area of the circle on the diameter ki. Hence it easily appears that D is = 4 times the fluent Gs^2 into the area of the circle whose diameter is tr. Let therefore c be the area of the circle whose radius squared is 1; and let $GA = a$, and $GS = x$. Then will $\dot{D} = 4\dot{x}x^2 \times (ca^2 - cx^2) = 4ca\dot{x}x^2 - 4c\dot{x}x^2$. Hence, taking the fluent, and making $x = a$, it will be $D = \frac{8}{15}ca^5$. But $A = \frac{4}{3}ca^3$. Hence $\frac{D}{A} = \frac{2}{5}a^2$.

Because of the affinity of the solution, I shall here add a problem on finding the centre of percussion.

Prop. 6. *Prob.* 3. To find the centre of percussion of any body, revolving about a given point; which centre must be such, that striking against an obstacle, and at the same time the body being disengaged from the centre of suspension, it shall not incline to either one side or another.

First it appears that the point must be found in the plane of motion of the centre of gravity. For if the body be resolved into prismatic elements perpendicular to that plane, they will be carried about by a parallel motion; hence the moments on each side of that plane will be equal. Therefore, by the resistence made in this plane, no point of the body will be driven out of it. Let that plane then be AB, fig. 12, to which let the body be reduced, by a contraction of the prismatic elements into particles p, situated at the points z, as in prob. 1. Let C be the centre of rotation in this plane, or at least its projection made by a line demitted perpendicularly on the plane; and let Q be the point sought. Through C draw $C\xi$ at pleasure, in which take two points z and ξ, so that drawing zQ and ξQ, the angle CzQ may be obtuse, and the angle $C\xi Q$ acute; and in the points z and ξ let there be the particles p and π. Then draw zr and ξr perpendicular to $C\xi$, which may be to each other as Cz to $C\xi$, by which will be represented the absolute velocities of the particles p and π. But certain parts of these, which are in the directions zQ and ξQ, are taken away by the resistance of the point Q. Draw CD and Cd perpendicular to Qz and $Q\xi$, then because of the equal angles $zCD = rzQ$, and $\xi Cd = r\xi Q$, the other parts of the velocities, in the directions perpendicular to Qz and $Q\xi$, will be as zD and ξd. Hence having the ratio of the distances Qz and $Q\xi$, the forces of the particles p and π, to move the space AB towards opposite sides, will be as $Dz \times zQ \times p$ and $d\xi \times \xi Q \times p$. And by the conditions of the problem the sums of these contrary forces ought to be equal.

Because of the right angles at D and d, the points D and d are in the circumference of a circle described on the diameter CQ. Let E be the centre of this circle. Then drawing Ez and $E\xi$, meeting the circle in F, I and f, i, it will be $Dz \times zQ = Fz \times zI = EF^2 - Ez^2 = EQ^2 - Ez^2$, and $d\xi \times \xi Q = E\xi^2 - EQ^2$. Therefore the sum of all the $EQ^2 \times p - Ez^2 \times p =$ the sum of all the $E\xi^2 \times \pi - EQ^2 \times \pi$; and transposing the terms, the sum of all the $EQ^2 \times (p+\pi) =$ the sum of all the $Ez^2 \times p + E\xi^2 \times \pi$; that is, if p be put as well for the particle p within the circle, as for the particle π without it, the sum of all the $EQ^2 \times p$ will be equal to the sum of all the $Ez^2 \times p$. Draw zs perpendicular to CQ. Then will $Ez^2 = Cz^2 + EC^2 - QC \times Cs$. Which value of Ez^2 being substituted for it, and the equation ordered, there is at length found the sum of all the $CQ \times Cs \times p =$ the sum of all the $Cz^2 \times p$. Hence $CQ = \frac{\text{sum of all the } Cz^2 \times p}{\text{sum of all the } Cs \times p}$. But the sum of the $Cz^2 \times p$ is the quantity C itself, in the calculation of the centre of oscillation: and if the centre of gravity be G, and Gg be drawn perpendicular to CQ, and the body itself be called A, then will the sum of all the $Cs \times p = Cg \times A$. Hence is $CQ = \frac{C}{Cg \times A}$. Let O be the centre of oscillation; then by theor. 1, $CO = \frac{C}{CG \times A}$. Hence it is $Cg : CG :: CO : CQ$. Therefore a perpendicular to CO drawn through O, will pass through the point Q. Q.E.I.

An Account of the Eruption of Mount Vesuvius, in 1707. *By S. Valetta.* N° 337, art. 3, p. 22. *Translated from the Latin.*

The eruptions of this mountain are so frequent and continual, that they are almost innumerable: so that there hardly passes a month, much less a year, without its breaking out with more or less violence, and doing more or less damage. The greatest eruption that had happened for some time, was that in 1707, when in the height of summer, in the latter end of July, this mountain, which had been quiet for some time, began to show some signs of an eruption; for, at first those internal bellowings were heard, which resounded in the very centre of the mountain, yet without any smoke or flame; then by degrees it began to emit smoke and clear fire, which, especially in the night-time, illuminated all Campania; in the mean time, at different intervals, it made such a terrible noise, that the reports of the largest guns are scarcely to be compared with it: then it began to roll its ashes for several days and nights, conveying them aloft every way into the air, and dispersing them into different qnarters, according as the wind happened to blow; sometimes into the sea, at other times on the adjacent territories of Stabia, Nola, and Acerra; and, what was very remarkable, a prodigious shower of stones, that destroyed both men and cattle. After this it began to belch out at its gaping mouth a liquid torrent of bitumen, called glarea, or grit; which at first appeared like a gentle stream of fire, descending with the same slowness of motion, as is observable in melted pitch, or the like viscid substances; this matter, which resembled molten glass, as it cooled in its progress, became as hard as stone; it was observable, that the superior surface of this matter, as it cooled, was converted into small spongy stones, but its lower surface into a broad, hard, solid, flint, long used in paving the highways, as if what lay next the air had admitted and retained some of its particles, while its lower part became a solid compact mass, without any vacuities. Among a great many phænomena of this volcano, there were two that had not been observed for several ages before: for, about the third or fourth day it began to emit at its mouth flashes of lightning, almost like those sometimes observed in the air, but of a serpentine form; and very loud claps of thunder were heard, so thick and frequent, that at first it was thought it would rain, till it was perceived they proceeded from the mountain, and that the dark clouds did not consist of vapours, but of large quantities of ashes.

On the 2d of August, at 4 in the afternoon, there was such a thick cloud of ashes hovering over Naples, as intercepted the rays of the sun; and the darkness was so great, that people could not distinguish their friends in the streets; in short, no midnight darkness exceeded it. If any ventured abroad with torches, they were obliged to return home again; which only happened once before, viz, in the emperor Titus's time, according to Ziphilin.—So that every place was filled with the shrieks of women; but the more prudent betook themselves to prayer to Almighty God, and with apprehensions expected the event of such a prodigy. Both the magistracy and clergy appointed supplications to be made, and to carry in procession to the Capuan gate, which leads to the mountain, the relics of St. Januarius, the tutelar saint of this city; where about the first or second hour of the night, towards the north, where perhaps there was not so large a quantity of ashes, a star or two were seen, and the azure face of the heavens began to appear; and afterwards the darkness, which had obscured the day, gradually to diminish in the night; and the ashes, by the shifting of the wind, to be driven into the sea. The following day continued somewhat dark, by reason of the remains of the ashes interspersed in the air. Vesuvius, having thus covered the fields with ashes, and belched out its grit for several days, so that its black torrent had almost reached the neighbouring sea, at length, in about 15 days, it ceased.

Of the Motion of a Tense String. By Brook Taylor, Esq. F. R. S. N° 337, art. 4, p. 26. *Translated from the Latin.*

Lemma 1.—Let ADFB, and A$\Delta\phi$B, fig. 13, pl. 1, be two curves, so related that, drawing any ordinates CΔD, EϕF, it is every where CΔ : CD :: Eϕ : EF. Then the ordinates being diminished ad infinitum, so as the curves may coincide with the axis AB; I say that the ultimate ratio of the curvature in Δ to the curvature at D is as CΔ to CD.

Demonstration.—Draw the ordinate $c\delta d$ very near to CD, and to D and Δ draw the tangents Dt and $\Delta\theta$, meeting the ordinate cd in t and θ. Then, because $c\delta : cd$ :: CΔ : CD, by hypothesis, the tangents produced will meet one another and the axis in the same point P. Hence by the similar triangles CDP, ctP, and CΔP, $c\theta$P, it will be $c\theta : ct$ :: CΔ : CD (:: $c\delta : cd$ by hypothesis) :: $\delta\theta$ ($= c\theta - c\delta$) : dt ($= ct - cd$). But the curvatures in Δ and D, are as the angles of contact $\theta\Delta\delta$ and tDd; and because $\delta\Delta$ and dD coincide with cC, those angles are as their subtenses $\delta\theta$ and dt, that is, by the analogy above, as CΔ and CD. Therefore, &c. Q. E. D.

Lemma 2.—At any instant of its vibration, let a tense cord, stretched between the points A and B, take any form of curve A$p\pi$B, fig. 14. Then will the increment of the velocity of any point P, or the acceleration arising from the force of tension in the string, be as the curvature of the string in the same point.

Demonstration.—Conceive the string to consist of equal rigid particles, infinitely small, as pP and Pπ, &c.; and erect the perpendicular PR = the radius of curvature at P, in which let the tangents pt and πt meet at t, and their parallels πs and ps at s, also the chord $p\pi$ in c. Then, by the principles of mechanics, the absolute force, by which the two particles pP and Pπ are urged towards R, will be to the force of tension in the string, as st to pt; and half this force, by which one particle pP is urged, will be to the tension of the string, as ct to tp, that is, because of the similar triangles ctp, tpR, as tp or Pp to Rt or PR. Therefore, because of the force of tension being given, the absolute accelerating force will be as $\frac{Pp}{PR}$. But the acceleration generated is in a ratio composed of the ratios of the absolute force directly and of the matter moved inversely; and the matter moved being as the particle Pp; therefore the acceleration is as $\frac{1}{PR}$, that is, as the curvature at P. For the curvature is reciprocally as the radius of the osculatory circle. Q. E. D.

PROB. 1. *To determine the Motion of a Tense String.*—In this and the following problems, I suppose the string to move through a very small space from the axis of motion; and that the increment of tension from the increase of the length, as also the obliquity of the radii of curvature, may be safely neglected.

Therefore let the string be stretched between the points A and B, fig. 15; and by a bow let the point z be drawn to the distance CZ from the axis AB. Then, taking away the bow, because of the flexure in the point C alone, that will first begin to move, by lemma 2. But as soon as the string is bent in the nearest points φ and d, these points will also begin to move; and then E and e; and so on. Also because of the great flexure in C, that point will at first move very swiftly; and thence the curvature being increased in the next points D, E, &c. these will be accelerated very swiftly, and at the same time the curvature in C being diminished, that point in its turn will be accelerated more slowly. And in general, those points which are slower being accelerated the more, and those that are quicker, less accelerated, it will be brought about at length, that the forces being duly tempered to each other, all the motions will conspire together, and all the points will at the same time approach the axis, going and returning alternately ad infinitum.

Now for this purpose, the string must assume the form of a curve ACDEB, the curvature of which, in any point E, is as its distance $E\eta$ from the axis; the velocities of the points C, D, E, &c. being also in the ratio of the distances from the axis, CZ, $D\vartheta$, $E\eta$, &c. For in this case the spaces Cx, $D\delta$, $E\epsilon$, &c. described in the same infinitely small time, will be to each other as the velocities, that is, as the spaces to be run through CZ, $D\vartheta$, &c. Therefore the remaining spaces, xz, $\delta\vartheta$, $\epsilon\eta$, &c. will be to each other in the same ratio. Also, by lemma 2, the accelerations will be to each other in the same ratio. So that, the ratio of the velocities always continuing the same as the ratio of the spaces to be described, all the points will arrive at the axis together, and all at once depart from it; therefore the curve ACDEB is rightly determined. Q. E. D.

Further, the two curves ACDEB, A$x\delta\epsilon$B being compared together, by lemma 1, the curvatures in D and δ will be as the distances from the axis Dϑ and $\delta\vartheta$; therefore, by lemma 2, the acceleration of any given point in the string, will be as its distance from the axis. Hence, by sect. 10, prop. 51, of Newton's Principia, all the vibrations, both great and small, will be performed in the same periodical time, and the motion of any point be similar to the oscillation of a body vibrating in a cycloid. Q. E. I.

Corol.—Curvatures being reciprocally as the radii of the osculating circles; therefore, let a denote a given line, then will its radius of curvature at E be $= \frac{aa}{E\eta}$.

PROB. 2. *Given the Length and Weight of a String, with the Weight by which it is stretched; to find the Time of one Vibration.*—Let the string be stretched between the points A and B by the force of the weight P; also let the weight of the string be N, and its length L. Let the string be put in the position AFpCB, and at the middle point C raise the perpendicular CS = the radius of curvature at C, and meeting the axis AB in D, and taking a point p very near to C, draw the perpendicular pc and the tangent pt.

Therefore, as in lemma 2, it appears that the absolute force by which the particle pC is accelerated, is to the force of the weight P, as ct to pt, that is, pC to CS. But the weight P is to the weight of the particle pC, in a ratio compounded of the ratios of P to N, and of N to the weight of the particle pC, or of L to pC, that is, as P × L to N × pC. Therefore, compounding these ratios, the accelerating force is to the force of gravity, as P × L to N × CS. Constitute, therefore, a pendulum of the length CD: then, by sect. 10, prop. 52, of Newton's Principia, the periodical time of the string, will be to the periodical time of the pendulum, as $\sqrt{N \times CS}$ to $\sqrt{P \times L}$. But, by the same prop. the force of gravity being given, the lengths of pendulums are in the duplicate ratio of the periodical times; hence $\frac{N \times CS \times CD}{P \times L}$, or (writing $\frac{aa}{CD}$ for CS, by corol. to prob. 1) $\frac{N \times aa}{P \times L}$, will be the length of a pendulum, whose vibrations are isochronous with the vibrations of the string.

To find the line a, put the absciss of the curve AE $= z$, its ordinate EF $= x$, and the curve itself AF $= v$, also CD $= b$. Then, by corol. to prob. 1, the radius of curvature at F will be $\frac{aa}{x}$. But $\dot{v}$ being given, the radius of curvature is $\frac{\dot{v}\dot{x}}{\ddot{z}}$: hence $\frac{aa}{x} = \frac{\dot{v}\dot{x}}{\ddot{z}}$; therefore $aa\ddot{z} = \dot{v}x\dot{x}$; and, taking the fluents, $aa\dot{z} = \frac{1}{2}\dot{v}xx - \frac{1}{2}\dot{v}bb + \dot{v}aa$, where the given quantity $-\frac{1}{2}\dot{v}bb + \dot{v}aa$ is added, to make $\dot{z} = \dot{v}$ in the middle point

C. And hence, completing the calculation, it will be $\dot{z} = \frac{2a\dot{x} - b^2\dot{x} + x^2\dot{x}}{\sqrt{(4a^2b^2 - 4a^2x^2 - x^4 - b^4 + 2b^2x^2)}}$. Now let b and x vanish in respect of a, that the curve may coincide with the axis, and it will be $\dot{z} = \frac{a\dot{x}}{\sqrt{(bb - aa)}}$. With the centre C, and radius CD $= b$, describe the circular quadrant DPE, fig. 17, and make CQ $= x$, and erect the perpendicular QP; then, the arc DP being y, it will be $\dot{y} = \frac{b\dot{x}}{\sqrt{(bb - xx)}} = \frac{b}{a}\dot{z}$. Hence $y = \frac{b}{a}$ z, and z $= \frac{a}{b}y$. And making $x = b$ $=$ CD, in which case also it is $y =$ the quadrantal arc DPE, and z $=$ AD $= \frac{1}{2}$L; then it will be $\frac{1}{2}$L $= a \times \frac{\text{DE}}{\text{CD}}$, and therefore $a = \text{L} \times \frac{\text{CD}}{2\text{DE}}$. Let there be therefore CD : 2DE :: the diameter of a circle : the circumference :: $d : c$; and it will be $aa = \text{LL} \times \frac{dd}{cc}$. Substituting then this value for aa, it will be $\frac{\text{N}}{\text{P}} \times \text{L} \times \frac{dd}{cc}$ the length of the pendulum isochronous to the string. Let therefore D be the length whose periodical time is 1, so will $\frac{d}{c}\sqrt{\frac{\text{N} \times \text{L}}{\text{P} \times \text{D}}}$ be the periodic time of the string. For the periodic times of pendulums are as the square roots of their lengths.

Corol. 1.—The number of vibrations of the string, in the time of one vibration of the pendulum D, is $\frac{c}{d}\sqrt{\frac{\text{P}}{\text{N}} \times \frac{\text{D}}{\text{L}}}$.

Corol. 2.—Because $\frac{d}{c}\sqrt{\frac{1}{\text{D}}}$ is given, the periodic time of the string is as $\sqrt{\frac{\text{NL}}{\text{P}}}$. And the weight P being given, the time is as $\sqrt{\text{NL}}$. Also the strings being formed of the same thread, in which case N is as L, then the time will be as L.

An Account of some rare Plants, observed lately in several curious Gardens, and particularly the Society of Apothecaries' Physic Garden at Chelsea. By Mr. James Petiver, F. R. S. N° 337, art. 5, p. 33.

An enumeration of 108 plants at that time esteemed rare, with short observations on each.

Celestial Observations, made at the Royal Observatory, Greenwich. By Mr. Flamsteed, Astronomer Royal. N° 337, art. 6, p. 65.

These observations, made in the years 1711 and 1712, are on the sun and moon, and the three superior planets Saturn, Jupiter, Mars; but being all contained in the author's Historia Cœlestis, it would be of no use to reprint them in this work.

An Account of the Roman Legions. By Dr. W. Musgrave. N° 337, art. 7, p. 80.

From this account we may observe, 1. That the number of legions under the Roman consuls, at one and the same time, was greater than under the emperors, and fewest of all under the kings, 2. That under the consuls, their number was greater in civil than in foreign wars. 3. That their number is less certain under the consuls, than under the kings or emperors. 4. That the stated number of legions, according to Lipsius, was about thirty. 5. That none ever had a greater number of legions than Octavius Cæsar, as none ever had a more flourishing army, according to Lipsius in Analect. ad Milit. Roman. than Trajan. 6. And lastly, that a true judgment cannot be formed of the strength of their armies, from one and the same number of legions.

Here the two following rules may be laid down, which will be of considerable use for understanding the Roman history, especially their military affairs; the first, that where the number of soldiers in an army is expressed, without that of the legions, which is commonly Appian's way, in order to reduce the army into legions, we must observe in what age it was, and what was the usual number of men in a legion of that age: for that number dividing the number of men in the whole army, the quotient will be the number of legions in it; e. g. Lucullus's army against Mithridates was, according to Appian de Bellis Mithridat. 30000 foot and 1600 horse, i. e. supposing 6200 in each legion, which was the number of men, including auxiliaries, in a legion of that age, five legions; according to Tacitus Annal. lib. 4, an army of 70000 men was cut to pieces by Boadicea, i. e. supposing that each legion, according to the custom of that age, consisted of 12000 men, about six legions.

Rule 2d, given the number of men in a legion of any age, together with the number of legions in an army of the same age, you will hence have the number of men in that army. And hence it appears that Romulus's army, consisting at first but of one legion, did not exceed 3000, or according to Plutarch 3300 men; afterwards, on the admission of the Sabines into Rome, and the addition of another legion on this increase of the city, 6000, or 6600; nor indeed a greater number, though, as was said above from Dionysius, after Romulus's death, there were 45000 soldiers in Rome; in the war against the Volsci, Æqui and Sabines, there being 10 legions, and in each legion 4000 men, the whole must necessarily amount to 40000. Under the Roman emperors, when there were almost 30 legions, the whole number, when complete, amounted to, multiplying 6500 by 30, 195000, or 200000 men, more or less; and adding the allies, of whom there was an equal number, to 400000.

The Husbandry of Canary Seed. By the Rev. Mr. Edward Tenison. N° 337, art. 8, p. 91.

To prepare land for this seed, let it be broken up some time in April, and ploughed again about Midsummer, and again in August, that by frequent tillage the weeds may be burnt up and destroyed. Plough the last time about the latter end of February, or the beginning of March, if the season proves dry; if not, it is better to wait for a dry season; for then only will the ground be fit to receive the seed. With a hoe you must from time to time carefully cut up the weeds. If they are not kept entirely under, much of the seed will be lost for want of ripening. In very good land half a bushel of seed will be enough to sow an acre. It will thrive best on a stiff clay; it will grow on any sort of loamy land, rich enough to bear hemp. If you apprehend that the land is not sufficiently strong, you will do well to allow from half a bushel to 7 gallons of seed, to sow an acre with.

The seed is ripe sooner or later, as the spring affords an early or late season of sowing it. In some summers it is cut in August, but the most usual time is after wheat harvest. When it is cut, it must in most years lie 5 or 6 days in swarth, and then be turned, and lie till one side is dried and rotted as much as the other, which may be about 4 or 5 days longer.

The produce on land that is very good, is about 6 quarters per acre. If the land be but indifferent, or if the weeds be not kept under, then from 4 to 5 quarters an acre is as much as you can expect. The price of seed is from 2l. to 6l. per quarter; but the most usual price is from 40s. to 3l. It is difficult to thresh. So much of the seed as, after threshing, is beaten out, is to be run through a wire sieve, such as is used to separate cockle from corn, and the husks of every sifting, that will not pass through the sieve, are to be thrown by in a heap, to be threshed over again.

Extracts of several Letters from Mr. Edw. Llwyd, containing Observations in Natural History and Antiquities, made in his Travels through Wales and Scotland. Communicated by Dr. Hans Sloane, R. S. Secr. N° 337, art. 9, p. 93.

In the coal-pits of the forest of Dean, I found all the species of capillaries, besides some other new plants; with two species of astropodium, gathered on the Severn shore, the only rareties of the kind, I suppose, that have been discovered. I doubt not but the coal plants have been observed by the workmen long since, though they escaped the notice of naturalists. I find it well known to all our country colliers by the name of carreg redynog, i. e. the ferny stone; and Mr. Williams, Archdeacon of Cardigan, told me he had observed much finer patterns 25 years since in the coal pits of Glamorganshire, than some that I showed him. The whole braken that Kirkman mentioned was a noble curiosity; we saw none such in the forest; though we found them pretty large. The stalks of fern and hartstongue I think we often met with, but cannot say we saw any roots. We often met with the membranaceous substance of leaves.

I have been very inquisitive about coins of the Princes of Wales, since I began this undertaking, but could never see one of them; though the bishop of Bangor, who is very well skilled in British antiquities, told me, a relation of his had one of Lhywelyn 'ab Iorwerth, who was cotemporary with Richard I. and King John. By the princes of Wales, I understand the British Princes, from King Kadwaladr about the year 600, to the last prince Lhewelyn ap Gruffydh, about the year 1280. I have found several of the more ancient British coins; of which you see divers figures in Camden. Mr. Nicholson quotes Cæsar for the Britons having no coins; whereas, on the contrary, Cæsar's words are, nummo utuntur parvo et æneo: nor can I see any reason to doubt of British coins of all sorts of metal, till it can be shown whose coins those are, which Mr. Camden and other writers take to be British.

The druid beads are generally glass. Since the last edition of Camden I have met with two or three of them, having a snake manifestly painted round them: so that I take it for granted, the ova anguina of the British druids, were these glass beads; though those of the Gaulish were the shells of the echini orbiculati laticlavii.

We searched the high mountain near Brecknock, called Y Vann uwch deni, but found nothing in it new, nor any great variety of rare plants. The choicest were sedum alpinum ericoides, in abundance; argemone lutea; rhodia radix; muscus cupressiformis, and about half a dozen more of the common Snowdon plants. Lysimachia chamænerion dicta is a common plant (by the name of lhysie'r milwr, i. e. herba militaris) in the meadows through all the upper parts of this county. We also met with sorbus legitima and sorbus torminalis (grown to as great a height as the ornus) neither of which had ever occurred before in Wales. But of all these topical plants I was surprised at none so much as the capillus veneris verus, growing very plentifully out of a marly incrustation, both at Barry island and Porth Kirig in Glamorganshire, and out of no other matter; and also that gnaphalium majus Americanum should grow on the banks of Rymny River, which runs altogether over iron stone, for the space of at least 12 miles, beginning near the fountain-head in a mountain of this county; and yet not a plant of it to be seen elsewhere throughout Wales. In a great lake, called Lhyn Savadhan, I found a pellucid plant I had never met with before: the leaves are extraordinarily thin and transparent, in form not unlike small dock leaves; but the middle rib is continued beyond the extremity, so that each leaf has a soft prickle at the end. We found there also the hippuris saxea, and two elegant sorts of small leeches, which I suppose not described.

The limestone of this county affords small glossopetræ and siliquastra; but they are but very scarce in comparison of the quantity found in Oxfordshire, Northamptonshire, Berks, &c. The most considerable rarities it affords are Fairy Causeways, which I call so in imitation of the Giants' Causeway in Ireland; for whereas theirs may be half a mile long, ours seldom exceed a yard. Our lime quarries yield two or three bodies congenerous with it, though of a very different form; and perhaps all may be referrible to the coralline class, the second in my catalogue.

Our travels in the Highlands of Scotland, were through Cantire, Argyle, and Lorn, besides the isles of Macychormic, Mul, and y Columb Kil; and in the Lowlands through Glasgow, Sterling, and Edinburgh. We met with several inscriptions, but none of them Roman, nor indeed ancient: however, we copied all we met of 200 years standing, &c. for the sake of the orthography of the Irish names, which are written differently from what they are now. We also took figures of some broaches, or silver and brass fibulæ, used by the women to clasp their koleriv, a garment answering our nightrails. But we were most diverted with their variety of amulets; many of which, if not all, were certainly used by the Druids, and so have been handed down from parents to children ever since. Some of these may be rendered in English, 1. Snake-button. 2. Cock-knee stone. 3. Toad-stone. 4. Snail-stone. 5. Mole-stone. 6. Shower-stone; and, 7. Elf-arrow.

1. The snake-button is the same described in the notes on Denbighshire in Camden, by the name of adder-beads: but there is a great variety of these, as to colour and ornament; so that between Wales and the Highlands, I have seen at least 50 differences of them. In Ireland, though they are tenacious enough of all old customs, I could hear nothing of them, and conclude, that either the Irish had no Druids, or that their want of snakes frustrated their advancing that imposture among the people. Not only the vulgar, but even gentlemen of good education throughout all Scotland, are fully persuaded that the snakes make them, though they are as plainly glass as any in a bottle.*

2. The cock-knee stone is an echinites pileatus minor, of Flint; which they firmly believe is sometimes found in the knees of old cocks.

3. The toad-stone is some pebble, remarkable for its shape and sometimes variety of colours. This is supposed to prevent the burning of a house, and the sinking of a boat: and if a commander in the field has one of them about him, he will either be sure to win the day, or all his men fairly die on the spot.

4. The snail-stone is a small hollow cylinder of blue glass, composed of four or five annulets, and as to form and size, resembles a middling entrochus. This among others of its mysterious virtues, cures sore eyes. 5. The mole-stones are rings of blue glass, annulated as the aforesaid snail stones. 6. They have

* For some account of this superstition in Wales, see Pennant's British Zoology, vol. iii. p. 29 under the article Viper.

the ombriæ pellucidæ (which are crystal balls, or hemispheres, or depressed ovals) in great esteem for curing of cattle; and some on May day put them into a tub of water, and besprinkle all their cattle with that water, to prevent being elf-struck, bewitched, &c. And

7. As to this elf-striking, their opinion is, that the fairies (not having much power themselves to hurt animal bodies) do sometimes carry men away in the air, and furnishing them with bows and arrows, employ them to shoot men, cattle, &c. The arrow-heads they ascribe to elfs or fairies: they are just the same chipped flints the natives of New England head their arrows with at this day; and there are also several stone hatchets found in this kingdom, not unlike those of the Americans. I never heard of these arrow-heads nor hatchets in Wales, nor in England. These elf arrow-heads have not been used as amulets above 30 or 40 years; but the use of the rest is immemorial: whence I gather that they were not invented for charms, but were once used in shooting here, as they are still in America. The vulgar in this country are satisfied they often drop out of the air, being shot by fairies, and relate many instances of it.

Near Glasgow we found two fossils toto genere new: one resembling small joints of a lobster's arm, but much longer; the other somewhat like large glossopetræ, or perhaps like the mucro of a pinna marina. These figured stones are found there in an iron stone, though I never saw them in that kind of matter in Wales. We found both shells and entrochi gone off to that substance, having changed their matter and much of their shape. Near the same town, searching for these fossils, I found in the midst of the lime-stone some cochlitæ, composed of flint; but conchitæ of spar, gone off so far from the shape of shells, as hardly to be known, were it not from others in the same place retaining their shape more entirely.

Mr. Southerland gave me specimens of the chamæpericlymenum, adianthum acrosticon, and pyrola alsines flore Europæa. I had nothing for him in exchange, but samples of the vitis idæa foliis myrtinis crispis Meretti, together with some of the berries. This I found plentifully for some miles together in that part of Mul, next to Y Columb Kil. It is very different from the common vitis idæa sempervirens fructu rubro; being a larger plant, and much more branched; the leaves of a cripsed surface, and the berries (which they told me it retains all the year) like those of holly. Going up one of the high hills of Mul, we found rhodia radix; pes cati; cotyledon hirsut. vaccinia rubra; sedum alp. trifido folio; and alchemilla alpina quinquefolia, which I had never seen grow spontaneously. We found in this island a curious fucus arboreus, with a ruffled stalk.

A Description of the Plague in Dantzick, in the Year 1709. *By Dr. Gottwald.* N° 337, art. 10, p. 101.

This dangerous and destructive distemper has now raged for some years, in many cities, towns, and villages of Poland, where it has swept away vast numbers, and has even left some places quite desolate. It began near Pinczow or Pikzow in the year 1702, soon after the unfortunate battle between the Saxons and Swedes. The next year it appeared in some parts towards Cracow and Russia, and had already caused a great mortality near the Hungarian mountains, called Crapack; till it went eastward to the upper Volhinia, and again westward to Lemburg.

In 1704 it raged very violently in these two palatinates; so that Lemburg, the capital city of Russia, lost a vast number of its inhabitants, and many to save their lives were obliged to fly from it. In autumn it spread in neighbouring places, to the west and south, beginning in a village called Radymno, on this side Jarislaw; and afterwards invading other places, between the rivers San and Volodarora, it spread till it came towards the Samber. In 1705 it left Lemburg, and went north and west to Great Poland, through Jarislaw, Sieniawe, Zamose, and other adjacent places; and continued in that part of the country, as far as Posen, all that year. In 1707 it entered the city of Warsaw, where it destroyed that summer vast numbers of people. In 1708 it came nearer Polish Prussia; and broke out the latter end of August in Thorn, where it continued till the beginning of the next year, and swept away great numbers.

This approach made us (in Dantzick) very apprehensive of danger: public prayers were ordered in the churches; and the magistrates left nothing undone that could tend to our common safety. Commerce and communication with the infected, and even suspected places, were forbidden: no sort of merchandise or effects that came from such places was allowed entrance, especially such goods as might easily receive and retain the contagion; as wool, raw leather, furs, beds, &c. All strangers and travellers were strictly examined, and none permitted to enter without sufficient proofs, that they came from healthy places. All the inhabitants were cautioned neither to hold correspondence with, nor on any pretence whatsoever to harbour those of infected places, or to go to them.

These and other necessary precautions were taken, and by public edict enjoined from July 11, 1708, to February 27, 1709.* Notwithstanding all

* This injunction should have been continued for a much longer time. A quarantine of persons and goods coming from Thorn, should have been enforced for at least half a year after the cessation of the epidemic in that town. Had this been done, Dantzick would probably have remained free from infection. The magistrates of Dantzick were perhaps induced to discontinue so soon the salutary restraints they had till then (Feb. 1709) imposed upon commerce, in consequence of the temperature of the season; but the writer of this note has shown in a tract recently published on Contagion,

which, the distemper gradually insinuated itself: for in March, 1709, there died out of one district of the old town, called Raumbaum, 7 persons: the eighth remaining was a young girl, who by order of the magistrates was sent to the hospital, having already some bad symptoms, as buboes, about her; which being yet unknown, one would not have presently taken for pestilential, but rather venereal: but they soon showed their epidemic kind, by seizing other children kept in the same rooms.

In the same month I had under cure a Polish lady, from Jarislaw, labouring under a cachexy and tympany. In a fortnight she recovered so as to go to church: but she soon fell ill again, being taken with a slow fever, and a sudden decay of strength, and died within 8 days. On washing her dead body, there was found on the lower belly a brownish red swelling, about the size of a small hen egg, which I afterwards judged to be a furunculus: so much I could then guess, that it was not a common swelling; for it was surrounded with a lead-coloured circle, from which proceeded several blue rays in the adjacent parts. However, this was not a sufficient ground to demonstrate, or to give public notice, that the contagion was already among us; but in the mean time care was taken to prevent, as much as possible, the further progress of this distemper: the streets, the waters, and houses were ordered to be cleansed from all manner of nastiness.

On Sunday the 16th, the governors of the hospital desired that I would be physician in ordinary to their hospital, or at least that I would give my help and assistance at present, by reason of the many sick that were in the hospital, and which increased daily. Next day I went to the hospital, and inquiring for the surgeon in ordinary, I was informed that he died the day before; but they knew not of what disease. In viewing the patients, I found 10 in a room together, of various ages, some of which had buboes, others carbuncles, others gangrenous ulcers, which one cannot always judge to be pestilential. In another room there were above 20 children, from 6 to 13 years of age, all which, except 4 or 5, had either pestilential buboes in the groin, armpits, and about the neck, or else carbuncles on the arms, thighs, legs, and other parts of the body. After I had perfectly informed myself of the state of the hospital, I took my leave, having first recommended to the attending surgeon, such medicines as I judged proper in these cases to be given inwardly; but I could not forbear telling him my opinion, that these were symptoms, if not of a plague already insinuated, yet at least of something but little inferior to it, and certain forerunners of that destructive distemper.* The surgeon did not then think it so

that even in the northern parts of Europe extreme degrees of winter-cold have not afforded a security against the introduction and spreading of the plague.

* It is truly astonishing that any doubts should have been entertained of the actual introduction of the plague, under such circumstances; especially when it was known that this disorder had been raging only a few months before at Thorn, distant from Dantzick not more than 70 miles.

dangerous, but was soon after convinced of his mistake, the distemper seizing him and his family, of which himself, his wife, and all his children died. From this time the malady and number of patients daily increased, and mostly in the outer parts and suburbs of the town.

June the 26th, my daughter, of about $6\frac{1}{2}$ years old, began to complain of an unusual head-ach. I presently gave her some bezoardic drops, with volatile cephalic spirits. Four hours after she was much altered; her eyes stared, her extreme parts were distended, and violent convulsions ensued, though they did not continue long; but afterwards she lay as if she was paralytic, and could not be made sensible. No medicines availed; so she died the third day following. The same day also my wife fell dangerously ill; but in a few days got past the worst; and though she was confined to her bed a long time, yet with the assistance of other physicians, she at last happily recovered. I soon after found that all the efforts I could make were too weak to resist the violent progress of such a distemper: for one might daily perceive how the poison got strength, and the disease harder to cure.

Thus the distemper grew predominant, and by the end of August was spread almost over the whole town. All possible care was taken to supply the numerous poor with necessaries, both of food and physic; to have the streets and houses kept clean and neat; the communication of the sick with the sound as much as possible prevented; and the dead soon buried; and all this by the authority and direction of persons appointed by the magistrates, called provisors of health. Besides, the pest-houses were opened, and well provided with all manner of necessaries, as also with overseers and servants. Many persons of condition hired particular houses for their servants; and others made up convenient apartments in their gardens, and procured servants and nurses to attend them, that in case of infection they might there be taken care of. The pest-waggons and chairs went from early in the morning, till late at night; the former to carry away the dead to be buried, the other to convey the sick to the hospitals. Besides the ordinary church-yards, there were others made without the walls of the town. In short, every thing was ordered with wonderful convenience, and so well and carefully looked after, that without it the infection would have destroyed a much larger number of people.

In August and September the plague raged with the greatest violence, when the public lost many eminent men; but of our chief magistrates only 2, and as many of the judges: of our divines about a third part died: of the physicians and apothecaries none: of the surgeons in ordinary only 2; but of their assistants or subordinates, especially such as belonged to pest-houses, a vast number. The principal citizens suffered very little, but the garrison a great deal, though the officers escaped pretty well; but the handicraft and common tradesmen, as well masters as journeymen, apprentices, porters and labourers, were very much diminished, and died in the year 1709, to the number of 24533; in which are likewise included all such as were buried without the town, and some of another jurisdiction, of which we have not been able to get a true and exact account.

There were two things remarkable, which I must not omit to notice. The first is, (as observed by Dr. Shelwig) that the plague decreased in the same proportion as it had increased. For in June the number of the dead was 319; in July it rose to 1313; in August to 6139; and in Sept. to 8303; which was the highest degree of mortality. After this the numbers again decreased; so that in Oct. they were 4932; in Nov. 1961; in Dec. 584; and so on gradually lessening. The other thing remarkable is, that but few of the people of condition and quality died of the contagion, in comparison with those of the common sort; which may be attributed to the different number of poor and rich, and to the great care and precaution the latter made use of to avoid it.

The disease surrounded the whole town, and infected every quarter of it; and we heard that our neighbours on the frontiers had likewise received the infection. But it is to be admired, that in no district of the town the number that died was less observed, than in that part which we properly call the city. Though in the course of the infection it spread itself, and run as it were in a circuit, yet its motion was not so transient in shifting from place to place, but that it continued fast to its first hold, only with this difference, that it did not so severely infest the places it had at first possessed as those that it entered later.

That the plague is a poison, or rather carries a poison along with it, is acknowledged by all physicians: but of what kind and nature, and whence it proceeds, few can agree. It is well known, that it has a twofold operation, so that the blood of the infected is sometimes coagulated, and sometimes dissolved, according as the humours of human bodies are disposed; and yet they are both alike pernicious. If it coagulates, the juices stagnate, and the progressive motion ceases: if it dissolves, then the natural connection and cohesion of the particles become colliquative and incoherent, and the spirits gain a free exit, and leave the body motionless.

That the air was infected, during the contagion here, is certain: not that I mean a general infection, as if the air was by a supernatural power so tainted and corrupted, as to infect all things breathing; but, as it is a subtile, moveable, and every where expanded body, it attracts and receives all effluvia and exhalations, as a sponge does water, and imparts them likewise, by means of its motion, to other bodies; so that, as a communicative medium, by its entrance into our bodies, we receive whatever it carries along with it. It is generally observed, that the plague commonly ensues after great battles. The reasons alleged for it are, that the exhalations, proceeding from the vast number of dead bodies, corrupt the air, by which mankind are afterwards infected. How much more then must they be infected, when there is an actual contagion among them. The infection of the air must be still greater in proportion to the actual increase of the pestilence, when so many thousands die, and some continue to lie putrifying for many days above ground, and others are buried without a coffin, and but a very little way under ground. Besides the heat of the weather (for these casualities generally happen in summer) causes the bodies to corrupt sooner. Besides there is a great difference between those that die in battle, and those that die of the plague; the former being sound bodies; but the latter infectious, and a mere mass of corruption before they died, and must consequently infect the air more than the former.

What I have said concerning an infected air, is confirmed by the testimony of all who have written on the plague by experience; and they have likewise observed the circumstances and signs to be the same as they appeared to us. What they assert, I found to be true by experience: otherwise I know not to what cause to impute my having felt, during this contagion, the very same pains as they did, several times, one after another, insomuch that sometimes I was not able to stir. Besides, when I have come from places where people lay as yet unburied, or from infected houses, I have frequently found a palpitation at my heart, a pain in my head, and anxiety, with a retching to vomit, but without bringing any thing up.

In the beginning of October I fell ill of a violent catarrh, which obliged me to keep my bed. On the third day it turned to a salivation, which continued for 3 hours so violent, that my gums and mouth swelled as if I had taken mercury; but the next day I recovered and was well again. From whence I conclude, that I received it from the poison in the air. I had once like to have died of the venom of a viper; for, in April 1703, I was bitten by one in the fore-finger; of which in a moment after I felt disordered, and at last fell into convulsions. Having immediately after the bite sucked my finger, my face swelled, and my mouth almost closed up: being in this condition, few expected my life; yet 3 hours after, the convulsions began to cease, and I gradually recovered. Though many of our inhabitants kept continually at home, used all manner of preservatives, both inward and outward, suffered none of the infected to approach either them or their servants, yet they caught the infection. So that I cannot in the least doubt, but that the air is infected; and that by means of some morbid effluvia, with which it is impregnated, it also infects and destroys mankind.

The signs observed in the air, at the time when the plague rages, are very evident; and especially those that are observed in mankind themselves, will easily evince the infection of the air. On Aug. the 11th, at noon, I first observed a stinking mist, like a thick cloud, but of short duration; but at 4 o'clock it returned from the north west, so very thick, that it perfectly darkened the air, and hindered the sight. It was neither blue nor grey, as other common mists; but of a blackish yellow, like the vapours that rise from the effervescence of oil of vitriol with oil of tartar. After it had reached the middle of the town towards the south east, it inclined westward, and there emitted a violent stench. Another sign of an infected air was not, as may perhaps be thought, only a vulgar fancy, but the careful observation of learned persons, viz. That in the month of July the crows, daws, sparrows and other birds, which at other times are to be seen here in the town and about the gardens in vast numbers, were all fled, and none of them to be seen till Nov. The same was observed of the storks and swallows likewise; and I can positively affirm, that I saw none of those birds all those 4 months.

As to the effects on the people, I have said that the distemper began and increased gradually, and lessened in like manner; but the middle was the worst, and most violent: for at first the buboes were more common than the furuncles, carbuncles and vibices: afterwards again the petechiæ were more common than these; though during the whole time of the contagion, they were never wholly separated, so as to appear sometimes one without the other: at last the petechiæ and carbuncles went off; but the buboes continued last of all.

The buboes, which are to be reckoned the first of the external signs, lie very deep in the skin, and are at the beginning hard, unmoveable and round; afterwards they grow longer, and may be moved. Outwardly they do not look red, till they are drawn and brought to maturity. They are generally found in the groins, armpits, and about the neck. Most of them come with a very violent, cutting and pricking pain, accompanied with heavy symptoms, as pain in the head and back, shivering colds, interchanged with heat, anxieties, faintness, and frequently also bilious vomitings. According to the degree of malignity, the symptoms are more or less violent: sometimes they are very mild, and the bubo proceeds without any great pain.

Furuncles differ from the common buboes, as they appear mostly in the fleshy parts, to the number of 5, 7, or 9 on one body. They are sometimes red, and swell to a greater height than the buboes: their pain is very violent, and disturbs the patient's rest. The other symptoms are much the same as in buboes; and though they are not always alike, yet they are never without a fever, and are attended with pain in the back and belly.

Carbuncles and anthraces are much more pernicious, and of various kinds: and I dare affirm, that if they had been rightly noticed, there might have been observed among them many strange figures and species. I shall only mention 4 sorts. The first shows itself somewhat prominent and rising, of a dark brown, the uppermost skin appearing somewhat dry, as if it was burnt, and has a lead coloured circle. In the beginning it is frequently no larger than a pea; but, if not prevented, increases in a short time to the size of a crown-piece. Inwardly it is moister than the following species, and may be easier separated. Its seat is generally in the fleshy parts, as on the shoulders, sides, hips, neck, the arms, and legs.

But the 2d sort lies somewhat deeper, and seems a little more depressed. The eschar is in the middle of it, which is wholly dark and ash-coloured, full of small cuts, as if burst by too great a dryness, and has a strong lead-coloured circle, behind which the sound flesh looks red and shining. It corrodes the flesh about it, and fixes its roots very deep. In its separation it feels drier than the former, and may be taken out by pieces. It is fixed generally where the flesh is thickest, as on the buttocks, the calf of the legs, and under the short ribs towards the back. These two species burn much more violently than the rest, so that a red-hot iron can hardly occasion more pain; and indeed the patients are almost killed by the mere pain of them.

The 3d sort is not very large. At first it appears like a blood swelling, not so black as the former, the skin being also somewhat wrinkled. In its increase, small blisters arise on the middle of it, and form an eschar in small clusters, which were small carbuncles. They are commonly situated in membranous and tendinous parts, as towards the knees, behind the ears, on the toes, &c.

The 4th species is the most curious, but very deceitful. It appears with a high blister, yellowish, as if it contained corruption. At first there is a red, afterwards an ash or lead-coloured circle about it: the blister soon falls, and within is seen the carbuncle hardly the size of a pepper corn, which continually eats deeper and wider. These are seated on the cartilaginous parts: I have found them near the pit of the stomach on the cartilago ensiformis, and on the short ribs.

All these 4 species of carbuncles take deep root, and in the beginning burn very violently, but the two former more than the latter. The symptoms attending them are violent, though not always sensible, but so much the more dangerous; as generally restlessness, deliriums, sudden loss of strength, pain in the head and back, anxieties, inward burning heat, thirst, &c. The first onset is frequently attended with a shivering, bilious vomiting, &c. According to the greater or less power of the latent miasma, the symptoms are always more or less violent.

The petechiæ or malignant spots, which are always very dangerous, especially in this contagion, raged violently. I have observed of them also 4 distinct species. The first look like flea-bites, and have been called by some authors pulicares. They break forth reddish, and soon changing their colour, grow brown, and at last black. They are round, and spread all over the body, excepting the face, where they are not always found. The 2d species appears in the form of lentils, and are called lenticulares; they are likewise at first ruddy, but in about 24 hours change colour, becoming dark and ash-coloured: they spread, as the former, all over the body. The 3d sort appears in large round spots, of the same colour as the former, but are found only here and there on the body: sometimes they are also intermixed with the lentil kind. The 4th species is not unlike the measles, and spreads all over the body. After 2 or 3 days you find them shoot into little blisters rising to a head, but containing no matter. They dry away the 5th day, at which time the patient's death is not far off. After they are dried away, the skin is rough, and much like that of a smoked goose, only not quite of the same colour. To these might be added a 5th species. These appear not till after death, either in points, or spots. They show themselves mostly on the back and breast, and give plain indication that there was a malignity which killed the patient.

Many pernicious symptoms appear also at the breaking out of these several petechiæ; as pain in the head and loins, vomiting, diarrhœa's, palpitations of the heart, great anxieties, faintings, shivering in all parts of the body, which are frequently succeeded by heat or sweat, deliria, epileptic fits, lethargy, a dismal hippocratic face, staring eyes, bleeding at the nose, inordinate menstrual fluxes. In short the symptoms are so many and various, that it is impossible to observe them all.

Next are the plague-stripes or rays, called, by Joh. Bapt. Sitonius, vibices, and by others, molopes. They are not seen before the latter end, for death itself attends them: and this used to happen very unexpectedly, though the symptoms were tolerable, and attended by hopes of recovery; yet like lightning they shot upwards from the breast to the face, all in strokes of various colours, blue, green, brown, and yellow; first covering the face as high as the nose, and from thence spreading farther to the forehead. This so disfigured the patient, that he was frightful to behold: his eyes grew stiff, his tongue trembled, his speech gradually ceased, and inwardly there was great anxiety and confusion; from all which the struggle between life and death might well be observed.

I shall now last of all mention the fire-bladders, which I have only observed in two patients, and that in the beginning of the contagion, both which recovered; and therefore I do not think them so very dangerous, as Mr. Purman describes them to be, unless we mistake the species and property of them. To me they appeared as broad as a shilling, of an irregular height and figure, with a clear wrinkled skin, as if shrivelled by fire: they at last emitted a small moisture, and vanished in a few days. I have observed them only on the belly, thighs, and legs. They came forth with a small cold and succeeding heat, and with pains in the head and back, and weariness.

These were the external signs, as they appeared to us, and as far as I was capable of describing them. As for the symptoms, seeing they appeared very various, though the exanthemata were one and the same, it is impossible to describe them so nicely as might be wished; yet by reason of the prognostics I have divided them into several classes for my own practice, that I might judge the better of the event. In the 1st class I placed all those that were in themselves not dangerous: in the 2d those that were doubtful, and had various events, both good and bad: in the 3d, those that were quite dangerous.

The symptoms, not dangerous, were pain in the head, a small shivering or cold, a tolerable heat, nauseating of victuals, thirst, the belly distended with flatuses, anxieties, dejectedness, pains and stitches behind the ears, in the temples, and on the shoulders, heaviness in all the limbs. The dubious symptoms were, palpitations of the heart, shortness of breath, anxieties and faintness, looseness, vomiting, dryness of the throat, restlessness, a continual fever, delirium, &c. The symptoms quite dangerous, were sleepiness and lethargy, palsy, epileptic fits, cramp, bleeding at the nose, irregular menstrual fluxes, miscarriages in childbearing women, sudden loss of strength, rigor and shivering through all the limbs, burning heat, staring and watering eyes, continual inquietude, with great anxieties, external coldness of the limbs, and inward heat, with dryness of the tongue and throat.

I shall mention a remarkable thing, that was very common at the time when the plague raged, and is not to be reckoned among those signs that happen by accident. Several people, even the stoutest, were frequently struck with a fear, horror and anxiety, insomuch that they perceived a violent trembling and beating of the heart, and pain in the back. Many of them would presently fall into despair, and concluded for certain that the plague had already seized them. This was more frequently observed in the months of July, August, and September. It seemed strange to me at first, but I found that it had also its natural causes, which were stirred up by some secret passions of the mind, and therefore might also be remedied by natural means.

Chearful and encouraging discourse, to rouse and comfort the spirits, went a great way in the cure; but there were many on whom this availed nothing, but who remained inconsolable and melancholy, so far, that at last they died. At length this fear got the upper hand so much, that even the physicians themselves left the town, and fled. The nearest relations would not venture so much as to visit those that were so possessed, or give them any assistance: and we have many instances of parents, who in this case would not visit their own children, though they lived in the same house with them.

The particular accidents, as far as I could observe in my practice, and from thence prognosticate, are as follow: The buboes were in the beginning of the contagion much more dangerous than afterwards; and those which happened on the left side, were more pernicious than those on the right. Buboes did not always need to be drawn and extracted, nor would they sometimes be drawn or forced outward, but were dispersed by good emollient medicines. It was better not to lay plasters presently on the buboes, but to stay till the 5th day, and then it might be done with safety.

Carbuncles seated on the nervous* parts proved more dangerous than on the fleshy parts. Where carbuncles came not quickly to separation, the case was dangerous. Carbuncles, without the patient's being particularly sensible of them, without heat and great lassitude, were followed by death the 5th day. They generally after the 5th or 9th day admitted a cure, but required great care, especially that the patient might not take cold. If a bubo happened near a carbuncle, it was a good sign, and less dangerous than if thecarbuncle was alone. Carbuncles near petechiæ, or spots, were generally mortal.

The petechiæ, or spots like flea-bites, were mildest of all; some patients even went abroad with them, but seldom with any benefit. Petechiæ, that did not break forth before the 5th day, prognosticated death. Such petechiæ, as are called lenticulares and purpuratæ, were at this time all mortal. Occult petechiæ brought certain death. Vibices, or plague-stripes, were infallible signs of death.

As soon as a shivering, with pain in the head and back, bilious vomiting, and great lassitude happened, then was the patient taken with the plague. If the shivering was violent over the whole body, and a trembling in all the limbs, the pulse also weak, then on the 3d day infallibly death ensued. Hæmorrhages, or bleeding at the nose, or irregular menstrual fluxes, whether they happened the 1st, 2d, or 5th day, or even the 7th or 9th, were always dangerous and mortal. All evacuations generally, if they came with sudden loss of strength, and an unequal trembling pulse, were not critical, but colliquative, and occa-

* Tendinous?

sioned death. Ordinary menstrual fluxes, if there was no bubo or furuncle, were not dangerous. Generally all beginnings of this distemper, which affected the senses or nervous parts, were dangerous, and signified death. Great inclination to sleep, or lethargy at the first invasion, was a dangerous sign; and though the patient bore up under it the 2d or 3d day, yet he rarely escaped death.

All relapses were dangerous: if they happened soon after the 5th day, the 7th was the last; if after the 9th, then the patient fell into a hectic fever, and died some weeks after. If the patient the 3d day after the attack of the distemper, appeared strong, and talked much, yet had an inward fearfulness, then he died the 3d day following. If the patient was taken with a shivering, not succeeded by heat, but great weakness, and without any visible sweat, had a dry throat, a dry breast, and a tickling cough, the case was very dangerous.

As to indications from the appearance of the urine, I shall observe, that a clear and pale urine, which we otherwise call raw and unconcocted, if it does not soon change and give a sediment, is dangerous. A bloody and putrid urine, let it look otherwise as it may, if it had on the surface round about the glass a pale red circle, was at this time generally a sign of malignancy; nay, though the urine was not stirred, and only this circle was seen, it never signified any good, but was at least a sign of great weakness. Lixivious urine, without sediment, and half concocted, and also in small quantity, showed a defect in the strength, and a lamentable event.

The remedies and precautions which were advised, and directed, as far as we could prevail on people to use them, were chiefly the following. Common brandy, the panacea of the vulgar, was forbidden; and in its stead the moderate use of French brandy was allowed, and not without great benefit; for thereby the spirits were cherished, and fear and anxiety banished; all violent passions of the mind proving destructive to health. Pot-herbs, garden-fruits, and the like, which are generally flatulent, as also very salt and high seasoned meats, were discouraged as much as possible. The medicines in pharmacy, which were used, were many. Of simple internal medicines, which were in greatest esteem, were radix angelicæ, calamus aromaticus, myrrha rubra, sulphur, nitrum, ruta, and the like. Of compounds, the chief were Machandel-chalk, theriaca, mithridate, various preparations of pestilential vinegars, morsels, troches, all manner of prepared powders, also gun-powder, &c. Among other preservatives, the following sent from Thorn, being in great esteem, I shall here give the prescription of it. ℞ Aloes hepat. ʒvj. terræ sigillat. ℥ss. theriac venet. ʒvj. rhabarb. opt. ʒij. croci orient. ʒiss. zedoar. ʒij. myrrh. rubr. ʒiij.

agarici ʒj. gentian. dictamn. rad. tormentill. camphor. castor. rad. angelic. a. ʒiss. misce. Stamp these species small, and pour upon them 1½ pint of good French brandy; then set it in a warm place in a well closed bottle, and extract the tincture; of which tincture take 12 drops as a preservative: but those that are already seized with the plague, or any malignant fever, must take a spoonful of it, and sweat upon it. What the benefit of this arcanum may be, any judicious physician may judge for himself. For my part, I can neither commend the composition nor the operation of it, not having found that in the disease it performed any thing extraordinary; nor was it so beneficial as much simpler medicines. Those who made use of their own physicians, and conformed themselves to their prescriptions, fared much the better for it; as having their bodies seasonably prepared with alexipharmacs, bezoardics, antiscorbutics, diaphoretics, diuretics, gentle laxatives, &c.

My own preservative was an essence, which was prepared of the species of the essence of woods, saffron, aloes, and red myrrh, extracted with spirit of wine, and adding to it the mixtura simplex: which I found very beneficial, not only towards preservation, but likewise in the cure. Dr. Achatius Muller, at the very time when the plague was at the height, had great success with his acetum antimonii ex minera, which was mixed with bezoardics. Besides my essentia præservativa, I gave sometimes a bezoardic powder at night, consisting of the bezoardicum Senn. bezoardicum compositum Wagneri, or else this bezoar mixed with specificum cephalicum Michaelis. And if there was the least appearance of the distemper, I ordered my sulphur minerale confortans, mixed with a bezoardic or digestive.

Among the various compositions of bezoardic powders, I ordered the following very frequently to be taken, as much as would lie on the point of a knife, especially if my patients had a terror upon them, or by reason of melancholy found any alteration about them. To others I ordered it as a preservative, to be taken in a morning mixed with beer. ℞. Rad. tormentill. scorzoneræ, petasitid. symphit. aristol. rot. lign. guaiac. colubrin. a. ʒij. rad. curcum. zedoar. a. ʒss. myrrh. rubr. ʒj. corall. alb. conch. mar. a. ʒiss. succin. flav. terr. sigillat. rubr. et alb. carnis viper. oculor. canc. bol. armen. corn. cerv. ust. a. ℥ss. antim. diaphor. ʒvi. nitr. anodyn. min. ʒij. m. dosis ℈j. ad ʒj.

Those who were obliged to be frequently or constantly with their patients, made use of a vinegar prepared for that purpose, which they took inwardly, and which they likewise smelled to. Others made use of rotulæ sublinguales, of which also there are many preparations. I preferred to them the placentulæ senectutis, which Dr. Sylvius Bocco in his curious observations has described. These, when I visited my patients, I generally held in my mouth. To take away the ill scent, which was very frequent in the patient's chambers, we smoked the rooms with juniper, vinegar, powders, candles, and especially with gunpowder; but this last must be carefully made use of. I generally had red myrrh and salt-petre boiled in vinegar, and ordered it to be poured on a hot tile, to smoke the room with: and this was done at little expence, and with great benefit.

Evacuations were likewise by some reckoned among the preservatives. But as that common proverb, vis vi est repellenda, is not always allowable, but often hurtful and dangerous in the practice of physic; so likewise these violent means occasioned a great deal of harm. A gentle laxative taken once in a fortnight, or three or four weeks, according to the disposition of the body, could not be amiss, but it must be done with great care, and keeping within doors.

But much more consideration and regard ought to be had in respect to vomiting and bleeding. If we have regard to the indications, we cannot resolve upon either of these without danger. Bleeding is indeed generally allowed, if the patient is plethoric, and there is blood enough. Sometimes bleeding in the plague has been reckoned a thing indifferent, which could do neither good nor harm; but then the indications were very obscure, or wholly fallacious; yet I think it were better omitted than ordered. The question is not, whether bleeding be generally pernicious, but whether it can be allowed to be a preservative in time of a contagion; because, as I have said before, it is absolutely dangerous and pernicious to such as are infected, or disposed to infection.

As for vomiting, it is not only of less use than bleeding, but much more dangerous. I only speak here of vomits, when the contagion is at the height. For it is observed, that the contagious venom is not to be searched for in the stomach; because it finds other ways, through which it may insinuate itself into the body. And though some persons should receive some of this venom with their aliment into their stomach; yet by reason of its subtilty, it does not long stay there, but presently seizes and attacks some more noble part. From whence it may be concluded, that evacuations without great necessity are absolutely useless. It is the judgment of the most learned and experienced practitioners, that vomits in their nature and power differ little from ordinary poison. How is it then possible that one poison should qualify the other? If they be considered as auxiliary remedies, let them be administered with the greatest caution, especially at a time when they choak and kill so many symptomatically, If an argument a posteriori may be allowed conclusive, then, as we said before of bleeding, we may also of vomits, that they prognosticate an unhappy event to the disease, and that they have not the least use in preserving from contagion. It happens sometimes that a man overcharges and distends himself with victuals and drink, and that crudities lie in the stomach, in which cases there are plain indications for the use of vomiting; to prevent crudities fouling the blood, and then it is not dangerous.

As to remedies found most useful in the cure of the disease, some may deserve to be noticed here. I never advised my patients to much motion, but rather to keep in bed and at quiet, unless when out of fear they would unseasonably give themselves up to too much rest, and then it was best to make them stir about a little, that being the way to prevent melancholy affecting their minds; for a brisk and easy mind is very good, but a dissolute, passionate, fearful, and uneasy temper, very pernicious. As soon as ever we were satisfied our patients had the plague, we absolutely avoided purging, vomiting, and bleeding; for generally those to whom they were administered, though they seemed ever so robust, did not escape death.

In my practice, I took this method in curing my patients: first, to extinguish the pestilential venom; secondly, to mitigate and assuage the violent symptoms; and thirdly, to preserve as much as possible the natural strength. My first intention was undertaken with bezoardics and sudorifics; as tinctura bezoardica Michaelis, mixtura simplex, spiritus digestivus Schroder. spirit. bezoard. Bussii, essentia theriacalis cum et sine camph. essentia lignor. electuar. diascord. Fracast. pulvis bezoard. Wagner. pulv. bezoard. Sennert. bezoard. mineral. antihect. Poteri. antim. diaph. specific. cephal. Michaelis, and the like.

The before-mentioned essentia præservativa I ordered in the beginning of the distemper to be given 6 times in 24 hours, from 25 to 75 drops, according to the strength and age of the patient; and every 12th hour the patient took one of the bezoardic powders; which were taken with a great deal of benefit. I likewise found great benefit in using the bezoardicum Sennert. with the bezoard. Wagneri; or else this, or the former, intermixed with the specific. cephalic. Mich. or bezoard. mineral. antihect. Poteri with the addition of succin. alb. and myrrha rubra, flor. sulph. lac sulph. nitrum antimon. sulphur mineral. and according to the circumstances I added sometimes crocus orient. The electuarium diascord. Fracastor. dissolved in aqua destill. millefol. sambuci, tormentill. scorzoneræ, scordii, cardui bened. angelicæ, pimpinellæ, rutæ, galegæ, &c. with the addition of the essentia citri liquida, and made pleasant with a syrup of that kind, makes a good sudorific and alexipharmic potion. The following mixture I have prescribed with great benefit to my patients.

℞. Aq. scorzon. scordii, tormentillæ, millefol. aa. ℥j. aceti bezoard. ℥ss. essent. lignor. ʒij. mixtur. simpl. ʒj. elixir. proprietat. ʒss. syr. de symphit. Fernel. ʒij. f. mixtura. for three doses. On taking this mixture, the patient was forced to sweat, and not to drink any thing cold; and after the sweat he took a cordial powder, prepared much in the same manner as the species cordiales et solares alexipharmacæ Zwelferi, or my sulphur minerale mixed with magister. margar. lap. hyacinth. smaragd. bezoard. orient. and other cordials. Whenever my patients were very much inclined to be sleepy and doze much, I then added to the former medicines volatile salts. But if on the contrary I perceived any colliquations, then I used to add crocus orient. theriaca cœlestis, and other paregorics, but with caution. If convulsions and spasms happened, then cinnabarine medicines were successfully applied. In hæmorrhages the martialia and the tinctura antimonii were very beneficial.

When the patient is first seized, his limbs are quite stiff, and as it were benumbed with cold by the violent attack of a cold ague-like fit, in which case we presently applied hot bags filled with oats, or rubbed him well with warm cloths, to bring him to warmth again. Against restlessness and deliriums, anodynes signified nothing; but we took the fol. rutæ, salicis, plantaginis, scordii, serpylli, and the like, mixed with salt and moistened with rose-vinegar, which being clapped warm about the head, did good to a great many. Vertigoes and pains in the head were often mitigated with the ordinary species pro cucupha disp. Berol. of which one part, mixed with a quarter part of nitrum antimoniale, and fol. rutæ. portulacæ et endiviæ, of each half a part; is to be tied about the head. Against dozing and sleepiness, a blister was applied in the neck, and snuff blowed up the nose, especially some quickening salts or spirits rubbed in the nostrils. Violent diarrhœas were indications of outward convulsions, and were frequently stopped by a topic medicine of theriaca, mithridate, or orvietan, mixed with vinegar, and spread upon leather or linen, and applied warm to the navel. Bleeding at the nose was one of the most dangerous symptoms of any, and could not by any mere outward application be stopped; and if it was stopped, it was of no service, especially if it happened the third day. Yet I found it sometimes very useful, to put lint moistened with vinegar of roses into the nostrils, and to tie a small bolster with the lapis hæmatitis moistened with vinegar on the hollow part just above the nose between the eyebrows; or to put rue and red roses, stamped and sprinkled with rasberry-vinegar, between a double linen, and so applied to the neck.

When the throat was very dry, we used barley water, and sometimes added sal prunell. and sweetened it with syrup of mulberries, granats, or the like, and sometimes also used it without any syrup.

In such as suddenly lost their strength, and had their spirits dejected, we used all manner of cephalics and cardiacs, and whatever we thought would strengthen the patient; and if he desired a glass of good rhenish, we never denied it him; but this appetite seldom happened till the patient was past recovery. Otherwise we used to give him cordial juleps, which were mixed with bezoardics, if the circumstances of the disease so required; or the confectio de hyacintho et alkermes mixed with aqua cerasor. nigr. flor. boraginis, lilior. convall. rubi idæi, &c. To which were sometimes added the elix. citri, granator. acetositatis citri, cydoniorum, mororum, &c. Also ptisans and milk-whey; as also emulsions made of almonds, and semen pæoniæ, cardui benedicti, aquilegiæ, &c. Hartshorn jellies made with barley water, French or rhenish wine, with the addition of syrup. acetositatis citri, diamori, ribium, &c. All which do very much refresh the patient. Among the powders, the following are likewise of use: lapides pretiosi, corallia rubra, unicornu verum, lapis bezoard. orient. margarit. orient. magisterium margarit. orient. magist. corallorium, &c.: and, what is more than all the rest, the sulphur minerale, if well prepared, as it has been by the famous Basilius Valentinus and Franciscus de la Boe Sylvius.

Besides these, epithemata were applied to the head, heart, and pulse. I must confess the common people ascribed wonderful effects to these medicines, and they were therefore often allowed more than necessity required, merely for their satisfaction. Neither yet can I say that they were of no efficacy; but that by the pleasantness of their smell they had a great power to refresh and recreate the spirits, and so relieved the patient. Heating and cooling applications had both their use, and were applied as the physicians thought proper, and the symptoms would permit. As the aqua carbunculi, cephalica carol. v. apoplectica, odorifera reginæ Hungariæ, lilior. convall. &c. mixed with the confectio alkermes, species cordial. essent. citri sicca, essent. ambræ sicca; and the acetum bezoardicum, rubi idæi, sambuci, calendulæ, with red roses, white lilies, pœony, white mustard, saffron, theriaca, and mithridate, species diamargarit, frigid. diatragacanth. frigid. &c. And because in composing such topic medicines, one can hardly be much out of the way, as to their quantity and form, therefore nurses may as well mix them as an apothecary, if they have but the medicines. The common people made use of theriaca, bay-berries, bitter almonds, thyme, pepperwort, water germander, vipers-grass, or the like, mixed with crumbs of bread and salt, a little camphire, or vinegar; in which was stamped rue, plantain, houseleek, parsley roots; as also leven, and the like.

To mollify the buboes, some made use of cakes of black pepper, mixed with vinegar and oil of roses, or of white lilies; or they mixed these cakes with honey, figs, and wheat-flour, and applied them warm as a poultice; others took chamomile flowers, fresh butter, and linseed. To draw the buboes it was very common to apply roasted onions, roasted figs, or leven, mixed with vinegar, white mustard seed, and powder of Spanish flies; and afterwards they used a plaster of oil of turpentine, tar, and yellow wax. Some very much recommended the emplastrum manus dei Le Mort; others, the emplastrum diachylon cum gummis Vigonis, by itself, or mixed with the oxycroceum, to mollify the buboes. Some, by means of cupping glasses, or blisters, or even by incisions, happily took them out, and afterwards cured themselves. But the carbuncles required more pains, care, and caution. In the beginning, an emplastrum defensivum applied about the carbuncle to prevent its further progress, did much good; afterwards, to ease the pain, and to bring it to a separation, they pursued it with caustics, scarifications, unguentum Ægyptiacum, unguentum basilicon. simplex. &c. or what else the surgeon thought convenient. At last they applied digestives to cleanse it, and a healing plaster.

Carbuncles were soon cured by the following method. I ordered the part round the carbuncle, as yet untouched, to be well washed with aqua calcis; and then put the emplastrum apostolicum Zwelferi over the carbuncle; and over that again a poultice clapped between two linen rags, and applied three times a day, made of chamomile flowers, linseed, flowers of beans and barley, boiled in milk to a proper consistence. This drew forth the carbuncle admirably well; so that the eschar softened and began to sweat, and also came soon to separation. At last the ointment or digestive only, with a defensive plaster, cleansed it, and the cure was perfected with emplastrum Voegedingianum.

An Account of the Roman Eagles. By Dr. Musgrave. N° 337, art. 11, p. 145. *Abstracted from the Latin.*

The eagle was the military ensign of a whole legion, glittering, and fixed to the head of a long hasta or spear; which being raised aloft by the aquilifer, or eagle-bearer, and beheld by the soldiers, especially those of the first cohort, consisting of 1105 foot, and 132 horses, in all 1237, as appears from Vegetius, lib. 2, c. 6, it might be of use to such a number of men in keeping their ranks, observing their distances, and consequently, in preserving their order of battle: therefore to answer this purpose, the eagle must needs be large, and stand high. From coins, &c. it would seem that the eagle and spear together, were about 8 feet high, of which the eagle alone was about 8 inches.

Dr. Musgrave supposes, that the Roman eagle was made either of wood, cork, leather, or paper, or some such light substance, and either gilt, or laid over, with plates of gold or silver, with expanded wings, and sometimes with thunder under its talons. And he thinks that the matter is best explained by Ant. Augustinus, Dial. iii. Antiq. in Cappadociâ, who says that each legion carried a carved eagle on the end of a spear or hasta, which was incrusted or overlaid with plates of gold or silver.

An Experiment on the Proportions of the Ascent of Spirit of Wine between two Glass Planes, whose Surfaces were placed at certain different Distances from each other. By the late Mr. Fr. Hauksbee, F.R.S. N° 337, art. 12, p. 151.

I took two clean glass planes, about 6 inches long, and 2 inches broad, and separated them at each end by 32 pieces of brass plate, whose thickness, when laid on one another, and pressed together by screws, made a distance between the planes equal to $\frac{1}{16}$ of an inch. Being thus prepared, I plunged one end of them into some tinged spirit of wine: and after wetting the inward surfaces of the planes with it, by declining the upper end, I set them upright; and found that the surface of the wine between them remained higher than the surface of the wine on their outsides, by about $\frac{1}{6}$ of an inch. After this, I reduced the distances of the planes to half the former, by taking away 16 of the brass plates from each end of them; then being plunged into the liquid, and used in all respects as before, I found the spirit to stand between the planes just double the height it stood at in the former trial. Again removing half the number of brass pieces, leaving only 8 of them at each end, and using them as before, the spirit rose between them to a height just double to that in the foregoing trial. Thus from 8 I reduced them to 4 pieces at each end of the planes; then again the spirit was seen to remain suspended at twice the last observed height. In this last trial the planes were distant from each other only $\frac{1}{128}$ of an inch: nearer than that I could make no certain measure; but I suppose the forecited experiments are sufficient to ground a calculation on, even of the nearest approximations.

Some further Experiments, showing the Ascent of Water between two Glass Planes in an Hyperbolic Curve. By the late Mr. Fr. Hauksbee, F.R.S. N° 337, art. 13, p. 153.

The figure of the hyperbolic curve, formed by the ascending of water between two square glass planes, as mentioned Philos. Trans. N° 336, gave me occasion to make some further inquiries; and by many experiments I find, that the same curve holds in all directions of the planes, the asymptotes being always the one the surface of the water, and the other a line drawn along the touching sides. Thus, when the touching sides were plunged under the surface of the water, and the angle c was depressed and made to remain lower than the angle a, as in fig. 1, pl. 2, then would be produced such an oblique curve as shown in that figure. In all the several schemes, represented fig. 1, 2, 3, 4, 5, 6, 7, 8, ab represents the surface of the water on the outside of the planes, and ac the touching sides of the same. Now, though the curve dd rises between the planes in such an obliquity, yet it conforms in its figure to the asymptotes, viz. ab the surface of the water, and ac the touching sides of the planes; for supposing the asymptote ac to be continued, as in the pricked line, till it surmounts the surface of the water to such a height, or suppose the planes extended in the same manner, then would the water remain between them in the appearance of the pricked lines, being at all distances from the axis of the curve, equal in respect to the asymptotes; and so of all the rest of the curves, which are the result of the several angles, made by the touching sides of the planes. Now when the touching sides were placed upwards, parallel to the surface of the water, as in fig. 7, and plunged wholly under the same, then on lifting them up, in the same position, till the weight of the water between the planes overbalanced the power of their attraction, by this means a curve from each side of the planes would open itself, and meet each other in the middle, as represented in the aforesaid figure; where they would unite, and make a figure as joined by the pricked lines, being wider in the middle than towards the sides of the planes. And it is very remarkable, that this curve would always break out between the planes, at an equal distance, between the touching sides and the surface of the water.

The same figure is likewise produced between two round glass planes, as in fig. 8, the asymptotes being the same as the former; that is, the one the surface of the water, the other a tangent drawn from the touching point, parallel to a tangent drawn from the open or opposite part of the planes, being at right angles with a line drawn through the same. These experiments I find to answer the same in vacuo, as in the open air; so that that element has nothing to do in this extraordinary appearance.

The planes made use of in the foregoing experiments, were about 7 inches square, opened on one side to an angle of about 20 degrees; the round planes were near 3 inches in diameter.

A further Account of the ascending of Drops of Spirit of Wine between two Glass Planes $20\frac{1}{2}$ *Inches long; with a Table of the Distances from the touching Ends, and the Angles of Elevation. By the late Mr. Fr. Hauksbee, F.R.S.* N° 337, art. 14, p. 155.

The spirit of wine did not move so nimbly between the planes, as oil of oranges, which gave me the liberty to observe the angles with more deliberation. The limb, on which the planes were laid, moved in the centre of a quadrant of 4 feet radius; the magnitude of which gave me the opportunity of measuring the angles with greater accuracy: but the distance between the drop on the planes, and the graduations on the quadrant, made it a little difficult to observe them both at once. Yet I believe the following tables may be depended on, to be as true as the nature of such an experiment is capable of. I have formerly given a particular account of the manner of making this experiment, in the Phil. Trans. N° 334. These tables are calculated from the touching ends of the planes; and it is to be observed, that in the table where the planes were opened only to an angle of 10′, that I could not come nearer than 4 inches of the touching ends in my observations: but so far as I could go, seems to be much in the same proportion (as I have often observed in the course of these experiments) with the table where the planes were opened to an angle of 18′.

The Planes opened to an Angle of 18′.

Distances in Inches from the touching Ends.	Angle of Elevation.
18½	0° 45′
16½	0 55
14½	1 05
12½	1 20
10½	1 30
9½	1 40
8½	2 00
7½	2 30
6½	3 20
5½	4 25

The Planes opened to an Angle of 18′.

Distances in Inches from the touching Ends.	Angle of Elevation.
4½	6° 00′
4	7 23
3¾	8 40
3½	9 25
3¼	10 30
3	12 40
2¾	15 00
2½	18 50
2¼	23 25
2	30 00

The Planes opened to an Angle of 10′.

Distances in Inches from the touching End.	Angle of Elevation.
18½	1° 30′
16½	1 50
14½	2 10
12½	2 40
10½	3 10
9½	3 30
8½	4 00
7½	5 05
6½	7 40
5½	10 50
4½	14 00
4	18 00

Inscriptio Tarraconensis: cum Commentario Guil. Musgrave, M. D. Coll. Med. et Societ. Reg. Lond. Socii. N° 337, art. 15, p. 157.

Some further Microscopical Observations on the Animalcula found on Duckweed, &c. By Mr. Leuwenhoeck. N° 337, art. 16, p. 160.

In N° 295*, I took notice of the surprising figure of an animalculum, fixed in a little scabbard or sheath, fastened to some of the small green weeds, found in ditches of water. And as often as I have viewed these animalcula, and showed them to others, we could not satisfy ourselves with looking on such surprising objects; and the more, because we could not conceive how so strange a motion, as they all had, could be performed; as also what should be the use of such a motion. For when we observe other animals, that are endued with

* Page 175, vol. v. of this Abridgment.

motion, moving any part of their body, we presently conclude, that these parts are not formed in vain; and consequently we may conclude, that the wheel-like motion of those animalcula is useful to their bodies, though we cannot tell exactly how.

The latter end of July, and beginning of August, I caused some of those green weeds, commonly called duck-weed, to be taken out of the water, that runs with a gentle stream through the town (Delft), for the pleasure of observing these animalcula, with others of several sorts, that were fastened to the duck-weed, or ran about upon it. Among others, I have found some animalcula, whose sheaths, at the extreme part, were a little thicker than a head hair, and composed of small globules, were very easy to be distinguished.

I viewed one of these animalcula a good while together, and observed several times, one after another, that when the animalculum thrusts its body out of the sheath, or case, and that the wheel-like or indented particles moved in a circle, at the same time, out of a clear and transparent place, a little round particle appeared, which, without nicely viewing, could hardly be perceived; which particle growing larger, moved with great swiftness, as it were, about its own axis, and continued without any alteration in its place, till the animalculum had drawn part of its body back into its sheath; in doing which, it placed the said round particle on the edge of its sheath, which thus became augmented with a round globule: and whereas the animalculum had placed the said globule on the east part of its sheath, another time it fixed it on the south or north side; by which means the sheath was regularly increased on all sides.

Having further, and with great exactness, viewed the circulating indented wheel-work, I observed that it caused an exceedingly great motion in the water about it; by which means many very small particles, which were only visible through the microscope, were wafted to the said animalculum, and others were driven away. The animalculum made use of some of these particles, that were thus drawn to it by its circulating instrument, for food and nourishment; and other particles that were thus drawn to it, were with great nimbleness driven away, and, as if rejected by the animalculum: from whence I inferred, that those particles which were thus thrust away, were not proper for its food. From this discovery we may conclude, that since this kind of animalculum cannot move from place to place in the water, nor consequently pursue its food, as other creatures do, that are endued with motion, being fastened by the tail or other parts of the body, it must necessarily be provided with such instruments as are fit to move the water, and by that means come at the particles floating in it, which serve for the nourishment, increase, and defence of its body.

If we observe those animalcula, which, with their long tails, are fastened to some part of the weed, as we have discovered a great many on the small roots of the duck-weed; we may observe, that they do not only make a circular motion with the extreme part of their bodies, which motion, in proportion to the said part, is very great; but they can likewise draw in their tail, and that with a very quick motion; by which means they can move the water out of its place, when they stretch their tails out again, and so bringing fresh water under them, they procure new food.

I likewise observed a very few animalcula, whose bodies were short and thick, and much larger than those other animalcula that lodged themselves in a sheath, and were fastened by their tail, or extreme parts, to the little roots of the duck-weed: and though these short and thick animalcula could move from place to place, yet they also had a circular motion in the fore part of their bodies. From whence I concluded, that those motions served some other purposes than only to draw their food to them. On further considering what could be the use of these indented wheel-works, which are so like the indented wheels of a clock, or watch, I must own that they are very necessary to produce a great motion in the water; for, were it a round and smooth wheel, it would cause but a very small motion; whereas now, each tooth in the said wheel or circle produces a great motion, in comparison of what a smooth and plain wheel would do. Whence appears the surprising order in the formation of such small creatures, which are not to be perceived by the naked eye.

In the beginning of August being in a garden that had a pond full of fish, on the water of which I observed floating a thin scum, of a greenish colour, though there was nothing green in the water; which seemed strange, because at other times I had observed that the water of the pond was very clear, as well as the stream which fed it; and I was told that when it rained, the said scum disappeared. I took a little piece of a wooden lath, and drawing it over the superficies of the water, I placed a small drop of the water on a green wine glass, and viewing the same with the microscope, I discovered an inexpressible number of animalcula, so exceedingly small, that they almost escaped my sight through the microscope: there were also several sorts of larger animalcula, mixed with a great number of little air bubbles, of an exceeding smallness.

I cannot forbear acquainting you, that I have lately observed in the flesh of a whale, and the same is to be said of an ox, and even down to a mouse, that its fibres, though 16 times smaller than a hair of my beard, yet are surrounded or involved in little membranes, in such manner, that the fleshy particles do not touch each other.

The Case of a pregnant Woman, who, recovering of the Small-Pox, was afterwards delivered of a dead Child full of the Pustules of that Distemper. By the Rev. Mr. W. Derham, F. R. S. N° 337, art. 17, p. 165.

In my neighbouring parish was a pregnant woman, who was pretty well recovered from the small-pox, so that she was able to take something to purge her after it: and on August 30, she took a purge, which did not work; and on September the 1st, another purge, which gave her only a stool or two. On which, September the 3d, she took another stronger purge, that worked so violently upwards and downwards, that she fell into faintings and convulsions: about which time I suppose her child died, but of which she was not delivered till September the 8th. The child was a female, and in appearance well made, lusty and strong. At its delivery, the midwife judged it had been dead 5 or 6 days; so that its belly was burst, and the bowels came out, and the whole body tending to putrefaction. But what seemed most remarkable, the child was so full of the small-pox, that hardly a pin's head could be put between the blisters, which were very plump, and full of matter, like the pustules of an adult, when the small-pox is at the height, only a little depressed in the middle. But as full as the child was, the mother had but few, and very favourably; the child, I suppose, undergoing that which would have been more severe on the mother.

Hence I would inquire, 1. When a woman in pregnancy has the small-pox, whether it be likely that the child should be in danger of taking and having that distemper after its birth? 2. Whether the above-mentioned child had the small-pox at the very same time with the mother, and not rather afterwards, by reason the child was full of it, after the mother was well recovered?

For my part, I am apt to think, that the great flux and tendency of the blood to the child, in the womb, might draw in the humour, and prevent the greater eruption of the small-pox in the mother; and that for want of a due expence of it, the remainder afterwards broke out in the child, and that the child really had it after the mother; nature making the discharges on the child, which were not completed on the mother.*

Several Observations in Natural History, made at North-Bierley in Yorkshire. By Dr. Richard Richardson. N° 337, art. 18, p. 167.

One John Worsnape, of North Bierley, a poor boy, lived till he was 17 years of age, and never made water, and yet was very healthy, vigorous, and active. He had constantly a diarrhœa on him, but without much uneasiness.

* Other instances of small pox communicated from the mother to the fœtus in utero are related by Dr. Watson, Mr. J. Hunter, and Dr. Wright, in vols. 46, 70, and 71, of the Phil. Trans.

The obstruction must have been in his kidneys, for he never had any inclination to make water. The serous part of the blood, which should have been thrown off by urine, was discharged by the cœliac and mesenteric arteries, by the mediation of the glands, into the guts. He died of a fever.

The second case, is an instance of old age in Martha Waterhouse and Hester Jager, two sisters, both born in the township of North-Bierley. Martha, who had been married, died in 1711, in the 104th year of her age; and Hester this present year 1713, in the 107th year of her age. I should not have taken notice of their ages separately, it falling so far short of several instances which have happened in this county; but jointly I do not remember any that have come up to them.

From the longevity of human kind, I shall proceed to that of fish out of their proper element. About 6 years since, great quantities of tench were taken in a pond at Craven, and sent to the neighbouring markets. The fish were taken on Monday towards night, and some brought to Bradford on Tuesday about the same hour; and not being frequent in our markets, 6 of them were sent hither to me on Wednesday. I not being at home, the basket was set upon the kitchen table, not far from a good fire; where it continued till Thursday morning, the servants not knowing what was in it. On opening the basket, and looking upon the fish, I thought the eyes of some of them looked clear: I put two of them into a pail of water, and in less than 2 hours time they swam very lively in the water. The remaining four showing no signs of life, I put them into the same pail, and before night they all swam about in it.

Burbolts* being a fish not frequently met with in the southern rivers of England, are often found in this county, especially in slow rivers and standing waters, as in the river Foss in York, and also in the Derwent; but in no place more frequent, than in the fen ditches of the levels, about four miles from Doncaster.

I have several times seen plenty of small trouts caught in the mountainous lakes of North Wales by angling; and have, with no small admiration, considered the difficult access to these places, where a good footman can scarcely climb up to them. That these lakes are yearly supplied from the brooks at the bottom of the mountains I do not at all doubt, especially in spawning time, when the trouts endeavour to surmount all difficulties, by passing up the small rivulets, to deposit their spawn, for the preservation of their species, where it is the most secure from the violence of other fishes, and there by accident fall

* Gadus Lota. Linn.

into these natural ponds, where they continue all summer; no person having yet observed trouts to breed in ponds. Not only the trouts, taken in these mountainous lakes, are small, but also the charrs taken as they ascend the small river, out of the great lakes nigh Lhan Berys, to deposit their spawn in the sands there. These very rarely exceed a fresh herring in magnitude, and they are in no respect different from those taken in Winander-Meer, excepting in magnitude, where it is no rare thing to meet with them of 2 pounds weight, and upwards. This smallness in the fish I have some times thought to proceed from the coldness of the water, these lakes being supplied with snow-water from the mountains 8 months in 12. The minera of vitriol and alum, being often met with, in the hills through which some of the water must drain, perhaps does not a little contribute to the roughness and coldness of the water. The contrary we find in our waters that run through the lime-stone rocks, where no rough salts are found; the trouts there are large and fat. An instance of which we find in the trouts in Malham Tarr, in Craven, near Setle, where they are frequently found 2 feet long.

I must also take leave to correct one mistake in Mr. Ray's Synopsis Quadruped. &c. p. 195, where he says, that mustela vulgaris is called here a foumart or fitchet. Putorius is called here a foumart, quasi foul mart, or stinking mart, in opposition to the martes which emit a musky smell, and are often met with in our woods, and taken by the hunters in snows.

The ermin* is not unfrequently met with here in winter, and when they appear, are thought to presage snow. I should not here have taken notice of it, it being also met with in most counties of England, but that I have had an opportunity, in two or three instances, of observing the time of its changes. It begins to change its colour from brown to white, about the beginning of November. I had one of them brought me about November two years since, when I first observed this change. I have seen one or two of them, that in the beginning of March were changing from white to brown. Quere, whether these animals do not always continue white in the more northern parts of the world?

The nut-hatch,† or nut-jobber, is not frequently to be met with in the south, yet is so common with us, that I have sometimes seen 6 or 7 of them in one day in my own woods. This must be the bird that Dr. Plot, in his Nat. Hist. of Oxfordshire, calls a wood-cracker, and takes to be an undescribed bird. I have with much pleasure often observed these birds to crack nuts, which they do with great dexterity. I ordered one of my servants, that was with me in a

* Mustela erminea. Linn. *Gmel.* † Sitta Europæa. Linn.

wood last Christmas, to observe from whence she fetched her provision; which he soon discovered in a hollow tree, and cutting the place open, brought from thence several pints of very choice nuts.

I met with a nest of the regulus cristatus,* in a thick thorn hedge, in my own orchard, which was built round, and a small hole at the side; the outside was green moss, the inside hair and feathers, not much unlike that of the common wren. The eggs were small and white, with many brown spots on them. The note of the cock is very agreeable, not much unlike some of the parus kind. I do not remember that I have seen any of these birds in summer before.

Cochlea pomatia majoredul is Gesneri.† I never met with it in the north; but I found it in plenty last year about the middle of May, in Stunsfield fields, among the briers and brakes, especially near the famous Roman pavement.

Epistola Nicolai Facii, R. S. L. S. ad Fratrem Joh. Christoph. Facium dict. Soc. Sod. qua vindicat Solutionem suam Problematis de Inveniendo Solido Rotundo seu Tereti in quod Minima fiat Resistentia. N° 337, art. 19, p. 172.

[This intricate mathematical paper, of a controversial nature, could be of no use here, unaccompanied by the others, to which it refers, and which were published elsewhere.

About this time, it was very much the fashion for the English and the continental mathematicians to propose intricate problems to each other, as trials of skill, which often ended in violent controversies, and bitter aspersions. Mr. Nicolas Facio, though a native of Switzerland, from long residence in England, took part with the mathematicans of this country, and was of course obnoxious to those of the continent. This gentleman having published a small tract, containing a solution of the problem of finding the solid of least resistance, it was criticised in the Leipsic acts by Mr. John Bernouilli, and explained by the author in the same work, and here in this paper still further explained and defended. But which is now quite uninteresting, both in its own nature, and from the insulated situation in which it is placed.]

Botanicum Hortense 3.—*Giving an Account of divers Rare Plants, observed this Summer, A. D.* 1713, *in several curious Gardens about London, and particularly in the Society of Apothecaries Physic Garden at Chelsea. By James Petiver, F. R. S.* N° 337, art. 20, p. 177.

[Contains a short account and character of 161 of those rare plants, in a con-

* Motacilla Regulus. Linn. † Helix Pomatia. Linn.

tinuation of the same subject begun, and prosecuted in some former papers in these Transactions.

A short Account of some Swedish Minerals, &c. sent from Mr. Angestein, Overseer of the King of Sweden's Mines, to Mr. James Petiver, Apothecary, and F.R.S. N° 337, art. 21, p. 222.

1. Bitumen fossile, coagulatum, in Ferrifodina Betzberg apud Suecos. This is very light, black, and shining, like pitch, or Canel-coal; in taste insipid, brittle and not clammy.

2. Minera Ferri Betzbergæ. A blackish mineral, full of small glittering sandy particles, which easily break and crumble small like black sand; it is ponderous, and without any drossy mixture.

3. Minera Martis Betzbergæ. The outside lumps are smooth and shining; its inside of finer particles than the last, but, when broken, more shining, large and flaky.

4. Minera Martis Betzbergæ, carbonis facie.—The outside smooth and somewhat shining like pit-coal, the inside more gray and glittering.

5. Minera Martis ditissima, fodinæ Danemoræ Sueciæ.—This resembles a slate-coal of a lead colour, heavy, but neither shining nor glittering, except a little brassy in the interstices, which are but few, and those irregular: this had a sparry vein through it, about the thickness of a quarter of an inch.

6. Rubinus globulosus, è Fodina Garpenberg Sueciæ.—These are near as large as hazel nuts; the outside in many places transparent, but broken and rugged; they seem more ponderous than a clear ruby of the same size.

7. Minera Veneris seu Cupri, Garpenbergæ.—Its exterior face brassy and shining, with some few spaces of deep copper, with blue in the centre; the interstices are pale brass in some places, and where it is fresh broken into the heart of the ore, it is of a glorious lustre; in some places it is coated with a white or discoloured spar.

8. Minera Cupri è fodina Fahlkunensi Sueciæ.—This is mostly composed of pale brassy micæ, with a small mixture of a deeper colour, and near a quarter part of black sprinkles, somewhat shining.

9. Minera Cupri ditissima Lazur dicta, è fodina Linsnadiensi provinciæ Heerdacensis Sueciæ.—The ore is very like N° 7; it is coated with yellowish spar, mixed with a palish white.

10. Minera Argenti-Plumbea è fodina Hollefarsen Sueciæ.—Very like our Mendip lead ore, full of large sparkling flakes or micæ.

11. Certum genus Aluminis plumosi cum Minera Plumbi intermixtum, e fodina Sahlberg Sueciæ.—The amiant part is of a light gray or lead colour,

composed of fine long brittle filaments; the outsides and hollows are filled with very white short and brittle spar.

12. Matrix Mineræ Argenteæ, Sahlbergæ.—This is a short irregular white shining spar, much like the last, which lies with the amiantus; it easily breaks into small pieces with the hammer.

13. Minera Lunæ et Saturni è Sahlberg.—The sparks of this are very bright and shining, with smaller micæ than N° 10; in it are interspersed some little bits of white spar.

14. Minera Lunæ et Saturni Sahlbergæ unâ cum Matrice.—This ore has a particular face, being composed of many ruinous angular columns lying confusedly; some long and crooked, others short; among them are some like glass, greenish and transparent; these very easily fall to pieces with a light blow of a hammer.

15. Minera Lunæ, Arsenici et Saturni ex Argenti fodina Sahlberg.—The flakes of these are pretty large, like N° 10, and very glittering.

16. Cuprum præcipitatum, fodinæ Schilou Sueciæ.—Its outer coat is rusty, and resembles an iron stone or clay, under which are some thin strata of a dark or bluish hue; the centre of an iron brown, full of small copper micæ, which in some places are of a refulgent fiery lustre.

17. Minera Cupri rarissima è Schilou.—This resembles colcothar, but is of a brighter red, and very weighty; it seems to have some very small shining particles in it, and in some parts of its outsides it has a sort of sullen greenish wood-like rust; but it is not very heavy.

Some Observations on the Mechanic Arts and Physic of the Indians. By Father Papin. N° 337, art. 22, p. 225.

This country (the East Indies) furnishes more materials for mechanic arts and sciences, than any that I know of. The artizans here have great skill and dexterity: they excel particularly in making linen cloth; which is of such fineness, that very long and broad pieces of it may easily be drawn through a small ring.

If you tear a piece of muslin into two pieces, and give it to one of their fine drawers to set it together again; it will be impossible for you to discover where it is joined, though you mark it on purpose to know it. They will place together so artificially the pieces of glass or China ware, that one cannot perceive it ever was broken. Their embroiderers work in filigree very curiously: they imitate exactly any work made in Europe, though the implement they make use of, and all their other utensils, do not cost them more than a crown. The looms used by their weavers cost no more: with these they sit in their courts and yards, or on the side of the highway, and work those fine stuffs that are so highly esteemed over all the world.

They have here no need of wine to make aqua vitæ; but make it of a syrup, sugar, some certain sorts of barks, and raisins; it burns better, and is stronger than that made in Europe. They paint flowers, and gild very finely on glass. I was surprised to see their vessels, which they use to cool water in, which are not thicker than two leaves of paper pasted together. Their watermen row after a different manner from ours: they move the oar with their feet, and their hands serve instead of the hypomochlion, or roller on which it turns. The liquor which their painters use does not any way lose its colour, nor is it tarnished by lee.

The husbandmen in Europe prick their oxen with a goad, to make them go faster; but here they only twist or wring their tails. These beasts are very docile: they teach them to lie down, and rise up, when they take up or lay down their burdens. They make use of a kind of hand mill to break their sugar canes, which does not cost them above the value of 10d. The person that grinds, works and fashions the stone himself with lac and emery.

Their masons will pave the largest rooms with a sort of cement made of brick dust and lime, so that it shall seem to be but one stone, and is much harder than gravel. I saw them make a sort of pent-house, that was 40 feet long, 8 feet broad, and 5 or 6 inches thick; which they raised up in my presence, and fixed it to the wall on one side only, without putting any prop under it to support it.

Their pilots take the altitude, or latitude of places, with a cord that has several knots in it. They put one end of the cord between their teeth, and by means of a piece of wood fixed to it, having a hole through it, they easily observe the tail of ursa minor, which is commonly called the polar star, or north pole. Their lime is usually made of sea shells: that which is made of snail shells serves to whiten their houses; and that which is made of stones they chaw, with the leaves of betel. I have seen some of them that would take as much of it in a day as the quantity of an egg.

They make their butter in the first pot that comes to hand: they cleave a stick into four quarters at one end, and stretching them out asunder in proportion to the size of the pot that contains the milk, they turn the stick round different ways, backwards and forwards, by means of a cord twisted about it; and by this means in a short time make the butter. Those that sell butter, have the art of making it pass for fresh, when it is old and rank. To do this, they melt it, and pour upon it sour curdly milk; and 8 hours after, they take it out in lumps, and strain it through a cloth for sale.

Their chemists make use of the first pot they meet with, to revive cinnabar, and other preparations of mercury, which they do after a very simple manner. They easily reduce all metals into a powder. They set a great value on talc and brass, which consume, as they say, all viscous humours, and remove the most stubborn obstructions.

Their physicians are more cautious in using sulphur, than they are in Europe: they correct it with butter; and put broth upon it, made with long pepper, in which are boiled the kernels of the Indian pine apple. Wolfs-bane corrected in cow's urine, and arsenic corrected with juice of lemons, they use with success in fevers. A physician is not permitted to take care of a sick person, unless he can guess at his disease, and what humour is most predominant; which they easily know by feeling the pulse of the patient. Nor are they often deceived, as I can witness, having myself some experience in this art.

The principal diseases in this country are, 1. The mordechin, or cholera morbus. The means by which they cure it, is by not suffering the patient to drink, and by burning the soals of his feet. 2. The sonipat, or lethargy; which is cured by putting into the person's eyes bruised pepper mixed with vinegar. 3. The pilhai, or obstruction of the spleen; for which they have no specific remedy, unless it be that of the joghis, or converted Indians: they make a small incision under the spleen, and put in between the skin and flesh a long needle; from whence by sucking with the end of a horn, they draw out of the orifice a kind of fat matter that resembles pus or corruption. Most of the physicians have a custom of putting a drop of oil on the urine of the sick person: if it spreads abroad, they say it is a sign that the patient is very hot within; but on the contrary, if it keeps together entire, it is a sign that he wants heat.

The common people use very simple medicines. For the megrim, they smoke, like tobacco, the dried bark of a pomegranate tree, reduced to a powder, and mixed with four corns of pepper. For the common head-ach, they smell to a nodule, composed of a mixture of sal ammoniac, lime, and water, tied up together in a linen rag. Such dizzinesses of the head, as proceed from a cold thick blood, they cure by drinking wine, in which are steeped a few grains of frankincense. For deafness, occasioned by too great a quantity of cold humours, they drop into the ear a drop of juice of lemons. When the brain is charged and oppressed with watery humours, they smell to black cummin-seed, bruised and tied up in a nodule. For the tooth-ach, they put upon the tooth affected a paste made of crumbs of bread, and the seed of the stramonium, which stupifies the part affected, and eases the pain. In an hæmorrhage, or flux of blood, they make the person smell to bruised mother-worth, or wormwood. For a too great heat of the breast, and spitting of blood, they cover over with paste a giraumont, which is an Indian fruit like a gourd, and tastes like a citrull, which they bake in an oven, and drink the water that comes from it. For the colic, that either proceeds from wind or watery humours, they give to drink 4 spoonfuls of water, in which aniseeds and a little pepper have been boiled to a consumption of half. They also bruise an onion with ginger, and apply it to that part of the belly where there is most pain. For the lientery, they roast a clove of garlic under the ashes, and when they go to bed they hold it in the mouth, and suck out the juice of it. If they drink the juice of the leaves of cucumber bruised, it purges and vomits them. They cure a difficulty of urine, by drinking a spoonful of oil of olive, well mixed together with a like quantity of water. For a looseness, they torrify a spoonful of white cummin-seed, and a little powdered ginger, which they swallow mixed with sugar. I have seen them cure fevers which begin with a shivering fit, by giving the patient 3 large pills, made of ginger, black cummin, and long pepper. For tertian agues, they give the person, for 3 days together, 3 spoonfuls of the juice of teucrium, or great germander, with a little salt and ginger.

Concerning the Luminous Appearance observable in the Wake of Ships in the Indian Seas, &c. By Father Bourzes. N° 337, art. 23, p. 230.

1. When the ship ran apace, we often observed a great light in the wake, or the water that is broken and divided by the ship in its passage. Those that did not view it narrowly, often attributed it to the moon, the stars, or the lantern at the stern; as I did myself, when I first perceived it; but having a window that looked directly down upon it, I was soon undeceived, especially when I saw it appear more bright when the moon was under the horizon, the stars covered with clouds, and no lights in the lantern, or any other light whatever cast upon the surface of the water.

2. This light was not always equal: some days it was very little, others not at all; sometimes brighter, others fainter; sometimes it was very vivid, and at other times nothing was to be seen. 3. As to its brightness, I could easily read by it, though I was 9 or 10 feet above it from the surface of the water: that is, the title of my book, which was in large letters.

4. As to the extent of this light, sometimes all the wake appeared luminous to 30 or 40 feet distance from the ship; but the light was very faint at any considerable distance. 5. Some days one might easily distinguish in the wake such particles as were luminous from those that were not: at other times there was no difference. The wake seemed then like a river of milk, and was very pleasant to look on.

6. At such times as we could distinguish the bright parts from the others, we observed that they were not all of the same figure; some of them appeared like points of light; others almost as large as stars, as they appear to the naked eye. We saw some that looked like globules, of a line or two in diameter; and others like globes, as large as one's head. These phosphori often formed themselves into oblongs, of 3 or 4 inches long, and 1 or 2 broad. Sometimes we could see all these different figures at the same time. Another day when our ship sailed slowly, the vortices appeared and disappeared again immediately, like flashes of lightning.

7. Not only the wake of a ship produces this light, but fishes also in swimming leave behind them a luminous track; which is so bright, that one may distinguish the size of the fish, and know of what species it is. I have sometimes seen a great many fishes playing in the sea, which have made a kind of artificial fire in the water, that was a very pleasant sight. And often only a rope, placed crosswise, will so break the water, that it will become luminous. 8. If one take some water out of the sea, and stir it ever so little with his hand in the dark, he may see in it an infinite number of bright particles. 9. Or if one dip a piece of linen in sea water, and twist or wring it in a dark place, he will see the same thing, and if it be even half dry.

10. When one of the sparkles is once formed, it remains a long time; and if it fix upon any thing that is solid, as on the side or edge of a vessel, it will continue shining for some hours together. 11. It is not always that this light appears, though the sea be in great motion; nor does it always happen when the ship sails fastest; neither is it the simple beating of the waves against one another that produces this brightness, as far as I could perceive; but I have observed that the beating of the waves against the shore has sometimes produced it in great plenty; and on the coast of Brazil the shore was one night so very bright, that it appeared as if it had been all on fire.

12. The production of this light depends very much on the quality of the water; and, if I am not deceived, generally speaking, I may assert, other circumstances being equal, that the light is largest when the water is fattest and fullest of foam; for in the main sea the water is not everywhere equally pure; and sometimes linen dipped into the sea is clammy when it is drawn up again. And I have often observed, that when the wake of the ship was brightest, the water was more fat and glutinous; and linen moistened with it, produced a great deal of light, if it were stirred or moved briskly.

13. Besides, in sailing over some places of the sea, we find a matter or substance of different colours, sometimes red, sometimes yellow. In looking at it, one would think it saw-dust; our sailors say it is the spawn or seed of whales.

What it is, is not certain; but when we draw up water in passing over these places, it is always viscous and glutinous. Our mariners also say, that there are a great many heaps or banks of this spawn in the north: and that sometimes in the night they appear all over of a bright light, without being put in motion by any vessel or fish passing by them.

14. But to confirm further what I say, viz. that the water, the more glutinous it is, the more it is disposed to become luminous, I shall add one particular which I saw myself. One day we took in our ship a fish, which some thought was a boneta. The inside of the mouth of the fish appeared in the night like a burning coal; so that without any other light I could read by it the same characters that I read by the light in the wake of the ship. Its mouth being full of a viscous humour, we rubbed a piece of wood with it, which immediately became all over luminous; but as soon as the moisture was dried up, the light was extinguished.

As to the marine rainbows, I observed one after a great tempest off the Cape of Good Hope. The sea was then very much agitated, and the wind, carrying off the tops of the waves, made a kind of rain, in which the rays of the sun painted the colours of a rainbow. It is true the common iris has this advantage over ours, that its colours are more lively, distinct, and of longer extent. In the marine iris we could distinguish only two colours, viz. a dark yellow on that side next the sun, and a pale green on the opposite side; the other colours were too faint to be distinguished. But, in recompence for this, these irises are in greater numbers, one may see 20 or 30 of them together, they appear at noon day, and in a position opposite to that of the common rainbow, that is to say, their curve is turned as it were towards the bottom of the sea.

As to exhalations in the night that form in the air a long tract of light, these make a much larger tract of light in the Indies than they do in Europe. I have seen two or three that I should have taken for real rockets: they appeared near the earth, and cast a light like that of the moon some days after her change. They fall slowly, and in falling make a curve line.

The Case of a Woman who had her Menses regularly to 70 Years of Age. By Mr. James Yonge, of Plymouth, F.R.S. N° 337, art. 24, p. 236.

At Lamerton, 15 miles from Plymouth, there died lately a woman of 86 years of age, who to the age of 70 had her menses plentiful and regular. At that time they ceased, and soon after followed the like efflux from the hæmorrhoids, which continued till she was past 80. She was till then healthful and strong, of a vigorous aspect, smooth, plump, and florid in countenance, like one not half so old; her appetite was very good; her intellects clear and sound;

and her sight so perfect, that she could to the last thread a needle, and read small print without glasses. When that flux ceased, she became gouty, and about a year before she died, there arose an aposthumation on one of her wrists, which opened, and discharged much chalky matter, and some stones. The day she died she arose out of bed, and after performing some christian devotions, expired. She was never sick before the hæmorrhoidal flux stopped, except once at Exeter, where she was born, and then lived, she became infected with what they called the plague; it ended in a critical abscess in one of the emunctories; and, which is very strange, during all the time of that sickness, she nursed a male child.

The Description of a Tartarian Plant, called Ginseng; with an Account of its Virtues. By Father Jartoux, at Pekin, April* 12, 1711. N° 337, art. 25, p. 237.

The map of Tartary, which we made by order of the emperor of China, gave us an opportunity of seeing the famous plant ginseng, so much esteemed in China, and so little known in Europe. Towards the end of July, 1709, we arrived at a village not above 4 small leagues from the kingdom of Corea, which is inhabited by those Tartars called Calca tatze. One of these Tartars went and found on the neighbouring mountains, four plants of the ginseng, which he brought us entire in a basket. I took one of them, and drew it, in its exact dimensions, as well as I could.

The most eminent physicians in China have written whole volumes on the virtues and qualities of this plant; and make it an ingredient in almost all remedies which they give to their chief nobility; for it is of too high a price for the common people. They affirm that it is a sovereign remedy for all weaknesses occasioned by excessive fatigues, either of body or mind; that it dissolves pituitous humours; that it cures weakness of the lungs, and the pleurisy; that it stops vomitings; that it strengthens the stomach, and helps the appetite; that it disperses fumes or vapours; that it fortifies the breast, and is a remedy for short and weak breathing; that it strengthens the vital spirits, and increases lymph in the blood; in short, that it is good against dizziness of the head and dimness of sight, and that it prolongs life in old age.

Nobody can imagine that the Chinese and Tartars would set so high a value on this root, if it did not constantly produce a good effect. Those that are in health often make use of it, to render themselves more vigorous and strong; and I am persuaded that it would prove an excellent medicine in the hands of

* Panax quinquefolium. Linn.

any European who understands pharmacy, if he had but a sufficient quantity of it to make such trials as are necessary, to examine the nature of it chemically, and to apply it in a proper quantity, according to the nature of the disease for which it may be beneficial.

It is certain that it subtilizes, increases the motion of, and warms, the blood; that it helps digestion, and invigorates in a very sensible manner. After I had drawn the root, as above-mentioned, I observed the state of my pulse, and then took half the root, raw as it was and unprepared; an hour after, I found my pulse much fuller and quicker; I had an appetite, and found myself much more vigorous, and could bear labour better than before. But I did not rely on this trial alone, imagining that this alteration might proceed from the rest that we had that day; but 4 days after, finding myself so fatigued and weary that I could scarcely sit on horseback, a mandarin, who was in company with us, perceiving it, gave me one of these roots; I took half of it immediately, and an hour after I was not the least sensible of any weariness. I have often made use of it since, and always with the same success. I have observed also, that the green leaves, and especially the fibrous part of them chewed, would produce nearly the same effect. The Tartars often bring us the leaves of gin-seng instead of tea; and I always find myself so well afterwards, that I should readily prefer them before the best tea. Their decoction is of a grateful colour; and when one has taken it twice or thrice, its taste and smell become very pleasant.

As for the root of this plant, it is necessary to boil it a little more than tea, to allow time for extracting its virtue; as is practised by the Chinese, when they give it to sick persons, on which occasion they seldom use more than the 5th part of an ounce of the dried root. But as for those that are in health, and take it only for prevention, or some slight indisposition, I would advise them not to make less than 10 doses of an ounce, and not to take of it every day. It is thus prepared: the root is to be cut into thin slices, and put into an earthen pot, well glazed, and filled with about a quarter of a pint of water Paris measure: the pot must be well covered, and set to boil over a gentle fire; and when the water is consumed to the quantity of a cupful, a little sugar is to be mixed with it, and so drank immediately. After this, as much more water is to be put on the remainder in the pot, and to be boiled as before, to extract all the juice, and what remains of the spirituous part of the root. These two doses are to be taken, one in the morning, and the other at night.

As to the places where this root grows, it is in general between the 39th and 47th deg. of north lat. and between the 10th and 20th deg. of east long. from the meridian of Pekin. There is a long tract of mountains, which the thick forests, that cover and encompass them, render almost unpassable. On the declivities of these mountains, in the thick forests, on the banks of torrents, or about the roots of trees, and amidst a thousand other different sorts of plants, the ginseng is found. It is not to be met with in plains, valleys, marshes, the bottoms of rivulets, or in places too much exposed and open. If the forest take fire and be consumed, this plant does not appear till 2 or 3 years after: it also lies hid from the sun as much as possible; which shows that heat is an enemy to it. All which makes me believe, that if it is to be found in any other country in the world, it may be particularly in Canada,* where the forests and mountains, according to the relation of those that have lived there, very much resemble these here.

The places where the ginseng grows, are on every side separated from the province of Quantong (which in our old maps is called Leaotum) by a barrier of wooden stakes which encompasses this whole province, and about which guards continually patrole, to hinder the Chinese from going out and looking after this root. Yet vigilant as they are, their greediness after gain incites the Chinese to lurk about privately in these deserts, sometimes to the number of 2 or 3000, at the hazard of losing their liberty and all the fruit of their labour, if they are taken either in going or returning.

The emperor, wishing that the Tartars should have the advantage of this plant rather than the Chinese, gave orders this year, 1709, to 10,000 Tartars, to go and gather all that they could of the ginseng, on condition that each person should give his majesty 2 ounces of the best, and that the rest should be paid for according to its weight in fine silver. It was computed, that by this means the emperor would get this year about 20,000 Chinese pounds of it, which would not cost him above $\frac{1}{4}$ part of its value.

These herbarists carry with them neither tents nor beds, every one being sufficiently loaded with his provision, which is only millet parched in an oven, on which he must subsist all the time of his journey. So that they are constrained to sleep under trees, having only their branches and barks, if they can find them, for their covering. Their mandarins send them from time to time some pieces of beef, or such game as they happen to take, which they eat very greedily and almost raw. In this manner these 10,000 men passed 6 months of the year; yet, notwithstanding their fatigues, continued lusty, and seemed to be good soldiers.

Fig. 9, pl. 2, represents the ginseng plant. A the root; which, when washed,

* This conjecture has been verified. The ginseng plant has been found in Canada, Virginia, and other parts of North America.

was white and a little rugged and uneven, as the roots of other plants generally are. BCD represent the length and thickness of the stalk; which is smooth and pretty round, of a deepish red colour, except near its beginning at B, where it is whiter, by its nearness to the ground. D is a sort of knot or joint, made by the shooting out of 4 branches, which all rise from the same centre, and divide from one another at equal distances, and at the same height from the ground. The underside of the branch is green, mixed with white; the upper part is much like the stalk, of a deep red, inclining to the colour of a mulberry. These two colours gradually decrease, and unite together on the sides, in a natural mixture. Each branch has 5 leaves, as represented in the figure. It is remarkable, that these branches separate from each other at equal distances, as well in respect of themselves as of the horizon, and make with their leaves a circular figure nearly parallel to the surface of the ground. I do not know that ever I saw leaves so large as these, that were so thin and fine: their fibres are very distinguishable; and on the upper side they have some small whitish hairs. The skin between the fibres rises a little in the middle above the level of the fibres. The colour of the leaf is a dark green above, and a shining whitish green underneath. All the leaves are serrated, or very finely indented on the edges.

From D, the centre of the branches, rises a second stalk DE, which is very straight and smooth, and whitish from bottom to top, bearing a bunch of round fruit of a beautiful red colour. This bunch was composed of 24 berries, two of which I have here drawn, marked gg. The red skin of the berry is very thin and smooth: it contains within it a white softish pulp. As these berries were double (for they are sometimes found single) each of them had two rough stones, separated from each other, of the size and figure of our common lentils, excepting that the stones have not a thin edge like lentils, but are almost every where of an equal thickness. Each berry was supported by a smooth, even, and very fine sprig, of the colour of those of our small red cherries. All these sprigs rose from the same centre, and spreading exactly like the rays of a sphere, they make the bunch of berries of a circular form. This fruit is not good to eat. The stone is like the stones of other common fruit; it is hard, and incloses a kernel. It is always placed on the same plane or level with the sprig that bears the berry. From whence it is that the berry is not round, but a little flat on each side. If it be double, there is a kind of depression, or hollow place in the middle, where the two parts unite. It has also a small beard at top, diametrically opposite to the sprig on which it hangs. When the berry is dry, there remains only a shrivelled skin adhering close to the stones, and is then of a dark red, or almost black colour.

This plant dies away, and springs again every year. The number of its years may be known by the number of stalks it has shot forth, of which there always remains some mark; as may be seen in the figure by the letters bbb. From whence it appears that the root A was 7 years old, and that the root H, fig. 10, was 15.

As to the flower, not having seen it, I can give no description of it. Some say that it is white, and very small: others have assured me, that this plant has none, and that nobody ever saw it. I rather believe, that it is so small, and so little remarkable, that they never took notice of it: and what confirms me in this opinion is, that those who look for the ginseng, having regard to and minding only the root, commonly neglect and throw away all the rest of the plant, as of no use.

There are some plants which, besides the bunch of berries, have also one or two berries like the former, placed an inch or an inch and a half below the bunch. And when this happens, they say, if any one takes notice of the point of the compass that these berries direct to, he cannot fail of finding the plant thereabouts. The colour of the berries, when the plant has any, distinguishes it from all others, and makes it remarkable at first sight: but it sometimes happens that it bears none, though the root be very old; as that marked by the letter H had no fruit, though it was in its 15th year.

The height of the plants is proportionable to their size and the number of their branches. Those that bear no fruit are commonly small and very low.

The root, the larger and more uniform it is, and the fewer small strings or fibres it has, is always the better: on which account that marked with the letter H, is preferable to the other. I know not for what reason the Chinese call it ginseng, which signifies the representation or form of man; as it has no resemblance to the signification of its name; though there may now and then be found some roots which by accident have very odd forms. The Tartars, with more reason, call it orhota, which signifies the chief of plants.

It is not true that this plant grows in China, as father Martini affirms, on the authority of some Chinese books, which make it grow on the mountains of Yong-pinfou in the province of Pekin. They might easily be led into this mistake, from that being the place where it first arrives, when brought from Tartary into China.

Those that gather this plant preserve only the root, which they bury together in some place in the earth. They wash it well, and cleanse it with a brush from all extraneous matter; then dip it into scalding water, and prepare it in the fume of a sort of yellow millet, which communicates to it part of its colour. The millet is put into a vessel with a little water, and boils over gentle fire; the roots are laid upon small transverse pieces of wood over the vessel, and are thus prepared, being covered with a linen cloth, or some other vessel placed over them. They may also be dried in the sun, or by the fire; but then, though they retain their virtue well enough, yet they have not that yellow colour which the Chinese so much admire. When the roots are dried, they must be kept close in some very dry place; otherwise they are in danger of corrupting, or being eaten by worms.

An Examen of the Chalybeate, or Spa-Waters. By Dr. Fred. Slare, F.R.S. N° 337, art. 26, p. 247.

Germany abounds much with these waters, which bear one general name; they are called sauer-brunns, that is, sour wells or springs of water. The learned Germans call them acidulæ, ex. gr. spadenses, swalbaccenses, vel pyromontanæ, &c. Henricus ab Heers agrees with Vitruvius, Fallopius, and Helmont, &c. in justifying the acidity of the several sorts of spa and chalybeate waters; but, not being satisfied with their reasons, assigns others; and after a tedious harangue, concludes, that they owe their virtues to vitriol and sulphur. He observes, that vitriol and sulphur are found in the earth whence these waters spring; but yet does not give one proof or experiment of his having found any real vitriol, or true sulphur, or an acidity in these waters; but fills his book with imaginations, and quotations, exposing other men's ignorance.

Dr. Jordis, a fellow of the Royal Society, who practised physic at Frankfort, and often at Swalbac in summer-time, gave me an account of some ochres, or ferruginous parts, which he calcined; but in all his experiments, he did not satisfy me that the water held one drop of an acid by distillation, &c. What gave me the first suspicion, that the chalybeate waters did not contain any rough, or vitriolic, or acid salts in them, proceeded from an accidental use of a strong iron water, in which I dissolved soap, and found it lather and wash my hands well, and then I used a wash-ball and shaved with it; and tried several other waters of this sort, which did the same, and much better than some pump-waters.

1. I consulted my palate, and tried whether I could discover any sharpness or acidity in our English steel-waters at Tunbridge, at Black-Boy, in the parish of Franfield in Sussex, Hampstead, Sunning-hill in Berkshire, &c. but I was so far from discovering any such thing, that these waters seemed rather to leave a sweetish flavour behind: thus many alkali salts, of the fixed kind, if nicely examined, have affected my taste. 2. I made experiments with several sorts of such spirits as are apt to ferment with acids; such as spirit of hartshorn,

of sal ammoniac, &c. but these made no ferment, nor any motion or change in these waters. 3. I considered the diseases in human bodies, for which physicians prescribe these waters; and that they were often such as proceeded from sharp, acid, or acrimonious causes, as cardialgiæ or heart-burnings, sour vomitings, corrosive diarrhœas, colics from scurvies and stranguries; and that sweetning and alkalisate remedies are used for these distempers.

I consider these waters as containing in them the properties of iron; and I find by experience, that it is most opposite to acids, being one of their great correctors, and therefore rather to be esteemed an alkali. Thus, 1. Take some filings of iron, perhaps a drachm, and pour on them about an ounce of the milder acids, such as vinegar, verjuice, or the juice of lemons, and it will destroy the sharpness of these juices: or if you pour on these filings mineral acids, as the very corrosive spirit of nitre, or of salt, or what is called oil of vitriol, they will immediately lose their acidity, be disarmed of their sharp points, and by evaporation give a salt that will taste sweetish, and is by chemists called saccharum martis, if duly prepared; which is safely given inwardly, and is esteemed a good altering medicine. 2. Steel beaten to a fine powder is, without any further preparation, given inwardly with great success for stomachic diseases, as in the green sickness, hypochondriac, and various other acid and acrimonious disaffections.

I considered milk to be a very proper and obvious subject to bring this controversy to a plain and unquestionable decision. I made this experiment with all possible exactness: I first proved the chalybeate waters, more particularly the spa-waters, by trying whether they tinged with galls. These being very good, I put part of the waters to cold milk; some I made only luke-warm, and some I boiled together, in equal proportions: but they were so far from affording any curd or coagulation, that they continued several days without being sour.

The German physicians, supposing these waters to be sour, prohibit the use of all lacticinia, as if they were as noxious as deadly poisons, while any are in a course of their medicinal waters. But I have frequently advised, in some cases, milk to be given daily in the evening, through a whole course of steel-waters, with good effect: and some could not even bear the waters without having a third part of milk or more mixed with them, and have continued them so for many weeks, with good benefit: nor do I find the least reason to prohibit the use of milk in a course of Bath waters, having for more than a year and half been making the best scrutiny I can into the properties, virtues, and vices (if any) of these waters.

Since our experiments discover, that those things which are of a sweetning alkalisate nature, so well agree with these mineral waters, it will appear by the following experiment that acids very much disagree. 1. I put one drop of oil of vitriol to a large glass full of strong spa-waters, which before the addition of this acid gave a deep purple to the solution of galls; but now would not give the least tincture, though I put in 4 times as much of the galls. From hence I conclude, that the virtues of the chalybeate ingredients, which I take to be the life and soul of these waters, were so far destroyed, as to have lost their cordial or corroborating faculty; and that the bile or gall, in the human bowels, could not be able to separate the chalybeate (which are the only medical) particles, and mix them with the chyle, in order to answer any end in physic. This should be a caution to those that design to make these waters pass better by urine, that they do not make use of any acids; it being a common practice to use spirit of vitriol, spiritus nitri dulcis, &c. as a diuretic: unless it should so happen that they have a design to take off, and divest them of their warm cordial or altering power, and so to bring them near to common water; which I must confess we are forced to do, especially in the use of Bath waters, in some hot inflammatory cases. 2. I shall conclude with one short experiment in favour of our alkalis; viz. that if you put any alkali salt, volatile or fixed, such as volatile salt of hartshorn, or of sal ammoniac, or fixed salt of tartar, of wormwood, or any other true alkali, you will then destroy the above-named acid spirit, recover the virtue of the waters, and dispose them to give their tincture, as they used to do in their natural state.

An Account of several Urns and Sepulchral Monuments lately found in Ireland. By Francis Nevill, Esq. N° 337, art. 27, p. 252.

Within a mile of Castle-doe, an urn was found in a small island, surrounded with bogs. The island was very dry, light, sandy ground, situated on an isthmus, about half a mile over, between the bay of Dunfannaghan and Lough Kinnevier. In taking up a flat stone there was found a cavity under it, which may be called a sepulchre, or tomb, containing an urn, which was broke by the person who found it, because, it contained nothing but bones and ashes. In the same tomb there were some bones of one about 10 or 12 years of age. The tomb stood east and west: the urn was found in the west end; it was the smallest urn I have seen, but the cavity wherein it lay was near 5 feet long, $2\frac{1}{2}$ broad, and about the same depth: it was made up of six coarse flag stones, viz. one on each side, one at the head, another at the foot, one above and one below: the bones were much wasted, and but few of them remained. While I staid there, we opened three more, which the man quickly found out, because he had made his marks by the plough. These three were much larger than the former; one of the three was near the centre of the island, and the largest of all; but all were alike made. There was no urn in either of them, and bones in one only, which was the largest. The bones seemed to be of a man of an ordinary stature: if any had been in the other two, they were consumed. This seemed to have been a common burying place, there being so many of that kind of tombs in it; and one may gather from thence, that at that time they burnt some, and others they did not; because here was an urn with bones burnt, and there were bones unburnt.

There were three such urns found in three small stone chests, under a great kern, or heap of stones, near to Ban bridge, in the county of Downe. Similar urns were found near Omagh in the county of Tyrone, in the like chests, under two heaps of stones, which were removed to build some houses in the town. One urn was found in a little sandy hill near Cookston, on the road to Lisson, in the county of Tyrone: it was covered with a large rough lime-stone; which being removed, to make lime, the urn was discovered in a hole encompassed with six stones of equal size, which formed a hexagon, inclosing the urn. The water that had fallen on the urn from the lime stone, or the air condensing, had petrified, and made a stony crust on its outside: some bones and ashes were found in it.

At Dungannon, in the same county, a servant of mine, working in a sand-pit near the town, struck on an urn, which was the largest I ever saw. It was found with the mouth whelmed downward, the bones and ashes on a flat stone, and the urn covering them: it would have held about 3 quarts, and had been better burnt in the fire than they usually are: but this met with the fate of others; it was broke by the spade before the man was aware, and had no stones about it as the others, but was buried in the earth about a foot under ground. As they dug the bank for sand, the place where the carcase was burnt was discovered by the coals and pieces of bones, which spread a great way, about a foot under ground.

Near the same town is a place called Killimeille, i. e. Lousey Cell, or Lousey Burying-ground, where on the top of the hill are two circles of dry stone, about 20 yards in diameter each; they meet on two sides, and form the figure of 8. I suppose when first formed they made a dry wall for two distinct burying-places, one for the men, the other for the women; or rather two repositories for urns. One James Hamilton, wanting stones to build a house, drew off most of them from this place. When he had entered within one of the circles, he found three urns in three several holes, set round with 6 stones, and covered with flat stones, and other stones thrown on the top; he broke what he found, not finding what he expected. On the same hill, about 30 yards to the eastward of these circles, we found the altar on which they used to burn their dead, overgrown with earth and green sod, which we caused to be uncovered: it was made of dry stone, 8 feet long, and 4 feet broad; the coals and bones were fresh among the stones, and the stones burned with fire. At the east end of this altar there was a pit, which was likewise overgrown with earth and green sod; which we opened, and found it to be the receiver, where they swept in all that remained on the altar after burning. We searched deep, and the substance was all alike, black and greasy: it had tinged the hill in a straight line from the pit to the bottom of the hill; and discovered itself to our view, the land being then ploughed.

I shall add only one more that I have seen, besides the many that are yearly discovered; to shew that this was the way the Irish had of burying, in heathen times, though the people know nothing of it by history or tradition. In the county of Farmanagh, on a hill over Wattle-bridge, there has been a vast heap of stones, the basis encircled with very large stones, standing on end. This heap has been removed, to pave our roads, and build that bridge; under which there were some urns in stone coffins, and I believe there are some remaining. These I suppose were the urns of some great personages. The heap was so vast, and the stones about it so large and so many, that it must have cost great pains to bring them there: or perhaps there might have been a battle, and some of the great officers might have their bones interred there, and the army made that great work over them; for it seemed to be a work done by many. I have seen several such heaps in this kingdom, and I doubt not but they are all monuments for the dead.

An Account of a great number of Urns dug up at North Elmham, in Norfolk. Communicated by Peter Le Neve, Esq. Norroy, F. R. S. N° 337, art. 28, p. 257.

In the parish of Elmham, about half a mile from the town, there is a field called the Broom Close, lying on the west-side of the road from Elmham to Beetly. Some labourers repairing the fence on the south-side of the close, in the bottom of the ditch accidently pitched on a pot, which they expected to have been full of money, and fell to ransacking; but finding nothing but dust and ashes, went to their work again; and digging on, they found two or three more; but the contents the same. On a further search, and digging first under the hedge, afterwards further into the close, there were found great quantities of such urns, and several very near together. There is one man in the parish who has been chiefly employed in this search for several people, and the number that he has taken up since the first discovery is near 120, and yet the compass of ground turned up on this occasion, does not amount to more than a quarter of an acre. The close where they are found is high land, and this place the highest part of it; the soil a sharp gravel, and very dry, and lies next to a highway. As for the urns themselves, they are generally of the same shape, but of very different sizes.

The shape of these is conformable to the representations usually exhibited in the descriptions of urns; viz. the bottom narrow, a little flatted (and in some quite round) wider upward; the top contracted to a narrow mouth: the earth coarse, the work rough and uneven, but generally well burnt; some of them slightly wrought and indented (the work expresses very little skill or care) and some plain. The size is various; some of the capacity of a quart, some 2, some 3 quarts, and one I have, unopened yet, which I believe will contain a gallon.

The pots are very tender when they come first out of the ground, and frequently suffer by the wounds of the spade: they are most of them broken (more or less) in taking them up, and hardly any that have not their mouths broken; often it would seem, as they lie in the ground, by the weight of the earth pressing upon them, or the feet of horses going over them, as appears by the broken pieces of several of them found a good way down among the earth. The urns are found at uncertain depths; some very near the surface, some 2, some 3 spits deep, which is the deepest any body has taken the pains to dig for them.

The contents are generally the same. I have opened several of them, and found in all of them pieces of broken bones, some black with burning, and some turned to ashes, with some pieces of coarse glass run and sticking to the bones; which, whether it proceeded from any thing of that kind burnt with the body, or only the sandy earth vitrified with the strength of the fire, as I am inclined to think, is doubtful. Besides, I found some pieces of brass, some run, some much burnt, and some not injured, with some pieces of iron, but so decayed with rust, that their figure or use is hard to judge of. I have some knives, and other odd things, but much eaten and decayed with rust: but the brass, which is not burnt to pieces, remains generally firm and entire. One thing is remarkable, we find a great many pair of small nippers (such as we pull out hairs with) commonly of brass; and most of them so perfect and good, that the edges are full square, and the spring as strong as any we can make of the metal. These are chiefly the particulars of what we find: but as for coins (which of all things were most to be wished for) we meet with none. I hear of two in the hands of a person of Elmham, but had not an opportunity of examining them: but when they were found or taken up I cannot tell. I have one, found the other day in an urn, but very imperfect; what remains of the impression looks more like British than Roman; but by the shape of the coin and metal it should be the latter, though I think it not easy to determine.

It is pretty certain that the urns are Roman, and consequently the number must denote a station or colony of that people: so I should be glad to hear your thoughts and opinion of the place where that station was appointed, or colony planted; on what occasion, and at what time it might be placed there.

Observations on Lough-Neagh in Ireland. By Francis Nevill, Esq. N° 337, *art.* 29, p. 260.

As to the lake, Lough Neagh, so much talked of for its changing wood into stone, which report is too much credited by some, who live near the Lough; I can assure you there is no such petrifying quality in that water. I lived 14 years in Dungannon, within 5 miles of it, and was very often there, about the skirts, for many miles, and in a boat upon it several times. I have taken the survey of a great part of its shore, when I drew the scheme for making the Glan-bog navigable, from the Lough through part of the upper Bann, to Newry; which was done at a time when the waters were very low, and a large strand left in several places. Many trees lay in the verge of the lough, some of which might have lain there many centuries, having been overturned by the lough's encroaching on the land, where great woods had grown; and many roots of great trees were standing in their proper places, where the water had prevailed on the land, and no alteration in the wood at all, but it was firm, sound wood, without any petrifaction.

I have had an occasion, among other things, to talk to Mr. Brownlow on this subject, a great part of whose estate lies contiguous to the lough; and he told me, that he believed that there was not any petrifying quality in the water; for that he had made several trials, and had holly stakes driven into the ground within the verge of the lough, and that some of them continued there many years, but without any alteration.

And yet there have been great quantities of such sort of stone, resembling wood, found on the strand, after great floods and storms of wind, which have put the lough into a ferment; the waves breaking down the banks, encroaching on the land, and tumbling over trees, by which these stones are discovered: and if ever they were wood, they have been petrified by the earth, and not by the water; of which kind I have seen several pieces, large and small, some like oak, some ash, and some like holly with bark, grain and knots like wood; so that any person by the eye would judge it to be wood, till it is tried. I had a piece about 16 inches long, that looked as if it had been a large chip, cut out of the side of an oak-block, with the bark on it; and in cutting such chips, there happens generally some shakes or flaws in them, so that there will be a separation of parts at one end, while they remain firm at the other, as it was in this. I could have raised several of such splinters of this large chip; and when so raised, they would flap down again, like a spring. Some of those stones would appear at one end as if rotten, and decayed wood; but trying it, it was as much stone, as any other part.

The lake is reputed to be 24 miles long, and 12 broad, and navigable from Charlemont to Portlenoue, which is about 35 miles. It does not abound with many sorts of fish, but those are very good, such as salmon, trout, pike, bream, roach, eels and pollans, with which last it greatly abounds: the English call them fresh water herrings, for want of another name; for pollan is an Irish name. They catch them in the summer with sieves, as they do herrings, and they are a great relief to the poor, being very cheap: they are in shape and size like the largest smelts, full of very large bright scales, and pleasant meat, being eat fresh. These were supposed to be a fish peculiar to that lake; but I find Lough Earne has the same sort, but not in so great plenty. They are generally caught here in their eel-nets, running to the sea; so that I am of opinion, that they are that sort of fish that is caught in the sea, or between the fresh and salt-water, called shads; and that the large ones come from the sea, as the salmon does, and leave their spawn in the lough; which, when they grow large, go to the sea, and there come to their full growth.

That there is some healing quality in the water of this lough, is certain; but whether diffused through all parts, is not known, nor pretended. There is a certain bay in it, called the fishing-bay, which is about half a mile broad: it is bounded by the school-lands of Dungannon, has a fine sandy bottom, not a pebble in it, so that a man may walk with safety and ease from the depth of his ancle to his chin, on an easy declivity, at least 300 yards before he come to that depth. It is in great repute for curing the evil, running sores, rheumatism, &c. Many come there, having running sores, and are cured after a little time Great crouds come there on Midsummer-Eve, of all sorts of sick; even sick cattle are brought, and driven into the water for their cure; and people believe they receive benefit. I know it dries up running sores, and cures the rheumatism, but not with once bathing, as people now use it; and the drinking the water I am told will stop the flux. I look upon it to be one of the pleasantest bathing places I ever saw.

An Account of a Woman who had lain 6 Days covered with Snow, without receiving any Nourishment, &c. By Mr. Samuel Bowdich. N° 337, art. 30, p. 265.

Joanna Crippen, of Chardstock in Dorset, being a spinner of worsted, and going home on the 24th of January, with some work, but it snowing very hard, and being very deep, she was forced to lie down under a hedge, having lost one of her shoes; and her clothes, which were very mean, were by the brambles and thorns torn almost quite off her back: in which place she lay from Monday evening about 6 o'clock, until Sunday following about 4 in the afternoon, and then was discovered by some of our neighbours, who went out with poles, shovels, &c. to search for her; and after some time spent in it, at last found her buried in 4 feet deep of snow. One of the men thrusting at her with his pole, found she was there, and alive. She immediately spoke, and begged he would not push her too hard, for she was almost naked; and desired that some of the women would come to her, and take her out, which was accordingly done; when they found her without stockings or shoes, an old whittle about her shoulders, with a large hole in it, which she had eat through; the snow melting down on her she drank to quench her thirst. She had a mortification on one of her great toes, but she now is very hearty, and in a fair way of a perfect recovery. She was very sensible at the first taking her out, and still continued so; and she knew every body perfectly well: and yet she had taken no manner of food all the time of her being in the snow.

An Account of the Subsiding, or Sinking down, of Part of a Hill, near Clogher in Ireland. By the Bishop of Clogher, F. R. S. N° 337, art. 31, p. 267.

Let ST, pl. 2, fig. 11, represent part of the ridge of a hill, gradually rising from S to T, for near half a mile; and STWV the north side of the hill, with a declivity from S to V, and from T to W. The perpendicular height at X to the plain of the bottom at Y, 150 feet, and the slope line or hypothenuse XY, 630 feet. The declivity pretty uniform from X to L, and from L to Y considerably steeper; the bank AEFD overgrown with shrubby wood; all the ground on the side of the hill being firm, green, and arable; of a mixed soil, clay, and gravel, but more clayey.

On Tuesday the 10th of March, 1712-13, in the morning, the people observed a crack in the ground like a furrow made with a plough, going round from A by BC to D. They imputed this to what they call a thunderbolt, because there had been thunder and lightning on Monday night. But on Tuesday evening a hideous dull noise raised their curiosity; and they observed that the whole space ABCD containing about 3 Irish, or $4\frac{3}{4}$ English acres, had been all day in a gentle motion; and the noise continued all night, occasioned by the rubbing of bushes, tearing of roots, rending and tumbling of earth. The motion ceased on Wednesday afternoon, when they saw the bushes on the bank EF were removed, some standing and some overthrown, to the plain meadow YY. The green ground above EF, when it came to the top of the steep part EF, rent with hideous chasms, 10, 15, or 20 feet deep, and tumbled down in rolls of a yard or two thick, and 10 or 20 long and broad, not unlike a smooth water breaking over a cataract, and tumbling in waves below.

There was a precipice at the top xx, 65 feet perpendicular, making the slope line xx, 126 feet. The ground from x to L, was made more level, the whole perpendicular height of x not exceeding the plain of L, above 30 feet: but the ground at L, in the whole line from E to F, was mounted above 20 feet higher than the unmoved ground on either side, at E and F; and the height of L, above the plain of y, is 55 feet. There was a ditch HI, went across the ground, which being broken off at oo, is removed, together with the moving part, 34 feet lower down than the immoveable; but at the bottom y, it is tumbled 60 feet over the plain meadow. The breadth at the bottom ab is 400 feet, and at cd about 300.

The whole face of the precipice xx, is of a blue clay, mixed with many little blue stones. The metal is very hard when dry; but on any rain it softens to a kind of mortar, without the degree of toughness and stiffness that is natural to clays. It is very much like that gravel or sand, which is somewhat of a grey marly nature, and with which of late they so much improve the ploughed land in this country. About x there are chasms or gapings, full of water, which make a rill down the hiatus BEA, but in no greater quantity than might have been expected from a well sunk to a less depth. Though I was told that there were holes in the higher mountains, that received water under ground; yet I can find no such thing, nor any symptoms of a current under ground, either where it enters or rises, in all the neighbouring ground for some miles. It seems to me that there has been no vacuity under ground, to receive the subsiding earth; for what the bank ELF is raised higher, and what is tumbled down to the plain ab, may very well compensate the subsiding at the precipice xx. It is to be observed, that before the rupture, the declivity from x to L, was not altogether uniform, but was hollower where x is now, than the adjacent parts; it might have been 10 feet deep in the middle, and 100 feet diameter; and they have a tradition, that this was made by a subsiding before the 41 wars, the oldest epocha the country Irish know of.

I have inquired diligently of the neighbours, if they found any shocks or indications of an earthquake, but do not find the least appearance of any. They impute it to the great and constant rains we have had last harvest and winter, which have soaked and steeped all the ground, but cannot guess after what manner they should produce this effect; for it is impossible any water shonld stand on the ground, or in the vicinity, it being all on the declivity of the hill.

An Account of some Ancient Trumpets, and other Pieces of Antiquity, found in the County of Tyrone in Ireland. By Francis Nevill, Esq. N° 337, art. 32, p. 270.

About 7 years since, there were 8 trumpets found together in the lower Barony of Dungannon, in the County of Tyrone, 4 of one make and 4 of another. They are of cast brass, of the thickness of an English half crown.

Fig. 18, pl. 1, represents one of the trumpets 24 inches long, according to the turn or arch it makes, and is 3 inches in diameter at the large end, but at the small end it is solid for about 2 inches, with a loop at top to hang it by, and another loop between the solid part and the mouth-piece, The mouth-piece is oval, 5 inches from the solid end, $1\frac{3}{4}$ inch long, and 1 inch wide; its sides are smooth, round, and even, easy for the lips of a man, but will not admit of any sound by blast as a horn does, but by the articulate voice of tooting it will; a person may raise his voice in it to the highest pitch, and bring it to the deepest base.

Fig. 19, is another trumpet 26 inches long, 1 inch wide at the small end, and 3 inches at the other; but it seems to be imperfect, for want of a mouth-piece, and the small end seems to be fitted to receive one. On the back, at a, there is a hole, and another under the moulding at b; the first I believe was to fasten the mouth-piece, the second I imagine was to stop or open with the finger to alter the sound at pleasure. But as it is now, it cannot be sounded: for it is no way fitted for the mouth, it being thin and sharp, as appears by the figure.

I cannot find in any Irish story, or by tradition, any account of this sort of trumpets, nor indeed of any other; from whence I gather they are of great antiquity; for had they been of use at, or since the first of the English conquests, there would have been some hints of them. And therefore I conclude they were of use when the country was Pagan, and not in martial affairs, but by the priests, at their funeral rights, when they buried their dead, and bore a part with those who cried at those funerals, or made a howling sort of a noise, which sort of noise is used among the natives to this day.

Fig. 20 is an instrument of fine gold, but what to call it, or of what use, I never could learn. There have been five found in different places, three of which I have seen since I came to this place. One was found near Coothill, in scowring a ditch, under the side of a large stone, which was one of three that were placed triangular-wise; whether set in this form as a mark to find this thing when hid, or whether for any other use, I cannot guess; but I have seen stones in several parts of this kingdom set in this order. It is reported, that there were some other pieces of gold found with this, but I could not see them; one, I was told, was somewhat like a scepter, about 18 inches long; and another was round like a large medal, as thick as two crown pieces, and as broad as the palm of a man's hand, with strange characters on it. Its beauty and colour surpassed any gold I have seen; it had been cast, and not wrought with the hammer. The two cones were 2 inches in diameter each, and 2 inches asunder from each other. The bow or handle was made like an arch, about a quarter of an inch thick; the handle was round, tapering towards both ends, where it was fixed to both the cones. The goldsmith told me, that when he had wrought part of it, he could not bring the gold to the right colour again, though it was in fineness equal to our standard.

An Account of a Person who had a new Set of Teeth after 80 *Years of Age; with some Observations on the Virtues and Properties of Sugar. By Dr. Fred. Slare.* N° 337, art. 33, p. 273.

I have had reason to give a good character of sugar, on account of some extraordinary effects it seemed to have on my grandfather. He made it his daily practice to eat as much sugar as his butter spread upon bread would receive for his constant breakfast, unless he happened to exchange it for honey sometimes. He frequently sweetened his ale and beer with sugar; he had sugar put to all the sauces he used with his meat. At 80 years of age he had all his teeth in his mouth, strong and firm; never had any pain or soreness in his gums, or teeth; never refused the hardest crust. In his 82d year one of his teeth dropped out, and soon after that a second, which was one of the fore teeth; he bad me feel the cavity, where I struck my nail upon a bone. In short, all his teeth came out in two or three years, and the young ones filled up their room; thus he had a new set quite round. His hair, from a very white colour, became much darker. He continued in good health and strength, without any disease, and died in his 99th or 100th year, of a plethora, as I guess, for want of bleeding.

This induced me to vindicate sugar, which I have done formerly before the Royal Society; and have shown the unjust calumny of the famous Dr. Willis against sugar, who charges it with a corrosive liquor, as bad as aquafortis; he calls it aquastygia. I examined it, and found the charge unjust; that sugar contained no worse substance in it than milk and honey, and manna, nay even bread itself.

The difficulty of showing the figure that sugar naturally shoots into lies in this, viz. that all other salts shoot or crystallize, and make their figure in a cool place; but sugar will crystallize only in a hot stove, and is more apt to be compounded, and not to show its true primitive texture. Thus it happens with snow, which in its true simple shape is a hexagon, but cannot be always discovered single. This is yet more easy to be accounted for than snow, and we have been able to choose such parcels of that sugar called candy, as represent the following figure, being a prism. I never questioned but that it was a true salt, having all the properties of one.

Fig. 21, pl. 1, shows the form of the crystals or salts of sugar, having two bases, opposite, equal, and parallel; the others are parallelograms.

Fig. 22, shows the basis of the preceding figure.

Remarks on an undescribed Plant; and other Particulars, observed in Wales. By Mr. Edw. Lhwyd. N° 337, art. 34, p. 275.

Pl. 3, fig. 1, represents a remarkable sea-plant,* met with in dredging for oysters, near Lhan Danwg in Meirionydshire. The plant is of a straw colour, and about 3 inches high in the whole. The stems are hollow, and filled with a kind of thick reddish liquor, as much resembling blood as the juice of plants; so that it seems referrible to the zoophytes. On pressing these stems at the bottom between the fingers, the red liquor is forced up, and causes the drooping flowers, or seed-vessels, to mount erect.

We have lately discovered a sort of marble in that county, which when polished represents a number of small oranges cut across; the cause of which is an infinite quantity of tubipores, or alcyonium, stuck through the stone. This might serve very well for inlaying work, as tables, windows, cabinets, closets, &c. and would make curious saltsellers.

Wales affords a good quantity of alum and copperas, particularly Pembrokeshire and Caermarthenshire for the former, and Merionethshire for copperas, where I saw a great vein of pyrites strongly impregnated.

An Account of a Scirrhous Tumour, included in a Cystis, &c. By Mr. Richard Russel, Surgeon at Lewes in Sussex. N° 337, art. 35, p. 276.

Aug. 18, 1713, I was sent for to Mrs. Smith, who had been much reduced

* Tubularia indivisa. Linn. Gmel.

by a fever, which from her cough, a sharp pain under her breast, and other symptoms, was judged pleuritic. But on a discharge from her breast, of a thin serum or gleet, all symptoms vanished. When I saw her first, the liquor discharged by a small pin-hole near the papilla, was little more than would have wet a handkerchief fourfold. Examining the breast, I found a large tumour, that lay deep, yielding to my fingers, and pasting like dough. I searched the abscess with a probe, and twisted out with it a matter like saw-dust, or bran, mixed with hair. On laying open the breast, I separated a cystic tumour, which weighed 8 oz. and contained a solid matter, like the above mentioned, mixed with a body like hair.

Inquiring into the manner of its coming, she told me, that 38 years since, she received a contusion in that breast by a fall from a horse, which was attended with great pain and fluxion; insomuch, that the veins of her breast appeared varicose, and turgid, as in a cancer; but her pain ceasing, they sunk, and left an indolent tumour in her breast, supposed by her surgeon to be a true scirrhus: since which time it has always continued nearly in the same state, without pain, increasing very little in magnitude, but obstructed in such a manner, that she could not nurse her child with that breast.

The tunic was pretty thick, nourished with very small vessels, but had formed a scirrhus of the glands it adhered to, by keeping up a distension of parts, till there was a cohesion of their membranes and vessels. I make no doubt, but this was a body of diseased glands, which had suffered a colliquation by some extravasated fluid, and that the membrane of the tumour was their proper tunic.

After this manner all our tunicated tumours seem to be formed; for when an obstruction proceeds to extravasation, there is a liquor poured out, which consists of such particles, that by degrees make a colliquation of the glandulous flesh, which is not very sensible of pain; and by degrees the capsula becomes distended with a matter of a very different consistence, which gives the name to the tumour, either steatoma, atheroma, or meliceris. Thus, pour oil of olives on spirit of nitre, and the oil first becomes a little hardened, then of the colour and consistence of marrow, till by degrees it is hardened into a white fat, resembling that of animals. The possibility of this colliquation and digestion, we may easier be induced to believe, if we consider how often we find the glands of the viscera petrified, without any degree of pain, or the membrane in any measure destroyed: the truth of which, every one conversant with the dissection of morbid bodies, must have observed.

Concerning a quarry of Marble, discovered in the County of Farmanagh in Ireland. By Francis Nevill, Esq. N° 337, art. 36, p. 278.

I discovered a marble quarry in the mountains. It lies on the north side of Calcagh, in the parish of Kilasher, and county of Fermanagh. There are marble rocks, whose perpendicular height is 50 or 60 feet, discovered by subterraneous rivers, which have gradually washed away the earth and loose stones, and discovered these vast rocks. There are many great pits fallen in, on the sides of the great mountain; several of them in a small compass of ground; so that it is dangerous travelling near them. There are many caves formed, some very large, the sides and arches of marble; some of a liver colour, varied with white in many little figures; some of a light blue, varied with white; but I could find no entire white or black among them.

Remarks on the Plague at Copenhagen, in the Year 1711. *By John Chamberlayne, Esq. F. R. S.* N° 337, art. 37, p. 279.

This distemper began to show itself first in this city, about the beginning of July, 1711; and increased till the beginning of September; after which it gradually diminished to the end of the year, at which time it totally ceased. It appears, that before this distemper there were about 60,000 souls in Copenhagen: from whence they infer, that there is born every year about 2000, and that there dies nearly the same number; which being multiplied by 30 makes 60,000.

In the 6 months which this distemper continued, it is thought it carried off about 25000 souls. It is true, the public lists reckon only 22535; but it is agreed by all, that in the last week of August, and the first two weeks of September, each of which carried off above 2300 souls, there died a great many, of which no notice was taken.

Almost the very same happened 2 years before at Dantzic; where, before the plague broke out, there died weekly from 45 to 50; but the number of the dead increased gradually to the beginning of September; so that in the first week of that month there died 2205 souls, in the second week 2070, and in the third 2075. After which the mortality decreased to the end of the year.

It is observable, 1. That there were some houses which escaped the infection; but that there were few where it did not carry off more than one or two persons; and that there were many in which it did not leave a soul alive. 2. That, generally speaking, this distemper was most fatal to the meaner sort of people; scarcely any person of note dying; but on the contrary a great number of the poor. Which may be attributed to several causes: the first, and most general of which, is their nasty manner of living. The 2d is, that this sort of people live very close together, and as it were heaped one upon another; so that sometimes there are four families in one room. The 3d is, the foolish curiosity they have of seeing the dead bodies. And 4thly a great many of them are so biggoted to the Turkish notion of predestination, that they say, if it pleases God that I should die of this disease, I shall not escape it; and if it be his pleasure that I shall live, I cannot die: and on this notion they go abroad every where, and so catch the infection. Some of them even make no scruple of lying in the same beds, where others have died.

The 3 sorts of trades, of which there died most, were coffin makers (who took measure of the dead bodies) surgeons, and shoe-makers. The care that was taken, and the medicines that were used, did great service. I was told that Theriaca did little good; and the same also was observed at Dantzic.

Some Anatomical Observations. By Mr. William Cheselden, Surgeon, F. R. S.
N° 337, art. 38, p. 281.

Fig. 2, pl. 3, shows the beginning of the aorta, or great artery, from the heart of a woman who died of a dropsy. A is the aorta; and BB two chalk-stones, which possessed the place of the semilunar valves. The left ventricle of the heart was dilated to twice its natural size. We supposed that these stones occasioned the dropsy, by obstructing the valves, and obstructing a regular distribution of the blood.

Fig. 3, shows a bone taken from the falx, or first process of the dura mater, of a man who died of violent head-achs.

Fig. 4, shows a bone taken from between the ventricles of the heart of a man, who died hydropic and tabid. In this body the whole pericardium adhered to the heart.

Fig. 5, shows the optic nerves; the right nerve being wasted and discoloured: the eyes both appeared to be very good. I had not an opportunity of inquiring into the case of this person; but I suppose it must have been a gutta serena. I opened another eye of a man who died of that distemper; in which I found that part of the nerves, which is within the cranium, crushed flat by the anterior lobes of the brain; their ventricles being full of lymph.

Fig. 6, shows three spleens taken from one body.

Fig. 7, two spleens taken from a man.

Fig. 8, two spleens taken from a woman.

Note, that, in all these three cases of the spleens, each had proper vessels, but the arteries only are here expressed; and that the spleens in each body, taken together, were only equal in magnitude to the one we usually meet with.

A heart, with the vena azygos inserted into the right auricle; and the descending cava coming round the basis of the heart, above the aorta and pulmonary vessels, to enter the auricle at the lower part with the ascending cava.

A ureter double two-thirds of its length next the kidneys, and distended by stones passing through it.

The tubæ Fallopianæ impermeable, and without alæ vespertilionis; the outer ends being connected to the testes.

An Account of a Book, entitled, Julii Vitalis Epitaphium; cum Notis Criticis Explicationeque, V. C. Hen. Dodwelli, et Commentario Guil. Musgrave. Iscæ Damnoniorum. Sumptibus Philippi Yeo. 1711. N° 337, art. 39, p. 283.

This book contains a description, with many critical notes, of a stone dug up near Bath, 1708, and is said to be set up in a wall at the east end of the church, in the Abbey-green. The author thus reads it at length, according to the modern way of writing Latin.

Julius Vitalis, Fabricensis Legionis Vicesimæ Valerianæ Victricis, stipendiorum novem, Annorum viginti novem, Natione Belga, ex Collegio Fabricensium elatus, hic situs est.

END OF VOLUME TWENTY-EIGHTH OF THE ORIGINAL.

Logometria Auctore Rogero Cotes, Trin. Coll. Cantab. Soc. Astr. et Ph. Exp. Professore Plumiano, et R. S. S.* N° 338, p. 5. *Vol. XXIX.*

This paper is omitted here, because it was from hence extracted into the

* Roger Cotes was an eminent mathematician of the Newtonian school. He was born 1682, at Burbach in Leicestershire, where his father was rector. At 12 years of age he discovered a strong inclination to the mathematics; which induced his uncle, the Rev. Mr. John Smith, to take him to his house in Lincolnshire, to assist him in those studies. From hence he was removed to St. Paul's school, London, and in due time to Trinity College, Cambridge, where he took his degrees, and became fellow. In 1706 he was appointed professor of astronomy and experimental philosophy, on the foundation of Dr. Plume, Archdeacon of Rochester; being the first that enjoyed that office. In 1713 he entered into orders; and the same year at the desire of Dr. Bentley, he published the second edition of Newton's Principia, inserting in it all the improvements the author had made to that time. To this edition Mr. Cotes prefixed an excellent preface, in which he pointed out the true method of

collection of the author's works, published in 1722, by Dr. Smith, under the title of Harmonia Mensurarum, making the first part of that work, which has long been in the hands of mathematicians.

An Extract from the Acta Eruditorum for the Month of March, 1713, *p.* 111. *De Contagiosâ Epidemiâ, quæ in Patavino Agro et totâ fere Venetâ ditione in Boves irrepsit, Dissertatio. Auctore Bernardino Ramazzini, Practicæ Medicinæ Professore Publico. Patavii,* 1712, *8vo.* N° 338, p. 46.

A year and a half before the publication of this treatise, a dreadful and violent contagion seized the black cattle in the Venetian territories, and especially in the neighbourhood of Padua. Like an increasing fire, it could neither be extinguished nor stopped by any human means.

It was first observed in Agro Vincentino, and soon discovered itself more openly in the country, spreading every way, even to the very suburbs of Padua, with a dreadful destruction of the cows and oxen. It also extended to some parts of Germany, and still continued at the above-mentioned date in the territory of Milan.

In this dissertation the author inquires into the causes of the distemper, and points out what remedies should be used for putting a stop to it. According to the account here given, this distemper among the horned cattle was a malignant pestilential fever, accompanied with rigors, followed by a burning heat, quick pulse, difficulty of breathing, &c.

The author deduces this distemper from a contagious original. He says, it is certain, that out of a great drove, such as the merchants bring yearly into Italy, out of Dalmatia and the bordering countries, one beast happened to

philosophizing, exhibiting the foundation on which the Newtonian philosophy was raised, and refuting the objections of the Cartesians and all other philosophers against it.

The publication of this edition of the Principia added greatly to Mr. Cotes's reputation; which was also much increased by several publications of his own, which soon after appeared; particularly the above paper, the Logometria, in this volume of the Transactions, and another, on the great fiery meteor, seen March the 6th, 1716, in vol. 31. His career, however, was soon arrested by the hand of death, in this same year, in the very prime of life, in the 34th year of his age, to the great regret of all lovers of the sciences. So high, indeed, was Newton's opinion of our author's genius, that he used to say, "had Cotes lived, we should have known something."

Mr. Cotes left behind him some ingenious and vauable tracts; part of which, with the Logometrial were published in 1722, by Dr. Rob. Smith, his cousin, and successor in his professorship, and afterward master of Trinity College, under the title of Harmonia Mensurasum, which contains a number of very ingenious and learned works. He wrote also a Compendium of Arithmetic; likewise on the Resolution of Equations; on Dioptrics; and on the Nature of Curves. Besides these tracts, he drew up in the time of his lectures, a course of hydrostatical and pneumatical lectures, in English, published also by Dr. Smith in 1737, which are still held in great estimation.

straggle from the rest, and be left behind, which a cowherd finding, brought to a farm belonging to Count Borromeo, Canon of Padua. This beast infected all the cows and oxen of the place where he was taken in, and died itself in a few days, as did all the rest, except one, which had a rowel put into its neck.

In the dead bodies of all the cattle, it was particularly observed, that in the omasus, or paunch, there was found a hard compact body, firmly adhering to the coats of the ventricle, of a large bulk, and an intolerable smell; in other parts, as in the brain, lungs, &c. were several hydatides, and large bladders filled only with wind, which being opened gave a deadly stench; there were also ulcers at the root of the tongue, and bladders filled with a serum on its sides. This hard and compact body, like chalk, in the omasus, the author takes to be the first product of the contagious miasma. He adds a prognostic, believing that, from so many experiments, and the method observed in the cure, a specific remedy will at last be found out, to extirpate the poisonous malignity. He does not think this contagion can affect human bodies, since even other species of ruminating animals, symbolizing with the cow-kind, are yet untouched by it, nor was the infection caught from the air, where due care was taken in burying the dead bodies.

As for the cure: for the surgical part he commends bleeding, burning on both sides the neck with a broad red-hot iron, making holes in the ears with a round iron, and putting the root of hellebore in the hole, a rowel or seton under the chin, in the dew-laps, he also orders the tongue or palate to be often washed and rubbed with vinegar and salt.

For the pharmaceutical part he recommends alexipharmics and specific cordials; and from the vegetable kingdom, 3 ounces of Jesuit's bark, infused in 10 or 12 pints of cordial water, or small wine, to be given in 4 or 5 doses, which is to be done in the beginning of the fever, when the beast begins to sicken. From the animal, ʒii of spermaceti dissolved in warm water. From the mineral, antimonium diaphoreticum. Against worms breeding, an infusion of quicksilver, or petroleum and milk is to be given. And lastly, as to the food, drinks made with barley or wheat flour, or bread, like a ptisane, fresh sweet hay made in May and macerated in fair water. In the mean time the cattle must be kept in a warm place, and clothed, to keep them as much as possible from the cold air, daily making fumigations in the cowhouses with juniper berries, galbanum, and the like. As to prevention, he enjoins care in cleaning the stalls, and scraping the crust from the walls; care also is to be taken of their food, that it be good, the hay and straw not spoiled by rain in the making, and judges their food ought to be but sparing; friction, rubbing, and currying, not only with the hand, but with a currycomb and brush, with setons under their chin, made with a hot iron run through the part, and kept open by a cord put through it.

A Recipe: or the Ingredients of a Medicine for the spreading mortal Distemper among Cows; lately sent over from Holland, where a like Distemper raged among the Black Cattle. N° 338, p. 50.

Recipe veronicæ, pulmonariæ, hyssopi, scordii, ana m. iv. rad. aristolochiæ rotundæ, gentianæ, angelicæ, petasitidis, tormentillæ, carlinæ, ana unc. 12. bac. lauri et juniperi, ana unc. 12. Misc. fiat pulvis.

Bleed the cow, and give her every morning, for 3 or 4 mornings, an ounce of this powder with a horn in warm beer. If the cow's illness continues, after an omission of 2 or 3 days, repeat the medicine for 3 or 4 days again.

A new Method for making Logarithms, and vice versâ, for finding the Number corresponding to a Logarithm given, by help of the following Table. By Mr. John Long, C. C. Coll. Oxon. N° 339, p. 52.

Log.	Nat. Numb.	Log.	Nat. Numb.	Log.	Nat. Numb.
0,9	....7.943282347	0,003	....1.006931669	0.000006	....1.000013816
0,8	....6.309573445	0,002	....1.004615794	0,000005	....1.000011513
0,7	....5.011872336	0,001	....1.002305238	0,000004	....1.000009210
0,6	....3.981071706	0,0009	....1.002074475	0,000003	....1.000006908
0,5	....3.162277660	0,0008	....1.001843766	0,000002	....1.000004605
0,4	....2.511886432	0,0007	....1.001613109	0,000001	....1.000002302
0,3	....1.995262315	0,0006	....1.001382506	0,0000009	....1.000002072
0,2	....1.584893193	0,0005	....1.001151956	0,0000008	....1.000001842
0,1	....1.258925412	0,0004	....1.000921459	0,0000007	....1.000001611
0,09	....1.230268771	0,0003	....1.000691015	0,0000006	....1.000001381
0,08	....1.202264435	0,0002	....1.000460623	0,0000005	....1.000001151
0,07	....1.174897555	0,0001	....1.000230285	0,0000004	....1.000000921
0,06	....1.148153621	0,00009	....1.000207254	0,0000003	....1.000000690
0,05	....1.122018454	0,00008	....1.000184224	0,0000002	....1.000000460
0,04	....1.096478196	0,00007	....1.000161194	0,0000001	....1.000000230
0,03	....1.071519305	0,00006	....1.000138165	0,00000009	....1.000000207
0,02	....1.047128548	0,00005	....1.000115136	0,00000008	....1.000000184
0,01	....1.023292992	0,00004	....1.000092106	0,00000007	....1.000000161
0,009	....1.020939484	0,00003	....1.000069080	0,00000006	....1.000000138
0,008	....1.018591388	0,00002	....1.000046053	0,00000005	....1.000000115
0,007	....1.016248694	0,00001	....1.000023026	0,00000004	....1.000000092
0,006	....1.013911386	0,000009	....1.000020724	0,00000003	....1.000000069
0,005	....1.011579454	0,000008	....1.000018421	0,00000002	....1.000000046
0,004	....1.009252886	0,000007	....1.000016118	0,00000001	....1.000000023

This table is what I sometimes make use of for finding the logarithm of any number proposed, and vice versâ. For instance, suppose I had occasion to find the logarithm of 2000. I look in the first class of my table, which consists of 8 classes, for the next less to 2, which is 1.995262315, and against it is 3, which consequently is the first figure of the logarithm sought. Again, dividing the number proposed 2 by 1.995262315, the number found in the table, the quotient is 1.002374467; which being looked for in the second class of the table, and finding neither its equal nor a less, I add 0 to the part of the logarithm before found, and look for the said quotient 1.002374467 in the third class, where the next less is 1.002305238, and against it is 1, to be added to the part of the logarithm already found; and dividing the quotient 1.002374467 by 1.002305238, last found in the table, the quotient is 1.000069070; which being sought in the fourth class gives 0, but being sought in the fifth class gives 2, to be added to the part of the logarithm already found; and dividing the last quotient by the number last found in the table, viz. 1.000046053, the quotient is 1.000023015, which being sought in the sixth class, gives 9 to the part of the logarithm already found; and dividing the last quotient by the new divisor, viz. 1.000002072, the quotient is 1.000000219, which being greater than 1.000000115, shows that the logarithm already found, viz. 3.3010299, is less than the truth by more than half a unit; therefore adding 1, we have Briggs's logarithm of 2000, viz. 3.3010300.

If any logarithm be given, suppose 3.3010300, omit the characteristic, then opposite these figures 3. . .0. . 1. . .0. . 3. . 0. . 0, we have in their respective classes 1.995262315. . . . .0. . . . .1.002305238. . . . . . 0. . . . . 1.000069080. . . . 0. . .0 which multiplied continually into one another, the product is 2.000000019966, which because the characteristic is 3, becomes 2000.00 0019966 &c. that is, 2000, the natural number sought.

It is obvious, that these classes of numbers, are no other than so many scales of mean proportionals: in the first class, between 1 and 10; so that the last number thereof, viz. 1.258925412, is the 10th root of 10, and the rest in order ascending are its powers. So in the second class, the last number 1.023292992 is the 100th root of 10, and the rest in the same manner are its powers. So 1.002305238 in the third class, is the 10th root of the last of the second, and the rest its powers, &c. Or, which is the same, each number in the preceding class, is the 10th power of the corresponding number in the next following class: whence it is plain, that to construct these tables, requires only one extraction of the 5th or sursolid root for each class, the rest of the work being done by the common rule of arithmetic; and for extracting the 5th root, we find more than one very compendious rule in N° 210 of these Transactions.

The process is exactly the reverse of Mr. Briggs's Doctrine, in cap. 14, of his Arithmetica Logarithmica; and had Briggs been apprized of it, it would have greatly eased the labour of deducing the logarithms of the first prime numbers, which appear to have cost him so much pains.

Microscopical Observations on the Fibres of the Muscles. By Mr. Leuwenhoeck, F.R.S. N° 339, p. 55.

I send you a copy of my observations, concerning the membranes with which the fibrillæ of the muscles appear to be encompassed, both in the flesh of a whale, cod-fish, salmon, and smelt; and also in that of beasts, from an ox to a mouse; in all which the appearance was the same.

The flesh of the whale, was a small piece cut off near the tail. Viewing this through the microscope, I judged its fibres to be 4 times as large as those I had formerly observed in another piece of whale's flesh, taken from another part of the fish; which made me consider, whether the fibres of that part might not be the larger, for its greater strength. Cutting the said fleshy parts lengthwise, and across the fibres, I discovered more plainly than before, that each flesh-fibre was enwrapped in a fine thin membrane.

To have a better idea of these flesh-fibres of a whale, I cut a thin slice of it across, which I laid on a wetted piece of glass, that the flesh, which was very dry and shrunk, might be swelled by the moisture, and thereby distended to the natural size on the body of the fish itself. In this state, placed before the microscope, it appeared as represented in fig. 9, pl. 3, in which the parts lay so close together, that their encompassing membranes, represented by the black lines, were but just discernible, some of them however appearing larger than others: these, when attentively viewed, seemed plainly to be divided into multitudes of others, cut also transverse, the size of which was no larger than a common sand to the naked eye. These were so close crowded together, that their figure was very irregular, and their sizes different; for though each seemed encompassed with six others, yet some of them were twice as large as others.

Having formerly mentioned the slenderness of these fibrillæ in the flesh of a whale, and judging these to be 4 times as large, I took a thin slice of the formerly mentioned whale-flesh, which I had still kept by me, and after having made it thoroughly wet, I viewed it with the same microscope as I had done this of the tail. This appeared as is represented fig. 10. When the moisture dried away from these slices, so stuck on to the glass, the particles became much smaller, and the membranes with which each was encompassed, became very visible; viz. those that were not shrunk away; which was a very entertaining object to the curious; and as often as I made new cuts, a new object presented itself.

Fig. 11, represents a small part of this flesh. These particles seemed to touch and be joined to others; but now being dried, they shrunk in from the membranes about them; which membranes could not shrink, being all joined to one another. Along these flesh-fibres there run some membranes, of the thickness of a hair or more, scarcely distant the breadth of a sand from each other; from these larger membranes other parts are spread, dividing each fibre into numerous fibrils; so that it may be said, that each flesh-fibre, no thicker than a hair, is a small muscle, encompassed by its peculiar coat or membrane. And as the painter had not the same idea of the size of these fibres, as I and some other persons had, I made him draw a small bit as large as it appeared to my apprehension, as in fig. 12, whence appears the difference between one man's sight and another.

I have also often seen some few of these fibres, though joined to others, yet only the 4th part of the size of those to which they were joined.

When I again moistened those represented in fig. 11 and 12, (that were dried and shrunk up) they again became so distended, as to fill up the spaces between the membranes, and re-assume the shape they had before they were dried. Among several little pieces of flesh placed before another microscope, and moistened as before, there was one, whose particles were not separated on drying, which I supposed to be, from the splitting and tearing asunder of a large membrane, that ran through the middle of it, as in fig. 13, where between S, T, and V, the dried particles remain unseparated; these being cut a little thicker appeared also of a darker hue, and had they been sliced yet thicker, they would have appeared of a dark red. SW represents the thick membrane dividing this piece, which was about the size of a hair; this at T sent out a branch, and near W is split into two; I apprehend that a great number of blood-vessels are spread over this membrane, which by their smallness are not visible; for it is by these the nourishment is conveyed. Between RS and QW the exceedingly fine membranes torn from the great are visible. It is amazing that in so huge an animal as a whale, such exceedingly small fibrils should be found; nay, the same as they are in small animals; and that the whole 13th figure is not so large as a coarse grain of sand.

I caused a very little piece, consisting only of 5 fibrils, to be drawn lengthwise, as they were seen through the microscope, and represented in fig. 14, in which figure about A, it is divided into two fibrils. Between C and F are to be seen the little membranes which encompass the fibrils, which are here torn asunder.

I have frequently with pleasure observed these flesh-fibres lengthwise, as it were corrugated or wrinkled, which I imagined to be the representation of their unbent position; and still more, when the part to which they belong is bowed together, or brought nearer; but when the muscle is extended, and its antagonist acts, there is not the least wrinkle observable in these fibrils. However, all the little inequalities in these fibrils must not be taken for those corrugations, since many of them are only the particles torn off from the membranes which encompass the fibrils.

Fig. 15, represents 4 small fibres of a piece of whale's flesh, procured 2 years since: this I caused to be drawn, to show the difference. By the two figures 14 and 15, it is visible that the diameters of the fibres are as thick again in one as in the other, therefore the fibres must be four times as large in fig. 14 as in fig. 15. Now each flesh fibre being composed of a great many smaller fibrils, we may imagine each of these inclosed fibres likewise to consist of others of the like nature.

I have again viewed several small fibres of ox-flesh, and observed, that each of the fibrils in them was encompassed with a thin membrane. But I cannot show these membranes so clearly to other persons in cow's flesh as in whale's flesh, because the parts of the former are of a much more compact and close texture, than that of the whale, from whence they do not shrink so much in drying.

Several Observations on the Frame and Texture of the Muscles. By Mr. Muys of Franequer. Extracted from the Journal Litteraire for January and February 1714. N° 339, p. 59.

Mr. Muys observed, that the fleshy fibres of the muscles are composed of other smaller fibres, which he calls fibrils; that these fibrils are of the size of a slender hair, and that 500 or 600 of them, may be counted in one fleshy fibre, whose diameter is no more than a 24th part of an inch. That each of these fibrils is also composed of more than 300 small transparent tubuli, but so slender, that if a blood globule, which, according to Mr. Leuwenhoeck, is but the millionth part of a grain of sand, were divided into 24 parts, one of these could hardly pass through these small tubes.

He has shown, that though the fleshy fibres of the muscles are joined to the tendons and tendinous membrane of a muscle, yet these tendinous fibres are not a continuation of the fleshy ones, as most anatomists suppose, which he proves thus: if by means of a wooden knife, or only by pulling it, you separate the fleshy fibres from the tendon, the end of the tendon to which they were joined will remain smooth and even, and not rugged.

Having made several injections of warm water into the crural artery of a lamb of a year old, all the fleshy fibres became entirely white. He then injected a coloured liquor by the same artery; and then not only the small arteries appeared filled with this tinged liquor, but he found also that the liquor passed through each fibre, either in a serpentine manner, or undulating, or forming several angles, or joined by a great number of anastomoses. He observed also, that many small branches of the arteries, which before could not be seen, appeared visibly, spread all round the little fibrils, and tinged with the same colour. Having remarked, that the parts of the fleshy fibres, which were near the extremities of the arteries, appeared tinged with the liquor, he examined them with a microscope, and found the little fibrils filled and tinged with the same liquor, and yet there was not the least appearance of the liquor in the interstices between the fibrils.

Having made injections by the crural artery, of another coloured liquor, in the muscles, whitened, as before, with water, he saw not only the fibres in some of the muscles, and the most part of them in the others filled with this matter; but having examined them with a good microscope, he found the fibrils and even the least tubuli which compose them, filled and tinged with the same matter; and yet the small ramifications of the nerves appeared perfectly white.

It results from all these observations, 1st. That the small tubes, which make a fibril, are really hollow, and that the extremities of the capillary arteries open into them, and empty there a part of their liquor, which is reconveyed by the veins to the heart. 2d. That the blood globules must be divided into an almost infinite degree of smallness, before they can enter and pass these tubuli. That the blood globules may be so divided, and when so divided, pass through the small tubuli, is evident from the redness of the fibres and fibrils of animals, which have a red flesh; which will be no surprise to those who have read Mr. Leuwenhoeck's letter, where he says, that these globules divide themselves after this manner, to pass through the last extremities of the capillary arteries of the brain, nor to those who know that the globules are extremely soft, and easily separable, as M. Muys has evinced by arguments grounded on very curious observations.

An Account of several Observations made in New England, in 1712. *By Dr. Mather.* N° 339, p. 62.

Dr. Mather inclines to the opinion of there having been, in the antediluvian world, giants, or men of very large and prodigious stature, by the bones and teeth of some large animals, found in Albany, in New England, which he judges to be human; particularly a tooth, which was a very large grinder, weighing 4 pounds and 3 quarters, with a bone, supposed to be a thigh-bone, 17 feet long. He also mentions another tooth, broad and flat like a fore-tooth, 4 fingers in breadth: the bones crumbled to pieces in the air after dug up; they were found near a place called Cluverack, about 30 miles on this side of Albany. He then gives the description of one, which he resembles to the eye-tooth of a man; he says it has 4 prongs, or roots, flat, and somewhat worn on the top; it was $5\frac{7}{8}$ inches high, as it stood upright on its root, and almost 13 inches in circumference; it weighed 2lbs. 4 oz. Troy weight: there was another, near a pound heavier, found under the bank of Hudson's River, about 50 leagues from the sea, a great way below the surface of the earth, where the ground is of a different colour and substance from the other ground, for 75 feet long, which they suppose to be from the rotting of the body, to which these bones and teeth he supposes once belonged.

The Doctor conjectures that the Shittim wood, mentioned in the Scriptures, to be made use of for the Ark, &c. and said not to be subject to rot, like most other woods, was the black Acacia; that the Gopher wood, was the Juniperus arbor tetragonophyllos, frequent in the East Indies, &c. He observes that the Indians often perform great cures with their plants; of which there is a great variety, differing from the European. He instances in some; as, a plant efficacious in curing inflammations, whence they call it antierisypelas, which grows plentifully in the woods: a chemical oil extracted from it, taken inwardly, performs wonders in absorbing scorbutic salts. Another plant, which goes by the name of partridge-berries, is excellent in curing the dropsy; a decoction of the leaves, being drank several days as a tea, discharging a vast quantity of urine, as long as the disease lasts; after which it may be drank without observably provoking urine: gouty persons drink it with benefit. The root called the bleeding root, curing the jaundice in 5 or 6 days. Another for gangrenes, which he does not name. Another specific for the bite of the rattle-snake, and another for quinsies, or sore throats. A plant, called by the Indians, taututtipang; infallible for the lues venerea, the root being used in a decoction, and half a pint drunk; a cataplasm of the same root, bruised, applied to the ulcers, cures them also. A thistle, called the boar-thistle; very short and prickly, with a large and long root. To this they add a root, called the cancer root, and a sort of devils-bit: a decoction of which three roots is a cure for the king's evil, though very far gone: a small quantity being drunk every day, and the bruised roots applied to the scrophulous tumours.

As to the birds, they have many of the same species with those in England. He mentions very large wild turkies, some weighing 50 or 60lb. but the flesh is very tough and hard. He takes notice of a very large eagle with a great head, soaring very high, as all of that genus do. As to the itinerants, or birds of passage, there are vast flights of pigeons, coming and departing at certain seasons.

As to antipathies, and the force of imagination; he says, a gentlewoman of his neighbourhood swoons upon the seeing any one cut their nails with a knife; but when done with a pair of scissars, it has no effect on her, &c.

He observes, that the Indians have no division of time, except by sleeps, moons and winters. Though the Indians have not distinguished the stars into constellations, yet it is observable that they call the stars of Ursa Major, Paukunawaw, that is, the Bear; and this long before they had any communication with Europeans. He says, there is a tradition among them, that in November 1668, a star appeared below the body of the moon, within its horns. That the evening glade, first noticed by Dr. Childrey, is constantly observed there in February, and a little before and after that month; adding, that the cause of that appearance must be sought for above the atmosphere.

As to the appearances of several uncommon rainbows and mock suns: on the 2d of January, in a clear sky, but very cold, the sun was from 10 o'clock, for near 3 hours after, attended with 4 parhelia, in the middle of which were two rainbows. About 6 weeks after, in a day much colder than used to be at that time of the year, the air a little hazy, a little after one o'clock, for about half an hour, 4 mock-suns were seen.

As to earthquakes, which though they have not done the mischiefs frequent in Sicily, Italy, &c. yet they have had several very sensible shocks. In the year 1663, they had 6 or 7 violent shakes in the space of 3 days: a town lying on the river Connecticut, has had scores of them in a year, for many years together. The Indians affirm, that several rivers have not only been stopped in their course, and diverted, but some wholly swallowed up by earthquakes. He mentions as an accident sometimes happening to them in the winter, that it has rained plentifully, and at night frozen so extremely, that the weight of the icicles has broken the limbs of the trees, and not unfrequently split their trunks. Though they have not those hurricanes to which the Caribbe Islands are subject, yet they have had whirlwinds, or gusts, drive along a particular narrow tract, for several miles together, with a violence not to be opposed by any thing on earth; that if their towns had stood in the way, they must undoubtedly have been destroyed. Of these, he says, a thick dark, small cloud has arisen, with a pillar of light in it, of about 8 or 10 feet diameter, and passed along the ground in a track not wider than a street, tearing up trees by the roots, blowing them up in the air like feathers, and throwing up stones of a great weight to a considerable height in the air, and throwing down all in its passage; the noise this cloud made was so great all the while, that the noise of the mischiefs done by it, was as nothing.

The Doctor gives a calculation of the possible increase of the descendants of Adam; and from this introduction proceeds to the account of some long-lived persons there, and of their fruitfulness. He says it is no rare thing for an aged gentlewoman to see more than 100 of her offspring. He mentions one woman that had 23 children, of which 19 lived to man's estate. Another that had 27; another 26, of which 21 were sons, one whereof was Sir William Phipps; another 39 children. Here he gives several instances of persons living to above 100 years of age. One Clement Weaver lived to 110, his wife being upwards of 100. This man, to the last year, could carry a bushel of wheat to the mill, above 2 miles. He relates the case of an old man, above 100, who lost the memory of several of the latter years of his life, but very well retained the remembrance of what passed in his younger days.

An Account of the Procuring of the Small Pox by Incision, or Inoculation; as it has for some time been practised at Constantinople. Being an Extract of a Letter from Emanuel Timonius, Oxon. and Patav. M. D. S. R. S. dated Constantinople, December,* 1713. *Communicated by John Woodward, M. D. and S. R. S.* N° 339, p. 72.

The Doctor observes, that the Circassians, Georgians, and other Asiatics, have introduced this practice of procuring the small-pox by a sort of inoculation, for about 40 years, among the Turks and others at Constantinople. That though at first the more prudent were very cautious in the use of this practice; yet the happy success it has been found to have in thousands of subjects, for

* Emanuel Timoni was a native of Italy, but practised at Constantinople. He had travelled into various parts of Europe, and had visited England. Besides this account of inoculation, he also communicated to the Royal Society a narrative of the plague, which raged at Constantinople in 1714. This paper is inserted in the 31st vol. of the Phil. Trans. He had for many years enjoyed a high and well-merited reputation; but meeting at length with some unexpected check, he destroyed himself in a fit of desperation.

The communications of Timoni and Pylarini on the subject of inoculation, are both inserted in the same vol. of the Phil. Trans. although Timoni's appears to have been of an anterior date. These two accounts may be considered as the first that were published in England, upon this interesting subject; for although (as Dr. Woodville has remarked in his History of Inoculation, p. 71) Mr. Kennedy had described, in his Essay on External Remedies, published in 1715, *the manner of ingrafting the small-pox* as practised in Turkey; yet it is evident from that author's own words at p. 155 of his essay, that his knowledge on this subject was derived from Timoni, who resided at Constantinople, when Mr. Kennedy visited that capital, and who had previously inoculated there his two sisters.

these 8 years past, has now put it out of all suspicion and doubt; since the operation having been performed on persons of all ages, sexes, and different temperaments, and even in the worst constitution of the air, yet none have been found to die of it; where at the same time it was very mortal when it seized the patient the common way, of which half the affected died. This the Doctor attests from his own observation.

He next observes, that such as have this inoculation practised on them, are subject to very slight symptoms, some being scarcely sensible they are ill or sick; and, what is valued by the fair, it never leaves any scars or pits in the face.

The method of the operation is thus: choice being made of a proper contagion, the matter of the pustules is to be communicated to the person proposed to take the infection; whence it has, metaphorically, the name of incision or inoculation. For this purpose, they pitch upon some boy, or young lad, of a sound healthy temperament, that is seized with the common small-pox (of the distinct, not confluent kind) on the 12th or 13th day from the beginning of his sickness; they with a needle prick the tubercles (chiefly those on the shins and hams) and press out the matter coming from them into some convenient glass vessel, or the like, to receive it; and it is proper to wash and clean the vessel first with warm water: a convenient quantity of this matter being thus collected, is to be stopped close, and kept warm in the bosom of the person that carries it, and, as soon as may be brought to the place of the expecting future patient; who being in a warm chamber, the operator is to make several little wounds with a needle, in one, two or more places of the skin, till some drops of blood follow, and immediately drop out some drops of the matter in the glass, and mix it well with the blood issuing out; one drop of the matter is sufficient for each place pricked. These punctures are made indifferently in any of the fleshy parts, but succeed best in the muscles of the arm or radius. The needle is to be a three-edged surgeon's needle; it may likewise be performed with a lancet: the custom is to run the needle transverse, and rip up the skin a little, that there may be a convenient dividing of the part, and the mixing of the matter with the blood more easily performed; which is done, either with a blunt stile, or an ear-picker: the wound is covered with half a walnut-shell, or the like concave vessel, and bound over, that the matter be not rubbed off by the clothes. The patient is to be careful of his diet. In this place the custom is, to abstain wholly from flesh and broth for 3 or 4 weeks. This operation is performed, either in the beginning of the winter, or in the spring. Some, for caution, order the matter to be brought from the sick by a 3d person, lest any infection should be conveyed by the clothes of the operator; but this is not material.

As to the process of this matter, in respect of the idiosyncrasy; the small-pox begins to appear sooner in some than in others, in some with greater, in others with less symptoms; but with happy success in all. In this place the efflorescence commonly begins at the end of the 7th day, which seems to favour the doctrine of crises. It was observed, in a year when the common small-pox was very mortal, that those by incision were also attended with greater symptoms. Of 50 persons, who had the incision made on them almost in the same day, 4 were found in whom the eruption was too sudden, the tubercles more, and symptoms worse. There was some suspicion, that these 4 had caught the common small-pox before the incision was made. It is enough for our present purpose, that there was not one but recovered after the incision: in those 4 the small-pox came near the confluent kind. At other times, the inoculated are distinct, few and scattered; commonly 10 or 20 break out; here and there one has but 2 or 3, few have 100: there are some in whom no pustule rises, but in the places where the incision was made, which swell up into purulent tubercles; yet these have never had the small-pox afterwards in their whole lives; though they have cohabited with persons having it.

It is to be noted, that a no small quantity of matter runs for several days from the place of the incision. The pocks arising from this operation are dried up in a short time, and fall off, partly in thin skins, and partly contrary to the common sort, vanish by an insensible wasting. The matter is hardly a thick pus, as in the common, but a thinner kind of sanies; whence they rarely pit, except at the place of the incision, where the cicatrices left are not to be worn out by time, and whose matter comes near the nature of pus. If an apostem breaks out on any (which infants are most subject to) yet there is nothing to be feared, for it is safely healed by suppuration. If any other symptom happens, it is easily cured by the common remedies. Observe, they scarcely ever make use of the matter of the inoculated pox, for a new incision. If this inoculation be made on persons who have before had the small-pox, they find no alteration, and the places pricked presently dry up; except in an ill habit of body, where possibly a slight inflammation and exulceration may happen for a few days.

To this time, he says, I have known but one boy, on whom the operation was performed, and yet he had not the small-pox, but without any mischief; and some months after catching the common sort, he did very well. It is to be observed, that the places of the incision did not swell. I suspect this child prevented the insertion of the matter, for he struggled very much under the operation, and there wanted help to hold him still. The matter to be inserted will keep in the glass very well for 12 hours. I have never (he adds) observed any mischievous accident from this incision hitherto; and although such reports have been sometimes spread among the vulgar, yet having gone on purpose to the houses whence such rumors have arisen, I have found the whole to be absolutely false. It is now 8 years since I have been an eye-witness of these operations; and to give a greater proof of the sedulity I have used in this disquisition, I shall relate 2 histories.

In a certain family, a boy of 3 years old, was afflicted with the falling-sickness, the king's-evil, an hereditary pox, and a long marasmus. The parents were desirous to have the incision made upon him; the small-pox were thrown off with ease; about the 40th day he died of his marasmus. In another family, a girl of 3 years old, was troubled with the like fits, strumous, attended with an hereditary lues, and labouring under a colliquative looseness for 3 months. The operation was performed on this child; she came off very well of the small-pox, which was all over the 15th day; on the 32d she died of her looseness, which had never left her the whole time. But it is true, I never maintained the inoculation as a panacea, or cure for all diseases; nor do I think it proper to be attempted on persons like to die.

Then follows a long ætiologia respecting the manner in which this author supposes the variolous contagion to act upon the human body. He supposes it to act upon the mass of blood after the manner of a ferment or leven, and that all the symptoms which take place in the natural or inoculated small-pox are to be accounted for in the 1st instance, from the commotion excited in the blood, (and thence upon the constitution at large); in the next place from the defecation or separation of the vitiated particles from the healthy, &c. &c. Respecting a theory now universally exploded, it cannot be necessary to enter into further particulars.

Theoremata quædam infinitam Materiæ Divisibilitatem spectantia, quæ ejusdem raritatem et tenuem compositionem demonstrant, quorum ope plurimæ in Physica tolluntur difficultates. A Johanne Keill, M. D. S. R. S. N° 339, p. 82.

These theorems on the divisibility of matter, and the tenuity of its composition, were afterwards printed in the author's introduction to Natural Philo sophy, or Philosophical Lectures, where the subject is given more fully, and where it forms part of the 5th Lecture. See p.   of the Latin edition, or p. 63 of the English one.

Observations made at Rome, Nov. 21 in the Morning, N. S. 1713, on the Occultation of the Star τ at the Root of the Bull's Northern Horn, by the Moon's Disk; also an Eclipse of the Moon presently after; with some Emersions of Jupiter's First Satellite out of his Shadow. By Sig. Bianchini, F.R.S. N° 340, p. 88. *Translated from the Latin.*

At 12h. 54m. 34s., the star marked τ by Bayer was just occultated by that part of the moon nearly in the middle between the maculæ of Aristarchus and Galilæus.

At 14h. 0m. 14s. Sirius is in the meridian; whence the times are verified.

At 14h. 32m. 57s., the star τ, which had emerged some minutes from behind the moon, in its diurnal revolution, precedes the moon's western limb by 33s. of time, and the moon's centre by 103s. or 1m. 43s. of time.

At 15h. the penumbra on the moon's limb, which before was more dilute, gradually becomes more dense.

At 15h. 4m. 20s. the moon, in that part of her limb next the macula Schiccardi, begins to enter into the true shadow.

At 17h. 27m. 45s. the moon quite emerged from the true shadow.

Sept. $\frac{11}{22}$, at 8h. 38m. 20s. anno 1713, at Rome, Jupiter's first satellite emerged.—At 8h. 44m. the 3d satellite exactly covered the 4th.—Sept. $\frac{18}{29}$, at 10h. 36m. 23s. the first satellite emerged out of Jupiter's shadow.—Nov. $\frac{12}{23}$, at 7h. 32m. 22s. the first satellite emerged.—Nov. 28 O. S. or Dec. 9 N. S. at 5h. 45s. the first satellite emerged.—Dec. 21 O. S. at 5h. 50m. 22s. the first satellite again emerged from Jupiter's shadow.

From these observations, accurately calculated, it is plain, that the second equation, which we suppose to arise from the progressive motion of light, does necessarily take place: for, after 57 revolutions of the first satellite, in which Jupiter receded from the earth more than the radius of the orbis magnus, the last eclipse was seen almost 9 minutes later than it should have been, according to the tenor of the first observation; which agrees with Cassini's Hypotheses. From the same observations it is also plain, that the motion of Jupiter's first satellite is somewhat swifter than by Cassini's very accurate tables; yet that inconsiderable error seems scarcely to exceed 2 minutes of time, in each revolution of Jupiter, or in 12 years, by which the heavens anticipate Cassini's calculation: but with this correction, the agreement will be pretty accurate.

Observations on Mr. John Bernoulli's Remarks on the Inverse Problem of Centripetal Forces, in the Memoirs of the Academy of Paris for the Year 1710; *with a New Solution of the same Problem. By John Keill, M. D.* N° 340, p. 91.

To determine the curve described by a body, which is urged by a given law of centripetal force, when projected with a given velocity from a given place, in the direction of a given right line, is a most noble problem. In the Principia, Newton long since gave a complete solution of it, granting the quadrature of curve figures. Since that, the celebrated Mr. John Bernoulli has also undertaken the same problem, in the Memoirs of the Paris Academy for 1710. Now having compared his solution with that of Newton, I have made the following remarks upon them.

M. Bernoulli premises the same proposition as employed by Newton, for demonstrating his problem, which is the 40th in his Principia, and is no less elegant than easy to be demonstrated. It is this, viz. if a body be any how moved by a centripetal force, and another body ascend or descend directly; and if their velocities be equal in any case of equal altitudes, then will their velocities be equal at all equal altitudes.

M. Bernoulli says, the demonstration of this proposition is delivered by Newton in too complex a manner, and therefore he substitutes his own instead of it, which he calls a more simple one. But permit me to say, without offence to so great a man, that if there be any difference between their demonstrations it is this, that Newton's seems to be much the easier and less complex of the two. For with the centre C, fig. 1, pl. 4, let the two circles DI and EK be described, at the very small distance DE from each other, and let the velocities of the bodies at D and I be equal; and if there be drawn NT perpendicular to IK; then Newton fully shows that the accelerating force in DE is to that in IK, as IN to IT. For if the force in DE or IN be represented by the right line DE or IN, then the force in IN is resolved into the two TI, TN, of which, that only which is as TI accelerates the motion in the direction IK. But the accelerations, or the increments of the velocities, are as the forces and as the times in which they are generated conjointly; and, because of the equal velocities in D and I, the times are as the spaces described DE, IK: therefore the acceleration in the motion of the bodies through the lines DE and IK, are as DE to IT and DE to IK conjointly; that is, as DE^2 or IN^2 to the rectangle $IT \times IK$: and therefore, because $IN^2 = IT \times IK$, the increments of the velocities are equal. Therefore the velocities in E and K are equal. And by the same argument the velocities are always found equal at equal distances. This is the sum of Newton's demonstration, which he explains so clearly, that few easier can be found, even among elementary propositions. But Bernoulli does not proceed thus. He is satisfied with saying, that mechanics show that the force in DE is to the force in IK, as IK to DE; and that mechanics show the increments of the velocities to be in the ratio of the forces and times conjointly; also that at the beginning of the motion supposing the velocities to be equal, the times are as the spaces described DE, IK; and hence, by a reasoning exactly like Newton's, he concludes that the increment of velocity, acquired by the body in describing IK, is to the increment of velocity in describing DE, as DE $\times$ IK to IK $\times$ DE; and therefore that the increments of the velocities will always be equal at equal distances.

But if he wished to give an easy demonstration for the sake of novices, he ought to have cited the mechanical proposition, and have accommodated it to the present case. And indeed there was occasion for amplifying, that this might be done by the theorem which he seems to intend, in which is treated the descent of bodies on inclined planes: for here no plane is given which may impede the direct descent of bodies: nay, so far is the body from being impeded by a plane, that on the contrary it is continually attracted by a certain force from the plane or tangent. Doubtless therefore the force of his reasoning would have been more plain, if, omitting his mechanical propositions, he had demonstrated the whole matter from its own first principles, as Newton has done. For by resolving the right angled triangle KNI into two equiangular triangles, it is KI to IN as IN to IT, and therefore, instead of the ratio IN to IT, he might have put the ratio of KI to IN or to DE.

If the body fall from any place A in the right line AC; and from its place E a perpendicular EG be always raised, which may be proportional to the centripetal force; and if BFG be the curve line which the point G always touches; Newton demonstrates (prop. 39 and 40 of the Principia) that the velocity of the body at any place E, is as the square root of the curvilineal area ABGE. Therefore if the velocity be called v, then v^2 will be as the area ABGE. And if P denote the greatest altitude to which the body revolving in the trajectory can ascend, when projected upwards from any point of it, with the velocity which it has there; and if A denote the distance of the body from the centre, in any other point of its orbit; and if the centripetal force be always as any power of A, suppose as A^{n-1}; then the velocity of the body, at every altitude A, will be as $\sqrt{nP^n - nA^n}$.

In like manner M. Bernoulli shows, that if the distance from the centre be called x, the velocity v, and the centripetal force φ, then will $v = \sqrt{ab - \text{flu.}\varphi\dot{x}}$; where it is plain from quadratures, that the area ABGE $= ab - \text{flu.}\ \varphi\dot{x}$. It is therefore all the same, whether the square of the velocity be expressed by the area ABGE, or by the quantity ab — flu. $\varphi\dot{x}$, which is equal to it. And if the centripetal force φ be as $n\text{A}^{n-1}$ or nx^{n-1}, it will be $ab = \text{P}$, and flu. $\varphi\dot{x} = \text{A}$; so that ab — flu. $\varphi\dot{x}$ is as the quantity $\text{P}_n - \text{A}^n$.

Let the body describe the curve VK by a centripetal force tending to C, and let there be given the circle VXY, described with the centre C and any radius CV. Let Q be a constant quantity, and put $\frac{\text{Q}}{\text{A}} = z$; also let KI be an element of the curve, IN or DE an element of the altitude, and XY an element of the arc: then Newton demonstrates, that the element of the arc, or XY, may be expressed by this formula $\frac{\text{Q} \times \text{IN} \times \text{CX}}{\text{A}^2 \sqrt{\text{ABGE} - z^2}}$. In like manner, from the premises, M. Bernoulli, putting the arc VX $= z$, and the altitude or distance $= x$, reduces the element of the arc to this formula, $\dot{z} = \frac{a^2 c\dot{x}}{\sqrt{abx^4 - x^4 \text{ flu. } \varphi\dot{x} - a^2c^2x^2}}$. Now even at first sight Newton's formula would seem rather more simple than Bernoulli's, as consisting of fewer terms; but on examining the matter more carefully, it is found that the two formulas exactly coincide, the difference being only in the notation of the quantities. For if for ab — flu. $\varphi\dot{x}$ be put ABGE, for ac put Q, and x for A, also a for CX, and $\dot{x}$ for IN; then is

$$\frac{a^2c\dot{x}}{\sqrt{abx^4 - x^4 \text{ flu. } \varphi\dot{x} - a^2c^2x^2}} = \frac{\text{Q} \times \text{CX} \times \text{IN}}{\sqrt{\text{A}^4 \times \text{ABGE} - \text{Q}^2\text{A}^2}} = \frac{\text{Q} \times \text{CX} \times \text{IN}}{\text{A}^2 \sqrt{\text{ABGE} - z^2}},$$ having put $\text{A}^2z^2 = \text{Q}^2$, which Newton does for a more commodious notation. Hence it appears that the two formulas differ no otherwise than as any thing written in Latin characters would differ from the same thing written in Greek characters.

After having delivered the general formula, M. Bernoulli descends to a particular case, in which the centripetal force is reciprocally as the square of the distance; and by various reductions and troublesome operations, he shows the construction of curves which may be described by that centripetal force, and by reducing them to equations, he proves they are the conic sections. After which, he complains that Newton supposes, without any demonstration, that curves described by such a force would be conic sections.

But it is impossible he could believe that Newton was unacquainted with the demonstration of that fact; for he very well knew that Newton was the first and only person who had treated of this doctrine of centripetal forces in a geometrical manner, and had brought it to such perfection, that after more than 20 years, very little has been added to it by the most excellent geometricians. Bernoulli knew also, that besides giving the general solution of the inverse problem, Newton had showed how curves might be constructed, which are described by a centripetal force decreasing in the triplicate ratio of the distance; and that therefore he could not be ignorant of that other case. Nor indeed can I understand with what reason Bernoulli objects to Newton, that he had omitted the demonstration of this case; since he himself has often proposed theorems, without any where giving their demonstrations; and why may not Newton do the same, when in haste to proceed to other matters? But now in the new edition of the Principia is his demonstration of this very thing, which, though very short, is yet much easier and clearer than that of Bernoulli.

Lastly, that Bernoulli might show the necessity of his demonstration of the inverse problem in this particular case, he thus adds: it must be considered, says he, that the force which causes a body to move in the logarithmic spiral, must be reciprocally as the cube of the distance from the centre; but it does not hence follow, that such curves must always be described by such forces, since the like forces may also be the cause that the body may move in the hyperbolic spiral.

Now it is truly surprising how this great man could imagine, that Newton ever drew such a consequence. For, besides the logarithmic spiral, Newton shows how other curves, different and infinite in number, may be formed, all of which may be described by the same centripetal force as the logarithmic spiral, and among these may be reckoned this very hyperbolic spiral, as we shall show hereafter.

And from hence Newton concludes, that the conic sections only can be described by a centripetal force which is reciprocally proportional to the square of the distance: because the curvature of any orbit is given, by having given the velocity the centripetal force, and the position of the tangent. And that having given the focus, the point of contact, and the position of the tangent, a conic section may always be described, which shall have a given curvature, is what I have shown in the Philos. Trans. Anno 1708. Therefore by virtue of this force the body shall move in this curve and no other: since a body setting out from the same place, in the same direction, with the same velocity, and urged by the same centripetal force, cannot describe diverse courses.

Dr. Keill then gives another solution of this problem, of the inverse method of centripetal forces, by means of a fluxionary process; and he also applies it to a particular case, in which the force is reciprocally as the cube of the distance, and at the same time produces a demonstration of cor. 3, prop. 41, of Newton's Principia. After which, he then adds as follows.

Concerning the areas described by bodies, by means of a centripetal force which is reciprocally as the cubes of the distances, my worthy colleague, the excellent geometrician, professor Halley, observes, that if bodies by this law describe different circles, or different hyperbolic spirals; the areas of the sectors, both in the circles and in the spirals, will always be equal when described in equal times. For the velocities of bodies moving in circles by this law, ought to be reciprocally proportional to the radii or distances; and therefore the arcs described in the same time will also be in the same reciprocal ratio of the radii; whence it easily appears, that the sectors described in the same time will be equal.

In all other curves, since the velocity is to the velocity of a body moving in a circle, at the same distance, as $\frac{ax}{b}$ to p, or as $\frac{a}{b}$IK to KN; while the body in its trajectory describes the lineola IK, another body, moving at the same distance, will describe an arc $= \frac{b}{a} \times$ KN; and the area of the circular sector, and that of the trajectory described in the same time, will be $\frac{b}{a} \times$ KN $\times \frac{1}{2}$CN and KN $\times \frac{1}{2}$CN, which two areas are in the given ratio of b to a. Therefore, when $a = b$, as it is in the hyperbolic spiral, the area so described, will always be equal to the area of the circular sector, described in an equal time.

Rules for correcting the usual Methods of computing Amounts and present Values by Compound as well as Simple Interest, and of stating Interest Accounts. Offered to Consideration by Thomas Watkins, Gent. F. R. S. N° 340, p. 111.

The computations of interest, and other accounts, being found in numberless small books, which are in every one's possession, it is no ways interesting to retain this paper on the present occasion.

An Account of the Rain which fell every Year at Upminster in Essex, for 18 Years; with Remarks on that of the Year 1714. By W. Derham, F.R.S. Also a Comparison of what has been observed of that kind at Paris. By M. De la Hire. N° 341, p. 130.

Last Year, 1713, having been so dry, that the ponds about Upminster were mostly dried up, and the springs very low, I made an extract from my registers of the weather, &c. of the quantity of rain which had fallen at Upminster the last 18 years; for which see the following table, which shows the depth in inches and centesimals of inches, or what height it would have been had it not been imbibed by the earth, or lessened by exhalations, but been suffered to have stagnated on the ground.

In 1704 the drought was so considerable at Venice, that they were forced to fetch their water in barks 5 leagues off, as far as the Brenta. Yet we have had several years drier than that at Upminster. But of all, none was comparable to 1714, in which the whole quantity of rain was only 11.19 inches; whereas the least quantity of any of the preceding 18 years exceeded 15 inches in depth.

What effects this drought has had on the bodies of animals, I leave others to judge. It is well known how contagious and fatal a distemper has raged among our black cattle, as also in many other parts of Europe. And I observed the itch was epidemical among the poorer sort, at the beginning of the year; that the measles were very common, some parts of the year; and that pleurisies and malignant fevers, infested a great many, especially in the summer months. But how far these distempers might be owing to the dry season, I leave to the judgment of our learned physicians.

To compare with these, we have collected from the Memoirs of the Royal Academy of Sciences, the quantity of rain and dissolved snow which has fallen at the Observatory at Paris for the same years; according to the observation of M. De la Hire. And that the comparison might be made more justly, we have reduced the French measure to our own. But it is to be observed, that the diversity of stile makes the years not exactly the same, though, as to this matter, the difference may seem very inconsiderable, the one being only between 10 and 11 days later than the other.

A Table of Rain which fell at Upminster and Paris, from the Year 1697 *to the Year* 1714.

	Depth at			Depth at			Depth at	
	Upminster	Paris		Upminster	Paris		Upminster	Paris.
Year	In. Ct.	In. Ct.	Year	In. Ct.	In. Ct.	Year	In. Ct.	In. Ct.
1697	15.52	21.60	1703	23.99	18.51	1709	26.56	23.21
1698	24.46	23.20	1704	15.81	21.20	1710	18.37	17.10
1699	15.11	19.93	1705	16.93	14.82	1711	23.60	26.84
1700	19. 3	21.38	1706	24.29	16.32	1712	23.76	
1701	18.69	22.78	1707	16.31	19.11	1713	23.16	
1702	20.38	17.42	1708	19.22	19.51	1714	11.19	

Solutio Generalis Problematis XV. propositi à D. de Moivre, in tractatu de Mensura Sortis inserto Actis Philosophicis Anglicanis N° 329, *pro numero quocunque Collusorum: per D. Nicolaum Bernoulli,* Basiliensem, Reg. Soc. Sodalem.* N° 341, p. 133.

Solutio generalis altera præcedentis Problematis, ope Combinationum et Serierum infinitarum, per D. Abr. de Moivre, Reg. Soc. Sodalem. N° 341, p. 145.

The solution of the problems mentioned in these two articles being found in Mr. Demoivre's doctrine of chances, in an improved form, afterwards published, and in other books on the doctrine of chances, it were quite unprofitable to reprint them in this place.

* Nicolas was the second son of the celebrated John Bernoulli, who with his younger brother, Daniel, pursued the same mathematical studies with their father. These two brothers were invited to Petersburg on the foundation of the Academy, in 1725, where Nicolas died the year following, at an early age, and was honourably interred by the Czarina.

*An Account of several extraordinary Meteors or Lights in the Sky. By Dr. Edmund Halley, S. R. S.** N° 341, p. 159.

The theory of the air seems now to be perfectly well understood, and its different densities at all altitudes, both by reason and experiment, are sufficiently defined; for, supposing the same air to occupy spaces reciprocally proportional to the quantity of the superior or incumbent air, I have elsewhere proved, that at 40 miles high the air is rarer than at the surface of the earth about 3000 times; and that the utmost height of the atmosphere, which reflects light in the crepusculum, is not fully 45 miles. Notwithstanding which, it is still manifest, that some sort of vapours, and those in no small quantity, rise nearly to that height. An instance† of this may be given in the great light the society had an account of (vide Trans. Sept. 1676) from Dr. Wallis, which was seen in very distant counties almost over all the south part of England. Of which, though the Doctor could not get so particular an account as was requisite to determine its height, yet from the distant places it was seen in, it could not but be a great many miles high.

So likewise that meteor which was seen in 1708, on the 31st of July, between 9 and 10 o'clock at night, was evidently between 40 and 50 miles perpendicularly high, and as near as I can gather, over Sheerness and the Buoy on the Nore. For it was seen at London moving horizontally from E. by N. to E. by S. at least 50 degrees high, and at Redgrave in Suffolk, on the Yarmouth road, about 20 miles from the east coast of England, and at least 40 miles to the eastward of London, it appeared a little to the westwards of the south, suppose S. by W. and was seen about 30 degrees high, sliding obliquely downwards. I was shown in both places its situation, but could wish some person skilled in astronomical matters had seen it, that we might pronounce concerning its height with more certainty; yet, as it is, we may securely conclude, that it was not many miles more westerly than Redgrave, which, as I said before, is above 40 miles more easterly than London. Suppose it therefore, where perpendicular, to have been 35 miles east from London, and by the altitude it appeared at in London, viz. at 50 degrees, its tangent will be 42 miles, for the height of the meteor above the surface of the earth; which also is rather of the least, because the altitude of the place shown me, is rather more than 50 degrees;

* This is the first notice we find in the Philos. Trans. of Dr. Halley being secretary to the Royal Society.

† Dr. Halley here takes it for granted that the appearance is some kind of vapours enkindled, though he is at a loss to account for their great height and motion. These circumstances induced Dr. Wallis to conjecture that they may be small comets. See vol. ii, p. 389, of these Abridgments.

and the like may be concluded from the altitude it appeared in at Redgrave, near 70 miles distant. Though at this great distance, it appeared to move with an amazing velocity, darting, in a very few seconds of time, for about 12 degrees of a great circle from north to south, being very bright at its first appearance, and it died away at the end of its course, leaving for some time a pale whiteness in the place, with some remains of it in the track where it had gone; but no hissing sound as it passed, or explosion were heard.

It may deserve the honourable Society's thoughts, how so great a quantity of vapour should be raised to the very top of the atmosphere, and there collected, so as upon its accension, or otherwise illumination, to give a light to a circle of above 100 miles diameter, not much inferior to the light of the moon; so as one might see to take a pin from the ground in the otherwise dark night. It is hard to conceive what sort of exhalations should rise from the earth, either by the action of the sun or subterranean heat, so as to surmount the extreme cold and rareness of the air in those upper regions: but the fact is indisputable, and therefore requires a solution.*

* Here again Dr. Halley's mind fixes on nothing but vapour or exhalations, to solve the appearance; though the difficulty, not to say impossibility, of conceiving how any exhalations could be raised so high, ought to have hinted the idea of some other origin. Later observations however have induced a belief that these luminous appearances are allied to, if not the same as the stones which have frequently been known to fall from the atmosphere, at different times, and in all parts of the earth. Several of the phænomena are common to both. These luminous bodies are seen to move with very great velocities, in oblique directions descending; commonly with a loud hissing noise, resembling that of a mortar shell, or cannon ball, or rather that of an irregular hard mass projected violently through the air; surrounded by a blaze or flame, tapering off to a narrow stream in the hinder part of it; are heard to explode or burst, and seen to fly in pieces, the larger parts going foremost, and the smaller following in succession; are thus seen to fall on the earth, and strike it with great violence; that on examining the place of the fall, the parts are found scattered about, being still considerably warm, and most of them entered the earth several inches deep. After so many facts and concurring circumstances, it is difficult to refuse assent to the identity of the two phænomena: indeed it seems now not to be doubted, but generally acquiesced in. And hence it is concluded, that every such meteor-like appearance is attended by the fall of a stone, or of stones, though we do not always see the place of the fall, nor discover the stones.

This conclusion however has contributed nothing towards discovering the origin of the phenomenon, at least as to its generation in the atmosphere: on the contrary, it seems still more difficult to account for the production of stones, than gaseous meteors, in the atmosphere, as well as to inflame and give them such violent motion. In fact, it seems concluded as a thing impossible to be done, or conceived; and philosophers have given up the idea as hopeless. This circumstance has induced them to endeavour to discover some other cause or origin for these phænomena. But no idea that is probable, or even possible, has yet been started, excepting one, by the very celebrated mathematician Laplace, and that of so extraordinary a nature, as to astonish us with its novelty, and boldness of conception. This is no less than the conjecture that these stony masses are projected from the moon! a conjecture which none but an astronomer could have made, or at least have shown to

Like to this, but much more considerable, was that famous meteor which was seen to pass over Italy on the 21st of March O. S. Anno 1676, about an

be probable, or even possible. Any ordinary person might at random utter the vague expression of a thing coming from the moon: but no one, except the philosopher, could propose the conjecture seriously, and prove its possibility. This M. Laplace has been enabled to do by strict mathematical calculation. He has proved that a mass, if projected by a volcano from the moon, with a certain velocity, of about a mile and half per second, (which is possible to be done) it will thence be thrown beyond the sphere of the moon's attraction, and into the confines of the earth's; the consequence of which is, that the mass must presently fall to the earth, and become a part of it.

To prepare the way for a calculation, and a comparison of this supposed cause with the phænomena, it will be useful here to premise a short account of the late and best observed circumstances in the appearance of fireballs, and the fall of stony masses from the atmosphere, extracted from the last published accounts of some of the more remarkable cases.

It is remarkable how generally the tradition has prevailed, in almost all ages, and among all people, of the fall of solid materials from the atmosphere, under the various denominations of thunderbolts, showers of stones, masses of native iron, &c. generally believed by the common people, who had often witnessed the fact, as coming from the sky or the heavens, and thence ascribed to the miraculous judgments of the Deity, while they were as generally disbelieved by the philosophers, either because they had never seen the fall, or because they found it impossible to account for the cause of them.

In the later ages of the world however the fact has been observed by more respectable evidences, and recorded with circumstances of considerable accuracy. One instance of this kind, is that given by the celebrated astronomer Gassendi, who was an eye-witness of what he relates. Nov. 27, 1627, the sky being quite clear, he saw a burning stone fall on mount Vaisir, in the south-east extremity of France, near the city of Nice, on the coast of the Mediterranean Sea. While in the air, it seemed to be about 4 feet in diameter; it was inclosed in a luminous circle of colours like a rainbow; and in its fall it produced a sound like the discharge of cannon. It weighed 59lb. was very hard, of a dull metallic colour, and in specific gravity considerably more than that of marble.

Prior to this is another remarkable instance in the stone that fell near Ensisheim, a considerable town in Alsace, the north-east point of France, near the Upper Rhine, a little north of Basil. This was in 1492, Nov. 7, between 11 and 12 before noon, when a dreadful thunder-clap was heard at Ensisheim, and a child saw a huge stone fall on a field lately sowed with wheat. On the people going to the place, the hole was found, and digging out the stone, it was found to have entered 3 feet deep, and weighed 260lb., which makes its size equal to a cube of about 13 inches the side. No doubt has ever been entertained of this fact, and cotemporary writers all agree in its general belief by the neighbourhood, and the natives of the place must have known that in their wheat field no such stone or hole had formerly existed.

In the year 1672, two stones fell near Verona in Italy; the one weighing 300, the other 200lb. Soon after, one of the members of the Abbé Bourdelot's academy presented, at one of their meetings, a specimen of these two stones; stating that the phenomenon had been seen by 3 or 400 persons; that the stones fell in a sloping direction, during the night, and in calm weather; that they appeared to burn, fell with great noise, and ploughed up the ground.—It is a pity the record does not mention the bearing of their path, as to point of the compass.

It is related by Paul Lucas, the traveller, that when he was at Larissa, a town in Greece, near the gulph of Salonicha, a stone of 72lb. weight fell in the neighbourhood. It was observed to come hour and three quarters after sun set, which happened to be observed, and was well considered, by the famous professor of mathematics in Bononia, Gemini-

from the northward, with a loud hissing noise, and seemed to be enveloped in a small cloud, which exploded when the stone fell. It looked like iron dross, and smelled of sulphur.

In Sept. 1753, several stones fell, accompanied with loud noises, in the province of Bresse, a little west from Geneva; particularly one fell at Pont-de-Vesle, and one at Liponas, at 9 miles distance from each other. The sky was clear, and the weather warm. A loud noise and hissing sound were heard at those two places, and for many miles round, at the time the stones fell. The stones appeared exactly similar to each other, of a darkish dull colour, very heavy, and their surface showing as if they had suffered a violent degree of heat. The largest weighed about 20lb., and penetrated about 6 inches into the ploughed ground, a circumstance which renders it highly improbable that they could have existed there before the explosion. This phenomenon has been described by the astronomer Delalande, who seems to have carefully examined, on the spot, the truth of the circumstances he describes.

In the year 1768, three stones were presented to the Academy of Sciences at Paris, which had fallen in different parts of France; one at Lucè in the Maine, another at Aire in Artois, and the third in Cotentin. These were all externally of the very same appearance; and Messrs. Fougeraux, Cadet, and Lavoisier drew up a particular report on the first of them. They state, that on the 18th of Sept. 1768, between 4 and 5 afternoon, there was seen near the village of Lucè, in Le Maine, a cloud, in which a short explosion took place, followed by a hissing noise, but without any flame; that some persons about 10 miles from Lucè heard the same sound, and, looking upwards, they perceived an opaque body describing a curve line in the air, and fall on a piece of green turf near the high road; that they immediately ran to this place, where they found a kind of stone, half buried in the earth, extremely hot, and weighing about $7\frac{1}{2}$lb.

July 24, 1790, between 9 and 10 at night, a shower of stones fell near Agen, in Guienne, near the south-west angle of France. First, a luminous ball of fire was seen, traversing the atmosphere with great rapidity, and leaving behind it a train of light which lasted about 50 seconds; soon a loud explosion was heard, and sparks were seen flying off in all directions. This was soon after followed by the fall of stones, over a considerable extent of ground, and at various distances from each other. These were all alike in appearance, but of many different sizes, the greater number weighing about 2 ounces, but many a vast deal more: some fell with a hissing noise, and entered the ground, but the smaller ones remained on the surface. The shower did no considerable damage, only breaking the tiles of some houses. All this was attested in a proces-verbal, signed by the magistrates of the municipality: it was further substantiated by the testimony of several hundred persons, inhabitants of the place; and several learned men wrote the very same account to their scientific correspondents: one of those, (son of the celebrated chemist M. D'Arcet) mentions two additional and important circumstances, from his own observation: viz. that the stones, when they fell on the houses, had not the sound of hard and compact substances, but of a matter in a soft, half-melted state; and that such of them as fell upon straws, adhered to them, so as not to be easily separated. That these stones broke the roofs of houses, and were found above pieces of straw adhering to them, is a clear proof of their falling from above, and in a state of fusion.

December 18, 1795, several persons, near Captain Topham's house in Yorkshire, heard a loud noise in the air, followed by a hissing sound, and soon after felt a shock, as if a heavy body had fallen to the ground at a little distance from them: in fact, one of them saw a huge stone fall to the earth at 8 or 9 yards from the place where he stood; it was 7 or 8 yards above the ground when he an Montanari, as may be seen in his Italian treatise about it, soon after published at Bononia. He observes that at Bononia, its greatest altitude in the S. S. E.

first observed it: in its fall it threw up the mould on every side, and buried itself 21 inches deep: the stone, being raised, was found to weigh 56lb.

March the 17th, 1798, a body, burning very brightly, passed over the vicinity of Ville Franche, on the Saone, a little to the east of Lyons in France, accompanied with a hissing noise, and leaving a luminous track behind it. This phenomenon exploded with a great noise, about 1200 feet from the ground; and one of the splinters, still luminous, being observed to fall in a neighbouring vineyard, was traced: at the spot a stone was found, about a foot diameter, which had penetrated 20 inches into the ground.

While these circumstances in Europe were daily confirming the original, but long exploded idea of the vulgar, that many of the luminous meteors observed in the atmosphere, are masses of ignited matter, an account of a phenomenon, of precisely the same description, was received from the East Indies, vouched by authority particularly well adapted to procure general respect. Mr. Williams, F. R. S. residing in Bengal, hearing of an explosion, with a descent of stones, in the province of Bahar, diligently enquired into the circumstances, among the Europeans on the spot. He learned, that on Dec. 19, 1798, at 8 o'clock in the evening, a large fire ball, or luminous meteor, was seen at Benares, and other parts of the country: that it was attended with a loud rumbling noise; and that, about the same time, the inhabitants of Krakhut, 14 miles from Benares, saw the light, heard like a loud thunder-clap, and immediately after heard the noise of heavy bodies falling in the neighbourhood. Next morning the mould in the fields was found to have been turned up in many spots; and unusual stones of various sizes, but of the same substances, were picked out of the moist soil, generally from a depth of 6 inches. As the occurrence took place in the night, after the people had retired to rest, the explosion and the fall of the stones were not seen: but the watchman of an English gentleman near Krakhut, brought him a stone the next morning, which he said had fallen through the top of his hut, and buried itself in the earthen floor.

Several of the preceding accounts notice the material circumstance, of damage done to interposed objects by the falling stones. In one instance, not yet mentioned, still more distinct traces were left, to show that their progress was through the air: viz. during the explosion of a meteor near Bourdeaux, the 20th of August 1789, a stone, about 15 inches diameter, fell through the roof of a cottage, and killed a herdsman and some cattle. Part of this stone is now in the Museum of the Right Hon. Charles Greville, and the rest in that of Bourdeaux. See Mr. Greville's paper in the Philos. Trans. for 1803, pt. 1.

Hence it seems quite impossible to deny very great weight to all these testimonies, and many others that might be given; several of them by intelligent eye-witnesses, and others by more ordinary persons indeed, but prepossessed by no theory; all concurring in their descriptions; and examined by acute and respectable persons, immediately after the phænomena had occurred. Without offering any further remarks then, on this mass of external evidence, we shall only just notice the main points which it seems to substantiate in a very satisfactory manner. It proves then, that, in various parts of the world, luminous meteors have been seen moving through the air with surprising rapidity, in a direction more or less oblique, accompanied with a noise, commonly like the whizzing of large shot, followed by explosion, and the fall of hard, stony, or semimetallic masses, in a heated state. The constant whizzing sound; the fact of stones being found, similar to each other, but unlike all others in the neighbourhood, at the spots towards which the luminous body or its fragments were seen to move; the scattering or ploughing up of the soil at those spots, always in proportion to the size of the stones; the concussion of the neighbouring ground at the time; and especially the was 38 degrees, and at Siena, 58 to the N. N. W., that its course, by the concurrence of all the observers, was from E. N. E. to W. S. W. that it came over

impinging of the stones on bodies somewhat above the earth, or lying loose on its surface—are circumstances perfectly well authenticated in these reports; proving that such meteors are usually inflamed hard masses, descending rapidly through the air to the earth.

Having drawn this conclusion from the consideration of the more plain and obvious circumstances of these stones and meteors; we may now advert to those of the more close and intimate examination of the stones themselves: and this we find at once strengthening the foregoing conclusion, and conducting to a further knowledge of the subject, than is afforded by the mere external evidence only.

The reports of all those persons who saw and observed the meteors, and found the stones in the several places, after the explosions, uniformly agree, in describing those substances as different from all the neighbouring bodies, and as presenting in every case, the same external appearance of semi-metallic matter, coated on the outside with a thin black crust, and bearing strong marks of recent fusion. Besides this general resemblance, obvious to the most ordinary inspection, many of those singular substances have been most carefully examined by some of the first chemists and naturalists of the age, and their investigations have put us in possession of a mass of information, sufficient to convince the most scrupulous inquirer, that the bodies in question have a common origin, and that we are totally unacquainted with any natural process which could have formed them on our globe.

The more nice and chemical examination of those stones has been made by Messrs. De la Laude, Lavoisier, Fougeraux, Cadet, Vauquelin, Barthold, Count de Bournon, our learned countryman, Mr. Howard, and several other ingenious men; and all their reports agree in representing them of a similar nature and composition, formed of the same simple materials, of nearly the same specific weight, and with very slight variations in the proportions of the component parts, forming the aggregate of these masses. Mr. Howard and the Count de Bournon found that the specific gravities of all the stones were nearly the same, excepting that the greater abundance of iron in one of them caused a considerable increase in its gravity.

From their researches, it appears that the specific gravities of some of the more remarkable stones, are as in the annexed table, considering 1000 as the proportionate number for the specific gravity of water. From whence it appears that, in this respect, they greatly exceed all the known ordinary stones, and approach to those of the metallic ores.

	Spec. grav.
The Ensisheim stone	3233
Benares.	3352
Sienna	3418
Gassendi's	3456
Yorkshire	3508
Bachelay's.	3535
Bohemia	4281

All the stones examined by Count de Bournon and Mr. Howard were found to consist of four distinct substances, viz. small metallic particles, a peculiar martial pyrites, a number of globular and elliptical bodies, also of a peculiar nature, and an earthy cement surrounding the other component parts. The nature of the metallic particles was the same in all, being in each an alloy of iron and nickel. In the pyrites, nickel as well as iron was detected; and the easy decomposition of the pyrites by muriatic acid, afforded a distinguishing character of that substance. The globules contained silica, magnesia, and oxides of nickel and iron. The earthy cement consisted of the same substances, very nearly in the same proportions.

M. Vauquelin also, about the same time as Mr. Howard, analysed the Benares stone, and two others which fell in 1789 and 1790, in the south of France; and the results of his experiments agreed with those of Mr. Howard in every particular. So that we are now authorised to conclude, that the stones which have at different times fallen down on the earth, in England, France, Italy, and India, the Adriatic Sea as from Dalmatia: that it crossed over all Italy, being nearly vertical to Rimini and Savigniano on the one side, and to Leghorn on the other:

are exactly of the same nature, consisting of the same simple substances arranged in similar compounds, in nearly the same proportions, and in the same manner combined, so as to form heterogeneous aggregates, whose general resemblance to each other is complete. We are hence also warranted in another important inference, viz. that no other bodies have as yet been discovered on our globe, which contain the same ingredients; and that the analysis of these stones has brought us acquainted with a species of pyrites not formerly known, nor any where else to be found.

The general analogy between these stones and the masses of native iron, that have been found in different parts of the world, was too striking to escape the notice of the eminent inquirers who have investigated this subject. They resemble each other in their external character, though not so closely as the stones themselves; but in one circumstance of their chemical composition they have a notable similarity, both among themselves, and to the stony substances. M. Proust had before proved that the enormous mass of native iron found in South America, contained in its composition a large portion of nickel. Mr. Howard has been led to the same conclusion by analyzing another portion of the same: and he has also found that the like solitary masses discovered in Siberia, Bohemia, and Senegal, contained a mixture of the same metal with iron, though in various proportions. The Bohemian iron is an alloy, of which nickel forms 18 parts in 100; in the Siberian iron it forms 17; and in the Senegal iron 5 or 6. But what is still more striking, and tends to put the similarity of their origin beyond all doubt, the Siberian mass is interspersed with cavities, containing an earthy substance, of the very same nature as the earthy cement and globules of the Benares stone; and the proportions of the ingredients are also nearly alike, except only in the oxide of iron, which is considerably less in the Siberian earth. This remarkable fact greatly strengthens the idea, that the Siberian iron owes its origin to the same causes which formed and projected the different stones that have fallen through the air on the earth; and, joined to the other details of the analysis, it naturally leads us to conclude, that the masses of native iron, as they are called, differ in no respect from the metallic particles, or the alloy of iron and nickel, which constitute one of the four aggregate parts in every stone of this kind hitherto examined.

Concerning the Siberian iron, there exists a general tradition of the Tartars, that it formerly fell from the heavens. In addition to which, a pretty authentic testimony has been lately found, to prove the fall of a similar body in India. The Right Hon. Charles Greville has communicated to the Royal Society (Philos. Trans. 1803, pt. 1) a very interesting paper, translated from the emperor Tchangire's Memoirs of his own Reign. The prince relates, that in the year 1620, of our æra, a violent explosion was heard at a village in the Punjaub, and at the same time a luminous body fell through the air on the earth. That the officer of the district immediately repaired to the spot where it was said the body fell, and having found the place to be hot, he caused it to be digged, on which he found the heat kept increasing till they reached a lump of iron violently hot. That this was sent to court, where the emperor had it weighed in his presence, and ordered it to be forged into a sabre, a knife, and a dagger; that, after trial, the workmen reported it was not malleable, but shivered under the hammer: and that it required to be mixed with one third part of common iron, after which the mass was found to make excellent blades. The royal historian adds, that on the incident of this *iron of lightning* being manufactured, a poet presented him with a distich, that, "during his reign, the earth attained order and regularity; that raw iron fell from lightning, which was, by his world-subduing authority, converted into a dagger, a knife, and two sabres."

The exact resemblance of this occurrence, in all its essential circumstances, to the former accounts of fallen stones, and the particular remark on the unmalleable nature of the iron, give a high degree that its perpendicular altitude was at least 38 miles: that in all places near this course, it was heard to make a hissing noise as it passed, like that of artificial fire-

of credibility to the whole narrative, and throw additional weight on the inference before drawn from internal evidence, that the solitary masses of native iron found in different quarters of the globe, have the same origin with the stones analysed by Howard and Vauquelin.

Having now given a summary of the facts and evidence, as well with regard to the circumstances attending these singular bodies, as the ingredients they are composed of, and their outward appearance and structure, we are now to consider what inferences respecting their probable origin, may be drawn from this mass of information. And indeed we may safely conclude, as it has been inferred from the whole, by the philosophers best qualified to judge of the circumstances, as follow, viz. that the bodies in question have fallen on the surface of the earth; but that they were not projected by any terrestrial volcanoes; and that we have no right, from the known laws of nature, to suppose that they were formed in the upper regions of the atmosphere. Such a negative conclusion has been thought all that we are, in the present state of our knowledge, entitled to draw.

In this embarrasing predicament, the total want of any other possible way of accounting for the origin of those bodies, an idea has been started, perhaps at first merely at random, that since there is no other possible manner of accounting for them, then they must have dropped from the moon. And indeed, this singular thought has now advanced into a serious hypothesis, which it must be allowed is unincumbered with any of the foregoing difficulties: having at least possibility in its favour, which no other hypothesis yet proposed can claim.

As the attraction of gravitation extends through the whole planetary system, a body, placed at the surface of the moon, is affected chiefly by two forces, one drawing it toward the centre of the earth, and another drawing it toward that of the moon. The latter of these forces however, near the moon's surface, is incomparably the greater. But as we recede from the moon, and approach toward the earth, this force decreases, while the other augments; till at length a point of station is found between the two planets, where these forces are exactly equal; so that a body, placed there, must remain at rest; but if it be removed still nearer to the earth, then this planet would have the superior attraction, and the body must fall towards it. If a body then be projected from the moon towards the earth, with a force sufficient to carry it beyond this point of equal attraction, it must necessarily fall on the earth. Such then is the idea of the manner in which the bodies must be made to pass from the moon to the earth, if that can be done, the *possibility* of which is now necessary to be considered.

Now supposing a mass to be projected from the moon, in a direct line towards the earth, by a volcano, or by the production of steam by subterranean heat; and supposing for the present those two planets to remain at rest; then it has been demonstated, on the Newtonian estimation of the moon's mass, that a force projecting the body with a velocity of 12,000 feet in a second, would be sufficient to carry it beyond the point of equal attraction. But this estimate of the moon's mass is now allowed to be much above the truth; and on M. Laplace's calculation it appears that a force of little more than half the above power would be sufficient to produce the effect, that is, a force capable of projecting a body with a velocity of less than a mile and a half per second. But we have known cannon balls projected by the force of gunpowder, with a velocity of 2500 feet per second, or upwards, that is, about half a mile. It follows therefore, that a projectile force, communicating a velocity about three times that of a cannon ball, would be sufficient to throw the body from the moon beyond the point of equal attraction, and cause it to reach the earth. Now there can be little doubt that a force equal to that is exerted by volcanoes on the earth, as well as by the production of steam from subterranean heat, when we consider the huge masses of rock, so many times larger than cannon balls, works: that having passed over Leghorn, it went off to sea towards Corsica; and lastly, that at Leghorn it was heard to give a very loud report like a great cannon ;

thrown on such occasions to heights also so much greater. We may easily imagine too such cause of motion to exist in the moon as well as in the earth, and that in a superior degree, if we may judge from the supposed symptoms of volcanoes recently observed in the moon, by the powerful tubes of Dr. Herschel; and still more, if we consider that all projections from the earth suffer an enormous resistance and diminution, by the dense atmosphere of this planet, while it has been rendered probable, from optical considerations, that the moon has little or no atmosphere at all, to give any such resistance to projectiles.

Thus then we are fully authorised in concluding, that the case of *possibility* is completely made out; that a known power exists in nature, capable of producing the foregoing effect, of detaching a mass of matter from the moon, and transferring it to the earth, in the form of a flaming meteor, or burning stone; at the same time we are utterly ignorant of any other process in nature by which the same phenomenon can be produced. Having thus discovered a way in which it is possible to produce those appearances, we shall now endeavour to show, from all the concomitant circumstances, that these accord exceedingly well with the natural effects of the supposed cause, and thence give it a very high degree of *probability*.

This important desideratum will perhaps be best attained, by examining the consequences of a substance supposed to be projected by a volcano from the moon, into the sphere of the earth's superior attraction; and then comparing those with the known and visible phænomena of the blazing meteors or burning stones, that fall through the air on the earth. And if in this comparison a striking coincidence or resemblance shall always or mostly be found, it will be difficult for the human mind to resist the persuasion that the assumed cause involves a degree of probability but little short of certainty itself. Now the chief phænomena attending these blazing meteors, or burning stones, are these: 1. That they appear or blaze out suddenly. 2. That they move with a surprising rapid motion, nearly horizontal, but a little inclined downwards. 3. That they move in several different directions, with respect to the points of the compass. 4. That in their flight they yield a loud whizzing sound. 5. That they commonly burst with a violent explosion and report. 6. That they fall on the earth with great force in a sloping direction. 7. That they are very hot at first, remain hot a considerable time, and exhibit visible tokens of fusion on their surface. 8. That the fallen stony masses have all the same external appearance and contexture, as well as internally the same nature and composition. 9. That they are totally different from all our terrestrial bodies, both natural and artificial.

Now these phænomena will naturally compare with the circumstances of a substance projected by a lunar volcano, and in the order in which they are here enumerated. And first with respect to the leading circumstance, that of a sudden blazing meteoric appearance, which is not that of a small bright spark, first seen at immense distance, and then gradually increasing with the diminution of its distance. And this circumstance appears very naturally to result from the assumed cause. For, the body being projected from a lunar volcano, may well be supposed in an ignited state, like inflamed matter thrown up by our terrestrial volcanoes, which passing through the comparatively vacuum, in the space between the moon and the earth's sensible atmosphere, it will probably enter the superior parts of this atmosphere with but little diminution of its original heat; from which circumstance, united with that of its violent motion, this being 10 or 12 times that of a cannon ball, and through a part of the atmosphere probably consisting chiefly of the inflammable gas, rising from the earth to the top of the atmosphere, the body may well be supposed to become suddenly inflamed, as the natural effect of these circumstances; indeed it would be surprising if it did not. From whence it immediately after which, another sort of sound was heard, like the rattling of a great cart running over stones, which continued about the time of a credo.

appears that the sudden inflammation of the body, on entering the earth's atmosphere, is exactly what might be expected to happen.

2. Secondly, to trace the body through the earth's atmosphere; we are to observe that it enters the top of it, with the great velocity acquired by descending from the point of equal attraction, which is such as would carry the body to the earth's surface in a very few additional seconds of time, if it met with no obstruction. But as it enters deeper in the atmosphere, it meets with still more and more resistance from the increasing density of the air; by which the great velocity, of 6 miles per second, must soon be greatly reduced to one that will be uniform, and only a small part of its former great velocity. This remaining part of its motion will be various in different bodies, being more or less as the body is larger or smaller, and as it is more or less specifically heavy: but, for a particular instance, if the body were a globe of 12 inches diameter, and of the same gravity as the atmospheric stones, the motion would decrease so, as to be little more than a quarter of a mile per second of perpendicular descent. Now while the body is thus descending, the earth itself is affected by a two-fold motion, both the diurnal and the annual one, with both of which the descent of the body is to be compounded. The earth's motion of rotation at the equator, is about 17 miles in a minute, or $\frac{2}{7}$ of a mile in a second: but in the middle latitudes of Europe little more than the half of that, or little above half a quarter of a mile in a second: and if we compound this motion with that of the descending body, as in mechanics, this may cause the body to appear to descend obliquely, though but a little, the motion being nearer the perpendicular than the horizontal direction. But the other motion of the earth, or that in its annual course, is about 20 miles in a second, which is 80 times greater than the perpendicular descent in the instance above-mentioned: so that, if this motion be compounded with the descending one of the body, it must necessarily give it the appearance of a very rapid motion, in a direction nearly parallel to the horizon, but a little declining downwards. A circumstance which exactly agrees with the usual appearances of these meteoric bodies, as stated in the 2d article of the enumerated phænomena.

3. Again, with regard to the apparent direction of the body, this will evidently be various, being that compounded of the body's descent and the direction of the earth's annual motion at the time of the fall, which is itself various in the different seasons of the year, according to the direction of the several points of the ecliptic to the earth's meridian or axis. Usually however, from the great excess of the earth's motion, above that of the falling body, the direction of this must appear to be nearly opposite to that of the former. And in fact this exactly agrees with a remark made by Dr. Halley, in his account of the meteors in his paper above given, where he says that the direction of the meteor's motion was exactly opposite to that of the earth in her orbit. And if this shall generally be found to be the case, it will prove a powerful confirmation of this theory of the lunar substances. Unfortunately however, the observations on this point are very few and mostly inaccurate: the angle or direction of the fallen stones has not been recorded; and that of the flying meteor commonly mistaken, all the various observers giving it a different course, some even directly the reverse of others. In future, it will be very advisable that the observers of fallen stones, observe and record the direction or bearing of the perforation made by the body in the earth, which will give us perhaps the course of the path nearer than any other observation.

4. In the flight of these meteoric stones, it is commonly observed that they yield a loud whizzing sound. Indeed it would be surprising if they did not. For if the like sound be given by the smooth and regularly formed cannon ball, and heard at a considerable distance, how exceedingly great

He concludes, from the apparent velocity it went with at Bononia, at above 50 miles distance, that it could not be less swift, than 160 miles in a minute of time, which is above 10 times as swift as the diurnal rotation of the earth under the equinoctial, and not many times less than that with which the annual motion of the earth about the sun is performed. To this he adds its magnitude, which appeared at Bononia larger than the moon in one diameter, and above half as large again in the other; which with the given distance of the eye, makes its real less diameter above half a mile, and the other in proportion. This supposed, it cannot be wondered that so great a body moving with such an amazing velocity through the air, though so much rarefied as it is in its upper regions, should occasion so loud a hissing noise, as to be heard at such a distance as it seems this was. But it will be much harder to conceive, how such an impetus could be impressed on this body, which far exceeds that of any

must be that of a body so much larger, which is of an irregular form and surface too, and striking the air with 50 or 100 times the velocity.

5. That they commonly burst and fly in pieces in their rapid flight, is a circumstance exceeding likely to happen, both from the violent state of fusion on their surface, and from the extreme rapidity of their motion through the air. If a grinding stone, from its quick rotation, be sometimes burst and fly in pieces; and if the same thing happens to cannon balls, when made of stone, and discharged with considerable velocity, merely by the friction and resistance of the air; how much more is the same to be expected to happen to the atmospheric stones, moving with more than 50 times the velocity, and when their surface may well be supposed to be partly loosened or dissolved by the extremity of the heat there.

6. That the stones strike the ground with a great force, and penetrate to a considerable depth, as is usually observed, is a circumstance only to be expected, from the extreme rapidity of their motion, and their great weight, when we consider that a cannon ball, or a mortar shell, will often bury itself many inches, or even some feet in the earth.

7. That these stones, when soon sought after and found, are hot, and exhibit the marks of recent fusion, are also the natural consequences of the extreme degree of inflammation in which their surface had been put during their flight through the air.

8. That these stony masses have all the same external appearance and contexture, as well as internally the same nature and composition, are circumstances that strongly point out an identity of origin, whatever may be the cause to which they owe so generally uniform a conformation. And when it is considered, 9thly, that in those respects they differ totally from all terrestrial compositions hitherto known or discovered, they lead the mind strongly to ascribe them to some other origin than the earth we inhabit; and none so likely as coming from our neighbouring planet.

Upon the whole then it appears highly probable, that the flaming meteors, and the burning stones that fall on the earth, are one and the same thing. It also appears impossible, or in the extremest degree improbable, to ascribe these, either to a formation in the superior parts of the atmosphere, or to the irruptions of terrestrial volcanoes, or to the generation by lightning striking the earth. But on the other hand, that it is possible for such masses to be projected from the moon so as to reach the earth: and that all the phænomena of these meteors or falling stones, having a surprizing conformity with the circumstances of masses that may be expelled from the moon by natural causes, unite in forming a body of strong evidence, that this is in all probability and actually the case.

cannon ball; and how this impetus should be determined in a direction so nearly parallel to the horizon; and what sort of substance it must be, that could be so impelled and ignited at the same time: there being no volcano, or other spiraculum of subterraneous fire, in the N. E. parts of the world, that we ever yet heard of, from whence it might be projected.

I have much considered this appearance, and think it one of the hardest things to account for, that I have yet met with in the phænomena of meteors, and am induced to think that it must be some collection of matter formed in the æther, as it were by some fortuitous concourse of atoms, and that the earth met with it as it passed along in its orb, then but newly formed, and before it had conceived any impetus of descent towards the sun. For its direction was exactly opposite to that of the earth, which made an angle with the meridian at that time (the sun being in about 11 degrees of Aries) of 67°, that is, its course was from W.S.W. to E.N.E. so that the meteor seemed to move the contrary way. And besides, falling into the power of the earth's gravity, and losing its motion from the opposition of the medium, it seems that it descended towards the earth, and was extinguished in the Tyrrhene Sea, to the W.S.W. of Leghorn. The great report being heard on its first emersion into the water, and the rattling, like the driving a cart over stones, being what succeeded on its quenching; something like which is always observed on quenching a very hot iron in water. These facts being past dispute, I would be glad to have the opinion of the learned on them, and what objection can be reasonably made against the abovesaid hypothesis, which I humbly submit to their censure.

P. S. Since this was written, there has fallen into my hands an account of nearly such another appearance, seen in Germany, in the year 1686, at Leipsic, by the late Mr. Gottfried Kirch, who was for many years a very diligent observer of the heavens, and was perfectly well instructed in astronomical matters. In an appendix to his Ephemerides for the year 1688, he gives this remarkable account of it. "On the 9th of July, O. S. at half an hour past one in the morning, a fire ball with a tail was observed, in $8\frac{1}{2}$ degrees of Aquarius, and 4° north, which continued immoveable for half a quarter of an hour, having a diameter nearly equal to half the moon's diameter. At first its light was so great, that we could see to read by it: after which, it gradually vanished in its place. This phenomenon was observed at the same time in several other places, especially at Schlaitza, a town distant from Dantzic 11 German miles towards the south, its altitude being about 60° above the southern horizon."

At the time of this appearance the sun was in $26\frac{1}{2}$° of Cancer, and by the given place of the meteor, it is plain, it was seen about ¾ of an hour past the meridian, or in S. by W. and by its declination it could not be above 24° high at Leipsic, though the same, at Schlaize was about 60 high: the angle therefore at the meteor was about 36°. Whence, by an easy calculus, it will be found, that the same was not less than 16 German miles distant in a right line from Leipsic, and above 6½ such miles perpendicular above the horizon, that is at least 30 English miles high in the air. And though the observer says of it, immotus perstitit per semi-quadrantem horæ, it is not to be understood that it kept its place like a fixed star, all the time of its appearance; but that it had no very remarkable progressive motion. For he himself has, at the end of the said Ephemerides given a figure of it, which he has marked fig. D, whence it appears that it darted downwards obliquely to the right-hand, and where it ended, left two globules or nodes, not visible but by an optic tube (a telescope.)

The same Mr. Gottfried Kirch, in the beginning of a German treatise of his, concerning the great comet which appeared in the year 1680, entitled Newe Himmels Zeitung, printed at Nurenburg, anno 1681, gives an account of such another luminous meteor seen likewise at Leipsic, on the 22d of May 1680, O. S. about 3 in the morning: which though he himself saw not, was yet there observed by several persons, who made various reports of it, but the more intelligent agreed that it was seen descending in the north, and left behind it a long white streak where it had passed. At the same time, at Haarburgh, the like appearance was seen in the N. E. or rather N. N. E.; as also at Hamburg, Lubec and Stralsund, all which are about 40 German miles from Leipsic: but in all these places, by persons unacquainted with the manner of properly describing things of this kind. So that all we can conclude from it is, that this meteor was exceedingly high above the earth, as well as the former.

All the circumstances of these phænomena agree with what was seen in England in 1708; but it commonly so happens, that these contingent appearances escape the eyes of those that are best qualified to give a good account of them. It is plain however, that this sort of luminous vapour, is not exceedingly seldom thus collected; and when the like shall again happen, the curious are entreated to take more notice of them than has been hitherto done, that we may be enabled the better to account for the surprising appearances of this sort of meteor.

Some Remarks on the Variations of the Magnetical Compass, published in the Memoirs of the Royal Academy of Sciences, with regard to the General Chart of those Variations made by E. Halley; as also concerning the true Longitude of the Magellan Straits. By Dr. Halley. N° 341, p. 165.

The gentlemen of the Royal Academy of Sciences in France, have, for some years past applied themselves, with much candour and diligence, to examine the chart I published in the year 1701, for showing at one view the variations of the magnetical compass, in all those seas with which the English navigators are acquainted: and, to my no small satisfaction, I find that what I did so long ago, has been since verified by the concurrent reports of the French pilots, who of late have had frequent opportunities of inquiring into the truth of it. So that I am in hopes I have laid a sure foundation for the future discovery of an invention, that will be of great use to mankind when perfected; I mean that of the law or rule by which the variations change, in appearance regularly, all the world over. Of this I ventured to give my thoughts in N° 148 and N° 195 of these Transactions,* and as yet I see no cause to retract what I there offer for a reason of this change; but of this we might be more certain, had we a good collection of observations made in that ocean which divides Asia and America, and occupies about two fifths of the whole circumference of the globe.

In the mean time I cannot omit to take notice of two particulars, seeming to call in question the truth of my map, which I have lately observed in the Memoirs of the Royal Academy of Sciences. The one is in the Memoirs of the year 1700, concerning the variation observed at Paraïba in Brasil, about 25 leagues to the north of Pernambouc, by M. Couplet le fils, in words to this effect. "On the 20th of May, 1698, having before carefully drawn a meridian line, which I used in my astronomical observations, I observed the declination of the needle to be 5° 35′ north west." And the same observer tells us, that he found the latitude of the town of Paraïba 6° 38′ 18″. Now it happened, that I was in the river of Paraïba, in the month of March, 1699, and there fitted and cleaned my ship, so that I had full opportunity to observe the variation both on board and on shore, and found it constantly to be above 4° north east; so that I am willing to believe this to be an error of the press, putting N. W. for N. E.; or rather of the memory of M. Couplet, who, it seems, lost all his papers by shipwreck on his return. The like may be said of the latitude of Paraïba, which, though I did not observe myself, yet at the fort of

* Page 624, vol. ii, and 470, vol. iii, of these Abridgments.

Cabo Dello, at the mouth of the river, and which is about 3 leagues more northerly than the town, I found the latitude not less than 6° 55′ south, and consequently that of the town more than 7 degrees.

The other is in a discourse of M. de Lisle, in the Memoirs of 1710; where he compares the variations observed in some late voyages, with my map of the variations. Among other things, it is there said, that on the east side of the island St. Thomas, under the equinoctial line, M. Bigot de la Canté, second lieutenant of the king's ship la Sphere, had, in the beginning of the year 1708, found the variation $11\frac{1}{2}°$, whereas my chart makes it only $5\frac{1}{2}°$. It is true, that I never observed myself in those parts; and it is from the accounts of others, and the analogy of the whole, that in such cases I was obliged to supply what was wanting; and possibly there may be more variation on that coast than I have allowed. But consulting my chart, which was fitted to the year 1700, I find I then make the variation at the isle of St. Thomas full $7\frac{1}{2}°$ and not $5\frac{1}{2}°$, which, by the year 1708, might well rise to near 9°. So that the difference will become very tolerable; whereas an error of 6 degrees, such as is here represented, would render the credit of my chart justly suspected, and so useless as not to be confided in.

But I may further complain, that in the same Memoire of M. de Lisle, the geography of my chart is called in question; and we are told that I have placed the entrance of the Magellan Straits at least 10 degrees more westerly than I ought to have done: for that the ship St. Louis, in the year 1708, sailing from the mouth of Rio Gallega, in about the latitude of 52° south, and not far from Cape Virgin, directly for Cape Bonne Esperance (which course perhaps was never run before) had found the distance between the two lands not more than 1350 leagues, which, he concludes, is much less than my chart of the variation makes it. I know not from what computation M. de Lisle has deduced this consequence; but I find by my chart, that I have made the longitude of Rio Gallega 75° west from London, and that of Cape Bonne Esperance $16\frac{1}{2}$ east from it; that is in all $91\frac{1}{2}°$, difference of longitude. This, with the two latitudes, gives the distance, according to the rhumb-line 1364 leagues, but according to the arc of a great circle, no more than 1287 leagues; so that, instead of invalidating what I have there laid down, it absolutely confirms it, as far as the authority of one single ship's journals can do it.

I do not pretend that I have had observations made with all the precision requisite to lay down incontestably the Magellan Straits, in their true geographical site; but yet it has not been without good grounds that I have placed them as I have done. For when Sir John Narborough, in the year 1670, wintered in Port St. Julian, on the coast of Patagonia, Capt. John Wood, then his lieutenant, and an approved artist in sea affairs, observed the beginning of an eclipse of the moon, Sept. 18, stil. vet. at just 8 at night: and the same beginning was observed by M. Hevelius at Dantzic at 14h. 22m. whence Port St. Julian is more westerly than Dantzic 6h. 22m. or than London 5h. 6m. that is $76^{\circ}\frac{1}{2}$. Besides, I have had in my custody a very curious Journal of one Capt. Strong, who went into the South Seas in quest of a rich plate-wreck, and who discovered the two islands he called Falkland's Isles, lying about 120 leagues to the eastwards of the Patagon coast, about the lat. of $51\frac{1}{2}^{\circ}$. This Capt. Strong had a quick passage from the island of Trinadada (in $20^{\circ}\frac{1}{2}$ south) to the Magellan Straits; and in this Journal, which was very well kept, I found that Cape Virgin was, by his account, 45° of longitude more westerly than that island, whose longitude I know to be just 30 degrees from London: that is in all 75°.

From these concurrent testimonies, wanting better, I adventured to fix the longitude of this coast as I have done; and I can by no means grant an error of 10 degrees to be possible in it, though perhaps it may need some smaller correction. I will however readily grant, that those who go thither from Europe, shall find the land more easterly than is here expressed, by reason of a constant current setting to the westward near the equator, where ships are some times long detained by calms, while the stream carries them along with it; which happens to all ships bound to any part of the east coast of the South America.

Observations of the Remarkable Comet, seen the latter end of the Year 1680, *at Coburg in Saxony. By M. Gottfried Kirch.* N° 342, p. 170. *Translated from the Latin.*

The comet that appeared the latter end of 1680, was remarkable on several accounts; both for its being seen for 4 months, in which time it has run over 9 entire signs, and for the extraordinary size and brightness of its tail; but especially for the remarkable curvature of its orbit, by means of which the theory of comets was at length discovered; for, whilst astronomers applied themselves with the greatest diligence to determine its motion from observations, the great Sir Isaac Newton was the first who demonstrated that comets describe very nearly parabolic orbits; which he has shown how to construct from three given places, accurately observed, illustrating the subject by this comet of 1680; as may be seen at the latter end of the 3d book of his Princip. Philosoph.

Now it happened, that this comet which astronomers had so much observed in the evenings, was not once observed in the morning, either at Paris or Greenwich, before the 17th of November; whence it happened, that that part of the orbit, in which the comet descended to the sun, could not be determined with any certainty. But M. Gottfried Kirch, a German, published at Norimberg in 1681, a book entitled Newe Himmels Zeitung, i. e. Novus Nuncius Cœlestis, where he shows how he came to discover this comet which had yet no tail, and which was scarcely discernible by the naked eye; viz. while he was observing the moon, and Mars, that was near her, on the 4th of Nov. O. S. in the morning, at Colberg in Saxony, a town 11 degrees more easterly than London, and about 50° 20′ latitude, the moon being now come to some star unknown to Tycho (but which is in Mr. Flamsteed's British Catalogue, and the 44th star of Leo) he had a mind to determine the place of the said star from the fixed star near it; and while he was moving his telescope about, he lighted on a sort of nebulous spot, of an uncommon appearance, and which he presently concluded was either a new comet, or a nebulous star, resembling that in the girdle of Andromeda. He first saw this comet half an hour after 4 o'clock in the morning, somewhat higher than two small telescopic stars, with which at 6 o'clock it was seen exactly in a straight line: whence it appeared that it moved, and that direct. And, by calculations from several stars near its path, the place of the comet was found to be 29° 51′ of ♌, with 1° 17¾′ north latitude, at 6 o'clock, but at London 5 h. 2 m. of apparent time.

Afterwards, on the 6th of November, at 4 h. 42 m. in the morning, Mr. Kirch, with his two-foot telescope, observed the comet exactly in a straight line between Mars and the small star N which is the 45th of Leo in the British Catalogue, and then it was in 2° 42′ ♍, with 0° 16½′ south lat. Mars had at that time (on comparing together the observations made just before and after) 3° 46½′ ♍, with 1° 56′ north lat. whence, from its given path, the comet's place, at London 3 h. 58 m. in the morning, apparent time, was 3° 23′ ♍, with 1° 6′ north latitude.

November the 11th, at 5 h. 15 m. in the morning, the comet was equally distant from Bayer's two stars of Leo σ and τ, but had not yet reached the right line that joins them, though very near it: in the British Catalogue σ had at that time 14° 15′ ♍, and almost 1° 41′ north lat. but τ had 17° 3½′ ♍, and 0° 34′ south lat. consequently the lat. of the comet was somewhat less than the mean between these, viz. than 0° 33½′ north, and its long. somewhat less than 15° 39′ ♍. But this is not to be relied on, seeing it depends on the estimated equality of the distances, which is uncertain; the tail of the comet now began to be only half a degree in length, seen through a ten-foot telescope.

An Account of a Book entitled, Commercium Epistolicum Collinii et Aliorum, De Analysi promota; published by order of the Royal Society, concerning the Dispute between Mr. Leibnitz and Dr. Keill, about the Right to the Invention of the Method of Fluxions, by some called the Differential Method. N° 342, p. 173.

Several accounts having been published abroad of this Commercium, all of them very imperfect, it has been thought proper to publish the following account.

This Commercium consists of several letters and papers in the custody of the Royal Society, here put together in order of time, and either copied, or translated into Latin, from such originals as are mentioned in the title of each letter and paper; and a numerous committee of the Royal Society was appointed to examine the sincerity of the originals, and compare with them the copies so taken. It relates to a general method of resolving finite equations into infinite ones, and applying these equations, both finite and infinite, to the solution of problems by the method of fluxions and moments. We will first give an account of that part of the method which consists in resolving finite equations into infinite ones, and by that means squaring curvilinear figures. By infinite equations are meant such as involve a series of terms converging, or approaching to the truth nearer and nearer, in infinitum, so as at length to differ from the truth by less than any given quantity, and if continued in infinitum, to leave no difference.

Dr. Wallis, in his Opus Arithmeticum, published A. C. 1657, cap. 33, prop. 68, reduced the fraction $\frac{A}{1-R}$ by a continual division into the series $A + AR + AR^2 + AR^3 + AR^4 +$ &c.

Viscount Brounker squared the hyperbola by this series $\frac{1}{1\times 2} + \frac{1}{3\times 4} + \frac{1}{5\times 6} + \frac{1}{7\times 8} +$ &c. that is by this, $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} +$ &c. conjoining every two terms into one. And the quadrature was published in the Philosophical Transactions for April 1668.

Mr. Mercator, soon after, published a demonstration of this quadrature by Dr. Wallis's division, and soon after that, Mr. James Gregory published a geometrical demonstration of it. And these books were a few months after sent by Mr. John Collins to Dr. Barrow at Cambridge, and by Dr. Barrow communicated to Mr. Newton, now Sir Isaac Newton, in June 1669, then a fellow of Trinity College. In return, Dr. Barrow sent to Mr. Collins a tract of Mr. Newton's, entitled Analysis per Aquationes numero terminorum infinitas. This is the first piece published in the Commercium, and contains a general method of doing that in all figures, which Lord Brounker and Mr. Mercator did in the hyperbola alone. Mr. Mercator lived above 10 years longer, without proceeding further than to the single quadrature of the hyperbola. The progress made by Mr. Newton, as to all curves in general, shows that he wanted not Mr. Mercator's assistance. However, for avoiding disputes, he allows that Lord Brounker invented, and Mr. Mercator demonstrated, the series for the hyperbola some years before they published it, and consequently before he himself found out his general method.

Mr. Newton, in his letter to Mr. Oldenburg, dated Oct. 24, 1676, mentions the aforesaid treatise of Analysis, in the following manner: " At that very time when Mercator's Logarithmotechnia was published, my friend Dr. Barrow, then professor of mathematics at Cambridge, communicated to Mr. Collins a compendium of these series, in which I signified, that the areas and lengths of all sorts of curves, with the superficies and contents of solids, could be determined by given right lines, and vice versa; and I illustrated the said method by divers series."

In the years 1669, 1670, 1671, and 1672, Mr. Collins gave notice of this compendium to Mr. James Gregory in Scotland, to Mr. Bertet and Mr. Vernon, then at Paris, to Mr. Alphonsus Borelli in Italy, and to Mr. Strode, Mr. Townsend, Mr. Oldenburg, Mr. Dary, and others in England, as appears by his letters. Also, Mr. Oldenburg, in a letter dated Sept. 14, 1669, and entered in the letter-box of the Royal Society, gave notice of it to Mr. Francis Slusius at Liege, and cited several passages out of it. And particularly Mr. Collins, in a letter to Mr. James Gregory, dated Nov. 25, 1669, spake thus of the method contained in it: " Dr. Barrow has resigned his office of reading public lectures to one Mr. Newton of Cambridge, whom he mentions in the preface to his Optical Lectures, as a person of extraordinary genius; for that, before Mercator's Logarithmotechnia was published, he had invented the same method, and applied it to all curves in general, and to the circle in divers ways." And in a letter to Mr. David Gregory, dated August 11, 1676, he mentions it in this manner: " A few months after these books, viz. Mercator's Logarithmotechnia and Gregory's Exercitationes Geometricæ, were published, they were sent to Dr. Barrow at Cambridge; and the Doctor returned answer, that this doctrine of infinite series was invented by Mr. Newton two years before M. Mercator's Logarithmotechnia was published, and that he had applied it to all curves in general, and the Doctor at the same time sent Mr. Newton's MS. copy." The last of the said two books came out towards the end of the year 1668, and Dr. Barrow sent the said compendium to Mr. Collins in July following, as appears by three of Dr. Barrow's letters. And in a letter to Mr. Strode, dated July 26, 1672, Mr. Collins wrote of it to this effect: "I sent a copy of the Logarithmotechnia to Dr. Barrow at Cambridge, who immediately sent me some papers of Mr. Newton's, from which, and others formerly communicated to the Dr. by the author, it appears that that method was invented some years before by the said Mr. Newton, and applied universally; so that by means thereof in any proposed curvilinear figure, defined by one or more properties, the quadrature or area of the said curve may be found, accurately, if possible, but if not, yet infinitely near; as also the evolution or length of a curve, the centre of gravity of a figure, solids generated by its rotation, and their superficies may be obtained without any extraction of roots. After Mr. Gregory understood that this method, used by M. Mercator in his Logarithmotechnia, and applied to the quadrature of the hyperbola, and also improved by the said Mr. Gregory himself, was now made universal, and applied to all sorts of figures, he, after a great deal of close application, discovered the same method; and both Mr. Newton and Mr. Gregory propose to improve it; but the latter does not think it fair to anticipate Mr. Newton, the first inventor." And in another letter to Mr. Oldenburg, to be communicated to M. Leibnitz, dated June 14, 1676, Mr. Collins adds: "Such is the excellency of this method, that being so universal it stops at no difficulty; and I believe that Mr. Gregory and others are of opinon, that whatever was known before of this matter, was but like the glimmering light of the dawn when compared to the brightness of noon-day."

This tract was first printed by Mr. William Jones in 1710, who found it among the papers, and in the hand-writing of Mr. John Collins, and collated it with the original, which he afterwards borrowed of Mr. Newton. It contains the above-mentioned general method of analysis, showing how to resolve finite equations into infinite ones, and how by the method of moments to apply equations, both finite and infinite, to the solution of all problems. It begins where Dr. Wallis left off, and founds the method of quadratures on three rules.

Dr. Wallis published his Arithmetica Infinitorum in the year 1655, and by the 59th proposition of that book, if the abscissa of any curvilinear figure be called x, and m and n be integer numbers, and the ordinates erected at right angles be $x^{\frac{m}{n}}$, the area of the figure shall be $\frac{n}{m+n} x^{\frac{m+n}{n}}$. And this is assumed by Mr. Newton as the first rule on which he founds his quadrature of curves. Dr. Wallis demonstrated this proposition by steps or induction in many particular cases, and then collected all the propositions into one by a table of the cases. Whereas Mr. Newton reduced all the cases into one, by a power with an indefinite index, and, at the end of his compendium, demonstrated it at once by his method of moments, he being the first who introduced indefinite indices of powers into the operations of analysis. Also, by the 108th proposition of the said Arithmetica Infinitorum, and by several other propositions which follow it; if the ordinate be composed of two or more ordinates, taken with their signs + and —, the area will be composed of two or more areas, taken with their signs + and — respectively. And this is assumed by Mr. Newton as the second rule on which he founds his method of quadratures.

And the third rule is, to reduce fractions and radicals, and the affected roots of equations into converging series, when the quadrature does not otherwise sncceed; and by the first and second rules to square the figures, whose ordinates are the single terms of the series. Mr. Newton, in his letter to Mr. Oldenburg, dated June 13, 1676, and communicated to Mr. Leibnitz, taught how to reduce any power of any binominal into a converging series, and how by that series to square the curve, whose ordinate is that power. And being desired by Mr. Leibnitz to explain the origin of this theorem, he replied in his letter, dated Oct. 24, 1676, that a little before the plague, which raged in London in the year 1665, on reading the Arithmetica Infinitorum of Dr. Wallis, and considering how to interpolate the series x, $x - \frac{1}{3}x^3$, $x - \frac{2}{3}x^3 + \frac{1}{5}x^5$, $x - \frac{3}{3}x^3 + \frac{3}{5}x^5 - \frac{1}{7}x^7$, &c. he found the area of a circle to be $x - \frac{\frac{1}{2}x^3}{3} - \frac{\frac{1}{8}x^5}{5} - \frac{\frac{1}{16}x^7}{7} - \frac{\frac{5}{128}x^9}{9}$ — &c. And, by pursuing the method of interpolation, he found the theorem abovementioned; and by means of this theorem he found the reduction of fractions and surds into converging series, by division and extraction of roots; and then proceeded to the extraction of affected roots. And these reductions are his third rule.

When Mr. Newton had, in this compendium, explained these three rules, and illustrated them with various examples, he laid down the idea of deducing the area from the ordinate, by considering the area as a nascent quantity, growing or increasing by continual flux, in proportion to the length of the ordinate; and supposing the abscissa to increase uniformly in proportion to time. And from the moments of time he gave the name of moments to the momentaneous increases, or infinitely small parts of the abscissa and area, generated in moments of time. The moment of a line he called a point, in the sense of Cavallerius, though it be not a geometrical point, but a line infinitely short; and the moment of an area, or superficies, he called a line, in the sense of Cavallerius, though it be not a geometrical line, but a superficies infinitely narrow; and when he considered the ordinate as the moment of the area, he understood by it the rectangle under the geometrical ordinate and a moment of the abscissa, though that moment be not always expressed. "Let ABD, pl. 4, fig. 2, says he, be any curve, and AHKB a rectangle, whose side AH or KB is unity; and suppose the right line DBK, moving uniformly from AH, to describe the areas ABD and AK;

and that the right line BK, 1, is the moment by which the area AK, x, gradually increases, and the right line BD, y, the moment by which the curvilinear area ABD gradually increases; and that from the moment BD continually given, you may by the three preceding rules investigate the area ABD, described by it, or compare it with the area AK, x, described by the moment 1." This is Mr. Newton's idea of the work in squaring of curves, and how he applies this to other problems, he expresses in the next words. "Now, says he, by the same method that the superficies ABD is found, by the three foregoing rules, from its moment being continually given, by the very same any other quantity may be found from its moment in like manner given." And after some examples he adds his method of regression from the area, arc, or solid content, to the abscissa; and shows how the same method extends to mechanical curves, for determining their ordinates, tangents, areas, lengths, &c. And that by assuming any equation, expressing the relation between the area and abscissa of a curve, you may find the ordinate by this method. And this is the foundation of the method of fluxions and moments, which Mr. Newton in his letter, dated Oct. 24, 1676, comprehended in this sentence; data æquatione quotcunque fluentes quantitates involvente, invenire fluxiones; et vice versa; that is, from a given equation involving any number of fluents, or flowing quantities, to find the fluxions, and vice versa.

In this compendium, Mr. Newton represents the uniform fluxion of time, or of any exponent of time, by an unit; the moment of time, or of its exponent by the letter o; the fluxions of other quantities by any other symbols; the moments of those quantities by the rectangles under those symbols and the letter o; and the area of a curve by the ordinate inclosed in a square, the area being put for a fluent, and the ordinate for its fluxion. When he is demonstrating any proposition he uses the letter o for the finite moment of time, or of its exponent, or of any quantity flowing uniformly, and performs the whole calculation by the geometry of the ancients, in finite figures or schemes, without any approximation; and as soon as the calculation is at an end, and the equation is reduced, he supposes that the moment o decreases in infinitum and vanishes. But when he is not demonstrating, but only investigating a proposition, for making dispatch he supposes the moment o to be infinitely little, and forbears to write it down, using all manner of approximations, which he conceives will produce no error in the conclusion. An example of the first kind you have in the end of this compendium, in demonstrating the first of the three rules laid down in the beginning of the book. Examples of the second kind you have in the same compendium, in finding the length of curve lines, p. 15, and in finding the ordinates, areas, and lengths of mechanical curves, p. 18, 19. And he tells you, p. 19, that by the same method, tangents may be drawn to mechanical curves. And in his letter of Dec. 10, 1672, he adds, that problems about the curvature of curves, geometrical or mechanical, are resolved by the same method. Whence it is manifest, that he had then extended the method to the second and third moments. For when the areas of curves are considered as fluents, as is usual in this analysis, the ordinates express the first fluxions, the tangents are given by the second fluxions, and the curvatures by the third. And even in this Analysis, p. 16, where Mr. Newton says, momentum est superficies cum de solidis, et linea cum de superficiebus, et punctum cum de lineis agitur, it is all one as if he had said, that when solids are considered as fluents, their moments are superficies, and the moments of those moments, or second moments, are lines, and the moments of those moments, or third moments, are points, in the sense of Cavallerius. And in his Principia Philosophiæ, where he frequently considers lines as fluents described by points, whose velocities increase or decrease, the velocities are the first fluxions, and their increase the second. And the problem, data æquatione fluentes quantitates involvente fluxiones invenire et vice versa, extends to all the fluxions, as is manifest by the examples of its solution, published by Dr. Wallis, tom. 2, p. 391, 392, 396. And in lib. 2, Princip. prop. 14, he calls the second difference the difference of moments.

Now the better to know what kind of calculation Mr. Newton used, in or before the year 1669, when he wrote this compendium of his Analysis, I will here set down his demonstration of the first rule above-mentioned. " Let the base AB, of any curve ADδ, fig. 3, be $= x$, and the ordinate BD $= y$, and the area ABD $=$ z, as before; likewise let B$\beta = o$, BK $= v$, and the rectangle BβHK $(ov) =$ the space B$\beta\delta$D; therefore, Aβ will be $= x + o$, and A$\delta\beta =$ z $+ ov$. These things being premised, from assuming at pleasure the relation between x and z, y is sought for as follows: let the equation $\frac{2}{3}x^{\frac{3}{2}} =$ z, or $\frac{4}{9}x^3 =$ zz, be taken at pleasure: then substituting $x + o$ or Aβ for x, and z $+ ov$ or A$\delta\beta$ for z, you have $\frac{4}{9}$ into $x^3 + 3x^2o + 3xo^2 + o^3 =$ (from the nature of the curve) z$^2 +$ 2z$ov + o^2v^2$: and taking away equals, viz. $\frac{4}{9}x^3$ and zz, and dividing the rest by o, there remains $\frac{4}{9}$ into $3x^2 + 3xo + o^2 = 2$z$v + ov^2$. Now if we suppose, that Bβ decreases in infinitum and vanishes, or that o is nothing, v and y will be equal, and the terms multiplied by o will vanish; consequently there will remain $\frac{4}{9} \times 3xx = 2zv$, or $\frac{2}{3}xx$ $(=$ z$y) = \frac{2}{3}x^{\frac{3}{2}}y$, or $x^{\frac{1}{2}}(= \frac{x^2}{x^{\frac{3}{2}}}) = y$. Therefore, è contra, if $x^{\frac{1}{2}} = y$, $\frac{2}{3}x^{\frac{3}{2}}$ will be $=$ z, or universally, if $\frac{n}{m+n} \times ax^{\frac{m+n}{n}} = z$; or putting

$\frac{na}{m+n} = c$, and $m + n = p$; if $cx^{\frac{p}{n}} = z$, or $c^n x^p = z$; then putting $x + o$ for x, and $z + ov$, or (which is the same thing) substituting $z + oy$ for z, you have $c^n \times x^p + pox^{p-1}$ &c. $= z^n + noyz^{n-1}$ &c. viz. by omitting the other terms of the series, which at length would vanish. Now taking away the equal terms $c^n x^p$ and z^n, and dividing the rest by o, there remains $c^n px^{p-1} = nyz^{-1}$ $\left(= \frac{nyz^n}{z} = \frac{nyc^n x^p}{cx^{\frac{p}{n}}}\right)$ or dividing by $c^n x^p$, then px^{-1} will be $= \frac{ny}{cx^{\frac{p}{n}}}$ or $pcx^{\frac{p-n}{n}} = ny$; or by restoring again $\frac{na}{m+n}$ instead of c, and $m + n$ instead of p, that is, m instead of $p - n$, and na instead of pc, $ax^{\frac{m}{n}}$ will be $= y$. Therefore, è contra, if $ax^{\frac{m}{n}}$ be $= y$, then $\frac{n}{m+n} ax^{\frac{m+n}{n}}$ will be $= z$." Q. E. D.

By the same way of working, the second rule may be also demonstrated. And if any equation whatever be assumed, expressing the relation between the abscissa and area of a curve, the ordinate may be found in the same manner, as is mentioned in the next words of the Analysis. And if this ordinate drawn into an unit be put for the area of a new curve, the ordinate of this new curve may be found by the same method, and so on perpetually. And these ordinates represent the first, second, third, fourth, and following fluxions of the first area. Such then was Mr. Newton's way of working in those days, when he wrote this compendium of his Analysis. And the same way of working he used in his book of quadratures, and still uses to this day.

Among the examples with which he illustrates the method of series and moments, set down in this compendium, are these. Let the radius of a circle be 1, the arc z, and the sine x; then the equations for finding the arc whose sine is given, and the sine whose arc is given, will be

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \text{\&c.}$$
$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \text{\&c.}$$

Mr. Collins informed Mr. Gregory of this method in Autumn 1669; and Mr. Gregory, by the help of one of Mr. Newton's series, after a year's study, found out the method in Dec. 1670; and two months after, in a letter dated Feb. 15, 1671, he sent several theorems, discovered thereby, to Mr. Collins, with leave to communicate them freely. And Mr. Collins was very free in communicating what he had received, both from Mr. Newton and from Mr. Gregory, as appears by his letters printed in the Commercium. Among the series which Mr. Gregory sent in the said letter, were these two: let the radius of a circle be r, the arc a, and the tangent t, the equations for finding the arc whose tangent is given, and the tangent whose arc is given, will be these:

$$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.$$

$$t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \&c.$$

In this year, 1671, Mr. Leibnitz published two Tracts at London, the one dedicated to the Royal Society, the other dedicated to the Academy of Sciences at Paris; and in the dedication of the first he mentioned his correspondence with Mr. Oldenburg.

In February 1672-3, Mr. Leibnitz meeting Dr. Pell at Mr. Boyle's, he pretended to the differential method of Mouton. And though he was shown by Dr. Pell, that it was Mouton's method, he persisted in maintaining it to be his own invention, because he had found it out himself, without knowing what Mouton had done before, and had much improved it.

When one of Mr. Newton's Series was sent to Mr. Gregory, he tried to deduce it from his own Series combined together, as he mentions in his letter dated Dec. 19, 1670. And by some such method, Mr. Leibnitz, before he left London, seems to have found the sum of a series of fractions decreasing in infinitum, whose numerator is a given number, and denominators are triangular, or pyramidal, or triangulo-triangular numbers, &c. Behold the mystery! from the Series $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} +$ &c. subduct all the terms except the first (viz. $\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5}$ &c.) and there will remain $1 = 1 - \frac{1}{2} + \frac{1}{2} - \frac{1}{3} + \frac{1}{3} - \frac{1}{4} + \frac{1}{4} - \frac{1}{5} + \frac{1}{5}$ &c. $= \frac{1}{1 \times 2} + \frac{1}{2 \times 3} + \frac{1}{3 \times 4} + \frac{1}{4 \times 5} +$ &c. And from this Series take all the terms except the first, and there will remain $\frac{1}{2} = \frac{2}{1 \times 2 \times 3} + \frac{2}{2 \times 3 \times 4} + \frac{2}{3 \times 4 \times 5} + \frac{2}{4 \times 5 \times 6} +$ &c. And from the first Series take all the terms except the first two, and there will remain $\frac{3}{2} = \frac{2}{1 \times 3} + \frac{2}{2 \times 4} + \frac{2}{3 \times 5} + \frac{2}{4 \times 6} +$ &c.

About the end of February or beginning of March, 1672-3, Mr. Leibnitz went from London to Paris, and continuing his correspondence with Mr. Oldenburg and Mr. Collins, wrote in July 1674, that he had a wonderful Theorem, which gave the area of a circle, or any sector of it exactly, in a Series of rational numbers; and in October following, that he had found the circumference of a circle in a Series of very simple numbers; and that by the same method (so he calls the said Theorem) any arc whose sine was given, might be found in a like Series, though the proportion to the whole circumference be not known. His Theorem therefore was for finding any sector or arc whose sine was given. If the proportion of the arc to the whole circumference was not known, the Theorem or method gave him only the arc; if it was known, it gave him also the whole circumference: and therefore it was the first of Mr. Newton's two Theorems abovementioned. But the demonstration of this Theorem Mr. Leibnitz wanted. For in his letter of May 12, 1676, he desired Mr. Oldenburg to procure the demonstration from Mr. Collins, meaning the method by which Mr. Newton had invented it.

In a letter written by Mr. Collins, and dated April 15, 1675, Mr. Oldenburg sent to Mr. Leibnitz eight of Mr. Newton's and Mr. Gregory's Series; among which were Mr. Newton's two Series abovementioned, for finding the arc whose sine is given, and the sine whose arc is given; and Mr. Gregory's two Series abovementioned for finding the arc whose tangent is given, and the tangent whose arc is given. And Mr. Leibnitz, in his answer, dated May 20, 1675, acknowledged the receipt of this letter in these words.

" I received your letter, containing a great deal of algebraical knowledge; for which I thank you and the learned Mr. Collins. But being now very much occupied; besides my ordinary business, especially with mechanical affairs, I could not examine the Series you sent me, and compare them with my own; as soon as I shall have done it, I shall give you my opinion; for, it is some years ago since I invented my Series in a certain very peculiar way."

But yet Mr. Leibnitz never took any further notice of his having received these Series, nor how his own differed from them, nor ever produced any other Series than those which he received from Mr. Oldenburg, or numeral Series deduced from them in particular cases. And what he did with Mr. Gregory's Series, for finding the arc whose tangent is given, he has told us in the Acta Eruditorum for the month of April 1691, p. 178. Now in the year 1675, says he, I had composed a small tract of Arithmetical Quadrature, which from that time was read by my friends, &c. By a Theorem for transmuting figures, like those of Dr. Barrow and Mr. Gregory, he had now found a demonstration of this Series; and this was the subject of his Opusculum. But he still wanted a demonstration of the rest: and meeting with a pretence to ask for what he wanted, he wrote to Mr. Oldenburg the following letter, dated at Paris, May 12, 1676: " Since M. George Mohr, a Dane, brought me the method of expressing the ratio between the arch and the sine by the following infinite Series, which had been communicated to him by your learned Mr. Collins; viz. putting x for the sine, z for the arch, and 1 for the radius,

$$z = x + \tfrac{1}{6} x^3 + \tfrac{3}{40} x^5 + \tfrac{5}{112} z^7 + \tfrac{35}{1152} x^9 + \&c.$$

$$x = z - \tfrac{1}{6} z^3 + \tfrac{1}{120} z^5 - \tfrac{1}{5040} z^7 + \tfrac{1}{362880} z^9 - \&c.$$

since, I say, he brought me these, which appear to me to be very ingenious, and especially the latter Series, which has a certain peculiar elegance; therefore, Sir, you will oblige me very much, if you send me the demonstration; in return I shall send you my thoughts on this matter, which are very different from these, and of which I think I have written you now some years ago, without adding the demonstration, which I am now about polishing; I intreat you would remember me very kindly to Mr. Collins, who can enable you to satisfy my desire."

Here, by the word Inquam (I say), one would think that he had never seen these two Series before, and that his diversa circa hanc rem meditata were something else than one of the Series which he had received from Mr. Oldenburg the year before, and a demonstration of it, which he was now polishing, to make the present an acceptable recompence for Mr. Newton's method.

On the receipt of this letter, Mr. Oldenburg and Mr. Collins wrote pressingly to Mr. Newton, desiring that he himself would describe his own method, to be communicated to Mr. Leibnitz. On which Mr. Newton wrote his letter, dated June 13, 1676, describing the method of Series, as he had done before in the Compendium abovementioned, but with this difference, that here he described at large the reduction of the power of a binomial into a Series, and only touched on the reduction by division and extraction of affected roots; because in the Compendium he had already described these latter: there he described at large the reduction of fractions and radicals into Series by division and extraction of roots, and only set down the first two terms of the Series into which the power of a binomial might be reduced. And among the examples in this letter, there were Series for finding the number whose logarithm is given, and for finding the versed sine whose arc is given. This letter was sent to Paris June 26, 1676, with a MS. drawn up by Mr. Collins, containing Extracts of Mr. James Gregory's Letters. For Mr. Gregory died near the end of the year 1675; and Mr. Collins, at the request of Mr. Leibnitz, and some others of the Academy of Sciences, drew up Extracts of his Letters, and the collection is still extant in the hand writing of Mr. Collins, with this title; "Extracts of Mr. Gregory's Letters, to be lent to Mr. Leibnitz to peruse, who is desired to return the same to you." And that they were sent, is affirmed by Mr. Collins, in his letter to Mr. David Gregory, the brother of the deceased, dated August 11, 1676; and appears further by the answers of Mr. Leibnitz and Mr. Tschurnhause, concerning them.

The answer of Mr. Leibnitz, directed to Mr. Oldenburg, and dated August 27, 1676, begins thus; "Your letter, dated July 26, contains more, and those more remarkable things in analysis, than many voluminous books published on this subject. Therefore I thank you, Mr. Newton, and Mr. Collins, for communicating to me so many curious things." And towards the end of the letter, after he had done with the contents of Mr. Newton's letter, he proceeds thus: "I come now to other things contained in your letter, which the learned Mr. Collins was pleased to communicate; I wish he had added, the demonstration of Mr. Gregory's Linear Approximation; for he certainly had a genius for promoting such speculations." And the answer of Mr. Tschurnhause, dated Sept, 1, 1676, after he had done with Mr. Newton's letter about Series, concludes thus: "And what that excellent geometrician Mr. Gregory has done in this matter are certainly extraordinary. And indeed those who shall cause his MS. to be published, will do the greatest service to his reputation." In the first part of this letter, where Mr. Tschurnhause speaks of Mr. Newton's Series, he says, that he looked over them cursorily, to see if he could find the Series of Mr. Leibnitz for squaring the circle or hyperbola. If he had searched for it in the Extracts of Gregory's Letters, he might have found it in the letter of Feb. 15, 1671, abovementioned. For the MS. of those Extracts, with that letter in it, is still extant in the hand-writing of Mr. Collins.

And though Mr. Leibnitz had now received this Series twice from Mr. Oldenburg, yet in his letter of August 27, 1676, he sent it back to him by way of recompence for Mr. Newton's Method, pretending that he had communicated it to his friends at Paris 3 years before, or more; that is, 2 years before he received it in Mr. Oldenburg's letter of April 15, 1675; at which time he did not know it to be his own, as appears by his answer of May 20, 1675, abovementioned. He might receive this Series at London, and communicate it to his friends at Paris, above 3 years before he sent it back to Mr. Oldenburg: but it does not appear that he had the demonstration of it so early. When he found the demonstration, then he composed it in his Opusculum, and communicated that also to his friends; and he himself has told us that this was in the year 1675. However, it lies upon him to prove that he had this Series before he received it from Mr. Oldenburg. For in his answer to Mr. Oldenburg he did not know any of the Series then sent him to be his own; and concealed from the gentlemen at Paris his having received it from Mr. Oldenburg with several other Series, and his having seen a copy of the letter in which Mr. Gregory had sent it to Mr. Collins, in the beginning of the year 1671.

In the same letter, of August 27, 1676, after Mr. Leibnitz had described his quadrature of the circle and equilateral hyperbola, he adds:

"Again from the Series of regressions I found the following for the hyperbola, viz. if any number be less than unity, as $1-m$, and its hyperbolic logarithm be l; m will be $= \frac{l}{1} - \frac{l^2}{1 \times 2} + \frac{l^3}{1 \times 2 \times 3} - \frac{l^4}{1 \times 2 \times 3 \times 4} +$ &c. If the number be greater than unity, as $1 + n$, then for finding it, I have likewise discovered the rule, expressed in Mr. Newton's letter; viz. n will be $= \frac{l}{1} + \frac{l^2}{1 \times 2} + \frac{l^3}{1 \times 2 \times 3} + \frac{l^4}{1 \times 2 \times 3 \times 4} +$ &c. And as to the regression from arches, I directly lighted upon the rule that from the given arch gives the co-sine; viz. the co-sine $= 1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4} -$ &c. But afterwards I likewise found, that from this rule might be demonstrated that other communicated to me, for finding the right sine, which is $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} -$ &c. Thus Mr. Leibnitz put in his claim for the co-invention of these four Series, though the method of finding them was sent him at his own request, and he did not yet understand it. For in this same letter of August 27, 1676, he desired Mr. Newton to explain it further. His words are: "But I wish Mr. Newton would explain some things further; as, the origin of the Theorem he first lays down; also the method by which in his operations, he found the quantities p, q, r; and lastly, how he proceeds in the method of regressions, as when, from the logarithm the number is sought. For he does not explain how that is deduced from his method."

He pretended to have found two Series for the number whose logarithm was given, and yet in the same letter he desired Mr. Newton to explain to him the method of finding those very two Series.

When Mr. Newton had received this letter, he wrote back that all the said four Series had been communicated by him to Mr. Leibnitz; the first two being one and the same Series in which the letter l was put for the logarithm with its sign $+$ or $-$; and the third being the excess of the radius above the versed sine, for which a Series had been sent to him. On which Mr. Leibnitz desisted from his claim. Mr. Newton also in the same letter, dated Oct. 24, 1676, further explained his methods of regression, as Mr. Leibnitz had desired. And Mr. Leibnitz, in his letter of June 21, 1677, desired a further explanation: but soon after, on reading Mr. Newton's letter a second time, wrote back July 12, 1677, that he now understood what he wanted; and found by his old papers, that he had formerly used one of Mr. Newton's methods of regression; but in the example which he had then by chance made use of, there being produced nothing elegant, he had, out of his usual impatience, neglected to use it any further. He had therefore several direct Series, and consequently a method of finding them, before he invented and forgot the inverse method. And if he had searched his old papers diligently, he might have found this method also there; but having forgot his own methods, he wrote for Mr. Newton's.

When Mr. Newton in his letter dated June 13, 1676, had explained his method of Series, he added: "From these things it appears how much the bounds of analysis are enlarged by such sort of infinite equations: for, by their means, it extends itself, I had almost said, to all problems, excepting the numeral ones of Diophantus, and the like; yet it does not become quite universal, unless by some other methods of finding out infinite series. For, there are some problems in which we cannot come to infinite series either by division, or extraction of simple or affected roots. But how we are to proceed in these cases, I have not leisure to show; nor to mention some other things I have invented about the reduction of infinite Series into finite ones, where the nature of the thing will bear it. For I forbore writing on these speculations, now for almost these 5 years past, because I have long since been tired of them." To this M. Leibnitz in his letter of August 27, 1676, answered: "What you seem to say, that almost all difficulties (excepting Diophantus's Problems) may be reduced to infinite Series, I cannot come into; for there are several Problems so intricate and perplexed, as not to depend either on equations, or quadratures: such are, among a great many others, the Problems of the inverse method of tangents." And Mr. Newton in his letter of Oct. 24, 1676, replied: "When I said, that all Problems might be solved; I would be understood to mean those especially, about which mathematicians have already employed themselves, or at least such as can admit of mathematical reasoning. For it is true, others may be devised, so involved with perplexed conditions, that we cannot sufficiently comprehend them, and much less bear the fatigue of such prodigious calculations, as they may require. However, that I may not seem to exceed the bounds of modesty, I can resolve both the inverse Problems of tangents, and others more difficult. And for this purpose I make use of a twofold method; the one more concise, and the other more general. At present I thought proper to express both in transposed letters, that I might not be obliged, on account of others finding out the same thing, to alter my design in some respects; thus, 5*accdæ*10*effh*, &c. that is, one method consists in finding the fluent, or flowing quantity, from an equation involving it with its fluxion; the other method, only in assuming a series for any unknown quantity, from which the rest may be commodiously deduced; and in comparing the homologous terms of the equation resulting to find out the terms of the assumed Series." By these two letters of Mr. Newton's, it is certain, that he had then, or rather upwards of 5 years before, found out the reduction of Problems to fluxional equations, and converging Series; and by M. Leibnitz's answer to the first of those letters, it is as certain that he had not then found out the reduction of Problems, either to different equations, or to converging Series.

And the same is manifest also by what Mr. Leibnitz wrote in the Acta Eruditorum, anno 1691, concerning this matter.

"Now in 1675, says he, I had composed a small treatise of arithmetical quadrature, which from that time had been perused by my friends; but the matter enlarging under my hands, I had not leisure to prepare it for the press, and other avocations interfered afterwards; especially now as it does not seem worth while to explain prolixly in the common way, what my analysis performs briefly." This quadrature, composed in the common manner, he began to communicate at Paris in the year 1675. The next year he was polishing the demonstration of it, to send it to Mr. Oldenburg in recompence for Mr. Newton's method, as he wrote to him May 12, 1676; and accordingly in his letter of August 27, 1676, he sent it, composed and polished in the common manner. The winter following he returned into Germany, by England and Holland, to enter on public business, and had no longer any leisure to fit it for the press, nor thought it afterwards worth his while to explain those things prolixly in the vulgar manner, which his new analysis exhibited in short. He found out this new analysis therefore after his return into Germany, and consequently not before the year 1677.

The same is further manifest by the following consideration. Dr. Barrow published his method of tangents in the year 1670. Mr. Newton in his letter dated December 10, 1672, communicated his method of tangents to Mr. Collins, and added: "This is one particular, or rather corollary, of the general method, which extends without any troublesome calculation, not only to the drawing of tangents to any kind of curves, either geometrical, or mechanical, or any how regarding right lines, or other curves; but also to the solving of other more abstruse problems, of curvatures, areas, lengths, centres of gravity of curves, &c. Nor is it confined, (like Hudden's method de maximis et minimis) only to such equations as have no surd quantities. I have added this method to that other, in which I give an exegesis of equations, by reducing them into infinite series. M. Slusius sent his method of tangents to Mr. Oldenburgh. Jan. 17, 167$\frac{2}{3}$, and the same was soon after published in the Transactions. It proved to be the same with that of Mr. Newton. It was founded on three lemmas; the first of which was this, "The difference of two powers of the same degree, divided by the difference of the sides or roots, gives the members of the next lower degree of the binomial of the sides; as $\frac{y^3 - x^3}{y - x} = yy + yx + xx$, that is, in the notation of Mr. Leibnitz $\frac{dy^3}{dy} = 3yy$." A copy of Mr. Newton's letter, of Dec. 10, 1672, was sent to Mr. Leibnitz by Mr. Oldenburg, among the papers of Mr. James Gregory, at the same time with Mr. Newton's letter of June 13, 1676. And Mr. Newton having described, in these two letters, that he had a very general analysis, consisting partly of the method of converging series, partly of another method, by which he applied those series to the solution of almost all problems (except perhaps some numeral ones like those of Diophantus) and found the tangents, areas, lengths, solid contents, centres of gravity, and curvitures of curves, and curvilinear figures geometrical or mechanical, without sticking at surds; and that the method of tangents of Slusius was but a branch or corollary of this other method: Mr. Leibnitz, on his returning home through Holland, was meditating on the improvement of the method of Slusius. For in a letter to Mr. Oldenburg, dated from Amsterdam, Nov. $\frac{18}{28}$, 1676, he wrote thus: "The method of tangents, published by Slusius, does not reach so far but that something farther might be done in that kind, which would be of very great use in all sorts of problems, even in my method (without extractions) of reducing equations to series; to wit, a certain short table of tangents might be calculated, and continued so far, till its progression appear, so that any one might continue it as far as he pleased, without any calculation." This was the improvement of the method of Slusius into a general method, which Mr. Leibnitz was then thinking on, and by his words, "Something further might be done in that kind, which would be of very great use in all sorts of problems," it seems to be the only improvement which he had then in his mind, for extending the method to all sorts of problems. The improvement by the differential calculus was not yet in his mind, but must be referred to the next year.

Mr. Newton, in his next letter dated Oct. 24, 1676, mentioned the analysis communicated by Dr. Barrow to Mr. Collins, in the year 1669, and also another tract written in 1671, about converging series, and about the other method by which tangents were drawn after the method of Slusius, and maxima and minima were determined, and the quadrature of curves was made more easy, and this without sticking at radicals, and by which series were invented which brake off, and gave the quadrature of curves in finite equations, when it was possible. And the foundation of these operations he comprehended in this sentence, expressed enigmatically as above, having given an equation involving any number of fluent quantities, to find the fluxions, and vice versa. Which puts it past all dispute that he had invented the method of fluxions before that time. And if other things in that letter be considered, it will appear that he had then brought it to great perfection, and made it exceedingly general; the propositions in his book of quadratures, and the methods of converging series, and of drawing a curve line through any number of given points, being then known to him. For when the method of fluxions proceeds not in finite equations, he reduces the equations into converging series by the binomial theorem, and by the extraction of fluents out of equations involving or not involving their fluxions. And when finite equations are wanting, he deduces converging series from the conditions of the problem, by assuming the terms of the series gradually, and determining them by those conditions. And when fluents are to be derived from fluxions, and the law of the fluxions is wanting, he finds that law very nearly, by drawing a parabolic line through any number of given points. And by these improvements Mr. Newton had, in those days, made his method of fluxions much more universal, than the differential method of Mr. Leibnitz is at present.

This letter of Mr. Newton's, dated Oct. 24, 1676, came to the hands of Mr. Leibnitz in the end of the winter or beginning of the spring following; and Mr. Leibnitz soon after, viz. in a letter dated June 21, 1677, wrote back: "I agree with Mr. Newton, that Slusius's method of tangents is still imperfect: and I have long since treated the subject more generally, viz. by the differences of the ordinates.—Hence in future, calling dy the difference of the two nearest y, &c." Here Mr. Leibnitz began first to propose his differential method, and there is not the least evidence that he knew it before the receipt of Mr. Newton's last letter. He says indeed, "That he had long since treated the subject of tangents more generally, viz. by the differences of the ordinates:" and so he affirmed in other letters, that he had invented several converging series direct and inverse, before he had the method of inventing them; and had forgot an inverse method of series before he knew what use to make of it. But no man is a witness in his own cause. A judge would be very unjust, and act contrary to the laws of all nations, who should admit any man to be a witness in his own cause. And therefore it is incumbent on Mr. Leibnitz to prove that he found out this method long before the receipt of Mr. Newton's letters. And if he cannot prove this, the question, who was the first inventor of the method, is decided.

The Marquis de l'Hospital, in the preface to his Analyse des infiniments petits, published A. C. 1696, tells us, "that a little after the publication of the method of tangents of Descartes, Mr. Fermat found also a method, which Descartes himself at length allowed to be, for the most part, more simple than his own. But that it was not yet so simple as Mr. Barrow afterwards made it, by considering more nearly the nature of polygons, which offers naturally to the mind a little triangle, composed of a particle of the curve lying between two ordinates infinitely near each other, and of the difference of these two ordinates, and of that of the two correspondent abscissas. And this triangle is like that which ought to be made by the tangent, the ordinate, and the subtangent: so that by one simple analogy, this last method saves all the calculation which was requisite, either in the method of Descartes, or in this same method before. Mr. Barrow stopped not here; he invented also a sort of calculation proper for this method. But it was necessary in this, as well as in that of Descartes, to take away fractions and radicals, for making it useful. On the defect of this calculus, that of the celebrated Mr. Leibnitz was introduced, and this learned geometrician began where Mr. Barrow and others left off. This his calculus led into regions hitherto unknown, and there made discoveries which astonished the most able mathematicians of Europe, &c." Thus far the Marquis. He had not seen Mr. Newton's Analysis, nor his Letters of Dec. 10, 1672, of June 13, 1676, and Oct. 24, 1676: so that, not knowing that Mr. Newton had done all this, and signified it to Mr. Leibnitz, he reckoned that Mr. Leibnitz began where Mr. Barrow left off, and by teaching how to apply Mr. Barrow's method without sticking at fractions and surds, had enlarged the method wonderfully. And Mr. James Bernoulli, in the Acta Eruditorum of January 1691, p. 14, writes thus: "Whoever understands Dr. Barrow's calculus (which he sketched out in his Geometrical Lectures, and of which all the propositions, there contained, are specimens) can scarcely be ignorant of that other, invented by Mr. Leibnitz, since it is founded on the former, and differs not from it, unless perhaps in the notation of differentials, and some compendia in the operation."

Now Dr. Barrow, in his Method of Tangents, draws two ordinates indefinitely near each other, and puts the letter a for the difference of the ordinates, and the letter e for the difference of the abscissas: and for drawing the tangent gives these three rules: 1. "In computing, says he, I cast away all the terms in which the power of a or e is found, or in which they are multiplied into themselves: for these terms will become inconsiderable. 2. After constituting the equation, I cast away all the terms, consisting of symbols that denote known or determinate quantities, or in which either a or e is not found: for these terms, being always brought to one side of the equation, will be equal to nothing. 3. I substitute the ordinate for a, and the subtangent for e; hence at length the quantity of the subtangent will be known." Thus far Dr. Barrow.

And Mr. Leibnitz, in his letter of June 21, 1677, above-mentioned, wherein he first began to propose his differential method, has followed this method of tangents exactly, excepting that he has changed the letters a and e of Dr. Barrow, into dx and dy. For in the example which he there gives, he draws two parallel lines, and sets all the terms below the under line, in which dx and *dy* are (severally or jointly) of more than one dimension, and all the terms above the upper line, in which *dx* and *dy* are wanting, and for the reasons given by Dr. Barrow, makes all these terms vanish. And by the terms in which *dx* and *dy* are of only one dimension, and which he sets between the two lines, he determines the proportion of the subtangent to the ordinate. Well therefore did the Marquis de l'Hospital observe, that where Dr. Barrow left off, Mr. Leibnitz began: for their methods of tangents are exactly the same.

But Mr. Leibnitz adds this improvement of the method, that the conclusion of this calculus is coincident with the rule of Slusius, and shows how that rule presently occurs to any one who understands this method. For Mr. Newton had represented in his letters, that this rule was a corollary of his general method.

And whereas Mr. Newton had said that his method in drawing of tangents, and determining maxima and minima, &c. proceeded without sticking at surds; Mr. Leibnitz, in the next place, shows how this method of tangents may be improved so as not to stick at surds or fractions, and then adds: "I suppose that what Mr. Newton would conceal, about the method of drawing tangents, does not differ from this. And what confirms me in this is, that he adds, that from the same foundation quadratures may also be rendered more easy; for every figure is always quadrable, when the ordinate drawn into that differential of the absciss, becomes the differential of any quantity. By which words, compared with the preceding calculation, it is manifest that Mr. Leibnitz, at this time, understood that Mr. Newton had a method which would do all these things, and had been examining whether Dr. Barrow's differential method of tangents might not be extended to the same performances.

In November 1684, Mr. Leibnitz published the Elements of this Differential Method, in the Acta Eruditorum, and illustrated it with examples, of drawing tangents and determining maxima and minima, and then added: and these indeed are the rudiments of a certain kind of sublimer geometry, which extends even to the most curious and difficult problems of mixed mathematics, and which without the differential calculus, or some such method, are not easily to be attempted. The words *some such method,* plainly relate to Mr. Newton's method: and the whole paragraph contains nothing more than what Mr. Newton had affirmed of his general method, in his letters of 1672 and 1676.

And in the Acta Eruditorum of June 1686, p. 297, Mr. Leibnitz added: "I choose rather to make use of *dx*, and the like, than of letters for them, because *dx* is a certain kind of modification of *x* itself, &c." He knew very well that in this method he might have used letters with Dr. Barrow, but he chose rather to use the new symbols *dx* and *dy*, though there is nothing which can be done by these symbols, but may be done by single letters.

The next year Mr. Newton's Principia Philosophiæ came out, a book full of such problems as Mr. Leibnitz had called the most curious and difficult, &c. And the Marquis de L'Hospital thus speaks of it, presque tout de calcul; composed almost wholly of this calculus. And Mr. Leibnitz himself, in a letter to Mr. Newton, dated from Hanover, March $\frac{7}{17}$, 1693, and still extant in his own hand-writing, and communicated to the Royal Society, acknowledged the same thing in these words: "You had surprisingly enlarged geometry by your series, but by your Principia you have shown that you have penetrated into what was beyond the reach of the common analysis. I also have endeavoured, by using proper symbols for expressing the sums and differences, to reduce to a kind of analysis, that geometry which I call transcendent, and not without success." And again, in his answer to Mr. Fatio, printed in the Acta Eruditorum of May 1700, p. 203, l. 21, he acknowledged the same thing. In the second lemma of the second book of these Principles, the elements of this calculus are demonstrated synthetically, and at the end of the lemma there is a scholium in these words: "When I signified, in the letters that passed 10 years since, between me and M. Leibnitz, that I had a method of determining maxima and minima, of drawing tangents, &c. which succeeded as well in surd as in rational terms; and concealed the same by transposing the letters including this proposition, having an equation given, that involves any number of fluents or flowing quantities, to find the fluxions, and vice versâ; Mr. Leibnitz replied, that he had likewise hit on such a method, and which he communicated to me, scarcely differing from mine, except in the form of the words and symbols; the foundation of both is contained in this lemma." In those letters, and in another dated Dec. 10, 1672, a copy of which, at that time, was sent to Mr. Leibnitz by Mr. Oldenburg, as is mentioned above, Mr. Newton had so far explained his method, that it was not difficult for Mr. Leibnitz, by the help of Dr. Barrow's method of tangents, to collect it from those letters. And it is certain, by the arguments above-mentioned, that he did not know it before the writing of those letters.

Dr. Wallis had received copies of Mr. Newton's two letters, of June 13 and Oct. 24, 1676, from Mr. Oldenburg, and published several things out of them in his Algebra, printed in English 1683, and in Latin 1693; and soon after had intimation from Holland to print the letters entire, because Mr. Newton's notions of fluxions passed there with applause by the name of the differential method of Mr. Leibnitz. And thereupon he took notice of this matter in the preface to the first volume of his works, published in 1695. And in a letter to Mr. Leibnitz, dated Dec. 1, 1696, (printed in the 3d vol. of Wallis's works) he gave this account of it: "After the last sheet of the preface had been composed at the press, a friend of mine, skilled in such matters, and who happened to be abroad at that time, acquainted me that such a method was then spoken of in Holland, and also that it nearly coincided with Newton's method of fluxions; which made me insert an intimation of it." And in a letter dated April 10, 1695, and lately communicated to the Royal Society, he wrote thus about it: "I wish you would print the two large letters of June and August [he means June and October] 1676. I had intimation from Holland, as desired there by your friends, that somewhat of that kind were done; because your notions (of fluxions) pass there with great applause by the name of Leibnitz's calculus differentials. You are not so kind to your reputation (and that of the nation) as you might be, when you let things of worth lie by you so long, till others carry away the reputation that is due to you. I have endeavoured to do you justice in that point, and am now sorry that I did not print those two letters verbatim."

The short intimation of this matter, which Dr. Wallis inserted into the said preface, was to this effect: "In the 2d volume, among other things, there is Mr. Newton's method of fluxions, as he calls it, of a like nature with Mr. Leibnitz's differential calculus, as he terms it, (as any one who compares both methods will easily find; only under different forms of expression) which I have described cap. 91, and especially cap. 95, from Mr. Newton's two letters, or one of them, dated June 13 and October 24, 1676, written to Mr. Oldenburg, and to be communicated to Mr. Leibnitz (almost in the same words, or at least with little variation, from what is contained in the said letters) where he explains this method to Mr. Leibnitz, which he had invented about 10 years before, if not more, that is in 1666 or 1665; which I hint, that none may allege, I have said nothing of this calculus differentialis."

On this, the editors of the Acta Lipsiensia, for June, the following year, in the stile of Mr. Leibnitz, in giving an account of these first two volumes of Dr. Wallis, took notice of this clause in the Doctor's preface, and complained, not of his saying that Mr. Newton, in his two letters above-mentioned, explained to Mr. Leibnitz the method of fluxions found by him above 10 years before; but that while the Doctor mentioned the differential calculus, and said that he did it, "that none might allege that he had said nothing of the differential calculus," he did not tell the reader that Mr. Leibnitz had this calculus at that time when those letters passed between him and Mr. Newton, by means of Mr. Oldenburg. And in several letters which followed hereupon, between Mr. Leibnitz and Dr. Wallis, concerning this matter, Mr. Leibnitz denied not that Mr. Newton had the method 10 years before the writing of those letters, as Dr. Wallis had affirmed, and pretended not that he himself had the method so early; brought no proof that he had it before the year 1677; no proof even for that, besides the concession of Mr. Newton that he had it so early; affirmed not that he had it earlier; commended Mr. Newton for his candour in this matter; allowed that the methods agreed in the main, and said that he therefore used to call them by the common name of his Infinitesimal Analysis; represented, that as the methods of Vieta and Cartes were called by the common name of Analysis Speciosa, and yet differed in some things; so perhaps the methods of Mr. Newton and himself might differ in some things, and challenged to himself only those things wherein, as he conceived, they might differ, naming the notation, the differential equations, and the exponential equations. But in his letter of June 21, 1677, he reckoned differential equations common to Mr. Newton and himself.

This was the state of the dispute between Dr. Wallis and Mr. Leibnitz at that time. And 4 years after, when Mr. Fatio suggested that Mr. Leibnitz, the second inventor of this calculus, might borrow something from Mr. Newton, the oldest inventor by many years; Mr. Leibnitz in his answer, published in the Acta Eruditorum of May 1700, allowed that Mr. Newton had found the method apart, and did not deny that Mr. Newton was the oldest inventor by many years, nor asserted any thing more to himself than that he also had found the method apart, or without the assistance of Mr. Newton, and pretended that when he first published it, he knew not that Mr. Newton had found any thing more of it than the method of tangents. And in making this defence he added: "Which method, no geometrician that I know of had, before Mr. Newton and me, as none before him gave a public specimen of it, and none before both the Bernoullis and me communicated it." Hitherto therefore Mr. Leibnitz did not pretend to be the first inventor. He did not begin to put in such a claim till after the death of Dr. Wallis, the last of the old men who were acquainted with what had passed between the English and Mr. Leibnitz 40 years since. The Doctor died in October 1703, and Mr. Leibnitz began not to put in this new claim before January 1705.

Mr. Newton published his Treatise of Quadratures in the year 1704. This treatise was written long before, many things being cited out of it in his letters of October 24 and November 8, 1676. It relates to the method of fluxions; and that it might not be taken for a new piece, Mr. Newton repeated what

Dr. Wallis had published 9 years before, without being then contradicted, namely, that this method was invented by degrees in the years 1665 and 1666. Hereupon the editors of the Acta Lipsiensia in January 1705, in the style of Mr. Leibnitz, or Mr. L. himself, in giving an account of this book, represented that Mr. Leibnitz was the first inventor of the method, and that Mr. Newton had substituted fluxions for differences. And this accusation gave a beginning to this present controversy.

For Mr. Keill, in an epistle published in the Philosophical Transactions for September and October 1708, retorted the accusation, asserting, "that Mr. Newton was, beyond all dispute, the first inventor of the arithmetic of fluxions, as would easily appear to any one who reads his letters, published by Dr. Wallis. Though the same method, by only changing the name and manner of notation, was afterwards published by Mr. Leibnitz in the Acta Eruditorum."

Before Mr. Newton saw what had been published in the Acta Leipsica, he expressed himself offended at the printing of this paragraph of Mr. Keill's letter, lest it should create a controversy. And Mr. Leibnitz, understanding it in a stronger sense than Mr. Keill intended it, complained of it as a calumny, in a letter to Dr. Sloane, the secretary, dated March 4, 1711, N. S. and moved that the Royal Society would cause Mr. Keill to make a public recantation. Mr. Keill chose rather to explain and defend what he had written; and Mr. Newton, on being showed the accusation in the Acta Lipsica, gave him leave to do so. But Mr. Leibnitz, in a second letter to Dr. Sloane, dated Dec. 29, 1711, instead of making good his accusation, as he was bound to do, that it might not be deemed a calumny, insisted only on his own candour, as if it would be injustice to question it; and refused to tell how he came by the method; and said that the Acta Lipsica had given every man his due, and that he had concealed the invention above 9 years, (he should have said 7 years) that nobody might pretend (he means that Mr. Newton might not pretend) to have been before him in it; and called Mr. Keill a novice, unacquainted with things past, and one that acted without authority from Mr. Newton, and a clamorous man who deserved to be silenced, and desired that Mr. Newton himself would give his opinion in the matter. He knew that Mr. Keill affirmed nothing more than what Dr. Wallis had published 13 years before, without being then contradicted. He knew that Mr. Newton had given his opinion on this matter, in the introduction to his book of Quadratures, published before this controversy began: but Dr. Wallis was dead; the mathematicians which remained in England were reckoned novices; Mr. Leibnitz may question any man's candour without injustice; and Mr. Newton must now retract what he had published, or must be involved in wrangling disputes.

The Royal Society therefore, having as much authority over Mr. Leibnitz, as over Mr. Keill, and being now twice pressed by Mr. Leibnitz to interpose, and seeing no reason to condemn or censure Mr. Keill, without inquiring into the matter; and that neither Mr. Newton nor Mr. Leibnitz (the only persons alive who knew and remembered any thing of what had passed in these matters 40 years before) could be witnesses for or against Mr. Keill; appointed a numerous committee, to search old letters and papers, and report their opinion on what they might find; and ordered the letters and papers, with the report of their committee, to be published. And by these letters and papers it appeared to them, that Mr. Newton had the method in or before the year 1669; and it did not appear to them, that Mr. Leibnitz had it before the year 1677.

Mr. Leibnitz, to make himself the first inventor of the differential method, has represented that Mr. Newton at first used the letter *o* in the vulgar manner, for the given increment of x, which destroys the advantages of the differential method; but after the writing of his Principia, changed *o* into $\dot{x}$, substituting $\dot{x}$ for dx. It lies upon him to prove that Mr. Newton ever changed *o* into $\dot{x}$, or used $\dot{x}$ for dx, or left off the use of the letter *o*. Mr. Newton used the letter *o* in his Analysis written in or before the years 1669, and in his book of Quadratures, and in his Principia Philosophiæ, and still uses it in the very same sense as at first. In his book of Quadratures he used it in conjunction with the symbol $\dot{x}$, and therefore did not use that symbol in its stead. These symbols *o* and $\dot{x}$ are put for things of a different kind. The one is a moment, the other a fluxion or velocity, as has been explained above. When the letter x is put for a quantity which flows uniformly, the symbol $\dot{x}$ is an unit, and the letter *o* a moment, and $\dot{x}o$ and dx signify the same moment. Printed letters never signify moments, unless when they are multiplied by the moment *o*, either expressed or understood, to make them infinitely little, and then the rectangles are put for moments.

Mr. Newton does not place his method in forms of symbols, nor confine himself to any particular sort of symbols for fluents and fluxions. Where he puts the areas of curves for fluents, he frequently puts the ordinates for fluxions, and denotes the fluxions by the symbols of the ordinates, as in his Analysis. Where he puts lines for fluents, he puts any symbols for the velocities of the points which describe the lines, that is, for the first fluxions; and any other symbols for the increase of those velocities, that is, for the second fluxions, as is frequently done in his Principia Philosophiæ. And where he put the letters x, y, z for fluents, he denotes their fluxions, either by other letters as p, q, r, or by the same letters in other forms as X, Y, Z, or $\dot{x}$, $\dot{y}$, $\dot{z}$, or by any lines, as DE, FG, HI, considered as their exponents. And this is evident by his book of quadratures, where he represents fluxions by pointed letters in the first proposition, by ordinates of curves in the last proposition, and by other symbols, in explaining the method and illustrating it with examples, in the introduction. Mr. Leibnitz has no symbols of fluxions in his method, and therefore Mr. Newton's symbols of fluxions are the oldest in the kind. Mr. Leibnitz began to use the symbols of moments or differences *dx*, *dy*, *dz*, in the year 1677. Mr. Newton represented moments by the rectangles under the fluxions and the moment *o*, when he wrote his Analysis, which was at least 46 years since. Mr. Leibnitz has used the symbols *sx*, *sy*, *sz*, for the sums of ordinates ever since the year 1686; Mr. Newton represented the same thing in his Analysis, by inscribing the ordinate in a square or rectangle. All Mr. Newton's symbols are the oldest in their several kinds by many years.

And whereas it has been represented that the use of the letter *o* is vulgar, and destroys the advantages of the differential method; on the contrary, the method of fluxions, as used by Mr. Newton, has all the advantages of the differential and some others. It is more elegant, because in his calculus there is but one infinitely small quantity represented by a symbol, the symbol *o*. We have no ideas of infinitely small quantities, and therefore Mr. Newton introduced fluxions into his method, that it might proceed with finite quantities as much as possible. It is more natural and geometrical, because founded on the prime ratios of nascent quantities, which have a being in geometry, while indivisibles on which the differential method is founded, have no being, either in geometry or in nature. There are prime ratios of nascent quantities, but no prime nascent quantities. Nature generates quantities by continual flux or increase, and the ancient geometricians admitted such a generation of areas and solids, when they drew one line into another by local motion, to generate an area, and the area into a line by local motion, to generate a solid. But the summing up of indivisibles, to compose an area or solid, was never yet admitted into geometry. Mr. Newton's method is also of greater use and certainty, being adapted either to the ready finding out of a proposition, by such approximations as will create no error in the conclusion, or to the demonstrating it exactly; Mr. Leibnitz's is only for finding it out. When the work succeeds not in finite equations, Mr. Newton has recourse to converging series, and thereby his method becomes incomparably more universal than that of Mr. Leibnitz, which is confined to finite equations; for he has no share in the method of infinite series. Some years after the method of series was invented, Mr. Leibnitz invented a proposition for transmuting curvilinear figures into other curvilinear figures, of equal areas, in order to square them by converging series; but the methods of squaring those other figures by such series, were not his. By the help of the new

Analysis, Mr. Newton found out most of the propositions in his Principia Philosophiæ; but because the ancients, for making things certain, admitted nothing into geometry before it was demonstrated synthetically, he demonstrated the propositions synthetically, that the system of the heavens might be founded on good geometry. And this makes it now difficult for unskilful men to see the Analysis by which those propositions were found out.

It has been represented that Mr. Newton, in the scholium at the end of his book of quadratures, has put the third, fourth, and fifth terms of a converging series respectively, equal to the second, third, and fourth differences of the first term, and therefore did not then understand the method of second, third, and fourth differences. But, in the first proposition of that book, he showed how to find the first, second, third, and following fluxions in infinitum; and therefore when he wrote that book, which was before the year 1676, he did understand the method of all the fluxions, and consequently of all the differences. And if he did not understand it when he added that scholium to the end of the book, which was in the year 1704, it must have been because he had then forgot it. And so the question is only whether he had forgot the method of second and third differences before the year 1704.

In the 10th proposition of the 2d book of his Principia Philosophiæ, in describing some of the uses of the terms of a converging series, for solving problems, he tells us, that if the first term of the series represents the ordinate BC of any curve line ACG, fig. 4, and CBDI be a parallelogram infinitely narrow, whose side DI cuts the curve in G and its tangent CF in F, the second term of the series will represent the line IF, and the third term the line FG. Now the line FG is only half the second difference of the ordinate; and therefore Mr. Newton, when he wrote his Principia, put the third term of the series equal to half the second difference of the first term, and consequently had not then forgotten the method of second differences.

In writing that book, he had frequent occasion to consider the increase or decrease of the velocities with which quantities are generated, and he argues rightly about it. That increase or decrease is the second fluxion of the quantity; and therefore he had not then forgotten the method of second fluxions.

In the year 1692, Mr. Newton, at the request of Dr. Wallis, sent to him a copy of the first proposition of the book of quadratures, with examples of it in first, second, and third fluxions; as may be seen in the second volume of the Doctor's works, p. 391, 392, 393, and 396. And therefore he had not then forgotten the method of second fluxions.

Nor is it likely, that in the year 1704, when he added the aforesaid scholium to the end of the book of quadratures, he had forgotten not only the first proposition of that book, but also the last proposition, on which that scholium was written. If the word ut, which in that scholium may have been accidentally omitted between the words erit and ejus, be restored, that scholium will agree with the two propositions, and with the rest of his writings, and the objection will vanish.

Thus much concerning the nature and history of these methods, it will not be amiss to make some observations on them.

In the Commercium Epistolicum, mention is made of three tracts written by Mr. Leibnitz, after a copy of Mr. Newton's Principia Philosophiæ had been sent to Hanover for him, and after he had seen an account of that book published in the Acta Eruditorum for January and February 1689. And in those tracts the principal propositions of that book are composed in a new manner, and claimed by Mr. Leibnitz as if he had found them himself before the publishing of the said book. But Mr. Leibnitz cannot be a witness in his own cause. It lies upon him either to prove that he found them before Mr. Newton, or to quit his claim.

In the last of those three tracts, the 20th proposition (which is the chief of Mr. Newton's propositions) is made a corollary of the 19th proposition, and the 19th has an erroneous demonstration adapted to it. It lies upon him either to satisfy the world that the demonstration is not erroneous, or to acknowledge that he did not find that and the 20th proposition thereby, but tried to adapt a demonstration to Mr. Newton's proposition, to make it his own. For he represents in his 20th proposition, that he knew not how Mr. Newton came by it, and consequently that he found it himself, without the assistance of Mr. Newton.

By the errors in the 15th and 19th proposition of the third tract, Dr. Keill has showed that when Mr. Leibnitz wrote these three tracts, he did not well understand the ways of working in second differences. And this is further manifest by the 10th, 11th, and 12th propositions of this third tract. For these he lays down as the foundation of his infinitesimal analysis, in arguing about centrifugal forces, and proposes the first of them with relation to the centre of curvity of the orb, but uses this proposition in the two next, with relation to the centre of circulation. And by confounding these two centres with one another, in the fundamental propositions, on which he grounds this calculus, he erred in the superstructure, and for want of skill in second and third differences, was not able to extricate himself from the errors. And this is further confirmed by the 6th article of the second tract. For that article is erroneous, and the error arises from his not knowing how to argue well about second and third differences. When therefore he wrote those tracts he was but a learner, and this he ought in candour to acknowledge.

It seems therefore that he learned the differential method by means of Mr. Newton's aforesaid three letters, compared with Dr. Barrow's Method of Tangents; for 10 years after, when Mr. Newton's Principia Philosophiæ came abroad, he improved his knowledge in these matters, by trying to extend this method to the principal propositions in that book, and by this means composed the said three tracts. For the propositions contained in them, errors and trifles excepted, are Mr. Newton's, or easy corollaries from them, being published by him in other forms of words before. And yet Mr. Leibnitz published them as invented by himself long before they were published by Mr. Newton. For in the end of the first tract, he represents that he invented them all before Mr. Newton's Principia Philosophiæ came abroad, and some of them before he left Paris, that is before Oct. 1676. And the second tract he concludes with these words: "From what has been advanced, a great many things might be deduced, accommodated to practice, but it shall have now sufficed, that I have laid down geometrical principles, in which consisted the chief difficulty; and perhaps I may seem to an attentive considerer to have opened some new ways, that were pretty intricate before; for every thing answers to my Analysis of infinites, that is, to the calculus of sums and differences, some of whose elements I have given in the Acta Erudit. which I have expressed in as clear a manner as the thing would bear." He pretends here that the "geometrical foundations in which the chief difficulty consisted," were first laid by himself in this very tract, and that he himself had in this very tract opened "some new ways that were intricate before." And yet Mr. Newton's Principia Philosophiæ came abroad almost two years before, and gave occasion to the writing of this tract, and was written "in as plain a manner as the thing would bear," and contains all these principles and all these new ways. And Mr. Leibnitz, when he published that tract, knew all this, and therefore ought then to have acknowledged that Mr. Newton was the first who laid the "geometrical foundations in which the chief difficulty consists," and opened the "new ways that were intricate before." In his answer to Mr. Fatio, he acknowledged all this, saying, "which method, no geometrician that I know of, had before Mr. Newton and me; as none, before this celebrated geometrician, gave a public specimen that he had it." And what he then acknowledged he ought in candour and honour to acknowledge still upon all occasions.

Mr. Leibnitz, in his letter of May 28th, 1697, wrote thus to Dr. Wallis: "that Mr. Newton's method of fluxions had an affinity with my differential method, I not only perceived, after his book of Principia, and your book were published; but I also acknowledged as much in the Acta Erudit. and on other occasions; for, I judged that this became my own candour as well as his merit; therefore I usually call them both by the common name of Analysis infinitesimalis, which is more extensive than the Tetragonistica; for, as Vieta's and Descartes's methods are called Analysis speciosa, though here is some difference between them; so perhaps Mr. Newton's method and mine may differ in some things." Here also Mr. Leibnitz allows that when Mr. Newton's principles of philosophy came abroad, he understood thereby the affinity that there was between the methods, and therefore called them both by the common name of the infinitesimal method, and thought himself bound in candour to acknowledge this affinity; and there is still the same obligation upon him in point of candour. And besides this acknowledgment, he here gives the preference to Mr. Newton's method in antiquity. For he represents that as the common analysis in species was invented by Vieta and augmented by Cartes, which made some differences between their methods; so Mr. Newton's method and his own might differ in some things. And then he goes on to enumerate the differences by which he had improved Mr. Newton's method, as we mentioned above. And this subordination of his method to Mr. Newton's, which he then acknowledged to Dr. Wallis, he ought still to acknowledge.

In enumerating the differences and improvements which he had added to Mr. Newton's method, he names in the second place differential equations; but the letters which passed between them in the year 1676, show that Mr. Newton had such equations at that time, and that Mr. Leibnitz had them not. He names in the third place exponential equations; but these equations are owing to his correspondence with the English. Dr. Wallis, in the interpolation of series, considered fractional and negative indices of powers. Mr. Newton introduced into his analytical computations, the fractional, surd, negative, and indefinitive indices of powers; and in his letter of Oct. 24, 1676, represented to Mr. Leibnitz, that his method extended to the resolution of affected equations involving powers whose indices were fractional or surd. Mr. Leibnitz, in his answer dated June 21, 1677, mutually desired Mr. Newton to tell him what he thought of the resolution of equations involving powers whose indices were undetermined, such as were these $x^y + y^x = xy$, $x^x + y^y = x + y$. And these equations he now calls exponential, and represents to the world that he was the first inventor of them, and magnifies the invention as a great discovery. But he has not yet made a public acknowledgment of the light which Mr. Newton gave him into it, nor produced any one instance of the use that he has been able to make of it where the indices of powers are fluents. And since he has not yet rejected it with his usual impatience, for want of such an instance, we have reason to expect that he will at length explain its usefulness to the world.

Mr. Newton, in his letter of Oct. 24, 1676, wrote that he had two methods of resolving the inverse problems of tangents, and such like difficult ones; one of which consisted "in assuming a series for any unknown quantity, from which all the rest might conveniently be deduced, and in collating the homologous terms of the resulting equation, for determining the terms of the assumed series." Mr. Leibnitz many years after published this method as his own, claiming to himself the first invention of it. It remains that he either renounce his claim publicly, or prove that he invented it before Mr. Newton wrote his said letter.

It lies upon him also to make a public acknowledgment of his receipt of Mr. Oldenburg's letter of April 15, 1675, wherein several converging series for squaring of curves, and particularly that of Mr. James Gregory for finding the arc by the given tangent, and thereby squaring the circle, were communicated to him. He acknowledged it privately in his letter to Mr. Oldenburg, dated May 20, 1675, still extant in his own hand-writing, and by Mr. Oldenburg left entered in the letter-book of the Royal Society. But he has not yet acknowledged it publicly, as he ought to have done, when he published that series as his own.

It lies upon him also to make a public acknowledgment of his having received the extracts of Mr. James Gregory's letters, which, at his own request, were sent to him at Paris, in June 1676, by Mr. Oldenburg, to peruse: among which was Mr. James Gregory's letter of Feb. 15, 1671, concerning that series, and Mr. Newton's letter of December 10, 1672, concerning the method of fluxions.

And whereas in his letter of Dec. 28, 1675, he wrote to Mr. Oldenburg, that he had communicated that series above two years before to his friends at Paris, and had written to him sometimes about it; and in his letter of May 12, 1676, said to Mr. Oldenburg that he had written to him about that series some years before; and in his letter to Mr. Oldenburg, dated Aug. 27, 1676, that he had communicated that series to his friends above three years before; that is, on his first coming from London to Paris: he is desired to tell us how it came to pass, that when he received Mr. Oldenburg's letter of April 15, 1675, he did not know that series to be his own.

In his letters of July 15 and October 26, 1674, he tells us of but one series for the circumference of a circle, and says that the method which gave him this series, gave him also a series for any arc whose sine was given, though the proportion of the arc to the whole circumference be not known. This method therefore, by the given sine of 30 degrees, gave him a series for the whole circumference. If he had also a series for the whole circumference deduced from the tangeut of 45 degrees, he is desired to tell the world what method he had in those days, which could give him both those series. For the method by the transmutation of figures will not do it. He is desired also to tell us why in his said letters he did not mention more quadratures of the circle than one.

And if in the year 1674 he had the demonstration of a series for finding any arc whose sine is given, he is desired to tell the world what it was; and why in his letter of May 12, 1676, he desired Mr. Oldenburg to procure from Mr. Collins the demonstration of Mr. Newton's series for doing the same thing; and wherein his own series differed from Mr. Newton's. For on all these considerations there is a suspicion that Mr. Newton's series, for finding the arc whose sine is given, was communicated to him in England; and that in the year 1673 he began to communicate it as his own to some of his friends at Paris, and the next year wrote of it as his own in his letters to Mr. Oldenburg, in order to get the demonstration or method of finding such series. But the year following, when Mr. Oldenburg sent him this series, and the series of Mr. Gregory, and six other series, he dropped his pretence to this series for want of a demonstration, and took time to consider the series sent him, and to compare them with his own, as if his series were others different from those sent him. And when he had found a demonstration of Gregory's series by a transmutation of figures, he began to communicate it as his own to his friends at Paris, as he represents in the Acta Eruditorum for April 1691, p. 178, saying, "Now in 1675 I had by me a small tract I had composed, on the arithmetical quadrature, which from that time was perused by my friends, &c." But the letter, by which he had received this series from Mr. Oldenburg, he concealed from his friends, and pretended to Mr. Oldenburg that he had this series a year or two before the receipt of that letter. And the next year, on receiving two of Mr. Newton's series again by one George Mohr, he wrote to Mr. Oldenburg in such a manner as if he had never seen them before, and on pretence of their novelty, desired Mr. Oldenburg to procure from Mr. Collins Mr. Newton's method of finding them. If Mr. Leibnitz thinks fit to obviate this suspicion, he is in the first place to prove that he had Mr. Gregory's series before he received it from Mr. Oldenburg.

It lies upon him also to tell the world what was the method by which the several series of regression for the circle and hyperbola, sent to him by Mr. Newton, June 13, 1676, and claimed as his own by his letter of August 27 following, were found by him, before he received them from Mr. Newton.

And whereas Mr. Newton sent him, at his own request, a method of regression, which on the first reading he did not know to be his own, nor understood it; but as soon as he understood it, he claimed as his own, by pretending that he had found it long before, and had forgot it, as he perceived by his old papers: it lies upon him, in point of candour and justice, either to prove that he was the first inventor of this method, or to renounce his claim to it, for preventing future disputes.

Mr. Leibnitz, in his letter to Mr. Oldenburg dated Feb. 3, 1672-3, claimed a right to a certain property of a Series of numbers natural, triangular, pyramidal, triangulo-triangular, &c, and to make it his own, represented that he wondered that Monsieur Paschal, in his book entitled Triangulum Arithmeticum, should omit it. That book was published in the year 1665, and contains this property of the Series; and Mr. Leibnitz has not yet done him the justice to acknowledge that he did not omit it. It lies upon him therefore in candour and justice, to renounce his claim to this property, and to acknowledge Mr. Paschal the first inventor.

He is also to renounce all right to the differential method of Mouton, as second inventor: for second inventors have no right. The sole right is in the first inventor, until another finds out the same thing apart. In which case, to take away the right of the first inventor, and divide it between him and that other, would be an act of injustice.

In his letter to Dr. Sloane, dated Dec. 29, 1711, he has told us that his friends know how he came by the differential method. It lies upon him, in point of candour, openly and plainly, and without further hesitation, to satisfy the world how he came by it.

In the same letter he has told us that he had this method above 9 years before he published it, and it follows from thence that he had it in the year 1675, or before. And yet it is certain that he had it not when he wrote his letter to Mr. Oldenburg dated Aug. 27, 1676, wherein he affirmed that Problems of the inverse method of tangents, and many others, could not be reduced to infinite Series, nor to equations or quadratures. It lies upon him therefore, in point of candour, to tell us what he means by pretending to have found the method before he had found it.

We have showed that Mr. Leibnitz, in the end of the year 1676, in returning home from France through England and Holland, was meditating how to improve the method of Slusius for tangents, and extend it to all sorts of Problems, and for this end proposed the making of a general table of tangents: and therefore had not yet found out the true improvement. But about half a year after, when he was newly fallen upon the true improvement, he wrote back: "I agree with the celebrated Mr. Newton, the famous M. Slusius's method of tangents is not perfect. And I have now *long since* treated the subject of tangents in a more general manner, viz. by the differences of the ordinates." Which is as much as to say, that he had this improvement long before those days. It lies upon him, in point of candour, to make us understand that he pretended to this antiquity of his invention with some other design, than to rival and supplant Mr. Newton, and to make us believe that he had the differential method before Mr. Newton explained it to him by his letters of June 13 and Oct. 24, 1676, and before Mr. Oldenburg sent him a copy of Mr. Newton's letter of Dec. 10, 1672, concerning it.

The editors of the Acta Eruditorum in June 1696, in giving an account of the first two volumes of the mathematical works of Dr. Wallis, wrote thus, in the style of Mr. Leibnitz: "Besides, Mr. Newton himself, no less remarkable for his candour, than great merits in mathematics, acknowledged both in public and private, when (by means of Mr. Oldenburg, then secretary of the Royal Society of London) there was an epistolary correspondence between them, that is, upwards of 20 years before, that Mr. Leibnitz had his differential Calculus, and infinite Series, and also general methods for them; and this Dr. Wallis in the preface to his works, making mention of this correspondence, omitted, because perhaps he was not thoroughly acquainted with the matter. Besides, Mr. Leibnitz's method of differences, of which Dr. Wallis makes mention in the following words, viz. that none might allege, he had said nothing of the differential Calculus, disclosed speculations which did not equally arise from other principles." By the words here cited out of the preface to the first two volumes of Dr. Wallis's works, it appears that Mr. Leibnitz had seen that part of the preface, where Mr. Newton is said to have explained to him (in the year 1676) the method of fluxions found by him 10 years before, or above. Mr. Newton never allowed that Mr. Leibnitz had the differential method before the year 1677. And Mr. Leibnitz himself, in the Acta Eruditorum for April 1691, p. 178, acknowledged that he found it after he returned home from Paris to enter upon business, that is, after the year 1676. And as for his pretended general method of infinite Series, it is so far from being general, that it is of little or no use. I do not know that any other use has been made of it, than to colour over the pretence of Mr. Leibnitz to the Series of Mr. Gregory for squaring the circle.

Mr. Leibnitz, in his answer to Mr. Fatio, printed in the Acta Eruditorum for the year 1700, p. 203, wrote thus: "Mr. Newton himself alone best knows, and sufficiently declared to the public, when his Principia came out in 1687, that some new geometrical discoveries, which were common to us both, were not owing to any light we received from each other, but to the meditations of each apart, and that they were explained by me 10 years before, i. e. in 1677." In the book of Principles here referred to, Mr. Newton did not acknowledge that Mr. Leibnitz found this method without receiving light into it from Mr. Newton's letters abovementioned; and Dr. Wallis had lately told him the contrary, without being then confuted or contradicted. And if Mr. Leibnitz *had* found the method without the assistance of Mr. Newton, yet second inventors have no right.

Mr. Leibnitz in his aforesaid answer to Mr. Fatio, wrote further: "I can affirm, when in 1684, I published the elements of my Calculus, that I did not know any thing more of Mr. Newton's inventions in this kind, than what he formerly signified to me by his letters, viz. that he could find tangents without taking away surds; which Huygens afterwards also signified to me he could do, though I still know no more of that Calculus; but at length, on seeing Mr. Newton's book of Principia, I was fully satisfied that he had made much greater discoveries." Here he again acknowledged that the book of Principles gave him great light into Mr. Newton's method: and yet he now denies that this book contains any thing of that method in it. Here he pretended that before that book came abroad he knew nothing more of Mr. Newton's inventions of this kind, than that he had a certain method of tangents, and that by that book he received the first light into Mr. Newton's method of fluxions: but in his letter of June 21, 1677, he acknowledged that Mr. Newton's method extended also to quadratures of curvilinear figures, and was like his own. His words are to the following purpose: "I suppose what Mr. Newton would conceal, about the method of drawing tangents, differs not from this. And what confirms me in this opinion is, that he adds, that upon the same foundation quadratures may likewise be rendered more easy; for such figures are always quadrable, whose ordinate drawn into the difference of the absciss becomes the difference of any quantity."

Mr. Newton had in his three letters abovementioned (copies of which Mr. Leibnitz had received from Mr. Oldenburg) represented his method so general, as by the help of equations, finite and infinite, to determine maxima and minima, tangents, areas, solid contents, centres of gravity, lengths and curvities of curve lines and curvilinear figures; and this without taking away radicals; and to extend to the like Problems in curves usually called mechanical, and to inverse Problems of tangents, and others more difficult, and to almost all Problems, except perhaps some numeral ones like those of Diophantus. And Mr. Leibnitz, in his letter of Aug. 27, 1676, represented that he could not believe that Mr. Newton's method was so general. Mr. Newton in the first of his three letters set down his method of tangents deduced from this general method, and illustrated it with an example, and said that this method of tangents was but a branch or corollary of his general method, and that he took the method of tangents of Slusius to be of the same kind: and thereupon Mr. Leibnitz, in his return from Paris through England and Holland into Germany, was considering how to improve the method of tangents of Slusius, and extend it to all sorts of Problems, as we showed above out of his letters. And in his third letter Mr. Newton illustrated his method with Theorems for quadratures, and examples thereof. And when he had made so large an explanation of his method, that Mr. Leibnitz had got light into it, and had in his letter of June 21, 1677, explained how the method, which he was fallen into, answered to the description which Mr. Newton had given of his method, in drawing of tangents, giving the method of Slusius, proceeding without taking away fractions and surds, and facilitating quadratures; for him to tell the Germans that in the year 1684, when he first published his differential method, he knew nothing more of Mr. Newton's invention, than that he had a certain method of tangents, is very extraordinary, and wants an explanation.

At that time he explained nothing more concerning his own method, than how to draw tangents and determine maxima and minima, without taking away fractions or surds. He certainly knew that Mr. Newton's method would do all this, and therefore ought in candour to have acknowledged it. After he had thus far explained his own method, he added that what he had there laid down, were the principles of a much sublimer geometry, reaching to the most difficult and valuable problems, which were scarcely to be resolved without the differential calculus, aut simili, or another like it. What he meant by the words aut simili, was impossible for the Germans to understand without an interpreter. He ought to have done Mr. Newton justice in plain intelligible language, and told the Germans whose was the methodus similis, and of what extent and antiquity it was, according to the notices he had received from England; and to have acknowledged that his own method was not so ancient. This would have prevented disputes, and nothing less than this could fully deserve the name of candour and justice. But afterwards, in his answer to Mr. Fatio, to tell the Germans that in the year 1684, when he first published the elements of his calculus, he knew nothing of a methodus similis, nothing of any other method than for drawing tangents, was very strange, and wants an explanation.

It lies upon him also to satisfy the world why, in his answer to Dr. Wallis and Mr. Fatio, who had published that Mr. Newton was the oldest inventor of that method by many years, he did not put in his claim of being the oldest inventor thereof, but staid till the old mathematicians were dead, and then complained of the new mathematicians as novices; attacked Mr. Newton himself, and declined to contend with any body else, notwithstanding that Mr. Newton in his letter of Oct. 24, 1676, had told him, that for the sake of quiet, he had 5 years before that time laid aside his design of publishing what he had then written on this subject, and has ever since industriously avoided all disputes about philosophical and mathematical subjects, and all correspondence by letters about those matters, as tending to disputes; and for the same reason he has forborn to complain of Mr. Leibnitz, till it was showed him that he stood accused of plagiarism in the Acta Lipsiæ, and that what Mr. Keill had published, was only in his defence from the guilt of that crime.

Is has been said the Royal Society gave judgment against Mr. Leibnitz, without hearing both parties. But this is a mistake. They have not yet given judgment in the matter. Mr. Leibnitz indeed desired the Royal Society to condemn Mr. Keill, without hearing both parties; and by the same sort of justice they might have condemned Mr. Leibnitz without hearing both parties; for they have an equal authority over them both. And when Mr. Leibnitz declined to make good his charge against Mr. Keill, the Royal Society might in justice have censured him for not making it good. But they only appointed a committee to search out and examine such old letters and papers as were still extant about these matters, and report their opinion how the matter stood, according to those letters and papers. They were not appointed to examine Mr. Leibnitz or Mr. Keill, but only to report what they found in the ancient letters and papers: and he that compares their report therewith, will find it just. The committee was numerous and skilful, and composed of gentlemen of several nations, and the society are satisfied in their fidelity in examining the hands and other circumstances, and in printing what they found in the ancient letters and papers so examined, without adding, omitting or altering any thing in favour of either party. And the letters and papers are by order of the Royal Society preserved, that they may be consulted and compared with the commercium epistolicum, whenever it shall be desired by persons of note. And in the mean time I take the liberty to acquaint him, that by taxing the Royal Society with injustice in giving sentence against him, without hearing both parties, he has transgressed one of their statutes, which makes it expulsion to defame them.

The philosophy which Mr. Newton, in his Principles and Optics, has pursued, is experimental; and it is not the business of experimental philosophy to teach the causes of things, any further than they can be proved by experiments. We are not to fill this philosophy with opinions which cannot be proved by phænomena. In this Philosophy Hypotheses have no place, unless as conjectures or questions proposed to be examined by experiments. For this reason, Mr. Newton in his Optics distinguished those things which were made certain by experiments, from those things which remained uncertain, and which he therefore proposed in the end of his Optics in the form of queries. For this reason, in the preface to his Principles, when he had mentioned the motions of the planets, comets, moon and sea, as deduced in this book from gravity, he added: "I wish the other phænomena of nature could by the same way of reasoning be deduced from mechanical principles; for, several things induce me to believe, that all these things may depend upon certain forces, by which the particles of bodies are, by causes still unknown to us, either mutually impelled towards each other, and cohere together according to certain regular configurations, or mutually recede from each other; and for want of knowing these forces philosophers have hitherto attempted to no purpose to explain nature." And in the end of this book, in the 2d edition, he said that for want of a sufficient number of experiments, he forbore to describe the laws of the actions of the spirit or agent by which this attraction is performed. And for the same reason he is silent about the cause of gravity, there occurring no experiments or phænomena, by which he might prove what was the cause of it. And this he has abundantly declared in his Principles, near the beginning, in these words: "I do not inquire into the physical causes and seats of forces." And a little after; "I indifferently and promiscuously use for each other the words attraction, impulse, or any kind of propension towards the centre, by considering these forces not physically but mathematically. Whence I would caution the reader not to think, that by these words I define the species or manner of the action, or the physical cause or reason; or that I truly and physically ascribe forces to centres, which are only mathematical points, if I should happen to say, that either the centres attract, or that there are central forces." And at the end of his Optics; "Here I do not inquire by what efficient cause these qualities, viz. gravity, the magnetic and electrical forces are produced. What I call attraction, may possibly be produced by impulse, or in some other manner unknown to us. By attraction, I would here be understood to mean only in general, a certain kind of force, whereby bodies mutually tend towards each other, whatever cause that quality may be ascribed to. For we must first necessarily know by phænomena of nature, what bodies mutually attract each other, and what are the laws and properties of that attraction, before we can properly inquire by what efficient cause that attraction is produced." And a little after he mentions the same attractions as forces which by phænomena appear to have a being in nature, though their causes be not yet known; and distinguishes them from occult qualities, which are supposed to flow from the specific forms of things. And in the scholium at the end of his Principles, after he had mentioned the properties of gravity, he added: "But the reason of these properties of gravity I could not deduce from phænomena, and I do not devise hypotheses. For whatever is not deduced from phænomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, or of occult or mechanical qualities, have no place in experimental philosophy. It is sufficient that gravity really exists, and acts according to the laws I have explained, and that it solves all the motions of the celestial bodies and of our sea." And after all this, one would wonder that Mr. Newton should be reflected on, for not explaining the causes of gravity, and other attractions by hypotheses; as if it were a crime to content himself with certainties, and let uncertainties alone. And yet the editors of the Acta Eruditorum,* have told the world, that Mr. Newton denies that the cause of gravity is mechanical, and that if the spirit or agents by which electrical attraction is performed, be not the ether or subtile matter of Cartes, it is less valuable than an hypothesis, and perhaps may be the hylarchic principle of Dr. Henry Moor: and Mr. Leibnitz† has accused him of making gravity a natural or essential property of bodies, and an occult quality and miracle. And by this sort of raillery they are persuading the Germans that Mr. Newton wants judgment, and was not able to invent the infinitesimal method.

It must be allowed that these two gentlemen differ very much in philosophy. The one proceeds on the evidence arising from experiments and phænomena, and stops where such evidence is wanting; the other is taken up with hypotheses, and propounds them, not to be examined by experiments, but to be believed without examination. The one for want of experiments to decide the question, does not affirm whether the cause of gravity be mechanical or not mechanical: the other that it is a perpetual miracle if it be not mechanical. The one, by way of inquiry, attributes it to the power of the Creator that the least particles of matter are hard: the other attributes the hardness of matter to conspiring motions, and calls it a perpetual miracle if the cause of this hardness be other than mechanical. The one does not affirm that animal motion in man is purely mechanical: the other teaches that it is purely mechanical, the soul or mind (according to the hypothesis of an harmonia præstabilita) never acting on the body so as to alter or influence its motions. The one teaches that God (the God in whom we live and move and have our being) is omnipresent; but not a soul of the world: the other that he is not the soul of the world, but intelligentia supramundana, an intelligence above the bounds of

* Anno 1714, mense martio, p. 141, 142.—Orig.

† In tractatu de bonitate Dei et in Epistolis ad D. Harsoeker et alibi.—Orig.

the world; whence it seems to follow that he cannot do any thing within the bounds of the world, unless by an incredible miracle. The one teaches that philosophers are to argue from phænomena and experiments to the causes thereof, and thence to the causes of those causes, and so on till we come to the first cause: the other that all the actions of the first cause are miracles, and all the laws imprest on nature by the will of God, are perpetual miracles and occult qualities, and therefore not to be considered in philosophy. But must the constant and universal laws of nature, if derived from the power of God, or the action of a cause not yet known to us, be called miracles and occult qualities, that is to say, wonders and absurdities? Must all the arguments for a God taken from the phænomena of nature be exploded by new hard names? And must experimental philosophy be exploded as miraculous and absurd, because it asserts nothing more than can be proved by experiments, and we cannot yet prove by experiments that all the phænomena in nature can be solved by mere mechanical causes? Certainly these things deserve to be better considered.*

A new Star in the Neck of the Swan. By M. Gottfried Kirch. N° 343, p. 226. *Translated from the Latin.*

Though various changes happen among the fixed stars, as to their apparent magnitude, yet none was more surprising than that which Fabricius observed in the neck of the Whale, in 1569; though at first it was taken for a new star, which had never existed before, and after disappearing was to return no more; yet now it has been sufficiently confirmed by experience, that it still exists, and that it doubtless existed from the beginning of the world, in that very place where it now is. What is surprising in this star is, that it appears every year of a different magnitude, and commonly at certain times it cannot be distinctly seen by the naked eye; for which reason Hevelius called it Stella mira, or the wonderful star.

M. Kirch likewise observed another star, like this, in the neck of the Swan, but much smaller, and seen every year for a shorter space of time; whence it is not surprising that it was so long unknown; nay, it was lucky that it appeared at that time, and that it was seen in its greatest magnitude, when Bayer was observing and delineating the stars in the Swan, having represented it by χ, and reckoned it among the fixed stars of the 5th magnitude, that constantly appear: and at the same time he likewise found the above-mentioned star in the Whale's

* From the precise and correct language, from the highly important matter, and from the very strong and able manner of the foregoing composition, it seems to give evidence of its great author, Newton himself.

neck, to be of the 4th magnitude, and represented it by o, taking it for a fixed star that constantly appears.

The star next the head of the Swan, observed by Hevelius in 1670 and 1671, gave M. Kirch the opportunity of finding the mutable appearance of the star χ in the Swan's neck. For when he was in hopes that the same star would appear frequently, in the same manner as that other in the Whale's neck, which after its first disappearing had appeared again presently after to Hevelius, he sought for it on the first and 6th of July, 1686, O. S. both clear nights, but did not find it; but rather found that the star of the 5th magnitude in the Swan's neck, represented by Bayer by χ, was wanting. Yet on the 9th of October, O. S. he saw it very plainly with his naked eye. And because he thought that it would again disappear to the naked eye, he delineated some small stars round it, by means of a 2-foot telescope, that by comparing these with it, he might compute its magnitude, when it should decrease, as represented by fig. 5, plate 4.

He found also that that star had gradually decreased, till he could not see it through an 8-foot telescope; though he could still observe that other star in the Whale's neck through a 4-foot telescope. From that time he looked for that star for several nights together, but to no purpose; but at length he found it again on the 6th of August 1687, O. S. with an 8-foot telescope, but exceedingly small. And from that time he found it daily increase more and more; so that on the 23d of October, O. S. it appeared again for the first time to the naked eye, though still exceedingly small. On the 2d of November, O. S. it was very visible, and even after the 26th of November, O. S. though this last day it was decreasing again. Afterwards it could not be seen, but through a telescope, and at length it became so small, that he could not observe it with an 8-foot telescope. And thus M. Kirch found at this time, that from one disappearing to another, there was about a year, a month and a week. The following observations likewise showed that this star observed a pretty regular period in its appearing, though not of an equal magnitude at any particular time: nay it sometimes happened that it was quite invisible to the naked eye, though it could be seen through a telescope, and had come to its greatest magnitude; as in the latter end of 1688, and beginning of 1689. On the contrary, in 1690 this star became more visible, and considerably larger than the neighbouring star, which Bayer has placed without the Swan's neck, near χ, and represented by no letter; but M. Kirch marked it by ɔ. And after the latter had often observed the appearing and disappearing of this star, he found it to be very regular, and to perform its revolution in 404 days and a half.

This new star of M. Kirch's was observed at London about the 15th of July, 1715, O. S. when it appeared much brighter than the neighbouring star ℷ, and almost equal in magnitude to the middle star (called η by Bayer) in the Swan's neck. But in a month's time it could not be observed by the naked eye, and at length not through a telescope. According to the period, in which it is said to revolve, it should appear brightest in August, 1715.

Fig. 6 represents the Swan's neck, with the fixed stars next this new one, with two other new stars, which within the century were observed to emerge near it, of which that preceding the Swan's breast continues still visible, of the 5th magnitude; but that below the Swan's head, which had only been seen for 2 years, hitherto disappears.

Fig. 5 represents the telescopic stars next the new one.

Botanicum Hortense IV.—Giving an Account of some Rare Plants, observed A.D. 1714, *in several Gardens about London. By James Petiver, F.R.S.* N° 343, p. 229.

Observations on the Total Eclipse of the Sun, 22d April 1715, *made before the Royal Society. By Dr. Edmund Halley, R. S. S.* N° 343, p. 245.

Though it be certain from the principles of astronomy, that there necessarily happens a central eclipse of the sun in some part or other of the terraqueous globe, about 28 times in each period of 18 years; and that of these no less than 8 pass over the parallel of London, 3 of which 8 are total with continuance: yet, from the great variety of the elements of which the calculus of eclipses consist, it has so happened that since the 20th of March, 1140, I cannot find that there has been such a thing as a total eclipse of the sun seen at London, though in the mean time the shade of the moon has often passed over other parts of Great Britain.

The novelty of the thing being likely to excite a general curiosity, and having found, by comparing what had been formerly observed of solar eclipses, that the whole shadow would fall upon England, I thought it a very proper opportunity to get the dimensions of the shade ascertained by observation; and accordingly I caused a small map of England, describing the track and bounds of the shade, to be dispersed all over the kingdom, with a request to the curious to observe what they could about it, but more especially to note the time of continuance of total darkness, as requiring no other instrument than a pendulum clock, with which most persons are furnished, and as being determinable with the utmost exactness, by reason of the momentaneous occultation and emersion of the luminous edge of the sun, the least part of which makes day.

Nor has this advertisement failed of the desired effect; for the heavens having proved generally favourable, we have received from so many places so good accounts, that they fully answer all our expectations, and are sufficient to establish several of the elements of the calculus of eclipses, so as for the future we may more securely rely on our predictions: though it must be granted, that in this our astronomy has lost no credit.

The day of the eclipse approaching, I received the orders of the Society to provide for the observation to be made at their house in Crane-Court, and accordingly I procured a quadrant of near 30 inches radius, exceedingly well fixed with telescopic sights, and moved with screws so as to follow the sun with great nicety; as also a very good pendulum clock well adjusted to the mean time, and several telescopes to accommodate the more observers.

In order to examine both clock and quadrant, on the 20th of April, I observed the distance of the sun's upper limb from the zenith $36^{\circ}\ 16'$, and the next day $35^{\circ}\ 58'$; by which it appeared that the distances from the zenith taken by this quadrant ought to be increased by about one minute: and that allowance being made, by several observations taken before and after noon on the said 21st day, the clock was found to answer the apparent time or hour of the sun with sufficient exactness, as not going above $10''$ too fast. The next day, April 22, just before the eclipse began, we took three distances of the sun from the zenith, viz. at $7^{h}\ 42^{m}\ 52^{s}$ A. M. the correct distance of the sun's centre from the vertex was $62^{\circ}\ 1'\ 40''$; at $7^{h}\ 45^{m}\ 48^{s}$ it was $61^{\circ}\ 34'\ 40''$; and again at $7^{h}\ 48^{m}\ 55^{s}$ it was $61^{\circ}\ 6'\ 40''$: which with the given declination of the sun and latitude of the place, show the true times respectively to have been $7^{h}\ 42^{m}\ 38^{s}$, and $7^{h}\ 45^{m}\ 35^{s}$ and $7^{h}\ 48^{m}\ 39^{s}$; all concurring that the clock was only 14 seconds too fast, and had gained scarcely any thing sensible in a day's time: so that it might be entirely depended on during the continuance of the eclipse.

Having computed that the eclipse would begin at $8^{h}\ 7^{m}$, I attended soon after 8 with a very good 6-foot telescope, without stirring my eye from that part of the sun where the eclipse was to begin: and at $8^{h}\ 6^{m}\ 20^{s}$ by the clock, I began to perceive a small depression made in the sun's western limb, which immediately became more conspicuous; so that I concluded the just beginning not to have been above 5 seconds sooner; that is, exactly at $8^{h}\ 6^{m}$ correct time.

From this time the eclipse advanced, and by 9 o'clock it was about 10 digits, when the face and colour of the sky began to change from perfect serene azure blue, to a more dusky livid colour, having an eye of purple intermixed, and grew darker and darker till the total immersion of the sun, which happened at $9^{h}\ 9^{m}\ 17^{s}$ by the clock, or $9^{h}\ 9^{m}\ 3^{s}$ true time. This moment was determinable with great nicety, the sun's light being extinguished at once; and yet that of the emersion was more so, as the sun came out in an instant with so much lustre, that it surprised the beholders, and in a moment restored the day, viz. at $9^h\ 12^m\ 26^s$ true time, after he had been totally obscured for $3^m\ 23^s$ of time.

It was universally remarked, that when the last part of the sun remained on his east side, it grew very faint, and was easily supportable to the naked eye, even through the telescope, for above a minute of time before the total darkness; whereas on the contrary, my eye could not endure the splendour of the emerging beams in the telescope from the first moment. To this perhaps two causes concurred; the one, that the pupil of the eye did necessarily dilate itself during the darkness, which before had been much contracted by looking on the sun. The other, that the eastern parts of the moon, having been heated with a day near as long as 30 of ours, could not fail of having that part of its atmosphere replete with vapours raised by the so long continued action of the sun; and consequently it was more dense near the moon's surface, and more capable of obstructing the lustre of the sun's beams. Whereas at the same time the western edge of the moon had suffered as long a night, during which there might fall in dews all the vapours that were raised in the preceding long day; and for that reason, that part of its atmosphere might be seen much more pure and transparent. But from whatever cause it proceeded, the thing itself was very manifest, and was noted by every one.

About two minutes before the total immersion, the remaining part of the sun was reduced to a very fine horn, whose extremities seemed to lose their acuteness, and to become round like stars. And for the space of about a quarter of a minute, a small piece of the southern horn of the eclipse seemed to be cut off from the rest by a good interval, and appeared like an oblong star rounded at both ends, in this form  : which appearance could proceed from no other cause but the inequalities of the moon's surface, there being some elevated parts near her southern pole, by whose interposition part of that exceedingly fine filament of light was intercepted.

A few seconds before the sun was all hid, there appeared round the moon a luminous ring, about a digit, or perhaps a 10th part of her diameter in breadth. It was of a pale whiteness, or rather pearl colour, seeming a little tinged with the colours of the Iris, and concentric with the moon, whence I concluded it was the moon's atmosphere. But its great height far exceeding that of our earth's atmosphere; and the observations of some who found the breadth of the ring to increase on the west side of the moon as the emersion approached, together with the contrary sentiments of those whose judgment I shall always revere, makes me less confident, especially in a matter to which I must confess I gave not all the attention requisite. Whatever it was, this ring appeared much brighter and whiter near the body of the moon, than at a distance from it; and its outer circumference, which was ill defined, seemed terminated only by the extreme rarity of the matter it was composed of; and in all respects it resembled the appearance of an enlightened atmosphere viewed from far: but whether it belonged to the sun or moon, I shall not at present undertake to decide.

During the whole time of the total eclipse I kept my telescope constantly fixed on the moon, to observe what might occur in this uncommon appearance; and I found that there were perpetual flashes or coruscations of light, which seemed for a moment to dart out from behind the moon, on all sides, but more especially on the western side a little before the emersion: and about two or three seconds before it, on the same western side, where the sun was just coming out, a long and very narrow streak of a dusky, but strong red light, seemed to colour the dark edge of the moon; though nothing like it had been seen immediately after the emersion. But this instantly vanished on the first appearance of the sun, as did also the aforesaid luminous ring.

As to the degree of darkness, it was such that one might have expected to have seen many more stars than I find were seen at London: the three planets, Jupiter, Mercury and Venus were all that were seen by the gentlemen of the society from the top of their house, where they had a free horizon: and I do not hear that any one in town saw more than Capella and Aldebaran of the fixed stars. Nor was the light of the ring round the moon capable of effacing the lustre of the stars, for it was vastly inferior to that of the full moon, and so weak that I did not observe that it cast a shade. But the under parts of the hemisphere, especially in the south east, under the sun, had a crepuscular brightness: and all round us, so much of the segment of our atmosphere as was above the horizon, and was without the cone of the moon's shadow, was more or less enlightened by the sun's beams: and its reflection gave a diffused light, which made the air seem hazy, and hindered the appearance of the stars. And that this was the real cause of it, appears by the darkness being more perfect in those places near which the centre of the shade passed, where many more stars were seen, and in some not less than 20; though the light of the ring was to all alike.

During the time while the sun recovered his light, several altitudes were taken, to examine the regularity of the clock's motion; and though the sun now rose much slower than at the beginning, yet they all conspired, within a very few seconds, to show that the clock went still a quarter of a minute too fast. And the end of the eclipse approaching, I attended the moment of it with all the care I could, and concluded the complete separation of the sun and moon to be at $10^h\ 20^m\ 15^s$ by the clock, or exactly $10^h\ 20^m$ correct time.

What we have received from other places is as follows.

The Rev. Mr. James Pound, rector of Wansted in Essex, and R. S. S. gives the following account of the principal phænomena observed there; he being furnished with very curious instruments, and well skilled in the matter of observation, and having rectified his clock by several altitudes of the sun, taken both before and after, viz.

At 8^h	6^m	37^s	The eclipse first perceived.
9	9	28	The total immersion.
9	12	48	The emersion.
10	20	32	The just end of the eclipse.
0	3	20	The continuance of total darkness.

The near agreement of this observation with our own (the difference being only what is due to the difference of our meridians) makes us the less solicitous for what was noted at the Royal Observatory at Greenwich, from whence we can only learn that the duration of total darkness was $3^m\ 11^s$.

The Rev. Mr. W. Derham, rector of Upminster in Essex, and Reg. Soc. Sod. assisted by Samuel Molineux, Esq. secretary to his Royal Highness the Prince, and other persons of quality, made the following observations there, which he has lately communicated, viz.

At 8^h	7^m	41^s	The eclipse began.
9	10	58	Total darkness began suddenly, and Aldebaran appeared.
9	14	6	The emersion or end of total darkness.
0	3	8	Continuance of total darkness.
10	21	45	End of the eclipse, by a $13\frac{1}{2}$ foot glass.

Our professors of astronomy, in both universities, were not so fortunate: my worthy colleague Dr. John Keill, by reason of clouds, saw nothing distinctly at Oxford but the end, which he observed at $10^h\ 15^m\ 10^s$. As to the total darkness, he could only estimate it by the sudden change of the light of the sky; and reckoned its continuance to be but $3^m\ 30^s$; which was certainly too little, the centre of the shadow having doubtless passed very near Oxford. And the Rev. Mr. Roger Cotes, at Cambridge, had the misfortune to be oppressed by too much company, so that, though the heavens were very favourable, yet he missed both the time of the beginning of the eclipse and that of total darkness. But he observed the end of total darkness at $9^h\ 14^m\ 37^s$, and the exact end of the eclipse at $10^h\ 21^m\ 57^s$.

We have received several accounts from some places which lay near the track of the centre of the shade, and which might have been very proper to determine the greatest continuance of the darkness; as from Plymouth, Exeter, Weymouth, Daventry, Northampton, and Lynn Regis, all agreeing that the whole sun was obscured at those places full 4 minutes, and at some of them rather more. But these observers give us no account how they measured this time, and therefore it may well be supposed they took it in a round number, and perhaps from pocket minute watches. What I think may best be relied on for this purpose, are two corresponding observations made, the one at Barton near Kettering in Northamptonshire, where by the observation of John Bridges, Esq. treasurer of his majesty's revenue of excise, and R.S.S. with a good pendulum clock and all due care, the whole sun was hid no more than 3m. 53s. The other was by Mr. John Whiteside, A. M. keeper of the Ashmolean Museum at Oxford, and a skilful mathematician, who observed after the same manner, at King's Walden in Hertfordshire, near Hitchin, that the total eclipse continued but 3m. 52s. Hence it follows, that the centre of the shade passed near the middle between these two places, which are only 30 geographical miles asunder, and situated near at right angles to the way of the shade; and therefore that the total obscurity, where longest, could last only about 3m. 57s. or perhaps a second or two more at Lynn, and less at Plymouth, the velocity of the progress of the shade gradually decreasing, and its diameter increasing, as it passed on to the eastwards. And this situation of the middle line is confirmed by an observation made at the seat of Lord Foley, at Witley, 8 miles beyond Worcester, by his order, and communicated to the Royal Society; by which it appears that the total darkness lasted there 3m. 15s. Hence it follows that Witley was about 3 or 4 miles farther from the centre of the shade on the north side than London on the south; and Witley being, by Ogilby's Mensurations, 118 measured miles from London, it is plain that the centre passed over Islip, which is, by the same admeasurement, 57 such miles on that road, and about 5 miles almost due north from Oxford; so that the centre of the shade left Oxford but very little on the right hand, This situation agrees perfectly well with the former, between Barton and King's Walden; and, as far as the geography of our country may be relied on, I conclude the centre to have entered on England about Plymouth, and to have passed over Exeter, the Devizes, Islip, Buckingham, and Huntington, leaving Oxford and Bedford on the right, and Lynn on the left, and to have quitted the coast of Norfolk about Wells and Blakeney.

As for the limits of the shade, both on the north and south side, we have by inquiry obtained them with all the exactness the thing is capable of; and we should have been glad the French astronomers had done the like for the total eclipse that passed over Languedoc, Provence, and Dauphiny, on the 1st of May, 1706. But as this is the first eclipse of this kind that has been observed with the attention the dignity of the phenomenon requires, we hope those which may happen for the future to traverse Europe may not pass by so little regarded as hitherto.

As to the southern limit or term, where the eclipse ceased to be total on the South side of the sun, we have received an account of an observation made at Nortoncourt, about 10 miles on this side of Canterbury, by the Rev. Dr. John Harris, S. T. P. prebendary of Rochester, and R. S. S. assisted by that accurate observer Mr. Stephen Gray; by which we learn that the eclipse began there at $8^h\ 8^m\ 55^s$, and ended at $10^h\ 24^m\ 47^s$; and that the total darkness continued but about one minute, or rather less, the middle of it being at $9^h\ 13^m\ 52^s$. From this duration it will follow, that Nortoncourt was but about 3 or 4 miles within the shade. And that it was really so, is confirmed by the account of the inhabitants of Bocton, about midway between Nortoncourt and Canterbury, who assured Mr. Gray, as he was returning home that same day, that the eclipse was not total there, but, as one of them expressed it, before the sun had quite lost his light on the east side, he recovered it on the west; and that there was a small light left on the lower part of the sun that appeared like a star. And from Cranbrook in Kent, we are informed by William Tempest, Esq. R. S. S. that he observed there the sun to be extinguished only for a moment, and instantly to emerge again; so that the limit passed exactly over this town, which is about 38 geographical miles from London, and very near the right angle where the perpendicular from London falls on the line of the limit, being 3^m of time to the eastwards of London, in the latitude of $51^\circ\ 6'$, as near as I can gather.

How it passed over Sussex, we have not so authentic accounts, but we have learned that it was total at Wadhurst, beyond Tunbridge-wells, as also for some short time at Lewis; but that it was not so at Brightling, which place being situated on an eminence with a commanding prospect, all the country to the northward was seen in darkness, while they had there some benefit of a small remainder of the sun.

From these observations we may conclude, that this limit came upon the coast of England about the middle between Newhaven and Brighthelmstone in Sussex, and passing by Cranbrook and Bocton, left Canterbury about 4 miles on the right hand, and quitted the coast of Kent not far from Hern, toward the ancient Regulbium, now called Reculver. So that it seems scarcely one-third part of Kent, and not so much of Sussex, out of all the south coast of Great Britain, escaped being involved in this darkness.

The northern limit, having passed over a much greater space, has had more observers, and is not less curiously determined than the other. We find by the account given by the Rev. Mr. Roger Prosser, rector of Haverford-west, that the eclipse was total there a minute and a half; whence it follows, that Haverford was but about 6 miles within the shade; and therefore that it entered on Pembrokeshire about the middle of St. Bride's bay, leaving St. Davids and Cardigan on the left hand, and having traversed those two counties and Montgomeryshire it entered on Shropshire, leaving the town of Shrewsbury 1^m 40^s in the shadow, as was observed there by Dr. Hollings; whence it appears that Shrewsbury was about 8 miles within the limit. Thence it proceeded by the east side of Cheshire, leaving Whitchurch and Nantwich a very little without, and passing by Congleton, went over the Peak of Derbyshire into Yorkshire, and crossed the great northern road between Pontefract and Doncaster, somewhat nearer the former than the latter. For by the observations of Theophilus Shelton, Esq. at Darrington, about 2 miles on this side Pontefract, in lat. 53° 40′, and long. west from London 4^m 40^s of time, as may be concluded from Norwood's measure of a degree, the sun at 9^h 11^m was reduced almost to a point, which both in colour and size resembled the planet Mars; but while he watched for the total eclipse, that point grew larger, and the darkness diminished; whence he inferred, that the limit was very little more southerly. And since that, he has been informed that it was just total in Barnsdale, 3 miles south from thence. And that it was so at Badsworth about the same distance from Darrington, we are told by a letter of the reverend and learned Mr. Daubuz, that he has a certain account from that place, that the luminous ring round the moon was seen there, which was no where visible but while the eclipse was total. From these data we may securely determine the remainder of this track, and that the edge of the shadow, having passed over the rest of Yorkshire, went off to sea about Flamborough head.

So that of the 40 counties into which England is subdivided, only the 5 most northerly have not had the sun wholly hid from them; and 6 others have escaped only in part, viz. Shropshire, Cheshire, and Yorkshire, and the extreme part of Derbyshire on the north, and Kent and Sussex on the south; all the rest of the kingdom having more or less suffered an interval of total darkness.

I shall not at present consider this eclipse as universal, but only as it related to England; and it shall suffice to say, that the shadow came out of the Atlantic ocean, having passed over the Azores; and that its southern limit reached the isle of Ushant, and the northwest coasts of Britanny, between Brest and Morlaix; and dividing our islands of Guernsey and Jersey, just touched on the promontory of Normandy, called Cape la Hogue. And that after it had quitted England, and traversed the German ocean, it fell on Jutland on the south side, and Norway on the north; and thence proceeded to the eastwards over Sweden, Finland, &c.

It remains now to consider the figure, position, direction, velocity, and magnitude of the shadow, as it passed over us. And first, as to the figure, it is obvious that the shadow of the moon being a cone, and the earth's surface sufficiently spherical, the apparent shadow on the earth will be the common intersection of a cone and sphere, which is a figure hitherto little considered by geometers, and not being in plano, it is not to be exactly described, except in the spherical or conical surface. How to find the points of this curve in all cases, is taught by P. Coursier, in a very scarce Latin book, printed at Dijon in Burgundy, and published at Paris in the year 1663; nor do I hear of any other author that has handled the same subject since, though capable and worthy of further improvement.* By what he there delivers, prop. 11, 12, lib. 1, it will be easily understood, that the convexity of so small a part of the earth's surface as the shadow commonly occupies, can produce only an inconsiderable effect; so that without sensible error we may take it for a plane, and the section for a true Apollonian ellipsis, whose transverse axis, by reason of the smallness of the angle of the cone, will be to its conjugate, nearly as radius to the sine of the sun's altitude at its centre, especially if he be considerably elevated. But when he is near the horizon, it will be necessary to have regard to the true figure, by reason of the great length to which the transverse axe is extended, and particularly when the shade is entering on or leaving the earth's disk. Of these perhaps a fuller account may be given on a future occasion.

As to the position of the axis of the shadow, it is manifest that it must always lie in the plane of a great circle of the earth passing through the axis of the cone of the shade: and therefore it will be only requisite to obtain the azimuth and altitude of the sun, at the place where the centre of the shade at any time is found, to determine the situation of the axe and species of the ellipse required. Thus, the middle of the eclipse at London having been observed at $9^h\ 10^m\ 45^s$, by the given latitude and declination we find his azimuth about 59°, and altitude $40^{\circ}\ 46'$, that is just 40° high at the centre of the shadow. Therefore the trans-

* In some cases, the figure or curve, of the intersection of a cone and sphere, is a circle. See a dissertation on the nature and properties of such intersections, in Dr. Hutton's tracts, mathematical and philosophical, published in 1786, viz. props. 7, 8, 9, and their corollaries, p. 88. &c.

verse axe of the ellipse was to its conjugate, very nearly as radius to the sine of 40°, or as 1000 to 643 proximè; and made an angle of 59°, or very little more, with the meridian passing at that time through the centre of the shade.

Next as to the direction and the velocity of the motion with which the centre of the shade passed over England, it is to be observed that the shadow passes in a very compound curve, which in the former is not in plano, and only describable on the surface of the sphere; nor is its motion equable, but compounded of many elements, producing a great variety. By what method its points, and its tangents in those points, are to be obtained, I reserve to the next opportunity, this account being designed for the curious in general; only I must acquaint them, that for so small a part of the curve as went over England, it may be esteemed a right line, with more exactness than we usually find in most of our geographical charts. And the like may be said for the velocity, which, though in our present instance it was continually decreasing, may, for so short a time, be supposed to have been the same without sensible error.

By a careful calculation I have determined the velocity of the motion, at the time of the middle of the eclipse at London, to have been nearly 29 geographical miles in a minute of time; and that its way made an angle of 52° 45 with the meridian towards the east of the north; therefore the said way made an angle with the axis of the ellipsis of 68° 15′. And the greatest duration of total darkness having been $3^m\ 57^s$, as before shown, it will follow that that diameter of the elliptic figure, according to which the shade passed, was no less than $114\frac{1}{2}$ geographical miles. And from the elements of the conics it is easy to be proved, that supposing the figure of the shade a true ellipse, whose axes are as radius to the sine of 40°, the greater axis would be 171 geographical miles, and the lesser 110; and the nearest distance between the limits, supposed parallel, 164 such miles.

And this length of the axis of the shade, derived purely from the continuance of total darkness, is fully confirmed by the observed distance of the parallel limits; the one passing by Badsworth in Yorkshire, the other by Cranbrook in Kent. For by the two latitudes 53° 37′ and 51° 6′, with the difference of longitude $7^m\ 40^s$ of time, or 1° 55′, the distance of these two places is given $166\frac{1}{2}$ geographical miles: with the mean angle of position 25° from the north westwards; therefore this arch makes an angle with the track of the shade of $77\frac{3}{4}$°; and hence the nearest distance of the parallels becomes 163 such miles, which by the other way was found 164.

If therefore we conclude the axis of the shadow, when the sun was just 40° high, to have extended over 2° 50′ of a great circle, we may securely determine the difference of the sun and moon's diameters at this time. For the difference of the horizontal parallaxes of the sun and moon being found to be 60′ 38″, as shall be hereafter shown, but is not required with extreme exactness for this purpose, the difference of the parallaxes in altitude, at both ends of the axis, will be found to be 1′ 56″, and by so much did the diameter of the moon when 40° high exceed that of the sun; hence the horizontal diameter of the moon in this anomally is found 33′ 27″, which may serve for a rule in all other cases.

I forbear to particularise the chill and damp which attended the darkness of this eclipse, of which most spectators were sensible, and equally judges; as also the concern that appeared in all sorts of animals, birds, beasts, and fishes, on the extinction of the sun, which we ourselves could not behold without some sense of horror.

Lastly, I have added the following synopsis of such observations as have hitherto come to my hands.

Place	Observers	Beginning h. m. s.	Immersion h. m. s.	Emersion h. m. s.	Total m. s.	End h. m. s.
Barton	M. Bridges				3 53	
Bell-bar	M. Jones	8 6 25	9 9 45	9 13 27	3 42	
Broadway Carmarthenshire			8 47 0	8 49 30	2 30	10 21 57
Cambridge	M. Cotes			9 14 37		10 24 30
Canterbury	M. Gray	8 10 0				
Chester	M. Ward	7 57 40				10 6 35
Crew	M. Wright		9 2 8		2 0	10 9 0
Dublin	Lord Archbishop	7 42 11				9 49 40
Dublin	M. Hawkins	7 41 30				9 48 45
Exon	L. Bishop		8 55 0	8 59 0	4 0	10 0 0
Exon	M. Hudson	7 47 30			3 30	10 0 30
Greenwich	M. Flamsteed				3 11	
King's Walden	M. Whitside				3 52	
Llanidan Anglesey	M. Rowland	7 52 30				
London	Royal Society	8 6 0	9 9 3	9 12 26	3 23	10 20 0
Northampton	M. Hawkins		9 5 22	9 9 24	4 2	10 15 35
Nortoncourt	D. Harris	8 8 55	9 13 23	9 14 22	0 59	10 24 47
Oxon	D. Keill				3 30	10 15 10
Paris	Royal Academy	8 11 0				10 28 0
Plymouth	Mr. Heines	7 41 0	8 45 30	8 50 0	4 30	9 54 30
Portchester	C. Candler		9 2 25	9 6 15	3 50	
Salop	D. Hollings	7 58 0			1 40	10 6 0
Upminster	M. Derham	8 7 41	9 10 58	9 14 6	3 8	10 21 45
Wansted	M. Pound	8 6 37	9 9 28	9 12 48	3 20	10 20 32
Weymouth	M. Hobbs		8 53 0	8 58 0	4 0	
Witley	M. Baxter	7 59 0			3 15	10 13 0

An Account of a Book, viz. Bibliographiæ Anatomicæ Specimen, sive Catalogus omnium pene Auctorum, qui ab Hippocrate ad Harveium Rem Anatomicam ex professo vel obiter scriptis illustrarunt, &c. Curâ et Studio Jacobi Douglas, M. D. Reg. Soc. S. et in Colleg. Chirurg. Lond. Prælect. Anatom. 8vo. *Lond.* 1715. N° 343, p. 263.

The author of this treatise, whose skill in dissecting, as well as in the theory of the structure of the parts, leaves him few equals, in order to discover what progress anatomy has made, and with what industry it has been cultivated, has perused a great number of authors who have advanced the science; observing who were the first discoverers, and who have unjustly arrogated to themselves that title. In this decision he has impartially weighed their deserts, the better to lay before the reader the increase of these studies, and to determine more exactly the differences that have arisen about who are first inventors.

The history, lives, and eulogies ascribed to anatomists, which he has inserted, either from their own writings, or their editors, or commentators, will afford a great variety of pleasure, in which he has been particularly careful to set down the names, surnames, country, time of their birth, what year they died in, under what masters educated, where they flourished, and in what part of anatomy they excelled. Nor has he been less diligent in the account he has given of the books of anatomy, with which his friends supplied him in great numbers. The reader will see here laid before him all the several editions; with the places, date of the year, &c. together with an account of the plates whether originals or copies, cut in wood or engraven on copper, &c. To the whole are added three indexes.

An Account of some Barometrical Experiments, for finding the different Elasticities of the Air, made in several Parts of Switzerland. By Dr. John James Scheuchzer, M. D. Math. Professor at Tigurum, or Zurich, and F. R. S. N° 344, p. 266. *Translated from the Latin.*

The tube employed was 32 inches in length, and 2 lines in diameter, Paris measure. In the following tables, the first column shows the quantity of air left in the tube; the 2d the height of the mercury above the surface of the stagnant quicksilver; the 3d the spaces of the expanded air; and the 4th the descent of the mercury, on account of the air left in the tube.

At Zurich, Sept. 6, 1714, the height of the whole barometer, at 8 o'clock in the morning, was 26 Paris inches, 4 lines; but at $9\frac{1}{4}^{h}$ it was 26 inches, $4\frac{1}{4}$ lines.

Col. 1.	Col. 2.		Col. 3.		Col. 4.		Col. 1.	Col. 2.		Col. 3.		Col. 4.	
in.	in.	lin.	in.	lin.	in.	lin.	in.	in.	lin.	in.	lin.	in.	lin.
3	19	9 twice	12	6½ twice	6	7½	18	7	5½	24	8½	18	11
6	16	8	15	7½	9	8½		7	6	24	8	18	10½
	16	7½	15	8	9	9	21	5	3	27	0 twice	21	1½
9							24	3	3	28	11 twice	23	1½
12	11	11 twice	23	3 twice	14	5½	27	1	6	30	7½ twice	24	10½
15	9	9 twice	22	6 twice	16	7½	30	0	4	31	10½ twice	26	0

On Sept. 11, at 1^h afternoon, in a clear sky, on a ridge of Mount Liber, in the Alps, called Ennensewen gen Averen, in the jurisdiction of Glaris, the height of the whole barometer 23 inches 10 lines, twice.

Col. 1.	Col. 2.		Col. 3.		Col. 4.		Col. 1.	Col. 2.		Col. 3.		Col. 4.	
in.	in.	lin.	in.	lin.	in.	lin.	in.	in.	lin.	in.	lin.	in.	lin.
3	18	7	13	6	5	3	18	6	11	25	0	16	11
6	15	7½	16	4	8	2½	21	4	11	26	10	18	11
9	13	3	18	7	10	7	24	3	0	28	10	20	10
12	11	1½	20	9	12	8½	27	1	4	30	5	22	6
15	9	0	22	9	14	10	30	0	2	31	8	23	8

On Sept. 12, at 7^h in the morning, and a clear sky, on a high ridge of Mount Liber, called Aust Scherf, the height of the whole barometer 21 inches 8 lines.

Col. 1.	Col. 2.		Col. 3.		Col. 4.		Col. 1.	Col. 2.		Col. 3.		Col. 4.	
in.	in.	lin.	in.	lin.	in.	lin.	in.	in.	lin.	in.	lin.	in.	lin.
3	17	6	14	6	4	2	18	6	5	25	3	15	3
6	14	7	17	3	7	1	21	4	7	27	1	17	1
9	12	6	19	6	9	2	24	2	9½	29	0½	18	10½
12	10	5	21	6	11	3	27	1	4	30	6	20	4
15	8	5	23	6	13	3	30	0	2	31	8	21	6

Sept. 12, at 9^h in the morning, and a clear sky, on a high ridge of Mount Liber, called Aust dem Blattenstock, the height of the whole barometer was 21 inches 6 lines.

Col. 1.	Col. 2.		Col. 3.		Col. 4.		Col. 1.	Col. 2.		Col. 3.		Col. 4.	
in.	in.	lin.	in.	lin.	in.	lin.	in.	in.	lin	in.	lin.	in.	lin.
3	17	2½	14	6	4	3½	18	6	7	25	3	14	11
6	14	5	17	5	7	1	21	4	8	27	3	16	10
9	12	4	19	6	9	2	24	2	9	29	0	18	9
12	10	4½	21	5	11	1½	27	1	3	30	5	20	3
15	8	7	23	4½	12	11	30	0	3	31	6	21	3

Sept. 14, at 12^h noon, on a clear day, within the iron mine at Sarunatum, about 300 paces from its mouth, the height of the barometer was 24 inches 4 lines, and 24 inches 3 lines.

Col. 1.	Col. 2.		Col. 3.		Col. 4.		Col. 1.	Col. 2.		Col. 3.		Col. 4.	
in.	in.	lin.	in.	lin.	in.	lin.	in.	in.	lin.	in.	lin.	in.	lin.
3	18	9	13	1	5	7	18	7	0	24	10	17	4
6	15	9	16	1	8	7	21	4	11	27	0	19	5
9	13	5	18	5	10	11	24	3	0	28	10	21	4
12	11	3	20	7	13	1	27	1	4	30	6	23	0
15	9	1	22	9	15	3	30	0	3	31	6	24	1

Without this mine in the open air, I observed the same height of the mercury in the entire barometer, as also in 3 and 9 inches of air left in the tube; but it is to be observed, that the air in the inner parts of the mine, where I made these experiments, was rarefied by a fire, that was kindled there the day before, and the place had a moderate warmth like a stove.

Note.—It was found by several experiments made before the Royal Society of London, that the elastic forces of compressed air are directly as the compressing weights; and by Dr. Scheuchzer's observations it is evident, that the same ratio holds very nearly in rarefied air. For though there be found some difference, yet it is not so considerable, but that it may easily arise from the inequality of the diameter of the tube. That these experiments may be duly performed, the capacity of the tube must be divided into equal parts, by pouring into it an ounce of mercury only at a time, instead of dividing its length into equal parts.

Botanicum Hortense IV, &c. &c. By James Petiver, F. R. S. Continued from the last Transactions. N° 344, p. 269.

Observationes Cœlestes Britannicæ, Grenovici in Observatorio Regio habitæ, Anno 1713. *By Mr. Flamsteed.* N° 344, p. 285.

These celestial observations are of the planets Saturn, Jupiter, Mars, the Moon, and the satellites of Jupiter. But omitted here, as given more complete in the author's Historia Cœlestis.

Of an Experiment made by Dr. Brook Taylor assisted by Mr. Hawksbee, in order to discover the Law of the Magnetical Attraction. N° 344, p. 294.

By order of the Royal Society, Mr. Hawksbee and myself made an experiment with the great loadstone belonging to the Royal Society, in order to discover the law of the magnetical attraction; and not long after I gave an account of it to the society in a letter to Dr. Sloane, (who was then secretary) dated June 25, 1712. Since that, Mr. Hawksbee made another experiment of the same nature with a smaller loadstone; which he has given an account of in the Phil. Trans. N° 335. But on comparing the numbers of that experiment with those of the other, I find the numbers of the first experiment to be much more regular. I therefore conclude that to be the best experiment, and since no notice has been taken of the account I gave of it, and I have reason to believe Mr. Hawksbee lost the table I left with him for the society, of the numbers relating to it, I take this occasion to present the society with the following account of it.

We placed the great loadstone belonging to the Royal Society so, that its two poles lay in the plane of the horizon, and were in a line exactly at right angles with the natural direction of the needle we made use of, which was that Dr. Halley had made to observe the variations with. And by means of a carriage contrived for that purpose, the stone was easily moved to and fro, the poles continuing always in the same line. The needle was so placed, that the centre it played on was in the same line with the poles of the stone; the north pole being towards the needle. We measured the distances from the centre of the needle to the extremity of the stone; and found the variations of the needle from its natural position to be as in the following table.

Distant. Feet.	Variations.	Distant. Feet.	Variations.	Distant. Feet.	Variations.
1	81° 45′	4	16° 0′	7	3° 30′
2	58 00	5	9 20	8	2 20
3	30 00	6	5 35	9	1 35

On the Cause of the Saltness of the Ocean, and of the several Lakes that emit no Rivers; with a Proposal, by means thereof, to discover the Age of the World. By Edmund Halley, R. S. Sec. N° 344, p. 296.

There have been many attempts made, and proposals offered, to ascertain from the appearances of nature, what may have been the antiquity of this globe of earth; on which, by the evidence of sacred writ, mankind has dwelt about 6000 years; or according to the Septuagint above 7000. But as we are there told that the formation of man was the last act of the Creator, it is no where revealed in Scripture how long the earth had existed before this last Creation, nor how long those 5 days that preceded it may be to be accounted; since we are elsewhere told, that in respect of the Almighty a thousand years is as one day, being equally no part of eternity; nor can it well be conceived how those days should be to be understood of natural days, since they are mentioned as measures of time before the Creation of the sun, which was not till the 4th day. And it is certain that Adam found the earth, at his first production, fully replenished with all sorts of other animals. This inquiry seeming to me well to deserve consideration, and worthy the thoughts of the Royal Society, I shall take leave to propose an expedient for determining the age of the world by a medium, as I take it, wholly new, and which in my opinion seems to promise success, though the event cannot be judged of till after a long period of time; submitting the same to their better judgment. What suggested this notion was an observation I had made, that all the lakes in the world, properly so called, are found to be salt, some more some less than the ocean, which in the present case may also be esteemed a lake; since by that term I mean such standing waters as perpetually receive rivers running into them, and have no exit or evacuation.

The number of these lakes, in the known parts of the world, is exceedingly small, and indeed on inquiry I cannot be certain there are in all any more than 4 or 5, viz. 1st. The Caspian Sea; 2dly, The Mare Mortuum or Lacus Asphaltites; 3dly, The lake on which stands the city of Mexico; and 4thly, The lake of Titicaca in Peru, which by a channel of about 50 leagues communicates with a 5th and smaller, called the lake of Paria, neither of which have any other exit. Of these, the Caspian, which is by much the greatest, is reported to be somewhat less salt than the ocean. The Lacus Asphaltites is so exceedingly salt, that its waters seem fully sated, or scarcely capable to dissolve any more; whence in summer-time its banks are incrustated with great quantities of dry salt, of somewhat a more pungent nature than the marine, as having a relish of sal ammoniac; as I was informed by a curious gentleman who was on the place.

The lake of Mexico, properly speaking, is two lakes, divided by the causeways that leads to the city, which is built in islands in the midst of the lake, undoubtedly for its security; after the idea, probably which its first founders borrowed from their beavers, who build their houses on dams they make in the rivers after that manner. Now that part of the lake which is to the northward of the town and causeways, receives a river of a considerable magnitude, which being somewhat higher than the other, does with a small fall exonerate itself in the southern part, which is lower. Of these the lower is found to be salt, but to what degree I cannot yet learn; though the upper be almost fresh.

And the lake of Titicaca, being nearly 80 leagues in circumference, and receiving several considerable fresh rivers, has its waters, by the testimony of Herrera and Acosta, so brackish as not to be potable, though not fully so salt as that of the ocean; and the like they affirm of that of Paria, into which the lake of Titicaca does in part exonerate itself, and which I doubt not will be found much salter than it, if it were inquired into.

Now I conceive that as all these lakes receive rivers, and have no exit or discharge, so it will be necessary that their waters rise and cover the land, until such time as their surfaces are sufficiently extended, so as to exhale in vapour that water which is poured in by the rivers; and consequently that lakes must be larger or smaller, according to the quantity of the fresh they receive. But the vapours thus exhaled are perfectly fresh; so that the saline particles brought in by the rivers remain behind, while the fresh evaporates; and hence it is evident that the salt in the lakes will be continually augmented, and the water grow salter and salter. But in lakes that have an exit, as the lake of Genesaret, otherwise called that of Tiberias, and the upper lake of Mexico, and indeed in most others, the water being continually running off, is supplied by new fresh river water, in which the saline particles are so few as by no means to be perceived.

Now if this be the true reason of the saltness of these lakes, it is not improbable but that the Ocean itself is become salt from the same cause, and we are thereby furnished with an argument for estimating the duration of all things, from an observation of the increment of saltness in their waters. For if it be observed what quantity of salt is at present contained in a certain weight of the water, of the Caspian Sea, for example, taken at a certain place, in the driest weather; and after some centuries of years the same weight of water, taken in the same place, and under the same circumstances, be found to contain a sensibly greater quantity of salt than at the time of the first experiment, we may by the rule of proportion, make an estimate of the whole time wherein the water would acquire its present degree of saltness.

And this argument would be the more conclusive, if by a like experiment a similar increase in the saltness of the Ocean should be observed: for that, after the same manner as aforesaid, receives innumerable rivers, all which deposite their saline particles therein; and are again supplied, as I have elsewhere showed, by the vapours of the Ocean, which rise from it in atoms of pure water, without the least admixture of salt. But the rivers in their long passage over the earth imbibe some of its saline particles, though in so small a quantity as not to be perceived, unless in these their depositories after a long tract of time. And if, on repeating the experiment, after another equal number of ages, it shall be found that the saltness is further increased with the same increment as before, than what is now proposed as hypothetical, would appear little less than demonstrative. But since this argument can be of no use to ourselves, it requiring very great intervals of time to come to our conclusion, it were to be wished that the ancient Greek and Latin authors had delivered down to us the degree of the saltness of the sea, as it was about 2000 years ago: for then it cannot be doubted but that the difference between what is now found and what then was, would become very sensible. I recommend it therefore to the society, as opportunity shall offer, to procure the experiments to be made of the present degree of saltness of the Ocean, and of as many of these lakes as can be come at, that they may stand upon record for the benefit of future ages.

If it be objected that the water of the Ocean, and perhaps of some of these lakes, might at the first beginning of things, in some measure contain salt, so as to disturb the proportionality of the increase of saltness in them, I will not dispute it: but shall observe that such a supposition would by so much contract the age of the world, within the date to be derived from the foregoing argument, which is chiefly intended to refute the ancient notion, some have of late entertained, of the eternity of all things; though perhaps by it the world may be found much older than many have hitherto imagined.

Account of Books, viz. 1. *Linear Perspective, or a New Method of representing justly all Manner of Objects, &c. By Brook Taylor, LL. D. and R. S. Sec. 8vo. London,* 1715. N° 344, p. 300.

The author of this book, finding the art of perspective very imperfect in the books that have hitherto been published on that subject, thought it worth his while to consider the whole matter anew; and from a careful examination of the principles this art is founded on, he has endeavoured to establish some theorems, by means of which the practice of it might be rendered more general and easy. In order to this, at first sight he found it necessary to make use of new terms of art; the old ones seeming not to be expressive enough of what is meant by them, and being adapted to too confined an idea of the principles of this art. In the old perspective, the chief regard is had to the ground plane, that is, the plane of the horizon; from whence is derived the horizontal line, and by means of that line the representations of some figures are found by good simple constructions. But then the figures in all other planes are drawn by reducing them to the horizontal plane by means of perpendiculars, which is an inartificial round-about way, makes a great confusion of lines, and is not capable of so much exactness. This confined way of treating this subject, proceeds from the strong possession the mind is bred up in, of the notions of upwards and downwards, which makes one apt to refer all other irregular positions to those principal ones. But the minds of all artists should be drawn as much as can be from such confined ways of thinking, and they should be taught to accustom themselves, as much as may be, to consider nature in its general view, without minding those particular relations which things have with respect to themselves. For this reason our author has rejected the term of horizontal line, because it confines the mind too much to the particular consideration of the horizontal plane; but he considers all planes alike, and all figures as they are in themselves, without considering their relation to us; leaving the artist to do that, when he comes to apply the general rules of practice to any particular design.

This treatise is very short, because the author has confined himself only to give the general rules of practice, leaving the reader to himself, or to a master, to find out particular examples to exercise himself in. Yet he hopes he has omitted nothing that is material to the understanding of this art in its full extent. The whole book consists of 5 sections.

The first section contains an explanation of the fundamental principle of this art, with the definitions of the terms, and 4 theorems. The fundamental principle of this art is, that the representation of any point, is a point on the picture where it is cut by a line drawn from the original point really placed where it ought to seem to be. For these lines, which come from the several points of the original object to be placed in its proper situation, to the spectator's eye, are as so many visual rays which make the object sensible.

When a right line is continued in infinitum, the visual ray becomes at last parallel to it, and an object of any given size, if it goes still further and further off on that line, will at last seem to vanish; and at that time the place of its representation on the picture is the point where the ray parallel to the original line cuts the picture. For this reason our author has thought it proper to call that point the vanishing point of such an original line, and consequently of all others parallel to it (Def. 5.) And for the same reason, he calls that line on the picture a vanishing line (Def. 6) which is produced by the intersection of the picture with a plane passing through the spectator's eye parallel to an original plane. There are ten definitions in all, but these are the principal. And in our author's method, these vanishing points, and vanishing lines, are of great use for the representation of any line passing through its vanishing point. (Prop. 1.) Having found the representation of one point in any line, by any method whatever, he finds the representation of the whole line by its vanishing point, which he shows an easy way to find, in Prop. 6, 8, 12, which are in the 2d section. And by this means he solves several problems in perspective, which it is not possible to do by the common way, at least without a great deal of difficulty, and a great confusion of lines. And by this method he shows how the compleat representations of any proposed figures may be found, having given the representation only of some principal parts of them.

The 2d section contains several propositions to that purpose, showing how to find the vanishing points and lines of proposed lines and planes, according to the several circumstances proposed; and by the means of them, how to find the representation of any given figure. In the end of this section there are some examples, in the description of the regular solids and some other figures.

The 3d section shows how to find the representation of the shadows of all objects.

The 4th section shows how to find the representations of the reflexions of figures made by polished planes.

The 5th section contains a few propositions relating to the inverse method of perspective; or the manner of examining a picture already drawn; so as to find out what point the picture is to be seen from, or having that given, to find what the figures are which are described on the picture.

Our author has observed that there may be a very good expedient made use of in painting of large rooms and churches, which is drawn from the nature of those rays which produce the vanishing points. This not being mentioned in the book itself, he thinks it not improper to take notice of it here: the expedient is this, having some way or other found the representation of one point of a line that is wanted in the picture, to find the whole line, pass a thread stretched through the place of the spectator's eye, in a direction parallel to the direction the original line ought to be in, and the shadow of that thread cast by a candle, so as to pass through the given point on the picture will be the representation sought. The reason of this construction is, because the rays of light that pass from the candle to the thread so stretched, make the plane which generates the representation sought. (See Prop. 1.) And there may be other expedients of the like nature gathered from the same principle.

II. Ducatus Leodiensis; or, The Topography of the ancient Town and Parish of Leeds and Parts adjacent, in the County of York, &c. By Ralph Thoresby, Esq. F.R.S. London, fol. 1715. N° 344, p. 304.

Though the author does not professedly treat of any place but the ancient town and parish of Leeds, and the Regio Leodis, or adjoining territory called Elmet; yet not only the preface is more general, relating to the whole county, but there are many passages in the book itself, where he takes occasion to insert the pedigrees of such of the nobility and gentry, as have had any estates within the prescribed limits, though the chief seat of the family be distant; as esteeming all provinciales, who have but domicilium in provincia: to some of these he has premised several descents from ancient deeds yet remaining in the respective families: and to most of those that are inserted in the visitations in the College at Arms, London, he has added the dates from original deeds, registers, &c. and continued them to the present time. In the other parts, relating to the topography and etymology of the names of places, &c. he has been very particular, finding the name to be often a brief description of the place; and hath been thereby enabled to discover the vestigia of some considerable antiquities, in the actual survey that he made of those places to render the work more complete. But what relates more immediately to these Phil. Trans. is the annexed catalogue of the authors Musæum, justly celebrated for antiquities and for natural and artificial curiosities. The catalogue of the coins and medals is very copious and valuable.

The natural curiosities are ranked in the following method. 1. Human Rarities. 2. Quadrupeds, viviparous (multifidous and bifidous) and oviparous, with an account of certain balls and stones found in the stomachs of several animals. 3. Serpents. 4. Birds, land and water-fowls, with their eggs. 5. Fishes, viviparous and oviparous, scaled and exanguious. 6. Shells, whirled and single, double and multiple. 7. Insects, with naked and with sheathed wings, and creeping insects. 8. Plants, which begin with Dr. Nicolson's collection of above 800 dried plants; the rest are reduced to the accurate method of Dr. Sloane, in his Cat. Plant. in Insula Jamaicæ, proceeding from the corals and other submarines to the fruits and parts of trees. 9. Formed Stones, which are ranged according to Mr. Llwyd's curious tract, Lithophylac. Britan. only to the crystals and diamonds are premised the Margaritæ Cumbrenses, some of which have as good a water as the oriental. After the fossil shells and stones of the turbinated kind, the bivalves and shells amassed together into great stones by a petrified cement, follow the marbles and other irregular stones. 10. The metals, ores, salts and ambers, of which one with a fly, another with a spider inclosed.

The artificial curiosities relate to war, as Indian and Persian bows, arrows, darts, armour, shields, targets, tomahawks, poisoned daggers: to the mathematics, to household-stuff, habits, &c. from the remotest parts of the habitable world; not neglecting those that are obsolete of our own nation. Then follow statues, bass relieves, seals, impressions, copper-plates, heathen deities, amulets, charms and matters relating to Romish superstitions.

Some Accounts of the late great Solar Eclipse on April 22, 1715, *in the Morning. Communicated to the Royal Society from abroad.* N° 345, p. 314.

Since the publication of the late account given in Phil. Trans. N° 343, of what was observed in England, and particularly at London, of this eclipse, we have received from foreign parts the following observations; which seem not unworthy the acceptance of the curious. And first, Mr. John Edens, who has obliged us with the following most particular relation of the Peak of Teneriff, and of the ascent to it, being on his voyage to that island, observed the eclipse at sea, in latitude, by observation 34° 20′, and longitude 0^h 54^m west from London, as he concluded by their distance and position from the island Forte ventura, which they soon after fell in with. He writes that it began at $6^h\ 49^m$, and ended at $8^h\ 47^m$; this latter very exactly, though not quite so nice as to the beginning.

From Germany we have received the following accounts.

At Nurenburg, the beginning and greatest obscurity could not be seen for clouds, but the end happened at $11^h\ 10\frac{1}{5}^m$.

At Hamburg, the beginning was observed at $8^h\ 57^m$; the greatest obscurity at $10^h\ 5^m\ 30^s$, when $11\frac{1}{2}$ digits were darkened. The end could not be seen for clouds.

At Kiel in Holstein, the beginning $9^h\ 14^m$; the greatest obscurity $10^h\ 19^m\ 20^s$, and the quantity then eclipsed 11 digits 20′. The end was at $11^h\ 29^m$.

At Berlin, the beginning could not be seen for clouds; but the greatest obscurity was at 22 min. past 10, when 11 digits were eclipsed. The just end was at $11^h\ 34^m$.

At Franckfort on the Maine, the eclipse began at $8^h\ 50^m$; the greatest darkness at $10^h\ 11^m$, but perhaps should be 10^h 10 min. the digits being 10 and 34 min. The end was observed at 10 min. past eleven.

In a Dutch print, entitled Nouvelles Literaires, published at the Hague, there is an account of the observation of this eclipse at Upsal in Sweden, by M. Jo. Waller, professor of mathematics in that university, who was very careful to observe it exactly; the times being verified by three clocks perfectly agreeing with each other and with the sun: but more especially by a quadrant of 5 foot radius for taking the sun's altitude. By this instrument he has determined the height of the pole at Upsal 59° 51′ 54″. And by the same, a little before the beginning of the eclipse, he found the height of the sun 39° 36′ 42″, his clocks then showing the hour $9^h\ 47^m\ 50^s$, which proves that they were very near the true time. At $10^h\ 58^m\ 15^s$, the altitude of the sun being 44° 17′ 29″, was the beginning of the total darkness, and at $11^h\ 2^m\ 24^s$ was the end of it, alto sole 44° 29′ 13″: so that here the duration of the total eclipse was $4^m\ 9^s$, and the middle only one third of a minute after eleven. And lastly the end is said to have happened about 4 minutes before noon, the sun being 45° 42′ 6″ high: but in this is a manifest mistake, for it makes the time of emersion, or from the middle to the end, only $55^m\ 20^s$; whereas being so near the meridian, it is certain that this emersion was the greater part of the duration of the whole eclipse, and consequently more than an hour. Perhaps the times might be deduced from the altitudes only, and then the mistake might be in supposing the end so much before noon as it was really after it. However, to prevent all doubts, we have compared this observation with what we observed of this eclipse at London, and find that in the latitude of 59° 50′, the place where the middle of total darkness was at $11^h\ 0^m\ 20^s$, was near 19^0 more easterly than London (that is exactly in the meridian of Dantzic) and that the eclipse began there at $9^h\ 52\frac{1}{2}^m$, and ended at $12^h\ 10^m$. Therefore the duration could not be $2^h\ 7^m\ 50^s$, as the editor of the said Nouvelles has published; not considering that the beginning could not be seen for clouds, as in the very next words he assures us.

As to the darkness, it was such that they could scarcely distinguish each other: and besides Jupiter, Mercury and Venus; of the fixed stars, Cassiopeia, Capella, Oculus Tauri, and Orion, (Sirius not being yet risen) were visible.

An Account of a Journey from the Port of Oratava in the Island of Teneriff, to the top of the Peak in that Island, in August 1715; *with Observations. By Mr. J. Edens.* N° 345, p. 317.

On Tuesday, Aug. 13, N. S. at half an hour past 10 in the evening, I, in company with 4 more English and one Dutchman, with horses and servants to carry our provision, together with the usual guide, set forward from the port of Oratava. The night being somewhat cloudy, and the moon in the full at 12 the night following.

At half past 11 we came to the town of Oratava, which is about 2 miles from the port, where we stopped about half an hour, to get walking staves, to assist us in our ascending the steep of the peak. At one o'clock on Wednesday morning, we came to the foot of a very steep rising, about a mile and half above the town of Oratava, where it began to clear up; and we saw the peak with a white cloud covering the top of it like a cap.—At 2 o'clock we came to a plain place in the road, which the Spaniards call Dornajito en el Monte verde (the little trough in the green mountain) so called I suppose because a little below this plain, on the right hand as we went, there is a deep hollow; at the upper end of which hollow, there is a spout of wood placed in a rock, through which there runs very clear and cool water, which comes from the mountains; and at a descent a little lower than the spout there is a trough, into which the water comes.

At 3, after travelling a road, which was sometimes pretty smooth and at other times very rough, we came to a little wooden cross, by the road side on the left-hand, which the Spaniards call la Cruz de la Soltera, the cross of the Soltera. At this place we also saw the peak before us; and though we had come up hill quite from the port, yet it seemed almost as high here as when we were there, the white cloud still hiding the greatest part of the sugar-loaf.

After riding about half a mile further, we came to the side of a hill which was very rough and steep, (the place called Caravala;) where are a great many pine-trees that grow on both sides the road for a great way. Among these trees, not a great height in the air, we saw the sulphur discharge itself like a squib or serpent made of gunpowder, the fire running downwards in a stream, and the smoke ascending upwards from the place where it first took fire; and like this we saw another, while we lay under the rocks the next night at la Stancha, part of the way up the peak; but I could not observe whether either of them gave any report as they discharged.

At three quarters after 4 we came to the top of this high rough and steep mountain, where grows a tree which the Spaniards call el Pino de la Merenda. The pine-tree of the afternoon's meal. This is a large tree, and is burnt at the bottom, by having had fires made against it; and in the burnt place there issues out turpentine. At a few yards distance from this tree we had a fire made, where we staid and baited our horses, and breakfasted ourselves. These hills are very sandy, and a great many rabbits breed there; there is also much sand found a great way up the peak itself, and not a great way below the foot of the sugar-loaf. At three quarters after 5 we set forwards again, and at half an hour past 6 came to the Portillo, which in Spanish signifies a breach or gap. We saw the peak about 2 leagues and a half before us, covered still with a cloud at top; and the Spaniards told us we were come about 2 leagues and a half from the port.

At half past 7 we came to lus Faldas, that is the skirts of the peak; from whence all the way to la Stancha, which is about a quarter of a mile up from the foot of the peak, we rode over small light stones, about the size of one's fist; and a great many not much broader than a shilling: here if we kept the beaten track, it was not deep; but if we turned out of it, the horses went almost over their feet. I alighted and made a hole there, thinking to find how deep these little stones lie, but could not find the bottom; which makes me conclude they may cover the ground for a great thickness.

There are a great many vast rocks, some of them about 2 miles from the foot of the peak, which the peakman told us was cast out from the top of the peak at the time it was a volcano; many of them lie in heaps of above 60 yards long, and I observed that the farther these rocks lie from the foot of the peak, the more like they are to the stone of other common rocks: but the nearer we went to the peak, we found them more black and solid; and some of them, though not many, were glossy like flint, and all extremely heavy. Those that shone so, I suppose retained their natural colour, but there are some that look like dross that comes out of a smith's forge, which was doubtless occasioned by the extreme heat of the place they came from. Some of these large rocks were thrown out of the caldera or kettle in the top of the peak; and others from a cave or cistern, which is a pretty way up the side of the peak, and has by some been thought to have no bottom.

At 9 on Wednesday morning we arrived at la Stancha, about a quarter of a mile above the foot of the peak on the east side, where 3 or 4 large hard and solid black rocks are lodged: under some of these we put our horses, and under others we lay down ourselves to sleep, after having refreshed ourselves with a little wine: we had also a fire made to dress our dinner, where a cook we took along with us both roasted and boiled our meat and fowls very well. We slept here about 2 hours, then rose again, and at about 2 in the afternoon went to dinner.

There are several mountains that lie eastward from the peak, at 4 or 5 miles distance, called the Malpeses, and one lying a little more to the southward, called la montana de rejada: all which were formerly volcanoes, though not so great as that of the peak, as appears by the rocks and small burnt stones that lie near them, just in the same manner as about the peak.

At 9 at night, having supped, we retired to our former lodgings to sleep. Waking again about one, we arose, and by half past one we were all upon the march, and leaving our horses and some of our men behind, we went away fasting, excepting about two mouthfulls of wine each. Between la Stancha and the top of the peak there are two very high mountains besides the sugar-loaf, each of which mountains is almost half a mile's walking: on the first of them the rubbish is small, and we were apt to slip back as we stepped upwards. But the uppermost is all composed of hard loose rocky great stones, cast together in a very confused order. After resting several times, we came to the top of the first mountain, where we drank every one a little more wine, and ate a bit of gingerbread. Then, being pretty well refreshed, we set forwards again to ascend the second mountain, which is higher than the first, but is better to walk on, because of the firmness of the rocks. After we had travelled for about half an hour up the second mountain, we came within sight of the sugar-loaf, which before we could not see by reason of the interposition of these great hills. After we were arrived to the top of this second mountain, we came to a way that was almost level, but rather ascending; and about a furlong farther is the foot of the sugar-loaf, which we soon reached. Then looking upon our watches, we found it to be just 3 o'clock. The night was clear where we were, and the moon shone very bright; but below, over the sea, we could see the clouds, which looked like a valley at a prodigious depth below us. We had a brisk air, the wind being S. E. by S. as it was for the most part while we were on our journey.

While we sat at the foot of the sugar-loaf, resting and refreshing ourselves, as before in other places, we saw the smoke break out in several places, which at first looked like little clouds, but they soon vanished, others not long after succeeding them, from the same or other places. We set forwards to ascend the last and steepest part of our journey, viz. the sugar-loaf, exactly at half past 3, and after we had rested twice or thrice, we all arrived there by 4.

The shape of the top of the peak is partly oval, the longest diameter lying N. N. W. and S. S. E. and is, as near as I could guess, about 140 yards long; the breadth the other way being about 110. Within the top of the peak is a very deep hole, called the Caldera, or Kettle, the deepest part of which lies at the south end: it is I believe 40 yards deep, reckoning from the highest side of the peak : but it is much shallower reckoning from the side opposite to Gara chica. The sides of this kettle are very steep, in some places as steep as the descent on the outside of the sugar-loaf. We all went to the bottom of this kettle, where a great many very large stones lie, some of them higher than our heads. The earth that is within side the kettle, being rolled up long and put to a candle, will burn like brimstone. Several places within side the top of the peak are burning, as on the outside; and in some places on turning up the stones, is found very fine brimstone or sulphur sticking to them. At the holes where the smoke comes out, there also comes forth a great heat, so hot that one cannot endure one's hand there long. At the N. by E. side, within the top, is a cave, where we found a dead goat; in which cave sometimes the true spirit of sulphur distils, as they say, but it did not drop while I was there.

The report is false about the difficulty of breathing upon the top of this place; for we breathed as well as if we had been below: we ate our breakfast there, and I was there in all for about 2 hours and a quarter. Before the sun rose I think the air was as cold as I have known it in England, in the sharpest frost I was ever in; I could scarcely endure my gloves off. There was a great dew all the while we were there till sun rising, which we could find by the wet ness of our clothes; but the sky looked there as clear as possible. A little after sun rising we saw the shadow of the peak on the sea, reaching over the island of Gomera; and the shadow of the upper part, viz. of the sugar-loaf, we saw imprinted like another peak in the sky itself, which looked very surprising: but the air being cloudy below us, we saw none of the other islands, except Grand Canaria and Gomera.

At 6 on Thursday morning we came down from the top of the sugar-loaf; at 7 we came to the cistern of water which is reported to be without bottom: this the guide says is false, for about 7 or 8 years ago, when there was a great volcano in this country, the cave was dry and he walked all about it, and said that the deepest part of water, when we were there, was not above 2 fathoms. The dimensions of this cave I guess to be as follows:—Length about 35 yards, breadth 12, ordinary depth 14 from top to bottom. On the further side grows some white stuff, which the peakman told us was saltpetre. There was both ice and snow in it when we were there: and the ice was of a great thickness, covered with water about knee deep. We let down a bottle at the end of a string for some of the water, in which we put some sugar and drank it, but it was the coldest I ever drank in my life. The ice was broken just under the mouth of it, where we could see the stones lie at the bottom, for it was very clear. A little to the right-hand within this cave the ice was risen up in a high heap, in form of a spire steeple, or like a sugar-loaf; and in this place I believe the water comes in.

In our way home, we came by a cave 3 or 4 miles from the peak, where are a great many skeletons and bones of men; and some say there are the bones of giants in this cave. We came home to the port at about 6 o'clock this evening, being Thursday, August 15, 1715, N. S.

An extraordinary Dilatation or Enlargement of the left Ventricle of the Heart. By Jas. Douglass, M. D. and R. S. S. N° 345, p. 326.

I lately saw opened a young man in St. Bartholomew's Hospital, who died of the palpitation of the heart, the violent beating and prodigious subsultory motion of which, for some months before his death, was not only easily felt by laying the hand on the region of the heart; but seen to rise and fall by raising the bedcloaths that covered it. And, which is almost incredible, sometimes the trembling and throbbing made such a noise in his breast, as plainly could be heard at some distance from his bed-side. This was accompanied with frequent deliquiums, sometimes slow, sometimes swift, and often intermitting.

Johannes Fernelius in his Pathologia, lib. 5, cap. 12, gives an observation of a very uncommon and surprising case of this kind; where he says the frequent concussion of the heart was so violent and powerful, as not only to displace or luxate, but even to break some of the adjoining ribs. Franciscus de la Boe Sylvius, another writer of unquestionable integrity, has a parallel observation in his account of this disease. Theodorus Kerkringius relates the history of a woman he opened, whose heart was of a prodigious size, in his Spicileg. Anatom. obs. 16. And, to mention no more, Monsieur Dionis, at the end of his anatomy, gives a large description of a very uncommon case, in which the right auricle of the heart was prodigiously dilated, to the size of the head of a new born child.

In the dissection of this morbid heart, I observed the following remarkable particulars: 1. That the pericardium, or capsula cordis, was very thick, and firmly adhered, or grew by a fibrous connection, to all the outer surface of the heart. 2. Instead of the water called liquor pericardii, there was only in some places about the basis of the heart, a mucilaginous clear substance like a gelly. 3. In the right auricle, laid open, there was nothing preternatural. The ascending and descending cava opened into the same as usual. The vestigium, or mark of the foramen ovale, with its semicircular limbus, was very plain. And the orificium of the vena cordis coronaria was extremely large, yet its valve was less than usual. 4. In the right ventricle, laid open, the valvulæ, called tricuspides, were configurated after the usual manner. The sides of this cavity were thin and full of small fleshy columnæ, as they commonly are, with great variety of furrows and little holes. The three sigmoid or semilunar valves in the mouth of the arteria pulmonalis, were as they always are in a natural state. 5. The left auricle was not much larger than ordinary: but its muscular appendage, called the bulb of the pulmonary vein by the late Mr. Cowper, was extraordinarily dilated and enlarged, beyond any thing that I ever saw. 6. The left ventricle, whose capacity in a natural state is always less than the right, was here considerably larger. And if the experiment had been made, before dissection, of filling both with any liquor, this had certainly contained 3 times more than the other. 7. The valvulæ called mitrales, placed at the orifice of this ventricle, were much thicker in substance than ordinary: and the two fleshy columns, called by Nicolaus Massa, almost 200 years ago, duo parvi musculi, which send out abundance of small tendons to be inserted into these valves, were proportionably augmented in size. 8. The semilunar valves in the mouth of the aorta, or of that great vena pulsatilis that dispenses the blood to all the several parts of the human body, were very much preternaturally affected; as would easily appear on comparing them with those in the orifice of the pulmonary artery, in which they are thin and very broad, so as to be able to shut the cavity of that vessel, and hinder the blood from returning back into the ventricle, and likewise transparent: but in this they are very thick, contracted as it were, and furled together, and of a whitish colour; and in all appearance, if the person had lived longer, they had turned bony, or undergone a petrification.

This uncommon structure of the heart being thus demonstrated, let us endeavour to account for the following phænomena. The first is the palpitation of the heart, which was the chief symptom and complaint of the sick person. The second is the preternatural dilatation and enlargement of the left ventricle. It is not improbable but the firm adhesion of the capsula cordis membranosa to the substance of the heart, occasioned that uncommon trembling and throbbing: its free and easy motion being hindered by that thick involucrum which surrounded it so close on each side. The learned Dr. Lower, in his elaborate treatise de Corde humano, gives such an instance, and explains the palpitation after this manner.

As for the second, viz. the dilatation of the left ventricle and muscular bag of the pulmonary vein; that is altogether owing to the bad configuration of the valves we have now described: for as the great artery, or aorta, arises out of this ventricle, it has three valves, which separating give passage to the blood from the ventricle into the vessel; and in a natural state they shut that passage, and so prevent the blood from recoiling into the same, if it should endeavour to return. But in this case, by reason of its contracted narrowness and thickness, not being able to close or shut the passage, the blood flowed back again into the cavity, which it had gradually enlarged, and dilated to the size we see. Besides the muscular valves not being duly qualified for the performance of their office, the blood recoiled into the auricle, which it had distended in the like manner. This constant regurgitation, or reflux of the blood, is besides sufficient of itself to produce this extraordinary trembling, or παλμός καρδίας, as the Greeks call it.

A ready Description and Quadrature of a Curve of the Third Order, resembling that commonly called the Foliate. By Mr. Abr. de Moivre, F. R. S. N° 345, p. 329.

I have looked a little farther into that curve which fell lately under my consideration. It is not the foliate as I first imagined, but I believe it ought not to make a species distinct from it. AEB (fig. 7, pl. 4) is the curve I thus describe. Let AB and BK be perpendicular to each other. From the point A draw AR cutting BK in R, and make RE = BR, the point E belongs to the curve. Draw BC making an angle of 45° with AB; this line BC touches the curve in B. From the point E draw ED perpendicular to BC, and calling BD, x; DE, y; AB, a; and making $\sqrt{8aa} = n$, the equation belonging to that curve is $x^3 + xxy + xyy + y^3 = nxy$, or $\frac{x^4 - y^4}{x - y} = nxy$. Taking BG = AB, and drawing GP perpendicular to BG, PG is an asymptote. In the foliate, the equation is $x^3 + y^3 = \frac{1}{2}nxy$; in which the two terms $xxy + xyy$ of the former equation are wanting; and its asymptote is distant from B by $\frac{1}{3}$BA. Again, draw EF perpendicular to AB: let BF be called z, and FE, v; the equation belonging to the curve AEB is $vv =$

$\frac{azz - z^3}{a + z}$. In the foliate the equation is $vv = \frac{azz - z^3}{a + 3z}$. From these last two equations it seems that these curves differ no more from each other, than the circle from the ellipsis.

The quadrature of the curve here described has something of simplicity with which I was well pleased. With the radius BA, and centre B, describe a circle AKG, let the square HPST circumscribe it, so that HP be parallel to AG: prolong FE till it meet the circumference of the circle in M, and through M draw LMQ parallel to HP. The area BFE is equal to the area KHLM, comprehended by KH, HL, LM and the arc KM. And the area Bfe is equal to the area KmLH or KMPQ. Therefore if BF and Bf are equal, the two areas BFE, Bfe taken together are equal to the rectangle HQ, and therefore the whole space comprehended by BEAXBEYGZ (supposing Y and Z to be at an infinite distance) is equal to the circumscribed square HS.

N. B. This quadrature is easily demonstrated from the equation: for by it, $a + z : a - z :: zz : vv$, that is AF : EF :: MF : FB; and so φF the fluxion of AF to Ll the fluxion of MF. Hence the areola EFφe will be always equal to the areola ML$l\mu$, and therefore the area AEF always equal to the area MAL.

Hence it appears that this curve requires the quadrature of the circle to square it; whereas the foliate is exactly quadrable, the whole leaf of it being only one third of the square of AB, which in this is above three sevenths of the same. Again, in our curve, the greatest breadth, is when the point F divides the line AB in extreme and mean proportion: whereas in the foliate, it is when AB is triple in power to BF. And the greatest EF or ordinate in the foliate, is to that of our curve, nearly as 3 to 4, or exactly as $\sqrt{\frac{2}{3}\sqrt{\frac{1}{3}} - \frac{1}{3}}$ to $\sqrt{5\sqrt{\frac{5}{4}} - 5\frac{1}{2}}$.

But still these differences are not enough to make them two distinct species, being both defined by a like equation, if the asymptote SGP be taken for the diameter. And they are both comprehended under the 40th kind of the curves of the third order, as they stand enumerated by Sir Isaac Newton, in his incomparable treatise on that subject.

An easy Mechanical Way to divide the Nautical Meridian Line in Mercator's Projection; with an Account of the Relation of the same Meridian Line to the Curva Catenaria. By J. Perks, M. A. N° 345, p. 331.

The most useful projection of the spherical surface of the earth and sea for navigation, is that commonly called Mercator's; though its true nature and construction is said to be first demonstrated by our countryman Mr. Wright, in his Correction of the Errors in Navigation. In this projection the meridians are all parallel lines, not divided equally, as in the common plain chart, which is therefore erroneous, but the minutes and degrees, or strictly the fluxions of the meridian, at every several latitude, are proportional to their respective secants. Or a degree in the projected meridian, at any latitude, is to a degree of longitude in the equator, as the secant of the same latitude is to radius.

The reason of which enlargement of the elements of latitude is, to counterbalance the enlargement of the degrees of longitude. For in this projection the meridians being all parallel, a degree of longitude at, suppose, 60° latitude, is become equal to a degree in the equator, whereas it really is, on the globe's surface, only half as much, the radius of the parallel of 60°, that is its cosine, being but half the radius of the equator. Therefore to proportion the degrees of latitude to those of longitude, a degree, or elemental particle, in the meridian, is to be as much greater than a degree, or like particle, in the equator, as the radius of the equator is greater than the radius of the parallel of latitude, viz. its cosine.

In fig. 8, pl. 4, let the radius CD represent half the equator, DM an arc of the meridian; MS its sine, CE its secant; then is CS equal to its cosine; and CS : CM :: CD (= CM) : CE, that is, as cosine : to radius :: so is radius : to secant. The cosines being then, in this projection, supposed all equal to radius, or, which comes to the same, the parallels of latitude being all made equal to the equator, the radius of the globe, at every point of latitude, by the precedent analogy, is supposed equal to the secant of latitude; and consequently the elements, minutes, &c. of the meridian must be proportional to their respective secants.

The way Mr. Wright takes for making his table of meridional parts, is by a continual addition of natural secants, beginning at 1 minute, and so proceeding to 89 deg. Dr. Wallis, in Philos. Trans. N° 176, finds the meridional part belonging to any latitude by this series, putting s for its natural sine, viz. $s + \frac{1}{3}s^3 + \frac{1}{5}s^5 + \frac{1}{7}s^7 + \frac{1}{9}s^9$ &c. which gives the meridional part required. How to find the same mechanically by means of an easily constructed curve line, is what I shall now show.

1. Prepare a ruler AB, fig. 9, of a convenient length, in which let BO be equal to the radius of the intended projection. To the point O as a centre, on the narrower edge of the ruler, fasten a little plate-wheel WH tight to the ruler, and of a diameter a little more than the thickness of the ruler. Let KR, fig. 8, represent another long ruler, to which AR is a perpendicular line. Place the ruler AB on the line AR, with the centre of the wheel at A. Then with one hand holding fast the ruler KR, with the other hand slide the end B of the ruler

AB by the edge of KR; so will the little wheel wh describe on the paper a curve line ACB, to be continued as far as is convenient.

2. Having drawn the curve ACB, draw a straight line KR by the edge of the ruler KR, which line is the meridian to be divided, and also an asymptote to the curve ACB.

3. In this meridian, accounting R to be the point of its intersection with the equator, the point answering to any degree of latitude is thus found. In the perpendicular AR, make RG equal to the cosine of latitude, radius being AR, and from G draw GC parallel to KR, and intersecting the curve in C. With centre C and radius CM = AR, strike an arc cutting the meridian at M, so is M the point desired.

4. In the curve AC, let c be a point infinitely near to C, and cm, (= CM) a tangent to the curve at c, making the little angle Mcm, to which let the angle RAr be equal: so is Rr = Md a perpendicular from M to cm. Draw CD equal and parallel to AR, intersecting KR in S. With centre C and radius CD draw the arc DM, and its tangent DE and secant CE.

5. Because of the like triangles CDE, Mdm; CD : CE :: Md : Mm, that is, as radius to secant of the arc DM, whose cosine is CS = GR, :: so is Md, = Rr a degree or particle of the equator : to Mm the fluxion or correspondent particle of the meridian line RM. Whence, and from what is premised concerning the nature of this nautical projection, it is evident that RM is the meridional part answering to the latitude whose cosine is GR. Or thus, with centre R and radius AR describe the quadrant A$\varkappa\alpha$, in which let the arc A$\varkappa$ be equal to the given latitude. From $\varkappa$ draw $\varkappa$C parallel to KR, and intersecting the curve in C, so is C$\varkappa$ the meridional part desired, being equal to RM, as is easy to show.

As to the other properties of this curve, it is evident, from its construction, that its tangent, as CM, is a constant line, every where equal to AR; the curve being generated by the motion of the wheel at the end of the ruler, which is its tangent. And from hence the curve ACB may, for distinction, be called the equitangential curve.

7. The fluxion of the area ARMC is the little sector or triangle Mcd, which is also the fluxion of the sector CDM: whence the areas ARMC, CDM, are equal, and the whole area ACB &c, KMR being infinitely continued, is equal to the quadrant ARα.

8. To find the radius of curvature of any particle, as Cc, from C draw an indefinite line CT perpendicular to CM, on the concave side of the curve, and from c another line perpendicular to cm, which lines, because of the inclination of cm to CM, will somewhere meet, as at T, making an angle CTc = Mcm.

These angles being equal, their radii are proportional to their arcs; therefore MD : CC :: MC : CT. But CC = dm (because of CM = cm) so that MD : dm (:: CD : DE) :: CM : CT. But CD = CM; therefore CT = DE = tangent of the arc DM.

9. So that supposing ATt a curve line, in which are all the centres of curvature of the particles of ACB, any point as T being found as before, the length AT, by the nature of evolution of curves, is every where equal to the tangent of its correspondent circular arc DM. The point T is also found by making MT perpendicular to RM, and equal to the secant CE; for so is the angle CMT = MCD, and the triangle MCT equal to the triangle CDE.

10. Let AHh be an equilateral hyperbola, whose semi-axis is AR and centre R. In the meridian let RP be equal to the tangent DE. Join AP, and draw PH = AP and parallel to AR. Complete the parallelogram HNRP; so will the point H be in the hyperbola, and its ordinate HN (= RP = DE = CT) be equal to the curve ATt. From whence, and from prop. 3, corol. 2, of Dr. Gregory's Catenaria, Phil. Trans. N° 231, it appears that the curve ATt is that called the catanaria or funicularia, viz. the curve into whose figure a slack cord or chain naturally disposes itself by the gravity of its particles.

"11. Hence we have another property of the catenaria not hitherto taken notice of, that I know of, viz. that supposing AR (= a, the constant line in Dr. Gregory) equal to the radius of the nautical projection, and RN the secant of a given latitude, then is NT, the catenaria's ordinate at N, equal to RM the meridional part answering to the latitude whose secant is RN."

12. That TA is the catenaria, is also demonstrable from Dr. Gregory's first prop. Let TU be the fluxion of the ordinate NT; and tu (= NN) the fluxion of the axe AN. Then because of like triangles TCM, TUT, CM : CT (= TA) :: TU : ut, that is, as CM a constant line to TA the curve :: so is the fluxion of the ordinate, to that of the axe ($\dot{y} : \dot{x}$) according to prop. 1, Catenaria.

13. From the premises, the construction and several properties of the catenaria are easily deducible, one or two of which I will set down.

1. The area ATMR is equal to AOPR, a rectangle contained by radius AR and RP the tangent answering to secant HP = TM. For because of the like triangles CMm, CEe; CM : CE :: Mm : Ee, that is, (putting r, s, t, m, for radius, secant, tangent, and meridional part RM) $r : s :: \dot{m} : \dot{t}$, whence $r\dot{t} = s\dot{m}$, and all the $r\dot{t}$ = all the $s\dot{m}$, that is, AOPR = ATMR, which agrees with Dr. Gregory's cor. 5, of prop. 7.

14. Supposing the former construction, let be added the line RH, including the hyperbolic sector ARH. I say the same sector is equal to half the rectangle ARMQ contained by radius AR and the meridional part RM, (= $\frac{1}{2}rm$). For the sector ARH = triangle RNH wanting the semisegment ANH. The fluxion of the

triangle RNH is $\frac{1}{2}s\dot{t}+\frac{1}{2}t\dot{s}$. The fluxion of ANH is $t\dot{s}$. So the fluxion of the sector ARH is $\frac{1}{2}s\dot{t}+\frac{1}{2}t\dot{s}-t\dot{s}=\frac{1}{2}s\dot{t}-\frac{1}{2}t\dot{s}$. It is found before, sect. 13, that $r:s\,(s:\frac{ss}{r}::\dot{m}:\dot{t}$; whence $s\dot{t}=\frac{ss}{r}\dot{m}$. And because of the like triangles CDE, Efe, CD : DE :: Ef : fe. But Ef = Mm = $\dot{m}$, because both Ef and Mm are to Md in the same ratio, viz. as s to r; therefore $r:t\,(t:\frac{tt}{r})::\dot{m}:\dot{s}$: whence $t\dot{s}=\frac{tt}{r}\dot{m}$, and $\frac{s\dot{t}-t\dot{s}}{2}=\frac{ss-tt}{2r}\dot{m}=\frac{rr}{2r}\dot{m}=\frac{1}{2}r\dot{m}=$ the fluxion of the hyperbolic sector ARH, whose flowing quantity is therefore equal to $\frac{1}{2}rm=\frac{1}{2}$ARMQ. Q. E. D.

15. This shows another property of the Catenaria, viz. that it squares the hyperbola; for RM is equal to NT, the ordinate of the Catenaria.

16. In fig. 10, let AR be radius, ACB the equitangential curve, MRN its asymptote, in which let M, N, be any two points equally distant from R. On M draw ML parallel to AR, and equal to the difference of the secant and tangent of that latitude whose meridional part is RM (by sect. 3, 4). On N draw NO parallel to AR, and equal to the sum of the aforesaid secant and tangent. Do thus from as many points in the asymptote as is convenient. And a curve drawn equably through the points L. . .A. . .O, &c. will be a logarithmic curve, whose subtangent, being constant, is equal to the radius AR.

17. Let no be an ordinate infinitely near and parallel to NO, op = Nn the fluxion of the asymptote, OT the tangent, and TN the subtangent to the logarithmic curve in O. Then $op:po$ :: ON : NT. But ON $=s+t$; therefore $op=\dot{s}+\dot{t}$, $po=\dot{m}$, the fluxion of the meridian or asymptote. So the analogy is $\dot{s}+\dot{t}:\dot{m}::s+t:$ NT. By sect. 13, 14, $\dot{s}:\dot{m}::t:r$, also, $\dot{t}:\dot{m}::s:r$, and thence $\dot{s}+\dot{t}:\dot{m}::t+s:r$; therefore is NT (the subtangent to LAO) equal to radius AR a constant line, and consequently the curve LAO is the logarithmic curve, and its subtangent known.

18. The same demonstration serves for LM, any ordinate on the other side of AR, only changing the sine + into —; and then it agrees with Mr. James Gregory's prop. 3, p. 17, of his Exercitations, viz. "That the nautical meridian is a scale of logarithms of the differences whereby the secants of latitude exceed their respective tangents, radius being unity." So here RM is the logarithm of ML, the difference of the secant and tangent of the latitude whose meridional part is RM.

19. Supposing the preceding construction, if through any point C of the curve ACB be drawn a right line GCW, parallel to MR, terminated by the logarithmic curve in W, and the radius AR in G; then the same right line WG is equal to the intercepted part of the curve line AC.

20. Let wg be a line infinitely near and parallel to WG, and terminated by the same lines; and cs, wσ, perpendicular to the meridian; cs intersecting wg in z, and wσ in y. Let CM be a tangent to AC in C; wτ a tangent to AW in W; so is CM = $\sigma\tau$. Because of like triangles czc, CSM; and wyw, W$\sigma\tau$; cs : CM :: cz : cc: also wσ : $\sigma\tau$ :: wy : yw. But wσ = cs; $\sigma\tau$ = CM; cz = wy; therefore is *yw* the fluxion of GW, equal to cc the fluxion of the curve AC. Consequently GW = AC. Q. E. D.

21. It may be noted that this equitangential curve gives the quadrature of a figure of tangents standing perpendicular on their radius. In fig. 8, let A$\gamma\Gamma$ be a curve whose ordinates, as gγ, GΓ, are equal the tangents of their respective intercepted arcs Ak, Aκ. Let ΓG be produced to touch the curve AC in C: then is the area AΓG equal to the rectangle contained by radius AR and GC the produced part of the ordinate; or AΓG = AR $\times$ GC. The demonstration of which, and of the following section, I for brevity omit.

22. If we suppose the figure ACB &c KR, fig. 8, infinitely continued, to be turned about its asymptote RK as an axe, the solid so generated will be equal to a rectangled cone whose altitude is equal to AR. And its curve surface will be equal to half the surface of a globe, whose radius is AR. So that if the curve be continued both ways infinitely, as its nature requires, the whole surface will be equal to that of a globe of the same radius AR.

The description of the ruler and wheel, fig. 9, is sufficient for the demonstration of the properties of the curve; but in order to an actual construction for use, I have added fig. 11, where AB is a brass ruler: wh the little wheel, which must be made to move freely and tight upon its axe, like a watch wheel, the axe being exactly perpendicular to the edge of the ruler; s represents a little screw-pin, to set at several distances for different radii, and its under end is to slide by the edge of the other fixed ruler; p is a stud for conveniently holding the ruler in its motion.

Note.—Most of these properties of this curve by the name of la tractrice, are to be found in a Memoir of M. Bomie, among those of the Royal Academy of Sciences for the year 1712, but not published till 1715; whereas this paper of Mr. Perks was produced before the Royal Society in May 1714, as appears by their Journal.

An Account of a Book, entitled Methodus Incrementorum. Auctore Brook Taylor, LL.D. et R.S.S. By the Author. N° 345, p. 339.

When I applied myself to consider thoroughly the nature of the method of fluxions, which has been the occasion of so much honour to its great inventor Sir Isaac Newton, I fell by degrees into the method of increments, which I have endeavoured to explain in this treatise. For it being the foundation of the method of fluxions, that the fluxions of quantities are proportional to the nascent increments of those quantities, in order to understand that method thoroughly, I found it necessary to consider well the properties of increments in general. And from those properties I saw it would be easy to draw a perfect knowledge of the method of fluxions; for if in any case the increments are supposed to vanish, and to become equal to nothing, their porportions become immediately the same with the proportions of the fluxions. In this method I consider quantities, as formed by a continual addition of parts of a finite magnitude, and those parts I call the increments of the quantities they belong to, because that by the addition of them the quantities are increased. These parts being considered as formed in the same manner by a continual addition of other parts, thence follows the consideration of second increments, and so on to third, fourth, and other increments of a higher kind. For example, if x stands for any number in the series 0, 1, 4, 10, 20, 35, &c. in which the numbers are formed by a continual addition of the numbers in the series 1, 3, 6, 10, 15, &c. then the numbers in the latter series are called the increments of the numbers in the foregoing series; thus, for example, if to the third number (4) in the first series, I add the corresponding third number (6) in the second series, I shall produce the next, that is the fourth number (10) in the first series; and so of the rest. Any number in the first series being called x, the corresponding number, which is its increment, in the second series, I express by $\underset{\cdot}{x}$. And these numbers $\underset{\cdot}{x}$ being formed in the same manner by the numbers in the series 1, 2, 3, 4, 5, &c. I call these last numbers $\underset{\cdot\cdot}{x}$, they being the first increments of the numbers $\underset{\cdot}{x}$, and the second increments of the numbers x; and so on. Hence having given any series of numbers that are called by a general character x, their increments are found by taking their differences; thus in the present example, the first increments $\underset{\cdot}{x}$ in the series 1, 3, 6, 10, 15, &c. are found by taking the differences of the numbers x in the series 1, 4, 10, 20, 35, &c. and the second increments $\underset{\cdot\cdot}{x}$ in the series 1, 2, 3, 4, 5, &c. are found in the like manner, by taking the differences of the numbers $\underset{\cdot}{x}$; and so of the third and other increments. This method consists of two parts: one is concerned in showing how to find the relations of the increments of several variable quantities, having given the relation of the quantities themselves; and the other is concerned in finding the relations of the integral quantities themselves freed from the consideration of their increments, having given the relations of the increments; either simply, or being any how compounded with their integral quantities. In the method of fluxions, quantities are not considered with their parts, but with the velocities of the motions they are supposed to be formed by; or, to speak more accurately, they are considered with the quantities of the motions by which they are supposed to be generated; for the fluxions are proportional to the velocities, only when the moving quantities, which produce the flowing quantities considered, are equal. These quantities of motion, or velocities, when the moving quantities are equal, are what Sir Isaac Newton calls fluxions. As in the method of increments there are second, third, and other increments; so in the method of fluxions there are second, third, and other fluxions; the fluxions themselves being considered as quantities that are formed by motion, the quantity of which motion is their fluxions. As the method of increments consists of two parts: one being concerned in finding the increments from the integrals given, and the other in finding the integrals, having the increments given; so the method of fluxions consists of two parts: the one showing how to find the fluxions, having the fluents given; and the other showing how to find the fluents freed from fluxions, having given the relations of the fluxions, whether compounded with their fluents or otherwise. The principles of this method may all be drawn directly as a corollary from the principles of the method of increments. For Sir Isaac Newton having demonstrated, Phil. Nat. Princ. Math. sect. 1, and in the beginning of his treatise De Quadratura Curvarum, that the fluxions of quantities are proportional to their nascent or evanascent increments, if in any proposition relating to increments you make the increments to vanish, and to become equal to nothing, and for their proportion put the fluxions, you will have a proposition that will be true in the method of fluxions. This is but a corollary to Sir Isaac Newton's demonstration of the fluxions being proportional to the nascent increments. For this reason, to make the method of fluxions to be understood more thoroughly, I thought it proper to treat of these two methods together, and I have handled them promiscuously, as if they were but one method. Some people, because that the fluxions are proportional to the nascent increments of quantities, have thought that by the method of fluxions Sir Isaac Newton has introduced into mathematics the consideration of infinitely small quantities; as if there were any such thing as a real quantity infinitely small. But in this they are mistaken, for Sir Isaac only considers the first or last ratios of quantities, when they begin to be, or when they vanish, not after they have become something, or just before they vanish; but in the very moment when they do so. In this case, quantities are not considered as infinitely little; but they are really nothing at the time that Sir Isaac takes the proportions of their fluxions; and the truth of this method is demonstrated from the principles of the method of increments, in the same manner as the ancients demonstrated their conclusions in the method of exhaustions, by a deductio ad absurdum.

Having premised thus much in general, concerning the two methods here treated of, to come to a particular description of this book; in the preface I give a short description of the method of increments, and an account of Sir Isaac Newton's notion of the fluxions which I have already spoken of. The book consists of two parts, and contains 118 pages in 4to, the propositions being numbered throughout from the beginning. In the first part I explain the principles of both methods: and in the second part I show the usefulness of them in some particular examples.

After having explained the notation I make use of in the introduction, in the first proposition I explain the direct method, both of increments and of fluxions. The second proposition shows how to transform an equation wherein integrals and their increments, or wherein fluents and their fluxions are concerned; so as instead of the integrals or fluents, to substitute their complements to a given quantity, with their increments or their fluxions, these increasing in a contrary sense to the quantities in the first supposition. In the third proposition I show how to transform a fluxional equation, so as to change the characters of the fluents, making that quantity to flow uniformly, which in the first supposition flowed unequally, having second, third, and other fluxions, and making that quantity which in the first supposition flowed uniformly, now to flow unequally, so as to have second and third fluxions, &c. This proposition is of great use in the inverse method, when we would invert the expression of the relation of the flowing quantities; for example, if in the supposition z flows uniformly, and x variably, by the inverse method of fluxions we find x expressed by the powers of z; but if we would find z expressed by the powers of x, we must then transform the equation by this proposition. Sir Isaac Newton and Mr. de Moivre do this by the reversion of serieses; but I take this to be the more proper and more genuine method of doing it directly. In the 4th and 5th propositions are explained the method of judging of the nature and number of the conditions that may accompany an incremental or a fluxional equation. This is a circumstance that I do not find to have been explained by any one before, and the propositions are somewhat intricate. The conditions that attend incremental or fluxional equations I do not know to have been sufficiently taken notice of by any one; but they ought well to be attended to in the inverse methods; the solutions of particular problems being never perfect, unless there be provision made for the satisfying of them, by the indetermined coefficients in the equation that contains the solution of the problem. Examples of this may be seen in prop. 17 and 18, where I give the solution of the problems concerning the Isoperimeter and the Catenaria.

The sixth proposition contains the general explanation of the inverse method, both of fluxions and of increments, which consists in the solution of this problem: having given the relations of the increments, or of the fluxions of several quantities, whether they be considered with their proper integrals or with their proper fluents, or not; to find the relations of the integrals or of the fluents, freed from their increments or from their fluxions. The direction I have given for finding the solution in finite terms is but tentative. And I must confess I know of no other method that is general for all cases. For I can find no certain rule to judge in general, whether any proposed equation, involving increments or involving fluxions, can be resolved in finite terms. For this reason, we are obliged to seek the general solution in infinite serieses; which when they break off, or when they can any way be reduced to finite terms, they then contain the solutions which we always hope for. The method of finding these serieses is explained in the 8th proposition, and that is by means of a series that is demonstrated in the 7th proposition. And this I take to be the only genuine and general solution of the inverse methods. For in this solution we always have those indetermined co-efficients, which are necessary to adapt the equation that is found to the conditions of the problem proposed. For want of this circumstance all other methods are imperfect; and particularly Sir Isaac Newton's method of finding serieses by a ruler and parallelogram labours under this difficulty, because it brings no new co-efficients into the resulting equation, which may afterwards be determined by the conditions of the problem. However because this method is very ingenious and very elegant, I thought it proper to explain it in the following (viz. the 9th) prop. The 10th, 11th, and 12th propositions conclude the first part, and in them I treat of the manner of finding the integral or the fluent, having given the expression of a particular increment, or of a particular fluxion of it: without being involved with the integrals; or with the fluents, or with any other increments, or with any other fluxions of it. This is a particular case of the inverse method, but for its great usefulness I thought it deserved particularly to be taken notice of. This problem is treated of in general in the 10th proposition. The method of solving it in finite terms is only tentative; and when that does not succeed, recourse must necessarily be had to the solution by a series in the 8th proposition. In the 11th and 12th propositions I have showed how serieses may be conveniently found, in some particular cases when fluxions are proposed.

In the 2d part I have endeavoured to show the usefulness of these methods in the solution of several problems; the 13th proposition is much the same with Sir Isaac Newton's Methodus Differentialis, when the ordinates are at equal distances: and in an example at the end of this proposition I have showed how easily Sir Isaac Newton's Series, for expressing the dignity of a binomial, may be found by this incremental method. The 14th proposition, shows in some measure how this method may be of use in summing up of arithmetical serieses. In the 15th proposition I show by some examples how the proportions of the fluxions are to be found in geometrical figures; from whence immediately flows the method of finding the radii of their inosculating circles, the invention of the points of contrary flexure, and the solution of other problems of the like nature. In the 16th Proposition I show how the method of fluxions is to be applied to the quadrature of all sorts of curves. In the following Proposition I give a general solution of the problem of the Isoperimeter, which has been treated of by the two famous mathematical brothers the Bernoullis. In the 18th Proposition I give the solution of the problem about the catenaria, not only when the chain is of a given thickness every where, but in general, when its thickness alters according to any given law. In the following Proposition I show the fornix, or arch which supports its own weight, to be the same with the catenaria. In the two next Propositions I show how to find the figures of pliable surfaces which are charged with the weight of a fluid. In the 22d and 23d Propositions I treat of the motion of a musical string, and give the solution of this problem: to find the number of vibrations that a string will make in a certain time, having given its length, its weight, and the weight that stretches it. This problem I take to be entirely new, and in the solution of it, in the last part of Prop. 23, there is a remarkable instance of the usefulness of the method of first and last ratios. The 24th Proposition gives the invention of the centre of oscillation of all bodies; and in the 25th Proposition I have given the investigation of the centre of percussion. It is known that this problem is solved by the same calculus as the foregoing; therefore it is generally thought that these two centres are the same. But that is a mistake, because the centre of oscillation can be only one point; but the centre of percussion be any where in a certain line, which this Proposition shows how to find. There is an error in this Proposition, which I was not sensible of till after the book was published, therefore I take this opportunity of correcting it. It does not affect the reasoning by which I find the distance of the centre of percussion from the axis of rotation; but it is this, that I supposed the centre of percussion to be in the plane passing through the centre of gravity, and perpendicular to the axis of rotation: which is a mistake. It is corrected by the following Proposition.

Prop. Prob.—To find the Distance of the Centre of Percussion from the Plane passing through the Centre of Gravity, and perpendicular to the Axis of Rotation. —*Solution.* Let fig. 12, pl. 4, be supposed in the plane passing through the axis of rotation, and in which the centre of perscussion is sought. Let AB be the axis of rotation; AGC the intersection of this figure with the plane passing through the centre of gravity, and perpendicular to the axis of rotation; G the point on which a line, raised perpendicular to this figure, will pass through the centre of gravity; BE a line parallel to AG, which is the centre of percussion. Then to find the distance AB, let p stand for an element of the body proposed standing perpendicularly on any point D. Draw DC perpendicular to AGC; then AB will be equal to the sum of all the quantities p × GC × CD taken with their proper signs, divided by the body itself multiplied into the distance AG.

Having thus found the distance AB, suppose the plane of the figure in Prop. 25 to cut the present figure at right angles in the line BE, and the centre of percussion will be rightly determined by that Proposition.

The 26th Proposition shows how to determine the density of the air at any distance from the centre of the earth, supposing the density always to be proportional to the compressing force, and that the power of gravitation is reciprocally at the distances from the centre of the earth.

The last Proposition shows how to find the refraction of a ray of light in its passage through the atmosphere, on the supposition that light is a body, and that its refraction is caused by the attraction of the bodies the rays approach to. In this proposition there is a remarkable instance of the usefulness of the method of increments, in finding the co-efficients of a Series, which according to the values of a certain symbol, as *n*, expresses both all the fluents, and all the fluxions of a certain quantity.

II. Ludovici Ferdinandi Marsilii Dissertatio de Generatione Fungorum. Rom. 1714, *4to.* N° 345, p. 350.

In this dissertation the author gives an account of the various opinions, both ancient and modern, respecting the generation of mushrooms. He supposes the seed-like bodies observable in the fungus seminifer campaniformis Mentzelii, to be the ovaria of some insects; and therefore he concludes that these bodies ought to have another denomination than seed; neither is he of opinion that mushrooms are produced by parts of themselves. In his division of mushrooms he 1st treats of truffles; 2dly, of those mushrooms (fungi) which grow from wood, but are soft; 3dly, of hard woody mushrooms. Of all which sorts he gives figures. Subjoined to the whole is a communication from Lancisi, concerning the so called lapis fungarius; viz. that although this mushroom-producer has the name of a stone, it ought not to be reckoned of that genus, it being really no other than a mass or congeries of roots, seeds and juices, coagulated with earth into, as it were, a stony substance. On which pouring water, and setting it in a warm place, it loosens its hardened substance; and by mollifying its fibres, and moistening its concrete juices, out of its clefts and chinks, the mushrooms spring, as they do in other places from simple dung and loose earth. And it is also further to be noted, that when this stony mass has thus yielded these its offspring, the remainder grows light, porous, and decayed, its nutritive juices being then exhausted.

A short History of the several New Stars that have appeared within these 150 *Years; with an Account of the Return of that in Collo Cygni, and of its Continuance observed this Year* 1715. N° 346, p. 354.*

Whether it be owing to the greater diligence of the moderns, or that in reality no such thing has happened for many ages past, I will not undertake to determine; but this is certain that, within the space of the last 150 years, more discoveries have been made of changes among the fixed stars, than in all antiquity before. And though it be said that Hipparchus, on occasion of a new star that appeared in his time, was induced to number the stars, and make the first catalogue of them, which was, in the opinion of Pliny, Res vel Deo improba; yet neither he nor any of the ancients have left us the place of that new star, to compare with those lately seen, one of which might perhaps be the same with it, re-appearing after a long period of years. Now though several authors have severally described those that have been seen nearer to our times, it may not perhaps be amiss here to give a short recapitulation of what was principally remarkable in each of them, with the times of their first appearance, as far as can be collected.

And 1st. That in the chair of Cassiopeia, was not seen by Cornelius Gemma on the 8th of November 1572, who says, he that night considered that part of heaven in a very serene sky, and saw it not: but that the next night, Nov. 9, it appeared with a splendor surpassing all the fixed stars, and scarcely less bright than Venus. This was not seen by Tycho Brahe before the 11th of the same month, but from thence he assures us that it gradually decreased

* This anonymous paper, on the new stars, was probably written by Dr. Halley, then secretary to the Royal Society, as the composition very much resembles his style and manner.

and died away, so as in March 1574, after 16 months, to be no longer visible; and at this day no signs of it remain. Its place, in the sphere of fixed stars, by the accurate observations of the same Tycho, was $0^s\ 9^\circ\ 17'$ à 1^{ma} ✱ ♈^{is}, with $53^\circ\ 45'$ north latitude.

Such another star was seen and observed by the scholars of Kepler, to begin to appear on Sept. 30, O. S. 1604, which was not to be seen the day before: but it broke out at once with a lustre surpassing that of Jupiter; and like the former it died away gradually, and in much about the same time disappeared totally, there remaining no traces of it in Jan. 1605-6. This was near the ecliptic, following the right-leg of Serpentarius; and by the observations of Kepler and others, was in $7^s\ 20^\circ\ 00'$ à 1^{ma} ✱ ♈, with north latitude $1^\circ\ 56'$. These two seem to be of a distinct species from the rest, and nothing like them has appeared since.

But between them, viz. in the year 1596, we have the first account of the wonderful star in collo ceti, seen by David Fabricius on the 3d of August, O.S. as bright as a star of the 3d magnitude, which has been since found to appear and disappear periodically; its period being nearly 7 revolutions in 6 years; though it returns not always with the same lustre. Nor is it ever totally extinguished, but may at all times be seen with a six-foot tube. This was singular in its kind, till that in collo cygni was discovered. It precedes the first star of aries $1^\circ\ 40'$, with $15^\circ\ 57'$ south latitude.

Another new star was first observed by Will. Jansonius, in the year 1600, in pectore, or rather in eductione colli cygni, which exceeded not the 3d magnitude. This having continued some years, became at length so small, as to be thought by some to disappear entirely: but in the years 1657, 58 and 59, it again rose to the 3d magnitude, though soon after it decayed by degrees to the 5th or 6th magnitude, and at this day is to be seen as such in $9^s\ 18^\circ\ 38'$ à 1^{ma} ✱ ♈, with $55^\circ\ 29'$ north lat.

A 5th new star was first seen and observed by Hevelius, in the year 1670, on July 15, O. S. as a star of the 3d magnitude; but by the beginning of October it was hardly to be perceived by the naked eye. In April following it was again as bright as before, or rather greater than of the 3d magnitude, yet wholly disappeared about the middle of August. The next year, in March 1672, it was seen again, but not exceeding the 6th magnitude: since then, it has been no further visible, though we have frequently sought for its return; its place is $9^s\ 3^\circ\ 17'$ à 1^{ma} ✱ ♈, and has lat. $47^\circ\ 28'$ north.

The 6th and last, is that we described from the Acta Berolinensia, in N° 343 of these Transactions; discovered by Mr. G. Kirch, in the year 1686, and its period determined to be of $404\frac{1}{2}$ days: and though it rarely exceeds the 5th magnitude, yet is it very regular in its returns, as we found in the year 1714. Since then, we have watched, as the absence of the moon and the clearness of weather would permit, to observe the first beginning of its appearance in a six-foot tube, which bearing a very great aperture discovers most minute stars. And on June 15 last, it was first perceived like one of the very least telescopical stars: but in the rest of that month and July it gradually increased, so as to become in August visible to the naked eye; and so it continued all the month of September. After that it again died away by degrees, and on the 8th of December at night it was scarcely discernible by the tube, and as near as could be guessed, equal to what it was at its first appearance on June 15th: so that this year it has been seen in all near 6 months, which is but little less than half its period: and the middle, and consequently the greatest brightness, falls about the 10th of September. Those that please to seek for it, may expect its first appearance in July next, and find it in $9^s\ 6^\circ\ 30'$ circiter à $1^{ma} \ast \Upsilon$, with lat. bor. $52^\circ\ 40'$.

Botanicum Hortense IV; *continued from* N° 345. *By James Petiver, F.R.S.* N° 346, p. 353. *Sect. II.*

*An Account of the Pareira Brava. By Dr. Helvetius.** N° 346, p. 365.

This letter gives an account of the supposed medicinal virtues of the pareira brava, a root brought from the Brazils, with the manner of taking it. As this drug has long since lost its reputation for curing or relieving nephritic and calculous affections, it is deemed unnecessary to reprint this commendatory account of it.

* This physician, Adrian Helvetius, was a native of Holland, but he settled at Paris, where after some time he excited the attention of the public by a new and successful method of treating the dysentery by a remedy which he at first kept a secret, but of which he afterwards published a circumstantial account (see Vol. iv. p. 237, of these Abridgments), on receiving a premium (after the efficacy of the remedy had been sufficiently proved by repeated trials at the Hotel-Dieu) of 1000 louis d'or from the French king Lewis XIV. He was moreover appointed physician to the Duke of Orleans, and inspector general of the Military Hospitals. He died in 1727. From the above account of the Pareira Brava, as well as from his Traité des Maladies les plus frequentes et des Remédes specifiques, it appears that he was much addicted to empiricism, and that he ascribed to many drugs a degree of medicinal power far exceeding that which they really possess. His son John Claude Adrian Helvetius was likewise a physician, and was honored with the favor of the Court. He was member of several learned academies, and wrote a treatise in French on the Animal Œconomy; and died in 1755. He was father to the metaphysical Helvetius, author of the celebrated treatise de l'Esprit, of another work de l'Homme, and of a poem entitled le Bonheur, now very little read.

An Account of some large Teeth lately dug up in the North of Ireland. By Mr. Francis Nevile. N° 346, p. 367.

You here have the draught of two teeth lately found within 8 miles of Bulturbet, at a place called Maghery, in part of the Bishop of Killmore's lands, on digging the foundation for a mill near the side of a small brook, that parts the counties of Cavan and Monoghan.

There are in all 4 teeth, two of a larger and two of a smaller sort; the larger one is the farthest tooth in the under jaw; the other is like it, and belongs to the opposite side; the lesser tooth I take to be the 3d or 4th tooth from it, and has its fellow: these are all that were found, and one of them in a piece of the jawbone, which mouldered away as soon as taken out of the earth; there was part of the scull found also of a very large size and thickness, but as soon as exposed to the air, it mouldered away as the jaw had done.

Some few pieces of other bones were found, but none entire; yet by those bits that were found, one might guess that they were parts of those that were of a larger size.

The place where this monster lay was thus prepared, which makes me believe it had been buried, or that it had lain there since the deluge. It was about 4 feet under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about 30 yards from the brook. The bed on which it lay had been laid with fern, with that sort of rushes here called sprits, and with bushes intermixed, and nut shells. Under this was a stiff blue clay, on which the teeth and bones were found: above this was first a mixture of yellow clay and sand, much of the same colour; under that a fine white sandy clay, which was next to the bed: the bed was for the most part a foot thick, and in some places thicker, with a moisture clear through it; it lay close, and cut much like turf, and would divide into flakes, thicker or thinner at pleasure; and in every layer the seed of the rushes was as fresh as if new pulled; so that it was in height of seed-time that those bones were laid there. The branches of the fern, in every lay as we opened them, were very distinguishable, as were the seeds of the rushes and the tops of boughs. The whole matter smelt very sour, as it was dug, and tracing it I found it 34 feet long, and about 20 or 22 feet broad.

It will be worth considering what sort of a creature this might be, whether human or a brute; if human, there was some reason for the interment, and for that preparation of the bed it was laid on; if a brute, it was not worth the trouble: if human, it must be larger than any giant we read of; if brutal, it could be no other than an elephant, and we do not find that those creatures were ever the product of this climate. And considering how long this must have lain here, I do not believe the inhabitants then had any curiosity or conveniency to bring such into this kingdom; for I suppose the best of their ships could not carry one. Then if an elephant, or some other beast, which must have proportion to the teeth, it must have lain there ever since the flood; and if so, then the bed on which it lay must be of its own making: whence it will follow that the flood coming on him while he lay in his den, he was there drowned, and covered with slime or mud, which since is turned into the substance of the earth before-mentioned.

The two large teeth are of equal weight, $2\frac{3}{4}$lb. each; the two small teeth are 6 ounces each; but some of them are wasted, and some roots that enter the jaw broken off.

Remarks on the foregoing Paper and the Teeth. By Thomas Molyneux, M. D. and R. S. S. Physician to the State in Ireland. N° 346, p. 370.

Having examined the 4 teeth above-mentioned, I am fully convinced, and can on sure grounds affirm, that they must certainly have been the four grinding teeth in the lower jaw of an elephant: and that the many loose fragments of those large bones that were found with them, must have been remains of the same animal. This I take to be one of the greatest rarities that has yet been discovered in this country.

In pl. 4, fig. 13, AA represents the large grinder of the under jaw on the right side, weighing $2\frac{1}{4}$lb. bbbbbbb are white, rough, indented borders, of an irregular shape, rising about the 10th of an inch higher than the hard black shining surface of the tooth; this rough raised work serves for the bruising and grinding the animal's food, the tough grains of rice, the leaves, fruits and boughs of trees; and is of so extreme hard a texture, that it resembles large knotted threads of white glass, laid on and closely fastened to the dark superficies of the tooth; and answers to that glassy surface or enamel, wherewith nature has armed the outside of the teeth of most animals, to prevent their wearing by the constant attrition in chewing their food.

Also, ccccc is that part of the tooth which rises above the gums, and continues even now distinguished from the rest of the bone, by having its colour of a different shade. And ddddddd are several strong fangs or roots, seemingly united altogether, by which the tooth received its sense and nourishment; and though it was so large and ponderous, by these it was kept firmly fixed into the jaw.

As to the mechanism nature has followed in framing the teeth of this animal, it is no more than this: whereas in other animals, she has divided that bony substance with which they chew their food, with each its peculiar roots, to secure its articulation in the jawbone; she has in this huge animal, for the greater strength, stability, and duration of its teeth, and the better to provide for a complete attrition of the aliment, in order to perfect the digestion so thoroughly, as to sustain the life of the animal for 2 or 300 years, (as it is a common opinion in the east) contrived to make the substance of the teeth in their roots below, and in their upper parts above the gums, to unite closely together; and thus coalescing, form a few large massy teeth, instead of many small ones. As for instance, in the human body, which is of so much less size, the number of the teeth, (when the whole set is complete) is 32, whereas in the large elephant, the teeth of both the jaws amount in all but to 8, besides its two great tusks, which rather serve as horns for its defence than teeth to prepare its food, and therefore I think not so properly called teeth.

In fig. 14, EE represents the smaller grinding tooth of the under jaw on the same side: its surface covered over with the same white indented work, as before described for grinding the food. fff are three large roots, that kept it firmly fixed in the jaw bone. This smaller tooth weighed full 6 ounces.

In fig. 15, GG represents the large grinder of the under jaw on the left side, much of the size, shape and weight with its fellow, described in fig. 13. It shows its roots and all its parts, with the rough protuberant white work on its upper surface, formed after the same manner, and after the same strong model. And indeed, if one considers it, it is plain that were not the teeth of this animal made of so large a size, and of so massy and firm a substance, it were absolutely impossible they could resist the force, and bear all that pressure with which those vast muscles exert themselves, that move the lower jaw in mastication in so strong an animal.

In fig. 16, HH represents the smaller grinding tooth of the under jaw on the same side; it is less complete than the small tooth described before in fig. 14, for some of the root is wanting, and part of its outer grinding surface is broken off at kk, so that it weighs somewhat less; yet what remains shows exactly the same kind of work and shape as the other tooth that answered it on the right side.

The four teeth fully complete the set of the teeth, with which nature has furnished the lower jaw of the elephants; and are answered by just as many more, formed after the same manner, in the upper jaw, as Dr. Moulins informs us, who dissected the elephant that was burnt here at Dublin in 1681. In its anatomy, p. 40, speaking of the teeth, he assures, that there were besides the tusks, only 4 teeth in each jaw, two in every side: and that these 8 teeth were all molares, so that he had no incisores.

This letter of Mr. Nevile, with Dr. Molineux's curious draughts of the teeth, and his learned remarks upon them, having been produced and read before the Royal Society, they then ordered that what teeth they had of like sort should be looked out and laid before them ; to which Sir Hans Sloane was pleased to furnish a yet greater variety, out of his incomparable collection of natural rarities. And to obviate all doubts, there being at this time in Westminster the entire skull of a large elephant with the teeth in it, it was likewise ordered to be viewed and compared with the figures: which done, it appeared that the teeth in question could be no other than those of an elephant.

By this inquiry we were likewise satisfied, that the number of teeth found, being but 4, was no objection: it appearing that the number of molares in this animal is not certain. Pliny, lib. 11, cap. 37, says expressly dentes elephanto intus ad mandendum quatuor, præter eos qui prominent. And in the remains of that mighty elephant described by Tenzelius, Phil. Trans. N° 234, there were no more than 4 teeth found. In that at Westminster there are 6, viz. one in each lower jaw, and two in each of the upper, whereof the inner tooth is about three times as long as the other, and both together longer than those of the under jaw by about an inch; the upper small teeth being much worn by grinding. These we have thought fit to represent by fig. 17, showing the rough grinding surface of the left under tooth, being considerably concave; and fig. 18, the same roughness on the upper teeth is shown, having a convexity answering to the concavity of the under, which is a circumstance not observed by any of those that have described them.

And although by the observation of Mr. Du Verney, Dr. Moulins, and Mr. Blair, who dissected three different elephants, it appears that each of them had 8 molares; yet from them it is also evident, that in the division of them nature observes no rule. For Dr. Moulins found the two teeth in each of the upper jaws of that he dissected, to be divided after a different manner; so that the inner tooth on the one side, and the outer on the other, was larger than its adjoining fellow, yet not so as to be very unequal: and Mr. Du Verney and Mr. Blair had on both sides the much greater tooth outwards; whereas the Westminster skull, on the contrary, has only a small one outwards, and the much greater grinder within. All which considered, we may with assurance conclude, that this elephant found in Ireland had but four teeth in his head when he died; and that the two greater were those of the upper jaws, and the other two those of the under.

Again, by the size of the grinding part, we may conclude these to be the teeth of a very young and small elephant; since they are not much above half the length of those that are to be seen at Westminster, which belonged to a beast of not more than between 10 and 11 feet high; nor much above one third of the length of a fossil elephant's grinder in the Royal Society's repository, which is here represented by fig. 19, (all the figures being drawn to the scale of one-fourth their true dimensions). Hence it is not to be marvelled that the bones of so young an animal, having not acquired their firmness, being in a growing state, should be dissolved by long lying in the earth, as also the roots of the teeth.

On this occasion, perhaps it may not be amiss to quote a passage out of Matthew Paris's History, who assures us, that in his time Louis IX (afterwards St. Louis) king of France, made a present of an elephant to his cotemporary Henry III of England; and that in the year 1255, after the English had been 80 years masters of Ireland. Of this says Matthew, nec credimus quod unquam aliquis elephas visus est in Anglia præter illum.

An Account of a Book, viz. Guilhelmi Musgrave Reg. Societ. utriusque Socii, Geta Britannicus. Accedit Domus Severianæ Synopsis Chronologica: et de Icuncula quondam M. Regis Ælfredi Dissertatio. 8vo. Iscæ Dumnoniorum, 1715. N° 346, p. 385.

The author having some years since published a Comment on Julius Vitalis's Epitaph, which, (with his monument) is to be seen at Bath; he now presents the public with another volume of Belgic Antiquities; intended to illustrate part of a statue, which was found likewise near that city, and is at this time immured near the monument aforesaid, at the eastern end of the abbey-church, fronting the grove.

This fragment of an equestrian statue, is in basse relief: the rider has in his right hand a hasta pura, and a parma in his left. It appears from Dio, that Caius and Lucius, Cæsars, the nephews, and adopted sons of Augustus, had each of them a parma and a hasta given him: and there being no instance of this honour paid to any of an inferior rank among the Romans, but only to such as were of very great quality; if not to Cæsars only; we may from hence be allowed to think, that this statue represented some person of that quality.

But to endeavour to discover the particular person, the author compared a very good draught he had procured of this horseman, with such Roman coins, as he could meet with. This comparison showed a great resemblance between the face in the statue, and that in two of Geta's coins. This argument, drawn from the similitude of faces, is further confirmed by the horse; a creature of which Geta was very fond; insomuch that he affected to be represented under the figure of Castor, (as the Roman emperors often were under the figures of their gods) of whom it is said, Castor gaudet Equis;—Of this figure, there is in Oiselius, a coin of Geta's, very much to this purpose; represented tab. 4, fig. 5, of this book.

These things bring to mind the authority which Geta had in South-Britain: where (as Herodian affirms) all matters were under his administration, during the stay which Severus and Caracalla made in the north; which was a year, or more. In this time, Geta had it in his power to do many things in favour of cities and countries, here in the south. The great generosity of his mind prompted him to public works; such as are to this day attested by inscriptions, with his name in them: and it is highly probable, that this statue was erected to Geta on some such account.

If this be granted, as from the concurrence of so much, and so good testimony, it seems highly probable, here is a large and pleasant view opened into antiquity; not of late taken notice of by any writer: it shows, that Geta was a great benefactor to old Bath; either by laying, in a perfect morass, the foundation of that town; or by preserving the hot-springs entire, from the influx of other waters; or both: works of great munificence, and becoming Geta's spirit. By these, or some such ways, it is probable this place was obliged to Geta; but no one is more probable, than that of preserving the aquæ calidæ; which were in those days so famous, as to give a denomination to the place. It is well known that Rome had her Thermæ Severianæ and Antoninianæ, so called from their respective founders; the former being built by Severus, the father, the latter by Antoninus, the brother, of Geta; so that to take care of baths, was a sort of greatness that family seemed to delight in; and Geta may reasonably be supposed to have his share of this delight.

From the great probability of this opinion, the author has, out of love to his native country, and the honour due to Geta, collected together what he can meet with relating to that emperor. He has made a new edition of Geta's life, from the Historiæ Augustæ Scriptores; restoring it to its true author, Julius Capitolinus; and explaining it, with the notes of Casaubon, Gruter, and Salmasius; to which he has added some of his own. He has reprinted all the inscriptions, he can meet with, of Geta's, and many of his coins; with short notes on both.

To this dissertation, de Geta Britannico, he has added the chronology of his illustrious house; showing how his father, Severus, from a private gentleman in Africa, came by degrees to be Emperor of Rome; and indeed one of the greatest, that ever Rome had: how he, with his two sons, Bassianus and Geta, three Roman emperors, resided at one and the same time, here in Britain, and from hence sent their imperial edicts, orders, and dispatches, into all parts of the empire: and after an amazing greatness of about 24 years, and a course of almost all virtues and vices, at length tumbled down; submitting to the accidents and fate of other men; and were all buried at Rome, in the septizodium built by Severus.

To these memoirs of Geta, the author has subjoined a discourse, concerning that curious cimelium, which was some years since found at Athelney in Somerset. It belonged to King Alfred, and is now in the possession of Col. Palmer of Fairfield, in that county. Besides the critical use made of it, by the learned Dr. Hickes, our author writes of it as an undeniable instance of the use of images, coming from the heathens into the Christian church.

An Account of several Nebulæ, or lucid Spots like Clouds, lately discovered among the Fixed Stars, by help of the Telescope. N° 347, p. 390.

In the last number we gave a short account of the several new stars that have appeared in the heavens, within the last 150 years, some of which afford very surprising phænomena. But not less wonderful are certain luminous spots or patches, which discover themselves only by the telescope, and appear to the naked eye like small fixed stars; but in reality are nothing else but the light coming from an extraordinary large space in the ether; through which a lucid medium is diffused, that shines with its own proper lustre. This seems fully to reconcile that difficulty which some have moved against the description Moses gives of the creation, alleging that light could not be created without the sun. But in the following instances the contrary is manifest; for some of these bright spots discover no sign of a star in the middle of them; and the irregular form of those that have, shows them not to proceed from the illumination of a central body.* These are, as the aforesaid new stars, 6 in number, all which we will describe in the order of time, as they were discovered; giving their places in the sphere of fixed stars, to enable the curious, who are furnished with good telescopes, to take the satisfaction of contemplating them.

The first and most considerable is that in the middle of Orion's sword, marked with θ by Bayer in his Uranometria, as a single star of the 3d magnitude; and is so accounted by Ptolemy, Tycho Brahe, and Hevelius: but is in reality two

* All or most of these lucid spots have lately, by means of the more powerful telescopes, been discovered to be clusters of very small stars, the mixed and united light of which yield that white appearance.—This paper also bears tokens of the composition of Dr. Halley.

very near stars environed with a very large transparent bright spot, through which they appear with several others. These are curiously described by Hugenius, in his Systema Saturnium, p. 8, who there calls this brightness portentum, cui certe simile aliud nusquam apud reliquas fixas potuit animadvertere: affirming that he found it by chance in the year 1656. The middle of this is at present in ♊ $19^{\circ}\ 00$, with $28\frac{3}{4}^{\circ}$ south lat.

About the year 1661, another of this sort was discovered, if I mistake not, by Bullialdus, in Cingulo Andromedæ. This is neither in Tycho nor Bayer, having been omitted, as are many others, because of its smallness: but it is inserted into the catalogue of Hevelius, who has improperly called it Nebulosa instead of Nebula; it has no sign of a star in it, but appears like a pale cloud, and seems to emit a radiant beam into the north east, as that in Orion does into the south east. It precedes in right ascension the northern in the girdle, or υ Bayero, about a degree and three quarters, and has longitude at this time ♈ 24°, with lat. north $33\frac{1}{3}^{\circ}$.

The 3d is near the ecliptic, between the head and bow of Sagittary, not far from the point of the winter solstice. This it seems was found in the year 1665, by a German gentleman, M. J. Abraham Ihle, while he attended the motion of Saturn then near his aphelion. This is small but very luminous, and emits a ray like the former. Its place at this time is ♑ $4\frac{1}{2}^{\circ}$, with about half a degree south latitude.

A 4th was found by M. Edm. Halley, in the year 1677, when he was making the catalogue of the southern stars. It is in the Centaur, that which Ptolemy calls ὁ ἐπὶ τȣ̃ νωτȣ ἐκφυσεως, which he names in dorso Equino Nebula, and is Bayer's ω: it is in appearance between the 4th and 5th magnitude, and emits but a small light for its breadth, and is without a radiant beam; this never rises in England, but at this time its place is ♏ $5\frac{3}{4}^{\circ}$, with $35\frac{1}{5}^{\circ}$ south latitude.

A 5th was discovered by Mr. G. Kirch, in the year 1681, preceding the right foot of Antinous: it is of itself but a small obscure spot, but has a star that shines through it, which makes it the more luminous. The longitude of this is at present ♑ 9° circiter, with $17\frac{1}{6}^{\circ}$ north latitude.

The 6th and last was accidentally hit upon by M. Edm. Halley, in the constellation of Hercules, in the year 1714. It is nearly in a right line with ζ and η of Bayer, somewhat nearer to ζ than η: and by comparing its situation among the stars, its place is sufficiently near in ♏ $26\frac{1}{2}^{\circ}$, with 57° north latitude. This is but a small patch, but it shows itself to the naked eye when the sky is serene, and the moon absent.

There are undoubtedly more of these, which have not yet come to our knowledge, and some perhaps larger; but though all these spots are in appearance but small, and most of them but of few minutes in diameter; yet since they are among the fixed stars, that is, since they have no annual parallax, they cannot fail to occupy spaces immensely great, and perhaps not less than our whole solar system. In all these so vast spaces it should seem that there is a perpetual uninterrupted day, which may furnish matter of speculation, as well to the curious naturalist as to the astronomer.

A New and safe Method of communicating the Small-pox by Inoculation, lately invented and brought into use. By Jacob Pylarini, M. D. formerly Venetian Consul at Smyrna.* N° 347, p. 393. *Translated and Abridged from the Latin.*

This paper gives an account of a medical operation not less astonishing (says the author) in regard to its discovery, than for the consequences that result from it; an operation invented not by persons conversant in philosophy or skilled in physic, but by a vulgar, illiterate people; an operation in the highest degree beneficial to the human race, inasmuch as it converts a violent and dangerous disorder into a mild one. The name of the person who invented this method is unknown; but it is very certain that it was first practised in Greece, and particularly in Thessaly; and being gradually introduced into the neighbouring places, at length it found its way to Constantinople; where however for some years only a few persons, among the lower orders of people, now and then made trial of it. But latterly, when the small-pox raged epidemically, it began to be more resorted to. Still however it was not introduced into the families of the great, until a certain Greek nobleman, with whom the author was in habits of friendship, towards the end of the winter 1701, applied to him for his opinion of this inoculation, and desired to know whether he would undertake to perform the operation upon his 4 sons; for at that time the small-pox was making dreadful ravages. The author told him he was at a loss to decide upon a mode of practice, concerning which he had hitherto had no experience; and that he should wish to have some conversation with a person who

* Jacob Pylarini is said to have been descended from a noble family, and to have been born in the island of Caphalonia in 1659. He studied at Padua, first the law, and afterwards physic. When he had taken his degree of M. D., he set out upon his travels, in the course of which he visited different parts of Asia and Africa, and practised both at Smyrna and Constantinople. He afterwards went to Moscow, where he was appointed physician to the Czar. But being fond of change, he removed from thence to Smyrna for the second time, and resided there in the character of Venetian Consul as well as practising physician. He died at Padua in 1718. He and Timoni appear to be the first who published in Europe an account of the inoculation of the small-pox, as practised among the modern Greeks. This account was also published by the author, in the form of a separate tract, in 12mo. Venice, 1715.

was experienced in the business. Three days after, when the author was again at his friend's house, and while they were both engaged in conversation on this subject, there was shown into the room a Greek woman, who explained to them the whole of the process. She referred to a vast number of cases in proof of the safety and success of this practice. Some of these cases (for in so large a city it was not possible to examine them all) the author found, on inquiry, to be exactly as she had stated. After duly considering all the circumstances, it appeared to him, that there was nothing irrational or absurd in this practice. Accordingly a few days afterwards, when his friend pressed him more earnestly for his opinion, he said, with some hesitation, that he did not object to the operation; whereupon this nobleman ventured to have his 4 sons inoculated by the beforementioned Greek woman. The 3 younger, who were from 5 to 7 years of age, were but slightly indisposed. A few eruptions appeared on the 7th day, after which the fever went off and they got well; but the elder, who was turned 18, was extremely ill. He had a continued malignant fever, accompanied with some alarming symptoms and a considerable eruption. He was ill for a fortnight; but at length recovered.* In consequence of this success, it is surprising how many families among the nobility were induced to follow this example; so that at the present time all but a very few timid persons among them (the Greeks) avail themselves of the benefit afforded by this discovery. The Turks alone, so addicted are they to their predestinarian notions, and so rivetted to ancient prejudices, neglect to reap any advantage from it.

There is nothing of trick or imposture in this operation, as in the pretended sympathetic cures mentioned by Tentzelius, Bartholine, Maxwell, Etmuller, &c. On the contrary the transplantation of the small-pox is effected by a medium or substance, perceptible to the touch, as well as to the sight. It consists simply in the insertion into a healthy body, by small wounds or punctures made on purpose, of a fermentum morbificum or pus taken from small-pox eruptions. The pus introduced into the wounded parts puts on the nature of a real ferment; and being carried by the circulation into the mass of blood, excites therein (as the author supposes) an universal ebullition, by means of which the more impure and heterogeneous particles are separated from the rest and thrown out, by a crisis, upon the skin; and all this is effected, by virtue of this operation, without much accompanying indisposition.

After these preliminary remarks, Dr. Pylarini proceeds to describe the operation as performed by the aforesaid Greek woman, together with all the circum-

* Dr. Pylarini supposes that the severe form under which the disorder appeared in the elder son was owing to his bad habit of body, and the neglect of due preparation.

stances connected with it; of the chief and most material part of which he himself had been an eye-witness.

First, the proper season for performing the operation should be chosen. Winter was the only season in which the Greek woman inoculated; but Dr. Pylarini supposes that the spring would be equally favourable.

Secondly, the best sort of matter (fermentum) should be chosen. This person would not inoculate with matter taken indifferently from any subject; but when the small-pox prevailed epidemically, she fixed upon some young boy, who appeared to be of a sound constitution in other respects, and in whom the pustules were distinct and of a good sort, and having punctured the pustules when they had come to maturity, she squeezed the matter out of them into a little shell or small glass made very clean, and afterwards covering it from the air, she put it into the bosom of her servant, where it was kept of a proper temperature, in readiness for inoculation, which she performed without delay. She never used matter from the inoculated small-pox, deeming it inefficacious; although Dr. P. very shrewdly conjectures that such matter may be milder, and yet at the same time equally capable of exciting the disease.

Thirdly, she directed the patient's room to be kept warm.

Fourthly, when she entered upon the operation, she punctured the middle and upper part of the forehead, the chin and both cheeks, with a needle. The puncture was made not perpendicularly, but obliquely, the cutis being separated a little with the sharp point of the instrument, from the subjacent flesh. She then introduced into the wound the pus contained in the small vessel beforementioned; and afterwards tied on a bandage. She made similar punctures in the back of the hands, and on the feet, strictly cautioning the patient against scratching or wetting the inoculated places.* All other modes of operating besides this were deemed improper. The patient was directed not to lie in bed more than was necessary.

Fifthly, with regard to regimen and diet, the patients were ordered to abstain from all kinds of animal food (including even flesh-broths) for the space of 40 days. It is added, that where this regimen had not been strictly observed, a fresh crop of eruptions had taken place, accompanied with alarming symptoms.

The interval of time between the performance of the operation and the appearance of the eruption varies according to the diversity of temperaments,

* Dr. Pylarini remarks that he should choose for this purpose the more fleshy parts of the body, rather than the tendinous parts above-mentioned.

ages and constitutions; but in general the eruption begins to show itself on the 7th day, which is therefore considered as a critical day. In like manner the symptoms vary according to the diversity of temperament, habit of body, and other circumstances; being sometimes more slight, sometimes more violent; but in general they differ in no respects from those which accompany the natural small-pox, except in being milder. Many on whom the operation is performed are scarcely at all disordered.

The inoculated small-pox is almost always of the distinct sort, and the pustules few in number; 10, 20, 30, seldom 100, and very rarely 200, in the same subject.

It is to be remarked, 1st. That in some instances a single puncture in the arm has sufficed for communicating the small-pox; and that although in such persons but few eruptions appeared, they nevertheless continued afterwards to be proof against infection. 2dly, That in some instances, owing to a peculiarity of constitution, or to the want of activity in the matter employed, the operation did not take effect. Such persons were found as liable to take the infection in the natural way, whenever the small-pox prevailed epidemically, as those who had never been inoculated. 3dly, That the inoculated places always rise into pustules; and in some instances large suppurations or abscesses are formed there. Sometimes, but very rarely indeed, abscesses have formed in the glandular parts. Lastly, that hitherto scarcely a single instance had occurred of inoculation having terminated fatally; although it had been performed upon persons of all ages and constitutions: indeed, when the operation is performed in a proper manner, and upon subjects duly prepared for it, it is attended with perfect safety. The small-pox thus produced is much milder than the small-pox taken in the natural way, the eruptions being of the distinct sort, and the febrile symptoms much slighter. To these considerations are to be added the advantages gained by choosing the season of the year the most favourable for the operation, and by preparing the constitutions of those on whom it is to be performed, by a proper regimen and diet.*

* A similar mode of inoculation, under the appellation of *buying the small-pox*, seems to have been practised in various parts of the East, from time immemorial. (See Woodville on Inoculation.) And even in Wales, from a very remote period, under the same name, as appears from the communications transmitted to the R. S. on this subject by Dr. Perrot Williams and Mr. Wright. See Phil. Trans. vol. xxxii. It is highly curious, says Dr. Woodville, that in so many distant nations, differing widely in manners, customs, laws, habits, and religion, that the art of inoculation should be generally known by the name of *buying the small-pox*. He considers it as a remarkable proof of its great antiquity, that the less civilized part of mankind, or people of the most simple and uniform habits, have retained this custom the longest.

A general Solution of a Problem concerning Curves, formerly proposed in the Leipzig Acts. N° 347, p. 399. *Translated from the Latin.*

In the Acta Eruditorum for Oct. 1698, p. 471, Mr. John Bernoulli writes thus: "At length I have discovered the general method I wished for, for regularly cutting curves that are given in position, whether algebraical or transcendental, in an angle either right or oblique, whether invariable or varying according to a given law; to which, in the opinion of M. Leibnitz, nothing can be added for its further perfection, and for this reason, that it always leads to an equation; in which, though the indeterminate quantities be sometimes inseparable, the method is not the less perfect for that; for it belongs not to this, but to some other method to separate them. I entreat my brother therefore to try his skill in a matter of so much moment. And he will not repent of his labour, if he happen to be successful. I know he will then forsake the method he is now so fond of, which can only be applied on very few occasions."

These three great men, for 4 or 5 years, had been in the habit of exercising one another, in proposing and solving such kind of problems. Without the spirit of divination, it would be difficult to give the very same solution as that of M. Bernoulli. But it may suffice that the following solution is general, and always leads to an equation. The problem is as follows.

PROBLEM.—Required a general method of finding a series of curves, which shall cut at a given angle, or in an angle varying by a given law, curves that are constituted in any other given series.

Solution.—The nature of the curves to be cut gives the tangents of the same at any points of intersection; and the angles of intersection give the perpendiculars of the cutting curves; and two perpendiculars coinciding, by their last concourse, give the centre of curvature of the cutting curve at the point of any intersection. Let an absciss then be drawn in any convenient position, and let its fluxion be unity; then the position of the perpendicular will give the first fluxion of the ordinate belonging to the required curve; and the curvature of this curve will give the second fluxion of the same ordinate. And thus the problem will always be reduced to equations. Q. E. D.

Scholium.—It does not belong to this, but to another method, to reduce the equations, and separate the indeterminate quantities, absolutely if it can be done, if not, by infinite series. As this problem, however, is hardly of any use, it has therefore remained neglected and unsolved for many years, in the Acta Eruditorum. And for the same reason I shall not prosecute its solution any further.*

* These solutions seem to resemble mostly the composition of Sir I. Newton.

Some late curious Astronomical Observations communicated by the Reverend and Learned Mr. James Pound, Rector of Wansted, and R. Soc. Soc. N° 347, p. 401.

The occultation of Jupiter by the moon observed at Wansted the 14th of July in the morning, 1715.

Having after midnight carefully corrected the clock by 10 observations of the altitude of the lucida arietis, the error was found $5^m\ 13^s$ too fast, the extremes not differing above 6^s; and in the morning about 7^h, by as many altitudes of the sun, with a like agreement, the same error was found $5^m\ 14^s$, to be deducted from the times shown by the clock.

July 13° P. M. N.	Time by the clock.			Time corrected.		
The third satellite of Jupiter was hid by the moon	13^h	27^m	33^s	13^h	22^m	20^s
The first satellite was hid	13	32	35	13	27	22
The second satellite was hid	13	34	25	13	29	11
The first contact of the limbs of ♃ and ☾	13	34	54	13	29	41
Jupiter wholly hid	13	36	23	13	31	10
The third satellite emerged	14	7	25	14	2	12
The first satellite	14	12	25	14	7	12
The second satellite	14	14	38	14	9	25
The first limb of Jupiter came out	14	14	45	14	9	32
The following limb of Jupiter, or last contact	14	16	15	14	11	2
The fourth satellite emerged	14	18	49	14	13	36

Jupiter and the satellites were to the northward of the visible way of the moon's centre. This occultation was observed through a telescope, in which the focal length of the object-glass was $14\frac{1}{2}$ feet, and of the eye-glass $2\frac{3}{4}$ inches. And the aperture of the object-glass was $1\frac{1}{10}$ inch. I could perceive no colours on Jupiter's limb, either at his immersion or emersion, when the axis of the tube was directed to him.

An eclipse of the moon observed at Wansted Oct. 30, 1715.

At $15^h\ 9^m$ the eclipse had been for some time begun.

At $17^h\ 39^m$ the eclipse was thought to be ended; and was visibly so at $17^h\ 41^m$; but by comparing the last observations of the chords between the horns, it follows that the true end of the eclipse was at $17^h\ 38^m\ 20^s$. At $17^h\ 43^m$ the moon's diameter was $33'\ 40''$.

The middle cannot be supposed to be very accurately determined by these observations, which were not sufficiently distant from the time of the greatest obscuration. However by comparing several of them together, the middle will be obtained, viz. $16^h\ 15^m\ 47^s$. The digits eclipsed were $8\frac{1}{4}$.

The times by the clock were 17^{m} 45^{s} sooner than the apparent time, as was found by the observations of Cor Leonis and Arcturus.

The latitude of Wansted is 51° 34′. Its longitude is 8^{s} in time eastward from the Observatory at Greenwich.

The Account given of this eclipse by the reverend Mr. William Derham, who observed it at Upminster, is agreeable to this, as far as clouds would permit him to observe.

An Account of the late surprising Appearance of the Lights seen in the Air, on the 6th of March last; with an Attempt to explain their Principal Phænomena. By Edmund Halley, J. V. D. Savilian Professor of Geom. Oxon, and Reg. Soc. Secr. N° 347, p. 406.

On Tuesday, March 6, O. S. 1716, the afternoon having been very serene and calm, and somewhat warmer than ordinary, about 7 o'clock, out of what seemed a dusky cloud, in the N. E. parts of the heaven, and scarcely 10° high, the edges of which were tinged with a reddish yellow, as if the moon had been hid behind it, there arose very long luminous rays or streaks, perpendicular to the horizon, some of which seemed nearly to ascend to the zenith. Presently after, that reddish cloud was swiftly propagated along the northern horizon, into the N. W. and still farther westerly; and immediately sent forth its rays after the same manner from all parts, now here, now there, observing no rule or order in their rising. Many of these rays seeming to concur near the zenith, formed there a corona, or image, which drew the attention of all spectators, who according to their several conceptions made very differing resemblances of it; but by which compared together, those that saw it not, may well comprehend after what manner it appeared. All agree that this spectrum lasted only a few minutes, and showed itself variously tinged with colours, yellow, red, and a dusky green; nor did it keep in the same place; for when first it began to appear it was seen a little to the northward of the zenith, but gradually declining toward the south, the long striæ of light, which arose from all parts of the northern semicircle of the horizon, seemed to meet together, not much above the head of Castor or the northern Twin, and there soon disappeared.

After the first impetus of this ascending vapour was over, the corona we have been describing appeared no more; but still, without any order as to time or place, or size, luminous radii like the former continued to arise perpendicularly. Nor did they proceed as at first, out of a cloud, but oftener would emerge at once out of the pure sky, which was at that time more than ordinary serene and still. Nor were they all of the same form. Most of them seemed to end in a point upwards, like erect cones; others like truncated cones or cylinders, so much resembled the long tails of comets, that at first sight they might well be taken for such. Again, some of these rays would continue visible for several minutes; when others, and those the much greater part, just showed themselves, and died away. Some seemed to have little motion, and to stand as it were fixed among the stars, while others with a very perceptible translation moved from east to west under the pole, contrary to the motion of the heavens; by which means they would sometimes seem to run together, and at other times to fly one another, affording a surprising spectacle to the beholders.

After this sight had continued about an hour and a half, the beams began to rise much fewer in number, and not near so high, and gradually that diffused light, which had illustrated the northern parts of the hemisphere, seemed to subside, and settling on the horizon formed the resemblance of a very bright crepusculum. On the first information of the thing, I immediately ran to the windows, which happened to look to the south and south west quarter; and soon perceived, that though the sky was very clear, yet it was tinged with a strange sort of light; so that the smaller stars were scarcely to be seen, and much as it is when the moon of 4 days old appears after twilight. We perceived a very thin vapour pass before us, which arose from the precise east part of the horizon, ascending obliquely, so as to leave the zenith about 15 or 20 degrees to the northward. But the swiftness with which it proceeded was scarcely to be believed, seeming not inferior to that of lightning; and exhibiting as it passed on a sort of momentaneous nubecula, which discovered itself by a very diluted and faint whiteness; and was no sooner formed, but before the eye could well take it, it was gone, and left no signs behind it. Nor was this a single appearance; but for several minutes, about 6 or 7 times in a minute, it was again and again repeated; these waves of vapour, if I may so call it, regularly succeeding one another, and nearly at equal intervals; all of them in their ascent producing a like transient nubecula.

By this particular we were first assured, that the vapour we saw, whatever it was, became conspicuous by its own proper light, without help of the sun's beams; for these nubecula did not discover themselves in any other part of their passage, but only between the south east, and south, where being opposite to the sun they were deepest immersed in the cone of the earth's shadow, nor were they visible before or after. Whereas the contrary must have happened, had they borrowed their light from the sun.

A little after 10 o'clock, we found, on the western side, viz. between the W. and N. W. the representation of a very bright twilight, contiguous to the horizon; out of which there arose very long beams of light, not exactly erect toward the vertex, but something declining to the south, which ascending by a quick and undulating motion to a considerable height, vanished in a little time, while others, though at uncertain intervals, supplied their place. But at the same time, through all the rest of the northern horizon, viz. from the north west to the true east, there did not appear any sign of light to arise from, or join to, the horizon; but on the contrary, what appeared to be an exceedingly black and dismal cloud seemed to hang over all that part of it. Yet it was no cloud, but only the serene sky more than ordinary pure and limpid, so that the bright stars shone clearly in it, and particularly Cauda Cygni then very low in the north; the great blackness manifestly proceeding from the neighbourhood of the light which was collected above it. For the light had now put on a form quite different from all that we have hitherto described, and had fashioned itself into the shape of two laminæ or streaks, lying in a position parallel to the horizon, whose edges were but ill terminated. They extended themselves from the N. by E. to the north east, and were each about a degree broad; the undermost about 8 or 9 degrees high, and the other about 4 or 5 degrees over it; these kept their places for a long time, and made the sky so light, that I believe a man might easily have read an ordinary print by it.

While we stood astonished at this surprising sight, and expecting what was further to come, the northern end of the upper lamina by degrees bent downwards, and at length closed with the end of the other that was under it, so as to shut up on the north side an intermediate space, which still continued open to the east. Not long after this, in the said included space, we saw a great number of small columns or whitish streaks to appear suddenly, erect to the horizon, and reaching from the one lamina to the other; which instantly disappearing were too quick for the eye, so that we could not judge whether they arose from the under or fell from the upper, but by their sudden alterations they made such an appearance as might well be taken to resemble the conflicts of men in battle.

And much about the same time, to increase our wonder, there began on a sudden to appear, low under the pole and very near due north, three or four lucid areas like clouds, discovering themselves, in the pure but very black sky, by their yellowish light. These, as they broke out at once, so after they had continued a few minutes, disappeared as quick as if a curtain had been drawn over them. They were of no determined figure, but both in shape and size might properly be compared to small clouds illuminated by the full moon, but brighter.

Not long after this, from above the aforesaid two laminæ, there arose a very great pyramidal figure, like a spear, sharp at the top, whose sides were inclined to each other with an angle of about 4 or 5 degrees, and which seemed to reach up to the zenith, or beyond it. This was carried with an equable and not very slow motion, from the N. E. where it arose, into the N. W. where it disappeared, still keeping a perpendicular situation, or very near it; and passing successively over all the stars of the Little Bear, did not efface the smaller ones in the tail, which are but of the 5th magnitude; such was the extreme rarity and perspicuity of the matter it consisted of.

This single beam was so far remarkable above all those that for a great while before had preceded it, or that followed it, that if its situation among the circumpolar stars had at the same instant been accurately noted, for example, at London and Oxford, whose difference of longitude is well known, we might be enabled with some certainty to pronounce, by its diversitas aspectûs, concerning its distance and height; which were doubtless very great, though as yet we can nowise determine them. But as this phenomenon found all those that are skilled in the observation of the heavens unprepared, and unacquainted with what was to be expected; so it left them all surprised and astonished at its novelty. When therefore for the future any such thing shall happen, all those that are curious in astronomical matters, are hereby admonished and entreated to set their clocks to the apparent time at London, for example, by allowing so many minutes as is the difference of meridians, and then to note at the end of every half hour precisely, the exact situation of what at that time appears remarkable in the sky, and particularly the azimuths of those very tall pyramids so eminent above the rest, and therefore likely to be seen farthest; that by comparing those observations taken at the same moment in distant places, the difference of their azimuths may serve to determine how far those pyramids are from us.

It being now past 11 o'clock, and nothing new offering itself to our view, but repeated phases of the same spectacle, we thought it no longer worth while to bear the chill of the night air sub dio. Therefore returning to my house, I made haste to my upper windows, which conveniently enough looks to the N. E. parts of heaven, and soon found that the two laminæ or streaks parallel to the horizon, of which we have been speaking, had now wholly disappeared: and the whole spectacle reduced itself to the resemblance of a very bright crepusculum, settling on the northern horizon, so as to be brightest and highest under the pole itself; from whence it spread both ways, into the N. E. and N. W. Under this, in the middle of it, there appeared a very black space, as it were the segment of a lesser circle of the sphere cut off by the horizon. It seemed to the eye like a dark cloud, but was not so; for by the telescope the small stars appeared through it more clearly than usual, considering how low they were; and on this as a basis our lumen auroriforme rested, which was a segment of a ring or zone of the sphere, intercepted between two parallel lesser circles, cut off likewise by the horizon; or, if you please, the segment of a very broad iris, but of one uniform colour, viz. a flame-colour inclining to yellow, its centre being about 40 degrees below the horizon. And above this there were seen some rudiments of a much larger segment, with an interval of dark sky between; but this was so exceedingly faint and uncertain, that I could make no proper estimate of it.

I was very desirous to have seen how this phenomenon would end, and attended it till near three in the morning, and the rising of the moon; but for above two hours together it had no manner of change in its appearance, nor diminution nor increase of light: only sometimes for very short intervals, as if new fuel had been cast on a fire, the light seemed to undulate and sparkle, not unlike the rising of vapourous smoke out of a great blaze when agitated. But one thing I assured myself of by this attendance and watching, viz. that this iris-like figure by no means owed its origin to the sun's beams; for that about three in the morning, the sun being in the middle between the north and east, our aurora had not followed him, but ended in that very point where he then was; whereas in the true north, which the sun had long passed, the light remained unchanged, and in its full lustre.

Hitherto I have endeavoured by words to represent what I saw, but being sensible how insufficient such a verbal description of a thing so extraordinary and unknown may be to most readers, I have thought fit to annex a figure, exhibiting that particular appearance of the two laminæ, which I saw at London between the hours of 10 and 11; more especially, because I do not find, among the many accounts I have seen, any one that has taken notice of it. In this figure, fig. 1, pl. 5, AB is the under lamina, somewhat broader and brighter than the upper CD: it had near its under edge the lucida lyræ, and below its northern extremity, on the left hand, cauda cygni: and as well above and below these as in the intermediate space between them, and indeed all round about that part of the heavens, the sky was so unusually dark and black, as if all that exotic light that had showed itself before, had been then collected into those two streaks. Only at Q, between the west and northwest, and no where else, out of a brightness adjoining to the horizon, there arose conical beams, as M, L, N, after the same manner as at first.

While we stood looking on, the streak CD at its northern end bent downward, and joined with the under AB at E, and included the space DCEAB, which still kept open at the other end towards the east. And in the mean time, out of the very clear sky, some luminous spots, situated and figured as in the scheme at G, G, G, G, presented themselves to the eye, in colour much like the laminæ. These did not show themselves all together, but came successively, yet so as two or three of them were seen at a time; and as their coming was instantaneous, so they went away in a moment. At the same time likewise, the several little white columns marked F, F, F, F, occupied that part of the space between the two streaks next to E, and by their sudden and very irregular motion, and the vanishing of some, while others at the same time emerged, gave occasion to the conception of those that fancied battles fought in the air. Lastly, from about the middle of CD, there arose suddenly a cone or obelisk of a pale whitish light, greater than any we had yet seen, as H; which moving from east to west, with a motion sufficiently regular, was translated to K, in the north west, and there disappeared.

That we might by the same scheme show the appearance of the last hours, after midnight; the reader is desired to take notice that we have made the light at Q much larger than what appeared in the west about 10 o'clock; so as to represent truly that other. In this case the point Q must, by the imagination, be supposed transferred to the intersection of the horizon and meridian under the pole. And that we might the better be understood in what follows, we have made this short recapitulation as annexed to, and explicative of, the scheme, which could by no means be contrived to answer the wonderful variety this phenomenon afforded; since even the eye of no one single observer, was sufficient to follow it in the suddenness and frequency of its alterations.

Thus far I have attempted to describe what was seen, and am heartily sorry I can say no more as to the first and most surprising part thereof, which however frightful and amazing it might seem to the vulgar beholder, would have been to me a most agreeable and wished-for spectacle; for I then should have contemplated propriis oculis all the several sorts of meteors I remember to have hitherto heard or read of. This was the only one I had not as yet seen, and of which I began to despair, since it is certain it has not happened to any remarkable degree in this part of England since I was born: nor is the like recorded in the English annals since the year of our Lord 1574, that is above 140 years since, in the reign of queen Elizabeth. Then, as we are told by the historians of those times, Cambden and Stow, eye-witnesses of sufficient credit, for two nights successively, viz. on the 14th and 15th of Nov. that year, much the same wonderful phænomena were seen, with almost all the same circumstances as now.

Nor indeed, during that reign, was this so rare a sight as it has been since. For we find in a book entitled A Description of Meteors, reprinted at London in the year 1654, signed W. F. D.D. that the same thing, which the author there calls Burning Spears, was seen at London on Jan. 30, 1560; and again by the testimony of Stow, on the 7th of October 1564. And from foreign authors we learn, that in the year 1575 the same was twice repeated in Brabant, viz. on the 13th of Feb. and 28th of Sept.; and seen and described by Cornelius Gemma, professor of medicine in the university of Lovain, and son of Gemma Frisius the mathematician. In a discourse he wrote on the prodigies of those times, after several ill-boding prognostics, he thus very properly describes the cupola and corona he saw in the Chasma, as he calls it, of February. "A little after, says he, spears and new flames arising, the sky seemed to be all on a flame, from the north quite up to the zenith; and at last, the face of the sky was, for a whole hour together, changed into the uncommon form of a dice-box, the blue and white changing alternately, with no less swiftness and vertiginous motion than the sun beams do, when reflected from a mirror."

Here it is not a little remarkable, that all these four already mentioned fell exactly on the same age of the moon, viz. about two days after the change.

As to the other of September in the same year 1575, Gemma writes; "The form of the Chasma, of the 28th of Sept. following, immediately after sunset, was indeed, less dreadful, but still more confused and various; for, in it, were seen a great many bright arches, out of which gradually issued spears, cities with towers and men in battle array; after that, there were excursions of rays every way, waves of clouds and battles; mutually pursued and fled, and wheeling round in a surprising manner." From hence it is manifest that this phenomenon appeared in our neighbourhood three several times, and that with considerable intervals, within the compass of one year; though our English historians have not recorded the two latter; nor did Gemma see that of Nov. 1574, probably by reason of clouds. After this, in the year 1580, we have the authority of Michael Mœstlin, (himself a good astronomer, and still more famous for having had the honour to be the great Kepler's tutor in the sciences) in his book de Cometa 1580, that at Baknang in the country of Wirtemburg in Germany, these chasmata, as he likewise stiles them, were seen by himself no less than 7 times within the space of 12 months. The first and most considerable of these, was on the very same day of the month with ours, viz. on Sunday the 6th of March, and was attended with much the same circumstances. And again, the same things were seen in a very extraordinary manner on the 9th of April and 10th of Sept. following: but in a less degree, on the 6th of April, 21st of Sept. 26th of Dec. and 16th of Feb. 1581: the last of which, and that of the 21st of Sept. must needs have been more considerable than they then appeared, because the moon being near the full, necessarily effaced all the fainter lights. Of all these however no one is mentioned in our annals to have been seen in England, nor in any other place that I can find; such was the neglect of curious matters in those days.

The next in order that we hear of, was that of the year 1621, on Sept. the 2d. O. S. seen all over France, and is well described by Gassendus in his physics, who gives it the name of aurora borealis. This, though little inferior to what we lately saw, and appearing to the northwards both of Rouen and Paris, is no where said to have been observed in England, over which the light seemed to lie. And since then, for above 80 years, we have no account of any such sight, either at home or abroad; though for above half that time, these Philos. Trans. have been a constant register of all such extraordinary occurrences. The first we find on our books, was one of small continuance, seen in Ireland by Mr. Neve, Nov. 10, 1707; of which see Philos. Trans. N° 320. And in the Miscellanea Berolinensia, published in 1710, we learn that in the same year 1707, both on Jan. 24, and Feb. 18, O. S. something of this kind was seen by M. Olaus Romer at Copenhagen: and again Feb. 23, the same excellent astronomer observed there such another appearance, but much more considerable; of which yet he only saw the beginning, clouds interposing. But the same was seen that night by Mr. Gotfried Kirch at Berlin, above 200 miles from Copenhagen, and lasted there till past 10 at night. To these add another small one of short duration, seen near London, a little before midnight between the 9th and 10th of August 1708, by the Right Rev. Philip, Lord Bishop of Hereford, and by his lordship communicated to the Royal Society: so that, it seems, in little more than 18 months this sort of light has been seen in the sky, no less than 5 times, in the years 1707 and 1708.

Hence we may reasonably conclude that the air, or earth, or both, are sometimes, though but seldom and at great intervals, disposed to produce this phenomenon: for though it be probable that many times, when it happens, it may not be observed, as falling out in the day time, or in cloudy weather, or bright moon-shine: yet that it should be so very often seen at some times, and so seldom at others, is what cannot well be accounted for that way. Therefore considering what might be most probably the material cause of these appearances; what first occurred was the vapour of water rarefied exceedingly by subterraneous fire, and tinged with sulphureous steams; which vapour is now generally supposed by naturalists to be the cause of earthquakes. And as earthquakes happen with great uncertainty, and have been sometimes frequent in places, where for many years before and after they have not been felt; so these, which we might be allowed to suppose produced by the eruption of the pent vapour through the pores of the earth, when it is not in sufficient quantity, nor sudden enough to shake its surface, or to open itself a passage by rending it. And as these vapours are suddenly produced by the fall of water on the nitro-sulphureous fire under ground, they might well be thought to get from thence a tincture which might dispose them to shine in the night, and a tendency contrary to that of gravity; as we find the vapours of gunpowder, when heated in vacuo, to shine in the dark, and ascend to the top of the receiver though exhausted.

Nor should I seek for any other cause than this, if in some of these instances, and particularly this whereof we treat, the appearance had not been seen over a much greater part of the earth's surface than can be thus accounted for. It having in this last been visible from the west side of Ireland to the confines of Russia and Poland on the east, nor do we yet know its limits on that side, extending over at least 30 degrees of longitude; and in latitude, from about 50 degrees over almost all the north of Europe; and in all places exhibiting at the same time the same wonderous circumstances. Now this is a space much too wide to be shaken at any one time by the greatest of earthquakes, or to be affected by the perspiration of that vapour, which being included and wanting vent, might have occasioned the earth to tremble. Nor can we this way account for that remarkable particular attending these lights, of being always seen on the north-side of the horizon, and never to the south.

Therefore laying aside all hopes of being able to explain these things by the ordinary vapours or exhalations of the earth or waters, we must have recourse to other sorts of effluvia of a much more subtle nature, and which perhaps may seem more adapted to bring about those wonderful and surprisingly quick motions we have seen. Such are the magnetical effluvia, whose atoms freely permeate the pores of the most solid bodies, meeting with no obstacle from the interposition of glass or marble, or even gold itself. Some of these, by a perpetual efflux, arise from the parts near the poles of the magnet, whilst others of the like kind of atoms, but with a contrary tendency, enter in at the same parts of the stone, through which they freely pass; and by a kind of circulation surround it on all sides, as with an atmosphere, to the distance of some diameters of the body. This thing Descartes has endeavoured to explain (Princip. Philosoph. lib. iv.) by the hypothesis of the circulation of certain screwed or striate particles, adapted to the pores they are to enter.

But without inquiring how sufficient the Cartesian hypothesis may be for answering the several phænomena of the magnet: that the fact may be the better comprehended, we shall endeavour to exhibit the manner of the circulation of the atoms concerned therein, as they are exposed to view, by placing the poles of a terrella or spherical magnet on a plane, as the globe on the horizon of a right sphere: then strewing fine steel dust, or filings, very thin on the plain all round it, the particles of steel, on a continued gentle knocking on the underside of the plain, will by degrees conform themselves to the figures in which the circulation is performed. Thus, in fig. 2, pl. 5, let ABCD be a terrella, with its poles A the south, and B the north; and by doing as prescribed, it will be found that the filings will lie in a right line perpendicular to the surface of the ball, when in the line of the magnetical axis continued. But for about 45 degrees on either side, from B to G or I, and from A to H or K, they form themselves into curves, more and more crooked as they are remoter from the poles; and more and more oblique to the surface of the stone: as the figure truly represents, and as may readily be shown by the terrella and apparatus for that purpose, in the repository of the Royal Society. Hence it may appear how this exceedingly subtle matter revolves; and particularly how it permeates the magnet with more force and in greater quantity in the circumpolar parts, entering into it on the one side, and emerging from it on the other, under the same oblique angles: while in the middle zone, about C and D, near the magnet's equator, very few if any of these particles impinge, and those very obliquely.

Now by many and very evident arguments it appears, that our globe of earth is no other than one great magnet, or, if I may be allowed to allege an invention of my own, rather two; the one including the other as the shell includes the kernel; for so and not otherwise we may explain the changes of the variation of the magnetical needle: but to our present purpose the result is the same. It suffices, that we may suppose the same sort of circulation of such an exceedingly fine matter to be perpetually performed in the earth, as we observe in the terella; which subtle matter freely pervading the pores of the earth, and entering into it near its southern pole, may pass out again into the ether, at the same distance from the northern, and with a like force; its direction being still more and more oblique, as the distance from the poles is greater. To this we beg leave to suppose, that this subtle matter, no otherwise discovering itself but by its effects on the magnetic needle, wholly imperceptible, and at other times invisible, may now and then, by the concourse of several causes very rarely coincident, and to us as yet unknown, be capable of producing a small degree of light; perhaps from the greater density of the matter, or the greater velocity of its motion: after the same manner as we see the effluvia of electric bodies, by a strong and quick friction, emit light in the dark: to which sort of light this seems to have a great affinity.

This being allowed, I think we may readily assign a cause for several of the strange appearances we have been treating of, and for some of the most difficult to account for otherwise; as, why these lights are rarely seen any where else but in the north, and never, that we hear of, near the equator: as also why they are more frequently seen in Iceland and Greenland, than in Norway, though nearer the pole of the world. For the magnetical poles, in this age, are to the westward of our meridian, and more so of that of Norway, and not far from Greenland; as appears by the variation of the needle this year observed, full 12 degrees at London to the west.

The erect position of the luminous beams or striæ so often repeated that night, was occasioned by the rising of the vapour or lucid matter nearly perpendicular to the earth's surface. For any line erected perpendicularly on the surface of the globe, will appear erect to the horizon of an eye placed any where in the same spherical superficies; as Euclid demonstrates in a plane, that any line erected at right angles to it, will appear to be perpendicular to that plane from any point of it. That it should be so in the sphere is a very pretty proposition, not very obvious, but demonstrated from Prop. 5, Lib. i, Theodosii Sphæric. For by it all lines erected on the surface pass through the centre, where meeting with those from the eye, they form the planes of vertical circles to it. And by the converse hereof it is evident, that this luminous matter arose nearly perpendicular to the earth's surface, because it appeared in this erect position. And whereas in this appearance (and perhaps in all others of the kind) those beams which arose near the east and west, as L, M, N, were farthest from the perpendicular, on both sides inclining towards the south, while those in the north were directly upright: the cause of which may well be explained by the obliquity of the magnetical curves, making still obtuser angles with the meridians of the terrella, as they are farther from its poles.

Hence also it is manifest how that wonderful corona, that was seen to the southward of the vertex, in the beginning of the night, and so very remarkable for its tremulous and vibrating light, was produced; viz. by the concourse of many of those beams rising very high out of the circumjacent regions, and meeting near the zenith: their effluvia mixing and interfering with one another, and so occasioning a much stronger but uncertain wavering light. And since it is agreed by all our accounts that this corona was tinged with various colours, it is more than probable that these vapours were carried up to such a height, as to emerge out of the shadow of the earth, and to be illuminated by the direct beams of the sun: whence it might come to pass that this first corona was seen coloured, and much brighter, than what appeared afterwards in some places, where the sight of it was more than once repeated, after the sun was gone down much lower under the horizon. Hence also it will be easily understood, that this corona was not one and the same in all places, but was different in every differing horizon; exactly after the same manner as the rainbow, seen in the same cloud, is not the same bow, but different to every several eye.

Nor is it to be doubted, but the pyramidical figure of these ascending beams is optical: since probably they are parallel-sided, or rather tapering the other way. But by the rules of perspective, their sides ought to converge to a point, as we see in pictures the parallel borders of straight walks, and all other lines parallel to the axis of vision, meet as in a centre. Therefore those rays which rose highest above the earth, and were nearest the eye, seemed to terminate in cusps sufficiently acute, and have been for that reason supposed by the vulgar to represent spears. Others seen from afar, and perhaps not rising so high as the former, would terminate as if cut off with plains parallel to the horizon, like truncated cones or cylinders: these have been taken to look like the battlements and towers on the walls of cities fortified after the ancient manner. While others yet further off, by reason of their great distance, good part of them being intercepted by the interposition of the convexity of the earth, would only show their pointed tops, and because of their shortness have been called swords.

Next, the motion of these beams furnishes us with a new, and most evident argument, to prove the diurnal rotation of the earth: though that be a matter which, at present, is generally taken by the learned to be past dispute. For those beams which rose up to a point, and did not presently disappear, but continued for some time, had most of them a sensible motion from east to west, contrary to that of the heavens; the largest and tallest of them, as being nearest, swiftest; and the more remote and shorter, slower. By which means, the one overtaking the other, they would sometimes seem to meet and jostle; and at other times to separate, and fly one another. But this motion was only optical, and occasioned by the eye of the spectator being carried away with the earth into the east; while the exceedingly rare vapour which those beams consisted of, being raised far above the atmosphere, was either wholly left behind, or else followed with but part of its velocity, and therefore could not but seem to recede and move the contrary way. And after the same manner as the stars that go near the zenith, pass over those vertical circles which border on the meridian, much swifter than those stars which are more distant from it; so these luminous rays would seem to recede faster from east to west, as their bases were nearer the eye of the spectator; and è contra, slower as they were further off.

Nor are we to think it strange, if after so great a quantity of luminous vapour had been carried up into the ether, out of the pores of the earth, the cause of its effervescence at length abating, or perhaps the matter consumed, these effluvia should at length subside, and form those two bright laminæ which we have described, and, whose edges being turned to us, were capable to emit so much light that we might read by them. I choose to call them laminæ, because, though they were but thin, doubtless they spread horizontally over a large tract of the earth's surface. And while this luminous matter dropped down from the upper plate to the under, the many little white columns were formed between them by its descent, only visible for the moment of their fall. These by the swiftness with which they vanished, and their great number, showing themselves and disappearing without any order, exhibited a very odd appearance; those on the right seeming sometimes to drive and push those on the left, and vice versa.

These are the principal phænomena; of whose causes I should have more willingly and with more certainty given my thoughts, if I had had the good luck to have seen the whole from beginning to end; and to have added my own remarks to the relations of others: and especially if we could by any means have come at their distances. If it shall by any be thought a bold supposition, that I assume the effluvia of the magnetical matter for this purpose, which in certain cases may themselves become luminous, or rather may sometimes carry with them out of the bowels of the earth a sort of atoms proper to produce light in the ether: I answer, that we are not as yet informed of any other kinds of effluvia of terrestrial matter which may serve for our purpose, than those we have here considered, viz. the magnetical atoms, and those of water highly rarefied into vapour. Nor do we find any thing like it in what we see of the celestial bodies, unless it be the effluvia projected out of the bodies of comets to a vast height, and which seem by a vis centrifuga to fly with an incredible swiftness, the centres both of the sun and comet, and to go off into tails of a scarcely conceivable length. What may be the constitution of these cometical vapours, we the inhabitants of the earth can know but little, and only that they are evidently excited by the heat of the sun; whereas this meteor, if I may so call it, is seldom seen except in the polar regions of the world, and that most commonly in the winter months. But whatever may be the cause of it, if this be not, I have followed the old axiom of the schools, Entia non esse temere neque absque necessitate multiplicanda.

Lastly, I beg leave on this occasion to mention what, near 25 years since, I published in N° 195 of these Transactions, viz. That supposing the earth to be concave, with a less globe included, in order to make that inner globe capable of being inhabited, there might not improbably be contained some luminous medium between the balls, so as to make a perpetual day below. That very great tracts of the etherial space are occupied by such a shining medium, is evident from the instances given in the first paper of this Transaction: and if such a medium should be thus inclosed within us; why may we not be allowed to suppose that some parts of this lucid substance may, on very rare and extraordinary occasions, transude through and penetrate the cortex of our earth, and being got loose may afford the matter of which this our meteor consists. This seems favoured by one considerable circumstance, viz. that the earth, because of its diurnal rotation, being necessarily of the figure of a flat spheroid, the thickness of the cortex, in the polar parts of the globe, is considerably less, than towards the equator; and therefore more likely to give passage to these vapours; whence a reason may be given why these lights are always seen in the north. But I desire to lay no more stress on this conceit than it will bear.

It having been noted that in the years 1575 and 1580, when this appearance was frequent, that it was seen not far from the times of the two equinoxes; it may be worth while for the curious to bestow some attention on the heavens in the months of September and October next; and in case it should again happen, to endeavour to observe, by the method I have here laid down, what may determine, with some degree of exactness, its distance and height; without which we can scarcely come to any just conclusion.

A Description of the Phenomenon of March 6, 1716, *as it was seen on the Ocean, near the Coast of Spain. With an Account of the Return of the same Sort of Appearance, on March* 31, *and April* 1 *and* 2, 1717. N° 348, p. 430.*

In our last, we endeavoured to give the public as good an account of the late surprising meteor, seen in the heavens, on the 6th of March last, as could be gathered from the several relations of very distant spectators. And since then, we can only add, that at Paris, the light was so inconsiderable, that it was not regarded: but by a letter, dated on board a ship in Nevis road, in America, April 19, 1716, is the following psssage. " On the 6th of March, at 9 o'clock in the evening, we being then in the lat. of 45° 36', off of the N. W. coast of Spain, a clear cloud appeared east of us, not far distant from our zenith, which afterwards darted itself forth into a number of rays of light, every way like the tail of a comet, of such a great length, that it reached

* Probably by Dr. Halley.

within a short way of the horizon. There likewise appeared a body of light, N. N. E. of us, and continued as light almost as day, till after 12 o'clock. It appeared at a good distance from us, and darkened on a sudden." Hence it would seem, that the vapour which caused this appearance, arose indifferently out of the ocean, as well as from the land; by which we may conclude the great subtlety of the matter thereof, since it could permeate so great a quantity of water, and yet retain its velocity; which is a circumstance deserving the further consideration of the curious.

But since this, most of the same phænomena have been repeated three several nights successively, viz. on the last of March, and 1st and 2d of April. The best and fullest description of the first two, is given by Dr. Brook Taylor, LL. D. and secretary to the Royal Society, dated April 2, from Cotterstock, near Oundle in Northamptonshire, who thus describes them:—" On Saturday night last, and last night, I saw appearances of the same kind with those of March 6, but not to compare for extent and strength to the other. They both began soon after sun-set, and continued till after 12, but how much longer I cannot tell. They were both about 10 or 15 degrees to the westward of the north, and took up about 80 degrees of the horizon; and the aurora rose about 30° high, with a dark bottom, like what was seen in the first; and from whence there sprung out several bodies of light, which immediately ran into streams, ascending about 30, or at most 40° high. There was no flashing nor waving light, but in all other respects these lights were of the same kind with what we saw at London. Indeed in that last night, there was one phenomenon like the flashing light, for a body of light about 15 or 20 degrees long, parallel to the horizon, rose till it came about 6 degrees above the black basis, and then sent up two strong streams of light about 40° high, which at top dashed against one another, and disappeared."

At London, the first night, March 31, it did not begin to radiate, till towards midnight, and was seen only by few curious persons; the beams not rising very high, and scarcely appearing over the houses, were but little noticed: but by the accounts of those that saw it, it was much more considerable than the next night following Easter-day, for it then sent out but few and very short beams, mostly terminating in a sharp point, and presently disappearing. Only beginning to stream as soon as it became dusky, it was very observable that those rays which rose out of the west end of the luminous arc, next the sun, were enlightened by its beams, and showed themselves much brighter than those which sprung up under the pole, or to the eastward of it. And after 9, till midnight, no more beams arose;

and the luminous arc, with its black basis, settled down very low in the northern horizon.

The same two nights, by the observation of Mr. Wm. Lingen, the like appearance was seen at Dublin, about the hours of 9 or 10; at which time, in the former night, it was near as light as in a moon-light night. And from France we have an account, that both those nights, the same was seen at Paris, with much the same circumstances as at Dublin. So that it seems this meteor, though no ways comparable to that of the 6th of March, was seen not less than 150 leagues, and probably much farther.

The following night, April 2, when it began to be dark, a luminous arc appeared in the north, with a very narrow black bottom under it, very low, and depressed to the horizon; nor was it seen at, or about London, to project any pointed rays as the former.

But what was most remarkable, that evening, what was seen at London, by that ingenious gentleman Martin Folks, Esq. R. S. S. about 9 that night. He being in the open air at that time, saw in an instant, a bright ray of very white light, appear in the east, out of the pure sky, then very serene and still: it very much resembled the tail of a comet, and was about 20° inclined from the perpendicular to the right, beginning about γ of Bayer in the corona borea, and terminating about the informis by some called Cor Caroli. This having appeared only a very short time, disappeared at once, as in a moment. When on a sudden, such another beam was instantly produced, not exactly in the same place, but in the same situation. Its lower end being about 20° high, was terminated exactly between κ and γ, in the right hand and arm of Hercules, and the middle of it passed over σ and ϱ in the girdle of Bootes, and thence proceeded westwards, leaving Cor Caroli 4 or 5 degrees to the northwards. After it had continued in this situation near 10 minutes immoveable among the stars, it began to move slowly towards the north: and the lower end passing over the northern edge of the crown, and the ray itself over Cor Caroli, it grew fainter, and vanished, having continued in all about 20 minutes. This latter, with some interruptions, was extended between Castor and Pollux, very far into the west. And about that time, the same, or such another beam, was seen at St. Asaph, by Dr. Stanley, the Rev. Dean of that church.

An Account of some Experiments on Light and Colours, formerly made by Sir Isaac Newton, and mentioned in his Optics, lately repeated before the Royal Society by J. T. Desaguliers, F.R.S.* N° 348, p. 433.

The manner of separating the primitive colours of light to such a degree, that if any one of the separated lights be taken apart, its colour shall be found unchangeable, was not published before Sir Isaac Newton's Optics came abroad. For want of knowing how this was to be done, some gentlemen of the English College at Leige, and M. Mariotte in France, and some others, took those for primitive colours, which are made by admitting a beam of the sun's light into a dark room through a small round hole, and refracting the beam by a triangular prism of glass placed at the hole. And by trying the experiment in this manner, they found that the colours thus made were capable of change,

* John Theophilus Desaguliers, a very ingenious and respectable philosopher, was born in 1683, at Rochelle, from whence he was brought by his father, who was a minister, while an infant, on the revocation of the edict of Nantes. He studied at Christ's College, Oxford, where he proceeded to the degree of LL.D. and where, in 1702, he succeeded Dr. John Keill in reading lectures on experimental philosophy at Hart Hall. In 1712 he married and settled in London, where he was the first who introduced the reading of public lectures on such subjects in the metropolis, which he continued during the rest of his life, with the greatest applause, having the honour several times of reading his lectures before the king and royal family. So that it is said he went through no less than 150 courses of those lectures in London, besides some in Holland. In 1714 he was chosen F.R.S. of which he proved a very useful and valuable member. He entered into priest's orders in 1717, and afterwards enjoyed two livings; he was first chaplain to the Duke of Chandos, and afterwards to Frederick, Prince of Wales. But natural and experimental philosophy was his chief occupation and pursuit; so that in the latter part of his life, he removed to lodgings over the Great Piazza in Covent Garden, where he carried on his lectures with great success till the time of his death in 1749, at 66 years of age.

Dr. Desaguliers was a member of several foreign academies, and corresponding member of that of France; from whence he obtained the prize proposed there, for the best account of electricity. And he communicated a multitude of curious and valuable papers to the Royal Society, between the years 1714 and 1743, or from vol. 29 to vol. 42. Besides those numerous communications, he published several excellent works of his own, as well as some translations of other useful books; particularly, his large course of Experimental Philosophy, 1734, in 2 large vols. 4to. being the substance of his public lectures, and abounding with descriptions of the most useful machines and philosophical instruments. Also his System of Experimental Philosophy, proved by mechanics, in 4to. 1719; his Physico-mechanical Experiments, in 8vo. 1717; his translation of Gravesande's Mathematical Elements of Natural Philosophy, in 8vo. 1720, and in 2 vol. 4to. 1747; also an edition of David Gregory's Elements of Catoptrics and Dioptrics, with an appendix on Reflecting Telescopes, in 8vo. 1735; containing some original letters that passed between Sir Isaac Newton and Mr. James Gregory, relating to those telescopes.

Dr. Desaguliers left a son, who was in the Royal Artillery, where he rose to be a colonel of one of the battalions, and had the rank of general in the army. He died in the year 1780.

and thereupon reported that the experiment did not succeed, And lately the editor of the Acta Eruditorum for October 1713, p. 447, desired that Sir Isaac Newton would remove this difficulty. "The objections, says the editor, made by learned men, both in France and England, against the theory of colours, were happily answered by the sagacious Mr. Newton, as is abundantly evident from the Philosophical Transactions, N° 84, 85, 88, 96, 97, 121, 123, 128; whence many wish he would please to give his sentiments on the difficulty started against that theory by the late ingenious M. Mariotte, who was both an indefatigable and successful inquirer into nature, in his treatise of colours, p. 207, et seq. At the distance of about 25 or 30 feet, he received on a paper, a ray admitted through a small hole into a dark room, and transmitted through a triangular prism; then he received with another prism in a very oblique position the violet colour, possessing a space upwards of three lines, and trajected through a slit of two lines; on which he observed, that some part of it was changed into a red and yellow colour; and in like manner he found, that a part of the red light was changed into a blue and violet colour. Now on admitting this transmutation, it is manifest, from the Acta Erudit. for 1706, p. 60, et seq. that Sir Isaac Newton's theory falls to the ground; M. Mariotte took the distance of 30 feet, lest any should object, that by taking a less distance, the heterogeneous rays had not been perfectly separated; but to me M. Mariotte's experiment would seem decisive, had the entire blue light been changed into another." Thus far the editor of the Acta Eruditorum. In answer to which, it is to be observed, that the red and yellow which came out of the violet, and the blue and violet which came out of the red, might proceed from the very bright light of the sky next encompassing the sun, and that several sorts of rays which come from several parts of the sun's body, are intermixed in all parts of the coloured spectrum which falls on a paper at any distance from the prism. In this manner of trial, for making the experiment succeed, the light of the bright clouds, immediately surrounding the sun, should be intercepted by an opaque skreen, placed in the open air without, at the distance of 10 or 20 feet from the hole through which the sun shines into the dark room. And in the skreen there should be a small hole for the sun to shine through. The hole may be either round or oblong, and not above $\frac{1}{8}$ or $\frac{1}{10}$ part of an inch broad; so that the screen may intercept not only the bright light of the clouds next encompassing the sun's body, but also the greatest part of the sun's light. For thereby the colours will become less mixed. The beam of light which passes through this hole, must afterwards pass through the other hole into the dark room, and the prism must be placed parallel to the oblong hole in the screen, and its refracting angle be 60° or above. In this manner the experiment may be tried with success; but the trial will be less troublesome, if it be made in such a manner as is described in the 4th prop. of the first book of Sir Isaac Newton's Optics.

Sir Isaac Newton therefore, on reading what has been cited out of the Acta Eruditorum, desired Mr. Desaguliers to try the experiment in the manner described in the said proposition; and he tried it accordingly with success before several gentlemen of the Royal Society, and afterwards before M. Monmort and others of the Royal Academy of Sciences; and still shows it to those who desire to see it. How this and other concomitant experiments were tried and succeeded, is described as follows.

Exper. I.—Having sewed together endwise two pieces of ribbon, each 4 inches long, the one blue and the other red, whose common breadth was $\frac{3}{4}$ of an inch; I caused it to be held in such manner, that the light which fell from the clouds through the window was so reflected, that the angle made by the rays of light, which came in at the middle of the window, with the plane of the ribbon produced, was equal to the angle made by a line drawn from the ribbon to my eye and the said plane of the ribbon. My eye was placed as far behind the ribbon as the window was before it, the distance from which to me was about 12 feet. Then looking through a prism at the ribbon, it appeared broken asunder in the place where the blue and red half joined. When the prism was held with the refracting angle downwards, or laid with one of its planes flat upon the nose, the blue half of the ribbon appeared to be carried down lower than the red, as at BR, in fig. 1, pl. 6; but when the refracting angle of the prism was turned upwards, as when the prism has one of its planes laid flat to the forehead, then the blue half of the ribbon was lifted up, as at $\beta\rho$.

The prism was of white glass, having every angle of 60 degrees; but when instead of it, one of a greenish sort of glass, such as object glasses of telescopes are made of, was used, having the refracting angle which I looked through of about 48 degrees; the same phenomenon was more distinct, this glass having no veins, but the red and blue were nearer to a straight line: in such manner, that if A represent the ribbon seen through the first prism, B will represent the ribbon seen through the second prism, fig. 2. If the refracting angle of the last prism had been as great as that of the first, the light being transmitted through too great a body of greenish glass, the phenomenon would not have succeeded so well.

The blue ribbon being somewhat too pale, and the red a little dull, I repeated the experiment with a screen of blue, and one of red worsted, joined together in the middle, as the ribbons were before; and, the colours of both being very intense, the experiment succeeded better with both prisms. All that were present trying the experiment found it to succeed, and that every circumstance answered to the account given in prop. 1, theor. 1, book 1, of Sir Isaac Newton's Optics, as far as the directions there given were followed. So that it appeared that the blue being carried lower than the red in the first case, and lifted higher in the second, was owing to the greater refraction of the blue ray; for though each part of the ribbon or worsted reflected all manner of rays, yet the phenomenon was very apparent; as also that the blue ribbon or worsted reflected the blue rays more copioussly than the red rays, and that the red ribbon or worsted reflected the red rays more than the blue ones, because the red of the blue half, seen through the prism, was less intense than that of the red half, and the blue or purple of the red half, seen through the prism, was less intense than that of the blue half.

N. B. If the ribbon or worsted be laid on any enlightened body, the phenomenon will not succeed so well, the colours of the body, seen through the prism, mixing with those of the ribbon or worsted. Even a black body will not do, if light falls upon it; but there must be a black cloth behind, in such manner, that no light falling on it can be reflected so as to disturb the phenomenon. And if a short-sighted person looks through the prism, a concave lens between his eye and the prism will render the phenomenon more distinct than it would otherwise be.

Exper. II.—Some days after, the sun shining, I made two holes H, h, in the window shutter s s, of a darkened room, fig. 3; through which letting the sun's beams pass, by means of two prisms A, B, one near each hole, I opened the rays coming from the sun into the two coloured spectra α, β, where the following colours were very distinct, viz. red, orange, yellow, green, blue, purple, and violet. Now the reason of their being more distinct than ordinary, was, that the prisms which I used were made of the greenish glass mentioned before; which is very free from those veins by which the colours are too much thrown into one another, by the best white prisms of the common sort.

The forementioned coloured spectra being thrown into the room, to the distance of about 20 feet from the window where the sun's light came in, I caused a piece of white paper π, $\frac{3}{4}$ inch broad, and 5 inches long, to be held within the refracted rays, at the distance of 10 feet from the windows, which produced these colours in such manner, that by turning the prisms round their axes, I could make the red ray of the spectrum, made by the one prism, fall on one half of the paper, and the purple ray of the spectrum, made by the other prism, fall on the other half; for the spectra were both vertical, the lines which terminated their long sides towards each other, just touching, as appears in fig. 3. Then at the distance of 9 feet, looking through the prism c, at the paper thus coloured, the red half appeared very much separated from the purple, the one seeming lifted up from the other; the red or the purple appearing the highest, according as the refracting angle of the prism was either held upwards or downwards. The phenomenon is much more distinct this way than any other; for the paper not only seems divided into two, when it is coloured by a red and a purple ray, but also by a red and blue, as in fig. 4, by a red and a green ray, as in fig. 5, or indeed by any two colours that are different, how near soever their places in the spectra be to each other. The halves of the paper appear, when viewed through the prism, to be farther from each other, when the paper is tinged with such colours as are farther from each other in the series of colours in the spectrum; and nearest, though still divided, when neighbouring colours fall on the paper, as yellow and green, or a light and a deep green. But the paper appears no way divided, when coloured with the red of the two spectra, as in fig. 6, if those reds are equally intense; and so of the other colours.

Exper. III.—I held a lens, of about 3 feet radius, at the distance of 6 feet from the oblong paper, on which a red and purple ray falling, made it look half red and half purple; and I projected the image of the said coloured paper at the distance of about 6 feet on the other side of the lens, on a sheet of white paper; where it was observable, that when the red half was distinctly painted on the white paper, which was known by the edges of the image being regularly terminated, then the blue half of the image was confused; but when the white paper was brought about 2 inches nearer to the lens, the image of the blue half became distinct, and that of the red half confused.

I tried the experiment with a paper coloured half red and half blue, the red with carmine, and the blue with smalt, making the candle to enlighten the paper, the room being otherwise dark; and the experiment succeeded in the same manner. The experiment thus made is the same that Sir Isaac Newton gives an account of, book 1, part 1, theor. 1, of his Optics. Only it is to be observed, that when the oblong paper is coloured with red and blue from the prisms, the focal place, where the red part of the image is distinct, is more distant from the place where the blue part of the image is distinct, than when the paper is coloured with the painter's powders, and much more vivid.

Fig. 7 shows the projection of the paper tinged with the rays; and fig. 8, the projection of it when painted: where a black thread is wrapped round the red and the blue part, that the distinctness of the image of the thread may show when the red or when the blue part of the image of the paper is most distinct.

N. B. When the candle enlightens the painted paper, set an opaque body as B, between the candle and lens; lest the image of the candle, being also projected, should disturb the experiment.

Exper. IV.—Having made a hole of $\frac{1}{4}$ inch diameter in the window-shutter of the darkened room, I suffered a sun-beam to come into the room, which I intercepted with a prism at the distance of 5 inches from the hole; and after its refraction in passing through the prism, I received it on a sheet of white paper, where it was coloured, making an oblong image of the sun, or spectrum, of about 9 inches in length, and 2 in breadth, which breadth was nearly equal to the diameter of the round image of the sun received on a paper at the same distance from the hole, which here was 18 feet. Or if the sun be too high, a looking-glass being put instead of the prism, will throw a white round spectrum on the paper, which held at the said distance of 18 feet, will have its diameter equal to the breadth of the coloured spectrum.

The colours of the spectrum, as in fig. 9, were these; red, orange, yellow, green, blue, purple and violet, though the violet was so faint in this as to be scarcely perceivable.

N. B. The axis of the prism in this, and all the other experiments hereafter mentioned, must be perpendicular to the ray that falls on it; and the plane into which the ray enters, must be held in such a position, that the angle which such a ray makes with that plane when it enters, may be equal to the angle made by the middle line of those rays which emerge after refraction, on the other side of the refracting angle of the prism, with the plane out of which they emerge. That is, $\angle BDG = \angle AEH$.

If the plane AC, on which the sun-beam falls, be turned nearer to a perpendicular to the sun-beam than before, the spectrum will be much longer: if it be more inclined to the said beam, the spectrum will be shorter; and in both cases less distinct. See the spectrum DE and the spectrum de, in fig. 10 and 11, where HH represents the hole in the window-shutter in each case; AC, ac the plane of the prism on which the rays enter; BC, bc that out of which they emerge; P, p the perpendicular, and C, c the refracting angle.

If the plane AC be still more oblique to HF, all the light will be reflected, and there will be no coloured image or spectrum made by refraction at all; as in fig. 12.

But if it be held so as to be more nearly perpendicular to the sun-beam than in fig. 10, the whole beam will indeed enter the prism; but meeting with BC the lower surface of the prism, or rather the surface of the air contiguous to it, some of the light will by the plane BC be reflected to de, passing almost perpendicularly through AB; and the rest will emerge through BC, and by refraction make the imperfect spectrum DE; as in fig. 10.

If the sun-beam enter AC perpendicularly, and in the middle of it, the light will be all reflected, as in fig. 13; some of it by the plane BC to R, and the rest by the plane AB to ϱ. But if the beam fall nearer to A (still perpendicularly) it will all be reflected by the plane AB; if nearer to B, it will be all reflected by the plane BC.

In order therefore to have the coloured spectrum as it ought to be, care must be taken that the emerging coloured light may make the same angle with the plane BC, as the emerging light does with the plane AC; that is, the angle AEH must be equal to BDG, as was said before, fig. 9; which may also be seen on the enlightened dust in the air. But the best way is to turn the prism on its axis, and at the same time look at the coloured spectrum, which will rise and fall, and become longer or shorter, as you turn the prism; and between the ascent and descent of the image, it will appear stationary: there stop the prism, and the reflection will be such as is required for all the experiments hereafter mentioned.

In order to have the prism move freely on its axis, and stop any where, I fixed each end of it into a triangular collar of tin, from the end of which came a wire, which was the axis of the prism produced; and thus I laid it on two wooden pillars, with a notch on the top to receive the wires, and fixed it to a small board, just broad enough to stand fast; as in fig. 14.

Exper. V.—I took the prism CD, and through it looked at the coloured spectrum RP, which appeared then round and white as at S, just as if it had been the sun's light received on a paper from the hole H, and seen with the naked eye. In this case the prism CD must be held in a direct line with AB, and the refracting angles in the two prisms must be equal. This spectrum appearing white but just in one point, is not so readily found; but the best way is to look through the same prism AB which makes the spectrum, which may easily be done if it be pretty long, and then RP will be seen white and round, and as at S, as if coming directly from H, in fig. 15.

Exper. VI.—I held a broad lens Ll, ground to a radius of $2\frac{1}{2}$ feet, in such a manner, that the whole coloured spectrum fell upon it; and after refraction all the colours appeared to converge, when received on a paper at pp; but when the paper was held in the focus at F, in the position $\pi F \pi$, the spectrum was round and perfectly white, by the union of all the coloured rays. When the paper was held at ΠΠ, the colours appeared to diverge from each other, but then the red was uppermost, which before used to be the lowest, and so on in an inverted order.

I tried the same experiment with a lens of one foot radius, also with one of 9 inches, and with another of 7, and the success was the same. See fig. 16, where R, O, Y, G, B, P, V, express the colours.

N. B. Care must be taken that the very end of the red, and the extremity of the violet be taken in by the lens; otherwise the spectrum will not be perfectly white at the glass's focus. There is no fixed distance of the prism from the lens; but it ought to be brought so near the prism, that the two ends of the spectrum may fall nearer the axis of the lens, than the edges of the lens; because there the refraction is not so regular. Behind the lens L, which made the colours converge into white at the distinct base, or focus F, I placed the lens l, which made the white be at f the distinct base of the two glasses combined; and the experiment succeeded as before, as in fig. 17.

When the paper was held in the focus of the lens, so as to receive the white image of the coloured spectrum projected by the lens; if with a card I intercepted the red ray, the white appeared tinged with purple; and if I intercepted the violet or purple ray, or both, the white appeared tinged with red; and if the red was intercepted at the same time, the spectrum appeared to be a mixture of yellow, green and blue. If any single colour was suffered to fall upon the lens, the rest being intercepted, that colour would continue the same; only it would be more intense in the focus of the lens.

Exper. VII.—I took a board qhs, fig. 18, which stood reclining on a prop t, having a hole of a quarter of an inch diameter at h, and behind it a prism B supported on two props, as above-mentioned, so as to turn easily about its axis; and having set this board on the ground with the prism behind it at B; by turning the prism AC about its axis, I first made the red ray of the coloured spectrum pass through the hole h, and fall obliquely on the second prism B. This ray, after its refraction in passing through the second prism, was carried up to the ceiling of the room at the place marked R: I then made the purple ray fall on the board, and pass through the hole h, as the red had done before; and after refraction through the prism B it was carried up to the ceiling at P. And the green ray being afterwards made to pass the second prism in the same manner, went up to G: and so of all the intermediate rays, which were by this second refraction thrown to the intermediate places on the ceiling between R and P.

Care is to be taken that the second prism be placed obliquely to the rays which come through the hole h, lest they be reflected, as they would be, if the board being in the position as, and the second prism in the position LNM, the ray from the first prism be ρh; for then it will be reflected upwards to σ, instead of being refracted, as fig. 19. Neither must the plane of immersion be too oblique, least the incident ray be reflected downwards by it, as the ray RH is by the prism B thrown to E, in fig. 20. Several have confessed to me that they at first used to fail in this experiment, for want of setting the second prism in a due inclination.

Though the colours by the second refraction on the ceiling appeared unchanged, when seen by the naked eye, yet if viewed through a prism, they afforded new colours, except some part of the red, and some part of the violet, which was owing to their not being fully separated; for which reason I made the following experiment, to prove, that if the colours be well separated, they are truly homogeneal and unchangeable.

N. B. When the prisms are good, and no clouds are near the sun, the extremity of the red or violet will afford unmixed colours in this experiment; otherwise not.

Exper. VIII.—Having made a hole in the window-shutter 2 inches wide (fig. 21), I applied to it a tin plate, which sliding up and down hid all this hole in the wood, and only transmitted a small beam through its own hole H, whose diameter was $= \frac{1}{16}$ inch. This beam, by means of the looking-glass L, placed on the board of the window XW, I reflected horizontally to the other end of the room. But to correct the irregularity of the reflection of the looking-glass, I made use of the frame of paste-board PP, which had a hole in it h, of $\frac{1}{16}$ inch also: and placing it at PP, I suffered some of the reflected beams to pass through it, so as to fall on the lens FE (convex on both sides, and ground to a radius of $4\frac{1}{2}$ feet) at the distance of 9 feet, so that the image of the hole h was projected to f on the other side of the glass, at the distance of 9 feet more. Just behind the lens, which by a screw in the stand s might be raised or let down, so as always to receive the beam along its axis, I placed a prism A (upright on one of its ends, and easily moveable about its axis, by reason of its wire turning freely in a hole in the solid piece of wood T, which stood on another stand behind the lens) as near as I could to the lens EF, so that the image of h, instead of being round, white, and projected to f, was cast sidewise on a white paper stretched on a frame, and appeared coloured, and 30 or 40 times its breadth, as at MN. The colours in this case were very vivid and well separated, only the violet had some pale light darting from its end, on account of some veins in the prism A, and the light not coming directly from the sun, but reflected; which ought not to have been, if the sun had been low enough to have thrown the rays a good way into the room without the help of a looking-glass.

To show that the colours in this spectrum were simple and homogeneal lights, I made the following experiments.

Exper. IX.—Having made a hole h in the paper which received the coloured spectrum, I suffered the red light to pass; which being refracted by a second prism, fell on another paper at T, where it appeared still red, whether seen with the naked eye or prisms of different refracting angles. To the eye which saw it through the prism V, it appeared indeed lower, as at t, but red, round and unchanged. I made the experiment on all the colours, which by this means appeared to be simple and homogeneal. See fig. 22, where the same letters denote the lens, prism and first paper.

Through the same lens and prism the spectrum was made to fall on a book; then through the prism F it appeared unchanged; and the letters in the book which crossed the spectrum, were as distinct as when seen with the naked eye. See fig. 23.

N. B. The axis of the prism F ought to be perpendicular to the long axis of the spectrum sm thrown on the book, which will appear at $\sigma\mu$; and the prism in the position represented at F, with its flat side towards the nose: for that is the most convenient position for looking at the spectrum in these experiments.

I suffered the purple ray only to pass through the hole h and fall on a book at P, the letters of which appeared at π, and were as distinct through the prism Q as when seen with the naked eye: and I had the same success with all the other rays. See fig. 24.

But if a sun-beam, as r, comes through the hole H directly on the book at w, an eye looking at it through a prism at X will see this beam at Y oblong and coloured, and the letters on which it falls confused. See fig. 24.

N. B. The lens ought to be very good, without veins or blebs, and ground to no less a radius than mentioned in the experiment; though a radius of a foot or two longer is not amiss. The prism ought to be of the same glass as the object-glasses of telescopes, the white glass, of which prisms are usually made, being commonly full of veins. And the room in these last experiments ought to be very dark.

A few days after, having got very good prisms, made for the purpose, of the above-mentioned glass, I made all the experiments over again before several members of the Royal Society, with better success; and had the spectrum very regularly terminated, without any pale light darting from the ends of it.

For a further account of experiments to this purpose, see Sir Isaac Newton's Optics, b. 1, part 1, to which I might have referred the reader altogether; but that I was willing to be particular in mentioning such things as ought to be avoided in making the experiments above-mentioned; some gentlemen abroad having complained that they had not found the experiments answer, for want of sufficient directions in Sir Isaac Newton's Optics; though I had no other directions than what I found there.

A plain and easy Experiment to confirm Sir Isaac Newton's Doctrine of the different Refrangibility of the Rays of Light. By the same. N° 348, p. 448.

After the experimentum crucis made by two prisms, I should not give the following experiment, but that it is so easy to be made, that by it those who want the apparatus, or are unwilling to be at the pains to make the experimentum crucis, may at any time satisfy themselves of the truth of the forementioned doctrine.

Let the candle A be set before the bar of a chimney looking-glass, such as is represented by HH, fig. 25, pl. 6, which is a piece of looking-glass plate consisting of four planes, seen in the section of it αfdβ, viz. dβ which is quicksilver behind, fα a plane parallel to it, fd one of the side-planes bevelled towards dβ, or inclined to it in an angle of about 40°, (though from 30 to 40 will do, but the greater the angle the better, if it does not exceed 45°), $\alpha\beta$ the other side-plane inclined in the same angle to βd.

The rays of the candle which come from A to γ, fall obliquely on the plane $\alpha\beta$, so that instead of going on to a, they are by refraction made to incline more towards the perpendicular pp, namely to go on in the line γc, and then are reflected from the point c on the quicksilvered surface, in the direction c$\varkappa$, so as to make the angle $\varkappa$cd = γcβ. Now as the rays which would go to $\varkappa$, if not refracted, emerge obliquely from the plane $\alpha\beta$, they leave the direction c$\varkappa$, and decline from the perpendicular $\pi\pi$, and, being differently refracted, open into four differently coloured rays; viz. bR a red ray, bYO a ray made up of orange and yellow; bGB a ray made up of green and blue or a sea-green, and bP a purple ray.

If from the place Ee you look full on the point b, the spectrum or image of the candle at b will appear double, but not mixed; that is, there will appear a sea-green spot and a red spot, as it were one upon another; but not so as to produce a mixed or intermediate colour. Then if the right eye or eye at E be shut, there will appear only a green spot to the eye at e; if the eye at e be shut, the eye at E will see only a red spot.

If you come nearer to b, so that the eyes at ε1, ε2 receive the most and the least refrangible rays, there will be a double spectrum, viz. a red and a purple one just touching, or upon one another: and the phenomenon will answer as before. (Fig. 25).

If, keeping both eyes open, you direct their axes towards o, a point nearer than the usual place of the compound spectrum s, fig. 26, which point is in a line from the nose N to the point s; or in other words, if you look full at o, or at the end of your finger held in o, the red and the blue (or purple spot) will appear to be divided from each other after the manner represented at pr, in fig. 27, where the red will appear to be on the right-hand, and the blue on the left.

To make plain what is meant by seeing the spectra p and r while we look full at o, I beg leave to explain the distinction between looking and seeing; that I may the better show how this phenomenon proves that the sensation of different colours is caused by rays differently refracted.

1*st Definition.*—The optic axis is a line which, going through the centre of the convexity of all the coats and humours of the eye, falls on the middle of the retina, as αa or Aa, fig. 28.

2*nd Definition.*—To look at any point, is to turn both eyes towards it in such manner, that the optic axes making an angle at the said point as a, the rays from a may have the optic axis for their axis, and, by their convergence upon the retina after refraction in the eye, may paint the image of the said point on the middle of the retina of each eye, where the optic axis in each eye falls.

3*rd Definition.*—To see without looking, is to direct the optic axes to some other place than to the point which is then seen; and in such a case, the image of the point seen will be projected on a part of the retina of each eye, where the optic axis does not fall, namely either nearer to the nose N, as in fig. 26, at the points of the retina marked nn; or farther from the nose than the middle of the retina, as at oo in fig. 29.

Whatever is seen, by being looked at with both eyes, always appears single, by reason of the communication between the middle of the retina in one eye, and the middle of the retina of the other: there being no such communication between any other part of the retina in one eye, and the correspondent part of the retina in the other, when these correspondent parts are equally distant from the nose.

There is indeed a communication between the nervous fibres on the right-side of the retina of one eye, and the nervous fibres on the right-side of the retina of the other eye, and so of those on the left: but no single object can be so painted in each eye, as to have its image on the right or left part of one retina, that communicates with the right or left part of the other, of the same size and at the same time, as in the other; because in whatever position the object is, it must be nearer to one eye than to the other, except it be just in a line from the nose between the two eyes straight forward.

Hence it is, that if there be two candles set before any one, the first at the distance of one foot, and the second at the distance of 2 feet, from the eyes; he that looks at the second candle at B will see it single, but see the first candle or the candle A double; one appearance being in the line ADγ, the other in OAE, because it paints itself on oo in the retina of each eye, which points are not the middle points, but farther from the nose than the middles mm. So if B be the first candle, and C the second, he that looks at B will see C double, because it is painted in the retina at the points nn nearer the nose than mm; and so will appear to be in the same position as pr, in fig. 27.

If $\gamma\varrho$ be two candles so disposed, fig. 30, that by the interposition of a perforated board FF, γ can paint itself only in the eye R, and ϱ in the eye L. On making the optic axes meet at B, and to tend towards ϱ and γ, ϱ and γ will each paint an image on the middle of the retina of each eye, by crossing their rays at B: and thus the two candles will appear to be but one, or rather to be in one place, on account of the communication of the middle of each retina. But if, instead of the candles, ϱ be a piece of red silk, and γ a piece of green silk, the same position of the eyes will make an image at B, appearing like a red and green spot together, without a mixture of the colours. If ϱ be a red hot iron, and γ a candle of sulphur, the phenomenon will be more distinct. If the optic axes be turned directly towards γ and ϱ, as if there was no board FF in the way, there will appear two holes in the board, the one having the red hot iron in it, the other the candle.

Now if, of the refracted rays of the candle in the first case, fig. 25, those which diverge from each other, so as to fall into each eye, cause the same sensations respectively, as the rays which come from a red-hot iron and those which come from a blue candle; it is evident that the candle in the first case affords red-making and blue-making rays after refraction, and that those rays are differently refrangible; the red bR, fig. 25, the least refrangible, as declining less from the perpendicular $\pi\pi$; and the purple as bP declining most from the said perpendicular.

The same will, cæteris paribus, be found true in the intermediate rays; and to be certain that the experiment is as I have related it, the planes αf and fd of the bar may be covered with paper.

An Account of what appeared on opening the big-bellied Woman, near Haman in Shropshire, who was supposed to have continued many Years with Child. Communicated by Dr. Hollings, M. D. from Shrewsbury. N° 348, p. 452.

A married woman near Haman, 3 miles from Shrewsbury, about 40 years of age, supposed herself with child; she had the usual signs, and a good midwife assured her it was so, but that the child was so large she could not be delivered without bringing it away in pieces. But not submitting to that, her pains soon went off, and she continued without any other disorders 9 months longer, when she had again the signs of labour; and the same midwife assured her as before, and she, persisting in her former resolution, her pains, after a day or 2 went off again. Soon after her belly swelled to a surprising size, and she was exhibited as a show. I saw her first above 20 years since, when her belly was almost even with her chin, and the weight of it so great, that she was obliged to support it with a stool. She could not stand without the help of a rope from the ceiling, which assisted her in changing her posture of sitting. She slept commonly with her arms folded on her belly, and her head rested between them. She had no swelling in her legs: every other part emaciated as usual in the like cases. Thus she lived without any other considerable complaint above 30 years, the most remarkable circumstance, I think, in her case. She died in May 1715, when this appeared to be an ascites.

I need not mention the state the common teguments must necessarily be in from so great a distention, which had distorted many of her ribs, and forced the diaphragm so high, that it was surprising to find her breathing could be continued so long. The water was all contained in the duplicature of the peritonæum, 13 gallons, besides a quart that was spilt: it was saltish, with some little fat upon it, and towards the latter running tinged with blood as usual. There was no water in the cavity of the abdomen, except what was contained in a kind of bladder, which lay across the fundus uteri. This was divided by a cartilaginous substance into two cavities; in one there was $1\frac{1}{2}$ pint, in the other 3 parts of a pint of water. I believe it was this that imposed on the midwife. The uterus was of the natural size, without any alteration, except that the os tincæ and collum minus were filled with a gritty substance, hard as stone, which I take to be the humour separated there, and coagulated by time. Mr. Cooper, Tab. 15, fig. 4, says he found the same parts filled with a glutinous matter, which he thinks is useful to prevent abortion; which, if vitiated, impregnation is hindered.

The liver and other parts contained in the abdomen, were forced into an exceedingly small compass, and by that pressure a little changed in shape; to which the muscles of the abdomen, distended so as to be scarcely discernible, could give but little, if any assistance.

The awe that people have here for dead bodies, though never so prejudicial to the living, would not suffer her friends to let me make any further inquiry; so that I can send no account of any other part. The same error hindered me examining another woman, who died here about a week after, of an ascites which she had had 40 years, any further than to be satisfied she had 7 gallons of water contained between the duplicatures of the peritonæum, and none in the cavity of the abdomen.

A new Method of determining the Parallax of the Sun, or his Distance from the Earth; by Dr. Halley, Sec. R. S. N° 348, p. 454. *Translated from the Latin.*

It is well known that this distance of the sun from the earth, is supposed different by different astronomers. Ptolemy and his followers, as also Copernicus and Tycho Brahe, have computed it at 1200 semi-diameters of the earth, and Kepler at almost 3500; Riccioli doubles this last distance, and Hevelius makes it only half as much. But at length it was found, on observing by the telescope, Venus and Mercury on the sun's disk, divested of their borrowed light, that the apparent diameters of the planets were much less than hitherto they had been supposed to be; and in particular, that Venus's semi-diameter, seen from the sun, only subtends the fourth part of a minute, or 15 seconds; and that Mercury's semi-diameter, at his mean distance from the sun, is seen under an angle of 10 seconds only, and Saturn's semi-diameter under the same angle; and that the semi-diameter of Jupiter, the largest of all the planets, subtends no more than the third part of a minute at the sun. Whence, by analogy, some modern astronomers conclude that the earth's semi-diameter, seen from the sun, subtends a mean angle, between the greater of Jupiter and the less of Saturn and Mercury, and equal to that of Venus, viz. one of 15 seconds; and consequently, that the distance of the sun from the earth is almost 14,000 semi-diameters of the latter. Another consideration has made these authors enlarge this distance a little more: for since the moon's diameter is rather more than a quarter of the earth's diameter, if the sun's parallax be supposed 15 seconds, the body of the moon would be larger than that of Mercury, viz. a secondary planet larger than a primary one, which seems repugnant to the regular proportion and symmetry of the mundane system. On the contrary, it seems hardly consistent with the same proportion, that Venus, an inferior planet, and without any satellite, should be larger than our earth, a superior planet, and accompanied with so remarkable a satellite. Therefore, at a mean, supposing the earth's semi-diameter, seen from the sun, or which is the same thing, the sun's horizontal parallax, to be 12 seconds and a half, the moon will be less than Mercury, and the earth larger than Venus, and the sun's distance from the earth come out nearly 16500 semi-diameters of the earth. I shall admit of this distance at present, till its precise quantity be made to appear more certain by the trial I propose; not regarding the authority of such as set the sun at an immensely greater distance, relying on the observations of a vibrating pendulum, which do not seem accurate enough to determine such minute angles; at least, such as use this method will find the parallax sometimes none at all, and sometimes even negative; that is, the distance will become either infinite, or more than infinite, which is absurd. And it is scarcely possible for any one certainly to determine, by means of instruments, however nice, single seconds, or even 10 seconds; and therefore, it is not at all surprising, that the exceeding minuteness of such angles has hitherto baffled the many and ingenious attempts of artists.

While I was making my observations in the island of St. Helena, about 40 years since, on the stars round the south pole, I happened to observe, with the utmost care, Mercury passing over the sun's disk: and contrary to expectation, I very accurately obtained, with a good 24-foot telescope, the very moment in which Mercury, entering the sun's limb, seemed to touch it internally, as also that of his going off; forming an angle of internal contact. Hence I discovered the precise quantity of time the whole body of Mercury had then appeared within the sun's disk, and that without an error of one single second of time; for, the thread of solar light, intercepted between the obscure limb of the planet, and the bright limb of the sun, though exceedingly slender, affected my sight, and in the twinkling of an eye, both the indenture made on the sun's limb by Mercury entering into it, vanished, and that made by his going off, appeared. On observing this I immediately concluded, that the sun's parallax might be duly determined by such observations, if Mercury, being nearer the earth, had a greater parallax, when seen from the sun; for, this difference of parallaxes is so very inconsiderable, as to be always less than the sun's parallax, which is sought; consequently, though Mercury is to be frequently seen within the sun's disk; he will scarcely be fit for the present purpose.

There remains therefore Venus's transit over the sun's disk, whose parallax, being almost 4 times greater than that of the sun, will cause very sensible differences between the times in which Venus shall seem to pass over the sun's disk in different parts of our earth. From these differences, duly observed, the sun's parallax may be determined, even to a small part of a second of time;

and that without any other instruments than telescopes and good common clocks, and without any other qualifications in the observer than fidelity and diligence, with a little skill in astronomy. For we need not be scrupulous in finding the latitude of the place, or in accurately determining the hours with respect to the meridian; it is sufficient, if the times be reckoned by clocks, truly corrected according to the revolutions of the heavens, from the total ingress of Venus on the sun's disk, to the beginning of her egress from it, when her opaque globe begins to touch the bright limb of the sun; which times, as I found by experience, may be observed even to a single second of time.

But by the limited laws of motion, Venus is very rarely seen within the sun's disk; and for a series of 120 years, and upwards, is not to be seen there once; viz. from 1639, when Mr. Horrox was favoured with this agreeable sight, and he the first and only one since the creation of the world, down to 1761; at which time, according to the theories hitherto observed in the heavens, Venus will pass over the sun on May 26 in the morning; so that (vide Phil. Trans. N° 193) at London, nearly at 6 o'clock in the morning, she is to be in the middle of the sun's disk, and but 4 minutes more southerly than his centre. The duration of this transit will be almost 8 hours; viz. from 2 till near 10 o'clock in the morning. Consequently her ingress will not be visible in England: but the sun at that time being in 16° of Gemini, and almost in 23° of north declination, will be seen not to set throughout the whole northern frigid zone; and consequently the inhabitants of the coast of Norway, as far as its northern promontory, beyond the town of Drontheim, may observe Venus entering the sun's disk; and perhaps this ingress into the sun at his rising may be seen by the inhabitants of the north of Scotland and those of Zetland. But when Venus is nearest the sun's centre, he will be vertical to the northern coasts of the gulph of Ganga, or rather of the kingdom of Pegu; and consequently, in the neighbouring countries, when the sun shall, at the ingress of Venus, be almost 4 hours distant to the east, and almost as many to the west at her egress, her apparent motion within the sun's disk will be accelerated almost twice as much as in the horizontal parallax of Venus from the sun; because Venus at that time moves retrograde from east to west; while in the mean time an eye, on the surface of the earth, is carried the contrary way, from west to east.

Supposing the sun's parallax, as was said, to be 12 seconds and a half, Venus's parallax will be 43 seconds; and subtracting the sun's parallax, there will remain half a minute at least for the horizontal parallax of Venus from the sun, and consequently, Venus's motion will be accelerated $\frac{3}{4}$ of a minute at least by that parallax, while she passes over the sun's disk, in such elevations of the pole as are near the Tropic; and still more so near the equator. For Venus will at that time accurately enough describe within the sun's disk 4 minutes an hour; and consequently, at least 11 minutes of time (by which the duration of this eclipse of Venus will be contracted by reason of the parallax) answer to $\frac{3}{4}$ of a minute. And by this contraction alone we might safely determine the parallax, provided the sun's diameter and Venus's latitude were very accurately given; which yet we cannot possibly bring to a calculation, in a matter of such great subtlety.

We must therefore have another observation, if possible, in places where Venus possesses the middle of the sun at midnight, viz. under the opposite meridian, that is, 6^h or $90°$ more westerly than London, and where Venus enters the sun's disk a little before his setting, and goes off a little after his rising; which will happen in the said meridian in about $56°$ of N. lat. that is, at Nelson's harbour in Hudson's Bay. For, in the neighbouring places Venus's parallax will protract the duration of the transit, and make it at least 6 minutes longer; because while the sun seems to tend under the pole from west to east, these places on the earth's surface will seem to be carried with a contrary motion towards the west, that is, with a motion conspiring with the proper motion of Venus; consequently Venus will seem to move slower within the sun's disk, and continue longer on it.

If therefore in both places this transit happen to be duly observed by proper persons, it is evident that the Mora will be longer by 17 entire minutes in Nelson's harbour, than in the East-Indies; nor does it matter much whether the observation be made at Fort St. George, commonly called Maderas, or at Bencoolen on the western coast of the island of Sumatra near the equator. But if the French should incline to make the observation, Pondicherry on the western coast of the gulph of Ganga, at the elevation of $12°$, will be a proper place for that purpose: and for the Dutch, their famous emporium Batavia is a fit place. And indeed I would have several observations made of the same phenomenon in different parts, both for further confirmation, and lest a single observer should happen to be disappointed by the intervention of clouds from seeing what I know not if those either of the present or following age shall ever see again; and upon which, the certain and adequate solution of the noblest, and otherwise most difficult problem depends. Therefore again and again, I recommend it to the curious strenuously to apply themselves to this observation.

By this means, the sun's parallax may be discovered, to within its five hundredth part, which will doubtless seem surprising to some: but yet, if an accurate observation be had in both the places above-mentioned, it has already been shown that the duration of these eclipses of Venus differ from each other by 17 entire minutes, on the supposition that the sun's parallax is $12\frac{1}{2}$ seconds. And if this difference be found to be greater or less by observation, the sun's parallax will be greater or less nearly in the same ratio. And since 17 minutes of time answer to $12\frac{1}{2}$ seconds of the sun's parallax; for each second of the parallax there will arise a difference of upwards of 80 seconds of time; therefore, if this difference be obtained true within 2 seconds of time, the quantity of the sun's parallax will be got to within the 40th part of one second; and consequently his distance will be determined to within its 500th part; at least if the parallax be not found less than what I have supposed it; for $40 \times 12\frac{1}{2}$ is 500.

Here I have had no regard to the planet's latitude, both to avoid the trouble of a more intricate calculation, which would render the conclusion less evident, as also on account of the motion of the nodes of Venus not being hitherto discovered, and which can only be duly determined by such conjunctions of the planet with the sun as this. For it was only on the supposition, that the plane of Venus's orbit is immoveable in the sphere of the fixed stars, and that her nodes would continue in the same places as they were in 1639, that it was concluded, that Venus would pass 4 minutes below the sun's centre. But if in 1761 she should pass more southerly, it will be evident, that there is a regression of the nodes; and if more northerly, that there is a progression of them; and that at the rate of $5\frac{1}{2}$ minutes in 100 Julian years, for each minute by which the path of Venus will at that time be more or less distant from the sun's centre than the said 4 minutes. But the difference between the durations of these eclipses will be somewhat less than 17 minutes, by reason of the southern latitude of Venus; but greater if, by the progression of the nodes, she shall pass over the sun to the north of his centre.

But for the sake of such as are not thoroughly acquainted with the doctrine of parallaxes, I shall further explain the matter both by a figure and a somewhat more accurate calculation. Therefore, supposing that at London, May 25, $17^h\ 55^m$, 1761, the sun be in $15^\circ\ 37'$ of Gemini, and consequently that at his centre the ecliptic tends towards the north in an angle of $6^\circ\ 10'$; and that the visible path of Venus within the sun's disk at that time descends towards the south, forming an angle with the ecliptic of $8^\circ\ 28'$; then the path of Venus will tend a little towards the south in respect of the equator, intersecting the parallels of declination in an angle of $2^\circ\ 18'$. Supposing likewise that Venus be near the sun's centre at the said time, and distant from it towards the south 4 minutes, describing, by a retrograde motion on the sun's disk, 4 minutes an hour. The sun's semi-diameter will be nearly $15'\ 51''$, and that of Venus $37\frac{1}{2}''$.

And supposing, for trial sake, the difference of the horizontal parallaxes of Venus and the sun to be 31″, such as it is on the supposition of the sun's parallax being $12\frac{1}{2}''$. Therefore let a small circle, as AEBD, fig. 3, pl. 5, be described from the centre C, whose semi-diameter let be 31″, representing the earth's disk, and in it drawing DabE and cde the ellipses of the parallels of 22 and 56° N. lat. in the same manner as is now used by astronomers for constructing solar eclipses: and let BCA be the meridian in which the sun is, to which let be inclined the right line FHG, representing the path of Venus, in an angle of 2° 18′, whose distance from the centre C let be 240 such parts as BC is 31; and from C let fall the right line CH perpendicular upon FG. Then supposing the planet in H at $17^h\ 55^m$, or $5^h\ 55^m$ in the morning, let the right line FHG be divided into the horary spaces III; IV, IV; V, V; VI, &c. equal to CH, that is, 4 minutes. Let the right line KL be also equal to the difference of the apparent semi-diameters of the sun and Venus, or 15′ $13\frac{1}{2}''$. Then the circle, described with the radius KL, and from any point within the small circle, representing the earth's disk as a centre, will meet the right line FG in the point denoting what o'clock it is at London, when Venus shall touch the sun's limb in an angle of internal contact, in that place of the earth's superficies that lies under the assumed point on the disk. And if a circle, described from the centre C and with the radius KL, meet FG in the points F and G, the right lines FH, HG will be = 14′ 41″, which Venus will appear to pass over in $3^h\ 40^m$. Therefore F will fall upon $2^h\ 15^m$ at London, and G upon $9^h\ 35^m$ in the morning. Whence it is evident, that if the earth's magnitude should vanish, as it were, into a point, by reason of the immense distance; or if, divested of its diurnal motion, it should always have the sun vertical to the same point C, the entire mora of this eclipse would continue for $7\frac{3}{4}$ hours. But in the mean time while the earth revolves with a contrary motion to that of Venus through 110° of long. and consequently the duration of the said mora is shorter, suppose by 12 minutes, it will be nearly $7^h\ 8^m$, or 107°.

Now in the meridian itself Venus will be near the sun's centre at the eastern mouth of the Ganges, where the elevation of the pole is about 22°. Therefore that place will be equally distant from the sun on both hands, in the moments of the planet's ingress and egress, viz. $53\frac{1}{2}°$; as the points a, b, in the greater parallel DabE. But the diameter AB will be to the distance ab, as the square of the radius to the rectangle under the sines of $53\frac{1}{2}°$ and 68°, that is, as 1′ 2″ is to 46″ 13‴; and on making a due calculation, I find that the circle described with the radius KL, from the centre a, will meet the right line FH in the point M, at $2^h\ 20^m\ 40^s$; but described from the centre b, it will meet HG in N, at $9^h\ 29^m\ 22^s$ at London; consequently, the whole body of Venus will be seen from the banks of the Ganges, within the sun's disk, for $7^h\ 8^m\ 42^s$. Therefore we have rightly supposed its duration $7^h\ 8^m$, since here a part of a minute is inconsiderable.

But adapting the calculation to Nelson's harbour, I find that Venus shall pass over the sun's disk, when he is just about to set, and emerge out of his disk immediately after his rising, that place in the mean time being carried through the hemisphere opposite to the sun from c to d, with a motion conspiring with that of Venus. Therefore the mora of Venus within the sun's disk will become longer by reason of the parallax, suppose by 4 minutes, so as entirely to be $7^h\ 24^m$ or $111°$ of the equator. And since the latitude of the place is $56°$, it will be as the square of the radius is to the rectangle under the sines of $55\frac{1}{2}°$ and $34°$, so is AB $= 1'\ 2''$, to cd $= 28''\ 33'''$. And on duly making the calculation, it will appear, that the circle, described from the centre c, with the radius KL, will meet the right line FH in o, at $2^h\ 12^m\ 45^s$; but described from the centre d, it will meet HG in P, at $9^h\ 36^m\ 37^s$. Therefore the duration of the mora at Nelson's harbour will be $7^h\ 23^m\ 52^s$, viz. greater than at the mouth of the Ganges by $15^m\ 10^s$ of time. But if Venus should pass without latitude, the said difference will become $18^m\ 40^s$; and if she shall be 4 minutes more northerly than the sun's centre, the difference will be increased to $21^m\ 40^s$, and will be still greater by increasing the planet's N. lat.

From the above hypothesis it follows, that at London Venus will rise when entered into the sun, and at $9^h\ 37^m$ in the morning in her egress touch internally the sun's limb, and quite leave his disk not before $9^h\ 56^m$.

It is evident from the same hypothesis, that Venus should touch with her centre the extreme northern limb of the sun on May 23, 11^h, 1769, so that, by reason of the parallax, her whole body may be seen in the northern parts of Norway, within the sun's disk; while on the coast of Peru and Chili she will seem to ride on the disk of the setting sun with a small segment of her body; as in like manner in the Molucca islands, and the neighbouring parts, at sunrising. But if the nodes of Venus be found to have a retrocession, as there is reason to suspect from some later observations, then her whole body being every where seen within the sun's disk, the greatest differences of these eclipses will afford a still more evident proof of the sun's parallax.*

* "The transit of Venus in 1761 proved much less favourable to the proposed purpose than Dr. Halley expected. The motion of Venus's node not being well known, she passed much nearer the sun's centre than he supposed she would; which made the places he pointed out for observing the total duration not proper for the purpose; indeed the entrance of Venus on the sun could not be seen at Hudson's Bay. He made a mistake too in the calculation, in taking the sum instead of the difference, of the angle of the ecliptic with the parallel to the equator, and the angle of Venus's path

An Account of the Cause of the late remarkable Appearance of the Planet Venus, seen this Summer, 1716, *for many Days together, in the Day-time. By Edmund Halley, R. S. Secr.* N° 349, p. 466.

It may justly be reckoned one of the principal uses of the mathematical sciences, that they are in many cases able to prevent the superstition of the unskilful vulgar; and by showing the genuine causes of rare appearances, to deliver them from the vain apprehensions they are apt to entertain of what they call prodigies; which sometimes, by the artifices of designing men, have been employed to very bad purposes.

Of this kind was the late appearance of Venus in the day-time, generally taken notice of about London and elsewhere; and by some reckoned to be prodigious. This put me on the inquiry, how it came to pass that at that time the planet should be so plainly seen by day, whereas she rarely shows herself so, unless to those who know exactly where to look for her. To resolve this, the following problem arose, viz. to find the situation of the planet in respect of the earth, when the area of the illuminated part of her disk is a maximum.

To investigate this maximum, I found it requisite to assume the following lemmata. 1. That the visible areas of the disk of the same planet, at differing distances, are always reciprocally as the squares of those distances, which is evident from the first principles of optics. 2. That the area of the whole disk of the planet is to the area of its illuminated part, as the diameter of a circle to the versed sine of the exterior angle at the planet, in the triangle at whose angles are the sun, earth, and planet. 3. That in all plane triangles, 4 times the rectangle of the sides containing any angle, is to the excess of the square of the sum of the sides above the square of the base, as the diameter is to the versed sine of the complement of the contained angle to a semicircle, which I call the exterior angle; this is a new theorem, of good use in trigonometry, and easily proved from the 12th and 13th of the 2d Elem. Euclid.

This premised, putting m for the distance of the sun and earth, and n for that of the sun and Venus, and x for the distance of the earth and Venus, or the third side of the triangle which we seek; by the third lemma, $4nx$ will be to the excess of the square of $n + x$ above the square of m, as the area of the whole disk of Venus, to the area of the part illuminated; and by the first lemma, the areas of her whole disk are at all times as the squares of x reciprocally;

with the ecliptic; which affected the accuracy of his conclusions. He was mistaken also in thinking the external contact might be observed to a second of time; astronomers had disagreed 20 seconds in observing the internal contacts of Mercury, which is a similar phenomenon.

Greenwich, May 26, 1803. N. M."

whence the quantity $\frac{nn + 2nx + xx - mm}{4nx^3}$ will in all cases be proportional to the area of the illuminated part.

Now that this should be a maximum, it is required that the fluxion thereof be equal to 0, or that the negative parts thereof be equal to the affirmative, that is, that $\overline{2n\dot{x} + 2x\dot{x}} \times 4nx^3 = 12nx^2\dot{x} \times \overline{nn + 2nx + xx - mm}$; and dividing all by $4nx^2\dot{x}$, the equation becomes $2nx + 2xx = 3nn + 6nx + 3xx - 3mm$. Consequently $3nn + 4nx + xx = 3mm$, and therefore $x = \sqrt{3mm + nn} - 2n$.

From hence a ready and not inelegant geometrical construction (if I may be allowed to say so) becomes obvious: for with the centre S, and radius ST = m, describe the semicircle TDA, fig. 4, pl. 5; and with the same centre and radius SE = n, the semicircle EVB; which two semicircles will represent the orbs of the earth and Venus. Make the chord AD equal to the radius ST, and from D towards A, lay off DF = SE; draw TF, on which place FG = RE = 2n, and with the centre T and radius TG describe the arch GV, cutting the semicircle BVE in V; and draw the lines SV, TV; I say the triangle STV is similar to that at whose angles are the sun, earth, and Venus, at the time when the area of the enlightened part of that planet's disk, as seen from the earth, is greatest. How this geometrical effect follows from the equation is too evident to need repetition.

In consequence of this solution, I find this maximum always to happen when the planet is about 40° distant from the sun; and the times of it about the middle between her greatest elongations on both sides from him, and her retrograde conjunctions with him; when little more than a quarter of her visible disk is luminous, and resembling the moon of about 5 days old; and though her diameter is at that time only 50 seconds, yet she shines with so strong a beam, as to surpass the united light of all the fixed stars that appear with her, and casts a very strong shade on the horizontal plane they all shine on; an irrefragable argument to prove that the disks of the fixed stars are inconceivably small, and next to nothing, since shining with a native light, so many of them do not equal the reflex light of one quarter of a disk of less than a minute diameter.

In this situation Venus was found in July last, on the 10th day, about which time, when the sun grew low, she was very plainly seen in the day-time, for many days together, as she might have been in the mornings, about the latter end of September. But this, arising from the causes we have now shown, is nothing uncommon; for every 8th year it returns again, so that the planet may be seen on the same day of the month and hour, very nearly in the same place, as all acquainted with the heavenly motions must know.

Lastly, it may not be amiss to note that the equation $x = \sqrt{3mm + nn} - 2n$

has a limit; for if n be equal to $\frac{1}{4}$m, the point v will fall on B; and the whole disk of a planet at that distance from the sun would be the maximum, viz. when in its superior conjunction with the sun. And the like if n were less than $\frac{1}{4}$m; the arch GV in such case not intersecting the semicircle BE.

An Account of a very uncommon sinking of the Earth near Folkestone in Kent. In a Letter from the Rev. Mr. John Sackette, A. M. to Dr. Brook Taylor, R. S. Secr. N° 349, p. 469.

Concerning the pressing forward of the cliffs, and sinking of the hills in the neighbourhood of our town of Folkestone; fig. 5, pl. 5, represents a sketch of the situation of the country, showing a straight road from what is called the Mooring Rock to Tarlingham House; the manner of the country, as to the rising and falling, being much the same, for about a mile on either hand of the road described.

A represents the Mooring Rock, about halfway between high and low water mark; B the foot of the cliff, 50 yards from the rock; C the top of the cliff, about 6 yards high; CD a plain of 50 yards; DE a cragged cliff, of 60 yards high; EF a plain above a mile long; FG a hill of steep ascent, near half a mile; GH the land from the top of the hill to the house, near a mile; I Tarlingham House, lying near $2\frac{1}{2}$ miles, N. N. W. from the rock; EGH a line of sight; KBL the shore at high water mark.

The Mooring Rock, though surrounded with many others, is a very noted one, and has immemorially borne this name, as vessels are usually moored here, while they are loading with the other rocks. It has remained fixed thus beyond the memory of man, and old men have observed, that for 40 years and upwards, the distance between it and the foot of the lesser cliff AB, has been much the same; this creates great surprise: for they can prove by good marks, that the lesser cliff BC has been constantly falling in; insomuch that from time to time, in their memory, near 10 rods forward to the land has been carried away by the sea. From whence, as it appears that the plain between the top of the lesser cliff and the foot of the higher CD, has been formerly double the breadth that it is at present, so the distance between the rock and the foot of the lesser or lower cliff AB, should have increased in proportion, and would have been double at present to what it has been formerly; but this distance remaining the same, or rather less, as several think, is very surprising; nor can it be accounted for otherwise, than by supposing that the land pressing forward into the sea is washed away by the high tides, and as often as this happens, it presses forward again. This pressing forward of the land into the sea would be incredible, were it not shown to be matter of fact; and that not only at this one place of observation, but by like observations all along this coast, as far as the situation continues the same.

At the top of the higher craggy cliff, at the point E, it is to be observed that, as old men inform us, upwards of 40 years since, not so much as the top of Tarlingham House could be discerned, neither from hence nor yet a good distance off at sea; but it discovered itself gradually, till at this day not only the whole house, but a great tract of land below it, is plainly to be seen, as in the line of sight EGH. The tract of land is more in proportion than described in the sketch, between the point at H and the house. In this there can be no fallacy; and we can ascribe it to nothing less than the sinking of the hills, for their tops could never wear away considerably, being always covered with grass, and never broken up by the plough or otherwise. These hills are all of chalk, and have probably very large caverns within, springs of water always flowing plentifully from the foot of them; and on their tops frequent cracks have been noticed. Whatever be the cause of it, it is not to be doubted but that these hills are greatly sunk. And this sinking of the hills, the people at this place believe, forces the cliffs and all the land forward into the sea. The cliffs consist of large ragged sand-stones till we come to near a yard, at some places more, of the bottom; then we meet with what is called a slipe, i. e. a slippery sort of clay always wet. On this slipe at the bottom, they suppose that the hard stony land above slides forwards toward the sea, as a ship is launched upon tallowed planks.

Miscellaneous Observations made about Rome, Naples, and some other Countries, in the Years 1683 *and* 1684. *By Tancred Robinson, M. D. R. S. S.* N° 349, p. 473.

These observations chiefly relate to natural history; but there is nothing in them of sufficient interest to entitle them to be reprinted.

An Account of the Mischiefs ensuing from swallowing the Stones of Bullace and Sloes. By the Rev. Wm. Derham, F. R. S. N° 349, p. 484.

Among the accounts which the Royal Society has had of the mischiefs ensuing the swallowing of divers sorts of stones, I do not remember any case wherein the lesser stones of fruits, such as sloes particularly and bullace, have produced any dangerous symptoms, especially in the stomach alone. The larger stones of prunes and plums, have produced very fatal effects; but the lesser stones of sloes, cherries, &c. many swallow rather out of choice than with any apprehensions of danger, thinking them useful in preventing a surfeit from the fruit. But the following case will show the danger even of these lesser stones.

About 2 years since the man servant of a neighbouring clergyman complained to me of excessive pains in and about his stomach; that he laboured under a great dejection of appetite; and whenever he ate, that he could not retain it, but in a little time threw it up again. By which means he was, in a short time, reduced to a very low and languishing condition, insomuch that they began to despair of his life. On this he applied to some practitioners in physic: one of whom plied him with strong vomits 8 days together, with very little signs of success. But some time after having occasion to ride somewhat more than ordinary, he found himself sick and much pained in his stomach; which ending in violent vomiting and straining, brought up the first stones he ever perceived to come from him, about 20 in number. After this he had frequent returns of the vomiting up of bullace and sloe-stones, especially on strong exercises; particularly moving and stooping much in weeding in the garden; in riding also, though it was only to water his master's horse. On these occasions he would be seized with acute pains in his stomach, and soon after vomit up more of those stones. He has counted above 120 bullace and sloe-stones, besides several others that have come up when he was riding, or in his business. He is not yet free of them, but is often in pain, and vomits them up, especially in riding; but after discharging them, he is much easier for a while. He commonly brings up a slimy matter with them, mixed with blood, or something very like it.

The cause of all this disorder the man assures himself was this, namely, being in his youth a great lover of fruit, he used greedily to devour all sorts he could come at, and particularly bullace and sloes, which he used to swallow in great quantities, without evacuating many of the stones by stool, as he well remembers, and as he observed others did. These stones he thinks have lain in his stomach, some of them at least, above 10 years; but he felt no pains till about 4 years since; and at first not so violent, nor attended with such severe fits of vomiting, and loss of appetite, as afterwards.

Observations and Experiments relating to the Motion of the Sap in Vegetables By Mr. Richard Bradley, R. S. S.* N° 349, p. 486.

Plants in general are either terrestrial, amphibious, or aquatic; and so nearly do vegetables agree with animals in most particulars, excepting local motion and its consequences, that from the knowledge of the one we are reasonably led to the discovery of the other. Those plants which I call terrestrial, are such as trees, shrubs and herbs, which grow only on the land: and these, like land

* On this subject, the motion of the sap, some new and very interesting experiments have been lately communicated to the Royal Society by Mr. Knight. See Phil. Trans. for 1801 and 1804.

animals, have diversities of food, a method of generating, and certain periods of life. Of the amphibious race, which live as well on land as in the waters, are the willows, rushes, mints, &c. these are not unlike in many respects to the otter, tortoise, frog, &c. The aquatics, whether of lakes, rivers, or the sea, are very numerous; these may be compared with the fish-kind, and like them will not live out of their proper element. In fresh waters are the water-lilies, plantains, &c. and in the sea, corals, fuci, &c. Plants seem to possess only the next degree of life below the most stupid animal; or where animal life ceases, there the vegetable life seems to begin. The seasons of motion in plants are the same with those of animals, which sleep during the winter: an artificial heat will give motion to either of these in the coldest season.

The common opinions relating to the motion of the sap, are as follow: First, the sap does not rise by the pith; because some have observed the trunks of large trees to be without that part, and yet the same trees have continued to put forth fruit, and branches on their tops. I have observed, that the pith is not found in those branches of a tree which exceed two or three years growth; and it is certain, that the pith which is in a branch of this year, will (the greatest part of it) be distributed into those boughs which form themselves the next season.

It is said by some, that the tree does not receive its nourishment by the bark, for that trees having lost that part, will still continue their growth. Others tell us, that if the bark be cut away round the trunk of a tree, it will presently die. These various opinions seem to have been taken up without extraordinary consideration, on the belief that a tree has only one bark: whereas, on examination with the microscope, we find 4 distinct coverings to each branch, without the woody parts. The two outermost barks may be taken from a tree without great damage, but the other two, which lie nearer the wood, being stripped off, will kill the tree.

Some affirm, that the sap neither rises nor falls in the woody part of a tree, because they have not been able to perceive any sap to issue out of that part, when a branch has been cut. The microscope plainly shows us the vessels in the wood, through which the sap rises from the root; but as these tubes are not large enough to admit into them any thing more gross than vapour, so they have not been esteemed to be of any great use. But the explanation of the adjoined figure will in some measure discover their office, and that of such other parts of a plant as are severally designed for the growth of vegetables: but it will first be proper to inquire a little into the nature of the root.

The root of a tree is chiefly composed of a parenchyma, more gross than that in the stem or body of the tree; it has also vessels and a covering. The root, that is, the principal part of it, receives into it such juices of the earth as are proper for it, and no other. Somewhat like a wick of cotton, which having been impregnated with oil, will only admit oil into it. This provision being made in the stomach of the plant, as I may call it, chiefly in the autumn months, the tree is prepared for germination as soon as the earth is sufficiently warmed, either by the sun's beams, or an artificial heat, such as horse-dung, bran and water, or other such like ferments. These heats raise into vapour the juices contained in the root, and by that means cause vegetation.

Fig. 6, pl. 5, is part of the branch of an apple-tree, produced in May 1715, and cut in April 1716. It was cut in the figure of a half-cylinder, the length somewhat more than the diameter, which was about a quarter of an inch. This being magnified with one of Campani's microscopes, discovers the following parts, viz.

1, 2, 3, 4, 5, 6, 7, are capillary vessels, which run longitudinally through the branch, in the ligneous part, which was made in the year 1715. Through these tubes the steam rises from the root; the strength of which is well explained by Capt. Savory's engine for raising water by fire. From A to B we may view vessels of the same sort, produced at the same time. 8, 9 are vessels of the same use with the former, now forming for the use of the year 1716. By this means the diameter of the branch is increased, and additional nourishment suffered to pass into those buds which are to make new branches. These are made out of the 4th or innermost bark, marked CC.

The mouths of the capillary tubes of the years 1715 and 1716, are D, E. The vapour which rises from the root, is continued in these vessels, to the extremities of the branches; where it meets with parts (not here represented), resembling glands; which glands, if we may so call them, are likewise found at every knot or joint. At these places, the vapour coming near the air is condensed, and returns between the barks, by means of its own weight, down F, G, H, leaving in each bark marked I, K, L, such juices as each of them naturally is inclining to separate from it; till at last, the more oily part passing to the root, may lengthen its fibres, as icicles are lengthened; and by its oleous particles, preserve them from rotting by the wet. The parts which compose the several barks, are parenchymous or spongy.

The first marked M, is of a closer texture than the second N, and the second closer than the third O, and so on till these parenchymous parts are interwoven with the longitudinal wood-vessels, where they are somewhat constrained, till they come to make the pith, marked P. Then they are much larger than in any other part of the tree; and by what I have observed, seem to contain a more finished juice than the rest, and may well enough be stiled the medulla.

We may observe, that when the 4th, or innermost bark c, has once compleated its sap-vessels, and is firmly joined to the wooden part, then the third bark o takes its place for the succeeding year; and so the rest, only that the first, marked M, splits and divides itself, to supply the place of the second.

Before concluding I beg leave to recommend the following inquiry to the curious, viz. If the several barks, having different texture of parts, admit into each separate and different juices from the rest: whether those juices may not be of very different virtues; the first more astringent than the others, the second perhaps emetic; and the third cathartic.

Some Microscopical Observations, and Curious Remarks on the Vegetation, and exceedingly quick Propagation of Moldiness, on the Substance of a Melon. Communicated by the same. N° 349, p. 490.

I had lately a large melon, which I split lengthwise through the middle, to observe the vessels which composed the membrane or tunic of each ovary; but my affairs at that time not permitting me to continue the work I had begun, I laid by the one half of the melon, to be examined when I might have more leisure. After 4 days, I found several spots of moldiness began to appear on the fleshy or pulpy part of the fruit, somewhat green towards the rind; and of a paler colour towards the middle of the fruit. These spots grew larger every hour, for the space of 5 days; at which time the whole fruit was quite covered over. This surprising vegetation made me curious to examine, if there was any difference between those parts which were green and the others, besides their colour. The first being seen with the microscope, appeared to be a fungus, as fig. 7, pl. 5, whose cap was filled with little seeds, to the number of about 500; which shed themselves in 2 minutes after they had been in the glasses.

The other sort had many grass-like leaves, among which appeared some stalks with fruit on their top. Each plant might well enough be compared to a sort of bull-rush, as fig. 8. They had their seed in great quantities, which I believe were not longer than 3 hours before they began to vegetate; and it was about 6 hours more before the plants were wholly perfected: for, about 7 o'clock one morning, I found 3 plants at some distance from any others; and about 4 the same day, I could discern above 500 more growing in a cluster with them, which I supposed were seedling-plants of that day. The seed of all these were then ripe and falling.

When the whole fruit had been thus covered with mold for 6 days, this vegetable quality began to abate, and was entirely gone in 2 days more. Then was the fruit putrefied, and its fleshy parts now yielded no more than a stinking water, which began to have a gentle motion on its surface, that continued for 2 days without any other appearance. I found then several small maggots, fig. 9, to move in it, which grew for the space of 6 days; after which they laid themselves up in their bags. Thus they remained for 2 days more without motion, and then came forth in the shape of flies, as fig. 10. The water at that time was all gone, and there remained no more of the fruit than the seeds, the vessels which composed the tunics of the ovaries, the outward rind, and the excrement of the maggots; all which together weighed about an ounce. So that there was lost of the first weight of the fruit when it was cut, above 20 ounces.

We may judge from this, and other cases of the like nature, how much vegetable life is dependent on fermentation, and animal life on putrefaction.

The Art of Living under Water: or, a Discourse concerning the Means of furnishing Air at the Bottom of the Sea, at any ordinary Depths. By Edm. Halley, LL. D. Secretary to the Royal Society. N° 349, p. 492.

Many methods have been proposed, and engines contrived, for enabling men to remain a competent time under water: and the respiring fresh air being found to be absolutely necessary to maintain life in all that breathe, several ways have been thought of, for carrying this pabulum vitæ down to the diver, who must, without being supplied with it, return very soon, or perish.

The divers for sponges in the Archipelago help themselves, by carrying down sponges dipped in oil in their mouths: but considering how small a quantity of air can be supposed to be contained in the pores or interstices of a sponge, and how much that little will be contracted by the pressure of the incumbent water, it cannot be believed that a supply obtained by this means, can long subsist a diver: since by experiment it is found that a gallon of air, included in a bladder and by a pipe, reciprocally inspired and expired by the lungs of a man, will become unfit for any further respiration, in little more than one minute of time; and though its elasticity be but little altered, yet in passing the lungs, it loses its vivifying spirit, and is rendered effete, not unlike the medium found in damps, which is present death to those that breathe it; and which in an instant extinguishes the brightest flame, or the shining of glowing coals or red-hot iron, if put into it. I shall not go about to show what it is the air loses by being taken into the lungs, and shall only conclude from the aforesaid experiment, that a naked diver, without a sponge, may not be above a couple of minutes inclosed in water, nor much longer with a sponge, without suffocating; and not near so long without great use and practice: ordinary persons generally beginning to stifle in about half a minute of time. Besides, if the depth be considerable, the pressure of the water on the vessels is found by experience to make the eyes blood-shot, and frequently to occasion spitting of blood.

When therefore there has been occasion to continue long at the bottom, some have contrived double flexible pipes, to circulate air down into a cavity inclosing the diver as with armour, to bear off this pressure of the water, and to give leave to his breast to dilate on inspiration: the fresh air being forced down by one of the pipes with bellows or otherwise, and returning by the other; not unlike an artery and vein. This has indeed been found sufficient for small depths, not exceeding 12 or 15 feet: but when the depth surpasses 3 fathoms, experience teaches us that this method is impracticable: for though the pipes, and the rest of the apparatus, may be contrived to perform their office duly, yet the water, its weight being now become considerable, so closely embraces and clasps the limbs that are bare, or covered with a flexible covering, that it obstructs the circulation of the blood in them; and presses with so much force on all the junctures, where the armour is made tight with leather or skins, or such like, that if there be the least defect in any of them, the whole engine will instantly fill with water, which rushes in with such violence, as to endanger the life of the man below, who may be drowned before he can be drawn up. On both which accounts, the danger increases with the depth. Besides, a man thus shut up in a weighty case, as this must needs be, cannot but be very unwieldy and unactive, and therefore unfit to execute what he is designed to do at the bottom.

To remedy these inconveniences, the diving-bell was next thought of; in which the diver is safely conveyed to any reasonable depth, and may stay more or less time under water, according as the bell is of greater or less capacity. This is most conveniently made in form of a truncated cone, the smaller basis being closed, and the larger open; and ought to be so poised with lead, and so suspended, that the vessel may sink full of air, with its greater or open basis downwards, and as near as may be in a situation parallel to the horizon, so as to close with the surface of the water all at once. Under this receptacle the diver setting, sinks down together with the included air to the depth desired; and if the cavity of the vessel may contain a tun of water, a single man may remain in it at least an hour, without much inconvenience, at 5 or 6 fathoms deep. But this included air, as it descends lower, contracts itself according to the weight of the water that compresses it; so as that about 33 feet deep the bell will be half full of water, the pressure of it being then equal to that of the whole atmosphere: and at all other depths, the space occupied by the compressed air in the upper part of the bell, will be to the under part of its capacity filled with water, as 33 feet to the depth of the surface of the water in the bell below the common surface of it. And this condensed air, being taken in with the breath, soon insinuates itself into all the cavities of the body, and has no sensible effect, if the bell be permitted to descend so slowly as to allow time for that purpose. The only inconvenience that attends it, is found in the ears, within which there are cavities opening only outwards, and that by pores so small as not to give admission even to the air itself, unless they be dilated and distended by a considerable force. Hence on the first descent of the bell, a pressure begins to be felt on each ear, which by degrees grows painful, like as if a quill were forcibly thrust into the hole of the ear; till at length, the force overcoming the obstacle, that which constringes these pores yields to the pressure, and letting some condensed air slip in, present ease ensues. But the bell descending still lower, the pain is renewed, and again eased after the same manner. On the contrary, when the engine is drawn up again, the condensed air finds a much easier passage out of those cavities, and even without pain. This force on the auditory passages might possibly be suspected to be prejudicial to the organs of hearing, but that experience shows the contrary. But, what is more inconvenient in this engine, the water entering into it, so as to contract the bulk of air, according to the aforesaid rule, into so small a space, as that it soon heats and becomes unfit for respiration, for which reason it must be often drawn up to recruit it: and besides, the diver being almost covered with the water thus entering into his receptacle, will not be long able to endure the cold of it.

Being engaged in an affair that required the skill of continuing under water, I found it necessary to obviate these difficulties, which attend the use of the common diving-bell, by inventing some means to convey air down to it, while below; by which not only the included air would be refreshed and recruited, but also the water wholly driven out, in whatever depth it was. This I effected by a contrivance so easy, that it may be wondered it should not have been thought of sooner, and capable of furnishing air at the bottom of the sea in any quantity desired. The description of my apparatus is as follows:

The bell I used was of wood, containing about 60 cubic feet in its concavity, and was of the form of a truncated cone, the top diameter 3 feet, and the bottom 5. This I coated with lead so heavy that it would sink empty, and I distributed the weight so about its bottom, that it would go down only in a perpendicular situation. In the top I fixed a strong clear glass, as a window to let in the light from above; and likewise a cock to let out the hot air that had been breathed; and below, about a yard under the bell, I placed a stage which hung by 3 ropes, each of which was charged with about a hundred weight, to keep it steady. This machine I suspended from the mast of a ship, by a sprit, which was sufficiently secured by stays to the mast-head, and was directed by braces to carry it over-board clear of the ship side, and to bring it again within-board as occasion required.

To supply air to this bell when under water, I caused a couple of barrels, of about 36 gallons each, to be cased with lead, so as to sink empty; each having a bung-hole in its lowest part to let in the water, as the air in them condensed on their descent; and to let it out again, when they were drawn up full from below. And to a hole in the upper part of these barrels I fixed a leather trunk or hose, well liquored with bees-wax and oil, and long enough to fall below the bung-hole, being kept down by an appended weight; so that the air in the upper part of the barrels could not escape, unless the lower ends of these hose were first lifted up.

The air-barrels being thus prepared, I fitted them with tackle proper to make them rise and fall alternately, after the manner of two buckets in a well; which was done with so much ease, that two men, with less than half their strength, could perform all the labour required: and in their descent they were directed by lines fastened to the under edge of the bell, which passed through rings placed on both sides of the leather hose in each barrel; so that sliding down by those lines, they came readily to the hand of a man, who stood on the stage on purpose to receive them, and to take up the ends of the hose into the bell. Through these hose, as soon as their ends came above the surface of the water in the barrels, all the air that was included in the upper parts of them was blown with great force into the bell, while the water entered at the bung-holes below, and filled them: and as soon as the air of the one barrel had been thus received, on a signal given, that was drawn up, and at the same time the other descended; and by an alternate succession furnished air so quick, and in so great plenty, that I myself have been one of five who have been together at the bottom, in 9 or 10 fathoms water, for above an hour and half at a time, without any sort of ill consequence: and I might have continued there as long as I pleased, for any thing that appeared to the contrary. Besides, the whole cavity of the bell was kept entirely free from water, so that I sat on a bench, which was diametrically placed near the bottom, wholly dressed with all my clothes on. I only observed, that it was necessary to be let down gradually at first, as about 12 feet at a time; and then to stop and drive out the water that entered, by receiving 3 or 4 barrels of fresh air, before I descended further. But being arrived at the depth designed, I then let out as much of the hot air, that had been breathed, as each barrel would replenish with cool, by means of the cock at the top of the bell; through whose aperture, though very small, the air would rush with so much violence, as to make the surface of the sea boil, and cover it with a white foam, notwithstanding the great weight of water over us.

Thus I found I could do any thing that was required to be done just under us; and that, by taking off the stage, I could, for a space as wide as the circuit of the bell, lay the bottom of the sea so far dry, as not to be over-shoes on it. And by the glass window, so much light was transmitted, that when the sea was clear, and especially when the sun shone, I could see perfectly well to write or read, much more to fasten or lay hold on any thing under us, to be taken up. And by the return of the air-barrels, I often sent up orders, written with an iron pen on small plates of lead, directing how to move us from place to place as occasion required. At other times when the water was troubled and thick, it would be dark as night below; but in such case, I have been able to keep a candle burning in the bell as long as I pleased, notwithstanding the great expence of air requisite to maintain flame.

This I take to be an invention applicable to various uses; such as fishing for pearl, diving for coral, or sponges, and the like, in far greater depths than has hitherto been thought possible. Also for the fitting and plaining of the foundations of moles, bridges, &c. on rocky bottoms; and for the cleaning and scrubbing of ships bottoms when foul, in calm weather at sea. I shall only intimate, that by an additional contrivance, I have found it not impracticable for a diver to go out of our engine, to a good distance from it, the air being conveyed to him with a continued stream by small flexible pipes; which pipes may serve as a clew to direct him back again, when he would return to the bell.

Observations on the Glands in the Human Spleen; and on a Fracture in the upper Part of the Thigh-Bone. By J. Douglass, M. D. and R. S. S. N° 349, p. 499.

That anatomy, as well as physic and surgery, has received much improvement from a careful and true observation of what was found in dissecting morbid bodies, will appear from the two following instances, among many more that might be adduced for that purpose. For it is certain, that nothing has contributed so much towards forming a right notion of the nature of the several diseases, and a true knowledge of the structure of many parts of the human body, as their appearance in a preternatural state.

My first observation is of the glands visible to the naked eye, that appear dispersed through the fibrous substance of the human spleen. The subject I found them in, was a boy of about 4 or 5 years old, who died of a general atrophy, or consumption of all the muscular fleshy parts of the body, occasioned from the numerous glandulous swellings scattered up and down the whole mesentery; which by compressing the lymphatic vessels, called in this place vasa lactea, prevented the access and supply of the chyle, so necessary for the continued nourishment and increase of the parts. For without the constant recruit of this whitish balsamic liquor, the mass of blood will in a short time be unfit to perform any of those good offices, which a fresh accession of chyle qualifies it for.

In a piece of this spleen might be seen, without the assistance of a glass, several round whitish bodies, of a pretty hard consistence, and abundance of small white and softer specks; but both of the same nature. These, to me at least, appear to be so many distinct glands become visible; which in a natural state are only to be seen by a fine glass, as the curious Malpighius first observed. Vid. his Treatise de Liene, Cap. V.

The second observation. We had still been in the dark about the nature of a luxation of the head of the thigh bone, had we not carefully examined the part in the dead body. For by that sort of inquiry, the common mistake of surgeons was detected, and what was esteemed and treated by them as a luxation of the head of the femur, was discovered to be nothing else but a fracture of the same bone, near its neck; the globular head being still retained close in its own socket, called the acetabulum coxendicis.

Among all the writers of surgery and anatomy, I know of only three that were apprised of this mistake: the first was Ambrose Paree, the second Dr. Ruysch at Amsterdam, and Mr. Cheselden, a member of the Royal Society; whose observations on this subject I intend to communicate at another time, with an account of the true structure of this joint: in which I will consider the depth of the articulation; the wonderful strength of the muscles that surround it; the many strong ligaments that bind the head within the socket; the smallness of the neck of the bone; its porous and spongy substance, which makes it much weaker than the rest; and lastly, the disadvantageous oblique position of this neck, which exposes it the more to outward accidents. From a review of such like considerations, it will plainly appear that a fracture can much more easily happen, than a dislocation in that part from an external cause.

This os femoris belonged to an old woman turned of fourscore, who only fell from her chair, and thence suffered this breach of continuity in the substance of the bone. She lived 3 weeks after it; and though it never was reduced, yet she complained of very little or no pain, which may seem very extraordinary. It is observable that the fracture is not only oblique, near the neck of the bone; but that each trochanter, i. e. the two processes near its cervix, are likewise broken short off; and that they were both drawn up almost as high as the head of the bone itself, by the strong contraction of the glutæi and other muscles.

An Account of a Book, viz. Dissertatio de Dea Salute, in qua illius Symbola, Templa, Statuæ, Nummi, Inscriptiones exhibentur, illustrantur. Auctore Guilhelmo Musgrave, G. F. è Coll. Exon. Oxonii: Typis Leon. Lichfield: Impensis Phil. Yeo, Bibliopolæ Exon. Anno 1716. N° 349, p. 502.

Observations on some of the Primary Planets, and particularly the Occultation of a fixed Star by Jupiter. By the Rev. James Pound, F. R. S. N° 350, p. 506.

These partial observations can be of no use now that we possess the continued series of the British and the continental astronomers, and the theories and tables of the planets deduced from them.

A Description of that curious natural Machine, the Wood-Pecker's Tongue, &c. By Richard Waller, Esq. late Sec. to the Royal Society. N° 350, p. 509.

The picus martius, or wood-pecker, has several particulars in the structure and mechanism of its whole body, which may deserve an accurate observation and description: all which are wisely contrived and adapted, either for catching the food and sustenance of the individual, or continuing the species. He makes a round hole even in sound and hard trees, such as the oak, horn-beam, beech, and the like; where, the hollow being enlarged, the nest is made, the eggs laid and hatched, and the young brood fed, as by other birds.

For this purpose, that he may be enabled to perform such hard work, the muscles of the neck, breast, and thighs, are exceedingly strong, in proportion to the size of the bird: he has also a very firm strong sharp bill; his legs are strengthened with very strong tendons; and his toes, which are two before and two behind, are provided with sharp strong hooked claws or talons: besides this, his tail consists of 10 very stiff large and strong quills, firmly set into a robust strong uropygium or rump; so that when he has fastened his claws and feet into the clefts and inequalities of the bark of the tree, he claps his strong tail-feathers against the body of the tree; and so stands with his head erect, to give the strokes with his bill with the greater force.

This bird is known to throw out a long, slender, round tongue, to a considerable distance, 3 or 4 inches beyond the end of his bill; and to draw it in again very quick into his mouth or bill, with the caught insect spitted on the tip of it.

The explanation of the several draughts I made, with what exactness and care I could, in 8 or 10 several subjects, is as follows:—

Plate 7, fig. 1, represents the head with part of the neck of this bird, the skin being taken off; in which A shows the skull, having two shallow grooves or channels, or rather one broad one with a small rising in the middle, on the sinciput or back part, from each side of the neck to the top of the head, where they unite into one, which passes slanting towards the right side, and ends at the hole for the nostril on that side at c; b is the hole or passage for hearing; d a large white gland, containing a glutinous liquor, almost like cream as to colour and consistence, which empties itself into the mouth; I suppose to lubricate the cartilages; e the eye, which has a bony ring, encompassing the iris; f part of the tongue, which in this figure is represented as almost all drawn into the mouth, of which more when I come to describe the cartilages, &c. in the 2d fig.; g part of the neck, which is large, and furnished with very strong muscles; h the œsophagus, opening very wide at the fauces, and wholly musculous; iii a long, but thin and flat muscle in respect of its breadth, which is about $\frac{1}{8}$ of an inch, reaching from the end of the cartilage at c, to the under bill or beak at k, to the inside of which it is very firmly fastened; as is a similar one on the other side; k the under bill very strong and sharp pointed, articulated with the skull a little behind the ear-hole b; lll the cartilage on one side; the other being exactly the same. This cartilage is round, very smooth, even and slippery, about the size of a pretty large pin; and reaches, when the tongue is drawn in and the muscle ii relaxed, from the root of the upper beak at c, to the root of the tongue properly so called, or to the bones of the tongue where they are articulated, being bent like a hoop as in the figure, slipping very freely in a sheath or membranous duct fastened on the outer or convex edge of the flat muscle iii, which muscle accompanies it from its end at c, almost to the end of the canal or sheath, which opens at a hole a little before the larynx (as will be shown in the third figure); and thence the muscle proceeds to its insertion into the lower beak at k. From the concave edge of this muscle, there is a thin and transparent, but very strong membrane, strained like a drum-head to the skull at m, where it is very strongly fastened; this membrane is furnished with capillary veins and arteries, and doubtless is nervous; nn represent this membrane. This cartilage, when the tongue is exerted, parts about half an inch from the root of the beak at c: oo a pretty large vein and artery; pp a muscle reaching from one jaw to the other, under the throat, serving as a bandage to keep in the cartilages, and the root and os hyoides of the tongue, as I may call it, from starting out at that part where are the articulations of the cartilages with the bones, when by the muscles inserted into the sheath at or near p, and thence passing to the end of the tongue, it is drawn into the mouth; qq one of the last mentioned muscles, which is round, of the size in the figure, and fastened to the breast of the bird, cut off at r; s the aspera arteria consisting of perfect rings; tt a muscle accompanying the aspera arteria.

In fig. 2, AA represent the under part of the lower bill; Bb the tongue; b the place where the two cartilages and two bones, represented by ff in fig. 4, are brought into and inclosed in one tube or membranous sheath; cc two glands displaced in this figure; cc two muscles attending these glands, and fastened near the end of the bill; dd the two bony cartilages, bent, and passing on each side of the neck, but united at b; eee,eee, the pair of muscles, one attending each cartilage from its end at the upper beak, and firmly adhering to the vagina, in which it slips, till about ff; ff the place where these muscles leave the vagina, and pass on to the inside of the bill, where they are inserted. Their action is to thrust the tongue forward, or out of the mouth; gg a pair of muscles fastened a little below the larynx, to the musculous part of the aspera arteria, at i; their other end going up to the place b at the root of the tongue, whence they go on encompassed by the vagina to the articulation of the cartilages with the two bones. I take their action to be to draw the end of the tongue towards the larynx; kk two muscles fastened at one end within the thorax, under the merry-thought or clavicula; and at the other ends to the articulation of the cartilages with the two bones of the tongue, marked ff in fig. 4. These have the fore-mentioned nerves accompanying them. I take these to be chiefly concerned in drawing in the tongue; and each of these sends a branch to the gristle at the top of the aspera arteria at n; llll two muscles running along and fastened to the sides of the aspera arteria, from the thorax to the place where they are united, where each sends a branch; which binding over the bones and cartilages goes on to the fauces, where they are inserted; m part of the gula; n a cartilage at the top of the aspera arteria; oo the aspera arteria; p the neck bending like an s. The wind-pipe and gula in this bird pass always on the right side of the neck.

In fig. 3, AA represent the two long flat muscles, represented by ii in the first figure. These join close together at the top of the head, and so pass on to the end of the cartilages; to the end of which, as I take it, they are fastened: from whence a slender weak kind of ligament reaches to, and is inserted at, the right nose-hole, at the root of the upper beak. This ligament is relaxed when the tongue is thrust out; bb the cartilages, running in their vagina on the outside of the said muscles; c the larynx or passage to the aspera arteria. I observed no epiglottis; dd two articulations or joints in the under beak or bill; e the hole or passage, by which the tongue in its vagina comes out and is drawn in again; f what I call the tongue, in the inside of which the two cartilages are brought together, till they are both articulated to one single bone, at the end of which is the horny barbed tip; g one of the pyramidal glands; h the lower bill.

In fig. 4, A represents that part which I think may most properly be called the tongue; a small bone running through it: this, as far as c, is flat and thin at the sides. It is cut away at d, to show the bones within it; b the horny tip of the tongue, about a quarter of an inch long, strong and sharp, furnished with 4 or 5 barbs on each side; (not with an infinite number as Coiterus says.) These barbs are sharp and moveable, like the small teeth at the root of the tongue, and beginning of the gula, in the pike and jack-fishes, in that of eagles and the like; so as to let the prey slip easily on, but not so easily get off again; c the end of the bone of the tongue, where the two bony cartilages are articulated; d the place where the upper part of the tongue is cut away to show the bone; e several small tendons, or rather, as I take them to be, nerves, running through the tongue. Of these, some go to the end of the cartilages, others accompany the muscles to the neck; ff two bones or cartilages, which in the bird, are united by a thin membrane as far as the next joint, so as to open asunder to some distance, but not to separate quite. These two bones seem to answer to the ossa hyoidea in other animals. At the place marked gg, the muscle that draws the tongue into the mouth is fastened, or rather leaves the tongue at that place; it having its insertion near the end of it: this muscle is represented by qq in the first figure; hh the two bony and springy cartilages running on each side of the neck; which being joined close together on the top of the head, pass so joined to the nostril, or nose-hole on the right side.

By considering and comparing these four figures, the true mechanism and motion of the tongue, seems to be in short thus: the two long muscles inserted near the end of this lower beak, and reaching to the end of the cartilages, being contracted, the round hoop of the cartilages is drawn up, from each side of the neck, close to the pyramidal gland; and at the same time the muscles that draw the tongue into the mouth being relaxed, and the articulations at c and gg in the 4th figure, brought near to a straight line, the tongue is thrown out to the length of 4 or 5 inches. But when those long muscles are relaxed, the pair of muscles represented by kk in the 2d figure, being contracted, draw the articulations gg, where they are fastened, down into the throat or wide loose skin of the neck; and at the same time the cartilages opening into a wide hoop, the whole tongue is drawn into the mouth.

In fig. 5, A represents the skull; b the shallow crena, or groove, for the cartilages; c the place of their ending at the right nose-hole; d the orbit of the eye; e the hole for the optic nerve; f a hole or passage through from one orbit to the other; g a bone covering the hole to the ear; h the lower jaw and bill; i a ridge or processus in the skull, beginning at the root of the upper bill, and keeping the two ends of the bony cartilages in their place on the right side; k the os jugale; l the upper bill.

Fig. 6, represents the right leg and foot, in which there are two digiti before, and two behind. The strength, size, and sharpness of the hooked claws or talons, are remarkable.

Fig. 7, A represents the œsophagus; B the ingluvies or crop, partly musculous, and lined with a glandulous coat. This I found quite filled with small black pismires; as also c the ventriculus or gizzard, which joined close to the ingluvies; ddd the intestines nearly of the same size for the whole length; e the beginning of the rectum; f the pancreas.

Fig. 8, represents one of the middle pair of feathers of the tail, in which the great strength of the quill, for so small a feather, and its bifurcate end, are very remarkable.

Fig. 9, represents the roof of the mouth, where it is observable, that the rima or passage for the air to the nostrils, is beset on each side with a row of 10 or 12 small sharp teeth, with their points standing inwards, towards the gula. These take the prey from the end of the tongue, whose barbs or prickles are moveable, and are to keep it from going out of the beak again with the tongue, and from hence it is conveyed to the swallow.

The Natural History and Description of the Phœnicopterus or Flamingo; with two Views of the Head, and three of the Tongue, of that beautiful and uncommon Bird. By James Douglass, M. D. Reg. Soc. S.* N° 350, p. 523.

All authors, from Aristophanes down to Aldrovandus, have accounted the phœnicopterus a bird of the palmipede or web-footed kind; and though the latter author will not allow it to be so, yet he is obliged to own that it is not a true fissipede or digitated fowl. Dr. Charlton only, among all the later natural

* Phœnicopterus ruber. Lin. It is a native of many parts of Africa and South America, and occasionally appears in some parts of Europe. It is about the size of a heron, with the neck and legs enormously long. The colour of the full-grown bird is scarlet, except the long wing-feathers, which are black.

historians, has approved of his division, and accordingly ranked the phœnicopter in the class of aquatic fissipedes. But that it is a water fowl all agree; Aristophanes calls it λιμναῖος, i. e. palustris; and Aldrovandus says of it, Avis est aquas amans: not to mention others.

Willoughby writes, that in severe weather, it comes over to the coast of Provence, and is often taken about Martiquez, a sea-port town in that country, and in Languedoc, and is frequently found about Montpelier: but he is ignorant whence it comes and where it is bred;* but says positively, that they do not come from Flanders, where they are so far from being common, as some allege, that none was ever seen there.

Gesner says, "circa lacus et paludes victitat," and that it feeds on periwinkles and fish. By Dampier's account we learn, that they delight to keep together in flocks, and feed in mud and ponds, or in places where there is not much water; that they are very shy, and therefore it is hard to shoot them; that they build their nests in shallow ponds, where there is much mud, which they scrape together, making little hillocks, like small islands, appearing out of the water, a foot and a half from the bottom: they make the foundation of these hillocks broad, bringing them up tapering to the top, where they leave a small hollow pit to lay their eggs in. And when they either lay their eggs or hatch them, they stand all the while, not on the hillock, but over it, with their legs on the ground in the water, resting themselves against the hillock, and covering the hollow nest upon it with their wings: for their legs are very long, and building thus, as they do, on the ground, they could neither draw their legs conveniently into their nests, nor sit down on them otherwise than by resting their whole bodies there, to the prejudice of their eggs or young, were it not for this curious and instinctive contrivance. They never lay more than three eggs, and seldom fewer. The young ones cannot fly till they are almost full grown; but will run prodigiously fast. Thus far Dampier.

This beautiful and scarce bird was much esteemed by the Romans, and frequently made use of in their sacrifices and entertainments. Thus Suetonius, describing the exquisite sacrifices which were appointed by the mad emperor Caligula, to be offered to himself as a divinity, says of them, "Hostiæ erant phœnicopteri, pavones, tetraones, numidicæ, meleagrides, phasianæ, quæ generatim per singulos dies immolarentur." And he relates further, that this emperor "pridiè quam periret sacrificans respersus est phœnicopteri sanguine."

That the tongue of this volatile was in great esteem for its excellent flavour, will appear from the following quotations. We read in Pliny, that Apicius

* See the *English* Edition of his Works.

said the tongue of this bird was a delicious and savory bit, "Phœnicopteri linguam præcipui esse saporis Apicius docuit, nepotum omnium altissimus gurges." The poet Martial says, "Dat mihi penna rubens nomen: sed lingua gulosis nostra sapit."

And Juvenal, in that satire where he exposes the extravagant luxury and gluttony of the Romans, mentions this fowl, among some others equally rare, which they made use of in their feasts.

"Et scythicæ volucres et phœnicopterus ingens."

We read in Suetonius how the emperor Vitellius had them often served at his table, with a great many more varieties brought from the most distant parts of the universe; "In hâc scarorum jocinera, phasianorum cerebella, linguas phœnicopterûm, murænarum lactes à Carpathio usque fretoque Hispaniæ per Navarchos ac Triremes petitarum commiscuit; hoc est, ab extremis imperii finibus orientem versus et occidentem." And Heliogabalus, another of the Roman emperors, as Lampridius writes, treated his courtiers with sumptuous dishes made of the inwards and brains of phœnicopters: "exhibuit palatinis ingentes dapes extis et cerebellis phœnicopterorum refertas."

The way to dress the phœnicopter, and how to make a sauce fit for it, we may read in Apicius's book De Obsoniis et Condimentis, seu de Arte Coquinariâ, Lib. vi. c. 7.

According to Bellonius, this bird is of the size of our curliew, which he calls elorius.—Scaliger compares it to the heron, magnitudo ei ardeæ.—Gesner says it is as large as a ciconia or stork, or rather larger.—Aldrovandus writes, "de magnitudine ejus ego nihil certi assero, quia avem nunquam vidi."—Dampier. "The flamingo is a sort of large fowl much like the heron in shape, but bigger and of a reddish colour."—Du Tertre. "Le flamand est un oiseau gros comme une oye sauvage."—It has an extraordinary long neck according to Mr. Willoughby.—Du Tertre. "Il a le cou rouge, fort menu pour la grandeur de l' oyseau, et long d'une demy toise."

Dr. Grew has obliged us with a very curious account of the bill of this bird, for which he says it is most remarkable. The figure of each beak is truly hyperbolical: the upper jaw is ridged behind, before plain or flat, and pointed like a sword, with the extremity bent a little downwards: within, it has an angle or sharp ridge, which runs all along the middle, at the top of the hyperbole, not above a quarter of an inch high: the lower beak in the same place above one inch high, hollow, and the margins strangely expanded inward, for the breadth of above a quarter of an inch, and somewhat convexly. They are both furnished with black teeth, as I call them from their use, of an unusual figure, viz. slender, numerous, and parallel, as in ivory combs; but also very short, scarcely the eighth part of an inch deep. An admirable invention of nature, by the help of which, and of the sharp ridge abovementioned, this bird holds his slippery prey the faster.

Dr. Grew agreeing in opinion with many authors that this bird moves its upper jaw, argues for such motion from the peculiar structure of the rostrum; alleging however, that there can be no certainty on this point, without inspection into the muscles, and the articulation of the bones. "As for the phœnicopter," says he, "it must needs be said, that the shape and size of the upper beak (which here, contrary to what it is in all other birds that I have seen, is thinner and far less than the nether) speaks it to be the more fit for motion, or to make the appulse, and the nether to receive it."

The Explanation of the Figures. Tab. II.—Fig. 10, pl. 7, gives a side view of the head and bill. Fig. 11, represents a front view of the same parts. Fig. 12, exhibits the under side of the tongue next the under bill: in which a denotes a cartilaginous substance that covers the tip or extremity of the tongue; b a glandulous substance at its basis; c the horns of the os hyoides. In fig. 13, the upper side of the tongue is fairly delineated, on which are seen two rows of strong papillæ nerveæ; their apices or points turning inwards, for the better retention of the prey. In fig. 14, the tongue is drawn in a lateral view, to show the true figure of these papillæ, which being hooked and turned backwards in a great measure prevent the return of any little animal swallowed alive, which they feed upon. In fig. 15, the cornua or horns of the os hyoidæum are drawn, as all the other parts are, half the size of life.

END OF VOLUME TWENTY-NINTH OF THE ORIGINAL.

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* *Observations of the Occultation of a Fixed Star in Gemini by the Body of Jupiter, Jan. 11, 1717, O. S. And of a Transit of Mars over the Northern Star in the forehead of Scorpio, Feb. 5, in the Morning.* N° 351, p. 546. *Translated from the Latin. Vol. XXX.*

In the Phil. Trans. N° 344,† we hinted to the curious, that there was to be an occultation of a fixed star by Jupiter, and desired they would observe so very uncommon a phenomenon, and of such considerable use in astronomy; signifying at the same time, that this would happen Jan. 10, 1717. But Jupiter being almost stationary, and advanced towards the east a little farther than by our tables, the occultation did not happen before the 11th; and it was not observed at London, as could be wished, by reason of clouds.

* Apparently either by Mr. Flamsteed or Dr. Halley, but probably the latter.

† Page 168 of this volume of these Abridgments.

Yet Mr. Folkes, with some members of the Royal Society, saw at London on Jan. 11 at 8^h P. M., Jupiter's centre, at the distance of one diameter of his body, follow the fixed star, which was more northerly than the said centre about three quarters of his semidiameter. Afterwards Jupiter was covered with clouds; but from his motion Mr. Folkes concluded, that the star was in conjunction with Jupiter a little after midnight, and hid by the northern part of his disk.

At Westminster, Mr. Desaguliers and Mr. Gray saw the fixed star at 6 o'clock in the evening, distant an entire diameter of Jupiter from his limb, towards N. W. Whence, and from the observations made the following days, it appears that the conjunction happened about midnight.

At Wansted, Mr. Pound made the following accurate observations, with a pretty long telescope and a micrometer. Jan. 5, at $5^h\ 6^m$ equated time, Jupiter's centre was distant from the said fixed star 31′ 49″, which at $5^h\ 38^m$ it followed, 34′ 12″ of right ascension; and at the same time the southern limb of Jupiter had the same declination with the star.

Jan. 9, at $6^h\ 6^m$ Jupiter's centre was distant from the star 10′ 49″, and 8 minutes after, the difference of right ascension was 11′ 32″: at which time the planet's centre was but a small matter more southerly than the star.

Jan. 11, at $5^h\ 30^m$ equated time, the distance of their centres was 1′ 24″: and at the same time the star was seen more northerly than Jupiter's centre about one-fourth of his diameter, and Jupiter's least diameter was found to be 43″: then clouds came on.

Jan. 12, at $5^h\ 17^m$, the distance of the centres was 3′ 7″: and at $5^h\ 50^m$ Jupier preceded the star 3′ 30″ of right ascension; and at the same time the northern limb of Jupiter had the same declination accurately with the fixed star.

It is manifest from comparing these observations, that this fixed star was in conjunction with Jupiter on Jan. 11, at about 13^h, and only 17″ or 18″ more northerly than his centre, and consequently hid by him.

This fixed star, though hitherto in no catalogue, was then in 22° 13′ of Gemini, with 13½′ S. lat. and had a star accompanying it, that preceded it 17′, and was 7′ more northerly, or in 21° 56′ of Gemini, with 6½′ S. lat. with which Jupiter seemed to be in conjunction on the 16th of January at $6^h\ 30^m$ in the evening. Thus in less than two months time, Jupiter eclipsed with his body two fixed stars, of which there is not one single instance extant since the invention of telescopes; consequently such observations are of great value, and worthy to be transmitted to posterity. According to Gassendus's observation, our little star was in conjunction with Jupiter, when stationary, Feb. 6, 1634, and more southerly by three of his diameters; whence it will appear, on a just calculation, that Jupiter's nodes have not sensibly moved for these 83 years last past; being in $2^s\ 8^\circ\ 35'$ from the first of Aries.

The same astronomers have been as diligent in making that other remarkable observation of Mars's transit near the north star in the forehead of Scorpio. For, on the 5th of February in the morning, or the 4th, 16^h, Mars was seen so near the said star, that it could not be discerned by the naked eye; but by the telescope it was found above it, and to the east, consequently Mars was not yet in conjunction with it. At $16^h\ 10^m$ apparent time, Mars was in a straight line with the northern star of Scorpio's forehead, and the telescopic star, which follows it, to the north, at about 8' distance. At $16^h\ 35^m$ Mars was in a right line with the north and middle star of the forehead; and a quarter of an hour after, with the south star of the forehead; so that the conjunction was estimated as to long. at $16^h\ 54^m$ apparent time; at which time Mars was only 2', pretty accurately, more southerly than the star. Mr. Pound also observed the conjunction, as to right ascension, at $17^h\ 25^m$ apparent time, with the distance of the centres 2' 7". It was an agreeable sight to see Mars gradually entering on the star, and plainly discovering his motion, though otherwise exceedingly slow.

Compare this with Horrox's observations Feb. 7, in the morning, 1638, which see in his Letters, p. 304. For at that time Mars coming to the same star, approached it much nearer, but the conjunction was over before his rising. Add to these an observation of Saturn made by Mr. Pound, Jan. 25, at $12^h\ 25^m$ equated time, when the planet was distant from the 58th star of Virgo in Catal. Britan. 13' 16" towards the south, and the star in $19^\circ\ 21'\ 52''$ of Libra, with $2^\circ\ 47'\ 25''$ N. lat. followed it 2' 30" of right ascension.

An Account of a tessellated Pavement, Bath, and other Roman Antiquities, lately discovered near East Bourne in Sussex. By Dr. John Tabor, of Lewes. N° 351, p. 549.

The meadow in which the greatest part of the pavement lies is near a mile and half south-east of Bourne; it contains about 4 acres, and is of a triangular form: the southern side is towards the sea: on the northern side is a highway, which leads from from Bourne to Pevensey: and the west side is, by a fence of posts and rails, separated from a large corn field, in common belonging to the parish. About the middle of this fence is the pavement, distant from high water mark a furlong. In former times it might have been somewhat more, because from this point to the westward, the sea is always gaining on the land.

The pavement was little more than a foot below the common surface of the ground: what lay next it was a small sea gravel; its position is nearly due east and west; its length is 17 feet 4 inches; its breadth 11 feet. At first it seemed to have been bounded with a thin brick set on edge, about an inch above the tesseræ, as straight and even as if shot with a plane, and as well cemented as if it had been one entire brick. But when the outside of the pavement was broken up, we found that, instead of bricks set on edge, as was imagined, it was bounded with a border of bricks laid flat, with their ends next the tesseræ turned up. The thickness of these bricks was an inch and a quarter; their breadth between 11 and 12 inches; the length full 15; and, before they were turned up at their ends, could not have been less than 17 inches. They were very firm, and not in the least warped or cast in the burning; when broke, their substance was fine and well mixed, of as uniform and clean a red colour, as a piece of fine bole, excepting at the ends, where turned up, they were all over covered with a plaster, the same which Vitruvius calls the nucleus, half an inch thick; so hard, entire, and even, that it seemed as one stone, quite round the pavement.

Next within the bricks, there was a list or border of white tesseræ, 13 inches broad; within that, a list of brown tesseræ, somewhat darker than a whetstone, and somewhat lighter coloured than the touch-stone, 4 inches broad; then a list of the white, 5 inches broad; next within that, another list of the brown, 4 inches broad: all the rest of the pavement was set with white tesseræ, without any ornament or figure, which looked very neat and clean.

When the ground about the pavement was dug up, on the north side was discovered an entire bath, 16 feet long, 5 feet 9 inches broad, and 2 feet 9 inches deep. It was filled with rubbish of buildings, which seemed to have been burnt, viz. hard mortar adhering to pieces of Roman bricks, squared stones, and headed flint, mingled with ashes and coals of wood. From the northwest corner of the pavement was the passage into the bath, 3 feet 3 inches wide; at which place the bricks that bounded the pavement were not turned up at their ends, but lay even with the tesseræ. At the distance of 15 inches from the tesseræ there was a fall of 2 inches, to the landing place out of the bath; the landing place was also 3 feet 3 inches long, and 2 feet 2 inches broad; from thence, by two steps of stairs, was the descent into the bath, the length of the steps the same as of the landing place; the breadth of each step was 11 inches, and the height of each a little more than 10 inches; the lowest step was 20 inches from the farther side of the bath. The whole work was very compact, and exactly well made, not in the least injured by time, nor the violence it underwent when filled up.

The pavement was secured on every side, and its edges rested on a very firm and neat built wall, made of Roman brick, squared stone, and headed flint;

between 5 and 6 feet deep below the surface of the pavement, and full 23 inches thick, which we may suppose to be 2 feet Roman measure. The bricks were not in regular courses, as seen in those Roman buildings which are in view above ground, but without order dispersed in the wall. The top of the wall indeed was only 15 inches thick, and that was covered with the bricks which bounded the pavement; but about 14 inches below the top there was a set off on the inside of the wall 8 inches broad. We did not dig up the foundation of the pavement to the bottom, but opened it at one corner only, that we might discover how it was framed; for when it was bored through, they observed, next under the tesseræ, a bed of very strong mortar, more than a foot thick; and under the mortar a bed of clay 2 feet thick, and under the clay a firm foundation of brick. The clay, which the ground thereabouts does not afford, was very fine, red, and close, doubtless having been carefully rammed. The surface of the clay was neatly pitched with small flint and stones, pointed at their lower ends, and headed at their upper ends. This pitching or paving is by Vitruvius called statuminatio: and the stones it is done with, he calls statumina.

This pitched work was exactly even with the set off in the inside of the wall: on it was laid a bed of coarse mortar of about 9 inches thick; the skirts of this mortar, which by Vitruvius is called rudus, rested on the set-off above-mentioned; it was composed of lime, with a sharp coarse sand, small pebbles, and bits of brick. On this rudus was a finer composition, made, as near as I could guess, with lime, a fine sharp sand, some kind of ashes, and, which was the greater part, stamped brick and pot-sherds, in grains not larger than cabbage-seed, the flour or fine powder being separated from it. This bed was about half a foot thick; and is what Vitruvius calls the nucleus. Whether we may call it terrace, I must leave it to those who are better skilled than myself, in giving proper appellations to the several parts of masonry. Both this nucleus and the rudus under it, nearly equalled the Portland-stone in hardness and compactness. On this nucleus or terrace the tesseræ were set on end; but with such exactness, that two sorts of cement were used to fix them; their lower ends standing in a cement of lime only, well worked, and their upper halves cemented with a fine grey mortar, consisting of fine sand, and as it seemed, ashes and lime. This grey cement every where filled the intervals at their heads, and was much harder than the tesseræ themselves.

The tesseræ were only of two colours, white and a dark brown; they were harder than a glazed and well burnt tobacco-pipe, and of a somewhat finer grit; the brown seemed to be of the same substance with the white, but coloured by art, as Pliny informs us, the workers in clay of old had a method of doing, they seemed to have been formed in a mould, and afterwards burnt. Hence I am inclined to take the meaning of Vitruvius, where he makes so plain a distinction between the tesseræ and the sectilia; that, the one was according to the import of the name, formed by instruments out of stone, brick, and tile; the other shaped in a mould and burnt. They were not of an equal size, none exceeding an inch in length; the shortest were $\frac{6}{10}$ of an inch: most of them were equally made their whole length; but of some the lower ends terminated almost as sharp as a wedge, on purpose, as may be supposed, to be driven where any interstices were left; at their heads likewise they were not all equal and alike, some exactly square, some oblong square, some semi-lunar, but none triangular; the diameter of those that were square was about $\frac{4}{10}$ of an inch; the longest side of those that were oblong at the head little exceeded half an inch. The preparations for fixing this pavement here, go beyond those which Vitruvius prescribes, in the firm wall near 6 feet below the surface, in the bed of clay within it 2 feet thick, and in the foundation of brick under the clay. But when we consider the situation of the ground here is low, not many feet higher than the sea might be elevated at spring tides; and that it might either be annoyed by land-springs after great rains, or by water oozing through the earth from the sea so near; from which the work in time might receive damage, we must allow the above-mentioned additions to be the result of a very judicious foresight.

The bath also was formed and secured by a very compact wall, of the same breadth and depth with that on which the pavement rested; the wall, which sustained the north side of the pavement, formed the south side of the bath. On this side, from the east end to the ends of the stairs, there was a solid seat, 12 feet 9 inches long, nearly 10 inches broad, and 14 inches high. The bottom or floor of the bath was made after the same manner as the pavement, excepting the tesseræ, and the thick bed of clay; for under all, there was brick, then a bed of the rudus or coarse mortar somewhat more than a foot thick, above that the nucleus or terrace only, half a foot thick. The sides of the bath, the seat, and the stairs, were plastered over with this terrace about half an inch thick; all which were throughout so hard, compact, and smooth, that when first opened, the whole seemed as if it had been hewed out of one entire rock, and polished. At the middle of the east end, at the bottom, there was a sink hole, a little more than 3 inches long, and above 2 inches deep; about 4 inches above it there was another passage through the wall, of the same size; the first we may suppose to let out the water which had been used, the other to let in fresh. The stairs and seat were chiefly made of Roman brick, between 15 and 17 inches long, between 11 and 12 broad, and near one and a half thick. At the north side of the bath the ground was not opened, but at the east end of the bath and pavement, at the south side of the pavement, and at the west end of both, there seemed to have been several vaults or cellars: for there were very firm walls 23 inches thick, continued every way, whose foundations were as low as that which supported the pavement; so that, to the depth of 6 feet, the ground was filled with such rubbish as was taken out of the bath. The bricks in this rubbish, which were all broken, had several degrees of thickness, from 3 inches to a little more than 1 inch; some had one of their sides waved, some fretted, and others had roses on them well imitated; we found also two sorts of channelled bricks, the one like a trough, the channel 3 inches broad, and as many deep, the brick itself an inch and a half thick; the other sort, had a cylindrical channel; so that when two were clapped together, they formed a hollow cylinder of 3 inches diameter. These channelled bricks being all broken, their length when whole is uncertain, as is the use they served to; whether for passages to convey water, or whether they were placed in the walls to distribute heat throughout the building, as was usual in the ancient structures at Rome.

It is further observable, when the ground was opened the second time, that from the south-west corner of the pavement, 5 feet lower than its surface, there was discovered a large space, paved with brick, 11 inches broad, almost one and a half thick, and 15 long; well paved, having two courses of this brick. There was half a foot of mortar under the lower course, and about an inch of mortar between the two courses; these bricks also were perfectly well made; but on the under side of each, were two knobs, about the size of half a walnut; fixed on them perhaps to keep them steady, till the mortar they were set in should dry. This paved place was searched 6 or 8 feet every way; it was all over covered with a coat about 2 inches thick, of ashes and large coals of wood; on that lay confusedly large pieces of the rudus or coarse mortar abovementioned, and lumps of the tesseræ in all respects like those on the pavement, and cemented as they were. There were also mingled with the ashes many great iron nails, larger, but not quite so long, as those we call double tens; some hooks for doors to swing on; several small pieces of earthen ware; some like bits of urns; some of a fine yellow clay, some red, thin, neatly wrought and adorned with flowers; and lastly part of a human skull, and pieces of bones near it: which bones were not inclosed in any vessel, but lay loose; they were discoloured, like those seen in urns; so that the body they belonged to might perish by the same flames by which these buildings were destroyed. There was no inscription found either on stone or brick, no statue or other figure, excepting those on the bricks before-mentioned; neither were there any coins met with there. But somewhat more than a furlong northwest of these works, near 3 years since, in digging the foundations for a malt-house, there was found a coin of Posthumus; and in the ground dug for the foundation of a dwelling house, a coin of Constantine's.

From the nearness of the bath, it may reasonably be concluded that the pavement was neither a part of a temple, nor for a place of justice: the continuation of the foundations every way to be traced from it, and what was last discovered, are rather an argument it was an apartment of a magnificent palace.

Pliny (Sect. Hist. l. 36, c. 25) supposed that these lithostrota, or tessellated pavements, had their origin in Greece: but perhaps the Grecians borrowed their patterns from Asia: for from the book of Esther, (ch. 1, v. 5) we learn, there was a most royal banquet at Suza, on a lithostroton (so the Septuagint has it) of costly stones, 400 years before the time of Sylla, who brought them first into Italy. Josephus (against Apion, l. 2) affirms, that the Grecian laws, learning and arts were fetched from Asia: and indeed when we reflect on the antiquity of the Levitic law; the pyramids of Egypt; the temple of Solomon; the walls and palaces of Babylon; and the sumptuous remains of Palmyra and Persepolis; we have no reason to reckon the Grecians authors, but rather good imitators of those early examples of learning and arts they had to follow.

When Quinctus Cicero accompanied Cæsar, the second time he invaded Britain, his brother Tully had the oversight of some buildings he had appointed to be made in the Villa Manliana at Arcano: and in a letter sent into Britain, Tully (Cic. ad Quinct. Frat. l. 3, ep. 1.) informs Quinctus, that he was well pleased with the seat, and the more so, because the pavimented piazza was magnificent; that the pavement seemed to be exactly well made: that he had directed some chambers to be altered, because he did not approve of them: that in the bathing apartment, he had removed the sweating room into another corner of the apodyterium. And afterwards in the same letter makes mention of such another work, which was in hand for him in the city also. Again, about the time Quinctus returned out of Britain, and was fixed with the legion he presided over, in winter quarters among the Nervii (of whom Cæsar makes mention in his Commentaries; Tully (ib. epist. 3) takes notice of a pavement that was making for himself also. It is hinted by Varro (de Re rust. l. 3), that a Lithostroton was one of the members of a complete villa: Varro was 80 years old when his books de Re rustica were composed: Tully was somewhat more than 50 when the above cited epistles were written; Cæsar when a general, (in Suet. Tranq. Cæs. cap. 46) made the tesseræ and sectilia for pavements, to be part of his baggage; and Vitruvius, (l. 7, cap. 5) cotemporary with these three, calls the Lithostrota, Principia Expolitionum; which make it evident that these floors were held in great esteem. From the whole we may observe, that sometime before, and the first age of the empire, the humour of these kinds of floorings much prevailed among the Romans: therefore it is no wonder they are found in so many places of this island. But, in the time of Pliny, (Hist. l. 36, c. 25) they began to be out of use on the ground; though still made above stairs, or in chambers. Whether the lithostrota in chambers were usual in Vitruvius's days, we have no warrant to suppose, from any hint in his writings; though he gives rules for making them, plano pede, on the ground, and sub dio, (l. 7, c. 1) (which from the method prescribed must be) aloft: because for sustaining those sub dio, he orders the work underneath to be well secured, with two layers of plank across each other, and nailed down; and then the statuminatio or pitching, the mortar, terrace and tesseræ, as before on the ground. But because by sub dio Vitruvius could not mean chambers; and though Pliny informs us the Grecians used to cover or flat-roof their houses with these pavements; yet since neither Vitruvius nor Pliny mention any such mode prevailing in their times at Rome; it remains, that we may suppose sub dio, or the subdialia of Vitruvius, to mean pavements mounted on pillars or arches, which might afford delightful terraces out of the upper rooms, and shady piazzas underneath: and in this sense perhaps may be understood the porticus pavimentata of Tully above-mentioned.

By the many apartments, the foundations about these works point out, there seems to have been nothing wherein the buildings that once stood there, might come short of the magnificent structures, with which the Romans delighted to gratify their luxury. The uses each were designed for cannot be determined, nor whether there was a piazza covered with a lithostroton. But be that as it may, it is next to demonstration, that there was some upper floor sustained by wood, and paved with the tesseræ, after the same manner as Vitruvius directs; and on the brick pavement, last discovered, the coat of ashes and wood coals with nails, covered with large pieces of the rudus, and great lumps of the tesseræ well cemented together, and the nucleus adhering to them, show that there was an upper pavement broken by its fall, when the fire had consumed its support.

As to the Roman architecture, it may not be amiss here to note; that when they designed a building, they could not immediately begin it: their preparations required time: by their well shaped durable bricks, and by their stone-like mortar, we may plainly perceive, that they built not with such hasty materials as are now used. Vitruvius and Pliny both direct, that brick should be formed in the spring, and be 2 years drying: and where Pliny speaks of their mortar, he says, it was ordained by the old laws of Rome, that no builder should build a house with mortar which had not been made 3 years before. We find indeed, that their walls seem to bid fair for eternity; whereas ours, from parsimony and ill management, are scarcely able to endure one age.

Of the Nature and Virtues of the Pyrmont Waters; with some Observations on their Chalybeate Quality. By Dr. Fred. Slare, R. S. Soc. N° 351, p. 564.

Having procured about a dozen quarts of Pyrmont waters, I made some trials of them. I found by the taste that they contained a rich chalybeate virtue,* and also made a lively impression on the palate, more grateful and spirituous, than the best spa-waters I ever tasted. The spa-waters are considered as excellent, if they sparkle a little in the glass: but these in summer time, when poured into the glass, nay sometimes even in the bottle, as soon as the cork was opened, and the air admitted, would make an ebullition, somewhat like bottled cyder, though this was soon over; but they still retained their smart taste, and high chalybeate relish, to the last drop, though we were some hours in drinking them off. In the winter time, these waters neither sparkle nor ferment, at least mine did not; but they were not carefully preserved, being exposed in cold cellars, where our beer or wine stood in the winter: and yet they lost not their chalybeate taste and their pleasant brisk gout. These waters have been reckoned in the number of the German saur brunnen or acidulæ: and some of my friends, to whom I gave a glass of the water, have ascribed a sharp taste to it, and were apt to think it was sour: but on further consideration acknowledged, that the smart taste misled them to call it acid or truly sour. Thus cyder and soft ale, when bottled, will give such an acute affection to the palate, when it is far from being sour: and even volatile alkalies of sal ammoniac, or of hartshorn, may be made to give the like pungency on the tongue.

In order to a more nice inquiry into the acidity of these Pyrmont waters, we dropped in considerable quantities both of spirit of hartshorn, and of sal ammoniac, both justly prepared; but could not discover the least luctation or motion on this conjunction, as is usual with an acid.

I made a still more nice trial of these waters, by mixing milk with them, sometimes in equal, sometimes in double proportion; and in various degrees of warmth, from lukewarm to a boiling heat; but I could not perceive any curdling. But rather on the contrary, the water preserved the milk from coagulation, for 4 or 5 days, even in September, when hot weather.

* The Pyrmont mineral water, so well analyzed by Bergman, is a carbonated chalybeate water; or a water containing iron held in solution by carbonic acid gas, to which gas the water owes its sparkling property and brisk acidulous taste.

Take a very little gall [nut-gall] in powder, about half a grain to a glass of a quarter of a pint; this in a moment renders it turbid, and makes a dark purple, especially when stirred: but if you drop the powder on the surface of the same water, it then causes a fine blue tincture. To make a very fine tincture, take 5 leaves of strong green tea, put them into the bottom of a glass holding a quarter of a pint, and you will see those leaves unfold themselves, and in a quarter of an hour, tinge the water with such a cerulean or azure blue, that few vegetables afford the like. The longer these leaves, or any other styptics, (which are the precipitants) stay together, the more they degenerate into a deep purple, or even to an atramentous colour.

As to the internal use of these waters, I drank about a quart at a time, in the following manner: I first began with Spa-waters, which I procured very good, and drank them for a week, and they agreed very well. I then drank the Pyrmont waters for 3 or 4 days, and continued the use of these waters alternately for 20 days. By the result of my experiments it seemed to me very plain, that the Pyrmont water was more agreeable, gave more strength and spirit, and was as much or more preferable for its internal virtue, as for its excelling the other in a brisker and more sprightly taste.*

There is another excellency in these waters, which will make them more useful to us, than any foreign chalybeate waters we yet know; that is they will keep better, and are not so soon spoiled by any accidental insinuations of air, as the Spa are subject to be. The chalybeate mineral is here thoroughly dissolved, and well united and mixed in this water, so that it does not easily precipitate: for which reason it may also the better pass the vasa lactea, and even enter into the mass of blood itself, and work the more considerable effects. That this is not a bare hypothesis may be proved by the following experiment.

Having suffered the Spa-water to be exposed in a bottle which was half full, and unstopped 12 hours, I examined it, and found it tasted just like common water; but the Pyrmont waters that were opened to the air in the same manner, tasted strong of the mineral, and gave their tincture as at first; nay, they continued thus for full 2 days, and perhaps might have done so longer.

Hence I may fairly conclude, that since the Spa has been very beneficial to our patients in chronical diseases, these waters, of a much superior virtue, will surpass them in conquering many of our obstinate distempers.

* The Spa water, like the Pyrmont, is a carbonated chalybeate water; but the latter is more strongly impregnated with carbonic acid gas. The proportion of iron, according to Bergman, is the same in both; but the saline ingredients (in addition to the carbonate of iron) in the Pyrmont water, are in greater variety and quantity, than those contained in the Spa water. See Bergman's Chemical Essays translated by Cullen, vol. i. pp. 249-253.

In order to discover whether Pyrmont waters really contain any purging ingredients or qualities, I evaporated about a quart of this water ad siccitatem; I then poured on the reliquiæ some rain-water, enough to dissolve and take up the salts, and exhaled that water, and had a grain or two of the salts, that tasted muriatic, such as most river and pump-waters do. It is well known that the purging waters have a very bitter taste, and by Dr. Grew, that salt was called sal catharticum amarum, which distinguished it from all other species of natural salts: that of the Pyrmont water has no relation to this, but rather to the sea-salt, not being in the least bitter.*

It is also well known, that unless our waters be impregnated with a considerable quantity of this bitter salt, it will not purge at all: 2 or 3 grains have not the least cathartic power. For example: Put ℨij of the purging salts to a quart of common water; and this quantity will give but a stool or two to one who is naturally very easy to work on. I have examined several other chalybeate waters, and found much the like ingredients, and never any that I could suspect to carry any purging quality.

I think we can better demonstrate that the chalybeate waters contain styptic and astringent virtues, because they owe their origin to the iron mineral, and more particularly to the pyrites, which Dr. Lister suggests, (not without some reason) to be the parent even of all iron ores, as it is doubtless the cause of all chalybeate waters: thus I have often examined the solution of the pyrites by the rain-water at Deptford, and at other places where copperas is made, and found it a very strong chalybeate water.† It is from this mineral we have our strong styptic and constringent medicines, for external and internal use; from hence our powders and salts of steel, or vitriol of mars; nay, even those obstinate and inveterate diarrhœas which have baffled the force of all medicines, have, by a judicious use of Tunbridge and other iron waters, received a cure.

But after all we can say, it will be retorted, that there is matter of fact and experience against this, that the waters really do purge at Pyrmont, where they are drank. We allow, that Tunbridge waters do not only purge, but sometimes vomit, when drunk hastily, and in great quantity; but our physicians

* Dr. Slare should have mentioned the weight of the residuum which he obtained by evaporating a quart of the Pyrmont water to dryness. His examination was evidently very imperfect. According to Bergman's analysis, the Pyrmont waters do contain sulphate of magnesia (the sal catharticum amarum of Dr. Grew) as well as a small proportion of muriate of soda (sea-salt). The other solid and fixed ingredients (besides iron) are carbonate of lime, carbonate of magnesia, and sulphate of lime (selenite).

† By this action of rain-water upon martial pyrites, there would be obtained a solution of sulphate of iron; i. e. iron combined with the sulphuric acid. But this chalybeate impregnation is different from that of the Spa and Pyrmont waters, in which the iron is combined with the carbonic acid.

have corrected this irregularity, and we hear of no such complaints, where they observe a just regimen: and we all agree, that those waters are, in their own nature binding, and often require some opening medicine. The quantities of water drunk at Pyrmont are very large, often 2 or 3 English quarts. It is no wonder that their weight forces them through the bowels; for any common water, drunk hastily, and in such quantity, will do the same. Whereas, if you take this method, and will drink Pyrmont, or any other chalybeate waters, leisurely, viz. a pint-glass in an hour, or rather 2 half-pint glasses, you may drink 3 pints in so many hours without danger of losing them by dejection. And if any one will be careful, and take this caution with him, he will scarcely fail of success; that is, let him be very quiet and still,* both in body and mind; the less he stirs or walks, the better he will pass off his waters by urine. And though this will appear a paradox, especially to those physicians who practise abroad, and commend to their patients much action in walking, yet I know I have both reason and experience on my side.

Remarks on a Paper in the History of the Royal Academy of Sciences, for the Year 1711, *concerning the Cause of the Variation of the Barometer: to show that the Way of accounting for it in that Paper is sufficient, and that the Experiment made use of to prove what is there asserted, does no way prove it. By J. T. Desaguliers, M. A., F. R. S.* N° 351, p. 570.

The paper is as follows:—" It appears by the barometer, that when it rains, or a little before rain, the air commonly becomes lighter. That it must rain when the air becomes lighter, it is easy to imagine; for the imperceivable particles of water, that swim about in the air in prodigious quantities, not being sufficiently sustained when the air has lost a certain degree of its weight, begin to fall, and several of them joining together in the fall, form drops of rain. So when about half the air is drawn out of the recipient of the air-pump, and consequently the remaining air is as weak again as at first, something like a small rain falls. But why should the air become lighter? One might imagine that in the place where it rains, it may have lost some of its weight and bulk, by means of the winds carrying away some part of it: but M. Leibnitz, in a letter to the Abbot Bignon, gives a new and more ingenious reason for it."

" He pretends that a body, which is in a fluid, weighs with that fluid, and makes up part of its whole weight, so long as it is sustained in it; but if it ceases to be sustained, and consequently falls, its weight no longer makes a part of the weight of the liquid, which thus comes to weigh less. This may

* Dr. S. is perhaps the only physician who ever forbade exercise during the use of the Pyrmont or other chalybeate waters.

naturally be applied to the above-mentioned particles of water; they increase the weight of the air when it sustains them, which is diminished when it lets them fall: and as it may often happen that the particles of water that are highest, fall a considerable time before they join with those that are low, the gravity of the air diminishes before it rains, and the barometer shows it."

"This new principle of M. Leibnitz is surprising. For must not a strange body, whether sustained in a liquid or not, always weigh? Can it gravitate on any other bottom than that which sustains the whole fluid? Does that bottom cease to carry a strange body, because it falls? And is not that body all the while it is falling, part of the said liquid as to the weight? At that rate, whilst a chemical precipitation is made, the whole matter ought to weigh less, which has never been observed, and scarcely appears credible."

"Notwithstanding these objections the principle holds good, when more closely examined. What sustains a heavy body is pressed by it. A table, for example, which sustains a pound weight of iron, is pressed by it, and is so only because it sustains the whole action and effect of the cause of gravity, whatever it be, to push that lump of iron lower. If the table should yield to the action of that cause of the weight, or gravity, it would not be pressed, and therefore would carry nothing. After the same manner the bottom of a vessel, which contains a fluid, opposes itself to all the action of the cause of gravity against the said fluid: if an extraneous body float in it, the bottom opposes itself also to the said action against that body, which, being in equilibrio with the fluid, is in that respect really a part of it. Thus the bottom is pressed both by the fluid and the extraneous body, and it sustains them both. But if the body fall, it yields to the action of gravity, and consequently the bottom no longer sustains it; neither will it sustain it, till the said body is come down to the bottom. Therefore during the whole time of the fall, the bottom is eased of the weight of that body, which is no longer sustained by any thing, but pushed down by the cause of gravity, to which nothing hinders it from yielding."

"M. Leibnitz, to confirm his notion, proposed an experiment. He says, that two bodies must be tied to the two ends of a thread, the one heavier, and the other lighter than water, yet such as both together may float in water: put them into a tube full of water, the tube being tied to one end of the beam of a balance whose other end has a counterpoising weight: then if we cut the thread which ties the bodies together, that are of unequal weight, so that the heaviest may presently descend, he says, that in such a case the tube would be no longer in equilibrio, but its counterpoising weight would preponderate, because the bottom of the tube would be less pressed. It is plain that the tube must be sufficiently long, that the falling body may not reach the bottom before the tube has time to rise. In chemical precipitations, the vessels are either too short, or what is precipitated falls sometimes too fast and sometimes too slow; for then the little bodies are always, as to sense, in equilibrio with the fluid that contains them."

"M. Ramazzini, the famous professor at Padua, to whom M. Leibnitz had proposed his experiment, has made it with success, after some fruitless trials. M. Reaumur, to whom the academy had recommended it, has also made it with success: this is a new view in natural philosophy, which, though it depends on a well known principle, is very subtle and far-fetched; and gives us just reason to fear that in subjects that seem to be exhausted, several things may yet escape us."

Remarks on M. Leibnitz's New Principle.—Let AB, fig. 1, pl. 8, be the bottom of a vessel full of any fluid, whose top is either wider than the bottom as GH, narrower as EF, or equal to it as CD. The pressure of the fluid on the base AB will be equal to the weight of CB, or of a cylinder or prism of the same fluid, made up of the area of the base multiplied into the perpendicular height above it.

If the fluid be equally dense every way, as water, or of a density uniformly diminished upwards, this proposition (called by Mr. Boyle the hydrostatical parodox) will hold good. This is demonstrated by all hydrostatical writers.

Let EF, fig. 2, represent part of the surface of the earth, and GEFH a column of the atmosphere, whose height is GE the whole height of the air. Let us suppose the vapours rising out of the earth to form themselves into two clouds A and B, and to settle in that place where the air is of the same specific gravity with themselves. It is evident that they will cause the air to rise so much higher as their bulk amounts to, and will therefore make the surface which was at GH to rise up to IK, so that the bottom EF, which was pressed by a column of air, as GEFH, is now pressed by a higher column as IEFK. Now if the clouds A, B, by any cause whatever, change their place, so as to come downwards, for instance to C, D, the height of the pillar IEFK will remain the same as it was, and therefore the bottom EF will be pressed as before; by the foregoing proposition.

Corol. 1.—If the clouds A, B descend, and in their descent keep the same bulk as they had before, the surface IK will remain the same, and therefore EF will be pressed as before.

Corol. 2.—Whether a body be specifically lighter or heavier than a fluid; as long as it is detained in it, it will add to the fluid as much weight as the weight of an equal bulk of that fluid: therefore a body does not lose all that weight which it added to the whole weight of the fluid, when it ceases to be sustained in it: contrary to M. Leibnitz's principle.

Scholium.—If a cloud, by any cause whatever, become specifically heavier than that part of the air in which it floats, the excess of its gravity above an equal bulk of air will make it descend, and accelerate its motion downwards; and then indeed it will lose of its weight by the resistance of the medium, till it comes to a uniform, or sensibly uniform motion: but all the weight that it will lose, will only be the excess of its gravity above that of the air; for with the rest of its weight it will still make up part of the weight of the air.

Exper. 1.—Having with a weight in the scale C, fig. 3, of the balance AB, counterpoised the long glass of water EI, with a horse-hair I let down the leaden weight W into the water, which from FG rose up to EH; and therefore the water became heavier by the weight of a bulk of water equal to the lead. Having with another weight in C made up the counterpoise to the whole, with fine scissars I cut the thread of the plummet; and all the while the plummet was falling, the water descended rather than rose; and when the lead was at the bottom, the water overpoised, because it had then added to it all the excess of the weight of the lead above an equal bulk of water, which by experiment is about $\frac{10}{11}$ of its weight. Had M. Reaumur and Ramazzini tried the experiment in this manner, the success would have been the same; but M. Ramazzini (as I was informed from a gentleman who was present) tried it in the following manner, as I have since done.

Exper. 2.—Using the same machine, after having balanced the water and lead in it, I fixed to the end of the beam B, fig. 4, the thread of the plummet, which in the former experiment I held in my hand. This added to the weight hanging at B, and obliged me to put into the other scale a weight equal to $\frac{10}{11}$ of the lead, to recover the equilibrium. Then cutting the thread or hair, the scale with the weights overpoised while the lead was falling; but the equilibrium was restored when it came to the bottom. So that the lead even then must have lost only its excess of weight above that of the water.

Exper. 3, *fig.* 8.—I tried the method proposed by M. Leibnitz in the following manner. I took a cork C weighing an ounce, fig. 5, and somewhat more than 4 times lighter than an equal bulk of water, and a ball of antimony W about 4 times specifically heavier than water, and of 4 ounces weight. The cork, laid upon the water in the vessel EABD, raised the water from SS to GG, and added an ounce to the weight of the whole water: then suspending the ball of antimony by a string, and letting it hang in the water at N, it raised the water from GG to HH, and so added another ounce to the weight of the water. Then tying the antimony to the cork (as in the figure 6, of the vessel marked with little letters) the cork had added to it three quarters of the weight of the antimony which the hand before had sustained, and made it sink so as to be almost covered, and raised the water to ik, adding 3 ounces to its weight. Suspending this vessel of water to the balance, and a counterpoise at the other end, on cutting the string, the vessel of water was raised up, and the equilibrium was not restored till the antimony came to the bottom.

By observing that as the cork (being freed from the weight of the antimony) rose, and that during the fall of the body, the water sunk to hh, it appears that this is, in effect, the same experiment as the former, and concludes no more. As to the real cause of the variation of the barometer, namely, the accumulation of the air by winds over the place where the barometer rises; and part of the air being blown away where the Mercury in the barometer sinks, see Doctor Halley's account of it in the Phil. Trans. N° 181.

In making the first experiment before the Royal Society, of a piece of lead suspended by a thread, while it was wholly covered with water in the large tube in which it hung, whose length was 4 feet, it was observable, not only that the end of the balance (to which the tube of water with the lead in it was fixed) did not rise when the thread was cut, to let the lead fall from the top to the bottom of the tube, as it must have done according to Mr. Leibnitz's principle; but that the said end of the balance began to descend from the time that the lead began to fall. Therefore, to be assured that it was not the plummet's rubbing against the sides of the tube in its fall, which caused that phenomenon, I hung to the balance a long glass of 3 inches diameter, instead of the tube, and making the experiment as before, it succeeded in the same manner: the end of the balance which carried the vessel of water sunk as soon as the thread of the plummet was cut; though this glass was not above half so long as the tube.

When by holding the string I drew the lead upwards and downwards in the water, there was no sensible alteration of the equilibrium. Neither was it altered by cutting the string of a stone plummet, because of the shortness of the glass, and the little excess of specific gravity in the stone: for the greater the difference is between the body used in this experiment and water, as well as the larger the body itself is, the better the experiment will succeed.

Hence it appears, that when a body, specifically heavier than a fluid, is by any cause whatever detained in any place of the said fluid, it adds as much to the weight of the whole fluid as an equal bulk of the said fluid amounts to: and when the said body, by the action of its excess of specific gravity above the fluid, descends with an accelerated motion; so long as that motion is accelerated, the resistance of the fluid, which is as the square of the velocity, takes off something of the whole weight of the body; but as much as the body loses, so much the water gains, over and above what was given it by its rising on account of the immersed body.

A body therefore that falls in a fluid is so far from making the fluid lighter as it falls, that it makes it press more upon the bottom that sustains it, when it is falling, than when it was at rest in the fluid. If the vessel of water be long enough for the falling body to come to a uniform motion before it reaches the bottom, the force impressed on the water under the body will make it press the bottom, as much as if the body were actually at the bottom; the body in that case losing all its excess of gravity above that of the water, and the water gaining it.

Hence it follows, that a falling cloud, when it comes to a uniform motion, will not only add to the weight of the air as much as the weight of an equal bulk of air; but even as much as its whole weight amounts to, though it be specifically heavier than the air about it.

All the diminution of weight that can be allowed in this case is this: if we imagine the air to have a smooth, regular surface, as we have at first supposed, (or if that be not allowed, we may take any imaginary surface of it above the clouds) when a falling cloud is diminished in bulk, as when it is changed into rain, the surface of the air will subside in proportion to that diminution, and therefore will weigh less, by so much as is the weight of a quantity of air equal to the bulk that cloud has lost: but when the drops of rain, after their acceleration (occasioned by their excess of gravity above that of the air) are come to a uniform motion by the resistance of the air, they restore to the air the weight that it had lost. Now this uniform motion being acquired in about 2 seconds of time, and the diminution of gravity in the air being insensible, when compared to near 3 inches of mercury, (for such is the variation of the barometer with us) it can nowise be the occasion of such sensible alterations in it, as happen sometime before rain or fair weather. Add to this, that the whole quantity of rain that falls in England and France, in the space of one year, scarcely ever equals two inches of Mercury: and in most places between the tropics, the rains fall, at certain seasons, in very great quantities, and yet the barometer shows there very little or no alteration.

An Account of an extraordinary Effect of the Colic. By M. St. Andre. N° 351, p. 580.

The peristaltic motion of the intestines is by all anatomists supposed to be the proper motion of those cylindrical tubes. The use of this motion is to propel the chyle into the vasa lactea, and to accelerate the grosser parts of the aliment downward, in order to expel them, when all their nutritive contents are extracted.

This motion thus established, it naturally seems to follow, that an inversion of it (called therefore an antiperistaltic motion) should force the aliments, bile, pancreatic juice, and lastly the fæces, to ascend towards the mouth. The cause of this imaginary antivermicular motion, is assigned to a stoppage of the intestine, or to a great length of it being entangled, in the same manner as the fingers of a glove are choaked by inverting the glove in drawing it off: or as a silk stocking, which when it is not gartered, falls down on the foot, and is in a manner strangled, so that some force is required to pull it up again.

This supposed, the antiperistaltic hypothesis seems at first sight very natural, and answers most difficulties. For if the vermicular motion accelerates the contents of the intestines downward; the antivermicular, by the law of contraries, should force them upward towards the mouth. Were this supposition as certain as it is generally received, I should not presume to advance that there is no such thing as an antiperistaltic motion of the intestines; nor that the misere mei is oftener a violent contraction of the abdominal muscles, than a stoppage or inversion of the intestines, as it is supposed.

Supposing then that this disorder is a violent contraction of the abdominal muscles, caused by the redundancy of the intestines, or their contents: then comparing the symptoms of this disease, with those of the different kinds of Hernias, we shall find by the analogy of the parts, by reason, and repeated experience, that the chordapsus, so called by Celsus, is a disease in which the intestines and omentum, at other times the pancreas or spleen, nay even the mesentery itself, are forced through the diaphragm into the thorax. All these tender parts being strongly compressed, by the continual motion of this muscle, must consequently cause the same accidents as in the bubonocele or complete hernia, there being no difference in these two cases; but that the first is a strangling of the intestine by the diaphragm, and the latter a choking of the intestines by the abdominal muscles.

One example of the many of the like nature, that I could produce, will much confirm this assertion, and is as follows: A gentleman that came to town in good health, meeting with some friends, drank a great deal of new-bottled oat-ale, after some pints of wine. These liquors fermented so violently in his stomach and intestines, that he was taken with a violent colic the same night. In the morning an apothecary was sent for, who administered a clyster, and drew some ounces of blood to relieve the patient, who complained of a great pain in his left side. The clysters being repeated the following night, as also the next morning, and the patient growing worse; the apothecary, without order of any physician, gave him a violent vomit: which operated 8 or 9 times: this added fuel to the fire; and the patient having from that time been in a desperate condition, two eminent physicians were called; who ordered that the clysters should be repeated: but these not prevailing, I was sent for about 6 hours before the patient died: I found him complaining of a violent pain in all the region of the abdomen; a frequent inclination to vomit; a great difficulty of breathing, with a very slow pulse, and his belly very hard, though not swelled.

This last indication made me conclude, that the disease was a violent contraction of the abdominal muscles, which had overcome the diaphragm, and that probably the intestines might be forced into the thorax: and I was the more confirmed in this opinion from other examples of the like case. I ordered therefore a fomentation of hot milk, adding to every quart a drachm of liquid laudanum, which in these maladies gives great relief: but before it could be got ready, the patient expired in a violent convulsion.

On opening the body, I found the abdominal muscles so much contracted, that it was almost impossible to penetrate them with a very sharp scalpel. The stomach was empty, and some parts of the duodenum; but the jejunum and ilium so much distended with the fermented oat-ale, that the ilium was 4 inches in diameter, and the colon above 8. The ilium was also pretty much inflamed in its inferior part; and all the valves of the colon were obliterated, by the great distention of that intestine. But the greatest disaster, was the dilatation made in the diaphragm, as I supposed, just on the chink which remits the intercostal nerve to the viscera of the abdomen, through which a portion of the colon was forced, and the greatest part of the omentum and pancreas. These tender parts being choked, were soon inflamed, and a mortification ensued. A rupture of the pancreatic vein caused an internal hæmorrhage, which filled all the left cavity of the thorax, insomuch that the whole left lobe of the lungs was compressed almost under the musculus scalenus.

The quantity of extravasated blood was very great, and it was not in the least coagulated.

An Account of two late Northern Auroras, observed at Hone in Kent. By the Rev. Edmund Barrell. N° 351, p. 584.

On Feb. 5, 1716-7, at 8 at night, an aurora borealis appeared. It occupied at least $\frac{1}{6}$ or $\frac{1}{5}$ of the horizon; it was low, and shot out bright rays, and I believe would have appeared very light, had not the moon shone at the same time, being about 5 days old, and that the aurora disappeared before the moon set.

Again, on the 30th of March following, there was another aurora borealis. I saw it not till past 9: it was dim then, and its highest part covered the lowest star in Cassiopeia's chair. It did not seem due north, but one point to the west. About 10 it shot out very bright rays, high, and tending somewhat towards one another. Near 11 o'clock, there was, besides the northern brightness, a long streak, not very broad, extended east and west; which beginning in the Serpent's head, near Hercules' club, and covering Arcturus, proceeded near Berenice's hair, and so went over Cor Leonis, and thence to the Canicula, and ended a little beyond that star. It shone very bright at first, but faded away in about 8 or 9 minutes. If it had motion (which I am not sure of) it was southward. I waited for the next fit of brightness of the aurora; and in about 7 minutes, the eastern part of the streak, viz. from the Serpent's head to near Berenice's hair, became visible again, though dim, and was quite effaced in 4 or 5 minutes more: and I did not yet perceive any change of its place.

An Account of the Aurora Borealis, seen at London, the 30th of March last. By Martin Folkes, Esq. R. S. Soc.* N° 352, p. 586.

Being in the street, (of London) between 8 and 9 o'clock on March 30, I perceived a light over the houses to the northwards, little inferior to that of the full moon when she first rises. On this I made all the haste I could into the fields, where I was for some time agreeably entertained with the sight of an aurora borealis, attended with most of the phænomena that have been described in that very remarkable one of the 6th of March, 1715-6.

The whole northern part of the horizon was in the same manner covered with somewhat resembling a very black cloud, from behind which there issued a considerable light, whose lower part was pretty well defined by the common edge of the cloud, but the upper died away more gradually. This upper limb of the light resembled the arc of a circle, whose highest point, between 9 and 10 o'clock, when the meteor was most considerable, was elevated about 12 degrees, and bore, as I imagined, about 20 degrees westward of the due north. It touched the horizon in the west at the distance of about 65 or 70 degrees

* Mr. Folkes was born at Westminster about the year 1690. At the early age of 24 he was elected a fellow of the Royal Society, to which he proved an active and useful member, and had the honour afterwards to be chosen president of the Society, an honour which he enjoyed many years. Mr. Folkes wrote several papers in the Transactions, in the different Volumes, from the 30th to the 46th. Besides which, he was author of a treatise, in 4to. on the English coins, from the Conquest down to his own time. He died at London in 1754. By his will he bequeathed to the R. S. 200l. and his large cornelian seal ring, on which are engraven the arms of the Society, for the perpetual use of the president.

from the north, whence the whole intercepted arc of the horizon would have been of nearly 100 degrees, had not some few degrees in the east been hid by clouds, which lay between the eye and the meteor.

The seeming black cloud, when I first saw it, ran nearly parallel to the horizon, and at the distance of 6 or 7 degrees: but in about half an hour it changed its figure very much, sinking down in the north to about half its height, and rising in the west nearly as much. Why I principally took notice of this, was that the light issuing from behind it did not change with it, but remained of the same figure, however the cloud approached or receded from different parts of its limb.

There arose at first some streams in the N.N.W. but of no considerable length, few of them passing 5 degrees above the arc; but beginning from behind the seeming cloud, so as to be about 12 degrees high in all. They were pointed at the ends, and nearly vertical to the horizon. At times nothing but the arc was to be seen, and that only resembling a common aurora; and again in an instant, by a sort of tremulous motion, several parts of it would appear converted into a vast number of parallel streams, for the most part very little higher than the arc itself. About 20 minutes before 10, a small part of the arc, almost due north, grew remarkably lighter than the rest, and continued to increase for about half a minute; when there suddenly broke out some very tall streams, of at least 60° high, as I found by one in particular which arose full north, and passing over the pole star itself, reached some degrees beyond it. This was the most remarkable time of the appearance, some such lances, though not so high, immediately shooting out of the place that first of all radiated, as did some more a good way to the east. They were all nearly perpendicular to the horizon, and most of them arose quite from the black substance at bottom; though I saw some few that did not reach so low, appearing as if their lower parts had been broken off. Some of them were full as bright as any I saw the last year (1716), the axes (if I may so call them) of some of the tallest streams coming up very near to the colour of that pale fire we see in some sorts of lightning. About this time the ground westward was all covered with an odd sort of mist, the same from which I remember last year a great many people said there came an ill smell, which I did not at all perceive; however as I remember it to be the very same appearance, I thought it might not be improper just to take notice of it.

About 10, the phenomenon very much decreased, and so continued till after 11, only sending up now and then 2 or 3 streams. At half after 11, it was again pretty much increased, and I saw it again send out some streams almost as considerable as any of the former; the arc still continued, but not so entire; and from what I could judge, its middle was some degrees nearer the north than when I first took notice of it. Till a quarter before 12 the light continually abated, and then I left it; but a watchman, I ordered to bring me an account of it next morning, tells me it continued till towards day-break, but never streamed remarkably after I went away. Though I could not this time see any stars through the black matter at bottom, I am sensible it was not a cloud, though it bore the resemblance of one: for when a real cloud came over any part of it, (as several small ones did) their difference was very conspicuous.

I have since received two letters, one from Wisbeach in the Isle of Ely, the other from within 14 miles of Bath, both which take notice of it, though with no further particulars, than that on Saturday night, they had seen the same light, though not so considerable as in the beginning of March the last year.

Concerning Britain having formerly been a Peninsula. By Dr. Wm. Musgrave, F. R. S. N° 352, p. 589. *Abstracted from the Latin.*

Supposing Britain to have been a Peninsula, Dr. Musgrave proposes to examine, 1. Whether an isthmus or neck of land could not have been washed or worn away; and 2. Whether that between Britain and France really was so. The former is very strenuously denied by the learned Isaac Vossius; there being according to him no cause sufficient for producing such an effect; but the contrary will plainly appear from the following consideration.

And first, from that vast body of water, discharged from the Atlantic ocean into our western sea, and that with such prodigious force, as is rarely found in any other part of the globe; both the shallows and straits of the western sea, increasing almost continually, must cause the waters thus impelled to mount in a surprising manner, and to beat against our supposed isthmus, so as to overflow, wear and wash it away in time. And thus, that Britain should become an island, seems not at all impossible, but on the contrary very probable.

Secondly, the west wind is often so fierce and raging, after acquiring strength in the vast Atlantic ocean, that it is scarcely conceivable, with what fury it attacks the coasts of Britain and France; and it is very well known, that it commonly blows above half the year (which was also observed by Julius Cæsar) and that very violently, especially in autumn; whence our Michaelmas storms; so that should it happen to concur with a strong tide, both the Western and Severn seas would be greatly swelled; this isthmus would be strongly beaten on; and first its surface, consisting of flint and chalk, would be washed away; and that afterwards the remaining part of the isthmus should in the space of 2000 years and upwards, be worn away by the flux and reflux of the tide to 16 fathoms, its present depth, is by no means incredible.

As to the second point, viz. whether this isthmus was really worn away, or not: and 1. That remarkable ridge of land, in the strait itself, shows that the land there was formerly much higher, but being continually washed away by the tides, for some thousands of years, was reduced to the state in which it is at this day; especially, if we consider, that it is a constant and infallible rule, that the more the bottom of the sea is worn or washed by its waters, the more level and even it becomes. 2. The steep white cliffs, consisting of chalk and flint, on the opposite shores of the straits, and answering to each other for 6 miles on each side, plainly show that they were formerly separated by washing away the intermediate earth. 3. The state and condition of that tract of land, called Rumney-Marsh, agrees very well with the supposition of an isthmus: for, while the isthmus remained, it must have been an obstacle to the tides; and consequently have caused the overflowing of Rumney-Marsh, as being a plain low bottom; and that this marsh was formerly sea, appears from its strong bulwark, as also from the teeth and bones of a Hippopotamus, or some other sea-animal, dug up at Chartham in 1668, at 17 feet deep; Phil. Trans. N° 272, 275. But an anchor, dug up thereabouts, shows it very evidently. After the isthmus was broken through, and all obstacles removed, the sea retired from Rumney-Marsh, into its channel; whence what was formerly an æstuary, is now a fertile plain, 20 miles long and 8 broad, and yielding very good pasture for cattle.

Lastly, supposing, that there was formerly an isthmus here, it is very easy to conceive how wolves, and other noxious animals, might come into Britain: whereas on the contrary supposition it will be ridiculous to imagine, that they were transported thither in ships, for the conservation of their species.

Nor is it any objection, that no mention is made of the breaking through of this isthmus in any histories, either by the Latins, Greeks, or any other nation: for how modern is the date of history, compared with that of the world? from the beginning down to the first history now extant, which is that of Herodotus, there are about 3500 years, and from Noah's deluge 1800; but in such a vast space of time, what conjunctions of causes might happen, and what changes might thereby be produced in the earth, is not easy to determine. Yet we must not allow that we have no hints of this event in history; for what is plainer than this passage in Virgil.

——— Penitus toto divisos orbe Britannos.

" Do not ye think (says the learned British Antiquary Jo. Twin de rebus

Albionicis, p. 22,) that the word divisos may import the rending or breaking off one thing from another? And that Virgil knew its signification very well, and was well acquainted with antiquity, and had not forgot himself? On these words Servius says, "Because Britain was formerly joined to the continent." Than which nothing can be more plain, than that the breaking through of this isthmus was known to the ancients.

Therefore, from the whole the Dr. concludes, that Britain was not originally an island, but became such from a Peninsula; and that, as is probable, by the concurrence of some one or other of the more boisterous winds with the tides, and so breaking through the isthmus.

Extracts from Mr. Gascoigne's and Mr. Crabtrie's Letters; proving Mr. Gascoigne to have been the Inventor of the Telescopic Sights of Mathematical Instruments, and not the French. By W. Derham, R.S.S. N° 352, p. 603.

In M. de la Hire's first part of his Tab. Astron. published in 1687, I find an invention, which was undoubtedly our countryman's, Mr. Gascoigne, ascribed to M. Picard, viz. the application of telescopic sights to astronomical instruments. Mr. de la Hire's words are to this effect: "Some few years since M. Picard, a great astronomer, and member of the Paris Academy of Sciences, substituted telescopes instead of plain sights in astronomical instruments, &c." In which words it is not indeed expressly said that M. Picard was the inventor of this method, but only that he applied telescopes. But since it implies that it was M. Picard's invention, and is in fact claimed as such, in M. Auzout's Account of the Telescopic Micrometer, Phil. Trans. N° 21, I think myself bound to do Mr. Gascoigne the justice to assert his right to this invention; because all his papers, which by the late Mr. Towneley's diligence could be picked up, are now, together with Mr. Towneley's own papers, in my hands.

As for the invention of the micrometer, which Mr. Auzout claims as his and M. Picard's, I shall say little to it, Mr. Towneley having sufficiently proved it to be Mr. Gascoigne's, in the Phil. Trans. N° 25. And the descriptions and draughts of that and some other instruments of the kind are now by me, in Mr. Gascoigne's own hand, to confirm Mr. Towneley's account, if it were necessary.

And as Mr. Gascoigne was the first that measured the diameters of the planets, &c. by a micrometer, so I shall prove that he was the first that applied telescopic sights to astronomical instruments. In a long letter to his friend Mr. Crabtrie, of Jan. 25, 1640-1, in which he describes his micrometer, and shows his way of finding the refractions, the moon's parallax, and how he measured the diameters of the planets, Mr. Gascoigne tells him how the measuring glasses, which he had been speaking of, might be applied to a quadrant. "If, says he, here (that is in the distinct base) you place the scale that measures —, or if here a hair be set, that it appear perfectly through the glass —, you may use it in a quadrant, for the finding of the altitude of the least star visible by the perspective wherein it is. If the night be so dark, that the hair or the pointers of the scale be not to be seen, I place a candle in a lantern, so as it cast light sufficient into the glass; which I find very helpful when the moon appears not, or it is not otherwise light enough."

In another letter, dated Christmas-eve 1641, where he describes the wheel work of his micrometer, and shows how he could apply it to the taking of three points; and specifies his observations of the diameters of the sun and moon, and mentions a theory he had contrived of the sun, &c. and what pains he had taken in the anatomy of the eye, he tells Mr. Crabtrie, how he had applied his telescopic sights to a sextant. He says, "Mr. Horrox's Theory of the Moon I shall be shortly furnished to try. For I am fitting my sextant for all manner of observations, by two perspicills with threads. And also I am consulting my workman about the making of wheels like β, γ, δ, ϵ, diagr. 3, to use two glasses like a sector. If I once have my tools in readiness to my desire I shall use them every night. I have fitted my sextant by the help of the cane, two glasses in it and a thread, so as to be a pleasant instrument, could wood and a country joiner or workman please me."

In another letter, the date of which is worn out, but is, in Mr. Crabtrie's hand, called his 10th letter to him, he says, "I have given order for an iron quadrant of 5 feet, which will give me the 1000th part of one degree, which shall be furnished like my first scale; only my workman is so throng* for my father, that I fear it will not be furnished before the eclipse. I have caused a very strong ruler to be exactly made, and intend to fit it with cursors of iron, with glasses in them and a thread, for my sextant."

To these I could have added many other passages of the like nature: but these may be sufficient, to show that Mr. Gascoigne, as early as 1640, made use of telescopes on quadrants and sexants, as well as in his invention of the micrometer.

What commendations these contrivances procured him, and what expectations they raised in some of the astronomers of that time, particularly in two of the most acute of that age, Mr. Horrox and Mr. Crabtrie, may be seen in Mr. Crabtrie's letters to Mr. Gascoigne, which are also in my hands. Some pas-

* A Yorkshire phrase for fully employed.

sages of which I shall recite, and at the same time give the Society a taste of what those curious letters contain.

In Mr. Crabtrie's 2d letter, of Oct. 30, 1640, after a very clear demonstration that the solar spots are not planets at a distance from the sun, but something adhering to, or very near the sun's body; and also after a no less clear demonstration of the errors of Lansberg's Hipparchian Diagram, his Lunar Parallax, his Doctrine of Eclipses, and indeed his whole Lunar Astronomy, with several other curious matters, too many to be specified: after this, I say, Mr. Crabtrie adds, "Something I am sure you were telling me concerning a way of observing the places of the planets by your glasses. But I have not a little lamented that my time cut me so short, when I was with you, that I could not more fully ruminate and digest those strange inventions which you showed me, and told me of. My lassitude after an unexpected and unacquainted journey, my unpreparedness for those cogitations, not intending that journey the day before, and the multiplicity and variety of the novelties you showed me, so wholly distracted my thoughts into admiration, that I cannot now give my meditations any reasonable account of what I saw; but must entreat you, in a few lines, to rub up my memory, and tell me again what you showed me, and the extent of those your inventions. Which I desire, that I might consider, and rejoice to consider, how much and wherein Urania's structure will grow to perfection by your assistance; and that, what in me lies, I may help you to remember when and wherein your inventions and observations will be of most use. I should also desire you to inform me what size of a quadrant you conceive to be large enough for observation with your devices. For I am ere long going to Wigan, 12 miles from hence, where much brass is cast; and then I could see whether I could have such an one cast. You told me, as I remember, you doubted not in time to be able to make observations to seconds. I cannot but admire it, and yet, by what I saw, believe it; but long to have some farther hints of your conceit for that purpose. One means, I think you told me was, by a single glass in a cane, upon the index of your sextant, by which, as I remember, you find the exact point of the sun's rays. But the way how, I have quite forgotten, and much desire. Your device for the exact division of a quadrant, by dividing 11 degrees into 10 parts, I did then understand, but do not now fully remember. If it might not be too much trouble to you, I should intreat you to give me such a paper demonstration thereof as you showed me, and two or three lines plainly for the use thereof, how to find those small parts. I lost the little paper, wherein I noted the moon's diameter, which we observed when I was with you, I pray you send it me, if, &c.

I cannot conceal how much I am transported beyond myself with the remembrance, of that little I do remember, of those admirable inventions which you showed me when I was with you. I should not have believed the world could have afforded such exquisite rarities, and I know not how to stint my longing desires, without some further taste of these selected dainties. Happier had I been had I never known there had been such secrets, than to know no more, but only that there are such. Of all desires the desire of knowledge is most vehement, most impatient: and of all kinds of knowledge, this of the mathematics affects the mind with most intense agitations. I doubt not but you can experimentally witness the truth hereof, and one time or other have been no stranger to such thoughts as mine. And therefore, although modesty would forbid me to request any thing, until you give me leave, but what you please voluntarily to impart, yet the vehemence of my desire forces me to let you know how much I desire, and how highly I should prize any thing that you should be pleased to communicate to me in those optic practices. Could I purchase it with travel, or procure it for gold, I would not long be without a telescope for observing small angles in the heavens, nor want the use of your other device of a glass in a cane upon the moveable ruler of your sextant, as I remember, for helping to the exact point of the sun's rays. But seeing Urania is, &c."

Thus was the most ingenious Mr. Crabtrie transported with Mr. Gascoigne's devices, though at that time far less perfect than they were in a short time after. And no less affected was the incomparable Horrox, as Mr. Crabtrie sets forth, in his third long letter of Dec. 28, 1640, which has these words: "My friend, Mr. Horrox, professes, that little touch which I gave him of your inventions has ravished his mind quite from itself, and left him in an extacy between admiration and amazement. I beseech you, sir, slack not your intentions for the perfecting of your begun wonders. We travel with desire till we hear of your full delivery. You have our votes, our hearts, and our hands should not be wanting, if we could further you." And then, after many curious matters, he thus proceeds: "Your diagrams for perspectives I have viewed again and again, and cannot sufficiently admire your indefatigable industry, and profound ingenuity therein. I am much affected with the symbolical expressions of your demonstrations. I never used them before, but I will do, yet I understand them all at the first sight, and see well the truth of your demonstrations."

To these I shall only add one passage more, and this because it shows some other of Mr. Gascoigne's exquisite contrivances, or at least the accuracy of what are mentioned; and that is in Mr. Crabtrie's letter of Dec. 6, 1641, at the beginning of which he says, "That which you give me a full projection of was above my hope, and if the screws keep an exact equality of motion forward in each revolve, it is a most admirable invention; and with the other accommodations, I had almost said without compare. But that the divisions of a circle should be measured to seconds, without the limb of an instrument, or that distances, altitudes, inclinations, and azimuths should be taken all at one moment, without the limb of an instrument likewise, and each to any required number of parts; or that the diameter of Jupiter should be projected in such prodigious measures as you speak of, &c. were enough to amuse and amaze all the mathematicians in Europe, and may indeed be rather a subject of admiration than belief, to any that has not known your former inventions to exceed vulgar, I had almost said human, abilities. And for my part, I must confess modesty so checks my ambitious desires, that I dare scarcely hope such miracles should ever be produced in real practice to such exactness." Then follows an account of the agreement of Mr. Horrox's Theory of the Moon with Mr. Gascoigne's Observations; and also very curious ratiocinations, and a disquisition about finding the parallax of the sun and moon, and their distance from the earth. In which he censures Morinus's brags, &c. and then says, that "no man that has written of the diagram (of Hipparchus) understood it fully or described it rightly, but only Kepler and our Horrox; for whose immature death [which was suddenly, and about the age of 25] there is yet scarcely a day which I pass without some pang of sorrow."

Thus, among many, I have related some of the passages of Mr. Gascoigne's and Mr. Crabtrie's letters relating to telescopic sights. From whence it is very manifest, that long before the French gentleman's claims, our countryman Mr. Gascoigne had made use of those sights in his astronomical instruments; particularly in two or more sorts of micrometers, as I plainly find, and in his quadrant and sextant. And had it pleased God to have given him a longer life, we might have expected greater things from his pregnant and sagacious wit. For he was scarcely 20 years of age when he held these correspondences with Mr. Crabtrie. And at the age of 23 he was killed at Marston-Moor-Battle, on July 2, 1644, fighting for King Charles I. His father was Henry Gascoigne, Esq. of Middleton, between Leeds and Wakefield.

An Attempt towards the Improvement of the Method of approximating, in the Extraction of the Roots of Equations in Numbers. By Brook Taylor, S.R.S. N° 352, p. 610.

In the Philos. Trans. N° 210, Dr. Halley has published a very compendious and useful method of extracting the roots of affected equations of the common form, in numbers. This method proceeds by assuming the root desired nearly true to one or two places in decimals (which is done by a geometrical construction, or by some other convenient way) and correcting the assumption by comparing the difference between the true root and the assumed, by means of a new equation whose root is that difference, and which he shows how to form from the equation proposed, by the substitution of the value of the root sought, partly in known and partly in unknown terms.

In doing this, he makes use of a table of products (which he calls speculum analyticum,) by which he computes the co-efficients in the new equation for finding the difference mentioned. This table, I observed, was formed in the same manner from the equation proposed, as the fluxions are, taking the root sought for the only flowing quantity, its fluxion for unity, and after every operation dividing the product successively by the numbers 1, 2, 3, 4, &c. Hence I soon found that this method might easily and naturally be drawn from Cor. 2, Prop. 7, of my Methodus Incrementorum, and that it was capable of a further degree of generality; it being applicable, not only to equations of the common form, viz. such as consist of terms in which the powers of the root sought are positive and integral, without any radical sign, but also to all expressions in general, where any thing is proposed as given, which by any known method might be computed; if vice versâ, the root were considered as given: such as are all radical expressions of binomials, trinomials, or of any other nomial, which may be computed by the root given, at least by logarithms, whatever be the index of the power of that nomial; as also expressions of logarithms, of arches by the sines or tangents, of areas of curves by the abscissas, or any other fluents, or roots of fluxional equations, &c.

For the sake of this great generality, it may not be improper to show how this method is derived from the foresaid corollary. Therefore z and x being two flowing quantities, of which the relation to each other may be expressed by any equation whatever; by this corollary, while z by flowing uniformly becomes $z + v$, x will become $x + \frac{\dot{x}}{1.\dot{z}} v + \frac{\ddot{x}}{1.2\dot{z}^2} v^2 + \frac{\dddot{x}}{1.2.3\dot{z}^3} v^3 +$ &c. or $x + \frac{\dot{x}v}{1} + \frac{\ddot{x}v^2}{1 \times 2} + \frac{\dddot{x}v^3}{1.2.3} +$ &c. putting 1 for $\dot{z}$.

Hence if y be the root of any expression formed of y and known quantities, and supposed equal to nothing, and z be a part of y, and x be formed of z and the known quantities, in the same manner as the expression made equal to nothing is formed of y; and let y be equal to $z + v$; then the difference v will be found by extracting the root of this expression

$x + \frac{\dot{x}v}{1} + \frac{\ddot{x}v^2}{1.2} + \frac{\dddot{x}v^3}{1.2.3} +$ &c. $= 0$, For in this case z being become $z +$

$v = y, x$, which is now become $x + \dot{x}v + \frac{\ddot{x}v^2}{2} +$ &c. must become equal to nothing.

The root v in the equation $x + \frac{\dot{x}v}{1} + \frac{\ddot{x}v^2}{1.2} + \frac{\dddot{x}v^3}{1.2.3} +$ &c. $= 0$, is to be found on the supposition of its being very small with respect to z, as it must be, if z be taken tolerably exact; by which means the terms $\frac{\dddot{x}v^3}{1.2.3} + \frac{\ddddot{x}v4}{1.2.3.4}$ + &c. may be neglected, on account of their smallness with respect to the other terms, so as to leave the equation $x + \frac{\dot{x}v}{1} + \frac{\ddot{x}v^2}{1.2} = 0$, for finding the first approximation of v.

By extracting the root of this equation, we have $v = \sqrt{\frac{\dot{x}^2}{\ddot{x}^2} - \frac{2x}{\ddot{x}}} - \frac{\dot{x}}{\ddot{x}}$. That is,

First, $\sqrt{\frac{\dot{x}^2}{\ddot{x}^2} - \frac{2x}{\ddot{x}}} - \frac{\dot{x}}{\ddot{x}}$, if $x + \dot{x}v + \frac{\ddot{x}v^2}{2} = 0$.

2d, $\sqrt{\frac{\dot{x}^2}{\ddot{x}^2} + \frac{2x}{\ddot{x}}} - \frac{\dot{x}}{\ddot{x}}$, if $- x + \dot{x}v = \frac{\ddot{x}v^2}{2} = 0$.

3d, $\frac{\dot{x}}{\ddot{x}} - \sqrt{\frac{\dot{x}^2}{\ddot{x}^2} - \frac{2x}{\ddot{x}}}$, if $x - \dot{x}v + \frac{\ddot{x}v^2}{2}$, &c. $= 0$.

4th, $\frac{\dot{x}}{\ddot{x}} - \sqrt{\frac{\dot{x}^2}{\ddot{x}^2} + \frac{2x}{\ddot{x}}}$, if $- x - \dot{x}v + \frac{\ddot{x}v^2}{2}$, &c. $= 0$.

This approximation gives v exact to twice as many places as there are true figures in z, and therefore triples the number of true figures in the expression of y by z + v, which may be taken for a new value of z, for computing a second v, seeking other values of x, $\dot{x}$, $\ddot{x}$, &c. Though when z is tolerably exact (which it may be esteemed when it contains two or three or more true figures in the value of y, according to the number of figures the root is proposed to be computed to,) the calculation may be restored without so much trouble, only by taking $\sqrt{\frac{\dot{x}^2}{\ddot{x}^2} \pm \frac{2x}{\ddot{x}} - \frac{2\dddot{x}}{2.3\ddot{x}}v^3 - \frac{2\ddddot{x}}{1.2.3.4\ddot{x}}v^4}$ &c. instead of $\sqrt{\frac{\dot{x}^2}{\ddot{x}^2} \pm \frac{2x}{\ddot{x}}}$; taking every time for v its value last computed.

From the same equation $x + \dot{x}v + \frac{\ddot{x}v}{2} + \frac{\dddot{x}v^3}{1.2.3} +$ &c. $= 0$, may be gathered also a rational form, viz. $v = \frac{-x}{\dot{x} - \frac{x\ddot{x}}{2\dot{x}}}$. For, neglecting the terms $\frac{\dddot{x}v^3}{1.2.3}$ &c. we have $v = \frac{-x}{\dot{x} + \frac{1}{2}\ddot{x}v}$ which is nearly $= \frac{-x}{\dot{x}}$. Therefore in the divisor, instead of v writing $\frac{-x}{\dot{x}}$, we have more exactly $v = \frac{-x}{\dot{x} - \frac{x\ddot{x}}{2\dot{x}}}$, that is

1st, $\dfrac{-x}{\dot{x} - \dfrac{\ddot{x}x}{2\dot{x}}}$, when $x + \dot{x}v + \dfrac{\ddot{x}v^2}{2}$ &c. $= 0$.

2d, $\dfrac{x}{\dot{x} + \dfrac{\ddot{x}x}{2\dot{x}}}$, when $-x + \dot{x}v + \dfrac{\ddot{x}v^2}{2}$ &c. $= 0$.

3d, $\dfrac{x}{\dot{x} - \dfrac{\ddot{x}x}{2\dot{x}}}$, when $x - \dot{x}v + \dfrac{\ddot{x}v^2}{2}$ &c. $= 0$.

4th, $\dfrac{-x}{\dot{x} + \dfrac{\ddot{x}x}{2\dot{x}}}$, when $-x - \dot{x}v + \dfrac{\ddot{x}v^2}{2}$ &c. $= 0$.

This formula will also triple the number of true figures in z. And the calculation may be repeated, after every operation, taking for a divisor $\dot{x} \pm \frac{\ddot{x}}{2}v + \frac{\dddot{x}v^2}{1.2.3} + \frac{\ddddot{x}v^3}{1.2.3.4} +$ &c. instead of $x + \frac{\ddot{x}x}{2\dot{x}}$.

Dr. Halley has fully explained the manner of using both these formulas in equations of the common form; wherefore I shall be the shorter in explaining two or three examples of another sort.

Ex. 1. Let it be proposed to find the root of this equation $\overline{y^2 + 1}|^{\sqrt{2}} + y - 16 = 0$. In this case, for y writing z, and for 0 writing x, we have $\overline{z^2 + 1}|^{\sqrt{2}} + z - 16 = x$. Whence, by taking the fluxions, we have $\dot{x} = 2\sqrt{2} \times z \times \overline{z^2 + 1}|^{\sqrt{2}-1} + 1$, and $\ddot{x} = 2\sqrt{2} \times \overline{8 - 4\sqrt{2}}\, z^2 \times \overline{z^2 + 1}|^{\sqrt{2}-2}$. For finding the first figures of the root y, for $\sqrt{2}$ take $\frac{3}{2}$, and we have the equation $\overline{y^2 + 1}|^{\frac{3}{2}} + y - 16 = 0$, which being expanded gives $y^6 + 3y^4 + 2y^2 + 32y - 255 = 0$.

By this equation I find that for the first supposition we may take $z = 2$. Therefore in order to find v, let us now make $\sqrt{2} = \frac{7}{5}$, (which is nearer than before) and we have $x = \overline{z^2 + 1}|^{\frac{7}{5}} + z - 16 = \overline{2^2 + 1}|^{\frac{7}{5}} - 14 = 5^{\frac{7}{5}} - 14 = -4{,}48$; $\dot{x} = 10{,}66$; $\ddot{x} = 4{,}72$. Whence by the second rational

$$\text{form } v = \cfrac{4{,}448}{10{,}66 + \cfrac{4{,}72 \times 4{,}48}{2 \times 10{,}66}}$$

$= 0{,}38$; which must be too large, because $\frac{7}{5} < \sqrt{2}$, and therefore will require a larger value of y to exhaust the equation, than where $\sqrt{2}$ is exact. For the second supposition therefore, let us take $z = 2{,}3$, and make $\sqrt{2} = 1{,}4142136$, and by help of the logarithms we shall have $\overline{z^2 + 1}|^{\sqrt{2}} = 13{,}47294$, whence $x = -0{,}22706$; $\dot{x} = 14{,}93429$, and $\ddot{x} = 5{,}18419$.

Hence by the 2d irrational formula, $v = \sqrt{\frac{14,93429^2}{5,18419^2} + \frac{0,45412}{5,18419}} - \frac{14,93429}{5,18419} =$ 0,01516; which gives $y = z + v = 2,31516$, which is true to six places. If you desire it more exact than to the extent of the tables of logarithms, taking $z = 2,31516$ for the next supposition, the calculation must be repeated by computing $\overline{zz + 1}|^{\sqrt{2}}$ to a sufficient number of places; which must be done by the binomial series, or by making a logarithm on purpose, true to as many places as are necessary.

Exam. 2.—For another example, let it be required to find the number whose logarithm is 0,29, supposing we had no other table of logarithms but Mr. Sharp's, of 200 logarithms to a great many places. This amounts to the resolving this equation $l.y = 0,29$, or $ly - 0,29 = 0$. Hence therefore we have $x = lx - 0,29$, $\dot{x} = \frac{a}{z}$, (a being the modulus belonging to the table we use, viz. 0,4342944819 &c.) $\ddot{x} = \frac{-a}{z^2}$, $\dddot{x} = \frac{2a}{z^3}$, $\ddddot{x} = \frac{-6a}{z^4}$, &c. In this case, because $\dot{x}$ has a negative sign, changing the signs of all the co-efficients, the canon for v will be found in the 4th case, which in the irrational form gives $v = \frac{\dot{x}}{\ddot{x}} - \sqrt{\frac{\dot{x}^2}{\ddot{x}^2} + \frac{2x}{\ddot{x}} - \frac{2\dddot{x}}{2.3\ddot{x}}v^3 - \frac{2\ddddot{x}}{2.3.4\ddot{x}}v^4 \text{ \&c.}} = z - \sqrt{z^2 + \frac{2l.z - 0,58}{a} \times z^2 + \frac{2v^3}{3z} - \frac{2v^4}{4z^2} + \frac{2v^5}{5z^3} \text{ \&c.}}$ In this case, to avoid often dividing by z, it will be most convenient to compute $\frac{v}{z}$, which is got from this equation $\frac{v}{z} = 1 - \sqrt{1 + \frac{2l.z - 0,58}{a} + \frac{2v^3}{3z^3} - \frac{2v^4}{4z^4} + \frac{2v^5}{5z^5} \text{ \&c.}}$ The nearest logarithm, in the tables proposed, to the proposed logarithm 0,29, is 0,2900346114, its number being 1,95. Therefore for the first supposition taking $z = 1,95$, we have x ($= l.z - 0,29 = 0,2900346114 - 0,29$) $=$ 0,0000346114, and $\frac{2l.z - 0,58}{a} = \frac{0,0000692228}{0,4342944819} = 0,00015939139$, and $1 + \frac{2l.z - 0,58}{a} = 1,00015939139$. Whence for the first approximation we have $\frac{v}{z} = 1 - \sqrt{1,00015939139} = -0,00007969247$, and $v = -0,00015540032$, and $y = z + v = 1,94984459968$. Which is true to eleven places, and may easily be corrected by the terms $\frac{2v^3}{3z}$ &c. which I leave to the readers curiosity.

Being upon the subject of approximations, it may not be amiss to set down here two approximations I have formerly hit upon. The one is a series of terms for expressing the root of any quadratic equation: and the other is a particular method of approximating in the invention of logarithms, which has no occasion for any of the transcendental methods, and is expeditious enough for making the tables without much trouble.

A general Series for expressing the Root of any Quadratic Equation.—Any quadratic equation being reduced to this form $xx - mqx + my = 0$, the root x will be expressed by this series of terms.

$$x = \frac{y}{q} + \text{A} \times \frac{1}{\frac{mq^2}{y} - 2} + \text{B} \times \frac{1}{a^2 - 2} + \text{C} \times \frac{1}{b^2 - 2} + \text{D} \times \frac{1}{c^2 - 2} \text{ \&c.}$$

Which must be thus interpreted.

1. The capital letters A, B, C, &c. stand for the whole terms with their signs, preceding those wherein they are found, as $\text{B} = \text{A} \times \frac{1}{\frac{mq^2}{y} - 2}$.

2. The little letters a, b, c, &c. in the divisors, are equal to the whole divisors of the fraction in the terms immediately preceding; thus $b = a^2 - 2$.

For an example of this, let it be required to find $\sqrt{2}$. Putting $\sqrt{2} = x + 1$, we have $x^2 + 2x - 1 = 0$, which being compared with the general formula, gives $mq = -2$, and $my = -1$: therefore for m taking -1, we have $q = 2$, and $y = 1$, which values substituted in the series give $x = \frac{1}{2} - \frac{1}{2 \times 6} - \frac{1}{2 \times 6 \times 34} - \frac{1}{2 \times 6 \times 34 \times 1154} - \frac{1}{2 \times 6 \times 34 \times 1154 \times 1331714}$ &c. The fractions here written down giving the root true to 23 places.

A new Method of computing Logarithms.—This method is founded on these considerations.

1. That the sum of the logarithms of any two numbers is the logarithm of the product of those two numbers multiplied together.

2. That the logarithm of unit is nothing; and consequently that the nearer any number is to unit, the nearer will its logarithm be to 0. 3dly, That the product by multiplication of two numbers, whereof one is larger, and the other less than unit, is nearer to unit than that of the two numbers which is on the same side of unit with itself; for example the two numbers being $\frac{2}{3}$ and $\frac{4}{3}$, the product $\frac{8}{9}$ is less than unit, but nearer to it than $\frac{2}{3}$, which is also less than unit. On these considerations, I found the present approximation; which will be best explained by an example. Let it therefore be proposed to find the relation of the logarithms of 2 and 10. In order to this, I take two fractions $\frac{128}{100}$ and $\frac{8}{10}$, viz. $\frac{2^7}{10^2}$ and $\frac{2^3}{10^1}$, whose numerators are powers of 2, and their denominators powers of 10; one of them being greater, and the other less than 1. Having set these down in decimal fractions in the first column of the following

table, against them in the second column I set A and B for their logarithms, expressing by an equation the manner how they are compounded of the logarithms of 2 and 10, for which I write $l2$ and $l10$. Then multiplying the two numbers in the first column together, I have a third number 1.024, against which I write C for its logarithm, expressing likewise by an equation in what manner C is formed of the foregoing logarithms A and B. And in the same manner the calculation is continued; only observing this compendium, that before multiplying the last two numbers already got in the table, I consider what power of one of them must be used to bring the product the nearest to unit that can be. This is found, after we have gone a little way in the table, only by dividing the differences of the numbers from unit one by the other, and taking the quotient with the nearest, for the index of the power wanted. Thus the last two numbers in the table being 0.8 and 1.024, their differences from unit are 0.200 and 0.024; therefore $\frac{0.200}{0.024}$ gives 9 for the index; therefore multiplying the 9th power of 1.024 by 0.8, I have the next number 0.990352031429, whose logarithm is $D = 9C + B$. In seeking the index in this manner by division of the differences, the quotient ought generally to be taken with the least: but in the present case it happens to be the most, because instead of the difference between 0.8 and 1, we ought strictly to have taken the difference between the reciprocal 1.25 and 1, which would have given the index 10; and that would be too great, because the product by that means would have been larger than 1, as 1.024 is. Whereas this approximation requires that the numbers in the first column be alternately greater and less than 1, as may be seen in the table.

When I have in this manner continued the calculation, till I have got the numbers small enough, I suppose the last logarithm to be equal to nothing. Which gives me an equation, from which having got away the letters by means of the foregoing equations, I have the relation of the logarithms proposed. In this manner if I suppose $G = 0$, I have $2136\, l2 - 643\, l10 = 0$: which gives the logarithm of 2 true in 7 figures, and too great in the 8th; which happens because the number corresponding with G is greater than unit.

There is another expedient which renders this calculation still shorter. It is founded on this consideration, that when x is very small, $\overline{1+x}|^{n}$ is very nearly $1 + nx$. Hence if $1 + x$, and $1 - z$ be the last two numbers already gotten in the first column of the table, and their powers $\overline{1+x}|^{m}$ and $\overline{1-z}|^{n}$ be such as will make the product $\overline{1+x}|^{m} \times \overline{1+z}|^{n}$ very near to unit, m and n may be found thus: $\overline{1+x}|^{m} = 1 + mx$, and $\overline{1-z}|^{n} = 1 - nz$, consequently $\overline{1+x}|^{m} \times \overline{1-z}|^{n} = 1 + mx - nz - mnzx$, or (neglecting $mnzx$) $1 + mx$

$- nz$. Make this equal to 1, and we have $m : n :: z : x :: l.\overline{1 - z} : l.\overline{1 + x}$. Whence $xl.\overline{1 - z} + z\,l.\overline{1 + x} = 0$. To give an example of the application of this, let 1.024 and 0.990352 be the last numbers in the table; their logarithms being C and D. Then we have $1.024 = 1 + x$, and $0.990352 = 1 - z$, consequently $x = 0.024$, and $z = 0.009648$. Whence the ratio $\frac{z}{x}$ in the least numbers is $\frac{201}{500}$. So that for finding the logarithms proposed we may have $500\text{D} + 201\text{C} = 48510\,l\,2 - 14603\,l\,10 = 0$, which gives $l\,2 = 0.3010307$, which is too great in the last figure; but it is nearer the truth, than what is got from the logarithm F supposed equal to nothing. So that by this means we have saved 4 multiplications, which were necessary to find the number 9989595 &c. correspondent to F, and which must have been had if we would make the logarithm true to the same number of places without this compendium.

1.280000000000	$\text{A} = 7\,l\,2 - 2\,l\,10$	$l\,2 > 0.28$
0.800000000000	$\text{B} = 3\,l\,2 - l\,10$	< 0.33
1.024000000000	$\text{C} = \text{B} + \text{A} = 10\,l\,2 - 3\,l\,10$	> 0.300
0.990352031429	$\text{D} = 9\text{C} + \text{B} = 93\,l\,2 - 28\,l\,10$	< 0.30107
1.004336277664	$\text{E} = 2\text{D} + \text{C} = 196\,l\,2 - 59\,l\,10$	> 0.301020
0.998959536107	$\text{F} = 2\text{E} + \text{D} = 485\,l\,2 - 146\,l\,10$	< 0.3010309
1.000162894165	$\text{G} = 4\text{F} + \text{E} = 2136\,l\,2 - 643\,l\,10$	> 0.30102996
0.999936281874	$\text{H} = 6\text{G} + \text{F} = 13301\,l\,2 - 4004\,l\,10$	< 0.301029997
1.000035441215	$\text{I} = 2\text{H} + \text{G} = 28738\,l\,2 - 8651\,l\,10$	> 0.3010299951
0.999971720830	$\text{K} = \text{I} + \text{H} = 42039\,l\,2 - 12655\,l\,10$	< 0.3010299959
1.000007161046	$\text{L} = \text{K} + \text{I} = 70777\,l\,2 - 21306\,l\,10$	> 0.30102999562
0.999993203514	$\text{M} = 3\text{L} + \text{K} = 254370\,l\,2 - 76573\,l\,10$	< 0.30102999567
1.000000364511	$\text{N} = \text{M} + \text{L} = 325147\,l\,2 - 97879\,l\,10$	> 0.3010299956635
0.999999764687	$\text{O} = 18\text{N} + \text{M} = 6107016\,l\,2 - 1838395\,l\,10$	< 0.3010299956640
Com. Ar. 235313		
$0 = 364511\,\text{O} + 235313\,\text{N} = 2302585825187\,l\,2 - 693147400972\,l\,10$		> 0.301029995663987

I have computed this table so far, that the reader may see in what manner this method approximates; this whole work, as it appears, costing a little more than 3 hours time.

Some Simple Properties of the Conic Sections deduced from the Nature of the Foci; with General Theorems of Centripetal Forces; by means of which the Law of the Centripetal Forces tending to the Foci of the Sections, the Velocities of Bodies revolving in them, and the Description of the Orbits, may be easily determined. By Abr. Demoivre. N° 352, p. 622.

This paper may be seen in the author's Miscellanea Analytica, p. 233, &c.; into which it was from hence copied, much enlarged and improved.

On the Dissection of a Child much emaciated. By Dr. Patrick Blair, R. S. S. N° 353, p. 631.

This child was 5 months old, and was so emaciated, that it appeared rather to have decreased, than to have increased in bulk, from the time of its birth; its whole body not weighing above 5 lbs. The skin and muscles of the abdomen were very thin, but the peritonæum was preternaturally thick. The ventriculus was more like an intestine than a stomach, its length being 5 inches, and its breadth only 1 inch. Its coats were thick and fleshy, and the cavity very inconsiderable. The pylorus, and almost half of the duodenum were cartilaginous, and rather inclining to an ossification, so that no nourishment could have passed into the intestines, though the stomach had been capable of containing it; whence it is no wonder that the body was so emaciated. There were scarcely any traces of the omentum to be seen, even at the bottom of the stomach, to which it usually adheres. The right lobe of the lungs adhered firmly to the ribs, and had 3 exulcerations, which contained purulent matter. It was so very thin and compact, that it seemed as if that lobe had never been of use in respiration. The left lobe was of a more florid red, spongy, and free from any adhesion.

On inquiring into the symptoms this child had been affected with, the mother told me, it seemed to be healthy till about a month old, when it was seized with a violent vomiting, and a stoppage of urine and stool. Some time after, both these became more regular, but the vomiting still continued. It seemed to have a great appetite, taking what suck, drink, or other food was offered it, with a kind of eagerness; but immediately threw it all up again. It had all along breathed freely, and had no cough, notwithstanding the exulcerations above-mentioned. This confirmed me in the opinion that it had never breathed by the right lobe of the lungs.

There could be nothing more emaciated than this child was; and it seems to be worth considering, whether its illness might not be owing in a great measure to the want of the omentum, for it seemed never to have had any; as also, whence it is that this part is generally consumed in an atrophy, and in most hydropical cases, except where itself is more especially concerned.

A Treatise on Infinite Series; Part first. By Peter Remund de Monmort, F. R. S. To which is added an Appendix, in which several parts are treated in a different Way. By Dr. Brook Taylor, Sec. R. S.* N° 353, p. 633.

This is the first part of a treatise on the Summation of Infinite Series, the remainder of which it does not appear was ever published. Indeed it seems to be fitter for a separate book, than for a paper in the Philos. Trans. And most of its contents occur in other works; as those of Ja. Bernoulli, Dr. Taylor, Demoivre, Sterling, &c.

†*On the Advantages that may accrue from the Observation of the Moon's frequent Appulses to the Hyades. Or, on the usefulness of observing the Occultations of the Fixed Stars by the Moon for finding the Longitude.* N° 354, p. 692.

Of all the methods hitherto proposed for finding the longitudes of places, for geographical uses, none seems more adapted to the purpose, than that by the occultations of the fixed stars by the moon, observed in distant parts: for those immersions of the stars which happen on the dark semicircle of the moon, and their emersions from the same, are perfectly momentaneous, without that ambiguity to which the observations of the eclipses of the moon, and those of Jupiter's satellites, are subject. Besides, while the moon is horned, and her weaker light less dazzling, an ordinary short telescope, such as by experience is found to be manageable on ship board, suffices to observe those moments, even in the occultations of very minute stars: on which account, this way seems to bid fairest for the desired solution of the grand problem of finding the longitude at sea. But since it would be needless to inquire exactly what longitude a ship is in, when that of the port to which she is bound is still unknown, it were to be wished that the princes of the earth would cause such observations to be made, in the ports and on the principle head-lands of their dominions, each for his own, as might once for all settle truly the limits of the land and sea. This work however being likely to be left to the care and curiosity of private persons, it may not be amiss here to give notice of the present opportunity of performing it, in this our northern hemisphere, by help of the frequent appulses of the moon to the more southerly of the hyades, many of which she eclipses in each monthly revolution, and will continue so to do, during the years 1718, 1719, and 1720.

* A French mathematician, and author of a treatise on Chances, entitled Essay d'Analyse sur les Jeux de Hazard, in 4to, 1708.

† This paper is probably by Dr. Halley.

These stars are only 3 or 4 in all former catalogues, but the British of Mr. Flamsteed increases them to 16; to them we have added 3 others, somewhat smaller. If the times of the occultations of any one of these stars, or even of any two of them in the same night, be accurately observed under distant meridians, the difference of those meridians may thence be truly obtained, especially since the moon's parallax, and all other requisite parts of her theory, are at present sufficiently stated and known.

Solution of a Problem, lately proposed by M. Leibnitz to the English Geometricians. By Dr. B. Taylor, Secr. R. S. N° 354, p. 695. *Translated from the Latin.*

Though the late M. Leibnitz, in the controversy about the inventor of the method of fluxions, which he chooses to call the differential method, and obstinately to appropriate the invention to himself, has given no answer to those arguments which are alleged in favour of Mr. Newton as the inventor; yet by his encouragement Mr. John Bernoulli has proposed a problem to be solved by the English geometricians. But whether the problem be solved by them or not, it can be no argument against the right of Mr. Newton. However, that they may not take occasion to triumph on this problem not being attempted by the English, I venture to give my solution, such as it is, though the problem is nowise remarkable either for its use or difficulty.

The problem at first proposed by M. Leibnitz, was understood to mean nothing more than that conic hyperbolas, described with the same centre and vertices, should be cut at right angles. But when he was informed that this case had been immediately solved by some Englishmen, he replied, that it was not the solution of a particular case, but a general solution that was required. For which reason those particular solutions were not published, though in the Philos. Trans. N°. 347, a very general solution appeared. But M. Leibnitz and his partisans were not content with this, but seemed rather to despise it, as if the author was not able to apply it to any particular case. But if they could not perceive how equations were to be deduced from it, that is to be imputed to their own unskilfulness. A little before the death of M. Leibnitz, the following problem at last came out; which may be solved after different manners, by pursuing the steps of the general solution just mentioned; but at present we shall solve it in the following manner:

THE PROBLEM. "On the right line AG, fig. 7, pl. 8, as an axis, from the point A to draw an infinite number of curves, as ABD, of such a nature, that the radii of curvature BO, drawn from every point B, may be cut by the axis AG, in C, in a given ratio, or so that it may be BO : BC :: 1 : n. Then are to be constructed the trajectories EBF, cutting the former curve AB at right angles."

First part of the Solution, viz. to find the Curves ABD *to be cut.*—1. Drawing the ordinate BH perpendicular to the axis AG, make the absciss AH = z, the ordinate HB = x, and the curve AB = v: then, by the direct method of fluxions, BC = $\frac{\dot{v}}{\dot{z}}x$; and, v flowing uniformly, BO = $\frac{v\dot{x}}{\dot{z}}$. Hence, by the conditions of the problem, BO $(\frac{v\dot{x}}{\dot{z}})$: BC $(\frac{\dot{v}x}{\dot{z}})$ :: 1 : n; therefore $\dot{z}x - n\dot{z}\dot{x} = o$.

2. Comparing this equation with the second formula of fluxions, at the end of prop. 6 of the method of increments, it gives $\dot{z}x^{-n} = \dot{v}\alpha^{-n}$; α being a given line, by the value of which the curve ABD may be accommodated to any condition annexed to the problem.

3. For $\dot{v}$ writing its value $\sqrt{\dot{x}^2 + \dot{z}^2}$, the last equation gives $\dot{z} = \frac{\dot{x}x^n}{\sqrt{\alpha^{2v} - x^{2n}}}$. Hence will be known z from x being given, by the quadrature of the curve to the absciss x and ordinate $\frac{x^n}{\sqrt{\alpha^{2n} - x^{2n}}}$.

4. Let σ and τ be integer numbers, either affirmative or negative, such as that the simplest of the curves produced in this manner, may be that whose absciss is y, and ordinate $y^{\frac{1-n+2\sigma n}{2n}} \times \overline{\alpha - y}|^{\tau - \frac{1}{2}}$; then it will be the simplest of all the curves, by the quadrature of which the absciss z will be given from the given ordinate x.

5. The curve ABD is always geometrical, when n is assumed equal to the reciprocal of any odd number.

6. Hitherto we have considered the curve ABD as concave towards its axis AG, in which case the greatest ordinate x is equal to the given right line α, which may be conveniently called the parameter of the curve. And in this case the curve actually meets the axis. Hence the fluent of $\frac{\dot{x}x^n}{\sqrt{\alpha^{2n} - x^{2n}}}$, properly taken, viz. so as that z and x may vanish together, the curve will pass through the given point A, as the problem requires.

7. But if a curve ABD be required, which may be convex towards the axis; then in the same manner we shall come to the equation $\dot{z} = \frac{\alpha^n \dot{x}}{\sqrt{x^{2n} - \alpha^{2n}}}$; which may also be derived from the former equation by changing the sign of n. And in this case the curve ABD is geometrical whenever n is assumed the reciprocal of any even number. In which case the least ordinate x is equal to the parameter α; and therefore the curve no where meets the axis. Consequently the curve is limitted to the former case.

8. From the foregoing it is easily collected, that all the curves ABD are similar, and similarly posited about the given point A, their like sides being proportional to the parameters α.

Solution of the 2d Part, viz. the finding the cutting Curves.

9. From art. 2, it is $\dot{v} : \dot{z} :: \alpha^n : x$. But BC : BH :: $\dot{v} : \dot{z}$. Hence BC : BH :: $\alpha^n : x^n$. But, by the condition of the problem, BC is a tangent to the required curve EBF. Therefore if we now take AH (z) and BH (x) for co-ordinates of the curve EBF, the curve itself EB being called r; then, by the direct method of fluxions, $\dot{r} : -\dot{x}$ :: (BC : BH ::) $\alpha^n : x^n$. Hence $\frac{x^n}{\alpha^n} = \frac{-\dot{x}}{\dot{r}}$.

10. In the curve ABD suppose the equation $\dot{z} = \frac{\dot{x}x^n}{\sqrt{\alpha^{2n} - x^{2n}}}$ to be transformed into the equation $\dot{z} = \text{A}\dot{x}\frac{x^n}{\alpha^n} + \text{B}\dot{x}\frac{x^{3n}}{\alpha^{3n}} + \&c.$ not affected by radical signs. Then, by returning to the fluents, $z = \frac{1}{n+1}\text{A}\frac{x^{n+1}}{\alpha^n} + \frac{1}{3n+1}\text{B}\frac{x^{3n+1}}{\alpha^{3n}} + \frac{1}{5n+1}\text{C}\frac{x^{5n+1}}{\alpha^{5n}} + \&c.$ where no new coefficient is introduced, because, by the conditon of the problem, x and z must begin together. Here, instead of $\frac{x^n}{\alpha^n}$ substituting its value $\frac{-\dot{x}}{\dot{r}}$ by art. 9, then $z = \frac{1}{n+1}\text{A}x\frac{-\dot{x}}{\dot{r}} + \frac{1}{3n+1}\text{B}x\frac{-\dot{x}^3}{\dot{r}^3} +$ &c. which is a fluxional equation of the first order belonging to the required curve EBF. And this is reduced to a simpler form in finite terms in the following manner.

11. Let r flow uniformly, and a being a constant quantity, make $\frac{-\dot{x}}{\dot{r}} = \frac{s^n}{d^n}$; then this value of $\frac{-\dot{x}}{\dot{r}}$ being substituted in the equation last found, and multiplying the equation by $\frac{s}{x}$, it will be transformed into this, $\frac{zs}{x} = \frac{1}{n+1}\text{A}\frac{s^{n+1}}{d^n} + \frac{1}{3n+1}\text{B}\frac{s^{3n+1}}{d^{3n}} + \&c.$ Hence, taking the fluxions, $\frac{s\dot{z}x + \dot{s}zx - sz\dot{x}}{x^2} = \text{A}\dot{s}\frac{s^n}{d^n} + \text{B}\dot{s}\frac{s^{3n}}{d^{3n}} \&c. = \frac{\dot{s}s^n}{\sqrt{d^{2n} - s^{2n}}}$. This last appears from the analogy of the two series $\text{A}\dot{x}\frac{x^n}{a^n} + \&c.$ and $\text{A}\dot{s}\frac{s^n}{d} + \&c.$ Hence, substituting for s and $\dot{s}$ their values, derived from the equation $\frac{-\dot{x}}{\dot{r}} = \frac{s^n}{d^n}$, there will arise the equation $n\dot{x}^2\dot{z}z - \ddot{x}x\dot{z}z - n\dot{x}x\dot{z}^2 - \ddot{x}\dot{x}x^2 = 0$; which is reduced to first fluxions in the following manner.

12. In the last term $-\ddot{x}\dot{x}x^2$, instead of $\ddot{x}\dot{x}$ writing its value $-\ddot{z}\dot{z}$, and then dividing the equation by $\dot{z}$, there arises $n\dot{x}^2z - \ddot{x}xz - n\dot{x}x\dot{z} + xx\ddot{z} = 0$. Which equation multiplied by x^{-n-1}, is the fluxion of the equation $-\dot{x}x^{-n}\ z + x^n\dot{z} = d^{1-n}\dot{r}$, the quantities d and $\dot{r}$ being constant. Therefore this equation, or

$\overline{\dot{z}x - z\dot{x}} \times d^{n-1} = \dot{r}x^n$, is a fluxional equation of the first order belonging to the required curve EBF.

13. And in this equation d is the value of the ordinate BH, when the point H falls in the point A.

14. It will not be very easy, while n continues to be general, to bring this equation to fluents, or to the quadrature of curves. But the points of the curve EBF may be conveniently found by the description of the curve ABD, and of a certain geometrical curve; by geometrical, understanding a curve having in its equation no fluxions, nor fluents in the indices of the powers. For let the curve ABD, whose parameter is a, be cut in B by the geometrical curve whose equation is $da^n x^n - zd^n x^n = xd^n \sqrt{d^{2n} - x^{2n}}$; then that point of intersection B will be in one of the trajectories sought, viz. which passes through the point E; AE being $= a$, and perp. to AG.

15. Hence, if ABD be a geometrical curve, then EBF will be geometrical also.

Scholium.—The equation $\overline{\dot{z}x - z\dot{x}} \times d^{n-1} = \dot{r}x^n$ may be found in another way. For by a certain analysis, which at present I think fit to conceal, I have found the equation $\frac{\dot{a}}{a} = \frac{r\dot{r}}{\dot{z}z + \dot{x}x}$. This being compared with the equation $\frac{x^n}{a^n} = \frac{-\dot{x}}{\dot{r}}$ (in art. 9) by eliminating a and $\dot{a}$, we at last arrive at the foregoing equation $\overline{\dot{z}x - z\dot{x}} \times d^{n-1} = \dot{r}x^n$.

Example.—A very simple example may suffice to prove the truth of this solution. Thus make $n = 1$, in which case ABD is a semicircle on the diameter AG, and EBF also a semicircle on the diameter AE. Now in this case $\frac{\dot{x}x^n}{\sqrt{a^{2n} - x^{2n}}} = \frac{\dot{x}x}{\sqrt{a^2 - x^2}}$. Hence, in art. 3, it is $\dot{z} = \frac{\dot{x}x}{\sqrt{a^2 - x^2}}$; therefore $z = a - \sqrt{a^2 - x^2}$, an equation to the circle on the diameter AG $= a$, as it ought to be. Again for n writing 1, the equation $\overline{\dot{z}x - z\dot{x}} \times d^{n-1} = \dot{r}x^n$ (art. 12) becomes $\dot{z}x - z\dot{x} = \dot{r}x$. Hence, exterminating $\dot{r}$ by help of the equation $r\dot{r} = x\dot{x} + z\dot{z}$, it is $\frac{2\dot{z}zx - \dot{x}z^2}{x^2} = -\dot{x}$; therefore reverting to the fluents, $\frac{zz}{x} = -x + a$, an equation to the circle on the diameter AE $= a$, as it ought to be.

Of a Roman Inscription, lately dug up in the North of England. By Chr. Hunter, of Durham, M. D. N° 354, p. 701.

The inscription represented fig. 8, pl. 8, was dug up 2 years since in the Roman castrum, near Lancaster; it is very legible, and gives reason to hope that a search after the first fortifying this place will not be unsuccessful, especially, being able to fix the time of Gordian's repairing this fortress to the 243d year of Christ. We may reasonably ascribe its foundation to the prudent administration of Julius Agricola, in the reign of Fl. Vespasian, about 169 years before.

In the second year of the Emperor Claudius, Anno Dom. 44, the Romans invaded Britain, under the command of Aulus Plautius, in which expedition Vespasian, (Suetonius, Vespasian, cap. 4,) then legate of the second legion, made a conspicuous figure; having been engaged in 30 battles, and reduced two powerful provinces, above 20 towns, and the Isle of Wight. All these successes could not frighten the natives into an entire submission; especially as no progress was made into the country of the Brigantes, till the advancement of Vespasian to the imperial throne, about 26 years after, Anno Dom. 70. Then the whole empire was delivered from the miseries of Nero's, and the short but lamentable devastations of the three succeeding reigns, Vespasian then, resolved to push on his further conquests in Britain, sent over choice armies, commanded by experienced generals; and the 20th legion, which having in the preceding troubles acted seditiously, was with some difficulty reduced to submit to Vespasian. Julius Agricola was constituted legate, who, under the governor Petilius Cerealis, bore a considerable share in the successes against the Brigantes, (Tacitus in Vita. Agric, cap. 8.) The same author afterwards adds, cap. 17, that he conquered the greatest part of the country of the Brigantes. Notwithstanding these advantages, I dare not suppose that the Romans penetrated so far into this province as Longovicum, situated so near the northern bounds of the Brigantes, that at present it is not above 12 miles distant from Corbridge, the Roman Curia, the chief town of the adjoining people the Otadini. I fix upon the second year of Julius Agricola's government for this work, which Tacitus thus describes, cap 20, "The beginning of summer Agricola, having collected his army, fixed on the proper places for encamping, and in person examined the marshes and wood; and in the mean time gave the enemy no rest, that he might be the less exposed to their sudden excursions; and after sufficiently terrifying them, he would then by his lenity allure them to peace; by which conduct many states submitted and gave hostages; on which he built garrisons and fortresses round them." This excellent conduct Tacitus further confirms from the observation of others. "For, says he, persons skilled in these matters observed that no general ever chose his posts of advantage with better judgment; for that no fortress built by Agricola was either taken by storm, given up by capitulation, or deserted by the garrison."

Agricola, having this summer quieted so large a tract, and finished so many fortresses, it cannot be expected all should be built with the most exquisite art, sufficient to perpetuate them.

To proceed to Gordian's repairs: whose historian, Julius Capitolinus, though he has never once named Britain, yet gives so many hints of the excellent economy of his government, under the prudent administration of his father in-law, Misitheus, that I would fix this work to the third year of his reign, he having before been under the direction of the eunuchs and officers of the court, whom Capitolinus represents, in Misitheus's letter to Gordian, as having prostituted all employments to their own covetousness and mercenary creatures.

A new Genus of Plants, called Araliastrum, of which the famous Nin-zin or Ginseng of the Chinese is a Species. Communicated by Mr. Vaillant Prædemonstrator at the Royal Garden at Paris, to Dr. Wm. Sherrard,† LL.D. and by him to the Royal Society.* N° 354, p. 705.

Araliastrum is a genus of plants, whose flower A, vid. aralia Inst. Rei Herb, tab. 154, is complete, that is, has a calyx, is regular, polypetalous, and herma-

* Sebastian Vaillant, M. D. author of the work entitled *Botanicon Parisiense,* or an account, in alphabetical order, of all the Plants growing in the environs of Paris, as well as of several other publications, was born in the year 1669, and died of an asthma in 1722. The merit of Vaillant, as Dr. Smith observes,* is hardly sufficiently known. Dr. S. declares, that on consulting his Herbariums, preserved at Paris, he was astonished at the instances of profound knowledge and acuteness of judgment which he met with, both with respect to the genera, species, and synonima, of plants. Vaillant was also one of the first who was well acquainted with the sexes of plants, and his academical oration on that subject, though not without some errors, is full of good observations. His botanical papers on the various tribes of plants, &c. occur in the Memoirs of the Royal Academy.

† William Sherrard, or Sherwood, so eminent in the science of botany, was born, according to Dr. Pultney, in the year 1659, and was educated at Merchant Taylors' School, till he was entered of St. John's College, Oxford, in the year 1677. Of this college he became a Fellow, and took the degree of Bachelor of Law in 1683. After this time he accompanied Lord Viscount Townshend in his travels, and discharged his trust with so much reputation, that he was prevailed on to take the charge of Wriothesly, grandson of William, the first Duke of Devonshire, during a tour to the Continent. He returned about the year 1693, and communicated to Ray a catalogue of such plants as he had remarked on Mount Jura, Saleve, and the neighbourhood of Geneva. During his travels he gratified his favourite passion, and became acquainted with the most celebrated botanists. He was early skilled in English botany, and his assistance is acknowledged by Ray in his History of Plants. About the year 1702 he was appointed Consul at Smyrna, a department which his desire of investigating the plants of the east induced him to accept. In this place he had a country house at a village called *Sedekio,* where he spent his summers, and cultivated his botanical garden. He collected specimens of the plants of Natolia and Greece, and began the celebrated herbarium, which at length became the most extensive that had ever been seen, as the work of one person, and is said to have contained 12,000 species of plants. He returned to England in 1718, soon after which he had the degree of Doctor of Laws conferred upon him by the University of Oxford. In 1721 he again visited the Continent, and made the tour of Holland, France, and Italy. On his return he brought over with him the celebrated Dillenius, so eminent for his attention to the plants now termed *cryptogamic.*

* See an introductory discourse on the rise and progress of natural history, prefixed to the first volume of the Linnæan Transactions.

phrodite, standing on the ovary B. The ovary, which is crowned by a calyx cut into several parts, becomes a berry D, in which are, for the most part, two flat seeds, like a semicircle, which both together represent a kind of heart. Add to this the stalk, which is single, ending in an umbel, of which each ray bears only one flower. Above the middle of the stalk come out several pedicles, as on that of the anemone, on the extremities of which grow several leaves like rays, or like an open hand.

The species of this genus are: 1.* Araliastrum quinquefolii folio, majus, nin-zen vocatum D. Sarrazin. Gin-seng. Des lettres edifiantes et curieuses, tom. x, p. 172.

2. Araliastrum quinquefolii folio, minus. D. Sarrazin. Plantula marilandica, foliis in summo caule ternis, quorum unumquodque quinquefariam dividitur, circa margines serratis. N° 36, Raii Hist. III, 658.

3. Araliastrum fragrariæ folio, minus. D. Vaillant. Nasturtium marianum anemones sylvaticæ foliis, enneaphyllon, floribus exiguis. Pluk. Mantiss. 135. Tab. 435, fig. 7.

To show wherein araliastrum differs from aralia, from whence it takes its name, it is convenient to give also the character of this last genus, such as Mr. Vaillant established it, in his demonstrations of the year 1717.

Aralia, vid. Inst. Rei Herb. 300, tab. 154, is altogether like the araliastrum, as to the structure and situation of its flower, but its berry consists of 5 seeds placed round an axis. Its leaves are branched, almost like those of angelica; and its stalks, which in some species are naked, and in others have leaves set alternately, bear each several umbels at their top, in the form of a bunch of grapes.

The species of aralia are: 1. Aralia caule aphyllo, radice repente. D. Sarrazin. Christophoriana Virginiana Zarzæ radicibus surculosis et fungosis, sarsaparilla nostratibus dicta. Pluk. Almag. 98, tab. 238, fig. 5. Zarsaparilla

Dr. Sherrard lived in a state of honourable privacy in London, wholly immersed in the studies of natural history, and died August 12, 1728, leaving by will the sum of 3000 pounds, to provide a salary for a professor of botany, in the University of Oxford, on condition that Dillenius should be chosen the first professor. He also gave to this establishment his celebrated Herbarium. He was the author of several papers in the Philos. Trans. particularly an account of a new island raised near *Santorini* in the *Archipelago,* on the 12th of May, 1707.

His brother, James Sherrard, born in 1666, was an eminent apothecary in London, pursued the same studies with his brother, and had a celebrated botanical garden at Eltham in Kent, immortalized by Dillenius in his well known Hortus Elthamensis. He inherited the greater part of his brother's fortune, and had the degree of Doctor in Physic conferred upon him by the University of Oxford. He died in the year 1737.

* Panax quinquefolium. Linn.

Virginiensibus nostratibus dicta, lobatis umbelliferæ foliis, Americana. Ejusd. Almag. 396.

2. Aralia caule folioso lævi, D. Sarrazin. Aralia Canadensis. Inst. Rei Herb. 300.

3. Aralia caule folioso et hispido D. Sarrazin.

4. Aralia arborescens spinosa, D. Vaillant. Angelica arborescens, spinosa, seu arbor indica, fraxini folio, cortice spinoso Raii Hist. 2, 1798. Christophoriana arbor aculeata Virginiensis Pluk. Almag. 98, tab. 20.

All the species of these two genera, except the last of each of them, are common in Canada, whence Mr. Sarrazin, first sent them to the royal garden in 1700.

The inhabitants of that colony, and those of Virginia, call the first species of aralia by the name of sarsaparilla, because its roots have almost the same figure and virtues.

Mr. Sarrazin writes, that he had a patient who had been cured of an anasarca, about two years before, by the use of a drink made of these roots. He assures us also that the roots of the second species, well boiled and applied by way of cataplasm, are very excellent for curing old ulcers; as also the decoction of them, with which they bathe and syringe the wounds. He does not at all doubt, but the virtues of the third species are the same with those of the second.

Its roots creep, and send forth stalks, which rise commonly to the height of a foot and half, and sometimes to 2 feet; the bottom part of them is rough, with reddish, stiff, and prickly hairs. These stalks are set from the bottom almost to the top, which are divided successively into several naked branches charged with umbels, with branched alternate leaves, almost like those of podagraria hirsuta angelicæ folio et odore D. Vaillant; which plant is graved in the 2d tome of Boccone's Museum, by the name of cerefolium rugoso angelicæ folio, aromaticum, tab. 19, and in Rivinus by that of myrrhis folio podagrariæ.

See the account of the Chinese gin-seng, in Philos. Trans. Ann. 1713.*

Extract of a Letter from Mr. Edward Berkeley at Naples, giving several curious Observations and Remarks on the Eruptions of Fire and Smoke from Mount Vesuvius. Communicated by Dr. John Arbuthnot, M. D. and R. S. S. N° 354, p. 708.

April 17, 1717, with much difficulty I reached the top of Mount Vesuvius, in which I saw a vast aperture full of smoke, which hindered the seeing its depth and figure. I heard within that horrid gulph certain odd sounds, which seemed to proceed from the bowels of the mountain, a sort of murmuring,

* Page 56 of this (6th) vol. of these Abridgments.

sighing, throbbing, churning, dashing, as it were, of waves, and sometimes a noise like that of thunder or cannon, which was constantly attended with a clattering, like that of tiles falling from the tops of houses into the streets. Sometimes, as the wind changed the smoke grew thinner, discovering a very ruddy flame, and the jaws of the crater, streaked with red, and several shades of yellow. After an hour's stay, the smoke, being moved by the wind, gave us short and partial prospects of the great hollow, in the flat bottom of which I could discern two furnaces almost contiguous, that on the left seeming about 3 yards in diameter, glowed with red flame, and threw up red-hot stones with a hideous noise, which, as they fell back, caused the fore-mentioned clattering. May 8, in the morning, I ascended to the top of Vesuvius a second time, and found a different face of things. The smoke ascending upright, gave a full prospect of the crater, which is about a mile in circumference, and 100 yards deep. A conical mount had been formed since my last visit, in the middle of the bottom. This mount I could see was formed of the stones thrown up and fallen back again into the crater. In this new hill remained the two mouths or furnaces already mentioned; that on our left hand being in the top of the hill, which it had formed round it, and raged more violently than before, throwing up every 3 or 4 minutes, with a dreadful bellowing, a vast number of red-hot stones, sometimes in appearance above 1000, and at least 300 feet higher than my head as I stood on the brink. But there being little or no wind, they fell back perpendicularly into the crater, increasing the conical hill. The other mouth to the right was lower in the side of the same new formed hill. I could discern it to be filled with red-hot liquid matter, like that in the furnace of a glass-house, which raged and wrought like the waves of the sea, causing a short abrupt noise, like what may be imagined to proceed from a sea of quicksilver dashing among uneven rocks. This matter would sometimes overflow and run down the convex side of the conical hill, and appearing at first red-hot, it changed colour, and hardened as it cooled, showing the first rudiments of an eruption, or, as it were, an eruption in miniature. Had the wind driven in our faces, we would have been in no small danger of being stifled by the sulphureous smoke, or being knocked on the head by lumps of molten minerals, which we saw had sometimes fallen on the brink of the crater, upon those shot from the gulph at bottom. But as the wind was favourable, I had an opportunity of surveying this odd scene for above an hour and a half together; during which time it was very observable, that all the volleys of smoke, flame, and burning stones, came only out of the hole to our left, while the liquid matter in the other mouth wrought and overflowed as has been already described.

June 5, after a horrid noise, the mountain was seen at Naples to emit a little out of the crater. The same continued the 6th. The 7th, nothing was observed till within two hours of night, when it began a hideous bellowing, which continued all that night, and the next day till noon, causing the windows, and as some affirm, the very houses in Naples to shake. From that time it belched out vast quantities of molten matter to the south, which streamed down the side of the mountain. This evening I returned from a journey through Apulia, and was surprised, passing by the north side of the mountain, to see a great quantity of ruddy smoke lie along a large tract of sky over the river of molten matter, which was itself out of sight. The 9th, Vesuvius raged less violently: and that night we saw from Naples a column of fire shoot sometimes out of its summit. The 10th, when we thought all would have been over, the mountain became very outrageous again, roaring and groaning most dreadfully. This noise, in its most violent fits, resembled a mixed confused sound, made up of the raging of a tempest, the murmur of a troubled sea, and the roaring of thunder and artillery, all together. It was very terrible as heard in the further end of Naples, at the distance of above 12 miles. This gave me the curiosity to approach the mountain. Three or four of us got into a boat, and were set ashore at Torre del Greco, a town situated at the foot of Vesuvius to the south-west, whence we rode 4 or 5 miles before we came to the burning river, being then about midnight. The roaring of the volcano became exceedingly loud and horrible as we approached. I observed a mixture of colours in the cloud over the crater, green, yellow, red and blue; there was also a ruddy dismal light in the air over that tract of land where the burning river flowed; ashes continually showered on us all the way from the sea-coast. All which circumstances, set off and augmented by the horror and silence of the night, made a scene the most uncommon and astonishing I ever saw; which grew still more extraordinary as we came nearer the stream, resembling a vast torrent of liquid fire, rolling from the top down the side of the mountain, and with irresistible fury bearing down and consuming vines, olives, fig-trees, houses, in short, every thing that stood in its way. This mighty flood divided into different channels, according to the inequalities of the mountain. The largest stream seemed half a mile broad at least, and 5 miles long. The nature and consistence of these burning torrents hath been described, with so much exactness and truth, by Borelli, in his Latin treatise of Mount Ætna, that I need say nothing of it. I walked so far before my companions, up the mountain along the side of the river of fire, that I was obliged to retire in great haste, the sulphureous steam having surprised me, and almost taken away my breath.

During our return, which was about 3 o'clock in the morning, we constantly heard the murmur and groaning of the mountain, which would sometimes burst out into louder peals, throwing up huge spouts of fire and burning stones, which falling down again resembled the stars in rockets. Sometimes I observed two, at others three distinct columns of flame, and sometimes one vast column that seemed to fill the whole crater. These burning columns and the fiery stones seemed to be shot 1000 feet perpendicular above the summit of the volcano. The 11th at night, I observed it, from a terrass in Naples, to throw up incessantly a vast body of fire and great stones to a surprising height. The 12th in the morning, it darkened the sun with ashes and smoke, causing a kind of eclipse. Horrid bellowings this and the foregoing day were heard at Naples, whither part of the ashes also reached. At night I observed it throw up flame, as on the 11th. On the 13th, the wind changing, we saw a pillar of black smoke shot upright to a prodigious height. At night I observed the mount cast up fire as before, though not so distinctly, because of the smoke. The 14th, a thick black cloud hid the mountain from Naples. The 15th, in the morning, the court and walls of our house in Naples were covered with ashes. In the evening, flame appeared on the mountain through the cloud. The 16th, the smoke was driven by a westerly wind from the town to the opposite side of the mountain. The 17th, the smoke appeared much diminished, fat and greasy. The 18th, the whole appearance ended, the mountain remaining perfectly quiet without any visible smoke or flame. A gentleman of my acquaintance, whose window looked toward Vesuvius, assured me, that he observed this night several flashes, as it were of lightning, issue out of the mouth of the volcano. I saw the fluid matter rise out of the centre of the bottom of the crater, out of the very middle of the mountain, contrary to what Borellus imagines; whose method of explaining the eruption of a volcano by an inflexed syphon, and the rules of hydrostatics, is likewise inconsistent with the torrent's flowing down from the very top of the mountain.

An Account of an extraordinary Tumour, or Wen, lately cut off the Cheek of a Person in Scotland. Communicated by Dr. Tho. Bower, M.D. and F.R.S. N° 354, p. 713.

Alexander Palmer, of the parish of Keith, in the county of Bamff, in the north of Scotland, now about 54 years of age, observed, when about 27, a little hard swelling in the muscle of the lower jaw on the left side, without any hurt or visible cause. At first it increased slowly, but afterwards it proceeded more quickly, and the longer always the faster; till it increased to a prodigious bulk and weight. From the first appearance of this tumour to its total excision, was about 27 years. He had excessive pains and uneasiness in it, and at last it greatly emaciated him, though otherwise a strong and robust man.

This excrescence was of the natural colour of the skin, and seemed to be an atheroma, being a glandulous substance with several large blood-vessels in it, and had hair growing on it, as on the other parts of the body, as may yet be seen. It was almost round and very hard, and was as sensible as any other part of the body; for, when he was working in the fields, he accidentally made a great gash or wound in it with a sharp iron, which was very painful, but was cured by a surgeon, after the manner of an ordinary wound; the cicatrix is still to be seen in it.

This excrescence having grown so large, was attached to the muscle under the left eye, called obliquus minor or inferior, to the ear and its muscles, and to the muscle of the lower jaw, named deprimens. By reason of its great bulk and weight, it could not hang down freely without some support; therefore it rested on the top of the shoulder, making a considerable dimple in it, yet very observable; besides, it was held up by the man's hand in the day-time, and laid on a pillow in the night.

Three or 4 days before the total excision of this tumour, the patient observed it begin to mortify at the lower end, which made him so uneasy, that he took a knife and cut off a good part of it. This occasioned a great hæmorrhage; so that he reckoned there was lost a Scotch pint or 4lb. of blood, before it could be stopped. The patient, after so great trouble and pain, at last applied to Mr. Gordon, surgeon of the place, who made a total extirpation of it, as follows.

He made a close ligature, taking in the basis of the excrescence, and all the loose skin, and contracting it as much as possible, he cut it entirely off with a sharp razor. There gushed out of the excrescence, after it was cut off, and as it lay on the ground, about 2lb. of blood; having been nourished by several large blood-vessels. The basis, as it now appears, is five inches diameter. After all this blood was lost, the excrescence weighed full 19lbs; a most prodigious weight to be depending from such a place. It was of a spheroid figure.

The hæmorrhage, which was great, was stopped by the vitriolic powders and other astringents, and the ordinary dressing was used: the cure was thus completed in 6 weeks time, and the patient entirely recovered, and now goes about his business, to the great admiration and astonishment of every body. The lid of his left eye continues still downwards a little, as well as that side of the mouth, occasioned by the great weight so long depending on that side of the face. Though the skin, and even a deal of the musculous part of the cheek and lower jaw were cut away, it is grown up again, and is of the ordinary colour of the skin, and like the other side of the face; so that the hair grows on that side of the face as well as on the other, which he usually shaves; which is as surprising as any thing in the whole affair.

An Account of an Experiment to prove an interspersed Vacuum; or, to show that all Places are not equally full. By J. T. Desaguliers, M. A. F.R.S. N° 354, p. 717.

On dropping a guinea and a piece of fine paper; then a guinea and a feather together, from the top of an exhausted glass-receiver, about 20 inches high; they both fell to the bottom at the same instant of time. Now since the chief resistance of a medium (and indeed almost all of it) depends on the quantity of its matter (see Newton's Princ. l. 2, p. 40); therefore this diminution of resistance, by which the feather fell as soon as the guinea, showed a diminution of the quantity of matter, and consequently proved an interspersed vacuum.

Some plenists in England objecting against the shortness of the glass-receiver; as if the difference of time in the fall of the two bodies, which they affirmed to be real, could not be perceived in such a glass; and some philosophers from abroad affirming that in a glass-receiver 7 or 8 feet long, there would be such a manifest difference in the time of the fall of the said bodies, as to show this experiment no proof of a vacuum; though at the same time, some of the objectors well knew that there could be no receivers of half that length made at the glass house, and therefore thought the experiment impracticable. To obviate this, I contrived a machine for the purpose, which consisted of a strong wooden frame, 15 feet high, that held the air-pump and 4 cylindric glass-receivers of about 2 feet long each, and 6 inches diameter: having set the first of these upon the air-pump plate, I laid on the top of it a brass-plate of 7 inches diameter, with an oiled leather fixed to it above and below, and a hole through the middle, between 4 and 5 inches diameter; then on that plate I set the next receiver, with a like plate at top; and after the same manner fixed the other two with plates between them: the upper receiver being a little narrower at the neck, went into the hole of a board, by which it was screwed down pretty hard on the other glasses, and fixed to the whole machine. On the top of this upper receiver I laid the brass plate, wet leather, and brass springs, which contained the bodies to be dropped.

When the receiver was full of common air before pumping, the guinea came to the bottom, just as the paper was about the middle of the second glass; but when the receiver was exhausted, the guinea and paper came to the bottom precisely at the same instant of time.

Upon my giving an account of the success of this experiment to the Royal Society, they ordered me to repeat it before them on the 5th day of Dec. 1717, being the Thursday next after the yearly meeting for choosing officers on St. Andrew's day; on which day an annual experiment is appointed to be made, in conformity to the will of their late worthy member and benefactor Sir Godfrey Copley. I made the experiment first with two of the receivers; then with all the four; dropping a guinea and a small piece of paper together; and the success answered expectation. But not being willing to try with a down-feather, because I feared the air might insinuate between some of the glasses, by reason the number of persons present shook the room, the society ordered me to make the experiment at home before one or more of their members. Accordingly Martin Folkes, Esq. a very ingenious member of the society, did me the favour to be present when I made the experiment at my house; where we made four trials in the following manner.

The whole machine being fixed, as abovementioned, we first let fall a guinea and two papers; the one placed over, and the other under it, before any air was pumped out; and the guinea came to the bottom when the papers were only in the middle of the second glass from the top. Then having laid a feather on the brass-springs close by the guinea, we let them loose both together; and the feather was fallen only down to the 4th part of the length of the first glass or $\frac{1}{16}$ of the whole distance, when the guinea was got down to the bottom of the receiver. We then laid two papers and two feathers, one of each under, and the other over the guinea between the springs; and having drawn out so much of the air as to bring up the mercury in the gauge-tube within a quarter of an inch of the greatest height to which it could be then raised by the pressure of the external air, we caused the bodies to fall all at once: and though the papers came down to the bottom at the same time as the guinea, yet the feathers, being much lighter, wanted about three inches. But at last, having laid the papers, feathers, and guinea, as before, we pumped out all the air, and then the feathers, as well as the papers, came to the bottom of the receiver at the same instant of time as the guinea.

An Account of a small Telescopical Comet, seen at London the 10th of June, 1717. By Edm. Halley, LL. D. R. Soc. Sec. N° 354, p. 721.

That the number of comets traversing our solar system is much greater than some, on account of the late rareness of their appearance, have supposed it, may be collected from several small ones which have within few years been described in the memoirs of the French Royal Academy of Sciences; those diligent observers assuring us that they discovered one in Sept. 1698, another in Feb. 1699, a third in April 1702, and again a fourth in Nov. 1707; none of which, as far as I can learn, were ever seen in England; all of them having been very obscure and without tails, by means of which comets usually first show themselves. And besides these, two other comets, with remarkably long tails, the one in Nov. 1689, the other in Feb. 1702, passed by unobservable in these our northern climates, having great south latitude, and their motions directed toward that pole. Hence we may justly conclude that the returns of comets are much more frequent than is commonly reckoned, and that it is only contingent, that for these 35 years no one of them has been seen and observed by our astronomers.

But there may be still a much greater number of these bodies, which by reason of their smallness and distance are wholly invisible to the naked eye; so that unless chance direct the telescope of a proper observer, almost to the very points where they are, against which there are immense odds, it will not be possible to discover them: and that this is not barely a conjecture, take the following instance.

On Monday, June 10, in the evening, the sky being very serene and calm, I was desirous to take a view of the disk of Mars, then very near the earth, and appearing very glorious, to see if I could distinguish by my 24-foot telescope, the spots said to be seen on him. Directing my tube for that purpose, I accidentally fell upon a small whitish appearance near the planet, resembling in all respects such a nebula as I lately described in Phil. Trans. N° 347, but smaller. It seemed to emit from its upper part a very short kind of radiation, directed towards the east, but rather northerly; which, considering its situation, was nearly towards the point opposite to the sun. The great light of the moon, then very near it and also near the full, hindered this phenomenon from being more distinctly seen; but its place in the heavens was sufficiently ascertained from the vicinity of Mars, from which it was but about half a degree distant towards the southwest, the difference of latitude being somewhat more than that of longitude; and Mars being at that time in ♐ 17° 30′, with 3° 48′ south latitude. I concluded its place about ♐ 17° 12′, with 4° 12′ lat. south: which may yet be more securely determined by help of two small fixed stars I found near it, the more northerly of which I judged to have the same latitude with it, and to follow it at about the distance of 6 minutes; the other star was about 4 minutes more southerly than the former, and about one minute in consequence thereof; the angle at the northern star was a little obtuse, as of about 100 degrees, and the distance of our nebula from it sesquilateral to the distance of the two stars, or rather a little more. The slowness of its motion made me at that time conclude that it had none, and that it was rather a nebula than a comet.

However, suspecting that it might have some motion, I attended the next night, June 11, at the same hours, when with some difficulty, by reason of the thickness of the air, I found the two little stars, but the nebula could not at that time be seen, which I then imputed to the want of a clearer sky. But on Saturday, June 15, the moon being absent, and the air perfectly clear, I had again a distinct view of the two stars, with an entire evidence that there remained no sign of it in the place where we had first seen this phenomenon, which we therefore now found to be a comet; and that being far without the orb of the earth, and in itself a very small body, it appeared only like a small speck of a cloud, such as would scarcely have been discerned in an ordinary telescope, much less by the naked eye.

An Account of Books, viz. 1. *Joannis Poleni in Gymnasio Patavino Phil. Ord. Prof. et Scient. Societatum Regalium, quæ Londini et Berolini sunt, Sodalis, De Motu Aquæ mixto, Libri duo, &c. 4to. Patavii* 1717. N° 354, p. 723.

The subject here treated of not having hitherto fallen under the consideration of mathematical writers the author is obliged to make use of several terms, which are either wholly new, or at least are applied in a sense somewhat different from their common acceptation; for which reason he begins his work with a set of definitions.

Next follows a short history of the original, and progress of the doctrine of running waters, the invention of which our author justly ascribes to the learned Castelli, and defends him against Fabretti, who has maintained that Castelli's fundamental proposition of the quantity discharged being cæteris paribus in proportion to the velocity, was known, and publicly taken notice of before him by Frontini. The author allows Castelli to have been mistaken in determining the velocity of water running out at the bottom of a vessel, he having asserted that velocity to be as the depth of the water, instead of the root of that depth.

Three years after Castelli's book came out, this mistake was corrected by the famous Torricelli, who was the first that maintained, that the velocity of the water running out was in a subduplicate ratio of the depth; but gave no demonstration of it. This proposition, says our author, was confirmed by the experiments of Maggiotti, Mariotte, and Guglielmini, and has since been demonstrated by M. Varignon, by Herman in his Phoronomia, and John Bernouilli, as reported by Herman in the Acta Lipsiensia.

Here it may not be improper to take notice, that the demonstrations of those three learned persons are all grounded on this supposition, that the water running out from the hole is pressed on by the column of water incumbent on it, which may easily be demonstrated to be a mistake. Likewise, if their demonstrations be just, it will follow, that the first drops of water, which issue out from the hole, must run with the same velocity, as after the water has been running some time; the contrary of which appears to be true in fact by the experiments of M. Mariotte.

The author might have found a juster account of this matter in the writings of a great man, (Newton) whom he cites on another occasion; the second edition of whose book was come out some time before Herman published either of those demonstrations, and had been seen by him, as appears by his quoting it frequently, and mentioning the difference in this very particular between the first and second edition.

Our author goes on to consider the simple motion of water running out by a section perpendicular to the horizon, in the side of a receptacle, which is always kept up at the same height. He shows, that the velocities with which the water issues out at different depths, being as the roots of those respective depths, may be represented by the ordinates of a parabola, whose axis represents the entire depth of the water. Consequently, since the quantities of water, running out at different depths, are as those velocities, they likewise may be represented by the same ordinates, and the quantity of water discharged from the whole section, will be represented by the parabolic space; and the mean velocity by that same space divided by the abscisse.

The author proceeds next to the mixed motion of water; in order to discover the nature of which, he has made some curious experiments.

In order therefore to find a general rule for determining the proportion between the parabolic spaces, which represent the quantities discharged by the mixed and simple motion, or between the parameters of those parabolas, he draws some observations from the foregoing experiments, by the help of which he hopes such a rule may be found out.

In effect the author proceeds, in a tentative method, to find his rule, and having discovered it, he shows by calculation, that it answers all the conditions before required. This rule is expressed in a pretty high equation; which, besides other operations, requires the extracting the root of the sixth power. From this equation is derived another, serving to find either the quantity of water discharged, the depth of the running, or that of the dead water, the other two of them being given; as likewise a third equation, to find the mean velocity.

Our author goes on to show the usefulness and necessity of considering the doctrine of mixed motion, in all questions relating to the course of rivers, the quantities of water which they discharge, the enlarging or narrowing their outlets, the scouring and deepening their channels, and the motion of the tides in harbours. These he illustrates by several deductions from the equations abovementioned. But, to render these of greater evidence, it were to be wished, that those equations were built on a more solid foundation than a tentative calculus; and that allowance had been made for the velocity impressed on the preceding water in rivers, by the impetus of that which follows, which is omitted by the author in his Theory, both of mixed and simple motion.

In the second book, this learned writer proposes the state of the laguna of Venice, as a proper example, to demonstrate the usefulness of his new Theory. He considers very minutely the several causes of choaking up the laguna, examines the method proposed by various authors for scouring and keeping it clear, some of which he rejects as impracticable on account of the expence, others as useless, or prejudicial; and lastly, delivers his own opinion.

The principal causes, which he assigns, of filling up the laguna, are the rivers running into it, and the sea tides setting into it; both of which setting in with a great velocity, carry in a great deal of mud, and deposit it there; the water running out clearer than it goes in.

The remedy our author proposes for the river waters, is either wholly to divert the course of the rivers and carry them, by another way, directly into the sea: or at least, to secure their outlets with sluices, so as to suffer them to pass into the laguna, when their waters are clear; but after great rains, when they run foul and turbid, to stop their passage that way, and let them out by the other channel into the sea. And against the effect of the tides, he proposes some works of strong piles, and large stones thrown in between them to be carried directly forward into the sea, to break the violence of the waves, and prevent their washing and carrying away the land.

He seems likewise to favour a proposal made by the late famous Guglielmini, and some others, to let the tide enter the laguna by more passages than it is to go out at, in order to make it run out with a greater velocity, and thereby scour and deepen the channels. But he thinks this contrivance will scarcely perform all that is expected from it; besides, it will be attended with great difficulties in making works, and flood-gates of a sufficient strength, to resist the violence of the waters.

He occasionally combats the opinion of Guglielmini, and most other mathematicians who have thought upon the subject, that in order to give a greater velocity to the water of a river, thereby to scour and cleanse the channel, it is proper to make the outlet narrower. This our author maintains to be oftener false, than true, and endeavours to show from his theorem abovementioned, that making the outlet narrower, will frequently cause the mean velocity of the waters to become less than it was before. But whether a proposition of such consequence, and seemingly so well supported by reason and experience, ought to be condemned on the authority of a theorem founded only on a tentative calculation, must be left to the judgment of the learned.

II. Apollonii Pergæi Conicorum Libri Octo, et Sereni Antissensis de Sectione Cylindri et Coni Libri duo. Fol. Reg. e Theatro Oxon. 1710. N° 354, p. 732.

The curators of the Oxford press having obliged the public with a very elegant edition of the works of Euclid, Græco-Latinè, were pleased further to proceed in the laudable intention of giving the rest of the ancient Greek mathematicians in the same beautiful form. In this design they were chiefly animated by the late learned and beneficent Dean of Christ-church, Dr. Henry Aldrich, who pitching upon Apollonius, as most proper to succeed Euclid, engaged the two Savilian professors to take upon them the care and pains of the edition: Dr. David Gregory promising his assistance as to the first 4 books, which are still extant in Greek; and Dr. Edm. Halley undertaking to translate the 5th, 6th, and 7th books out of Arabic, in which language they were only to be found, and to endeavour to restore the 8th, long since wholly lost. But Dr. Gregory soon after dying, the care of the whole devolved on Dr. Halley, who has spared no pains to render the work complete.

In his preface, he tells us what helps he had to perfect the text, that he had the use of two Greek MSS. of the first 4 books, one of which was Sir Henry Savile's, and is in the Savilian Study at Oxford, the other is now in the Royal Society's Museum, having been lately presented to them by that skilful mathematician Mr. Wm. Jones, F.R.S. That he had only one manuscript of Eutocius's Commentary, out of the Bodleian Library; and two Greek copies, from the Savilian Study, of Pappus's Collections, out of whose 7th book he took the Lemmata, which serve as a comment on the more difficult places of his author; and that he was forced to revise and correct the mistakes and improprieties of the Latin translation of Commandine.

As to the latter books, which were only in Arabic, he informs us, that he made use of the Bodleian transcript of a manuscript which is at Leyden, which itself is a late copy of that ancient Arabic book of the Conics, heretofore Golius's, but since purchased by that great patron of universal learning, Narcissus late primate of Ireland, who was pleased to favour him so far as to send over into England this original book, by which he not only amended several faults committed by the copyists in a double transcription, but was also assured that this Arabic book was a verbal translation from the Greek; the same schemes marked with the same letters, and the whole context being the same in the first 4 books of it, as in the Greek Apollonius. This valuable manuscript, with about 800 others, Oriental and Greek, has since, by the donation of that most venerable prelate, made a noble accession to the Bodleian Library, where it is now deposited. It appears by an epigraph at the end, to have been written in the year of Christ 1303, and to have been a copy of a translation of the Conics, made some ages before by Thebit Ben Corah, but then newly revised by that famous Persian mathematician Nasireddin, who flourished about the middle of our 13th century.

Besides this, the editor tells us, that on this occasion he consulted another Arabic manuscript (heretofore Ravius's) of great antiquity, being an epitome of the same books by Abdolmelec of Schiraz, every where agreeing in the order and argument with the former, but abridged. So that having had these helps, he is in hopes that he has so far retrieved those 3 books of Apollonius, that the loss of the Greek text may henceforth be less lamented.

The 8th book of these Conics was wanting in the Greek copies, even before the translation of them into Arabic by Thebit: but it having been observed that there was a very near relation between the arguments of the 7th and 8th books, for that the same lemmata of Pappus were common to them both, which are different from all the rest, it seemed that the theoremata dioristica of the 7th book were designed to determine the limits of the problemata diorisména of the 8th; and therefore supposing what those problems might have been, and their order from that of the said theorems, Dr. Halley has, in 33 propositions, given the analyses and syntheses of them, after the method of the ancients, every where following the steps of Apollonius as found in his 7th book. This he calls conicorum liber octavus restitutes, and may serve the turn, till such time as the original 8th book come to light; if that be not now to be despaired of.

Because of the affinity of the subject, he has subjoined the two books of Serenus Antissensis, the Greek text of which was never before in print. This was procured by the abovesaid Rev. Dean of Christchurch, Dr. Aldrich, in a collated copy of three manuscripts, extant in the king's library at Paris, and by him communicated for the use of the public. To this also is added the Latin translation of Commandine, which in many cases needed castigation.

As to the authors themselves, little needs be said, having stood the test of so many ages, and been highly valued by the learned in all times, especially the Conics, justly esteemed a masterpiece in the geometry of the ancients: so that it may seem strange, that a book so excellent in its kind, should not till now have been printed in its native Greek, a language so peculiarly adapted to mathematical purposes. But this present edition may make ample amends, the paper and the elegance and correctness of the print being remarkable.

Considerations on the Change of the Latitudes of some of the principal fixed Stars. By Edmund Halley, R. S. Sec. N° 355, p. 736.

Having of late had occasion to examine the quantity of the precession of the equinoctial points, I took the pains to compare the declinations of the fixed stars delivered by Ptolomy, in the 3d chapter of the 7th book of his Almag. as observed by Timocharis and Aristyllus, near 300 years before Christ, and by Hipparchus about 170 years after them, that is about 130 years before Christ, with what we now find: and by the result of a great many calculations, I concluded that the fixed stars in 1800 years were advanced somewhat more than 25 degrees in longitude, or that the precession is somewhat more than 50″ per ann. But that with so much uncertainty, by reason of the imperfect observations of the ancients, that I have chosen in my tables to adhere to the even proportion of 5 minutes in 6 years, which from other principles we are assured is very near the truth. But while I was on this inquiry, I was surprised to find the latitudes of three of the principal stars in Heaven directly to contradict the supposed greater obliquity of the ecliptic, which seems confirmed by the latitudes of most of the rest: they being set down in the old catalogue, as if the plane of the earth's orbit had changed its situation, among the fixed stars, about 20′ since the time of Hipparchus. Particularly all the stars in Gemini are set down, those to the northward of the ecliptic, with so much less latitude than we find, and those to the southward with so much more southerly latitude. Yet the three stars Palilicium or the Bull's Eye, Sirius, and Arcturus contradict this rule directly: for by it Palilicium, being in the days of Hipparchus in about 10° of Taurus, ought to be about 15 min. more southerly than at present; and Sirius, being then in about 15° of Gemini, ought to be 20′ more southerly than now; yet on the contrary Ptolomy places the first 20′ and the other 22′ more northerly in latitude than we now find them. Nor are these errors of transcription, but are proved to be right by their declinations set down by Ptolomy, as observed by Timocharis, Hipparchus and himself, which show that those latitudes are the same as these authors intended. As to Arcturus, he is too near the equinoctial colure, to argue from him concerning the change of the obliquity of the ecliptic; but Ptolomy gives him 33′ more north latitude than he has now; and that greater latitude is likewise confirmed by the declinations delivered by the said observers. So then all these three stars are found to be above half a degree more southerly at this time than the ancients reckoned them. When, on the contrary, at the same time the bright shoulder of Orion has in Ptolomy almost a degree more southerly latitude than at present. What shall we say then? It is scarcely credible that the ancients could be deceived in so plain a matter, three observers confirming each other. Again these stars, being the most conspicuous in Heaven, are in all probability the nearest to the earth; and if they have any particular motion of their own, it is most likely to be perceived in them, which in so long a time as 1800 years may show itself by the alteration of their places, though it be utterly imperceptible in the space of a single century of years. Yet as to Sirius, it may be observed that Tycho Brahe makes him 2′ more northerly than we now find him; whereas he ought to be above as much more southerly from his ecliptic, (the obliquity of which he makes $2\frac{1}{2}'$ greater than we reckon it at present) differing in the whole $4\frac{1}{2}'$. One half of this difference may perhaps be excused, if refraction were not allowed in this case by Tycho; yet 2′ in such a star as Sirius, is rather too much for him to be mistaken.

But a further and more evident proof of this change, is drawn from the observation of the application of the moon to Palilicium Anno Christi 509, March 11th, when in the beginning of the night the moon was seen to follow that star very near, and seemed to have eclipsed it, ἐπέβαλλε γαρ ὁ αστηρ τῷ παρα την διχοτομίαν μέρει τῆς κύρτης περιφερειας τȢ πεφωτισμενȢ μέρȢς. i. e. Stella apposita erat parti per quam bisecabatur limbus lunæ illuminatus, as Bulliald, to whom we are beholden for this ancient observation, has translated it. Now from the undoubted principles of astronomy, it was impossible for this to be true at Athens, or near it, unless the latitude of Palilicium were much less than we at this time find it. Vide Bullialdi Astr. Philolaica, p. 172.

But whether it were really true, that the obliquity of the ecliptic was, in the time of Hipparchus and Ptolemy, really 22′ greater than now, may well be questioned; since Pappus Alexandrinus, who lived but about 200 years after Ptolomy, makes it the very same that we do. Vide Pappi Collect. lib. 6, prop. 35.

An Account of some Experiments shown before the Royal Society; with an Inquiry into the Cause of the Ascent and Suspension of Water in Capillary Tubes. By James Jurin, M.D. R.S.S.* N° 355, p. 739.

Some days ago a method was proposed to me by a friend, for making a per-

* Dr. Jurin was a very respectable philosopher, of the Newtonian school, who cultivated medicine and mathematics with equal success. He proved a very active and useful member of the Royal

petual motion, which seemed so plausible, and indeed so easily demonstrable from an observation of the late Mr. Hawksbee, said to be founded on experiment, that, though I am far from having any opinion of attempts of this nature, yet I confess I could not see why it should not succeed. On trial indeed I found myself disappointed. But as searches after things impossible in themselves are frequently observed to produce other discoveries, unexpected by the inventor; so this proposal has given occasion not only to rectify some mistakes into which we had been led, by that ingenious gentleman, but also to detect the real principle, by which water is raised and suspended in capillary tubes, above the level.

My Friend's Proposal was as follows.—Let ABC, fig. 2, pl. 8, be a capillary siphon, composed of two legs AB, BC, unequal both in length and diameter; the longer and narrower leg AB having its orifice A immersed in water, the water will rise above the level, till it fills the whole tube AB, and will then continue suspended. If the wider and shorter leg BC, be in like manner immersed, the water will only rise to some height as FC, less than the entire height of the tube BC.

This siphon being filled with water, and the orifice A sunk below the surface of the water DE, my friend reasons thus: Since the two columns of water AB and FC, by the supposition, will be suspended by some power acting within the tubes they are contained in, they cannot determine the water to move one way or the other. But the column BF, having nothing to support it, must descend, and cause the water to run out at C. Then the pressure of the atmosphere driving the water upward through the orifice A, to supply the vacuity, which would otherwise be left in the upper part of the tube BC, this must necessarily produce a perpetual motion, since the water runs into the same vessel, out of which it rises. But the fallacy of this reasoning appears on making the experiment.

Exper. 1.—For the water, instead of running out at the orifice C, rises upward towards F, and running all out of the leg BC, remains suspended in the other leg to the height AB.

Society, and for some time filled the office of one of their secretaries. He also succeeded Dr. Tyson as president of the College of Physicians in Jan. 1750, but died the 22d of March following. Dr. Jurin had great disputes with Michelotti, on the momentum of running water; also with Robins, on distinct vision; and with the partizans of Leibnitz, on the force of moving bodies A treatise of his, on vision, is printed in Smith's Optics; and he was concerned with Newton, in writing notes for their improved edition of Varenius's Geography. His communications in the Philos. Trans. extend from vol. 30 to vol. 66, inclusively. Dr. J. was among the earliest and most able advocates for the inoculation of the small-pox, a practice at that time newly introduced into England, and which had to struggle against the prejudices and opposition not of the vulgar only, but of a very large proportion of medical practitioners in all parts of the kingdom.

Exper. 2.—The same thing succeeds on taking the siphon out of the water, into which its lower orifice A had been immersed; the water then falling in drops out of the orifice A, and standing at last at the height AB. But in making these two experiments, it is necessary that AG the difference of the legs exceed FC, otherwise the water will not run either way.

Exper. 3.—On inverting the siphon full of water, it continues without motion either way.

The reason of all which will plainly appear, when we come to discover the principle, by which the water is suspended in capillary tubes.

Mr. Hawksbee's observation is as follows: "Let ABFC, fig. 10, be a capillary siphon, into the which the water will rise above the level to the height CF, and let BA be the depth of the orifice of its longer leg below the surface of the water DE. Then the siphon being filled with water, if BA be not greater than CF, the water will not run out at A, but will remain suspended." This seems indeed very plausible at first sight. For since the column of water FC will be suspended by some power within the tube, why should not the column BA, being equal to, or less than the former, continue suspended by the same power?

Exper. 4.—In fact, if the orifice C be lifted up out of the water DE, the water in the tube will continue suspended, unless BA exceed FC.

Exper. 5.—But when C is never so little immersed in the water, immediately the water in the tube runs out in drops at the orifice A, though the length AB be considerably less than the height CF.

Mr. Hawksbee, in his book of experiments, has advanced another observation, viz. that the shorter leg of a capillary siphon, as ABFC, must be immersed in the water to the depth FC, which is equal to the height of the column, that would be suspended in it, before the water will run out at the longer leg.

Exper. 6.—From what mistake this has proceeded, I cannot imagine; for the water runs out at the longer leg, as soon as the orifice of the shorter leg comes to touch the surface of the stagnant water, without being at all immersed in it.

I shall now inquire into the cause of the ascent and suspension of water in capillary tubes.

That this phenomenon is no way owing to the pressure of the atmosphere, has been, I think sufficiently proved by Mr. Hawksbee's experiments. And that the cause assigned by him, viz. the attraction of the concave surface, in which the suspended liquor is contained, is likewise insufficient for producing this effect, I thus demonstrate. Since, in every capillary tube, the height to which the water will spontaneously ascend is reciprocally as the diameter of the tube, it follows that the surface, containing the suspended water, in every tube, is always a given quantity: but the column of water suspended is, as the diameter of the tube. Therefore, if the attraction of the containing surface be the cause of the water's suspension, it will follow that equal causes produce unequal effects, which is absurd.

To this it may perhaps be objected, that, in two tubes of unequal diameters, the circumstances are different, and therefore the two causes, though they be equal in themselves, may produce effects that are unequal. For the less tube has not only a greater curvature, but those parts of the water, which lie in the middle of the tube, are nearer to the attracting surface, than in the wider. But if any thing follows from this, it must be, that the narrower tube will suspend the greater quantity of water, which is contrary to experiment. For the columns suspended are as the diameters of the tubes. But as experiments are generally more satisfactory in things of this nature, than mathematical reasonings, it may not be amiss to make use of the following, which appear to me to contain an experimentum crucis. The tube CD, fig. 11, is composed of two parts, in the wider of which the water will rise spontaneously to the height BF; but the narrower part, if it were of a sufficient length, would raise the water to a height equal to CD.

Exper. 7.—This tube being filled with water, and the wider end C immersed in the stagnant water AB, the whole continues suspended.

Exper. 8, fig. 12.—The narrower end being immersed, the water immediately subsides, and stands at last at the height DG equal to BF.

From which it is manifest, that the suspension of the water in the former of these experiments, is not owing to the attraction of the containing surface: since, if that were true, this surface being the same, when the tube is inverted, would suspend the water at the same height.

Having shown the insufficiency of this hypothesis, I come now to the real cause of the phenomenon; which is the attraction of the periphery, or section of the surface of the tube, to which the upper surface of the water is contiguous and coheres. For this is the only part of the tube, from which the water must recede on its subsiding, and consequently the only one, which by the force of its cohesion, or attraction, opposes the descent of the water. This is also a cause proportional to the effect, which it produces; since that periphery, and the column suspended, are both in the same proportion as the diameter of the tube. Though from either of these particulars it were easy to draw a just demonstration, yet to put the matter out of all doubt, it may be proper to confirm this assertion, as we have done the former, by actual experiment.

Fig. 13. Let therefore EDC be a tube, like that made use of in the 7th and

8th experiments, except that the narrower part is of a greater length; and let AF and BG be the heights, to which the water would spontaneously rise in the two tubes ED and DC.

Exper. 9.—If this tube have its wider orifice C immersed into the water AB, and be filled to any height less than the length of the wider part, the water will immediately subside to a level with the point G; but if the surface of the contained water enter never so little within the smaller tube ED, the whole column DC will be suspended, provided the length of that column do not exceed the height AF.

In this experiment it is plain, that there is nothing to sustain the water at so great a height, except the contact of the periphery of the lesser tube, to which the upper surface of the water is contiguous. For the tube DC, by the supposition, is not able to support the water at a greater height than BG.

Exper. 10, fig. 14.—When the same tube is inverted, and the water is raised into the lower extremity of the wider tube CD, it immediately sinks, if the length of the suspended column DH be greater than GB; whereas in the tube DE, it would be suspended to the height AF. From which it manifestly appears, that the suspension of the column DH does not depend on the attraction of the tube DE, but on the periphery of the wider tube, with which its upper surface is in contact.

For the sake of those, who are pleased with seeing the same thing succeed in different manners, we subjoin the two following experiments, which are in substance the same with the 9th and 10th. ABC, fig. 15, is a siphon, in whose narrower and shorter leg AB, if it were of a sufficient length, might be suspended a column of water of the height EF; but the longer and wider leg BC will suspend no more than a column of the length GH.

Exper. 11.—This siphon being filled with water, and held in the same position as in the figure, the water will not run out at C the orifice of the longer leg, unless DC, the difference of the legs AB and BC, exceed the length EF.

Exper. 12, fig. 16.—If the narrower leg BC be longer than AB, the water will run out at C, if DC the difference of the legs exceed EF; otherwise it will remain suspended.

In these two experiments it is plain, that the columns DC are suspended by the attraction of the peripheries at A, since their lengths are equal to EF, or to the length of the column, which by the supposition those peripheries are able to support; whereas the tubes BC will sustain columns, whose lengths are equal to GH.

Though these experiments seem to be conclusive, yet it may not be improper to prevent an objection, which naturally presents itself, and which at first view may be thought sufficient to overturn our theory. For since a periphery of the tube ED, fig. 13, is able to sustain no more than a column of the length AF, contained in the same tube; how comes it to sustain a column of the same length in the wider tube DC, which is as much greater than the former, as the section of the wider tube exceeds that of the narrower? Again, fig. 14, if a periphery of the wider tube DC be able to sustain a column of water in the same tube, of the length BG; why will it support no more than a column of the same length in the narrower tube ED? Which queries may also be made with regard to the 11th and 12th experiments.

The answer is easy, for the moments of those two columns of water are precisely the same, as if the sustaining tubes, ED and CD, were continued down to the surface of the stagnant water AB; since the velocities of the water, where those columns become wider, or narrower, are to the velocities at the attracting peripheries, reciprocally as the different sections of the columns.

Exper. 13, fig. 17.—From which consideration arises this remarkable paradox, that a vessel being given of any form whatever, as ABC, and containing any assignable quantity of water, how great soever; that whole quantity of water may be suspended above the level, if the upper part of the vessel c be drawn out into a capillary tube of a sufficient fineness.

But whether this experiment will succeed, when the height of the vessel is greater than that to which water will be raised by the pressure of the atmosphere, and how far it will be altered by a vacuum, I may perhaps have the honour of giving an account to the Society some other time, not being perfectly satisfied with those trials which I have hitherto had the opportunity of making.

Having discovered the cause of the suspension of water in capillary tubes, it will not be difficult to account for the seemingly spontaneous ascent of it. For, since the water, that enters a capillary tube as soon as its orifice is immersed, has its gravity taken off by the attraction of the periphery with which its upper surface is in contact, it must necessarily rise higher, partly by the pressure of the stagnant water, and partly by the attraction of the periphery immediately above that which is already contiguous to it.

P. S. When this paper was reading before the Society, I found that our president [Newton] was already acquainted with the above-mentioned principle; and I have since met with several passages in the 31st query subjoined to the late edit. of his Optics, which plainly show that he was master of it when they were written. I must do the same justice to Mr. John Machin, Prof. of Astr. in Gresham College. To these two worthy persons I am obliged for the following observation, that what I call a periphery, or section of the concave surface of the tube, is really a small surface, whose base is that periphery, and whose height is the distance to which the attractive power of the glass is extended.

Of the Motion of Running Water. By the same Author, Dr. Jurin. N° 355, p. 748. *Translated from the Latin.*

We often see, both in hydraulics and in applying its principles to the animal economy, that the motion of water running through a hole in the bottom of a vessel, is compared with other powers. And since no one has hitherto truly determined the quantity of this motion, hydraulic writers use to assume instead of it, the weight of a column of water incumbent on the said hole. But such as do this, never consider, that it is utterly impossible to compare any motion with a weight that is in a state of rest. But the motion of running water may be easily defined in the following manner.

Let SHAHS, fig. 18, pl. 8, be an infinite surface of water, CC a circular hole made in the bottom; AB a perpendicular drawn through the centre of the hole; SGCCGS a column or cataract of water running through the hole CC; SGC a curve, by whose rotation round the axis AB is generated the solid or cataract SGCCGS. For, when water descends freely, and with an accelerated motion, as all heavy bodies do, it is necessarily contracted into a less bulk, as it requires a greater velocity in falling, and runs out at the hole CC with that velocity, which it acquires by falling from the height AB.

But the velocity acquired by a heavy body in its fall, from what Galilæo has demonstrated, is in a subduplicate ratio of the height from which it has fallen. Therefore, if any ordinate DE be drawn to the curve SGC; and DE be called y and AD, x, the velocity of the water in the section EE will be expressed by $\sqrt{x}$, and the product of that velocity into the section by $\sqrt{x} \times y^2$. But this product is as the mass of water that passes through that section in a given space of time; and since the same bulk of water passes through each section of the cataract in a given time; therefore that product will be always invariable, and $\sqrt{x} \times y^2$ will be $= 1$, and $xy^4 = 1$. And this is the equation of the curve SGC, whose part contained within the given vessel, was delineated, and its equation not obscurely hinted at by Sir Isaac Newton, in prop. 36, lib. 2, Princip. who was the first that communicated to the learned world the true velocity of running water, and deduced it from its genuine principles.

But this curve is an hyperbola of the fourth order, one of whose asymptotes is the right line AS, parallel to the horizon, and the other AB perpendicular to it. Its power is the quadrato-cube of the ordinate FG, drawn to the point G, where the right line AG, bisecting the angle comprehended between the two asymptotes meets the curve. The space SADES, contained between the curve SGE, the ordinate DE, and the asymptotes AD, AS, is equal to $\frac{4}{3}$ of the rectangle HD, under the abscissa AD and the ordinate DE; and consequently the space SHE is $\frac{1}{3}$ of the same rectangle. And the solid SGEEGS, generated by the rotation of the space SADES, about the axis AD, is double the cylinder on the section EE; whence the concave solid generated by the conversion of the space SHEGS, about the same axis, is equal to the said cylinder. All which are discovered by an easy calculation by the inverse method of fluxions.

Theorem 1.—If water run out at a round hole, in the bottom of a vessel of an infinite capacity, the motion of the whole cataract of water towards the horizon, is equal to the motion of a cylinder of water whose base is the hole itself, and its height equal to that of the water, whose velocity may equal the velocity of the water running out at the hole; or is equal to the motion of the bulk of water, that runs out in any given time, whose velocity may be such, that a space equal to the height of the water may be run over in the same given time.

Demonstration of the first part.—Let another ordinate *de* be drawn to the curve SGC, very near the former ordinate DE. By the rotation of the curve about the axis AB, the ordinates DE, *de* will generate two circles, by which the nascent solid EE*ee* is intercepted. That solid is equal to the product of the height D*d* into the section EE; and its motion is equal to the product of the solid itself into its velocity, or to the product of the height D*d*, the section EE, and the velocity of the water in that section. And since it has been shown above, that the product of any section of a cataract, and of the velocity of the water in that section, is an invariable quantity; consequently the motion of the whole cataract, will be equal to the product of that invariable quantity into the sum of all the heights D*d*, or into AB, that is, equal to the motion of the cylinder on the hole and the height of the water, whose velocity is equal to that of the water running out at the hole. Q. E. D.

Corol. 1.—Having given the height of the water, the motion of the cataract will be in the ratio of the hole.

Cor. 2.—Having given the hole, the motion of the cataract will be in the sesquiplicate ratio of the height, or in the triplicate ratio of the velocity with which the water runs out at the hole.

Cor. 3.—Having given the motion of the cataract, the hole will be reciprocally in the sesquiplicate ratio of the height, or reciprocally in the triplicate ratio of the velocity.

Demonstration of the second part.—The bulk of water running out in a given time, is to the cylinder under the hole and height of the water, as the length of the space the effluent water shall run over with an equable velocity in that given time, is to the height of the water. And since the velocity which is ascribed to the bulk of the effluent water, is to the velocity of the cylinder, reciprocally in the same ratio, the quantities of motion in both will be equal. Q. E. D.

Cor. 1.—Having given the height of the water, and the bulk of the effluent water, the motion of the cataract is in the inverse ratio of the time in which that bulk runs out.

Cor. 2.—Having given the height and the time, the motion of the cataract is as the bulk of the water that runs out in that time.

Cor. 3.—Having given the height and the bulk of the effluent water, the motion of the cataract will be in the ratio of the height.

Cor. 4.—Having given the motion and height of the cataract, the bulk of the effluent water is in the ratio of the time.

Cor. 5.—Having given the motion of the cataract, and the bulk of the effluent water, the height is as the time.

Cor. 6.—Having given the time and motion of the cataract, the bulk of the effluent water will be reciprocally as the height.

Theor. 2.—If AB, as in fig. 19, be taken so as to be to BD, as DG^4 is to $DG^4 - BC^4$; and the water running out at a round hole CC, in the middle of the bottom of a given cylindrical vessel GGEE, which is always full; the motion of the cataract of water towards the horizon, will be equal to the motion of the cylinder under the hole and height AB, whose velocity is equal to that of the water running out at the hole; or it will be equal to the motion of the bulk of water that runs out in any given time, and whose velocity is such, that a space equal to the height AB, may be run over in the same given time.

Demonstration of the first part.—Let AS be drawn parallel to DG, and let the Newtonian curve SGC be supposed to be drawn with the asymptotes AS, AB, through the points GC. To have the height of the water constant, the place of the water that runs out, is to be supplied with a cylinder of water *gg*GG, descending with that uniform velocity, which is acquired by falling from A to D, as the incomparable Newton demonstrates in the above-mentioned proposition. The motion of the cataract SSGG, is equal to the motion of this cylinder by the preceding theorem. Therefore the motion of the descending water, since it is compounded of the motion of the cylinder of water *gg*GG, and of the motion of the cataract GGCC, is equal to the motion of the whole cataract SGCCGS, that is, by theor. 1, to the motion of the cylinder of water whose base is the hole, and height AB, whose velocity is equal to that of the water running out at the hole. Q. E. D.

The demonstration of the second part follows from the first.

Cor. 1.—Hence, by substituting the height AB, instead of that of the water, there arise all the corollaries of the preceding proposition.

Cor. 2.—If the vessel be of any other figure than cylindrical; or the figure of the hole be square, triangular, or any other except round; or if the hole itself be not in the middle of the bottom; or if it be made even in the side of the vessel; still the motion of the cataract will be the same, viz. equal to the motion of the prism of water on the hole, and height AB whose velocity is equal to that of the effluent water. For the same bulk of water will pass with the same velocity, as in the former hypothesis, both through the hole itself, and through each section of the cataract.

Cor. 3.—If the diameter of the vessel have a very great ratio to that of the hole, the height AD may be neglected, and the height of the vessel itself may be assumed for that of the cylinder or prism of water.

Hitherto I have considered apart, that particular case in which water, by the force of gravity, runs out at a vessel. And the rather, both because mathematicians mostly apply only this in treating of the impetus of fluids, and because I think the above explained property of the hyperbolic curve, by which the cataract of falling water is formed, not unworthy the consideration of geometricians. Otherwise that case might be easily deduced from the following general theorem.

Theor. 3.—If water run through any full pipe ABCD, fig. 20, in the direction of the right line EF, to which both the orifices of the pipe, AB and CD are perpendicular; the motion of the water towards the orifice CD, or the motion of the impediment, which in that orifice opposes the motion of the whole water, is equal to the motion of the prism of water on any section of the pipe GH, and the line of direction or the length of the pipe EF, which moves with the same velocity with which the water flows through that section: or is equal to the motion of a bulk of water, which runs out at the pipe in any given time, and whose velocity is such that a space equal to the length of the pipe is run over in the same given time.

Case 1.—Let the line of direction be any right line EF.

The first part is easily demonstrated, in the same manner as *Theor.* 1. For, the product of any section of the pipe GH, and of the velocity of the water in that section, is an invariable quantity.

The second part follows from the first.

Case 2.—If the line of direction ABCDE, fig. 21, consist of several right lines AB, BC, CD, EF, inclined to each other, the motion of the water will be the same. For, the motion of the water, in the whole compound pipe ABCDE, consists of the motions of the water in the several parts of it, AB, BC, CD, DE, added together. For water, flowing according to the right line AB, and changing that direction into another, by which it is carried according to the right line BC, loses nothing of its motion: for fluids do not follow the same laws as are observed in the motion of solids, when their direction is changed. Otherwise the motion of a fluid, on changing its direction into another perpendicular to the former direction, would be entirely stopped; which experiment by no means shows. Moreover, water running out at the hole of a vessel, whether it be carried downwards, or according to the plane of the horizon, or straight upwards, has the same velocity. But if at any time it should be found, either by more subtile reasoning, or by experiment, that some diminution of the motion proceeds from the change of direction, regard is to be had to it.

If the line of direction AB, fig. 22, be a curve, it is to be referred to this case; for it may be conceived as consisting of numerous small right lines.

Case 3.—If the pipe AB, fig. 23, be divided into several branches BC, BD, BE, of equal length, the motion of the water will be found in the same manner, assuming for the line of direction the length ABD, consisting of the length of the principal pipe AB, and the length of any branch BD. For it is the same thing, whether the water run from the principal pipe towards the branches, or from the branches towards the principal pipe. But if the branches be unequal, the motion of the water in each branch must be found out, taking for the line of direction the length, consisting of the length of each branch and that of the principal pipe. This is easily deduced from case 2.

Case 4.—If the equal branches, into which the pipe AB, fig. 24, is divided, unite again into one pipe FG, to find the motion of the water, we must take for the line of direction the whole length ABDFG, consisting of the length of the principal pipe AB, of any branch BDF, and of the united pipe FG. If the branches be unequal, the motion of the water in each must be found, and the sum of their motions added to the motion of the water in the united pipe. This follows from case 2 and 3.

Corol. 1.—Having given the length of the pipe, and any section thereof; the motion of the water will be in the ratio of the velocity, with which the water runs through that section.

Cor. 2.—Having given any section, and the velocity of the water running through that section, the motion of the water will be as the length of the pipe.

Cor. 3.—Having given the length of the pipe, and the velocity of the water in any section, the motion of the water will be in the ratio of that section.

Cor. 4.—Having given the motion of the water and any section, the length of the pipe will be in the inverse ratio of the velocity.

Cor. 5.—Having given the motion of the water and the length of the pipe, any section will be reciprocally as the velocity.

Cor. 6.—Having given the velocity in any section, and the motion of the water, that section will be in a reciprocal ratio of the length.

Cor. 7.—Given the length of the pipe, and the bulk of water running out in any determinate time, the motion of the water will be reciprocally at that time.

Cor. 8.—Given the length of the pipe, and the time, the motion of the water will be as the effluent mass.

Cor. 9.—Given the time and the mass of effluent water, the motion of the water will be as the length of the pipe.

Cor. 10.—Given the motion of the water, and the length of the pipe, the effluent mass is in the ratio of the time.

Cor. 11.—Given the motion of the water, and the effluent mass, the time will be as the length of the pipe.

Cor. 12.—Given the time and motion of the water, the effluent mass will be reciprocally as the length of the pipe.

Cor. 13.—If two masses of water, having a contrary motion, meet each other directly, and if both the superfices with which they impinge, as also the velocities, with which these superficies move towards each other, be equal; and should one of the masses be equal to a small drop of water, and the other be the whole quantity of water contained in the ocean, or even an infinite quantity of water; it is possible, for this small drop of water not only to sustain the whole quantity of water in the ocean, or an infinite quantity of water, but after concourse, to proceed in its motion in the same direction, and with the same velocity as before, and even repel the other towards the contrary part. Which is a surprising paradox in hydraulics.

Cor. 14.—If a certain mass of water run from a wider into a narrower tube, through a pipe consisting of two cylindrical tubes of unequal diameters, and if the motion of the water be neither diminished nor increased in its course; as soon as the first part of the water enters the beginning of the less tube, it will immediately begin to run slower, and by its continual efflux from the wider into the narrower tube, the water will gradually be more and more retarded in the narrower tube, till the whole come into that tube. But the contrary will happen, when water runs from a narrower into a wider tube. Which is another paradox in hydraulics. But water is supposed to cohere in all its parts. These two corollaries arise from case 1.

Cor. 15.—From case 2 there is given a method of estimating the motion of the blood in any artery.

Cor. 16.—Given any two arteries, that transmit an equal mass of blood, the impetus of the blood is greater in the more remote artery from the heart than in the nearer. Which is a paradox in the animal economy worthy observation.

Cor. 17.—From case 3 there arises another paradox in the animal economy, viz. that the motion or impetus of the blood is greater in all the capillary arteries, taken together, than in the aorta itself. And in like manner it is greater in the capillary veins than in the arteries.

Cor. 18.—From case 4 is deduced a method of determining the motion of the blood in any vein.

Cor. 19.—From the same case is deduced a third paradox in the animal economy, viz. that the impetus of the blood is greater in any vein than in its corresponding artery, and consequently that it is greater in the vena cava than in the aorta.

Problem 1.—To find the motion of the air, expired from the lungs.

Let l be $=$ the length of the whole wind-pipe, from the mouth and nostrils to the extreme ramifications of the trachea; $q =$ the quantity of air emitted from the lungs in a mean expiration; $Q =$ the quantity of air in the strongest expiration; $t =$ the time of the mean expiration; $T =$ the time of the strongest expiration. Then, by theor. 3, case 3, the motion of the air emitted from the lungs, in a mean expiration, is $= \frac{ql}{t}$, and in the strongest expiration $= \frac{QL}{T}$; that is, the motion of the air, expired from the lungs, is equal to the motion of the bulk of air, emitted in one expiration, whose velocity is such as to run over the length of the whole wind-pipe in the time of expiration. Q. E. I.

Borelli, by an experiment, defined the quantity of air emitted in a mean expiration, to be about 18 or 20 cubic inches. But this quantity is different, not only in different persons, but even at different times in one and the same person; I made the experiment in the following manner: I appended a weight to the lower part of a moistened bladder, and fitting to its upper part a glass tube about an inch in diameter, and stopping my nostrils, gently inspired air into the bladder, for the space of three seconds of time, the weight in the mean time resting on a table. Afterwards I immersed the bladder, with its included air and appended weight, into water, contained in a cylindrical vessel, carefully marking the height to which the water rose. Then expressing the air out of the bladder, I immersed it a second time with its weight into the water. On which the bulk of water which, poured into the vessel, made up the height above marked, was easily found. Having repeated the experiment 10 times, and adding together the several quantities found each time, their 10th part, or the mean bulk of water poured into the vessel, was found equal to 35 cubic inches; which is the bulk of air contained in the bladder, and adding about a 12th part more, or 3 cubic inches, by reason of the condensation of the air by the coldness of the water, it being in the winter time, there arose 38 cubic inches. Besides, there must somewhat be added, both on account of the pressure of the water on the bladder, and also on account of the vapour, emitted with the breath, and turned into moisture, which must necessarily happen both from the coldness of the water and from the contact of the moistened bladder. Therefore I estimated the quantity of air emitted in gentle expiration in 3 seconds of time, in round numbers, at 40 cubic inches. In the strongest expiration I emitted 125 cubic inches in one second of time. And by this sort of expiration, which, with a violent straining, I had continued till almost choaked, I emitted 220 cubic inches. Whence it appears, to mention this by the bye, that there remains much more air in the breast than is emitted by one mean expiration.

If therefore l be put $= 2$ feet; $q = 40$ cubic inches; $\text{Q} = 125$ cubic inches; $t = 3''$; $\text{T} = 1''$; the specific gravity of air to that of water, as 1 to 1000; a cubic foot of water $= 1000$ ounces avoirdupois; the mean motion of the air, expired from the lungs, will be equal to the motion of 4 scruples 9 grains weight, which runs over 1 inch in a second of time, or equal to the motion of $1\frac{1}{3}$ grain weight, which in the same time may run over the length of 5 feet 7 inches; which is the velocity of the air emitted through the larynx, supposing the section of the larynx $= \frac{1}{5}$ of a square inch.

The greatest motion of the air emitted from the breast is equal to the motion of about $1\frac{3}{4}$ oz. weight, which runs over 1 inch in a second of time; or equal to the motion of $1\frac{1}{3}$ gr. weight, which runs over 52 feet in the same time; which is the velocity of the air emitted through the larynx in the strongest expiration.

Cor. 1.—Given the quantity of air and length of the wind-pipe, the motion of the air is in the inverse ratio of the time of expiration.

Cor. 2.—Given the bulk of the air and the time, the motion will be in the direct ratio of the length.

Cor. 3.—Given the length and time, the motion will be as the quantity of the air.

Cor. 4.—Given the motion and quantity of air, the length will be in the direct ratio of the time.

Cor. 5.—Given the motion and length, the bulk of the air will be directly as the time.

Cor. 6.—Given the motion and the time, the bulk of the air will be reciprocally as the length of the wind-pipe.

Cor. 7.—The motion of the air is in a ratio compounded of the quadruplicate ratio of any similar diameter of the animal, and the inverse ratio of the time of expiration; or in a ratio compounded of the ratio of the weight of the whole animal, the subtriplicate ratio of the same weight, and the reciprocal ratio of the time. For the weight of the animal, the cube of any similar diameter, and the bulk of the expelled air, are all in the same ratio. And animal bodies are supposed to be machines alike formed.

Scholium.—We are to conceive the length here assumed, either as the length of the wind-pipe, supposing all the branches of the trachæa equal in length; or the mean length between the different lengths, if the branches are unequal.

Prob. 2.—To determine the impetus or impression made on the internal surface of the lungs by expiration.

Since re-action is equal and contrary to action, it must necessarily happen, that with what motion the air to be expired is urged by the internal superficies of the lungs, with just so much is the superficies of the lungs mutually repelled by the air; whence, by the preceding prob. the said impetus in a mean expiration is $= \frac{ql}{t}$ and in the strongest expiration $= \frac{Ql}{T}$. Q. E. I.

Hence, by supposing the same things as before, the mean impetus of the air on the lungs, is equal to the motion of about $1\frac{1}{2}$ drachm weight, which in a second of time may run over 1 inch; or equal to the motion of a weight of 19 pounds, that runs over $\frac{1}{1642}$ part of an inch; which is the velocity of the air on the internal superficies of the lungs: supposing, with Dr. James Keill, that the internal superficies of the lungs is equal to about 21900 square inches. But the greatest impetus of the air on the lungs, is equal to the motion of a weight of about $1\frac{3}{4}$ ounce, that runs over 1 inch in a second of time; or equal to the motion of a weight of 19 pounds, that runs over $\frac{1}{175}$ part of an inch in the same time; which is the velocity of the air on the superficies of the lungs in a strong expiration.

Cor. 1.—From this proposition arise the corollaries subjoined to the preceding prob.

Cor. 2.—A mean impetus incumbent on a part of the superficies of the lungs, equal to a section of the larynx, is the motion of a weight of $\frac{1}{1432}$ part of a grain, that runs over the space of 1 inch in a second of time; or the motion of a weight of $1\frac{1}{2}$ grain that runs over $\frac{1}{1642}$ part of an inch in the same time.

But the greatest impetus on an equal superficies, is the motion of a weight of $\frac{1}{131}$ part of a grain, that runs over 1 inch; or the motion of a weight of $1\frac{1}{3}$ grain, that runs over $\frac{1}{175}$ part of an inch each second of time.

Cor. 3.—The impetus of the air, impressed upon the lungs in a mean expiration, is equal to the motion of a column of water, which runs over the space of 1 inch in a second of time, whose base is the internal superficies of the lungs, and height the $\frac{1}{68458}$ part of an inch. And the height of the column is $\frac{1}{7300}$ part of an inch in the strongest expiration.

Cor. 4. The impetus incumbent on a superficies equal to the greatest circle of a globule of blood, in a gentle expiration, is $\frac{1}{14}$ part of the weight of that globule; but in a strong expiration $\frac{2}{3}$ part of the same weight, which runs over the space of an inch in a second of time.

I shall here show the manner of measuring the diameters of the globules of blood, since it may be of use to determine the magnitudes of other minute objects. I wound a fine and pretty long hair several times round a small needle, in such a manner, that all the circumvolutions touched each other exactly, as was discovered by the microscope. Then taking with a pair of compasses the distance between the extreme circumvolutions on each hand, I applied it to a diagonal scale, and divided the space thus found on the scale by the number of convolutions, whence is found the breadth of one convolution, or the diameter of the hair. Afterwards cutting the air into very small shreds or pieces, I strewed them on the plane of a microscope, on which I spread a little blood in such a manner, that the globules could be distinctly seen. On viewing these shreds with the microscope, I found them in some places disposed so conveniently, that I could reckon how many globules answered to the diameter of a shred. But the shreds were of unequal diameters, because the hair was more slender towards the extremity than nearer the root; so that sometimes 7 or 8, at other times 12 or 13 globules answered to a transverse section of the hair; and having repeated both experiments several times, I at length estimated the mean diameter of the hair to be the 324th part of an inch, and the diameter of a globule of blood the 10th part of the diameter of the hair, or the 3240th part of an inch.

Cor. 5.—The impetus, which the internal superficies of the lungs sustains by the expiration of the air, is less than the motion of the gentlest dew that falls.

Scholium.—In the solution of the two preceding problems, I have not considered the obstacle to the egress of the air out of the lungs from the friction of the sides of the trachea and its branches, since that is very inconsiderable; nor does it seem, that it can be accurately enough estimated by any experiment.

Nor have I been very solicitous about making nice numeral calculations, having only proposed to exhibit a method of estimating, with greater certainty than seems to have been hitherto done, the forces with which the air in expiration acts on the blood-vessels that creep along the internal superficies of the lungs. Whence it may be known, whether these forces be equal for producing these effects, which are ascribed to them by some of the most learned medical authors.

Prob. 3.—To determine the impetus of the blood in the cava near the right auricle of the heart; or the motions of the blood through all the veins and arteries, besides the pulmonary.

Let q be $=$ the quantity of blood thrown into the aorta in one systole of the heart; $l =$ the mean length of the whole arterio-venous duct, having a regard to the longer and shorter branches; $t =$ the time between two pulsations. Hence, by theor. 3, case 4, the impetus sought is $= \frac{ql}{t}$; that is, the impetus of the blood in the cava, is equal to the motion of the mass of blood, thrown out at one systole into the aorta, whose velocity is such, as with it the whole length of the veins and arteries may be run over, in the time between two pulsations. Q. E. I.

If in the human body we suppose $q = 2$ ounces avoirdupois; $l = 6$ feet; $t = \frac{3}{4}''$. Then the impetus of the blood in the vena cava will be equal to the motion of a weight of 12 lb. that runs over the length of an inch in each second of time; or to the motion of a weight of 2 lb. that runs over $\frac{1}{2}$ a foot in the same time, which is nearly the velocity of the blood in the cava; for, from Dr. Keill, we suppose the section of the cava to be $\frac{3}{4}$ of a square inch.

Cor.—From this problem arise, mutatis mutandis, all the corollaries of the first problem.

Prob. 4.—To determine the absolute motion of the blood in the cava, or the motion of the blood through all the veins and arteries, besides the pulmonary, omitting the resistance of the vessels.

Let the natural velocity of the blood, be to that velocity with which the blood would flow, after removing all resistance, as 1 to x. Then since by the cor. of the preceding prob. and cor. 1 of prob. 1, the motion of the blood is in the ratio of the velocity, hence the motion sought will be $= \frac{xql}{t}$. Q. E. I.

If the proportion found by experiment, by Dr. Keill, be admitted as near the truth, x will be $= 2.5$.

Whence, supposing the same things as in the preceding prob. the absolute motion of the blood in the cava, is equal to the motion of a weight of 30 lb. that runs over the length of an inch in a second of time; or to the motion of a weight of 2 lb. that runs over $1\frac{1}{4}$ foot in the same time; with which velocity nearly, the blood, being free of all resistance, will be carried through the cava.

Prob. 5.—To find the motion of the blood in the pulmonary vein near the left auricle of the heart; or the motion of the whole mass of blood flowing through the lungs.

Besides the symbols made use of in prob. 3, let λ be = the mean length of the arteria venosa, whence, by theor. 3, case 4, the motion sought is found = $\frac{q\lambda}{t}$; that is, the motion of the blood, flowing through the lungs, is equal to the motion of a mass of blood, thrown into the pulmonary artery in one systole, with such a velocity, as to run over the length of the pulmonary veins and arteries in the time between two pulsations. Q. E. I.

If in the human body λ be supposed = $1\frac{1}{2}$ foot, the motion of the blood in the lungs will be equal to the motion of a weight of 3 lb. that runs over the space of an inch in a second of time.

Prob. 6.—To determine the absolute momentum of the blood in the pulmonary vein.

By the same argument, as that made use of in prob. 4, the motion sought is found = $2.5 \times \frac{q\lambda}{t}$. Q. E. I.

And by supposing the same things as above, the absolute motion of the blood that flows through the lungs, is equal to the motion of a weight of $7\frac{1}{2}$ lb. that in each second of time runs over the space of an inch.

Scholium.—By Dr. Keill's experiment is determined the ratio, which the natural velocity of the blood, through the aorta and its branches, has to that resistance with which the blood would flow through the same, if the resistance of the arteries and preceding blood was removed. We have transferred that ratio to the blood flowing through the pulmonary artery; because, by either taking off or diminishing, according to any ratio, the resistance the blood meets with in flowing through both arteries, the blood must necessarily be equally accelerated in both; for unless that happened, both ventricles of the heart either will not be contracted in the same time, or will not discharge the same quantity of blood; neither of which can possibly happen, without very much disturbing and endangering the whole machine.

Cor. To the three preceding problems.—Hence follow the corollaries, mutatis mutandis, subjoined to the 5th prob.

Scholium. To the four preceding problems.—It is to be observed, that though the velocity of the blood, flowing both through the lungs, and also through the rest of the body, be not really equable, yet here it is supposed such, in order to find its mean motion.

General Scholium.—Should the numbers, which here and there are added to the symbols, appear less accurate to any one, he may easily, by finding by experiments numbers that approach nearer the truth, correct the above-mentioned examples of motion, either by the help of the propositions themselves, or their corollaries.

An Account of the Sinking of three Oaks into the Ground, at Manington, Norfolk. By Peter Le Neve, Esq. Norroy King at Arms, and F. R. S. N° 355, p. 766.

On Tuesday, July 23, 1717, near the seat of Sir Charles Potts, Bart. at Manington, nearly midway between Holt and Aylsham, and about 7 miles from the coast near Cromer, an oak sunk, with the roots and ground about it, and not long after, at about 40 yards distance, two other oaks sunk in the same manner into a much larger pit, about 33 feet diameter, but the former not quite 18. These, as they sunk, fell across, so that obstructing each other, the roots of only one of them reach the bottom, whereas the first stands perpendicular.

When the first tree sunk it was observed, that the water boiled up in the hole, but on the sinking of the greater pit, it drained off into it from the former, which now continues dry. Its depth to the firm bottom is 9 feet 3 inches, and the tree that stands upright in it, is 3 feet 8 inches in girt, and its trunk about 18 feet long, half of which is now within the pit. In the bottom of the greater pit, there is a pool of water about 8 feet diameter, its surface 11 feet 3 inches below the ground, and the trees in this pit are much of the same length with the others, but somewhat smaller, the one being in girt 3 feet 5 inches, and other only 2 feet 9 inches.

The soil on which these trees grew is gravelly, but the bottom is a quicksand over a clay, on which there are springs, that feed large ponds adjoining to Sir Charles Potts's house, at about a quarter of a mile from these pits.

The nature of the soil seems to afford a reasonable conjecture at the cause of this odd accident: for the springs running over the clay at the bottom of a bed of very fine sand, such as quicksands usually are, may reasonably be supposed in many ages to have washed away the sand, and thus to have excavated a kind of subterraneous lake, over which these trees grew; and the force of the winds on their leaves and branches agitating their roots, may well have loosened the sand under them, and occasioned it to fall in, more frequently than elsewhere; whence in time the thin bed of gravel only being left, it might become unable to support its own weight, and that of the trees it bore. That this is not a bare conjecture, may appear from the boiling up of the water at first in the less hole, and its standing in the larger and lower. And if it shall be found that it was a very windy day when this accident happened, it will much add to the probability of this solution.

An accident not unlike this lately happened in Fleet-street, London, by the defect of the arched roof of a very deep common sewer. The earth gradually falling into the sewer was carried away by it, so as not to obstruct the water; and the continual tremor of the ground, occasioned by the constant passing of carts and coaches, gradually shook down the earth, so as to leave a very great cavern, the top of which at length became so very thin, that one day a weighty cart having just passed it, a large space of the pavement sunk in, in the middle of the street, not without hazard to a coach then driving by.

A Rectification of the Motions of the Five Satellites of Saturn, with some accurate Observations of them. By the Rev. Mr. James Pound, F. R. S. N° 355, p. 768.

Above 30 years since, Mr. Cassini communicated his discovery of two new satellites of Saturn, which made their number five; and his account of them is to be seen in N° 187. About the same time M. Huygens presented to the Society the glasses of a telescope 125 feet long, with the apparatus for using them without a tube; by help of which we might have satisfied ourselves of the reality of these discoveries. But those here who first tried to use this glass, for want of practice, finding some difficulties in the management of it, caused it to be laid aside for some time. Afterwards it was designed for making perpendicular observations of the fixed stars passing by our zenith, to try if the parallax of the earth's annual orb might not be made sensible in so large a radius, according to what Dr. Hook had long since proposed; but in this we miscarried also, for want of a place of sufficient height and firmness, on which to fix the object-glass, so that it lay by neglected for many years.

In the mean time we could not but remark a great reserve in the French astronomers in regard to these satellites, of which they have given us in their memoirs no observations till very lately, nor have they seemed willing to show them in their glasses to such as requested it: so that it might possibly occasion in some persons a suspicion of the reality of this discovery; and Mr. Derham, having borrowed the Society's long glass, could not by it assure himself that the small stars he sometimes found about Saturn were really his satellites, their situation not agreeing with their places derived from the tables of their motions exhibited in N° 187, besides that he wanted a sufficient height to raise the object-glass, so as to view Saturn to advantage, above the vapour of the horizon. But in the Memoirs for 1714, M. Cassini, the younger, has given us some observations which clear up the point, and by showing the errors of those first tables, has made us certain that we have seen the whole satellitum of Saturn ourselves. Other observations of these satellites are in the Memoirs for 1715.

Having by the help of these late observations corrected the motions of the satellites, which it was not possible for their first discoverer to settle truly, in the short interval before 1687; and having fixed their epochs for the present year, we were enabled to know where to expect them with more certainty, and to distinguish them from one another, and from the small fixed stars appearing with them. And Mr. James Pound having, by means of his steeple of Wansted, provided a gnomon high enough for the purpose, and having fitted a very commodious apparatus for using the Society's said long telescope, soon discovered by it all these five satellites; and lately communicated the following very curious observations.

1718, April 21^d 10^h 40^m, the 3d and 4th satellites of Saturn were in Apogæo, a little past their conjunction with Saturn, a perpendicular from the 4th to the transverse axis of the ring, or line of the Ansæ, fell a little without the eastern Ansa; and a line through the 4th and 3d touched the eastern limb of Saturn.

The first was northward of the line of the Ansæ, and therefore in the Apogæon semicircle also, distant from the said line about as far as the end of the conjugate axis of the ring was from the centre of ♄, viz. nearly $\frac{3}{4}$ of Saturn's semidiameter; and it was about a semidiameter of the ring from the western Ansa.

The second was a very little southward of the line of the Ansæ, and therefore in the Perigæon semicircle, above a semidiameter of the ring, or about the semidiameter of the ring + the semidiameter of ♄, from the western Ansa. And the third, first, and second, were in a straight line.

At 10^h 50^m a perpendicular from the 3d to the line of the Ansæ, fell almost on the middle of the bright part of the eastern Ansa, but somewhat nearer the centre than the said middle.

April 22^d 11^h 5^m, the four innermost satellites were all eastward of ♄. The second and fourth in the Apogæon, and the first and third in the Perigæon semicircle. A line through the second and fourth touched the south-east limb of ♄. A line passing through the third, and the end of the conjugate axis of the ring, was parallel to the line of the Ansæ.

At 11^h 10^m, a perpendicular from the first to the line of the Ansæ, fell on the eastern extremity of the ring.

These distances and directions were taken only by estimation, and not by any actual measurement. The fifth, or outermost, satellite being at this time near its greatest elongation eastwards, among several very small telescopic stars, he could not determine its position. But by observing the motion of this some other nights before, he was now fully satisfied, from the motions rectified as above, that there are five satellites of Saturn,* as Mr. Cassini had long since asserted.

In the bright part of each ansa was a darkish ellipse nearer to the outside than the inside of the ring, as if it was composed of two rings near to each other.

On the body of ♄, beside the ring on the south side, there appeared on the north side a zone not so far from the centre as the ring, and not much unlike the smallest of Jupiter's belts. These appearances were first taken notice of by Mr. Cassini, as may be seen in Phil. Trans. N° 128.

Tables of the Motions of Saturn's Five Satellites, corrected according to later Observations, and rendered conformable to the Heavens. N° 356, p. 776.

This paper is anonymous; but it has the appearance of being drawn up by Dr. Halley. However, as these tables are not so correct as those that were afterwards given in Dr. Halley's large collection of Astronomical Tables, printed in 1750, and elsewhere, it can be of no use now to retain the present tables in this place.

Concerning the Situation of the ancient City of Anderida, and other Remains of Antiquity in the County of Sussex. By Dr. John Tabor of Lewes. N° 356, p. 783.

The southern part of this island was the greatest, if not the only acquisition, made to the Roman empire, from the death of Tiberius to the 6th year of Claudius; and we may well suppose was not passed over in silence by Tacitus: but his 4 books of Annals, containing the transactions of those 9 years, we have reason to fear are irretrievably lost. From the mention Suetonius makes of Claudius's expedition hither: it is commonly insinuated that his conquest here cost no blood. Our countryman Bede was of that opinion; for, in his account of Claudius, the words of Suetonius are copied. But Dio Cassius, from whom we have the most particular information of that war, gives a different account of the matter: he takes notice of at least 4 battles, fought with the Britons (before Claudius came over) by Aulus Plautius. In the first, Caractacus was defeated; in the second, Togodumnus, and, as may be inferred

* Dr. Herschel has since, viz. Anno 1787 and 1788, discovered two more satellites to this planet, both within the orbits of the former five. So that seven satellites in all have been discovered to the planet Saturn.

from his words afterwards, slain. From the manner of his delivering the story, all those battles seem to have been fought, south of the river Thames, and north of the Sylva Anderida, except the last; and that in the first campaign the conquests of Plautius could not have extended beyond Kent and Surry; for it is likely that the first two actions happened about the skirts of the Sylva Anderida, eastward of the river Medway; and the third, which continued two days, on the banks of that river; because, from the river, where they were routed two days successively, the Britons retiring, assembled their strength again before their fourth overthrow, in that part of Kent which borders on the Thames, not far from its entrance into the sea; and having passed it, were followed by Plautius's Germans, and on the other side put to flight; which was the 4th action mentioned by Dio. Claudius having been sent for, came the 2d year with powerful succours, to the assistance of Plautius; who with his forces waited his arrival near the Thames, probably in that large strong camp, yet to be seen not far from Bromley in Kent, on the river Ravensbourn. The emperor joining him, immediately crossed the Thames; overthrew the Britons posted on the other side to resist him; advanced to Cynobelin's chief residence Camalodunum. and took it; then receiving homage of some states, returned to Rome.

Considering therefore that Claudius staid but 16 days in this island, we must conclude his dispatch was great, and that his progress could not have been through more parts than Kent, Essex, Hertfordshire, Middlesex, and Surry. As to what else relates to the British war in the time of Claudius, we have no account from Dio, excepting that 3 years after, Titus rescued his father, Vespasian, when in great danger. But where Suetonius treats of Vespasian's life we are told, when that emperor commanded in Britain for Claudius, that he fought 30 battles, subdued two of the most powerful nations, won 20 towns, and brought the Isle of Wight under the Roman obedience.

Pliny tells us, that cherries were not known in Italy till the 680th year of Rome, when L. Lucullus first brought them thither from Pontus; and that in 120 years they were so increased, that not only many other countries, but Britain also, was supplied with them; which must have been about 3 years after Claudius himself had been here. The usual landing from Rome being then in the county of Kent; that fruit was doubtless there first planted; and the soil well agreeing with it, may be the reason that the best and greatest quantity of it is yet there to be had.

Agricola, in the second year of his lieutenancy here, when in winter quarters, pursued conciliating measures to gain the Britons, by making them acquainted with the Roman manners; he not only in private persuaded, but publicly helped and encouraged them to build temples, places for common assemblies, and private houses, after the Roman mode; he took care to have the principal youth instructed in the liberal arts: he allured them to affect the habit of the Romans; and last of all, to engage them the more firmly, encouraged a taste for Roman luxury, by introducing the use of shady piazzas and baths, and their way of banqueting. From hence it may be inferred, that should never any other tokens of the antiquity of these works be found, yet would the bath denote the age of the pavement, and set it near as high as the most early time that the Romans had any real authority in this island.

Malmsbury says, that in his time, there were here only the Abbeys of Battle and Lewes, and those not long erected. The earliest mention made of it, is by Bede, who informs us, that Bishop Wilfrid, in the year 678, being driven from his province of Northumbria by King Ecgfrid, settled at Selsey in 680, and staid 5 years, labouring in the conversion of the neighbouring parts. Bede spent most of his time in the monasteries of Wiremouth and Jarrow, and travelled little; so could leave us but few particulars.

The next records we have, are those of Ethelwerd, the Chronicon Saxonicum, and Henry, Archdeacon of Huntingdon. But that we may the more clearly apprehend the ancient state of this country, we must look into the best map of it. At the west end, we find West Harting and Stansted, distant from each other 6 or 7 miles. Imagine a straight line to be drawn from Harting to Bourne, near Pevensey, and another to be drawn from a point which must be little south of Stanstead to Brighthelmstone; what lies north of these lines, is the Weald or Low-lands, formerly the Sylva Anderida; that which is comprehended between these lines, and bounded by the sea, from Brighthelmstone to Bourne, is the Downs, so famous for their pleasant situation and fruitfulness. The part south of these lines, is a flat champaign ground, ending like a wedge at Brighthelmstone. These two last parts were those only that were inhabited in Bede's time; they contain not more than two-fifths of the whole county; which must be the reason why Bede said Sussex consisted not of more than 7000 families or farms; whereas in another place he computes Kent to have 15000 families.

In the three accounts above-mentioned it is agreed, that in the year 477, Ella, with his three sons, Cymen, Wlencing, and Cissa, landed his forces at Cymenes-ora, not far from which he routed the Britons, and drove them into the Weald, Andredesteige. Their farther progress is most distinctly and naturally delivered by the Archdeacon of Huntingdon, in these words: "The Saxons possessed themselves of the sea-coast of Sudsexe, spreading gradually over the country, till the 9th year after their landing in Britain; then penetrating still more boldly into the country, the kings and rulers of the Britons met at Mercredesburne, and fought with Ella and his sons, and the victory was rather doubtful; for both armies having suffered very much, they parted. And Ella sent to his countrymen to request their assistance.

This action was in the 9th year after Ella's first footing here, 3 years before Hengist's death, Anno Dom. 485. It so weakened Ella, that we hear no more of him till he received his supplies from Germany: which came not, according to H. Huntingdon, till the first year of the Emperor Anastasius, 3 years after Hengist's death, and 6 years after the hard battle, viz. Anno Dom. 491.

Being thus strengthened, Ella moved again, besieged Anderida, in Huntingdon's words, Urbem munitissimam, at last forced the place; and by reason of the stout resistance the besieged made, savage like, he left not a soul alive, and razed the city, which in Huntingdon's time remained desolate.

As to the field where the battle was fought, the Saxons extending their power eastward, the check that was given them, in all probability must have been where they pushed on their victories; and it being near Mercredesburne, this Bourne near Pevensey, may be the place meant, since it sounds like the latter part of that name, and likewise that Anderida, the Briton's last stake and support, was not far from it. It is probable therefore the battle was fought on the Downs, between the camp last mentioned at Burling-gap and East Bourne; for there are no where on the Downs marks of a greater battle than there; because, from the top of that very high cliff, by the inhabitants called the Three Charles, and by mariners Beachy Head, to Willington Hill, which is 4 miles, the ground is full of large tumuli, or places of burial; and in many parts within that tract, where the position of the ground seems to offer, there are deep trenches and banks, probably breast-works made to defend the front of an army; and the tumuli on each side of them seem to show that there was no small struggle, in forcing as well as defending them.

From these arguments, Anderida must have been somewhere in Sussex, not in the west but east part of it, and not far from the east end of the Downs, near the sea. From the bath, pavement, coins, and bricks, it is certain the Romans had once a station, and not a short one, at this place near East Bourne; from the large extent of foundations about the place where these were discovered, that there was a large town or city there; from the common height those foundations bear under the surface of the ground, that the buildings they sustained were effectually levelled or razed; and from the coals dug up among the rubbish, it is evident that part was burnt; all which circumstances well enough agree with the account given us of Anderida.

The situation likewise of a town here, gives reason to suppose it was a place of importance, and whence it had its name; no part hereabouts being anywise so convenient for a secure settlement: or for such a use as the Romans might have occasion to make of it. We are informed by Cæsar, that the maritime parts of Britain, speaking of what he saw, which was the south-east, were inhabited by people from Belgium; and that they called their settlements by the name of the places from whence they came. It was the opinion of Tacitus also, that those who inhabited next to Gaul came from Gaul. And Bede says, the tradition in his time was, that the southern part of the isle was peopled from Bretaign. In the third and seventh books of Cæsar's Commentaries, mention is made of the Andes, a city and a people belonging to it among the Celtæ, inhabiting on the sea-coast. Time varying the names of things, near 200 years after Cæsar, Ptolomy calls the city Anderidum; and near 250 years after him, when the Notitia Imperii, now extant, was in use, the Classis Anderetianorum is registered, and the residence of their admiral fixed at Paris. From whence it is to be inferred, that though the capital of the Andes might have been Angers near the Loire, yet their country had on the north the British channel, and on the east the Seine, for its bounds. The British coast about East Bourne is the nearest of any to the mouth of the Seine; therefore, according to the usage before Cæsar's time, the name of Anderida there is readily accounted for. Moreover, this place seems most naturally seated for giving an appellation to the great wood, to which it adjoined; for, as itself is on the shore, so also the Sylva Anderida here came very near the shore, and a large part of it might be seen from the sea before it.

Excepting the want of a navigable river, the spot of ground where this old town stood, yields to none in the county for importance and pleasure: for here, like a wedge, ends the firm soil of the Downs; nature has shaped it like an equilateral triangle, having each side half a mile in length: towards the sea, on the southern side, it is fenced by a low cliff, of 12, 15, and in some places 20 feet high. On the northern side is a morass, with a large rivulet of very good water. Between the west side and the Downs lies a small valley, by which advantage there was formerly a harbour, capable of holding a small fleet: the banks on each side of it are an evidence it was sunk by industry; but by weeds and gravel from the sea, and by mold annually added, as is observable in vallies, it is now so raised, that it is never flowed but at high spring tides, when a strong wind forces the waves into it.

As the situation described rendered this place strong, so it is very pleasant also: for the ground is high enough for a good prospect of the low lands adjoining, and the country towards Battle; besides, it has a commanding view over that bay, which is between Beachy Head and Hastings. If the use made of it by the Romans was to guard the coast, there was this advantage belonging to it, that a sentinel on the top of Beachy, not 2 miles from it, in a clear day, without turning his body, might see the Isle of Wight, the hills in France near Bologne, and the Ness in Kent; so that from the Ness to Selsey it must have been a small sail that could escape his eye.

On the Construction and Measure of Curves: by which many Infinite Series of Curves are either measured or reduced to Simpler Curves. By Colin Maclaurin, Professor of Mathematics, in the New College at Aberdeen.* N° 356, p. 803. *Translated from the Latin.*

Since in every curve line there is a certain regularity of curvature, though perhaps intricate, according to which the figure is described; therefore geometricians define the various characters of curves by an equation, expressing the relation of the ordinates to the abscisses of any axis or diameter. But as the same may be done from considering the curves in respect to one given centre, nay the very simple uniformity of nature often requires that this method be

* Mr. Maclaurin, a most eminent mathematician and philosopher, was the son of a clergyman, and born at Kilmoddan in Scotland, in the year 1698. He studied at the university of Glasgow, whither he was sent at eleven years of age, and where he remained 5 years, with the most intense application to learning, particularly the mathematics, and to such good purpose, that it is said in his 16th year he had invented many of the propositions in his Geometria Organica, afterwards published, and when also it is probable he wrote the paper on curves here printed in the Philos. Transactions. In his 15th year he took the degree of M. A. on which occasion he composed, and publicly defended, a thesis on the power of gravity, with great applause. In 1717, at 19 years of age, he was appointed professor of Mathematics at the New or Marischal College at Aberdeen; and 2 years after he visited London, when he was chosen a fellow of the Royal Society, to which he proved afterwards so useful and honourable a member. In 1724 he gained the prize of the Royal Academy of Sciences, for a memoir on the percussion of bodies; and in 1725 he was chosen Professor of Mathematics at Edinburgh, which he enjoyed to the time of his death in 1746, at 48 years of age. After the publication of Bp. Berkley's Analyst, Mr. Maclaurin began to write an answer to it, which he gradually extended to a complete treatise on the doctrine of Fluxions, which was published at Edinburgh in 1742, 2 vols. 4to. Mr. Maclaurin lent his advice profitably in several public and private works. Thus, he was consulted by the government on the passage by the north pole to the south seas; and on settling the geography of the Orkney and Shetland isles; on constructing various machines, the conveying of water, &c.: he settled rules for the Commissioners of Excise, to regulate the gauging of vessels at Glasgow; made calculations for the societies and funds for the widows and children of the Scotch clergy, and for the university professors, &c. Besides his publications before-mentioned, viz. the Geometria Organica and his Fluxions, and the piece which gained the Academical prize in 1724, he wrote many other valuable works, particularly in the Phil. Trans. from vol. 30 to vol. 42, and in the Edinburgh Medical Essays. In 1740 he shared the prize of the Paris Academy with Messrs. Dan. Bernoulli and Euler, for a paper on the motion of the tides, which he drew up in ten days; afterwards inserted in his treatise on Fluxions. Besides these, two posthumous works have been published by his friends, viz. the treatise on Algebra in 1 vol. 8vo. and the account of Sir I. Newton's discoveries, in 1 vol. 4to.

pursued; we shall therefore at present have recourse to this way of considering curves. And first we shall show how easy it is in this method to derive the more complex curves from the simple ones.

Sect. 1. Let L and l, fig. 1, pl. 9, be two points in the curve BlL very near each other; and lo an arc described with the centre S, and perpendicular to SL; then Ll will be as the moment of the curve, and Lo as the moment of the radius SL: and if there be given the ratio of Ll to Lo, or to lo in the distance SL, there will be given the equation of the curve to the centre S. Let LP, lp be tangents to the curve at the points L, l; on which draw the perpendiculars SP, Sp. In like manner, on all the tangents of the curve let perpendiculars be drawn from the given point S; and there will be constructed a curve passing through all the intersections of the tangents and perpendiculars. Of this curve the elementary triangle Pnp will be similar to the triangle Lol, which will therefore be given from the given curve BlL. For because of the equal angles Snp, PnL, and the right angles Spn, SPL, the triangles Spn, PnL will be equiangular, and therefore Pn : pn :: Ln : Sn :: Lo : lo; and because of the equal angles Pnp, SnL, Lol, the triangles Pnp, SnL, Lol will be similar. Since therefore the ratio of Ll to lo is the same as that of Pp to pn, and of SL to SP, it is manifest that, having given SL and the ratio of Ll to lo, there will be given the ratio of Pn to pn, and the line SP, and therefore the curve DPp. In the same manner, from DP there may be constructed a third curve, and from that a fourth; and so, proceeding in this manner, an infinite series of curves will be produced, which will all become known from the given one. Now if LN and ln be drawn perpendicular to the radii SL, Sl, meeting in n; and through all such points of concourse of the perpendiculars the curve EN be described; that will be a curve from which BL may be deduced, after the same manner as DP and BL were constructed. In like manner, from EN may be constructed another curve, and thence on this side likewise an infinite series of curves may be constructed.

2. But of all the curves thus produced, the simplest will be those in which Ll is to lo in the ratio of some power of the radius: so that if a be a given quantity, r the radius of the curve, and n any number, then if Ll : lo :: a^n : r^n, it will give their general equation. And all these will have an apsis when $r = a$, because then Ll = lo. To investigate the equation of the curve DP; since in BL it is, Ll : lo :: a^n : r^n :: r : SP =

$\frac{r^{n+1}}{a^n}$ :: $a^{\frac{n}{n+1}}\,\mathrm{SP}^{\frac{1}{n+1}}$: SP :: $a^{\frac{n}{n+1}}$: $\mathrm{SP}^{\frac{n}{n+1}}$ :: Pp : pn; therefore if $\dot{s}$ represent the moment of the curve, $\dot{y}$ the circular arc described by the radius from the centre S, and r the corresponding radius, whatever the curve be whose equation is sought, the equation of the curve BL will be $\dot{s} : \dot{y} :: a^n : r^n$; and the equation of the curve DP, $\dot{s} : \dot{y} :: a^{\frac{n}{n+1}} : r^{\frac{n}{n+1}}$; and the angle P$\mathrm{S}p$: angle L$\mathrm{S}l$ :: $\frac{pn}{\mathrm{SP}} : \frac{lo}{\mathrm{SL}}$ or :: $\frac{\mathrm{P}n}{\mathrm{SP}} : \frac{\mathrm{LO}}{\mathrm{SL}}$ or (calling SP, x, and SL, r) :: $\frac{\dot{x}}{x} : \frac{\dot{r}}{r}$, that is, (because $x = \frac{r^{n+1}}{a^n}$) :: $\frac{n+1}{r}\dot{r} : \frac{\dot{r}}{r}$, or :: $n + 1 : 1$. Hence (fig. 2) BSP : BSL :: $n + 1 : 1$; by which the curve BP may be more easily drawn without the tangents. If the angle BSP be taken to BSL in the ratio of $n + 1$ to 1, and LP be perpendicular to SP, the concourse of the perpendicular with SP, will be in the curve BP, before described by means of the tangents.

3. Having shown how, from one, an infinite series of curves may be deduced, I shall now proceed to show how the length of each may be known, from the length of that and one other being given. Since the angle S$\mathrm{P}p$ = S$\mathrm{L}l$, and L$\mathrm{S}l$: P$\mathrm{S}p$ :: $1 : n + 1$, it will be Ll : Pp :: SL : $\overline{n + 1}$ SP, or (because SL : SP :: Ll : lo) as Ll : $n + 1\ lo$, and therefore Pp = $\overline{n + 1\ \ lo}$; but lo = $ln - on = ln - \mathrm{LN} + \mathrm{N}n$, therefore P$p$ = $\overline{n + 1} \times \overline{ln - \mathrm{LN} + \mathrm{N}n}$. But ln − LN is the moment of LN perpendicular to SL, also Pp the moment of the curve BP, and Nn the moment of the curve BN: and since BP, BN, BL vanish together in B, they will be in the ratio of their moments; and therefore BP = $\overline{n + 1} \times$ BN + or − LN. Hence the curve BP is to the sum or difference of the last curve but one in the series, and of its tangent intercepted by the intermediate curve, as $n + 1$ to 1; or, putting m for the index of the equation of the curve BP (because $m = \frac{n}{n+1}$) as 1 to $1 - m$.

Hence, 1st, in the infinite series of curves described above, if there be given the lengths of two that are next each other, the lengths of all will be given; for the measure of any one depends always on the measure of the last but one in the series, and therefore one pair will suffice for measuring all. If one curve be commensurable or incommensurable to right lines, then half the series will be commensurable or incommensurable to right lines. Hence 2dly, though the curves BP and BN should be incommensurable to right lines, yet the difference of the curve BP from the $n + 1$ times of the curve BN, would be equal to an assignable right line. 3dly, If the curve pass through S, the line LN vanishing in S, it will be BPS = $\frac{\mathrm{BNS}}{1-m}$.

4. Of all the curves we have treated of, viz. of which $\dot{s} : \dot{y} :: a^n : r^n$, the circle is the chief, S being in the circumference, whose equation is $\dot{s} : \dot{y} :: a : r$, as appears from the similarity of the triangles Lol, BLS (fig. 3); therefore $n = 1$,

and therefore $m = \frac{n}{n+1} = \frac{1}{2}$, and the equation of the curve BP will be $\dot{s} : \dot{y} :: a^{\frac{1}{2}} : r^{\frac{1}{2}}$, which is the equation of the epicycloid described by the revolution of a circle on a base equal to itself, at the point where the describing point touches the base; which Pascal calls Roberval's snail, and which Delahire considers as a conchoid with a circular base, in the Memoirs of the Paris Academy for the year 1708. All the perpendiculars concur in the point B, and therefore $\text{BN} = 0$; whence $\text{BP} = \frac{\text{BN} + \text{NL}}{1 - m} = 2\text{BL}$. Hence the whole curve $\text{BPS} = 2\text{BS}$, or the length of the epicycloid is always double the chord of the corresponding circular arc. 2dly, From the epicycloid let the curve BΠS be described, in the same manner as the epicycloid was described from the circle: then in this case $n = \frac{1}{2}$, and $m = \frac{n}{n+1} = \frac{\frac{1}{2}}{\frac{1}{2}+1} = \frac{1}{3}$, therefore the equation of the curve BΠS will be $\dot{s} : \dot{y} :: a^{\frac{1}{3}} : r^{\frac{1}{3}}$. The length of the curve will be $\frac{\text{BL} + \text{LP}}{1 - m} = \frac{3}{2}.\, \overline{\text{BL} + \text{LP}} = \frac{3}{2}.\, \overline{\text{BL} + \text{LG}}$, and therefore BΠ is $\frac{3}{2}$ the sum of the circular arc and its right sine. Now if we take $\text{CD} = \text{BD}$, and with radius SD and centre S describe a circle meeting SP in H, and draw HK perpendicular to BS; then because $\text{DH} = \frac{3}{2}\,\text{BL}$, it will be $\text{B}\Pi = \text{DH} + \text{HK}$. Hence the arcs BΠ are neither commensurable to right lines nor to circular arcs, yet the difference of the arcs BΠ and DH is the right line HK. The line LG vanishes in the point S, and therefore $\text{B}\Pi\text{S} = \frac{3}{2}\,\text{BLS}$; hence the whole curve is $\frac{3}{2}$ of the semicircle. Yet no part of this assignable curve can be commensurable to the whole, nor is the entire curve divisible in any given ratio, so that the portions may have an assignable ratio to each other, or to the whole. If this curve could be divided geometrically in any given ratio, the quadrature of the circle would be completed. For instance, if it were $\text{B}\Pi : \text{B}\Pi\text{S} :: 1 : m$, and $\text{BL} : \text{BLS} :: 1 : n$, it would be $\text{B}\Pi =$
$\frac{\text{B}\Pi\text{S}}{m} = \frac{3\text{BLS}}{2m} = \frac{3n\text{BL}}{2m} = \frac{3}{2}.\, \overline{\text{BL} + \text{LG}}$; hence $\text{BL} = \frac{m\text{LG}}{n-m}$, and $\text{BLS} = \frac{nm}{n-m}\,\text{LG}$.
3dly, by the method already explained, from BΠS construct the curve BR; then because $n = \frac{1}{3}$, it will be $m = \frac{n}{n+1} = \frac{1}{4}$, and the equation of the curve BR will be $\dot{s} : \dot{y} :: a^{\frac{1}{4}} : r^{\frac{1}{4}}$. Hence the length of the curve will be $\frac{4}{3} \times \overline{2\text{BL} + \text{P}\Pi}$, and the whole length of the curve $\text{BRS} = \frac{8}{3}$ of the diameter SB. If the constructions of these curves were continued, there would arise such a series of equations as the following, which it is easy to continue at pleasure:

The equation of the circle, 1. $\dot{s} : \dot{y} :: a : r$.
of the epicycloid, 2. $\dot{s} : \dot{y} :: a^{\frac{1}{2}} : r^{\frac{1}{2}}$.
of the second, 3. $\dot{s} : \dot{y} :: a^{\frac{1}{3}} : r^{\frac{1}{3}}$.
of the third, 4. $\dot{s} : \dot{y} :: a^{\frac{1}{4}} : r^{\frac{1}{4}}$.
of any one, 5. $\dot{s} : \dot{y} :: a^{\frac{1}{n}} : r^{\frac{1}{n}}$. &c.

Here it may be observed in general, that all those which have the denominators of their indices even numbers, are capable of perfect rectifications; and since any one is to that before it, as 1 to $1 - m$, it will appear on consideration that the length of any curve will be $= \frac{1}{1-m} \times \frac{1-2m}{1-3m} \times \frac{1-4m}{1-5m} \times \frac{1-6m}{1-7m}$ &c. $\times$ SB, continuing the series till the fraction be reduced to nothing. But if the denominator of the fraction be an odd number, the curves will be incapable of perfect rectifications, and any one of these arcs will be incommensurable to any other, and to the wholes, and to right lines, and to circular arcs: yet they may all be expressed by circular arcs and right lines: and the total length of any curve will be to the semicircle, as $\frac{1}{1-m} \times \frac{1-2m}{1-3m} \times \frac{1-4m}{1-5m} \times$ &c. to unity. Lastly, if the areola described by a body revolving in any one of these be taken as constant, that is, if $r\dot{y} = 1$, the subtense of the angle of contact, to which (because of the time being given when the area is given) the centripetal force tending to S is always proportional, will be reciprocally as the power of the distance whose index is $2m + 3$. And this is no contemptible property of these curves, that in all of them the centripetal force tending to S, is reciprocally as some power of the distance: which is the most simple and useful law of centripetal forces, in searching into nature.

5. Of all the curves in which $\dot{s} : \dot{y} :: a^n : r^n$, the right line is next to be considered (which is indeed improperly called a curve) the point S being without that right line, fig. 4. In this line, because of the similar triangles Ppn, PBS, if BS $= a$ and SP $= r$, it will be $\dot{s} : \dot{y} :: r : a$. By the direct method nothing can be constructed from the right line, except the point B: but by the inverse method, from the concourse of the perpendiculars PL, pl, a curve may be constructed, whose index will be equal to $\frac{m}{1-m}$, if m be the index of the curve BP. For if the index of BL be n, then it will be $m = \frac{n}{n+1}$, and therefore $n = \frac{m}{1-m}$. Hence in this case, since $m = -1$, it will be $n = -\frac{1}{2}$, and the equation of the curve BL will be $\dot{s} : \dot{y} :: r^{\frac{1}{2}} : a^{\frac{1}{2}}$, which is an equation of the parabola referred to the focus. From this construct another, by making the angle LSN $=$ LSB, and erecting LN perpendicular to SL meeting SN in N. Now be-

cause $m = -\frac{1}{4}$, it will be $n = -\frac{1}{3}$, and the equation of the curve will be $\dot{s} : \dot{y} :: r^{\frac{1}{3}} : a^{\frac{1}{3}}$, and BP $= \frac{\text{BN} - \text{LN}}{1-m} = \frac{1}{2} \cdot \overline{\text{BN} - \text{LN}}$; therefore BN $=$ 2BP $+$ LN; so that this curve is rectifiable. If the series be continued, the equations will arise as before in this order:

$$\begin{array}{lllll}
\text{Equation of the right line,} & \dot{s} : \dot{y} & :: & r & : a . \\
\text{of the parabola,} & \dot{s} : \dot{y} & :: & r^{\frac{1}{2}} & : a^{\frac{1}{2}}. \\
\text{of the second,} & \dot{s} : \dot{y} & :: & r^{\frac{1}{3}} & : a^{\frac{1}{3}}. \\
\text{of the third,} & \dot{s} : \dot{y} & :: & r^{\frac{1}{4}} & : a^{\frac{1}{4}}. \\
\text{of any one,} & \dot{s} : \dot{y} & :: & r^{\frac{1}{n}} & : a^{\frac{1}{n}}.
\end{array}$$

In this series, the first are the right line and the parabola; whence it appears that half of this series, as well as the former, are commensurable to right lines: and the other half may be exhibited by right lines and parabolic arcs. In all these, the centripetal force at S, is reciprocally as that power of the distance, whose index is $3 - 2m$; and therefore is always between the duplicate and triplicate ratio of the distance reciprocally.

6. The equation of the equilateral hyperbola at the centre, is $\dot{s} : \dot{y} :: r^2 : a^2$; from which, by the direct method, may be deduced series of this kind:

$$\begin{array}{llll}
1. & \dot{s} : \dot{y} :: r^2 & : a^2 & . \\
2. & \dot{s} : \dot{y} :: a^2 & : r^2 & . \\
3. & \dot{s} : \dot{y} :: a^{\frac{2}{3}} & : r^{\frac{2}{3}} & . \\
4. & \dot{s} : \dot{y} :: a^{\frac{2}{5}} & : r^{\frac{2}{5}} & . \\
5. & \dot{s} : \dot{y} :: a^{\frac{2}{2n-1}} & : r^{\frac{2}{2n-1}} & .
\end{array}$$

Of these curves, those which have the denominators of their indices in this progression, $-1, 3, 7, 11$, &c. may be exhibited by right lines and hyperbolic arcs; and the rest by right lines and arcs of the curve, whose equation to the axis SB is $(xx + yy)^2 = a^2x^2 - a^2y^2$ (the absciss being x, and the ordinate y); and which is constructed (fig. 3) by bisecting the angle BSL, and taking SN a mean proportional between SB and SL.

The curves which may be constructed from the hyperbola by the inverse method proceed as in this series:

$$\begin{array}{lll}
\text{Of the hyperbola } 1. & \dot{s} : \dot{y} :: r^2 : a^2 . \\
2. & \dot{s} : \dot{y} :: r^{\frac{2}{3}} : a^{\frac{2}{3}}. \\
3. & \dot{s} : \dot{y} :: r^{\frac{2}{5}} : a^{\frac{2}{5}}. \text{ \&c.}
\end{array}$$

Where the curves which have the denominators of their indices in the progression 1, 5, 9, 13, &c. may be expressed in right lines and hyperbolic arcs; and the rest in right lines and the arcs of the curve above explained.

If other curves were desired which should exhibit other series, it might easily be done by means either of a circle or a right line; for by the one of them all the curves may be constructed, in which $\dot{s} : \dot{y} :: a^n : r^n$, by taking BSR to BSL as 1 to n, and SN × SR $= a^{\frac{n-1}{n}} \times$ SL$^{\frac{1}{m}}$, if the problem is to be solved by means of the circle, fig. 5; for the equation of the curve drawn through all the points N, will be $\dot{s} : \dot{y} :: a^n : r^n$. In like manner, by means of the right line, may be constructed the curves whose equation is $\dot{s} : \dot{y} :: r^n : a^n$.

We have exhibited two infinite series of curves commensurable to right lines: we have demonstrated another commensurable to circular arcs, another to parabolic, another to hyperbolic arcs with right lines. But these seem reducible to the measure of right lines by infinite art only, as they are expressed in right lines only by an infinite equation.

Remarks on a Fragment of an old Roman Inscription lately found in the North of England. By Dr. James Jurin, M. D. F. R. S. N° 356, p. 813.

Dr. Jurin, having resided for some time at Newcastle upon Tyne, had the curiosity to travel the country between that town and Carlisle, to observe what might occur worth notice in the remains of the ruins of the famous Picts-wall, built by the Romans to secure themselves against the incursions of the natives of that part of Britain which they did not care to conquer. In this perambulation, besides many other valuable observations, Dr. Jurin saw and transcribed no less than 20 Roman inscriptions, many of them wholly new; among them the following, which, though broken, and in great part illegible, suffices to fix the name of one of the ancient nations of Britain, which has hitherto been greatly miscalled.

CIVITATE CAT
VVILLAVA′
ORVM·[·*O*[*S*
C*DIO*

It is thus, as annexed, and is to be seen on the wall, about 2 miles west of Lenecross Abbey, near the confines of the two northernmost counties.

Here it is observable, that the last A of the second line has a mark that follows it, not unlike the last stroke of an N; and if instead of A′ we put N, we shall read it CIVITATE CATVVILLAVNORVM, which we cannot doubt to have been the true name of that people which Dion. Cassius, lib. 60, calls Κατȣελλανοὶ, and Ptolomy, in his geography, lib. 2, cap. 3, more falsly, Κατυευχλανοὶ; the first λ, by producing the transverse stroke, having been mistaken for χ. This nation appears by Dion to have been more potent than their neighbours the Dobuni, whom he calls Boduni, and had, according to Ptolomy, Verolamium for their capital, which was probably the Cassivellauni oppidum of Cæsar. So that it should seem Cassivellaunus, king of these Catuvillauni, when Cæsar invaded Britain, either gave his name to his people, or took theirs. But he was doubtless the most potent prince at that time in Britain, since by common consent of the rest, he was made general of their united forces, in defence of their country's cause against the Romans.

Observations on a Comet seen at Berlin. By M. Kirch. N° 357, p. 820. *Translated from the Latin.*

M. Kirch, observing the motions of the heavenly bodies on the 18th of January 1718, N.S. at half an hour after 7 o'clock in the evening, happened to spy a comet towards the north. It was next the right of Bayer's γ and β in the ursa minor, and appeared to the naked eye much brighter than β, though a remarkable star of the second magnitude, and though much paler, yet of a larger diameter, and pretty bright, especially about its centre. When seen through the telescope, it appeared like a bright round nubecula; but no signs of a tail could be observed, nor could the nucleus be distinguished. It proceeded with a very swift motion from 7 to 9 o'clock, having gone over $4\frac{1}{2}°$, as is collected from the observations.

On the 19th and 20th of January the heavens were overcast; but on the 21st the comet was gone a good way from its late place, and was found in Cassiopeia, where it formed a triangle with the stars ϵ and δ, viz. at $5^h\ 45^m$, in 17° 34′ of Taurus, and 49° 54′ N. lat. afterwards at $9^h\ 15^m$ it was seen in 16° 38′ of Taurus, and 49° 2′ N. lat. Then it decreased much, and remitted of its velocity, and besides, appearing paler than before, and did not seem to the naked eye to exceed a star of the 4th magnitude, nor did it proceed in its orbit above a degree and a half in $4\frac{1}{2}$ hours: its diameter by the telescope was 7′.

Jan. 23, at 4 o'clock in the morning, the comet formed an isosceles triangle with δ and φ of Cassiopeia; being distant from each 2° $41\frac{1}{2}$′. This morning it scarcely dispatched half a degree in 2 hours time: at 10 o'clock at night it was seen in a straight line with δ of Cassiopeia and φ of Perseus, and was distant from the former 3° 38′, and from the latter 3° 9′. Its diameter was 5′, and to the naked eye it appeared like a star of the fifth magnitude.

On the 24th of January, at 6 o'clock in the morning, it had not yet applied to φ of Perseus, but formed an isosceles triangle with υ and g of that constellation, and was distant from each not quite $3\frac{1}{2}°$.

An Eclipse of the Sun at Norimberg, by M. Wurtzelbaur; and at Berlin, by M. Kirch. N° 357, p. 822. *Translated from the Latin.*

In the Nov. Literar. Berolin. are two observations of a small eclipse of the sun on Feb. 19, 1718, O. S. the one made at Norimberg by M. Wurtzelbaur, and the other at Berlin by M. Kirch.

At Norimberg the sun rose somewhat eclipsed in his superior limb, which at length became full 3 digits; and the eclipse ended at $8^h\ 8^m\ 48^s$, about 60° from the vertex to the left.

At Berlin the sun began to be eclipsed, immediately on his rising, viz. at $6^h\ 49^m$ or $49\frac{1}{2}^m$. About the middle of the eclipse, viz. at $7^h\ 35^m$, the remaining clear parts in the sun were 24′ 40″; consequently, 2 dig. 50′ were eclipsed. The end was at $8^h\ 28^m\ 20^s$.

Observations on a Roman Inscription found near Lancaster. By Roger Gale, Esq. F. R. S. N° 357, p. 823.

Dr. Hunter, who communicated this inscription in N° 354, having only given us his conjectures as to the first fortifying the place where it was found, and the time of its repair after it had been destroyed, but said nothing relating to the explanation of the inscription itself, it will not be amiss to offer some thoughts upon it. I shall not in the least dispute the time of its foundation, as fixed by the Doctor, but begin with the place where it was discovered, namely, Lanchester or Lancaster, in the county of Durham, which I am, with him, fully persuaded was the Longovicus, where the Notitia Imperii places the Numerus Longovicariorum.

This place is seated upon a great military way, about 12 miles from Binchester, and 7 from Ebchester, the one the Vinovia, and the other the Vindomora of Antoninus, as the correspondence of the numbers may evince; Binchester being 19 Roman miles from Ebchester, as that is 9 from Corbridge, the exact numbers the Itinerary gives us between Vinovia, Vindomora, and Corstopitum. It is very surprising that the Itinerary, which must go upon the great road directly through this town of Longovicus between Vindomora and Vinovia, takes not the least notice of it, but measures the way at the whole length and number of miles from the first to the latter of those stations. After this place was repaired by Gordian, it subsisted even till the ruin of the Roman empire in Britain, as is evident by the mention of it in the Notitia Imperii; so that had this journey, which carries us from Vindomora to Vinovia, been composed after the reign of Gordian, it would be very hard to account for the omission of this remarkable station and town, as it appears to have been from this and many other inscriptions found there.

I shall take this opportunity of rectifying a mistake in the Essay towards the Recovery of the Roman Highways through Britain, printed in the 6th volume of Hearne's Itinerary of Leland, which having brought the Ermingstreet, not the Watlingstreet, as Dr. Hunter and the country call it, a little beyond Cattarick in Yorkshire, divides it there into two branches, tracing one of them to Tinmouth, and the other to Carlisle, but omits the main stem, that runs almost directly northward to Piercebridge, so to Denton, Houghton, Binchester, Lanchester, Ebchester, Corbridge, and through the heart of Northumberland into Scotland, about a mile and a half to the west of Berwick. It is in several places very entire and fair, especially between Corbridge and Binchester, its ridge there being for the most part 2 yards in height above the level of the soil, no less than 8 yards broad, and all paved with stones, which are as even as if new laid; as I am informed by the ingenious Mr. Warburton, who has often viewed it, and to whom we are obliged for the most accurate and useful map of the county of Northumberland that was ever yet published.

Having fixed the seat of this Longovicus where the inscription was found, let us consider next what sort of place it was; and on due inquiry it will appear to have been one of the most ancient and eminent stations the Romans were possessed of in those parts. As to its antiquity, Dr. Hunter has made it probable that we ought to look for it as high as Julius Agricola's commanding under Domitian in this island; as to its eminency, the inscription that came last from him to the Society, as well as several others found there, are undeniable evidence of its being a place of great importance; but nothing can put that more out of dispute than the first, which was some years ago sent by the same person, and mentioned in N° 266.

The stone on which the first is cut has been broke in two, by which some of the letters are defaced; however, it may be very well read as follows: the letters PRE in the fourth line I take to be a mistake of the workman, having seen several copies, where they are so transcribed; that they should be PER is evident from the fifth line of the second inscription.

I. Imperator Cæsar Marcus Antonius Gordianus
Pius Felix Augustus Balneum cum
Basilica à solo instruxit
Per Cneium Lucilianum Legatum Augustalem
Proprætorem Curante Marco Aurelio
Quirino Præfecto cohortis primæ Longovicariorum, or rather, Legionis Gordianæ.

The second can be read only after the following manner: fig. 8, pl. 8.

II. Imperator Cæsar Marcus Antonius
Gordianus Pius Felix Augustus
Principia et Armamentaria
Conlapsa restituit

Per Mæcilium Fuscum Legatum
Augustalem Proprætorem curante Marco Aurelio
Quirino Præfecto Cohortis primæ Legionis Gordianæ.

From these two inscriptions compared together, it will be apparent, that they were not only erected under the same emperor, but by the care of the very same person Aurelius Quirinus, though not in the same year. The emperor can be no other than Gordianus the youngest, or third of that name: the two former having been slain so very soon after they had assumed the purple, that it is improbable they should have given any orders or commands for the erecting of new, and repairing of ancient buildings, in so remote a province as Britain was from Africa, where they were murdered after a short joint reign of scarce 7 weeks.

The first inscription informs us, that the Emperor Gordian built the balneum and basilica, à solo, from the ground; whereas, by the second he appears to have been only the repairer of the principia and armamentaria. As this eminent building was erected by the emperor's command, it is an undeniable argument of the splendour of this town, as are the great heaps of rubbish, and ruins, where this inscription was found, of its size and extent.

The second inscription equally puts the being of the castrum stativum out of dispute, when it acquaints us with the rebuilding of the armamentaria and principia there, that is the arsenals and quarters either of the legionary soldiers, called the principes, or the place where the eagles and other military ensigns were kept. It is probable they did not belong to one particular legion, but to several, as they had occasion to be employed here; though the legio sexta victrix seems to have the best title to them, as being constantly quartered in the north; whereas, the legio secunda, and vicesima were generally garrisoned, the first at Caerleon in Wales, and Richburrow in Kent, and the other at and about Chester; so that the monuments they have left in the north were erected by them, when the wars, and other works, as particularly the walls carried cross the island, called them thither; which being finished, they returned home to their more southern quarters, and continued in them till commanded abroad on new services. I will not pretend to determine when these armamentaria and principia first fell to ruin; perhaps it might be when Adrian, Lollius Urbicus, and Severus had carried their conquests farther into the enemy's country, and having built those famous walls, the relics of which we still see in the shire of Stirling in Scotland, and in Northumberland and Cumberland in England, that this camp might be thought useless, the Roman forces being drawn nearer to, and quartered on the frontiers; and so this fortress abandoned and suffered to fall into decay, as the word conlapsa implies: and not that it was destroyed by any fire, war, or other enemy than age and neglect.

Though the word conlapsa is written here with an N, there can be no doubt but the pronunciation of it was as we usually find it spelt, collapsa; a certain argument of the letter N's being silent in the middle of a word, before two consonants, especially NS, and NT, when the T was pronounced like an S. To omit what Quintilian says to this purpose, it is confirmed by the absence of that letter in numberless inscriptions in Gruter, Reinesius, &c. and no wonder, since the workmen in those days, as well as ours, usually wrote as they spoke their words.

It will be as difficult to assign a reason for repairing the camp at Longovicus, as it was for its being deserted; unless the propraetors might judge it adviseable about the time of Gordian III. to fix their residence there, and consequently refortify the old camp for their state and security. And that it was not refortified upon any sudden emergency, but for time and duration, is evident both from the strong stone-works that encompassed it, and a body of forces lying here, even at the expiration of the Roman empire and authority in this island, which from its continuance in the same station, had got the name of the Longovicarii.

We are indebted to these two monuments, not only for the account they have preserved of the Roman arms and magnificence at Longovicus, but for the indisputable records of the names of two legates and propraetors of Britain, which would otherwise have been buried in oblivion, viz. Cneius Lucilianus and Mæcilius Fuscus: for from Virius Lupus (who was propraetor and legate here about the year 208, under Severus, and just before that emperor's coming into this island repaired a bath burned down at Lavatræ, or Bowes, in Yorkshire) we have no where extant the name of one of those officers, till we come to Nonnius Philippus, whom I take to have succeeded the last of these; the stone which was found at Old Carlisle in Cumberland, and has preserved his memory, setting forth that he was legate and propraetor when Atticus and Prætextatus were consuls, which was A. D. 242, the very year that our Gordian went on his Persian expedition, from which he never returned. And as that emperor left Nonnius Philippus in that post, when he marched into the east, where he was murdered about two years after, it is highly probable that he was the last propraetor of his appointing, and consequently, that Mæcilius Fuscus was his predecessor, and the repairs began at Longovicus before the year 243.

An Account of a fiery Meteor seen, in Jamaica, to strike into the Earth; with Remarks on the Weather, Earthquakes, &c. of that Island. By Mr. Henry Barham, F. R. S. N° 357, p. 837.

About the year 1700, as I was riding one morning about 3 miles north-west from St. Jago de la Vega, I saw a ball of fire, appearing to me of the size of a bomb-shell, swiftly falling down with a great blaze. When I arrived where it fell, I found the people wondering at the ground being broken in by a ball of fire, which, they said fell down there. I observed there were many holes in the ground, one in the middle of the size of a man's head, and 5 or 6 smaller holes round about it, of the size of a man's fist, and so deep, especially the largest, as not to be fathomed by what long sticks they had at hand. It was observed, that the green grass was perfectly burnt near the holes, and a strong smell of sulphur remained thereabouts for a good while after.*

We had a very rainy night before, with much lightning and thunder, which is frequent in Jamaica, often killing cattle in the fields. These claps are much louder and stronger than any in Europe, and our showers of rain are also more violent. We have lightning all the year round, but our great rains are in the months of May, August, and October.

Our island is full of mines, and I question not, very rich. It is very subject to earthquakes, several happening every year, especially after great rains, which fill up all the great cracks in the surface of the earth: for in a very dry time, they are so very large, deep, and gaping so open and wide, that it is dangerous to ride over some parts of the Savannas, for fear a horse should get his legs into them. Our earthquakes make a noise or rumbling in the earth, before we feel the shake; and seem to run swiftly to the westward.

An Attempt to prove the Antiquity of the Venereal Disease, long before the Discovery of the West-Indies; in a Letter from Mr. William Beckett, Surgeon, to Dr. James Douglass, M. D. F. R. S. Dated Lond. Feb. 4th, 1717-18. N° 357, p. 839.

The undertaking I am at present engaged in, has unavoidably obliged me to consult, among others, many old physical and surgical books, written by my own countrymen: from these I derive the opinion, that the venereal disease was

* Here is another instance of the fall of a flaming meteoric body, striking into the earth. See p. 100 of this volume. In this instance as in several there mentioned, the body was ignited; flew with great rapidity; struck the ground in a marshy situation, having burst into several pieces just before it entered the earth, as appears by the pieces striking near each other.

was known among us much earlier than the æra which has been generally assigned for its rise by modern authors; for it is believed not to have been known, at least in Europe, till about the year 1494. Yet it appears from the following papers, that it was frequent among us some centuries before that date. I shall in these, and some following papers, laying aside all foreign aids, trace out the symptoms of the disease, as they naturally arise, from the first infection to the last destructive period, and show that, by searching into our own antiquities, we may be furnished with instances of the frequency of the distemper among us, in all its respective stages, long before our modern authors suppose it appeared in Europe.

I shall begin with the first degree of this disease, and prove from authentic evidences, that it was anciently called the brenning or burning; and that this word has been successively continued for many centuries, to signify the same disease we now call a clap; and that it was not discontinued till that appellation first began to have its rise. For this purpose I shall examine those records that relate to the stews, which were by authority allowed to be kept on the Bankside in Southwark, under the jurisdiction of the Bishop of Winchester, and which were suppressed by an act of the 37th of Henry the 8th. For it is impossible but that, if there were any such distemper in being at that time, it must be pretty common among those lewd women who had a licence for entertaining their paramours, notwithstanding any rules or orders which might be established to prevent its increase: but if we shall find that there were orders established to prevent the spreading of such a disease, that persons might be secure from any contagious malady after their entertainment at those houses (which were anciently 18 in number, but in the reign of Hen. the 7th reduced to 12) we may be assured that it was the frequency of the disease that occasioned the necessity of making such rules and orders. For the same powers that granted a liberty for keeping open such lewd houses, must find it their interest to secure, as much as possible, all persons from receiving any injury there; lest the frequency of such misfortunes should deter others from frequenting them, and so the original design of their institution cease; from the entire sinking of the revenues. Now I find that, as early as the year 1162, divers constitutions relating to the Lordship of Winchester, were to be kept for ever, according to the old customs from time immemorial. Among which these were some, viz. No stew-holder to take more for a woman's chamber in the week than 14d; not to keep open his doors on holidays; no single woman to be kept against her will; no single woman to take money to lie with any man, unless she lie with him all night till the morning; no stew-holder to keep any woman that hath the perilous infirmity of burning. These and many more orders were to be strictly observed, or the offenders to be severely punished. Now we are assured there is no other disease that can be communicated by carnal conversation with women, except that which is venereal, as that only is contagious; and it is evident that the burning was certainly so: for, had it been only some simple ulceration, heat, or inflammation, there would have been no contagion; and that affecting only the woman, could not be communicated by any venereal congress, and so not infer a necessity of her being comprehended under the restraining article. These orders likewise prove the disease was much more ancient than the date above-mentioned; because they were only a renewal or such as had been before established from time immemorial.

To confirm this further, I find that in the custody of the Bishop of Winchester, whose palace was situated on the Bank-side, near the stews, was a book written upon vellum, the title of which runs thus: *Here begynne the Ordinances, Rules, and Custumes, as well for the Salvation of Mannes Lif, as for to aschewe many myschiefs and Inconvenients that dayley be lik there for to fall owte, to be rightfully kept, and due execution of them to be don unto any Personne wythin the same.* One of the articles begins thus: de his qui custodiunt mulieres habentes nephandam infirmitatem. It goes on, item, *That no stew-holder keep noo woman wythin his hous that hath any sycknesse of brenning, but that she be putte out upon the peyne of makeit a fyne unto the lord of a hundred shylyngs.* This is taken from the original manuscript which was preserved in the Bishop's Court, supposed to be written about the year 1430. From these orders we may observe the frequency of the distemper at that time; which, with other inconveniencies, was *dayley lik there for to fall owte:* and the greatness of the penalty, as the value of money then was, that is laid on it, proves it was no trifling or insignificant thing.

But the bare proof of there having been anciently such a disease as was called the burning may be thought to be insufficient, unless we were perfectly assured what it was, and how it was then described. I shall therefore do it from an unquestionable authority, which is that of John Arden, Esq. who was one of the surgeons to King Richard the 2d, and likewise to King Henry the 4th. In a curious manuscript of his upon vellum, he defines it to be a certain inward heat and excoriation of the urethra; which description gives us a perfect idea of what we now call a clap; for frequent dissections of those that laboured under that disease, have made it evident, that their urethra is excoriated by the virulency of the matter they receive from the infected woman; and this excoriation or ulceration is not confined to the ostiola or mouths of the glandulæ muscosæ, as has been lately thought, but may equally attack any part of the urethra not beyond the reach of the impelled malignant matter. The heat before described, which these persons are sensible of, as well now as formerly, is a consequence of the excoriated urethra; for the salts contained in the urine must necessarily irritate the nervous fibrillæ, and excite a heat in those parts of the urethra which are divested of its natural membrane; which heat will always be observed to be more or less, as the salts are diluted with a greater or less quantity of urine; a thing I have often observed in persons that have laboured under this infirmity in hot weather, when the perspirable matter being thrown off in greater quantities, the salts bear a greater proportion to the quantity of urine, and thereby make its discharge at that time so much the more painful and troublesome.

Thus we see this very early and plain description of this disease among us to be entirely conformable to the latest and most exact anatomical discoveries. Here is no tone of the testicles depraved, according to Trajanus Petronius; no exulceration of the parastatæ, according to Rondeletius; no ulceration of the seminal vessels, according to Platerus; no seat of the disease in the vesiculæ seminales or prostatæ, according to Bartholin; nor in those parts and the testicles at the same time, according to our countryman Wharton and others, who have falsely fixed the seat of this disease, and whose notions, in this respect, are now justly exploded; but a single and true description of it, and its situation, about 150 years before any of those gentlemen obliged the world with their labours.

As to the ancient method used to cure the disease, we are not to expect that the measures our predecessors employed should be calculated to remove any malignity in the mass of blood, or other juices, according to the practice in venereal cases at this time; because they looked upon the disease to be entirely local, and the whole of the cure to depend upon the removal of the symptoms: hence they recommended such remedies as were accommodated to the taking off the inward heat of the part, and to cure the excoriations or ulcerations of the urethra. The process for the accomplishing of this, I shall set down from the before-mentioned John Arden, who wrote about the year 1380. His words are as follow, contra incendium. Item contra incendium virgæ virilis interius ex calore et excoriatione, fiat talis syringa (i. e. injectio) lenitiva. Accipe lac mulieris masculum nutrientis, et parum zucarium, oleum violæ et ptisanæ, quibus commixtis per syringam infundatur, et si prædictis admiscueris lac amygdalarum melior erit medicina. There is no doubt but this remedy, used to patients at this time, would infallibly take off the inward heat of the part, and cure the excoriations or ulcerations of the urethra, by which means what issued from thence would be entirely stopped; and this was all they expected from their medicines, as they were entirely unacquainted with the nature of the distemper, and did not in the least imagine, but if the symptoms that first attacked the part were removed, the patient was entirely cured.

I shall now, as a further confirmation of what I have advanced, proceed to prove, that by this brenning or burning, is meant the venereal disease, by demonstrating that succeeding historians, physical and surgical writers, and others, have all along with us in England used the very same word to signify the venereal malady. In an old manuscript I have by me, written about the year 1390, is a receipt for *brenning of the pyntyl, yat men clepe ye apegalle*; *galle* being an old English word for a running sore. They who know the etymology of the word apron, cannot be ignorant of this. And in another manuscript, written about 50 years after, is a receipt for burning in that part by a woman. Simon Fish, a zealous promoter of the Reformation in the reign of Henry the 8th, in his supplication of beggars, presented to the king in 1530, says as follows, these be they (speaking of the Romish priests) that corrupt the whole generation of mankind in your realm, that catch the pockes of one woman and bear them to another; that be burnt with one woman and bare it to another; that catch the lepry of one woman and bare it unto another. But to make this matter still more evident, Andrew Boord, a Doctor in Physic, and Romish priest, in the reign of Henry the 8th, in a book, entitled The Breviary of Health, printed in 1546, speaks very particularly of this sort of burning. One of his chapters begins thus, the 19th chapiter doth shew of burning of an harlotte; where his notion of communicating the burning is very particular. The same author adds, that if a man be burnt with an harlot, and do meddle with another woman within a day, he shall burn the woman that he shall meddle withal; and as an immediate remedy against the burning, he recommends the washing the pudenda two or three times with white wine, or else with sack and water; but if the matter have continued long, to go to an expert surgeon to have help. In his 82d chapter, he speaks of two sorts of burning, the one by fire, and the other by a woman through carnal copulation, and refers the person that is burnt of a harlot to another chapter of his for advice, what to do, *yf he get a dorser or two,* so called from its protuberancy or bunching out: for I find about that time the word bubo was mostly made use of, to signify that sort of swelling which usually happens in pestilential diseases.

From hence it appears, that the burning, by its consequents, was venereal, since every day's experience makes it evident, that the ill treatment of the first symptoms of the disease, either by astringent medicines, or the removing them by cooling and healing the excoriated parts, will generally be attended with such swellings in the groin, which we rarely observe to happen from any other cause.

I shall give a few more instances of this disease being called the burning, and conclude. In a manuscript I have of the vocation of John Bale to the bishopric of Ossory in Ireland, written by himself, he speaks of Dr. Hugh Weston (who was Dean of Windsor in 1556, but deprived by Cardinal Pole for adultery) as follows, "At this day is lecherous Weston, who is more "practised in the art of brech burning than all the whores of the stews." And again, speaking of the same person, he says, "He not long ago brent a beggar "in St. Botolph's parish." The same author says of him elsewhere, "He had "been sore bitten with a Winchester goose, and was not yet healed thereof;" which was a common phrase for the pox at that time, because the stews were under the jurisdiction of the Bishop of Winchester. Mich. Wood, in his Epistle before Steph. Gardiner's Oration de vera Obedientia, printed at Rhoan, 1553, gives another evidence of the burning. And William Bullein, a physician in the reign of Queen Elizabeth, in a book he published, called the Bulwark of Defence, &c. printed in 1562, bringing in sickness demanding of health what he should do with a disease called the French pox, health answers, "He would not that any should fishe for this disease; or to be bold when he is "bitten to thynke thereby to be helped, but rather to eschewe the cause of "thys infirmity, and filthy rotten burning of harlots."

I believe I have now sufficiently proved what I proposed, that the first degree of the venereal disease was very anciently known among us, under the title of burning. I shall reserve my collections, which show that the disease, when it came to be confirmed, was no novelty here in those early times, for a further opportunity.

Astronomical Observations for the Years 1717 *and* 1718. N° 357, p. 847.

These astronomical observations were chiefly made by the Rev. Mr. Pound, at Wansted. Some few by Mr. Derham, Mr. James Bradley, &c. They relate chiefly to appulses or occultations of the fixed stars, by the planets Saturn, Jupiter, Mars, &c. which are now of no use. An eclipse of the moon was also observed, viz. on Aug. 29, 1718. The moon was then totally and almost centrally eclipsed. She rose when the eclipse began; and Mr. Pound observed that at $7^h\ 2^m\ 41^s$ the moon totally immerged in the shadow. At $8^h\ 48^m\ 18^s$ she began to emerge out of the shadow. The eclipse ended $9^h\ 52^m$. The moon's diameter measured $29'\ 51''$.

To find the Curve which a Descending Body describes in the shortest Time; being urged by a Centripetal Force tending to a Given Point, and increasing or decreasing according to any Power of the Distance from the Centre: having given the Point of the Curve and the Altitude at the Beginning of the Fall. By John Machin, Gresham Astron. Profes. and R. S. Secr.* N° 358, p. 860. *Translated from the Latin.*

Let the centre of force be C, fig. 6, pl. 9; with which centre and the distance CB, equal to the altitude from which the body falls, let a circle BEG be described; and let BCG be a right angle. Let A be the lowest point of the curve, where it meets the axis CB at the given distance CA. It is required to find the point Q, where the curve of quickest descent EQA meets the circle QF, at another given distance CF.—This problem has two cases; one depending on the hyperbola and circle, the other on the ellipse and circle.

Case 1.—When the centripetal force is reciprocally as the distance from the centre. Let KLM be any rectangular hyperbola, having its centre C and asymptote CB, and meeting the perpendiculars BK, FL, AM, in the points K, L, M. Make CD to CG as $\sqrt{\text{AFLM}}$ to $\sqrt{\text{ABKM}}$, and draw DH perpendicular to CG. Also take the sector RCB to the area HDCB as the given hyperbolic area ABKM to the given rectangle CA $\times$ AM. Then the right line RC will meet the circle FQ in the point Q, which will be in the curve of swiftest descent EQA.

And the point B will be found, from whence the fall of the body should begin, by taking the sector BCE to the quadrantal area BCG, as the hyperbolic area ABKM is to the rectangle CA $\times$ AM.

Corol.—Hence, if the right line RC, revolved about the centre C, make the sectors RCB proportional to the areas HDCB, in which the squares of the bases CD are taken in arithmetical progression: then the right lines CR will intersect the curve EQA at the distances CQ from the centre, which will decrease in geometrical progression.

Case 2.—When the centripetal force is reciprocally as any other power of the distance from the centre. Let $n + 1$ be the index of that power, n being any number, either integer or fraction, affirmative or negative; and let $\text{H} = \text{CB}$, fig. 7, be the greatest altitude of the required curve EQA, $h = \text{CA}$ the least altitude of the same, and $\text{A} = \text{CF}$ any other intermediate altitude.

In the right line CG take CD to CB as $\sqrt{h^n}$ to $\sqrt{\text{H}^n}$, and also CH to CD as $\sqrt{\text{A}^n - h^n}$ to $\sqrt{\text{H}^n - h^n}$. Then with the centre C, and semiaxes CD, CB, describe the ellipse BLD, meeting the ordinate HL in L; and draw the right line LK touching the ellipse in L, and meeting the less axis CD produced in K: then

* Mr. Machin, who was some time professor of astronomy, and secretary of the Royal Society, was elected to the professorship of Gresham College, the 16th of May, 1713, on the resignation of Dr. Torriano, and died June 9, 1751. His communications to the Society, besides the present paper,

draw NM parallel to the tangent KL, touching the circle BEMG in M, and meeting CD in N. Lastly, take the sector RCB to the area NMBLKN, as the number 2 to the number n. Then the right line RC will cut the circle FQ in the point Q, which will be in the curve EQA of quickest descent.

And if the sector BCE be taken to the area BDG, in the said ratio of 2 to n, that is, the points L, D, and M, G coinciding, because of $A^n = H^n$, the point E will be that from whence the body should begin to fall, to arrive at A in the shortest time, and which in its descent describes the curve EQA, which the right line CE touches in E, and which CB cuts at right angles in A.

The demonstrations of these constructions, which are derived from Newton's Quadratures, and from his Principles of Natural Philosophy, p. 39, &c. shall be given on some other occasion. But it is a problem of another kind, to describe curves through which bodies would move from the highest point E, or the beginning of the fall, with the swiftest descent to lower given points Q, when urged by any centripetal force; the solution of which problem is in my power. For the present it may suffice to have given a general idea of such curves, and to show their relation to the quadratures of the circle and hyperbola, without which it will not be very easy to construct them geometrically.

De Potentiâ Cordis. Dissertatio Autore Jac. Jurin, M.D. R.S.S. N° 358, p. 863.

[An attempt to estimate the muscular power of the heart upon mathematical principles. Such calculations display great ingenuity, but they are necessarily involved in much uncertainty, being founded on data taken from inanimate matter, the laws of which are not applicable to organs endowed with the principle of vitality. In this paper Dr. Jurin rates the propulsive force of the heart much higher than Dr. Keill had done in his Tentamina Medico-Physica, and represents Dr. Keill to have misunderstood and misapplied the Newtonian law or corollary on which his (Dr. K.'s) calculations are founded. To this Dr. Keill replied in a Latin paper inserted in this same vol. of the Phil. Trans. and this reply was followed by a rejoinder from Dr. Jurin; which two controversial papers, together with the present, it is deemed unnecessary to translate for the reason above stated. The subject, not being reducible to mathematical demonstration.]

An Account of a Contagious Distemper which raged among the Cows near London, 1714. *By Thomas Bates, Esq. F.R.S.* N° 538, p. 872.

About the middle of July, 1714, the distemper appeared at Islington, and

were one in vol. 37, on a Distempered Skin; and another in vol. 40, being a solution of Kepler's problem. By an approximation of Dr. Halley's, Mr. Machin computed the circumference of the circle to 100 places of figures. And an ingenious approximation of his own is printed in Mr. Jones's Synopsis Palmariorum Mathexios, the investigation of which is given in Dr. Hutton's large book on Mensuration, p. 89, 90, ed. 3d.

the lords justices having notice of it, ordered that I should examine into the truth of the report of its being contagious; and the Lord Chancellor having granted such authority as was proper to make the discovery, four justices of the peace for the county of Middlesex were also appointed to make the necessary examinations.

Pursuant to those orders we went to Islington, where Mr. Ratcliff had lost 120 cows out of 200; Mr. Rufford 62 out of 72; and Mr. Pullen 38 out of 87. Mr. Ratcliff gave the following account of this distemper, viz. That the cows first refused their food; the next day they had huskish coughs, and voided excrement like clay; their heads swelled, and sometimes their bodies. In a day or two more there was a great discharge of a mucous matter by the nose, and their breaths smelled offensively. Lastly, they had a severe purging, sometimes bloody, which terminated in death. That some died in 3 days, and others in 5 or 6, but the bulls lived 8 or 10 days. That during their whole illness, they refused all manner of food, and were very hot. Several of the cow-doctors agreed that it was a murrain, or rather a plague; and all the methods they had tried for a cure, had proved unsuccessful.

Being ordered by the lords justices to deliver in writing what would be proper to be done, I drew up, and gave them the following proposals:

1. That all such cows as are now in the possession of Mr. Ratcliff, Rufford, and Pullen, be bought, killed, and burnt: or at least that the sick be burnt; and the sound kept and secured on the grounds where they now are, that such of them as sicken or die of this distemper may be burnt.

2. That the houses, in which those sick cows have stood, be washed very clean, and then smoked by burning pitch, tar, and wormwood, and be kept 3 months at least before any other cows are put into them.

3. That the fields where those sick cows have grazed, be kept 2 months before any other cows are suffered to stand or graze there.

4. That the persons looking after such as are ill, should have no communication with those that are well.

5. That the same methods be observed if any other of the cow-keepers should get this distemper among them; and that they be all summoned and told, that as soon as they perceive any of their cows to refuse their meat, or have any other symptoms of this distemper, that they immediately separate them from their others, and give notice to such persons as the lords justices shall appoint, that they may be burnt; and the places where they have stood or grazed to be ordered as above.

6. That the cow-keepers be required to divide their cows into small parcels, not more than 10 or 12 in a field together; and that they be allowed such

satisfaction for complying with these proposals as the lords justices shall think fit.

The 4 justices received orders to comply with the preceding proposals, and to allow 40 shillings for every sick cow which they burnt, belonging to Mr. Ratcliff, Rufford, and Pullen. But the free intercourse, which both masters and servants had had with each other's cows, had spread the contagion; and the distemper began soon to appear in several other neighbouring places.

The gentlemen then summoned all the cow-keepers in the county, and acquainted them with the above-mentioned proposals, and offered them 40 shillings for every cow they burnt, that had not been sick above 24 hours; but for such as had been longer ill, or were dead, they would allow them only the value of their skins and horns.

On dissecting 16 cows, in different degrees of infection, I found the putrefaction of their viscera to increase in proportion to the time of their illness. The first 5 that I opened, had herded with those that were ill, and the symptoms of the distemper were just become visible; in these, the gall-bladders were larger than usual, and filled with bile of a natural taste and smell, but of a greener colour. The pancreas was shrivelled, some of the glands were obstructed, and tumefied. Many of the glands in their mesenteries were twice or thrice their natural size. Their lungs were a little inflamed, and their flesh felt hot. All other parts of their viscera appeared as in a healthful state.

The next 6 that I opened, had been ill about 2 days. In them the livers were blacker than usual, and in two of them, there were several cysts filled with a petrified substance like chalk, about the size of a pea. Their gall-bladders were twice their usual size, and filled with bile of a natural taste and smell, but of a greener colour than the first. The pancreas was shrivelled, some of the glands were very large and hard, and of a blackish colour. The glands in their mesenteries were many of them 5 times their natural size, and of a blackish colour. Their lungs were inflamed, with several small cysts forming. Their intestines were full of red and black spots. Their flesh was very hot, though not altered in colour.

The 5 last that I opened, were very near dying. In them I found the liver to be blackish, much shrivelled and contracted, and in 3 of them, there were several cysts as large as nuts or nutmegs, filled with a petrified substance like chalk. Their gall-bladders were about 3 times their usual size, and filled with bile of a natural taste and smell, but of a deep green colour. The pancreas was shrivelled and contracted, many of the glands were very large and hard, and of a black colour. The glands in their mesenteries were many of them distended to 8 or 10 times their natural size, were very black, and in the pelvis of most of those glands in 2 cows, there was a yellow petrifaction, of the consistence of a sand stone. Their intestines were the colour of a snake, their inner coat excoriated by purging. Their lungs were much inflamed, with several cysts containing a yellow purulent matter, many of them as large as a nutmeg. Their flesh was extremely hot, though very little altered in colour.

I have here only given a general account of the dissections, in the three different stages of the disease; in which the difference was but small; but the following cases being very extraordinary, I could not omit the mention of them, viz. in one of them the bile was petrified in its vessels, and resembled a tree of coral, but of a dark yellow colour, and brittle substance. In another there were several inflammations on the liver, some as large as a half-crown, cracked round the edges, and appeared separating from the sound part, like a pestilential carbuncle. In a 3d, the liquor contained in the pericardium (for lubricating the heart in its motion) appeared like the subsidings of aqua calcis; and had excoriated, and given as yellow a colour to the whole surface of the heart and pericardium, as aqua calcis could possibly have done.

In giving my opinion on this distemper, I must beg leave to premise, that all cows have naturally a purgation by the anus for 5 or 6 weeks in the spring, from the frimness of the grass, as the cow-keepers term it; during which time they are brisk and lively, their milk becomes thinner, and of a bluish colour, sweeter to the taste, and in greater quantity: but the spring preceding this distemper, was all over Europe so dry, that the like has not been known in the memory of any one living; the consequence of which was little grass, and that so dry and void of that frimness which it has in other years, that I could not hear of one cow-keeper, who had observed his cows to have that purgation in the same degree as usual; and very few who had observed any at all. They all agreed that their cows had not yielded above half so much milk that summer as they did in others; that some of them were almost dry; that the milk they did give was much thicker and yellower than in other years. It was observed by the whole town, that very little of the milk then sold would boil without turning; and it is a known truth, that the weakest of the common purges you can give a cow entirely takes away her milk. From all which circumstances, and several others of less moment that occurred during my daily conversation at that time with cow-keepers, &c. I think it evident, that the want of that natural purgation was the sole cause of this disease; by producing those obstructions which terminated in a putrefaction, and made this distemper contagious.

Cows are likewise subject to a purgation, though in a less degree, from the same quality in the grass, about the latter end of September; which is called the latter spring; and which I believe contributed not a little to the preventing the increase of this distemper; for this purgation coming so soon after the disease appeared, it is not unreasonable to suppose that it freed such cows as were not much injured from the ill effects of those obstructions, occasioned by the want of their vernal evacuations.

Several physicians attempted the cure of this distemper, and made many essays for that purpose; but the dissections convinced me of the improbability of their succeeding. However, they having received the following recipe and directions from some in Holland, said to have been used there with good success, I made trial of it: but the effect was answerable to my expectation, for in many instances, I was not sensible of the least benefit:

Herb. Aristoloch. rotundæ, Veronicæ, aa M. viij. Pulmonariæ, Hyssopi, Scordij, aa M. 4. Rad. Gentianæ, Angelicæ, Petasitidis, Tormentillæ, Carlinæ, aa lb. ss. Bacc. Lauri, Juniperi, aa ℥xij. Misce fiat Pulv. See Phil. Trans. N° 338, in fine. This powder is to be given in water, 1 oz. at a time, 3 or 4 mornings successively; then rest 4 days; and if the disease continues, repeat the powders in warm water, as before.

I think every method in practice was tried on this occasion, though I cannot say that any of them was attended with an appearance of success; except that of bleeding* plentifully, and giving great quantities of cooling and diluting liquids. But by this method, the instances of success were so few, that they deserve no further mention.

The lords justices being informed that the feeding of cows with distillers grains was a new custom, and was the cause of this disease, gave me orders to examine into the truth of it: but on inquiry, I found it to have been the practice of several of the cow-keepers above 20 years, without the least appearance of any inconvenience; and that some of those persons who had suffered most, had never given any. Nor is there any difference between those of brewers and distillers, only that the latter are the drier.

It was also said, that the want of water was the cause of this disease, for that the springs and places where people used to water their cows, were almost every where dry; and that many were obliged to send them several miles for water. This might produce some diseases, but such only as they got by the fatigue of being driven so far; for Mr. Ratcliff, Mr. Rufford and Mr. Pullen,

* When it is considered that the cows had been stinted in their food (from the scarcity of grass occasioned by the great drought) for some time previous to the appearance of the distemper among them; the propriety of bleeding will be much questioned. Should a similar disorder occur again, it would be desirable to try the effects of the Peruvian bark in powder, together with *cold ablution*, i. e. throwing buckets of cold water upon the sick cattle, until the heat of their bodies be brought down to the natural standard.

the 3 persons where this disease first appeared, had the New River water running through the very grounds where their cows constantly grazed, and could drink at their pleasure, and so had most of the cow-keepers at Islington.

About the latter end of September, the disease increased, and the numbers brought to be burnt were so great, that it could not be well executed; therefore it was judged proper only to bury them 15 or 20 feet deep; first making large incisions in their most fleshy parts, and covering them with quicklime. Also such as had sick cows, were ordered to bring their calves to be buried; and were allowed from 5 to 10 shillings per calf.

In the beginning of October, being informed that some of the cows in Norfolk, Suffolk, and Hertfordshire, had caught this distemper, and apprehending that it would become general, I gave in the following report to a committee of council.

The distemper among the cattle increasing, and beginning to appear in several other counties, I thought it my duty to acquaint your lordships with the hazard that may attend their not being duly buried. It is the opinion of all writers that treat on contagious diseases, as well as of several of the physicians in town, that a putrefaction of so many cows as there is reason to fear will die of this distemper, may produce some contagious disease among men; unless they are buried so deep that the infectious effluvia cannot injure the air, which I am certain has very seldom been complied with, except in the counties of Middlesex, Essex, and Surry; the gentlemen employed being capable of acting in those counties only. It is affirmed by several now living, that there was a mortality among the cattle a little before the last great plague in the year 1665, which was imputed to the want of a due care in burying them. And your lordships may know of what importance it was judged by the King of Prussia, the States of Holland, and several other princes and states, by the care they took to publish decrees and placarts, commanding them to be buried on pain of death, or other severe penalties; and I humbly conceive it would be necessary, not only to bury those which shall die, but likewise such as are already dead; as also that they be buried 9 or 10 feet deep at least.

Their lordships thought fit to defer all proceedings on this report, till the distemper becoming more general should make it necessary; but that necessity never happened; for within 3 or 4 weeks after giving in that report, the following particulars concurred to put an end to the distemper.

The cows began their latter purging, which contributed much to prevent the disease from appearing in fresh places; and the cow-keepers were convinced that the disease was incurable. The knowledge of the distemper was spread all over England, so that none would buy a cow in the country; and the gentlemen prevented their being killed in town, by having the markets examined daily; and such meat condemned as appeared suspicious. The cow-keepers now divided their cows into small parcels, by which they lost only that in which the disease happened; whereas before that method, when one cow took this disease, if she had herded with 100, 200, or 300, the contagion was such that scarcely one escaped. Those who had no sick cows avoided all communication with such as had. They likewise found that the keeping their cows so long when ill, had been the chief cause of their loss; they therefore now brought them to be buried on the first appearance of the disease, before the contagion could possibly have got to any great height.

These were the effects of the cow-keepers dear bought experience; but it was the indefatigable care and diligence of those 4 gentlemen, who gave a daily attendance, both early and late, that secured Great Britain from that terrible ravage, which was made by this distemper in several parts of Europe. The severity of this disease in England did not last above 3 months; though it was not entirely suppressed till about Christmas: but in several other countries it continued 2 or 3 years; and I am credibly assured, that in Holland it still rages with as much violence as ever; and that they have lost in cows, oxen and bulls, above 300,000.

The Divine Providence has so disposed the matter of animal bodies, as to render contagious diseases very seldom infectious to different species; but experience shows that contagions may be communicated to the same species, by touching the woollen, linen, &c. to which the infectious effluvia of the diseased had adhered, though the two bodies should be at a great distance; and I believe that more hundreds died from the infection, which was carried by the intercourse that the cow-keepers had with each other, than single ones by the original putrefaction.

The number of bulls and cows lost by this disease, in the counties of Middlesex, Essex and Surry, were 5418; and of calves, 439; and the money issued for them, at 40 or 10 shillings per cow, &c. was the royal bounty of his Majesty, from his own civil list: and though neither the 4 gentlemen, nor I, made any demand for a reward, or for expences, yet it amounted to 6774l. 1s. 1d. But the entire loss to the cow-keepers, as delivered in upon oath, was 24500l. though computed only at 6l. per cow; which at a medium, was not more than their prime cost. His Majesty was further pleased, on the solicitation of the 4 gentlemen, to grant a brief for the 24500l. but the many false reports that were then industriously propagated, to lessen the value of those poor men's losses, so frustrated that charity, that the entire sum collected (after paying the charges of collecting, was only 6278l. 2s. 6d. which on a dividend, amounted to 5s. $1\frac{1}{4}$d. in the pound, computing their loss as above, at 6l. per cow; though if we consider their contracts with brewers for grains, their rent of grounds which lay useless, servants wages, &c. their real loss may, by a modest computation, be allowed to be 10l. for every cow that died.

A Description of the Organ of Hearing in the Elephant, with the Figures and Situation of the Ossicles, Labyrinth, and Cochlea, in the Ear of that large Animal. By Dr. Patrick Blair, R. S. S. N° 358, p. 885.

In the description I formerly wrote of the elephant I dissected in Dundee, Anno 1706, and inserted in the Philos. Trans. N° 226, 227, I treated of the bony part of the ear of that prodigious animal a little too superficially; because I was unwilling at that time to break up the os petrosum of the right ear, which had accidentally been separated on dividing the skull, by which the account I then gave of the lineæ semilunares, or labyrinth and cochlea, was rather lame. But I have chosen since rather to destroy that bone than that the public should be deprived of an exact description of that curious organ; and that I may give a clear idea of all its bony parts, I shall repeat what I formerly advanced on that subject, and add what improvements I have made on it since.

The meatus auditorius is a long straight tube situated horizontally, and reaching from the outer to the inner table of the skull, not unlike the barrel of a pistol, but somewhat oval, the sides of the cavity are hard and solid, about the thickness of a halfpenny, and from the outer part several of the laminæ between the 2 tables of the skull arise, fig. 8, pl. 9: its cavity is an inch or $\frac{3}{4}$ of an inch diameter, and length $9\frac{1}{4}$ inches; being somewhat enlarged as it arrives at the crena for the membrana tympani. Fig. 9.

This crena is 2 inches in circumference, within which is the cavitas tympani, consisting of 2 different surfaces; the one much deeper and cellulous, the other more superficial and smooth. The first runs perpendicularly down $\frac{1}{2}$ inch from the crena tympani. Its bottom is variously divided into several cellules, not unlike a honeycomb, but irregularly disposed. Its bony laminæ, by which these cellules are distinguished from each other, are thicker at the top than at the bottom, being about $1\frac{1}{2}$ line distant from each other, and about $\frac{1}{4}$ inch deep.

Besides the use of these two cavities, to receive and discharge the superfluous moisture, they are also most beneficial and assisting to the hearing; for, no sooner is the external air modulated, and the membrana tympani moved by it, than the sound is conveyed by the ossicles to the nervus auditorius, and the undulation continued, first by the anfractuosities of the first cavity, and then by the gyres and incurvated lines of the second; so that we may easily account for the acute sense of hearing with which elephants are said to be endowed.

The aqueduct is a flat tube, whose orifice is so situated between the two forementioned cavities, that if there be any superfluous humidity contained in them, it must needs be discharged, at least in this animal, into the mouth. This situation of the aqueduct makes it plainly appear, that its use is to receive the superfluous moisture from the cavitas tympani; for beside the glands fit for separating such a quantity of humidity as may lubrify the muscles, and facilitate both their motion and that of the ossicles, the very vapours that arise in such a cavity as that of the tympanum in this animal, must at last be converted into a liquor, and that must either again be received into the blood-vessels, or otherwise discharged by such a receptacle as this. Some are of opinion that this aqueduct is also assisting to the hearing, especially in men, because it is generally observed that they who are deaf, open their mouths wide when they are desirous to hear more distinctly; but I see not how that can be, for though the cavity of the bony part of the aqueduct, in most of animals, is proportionally large enough; yet its carnous or fleshy part lies for the most part so flat, and its two sides are so collapsed together, that scarcely any air can be admitted, at least so far as to be subservient to the hearing.

The ossicles in this, as in other animals, are 3, or rather 4 in number. The malleolus is an irregular bone, and doubtless has been endowed with pretty large muscles, because of the rugosities, protuberances, and sinuses, observable in it. It has a protuberant head, fig. 11, 4 lines broad, next to that a crena or semicircular sinus (2), after which the bone is raised, affording a protuberant margin to an oblong sinus (3), for receiving the head of the incus, 4 lines broad. The opposite part of this sinus, or back part of the bone, is convex, of an unequal rugous surface, with a great many protuberances and depressions, for the origins and insertions of the muscles, for the space of 5 lines, where it forms an angle, from whence it becomes flat and smooth, being 3 lines broad, and reaching 4 lines to another angle (5), where the manubrium malleoli begins, and where it becomes more round, from whence it gradually tapers to the point, being 6 lines in length.

The head of the incus is 4 lines broad, fig. 13, (1) below which is the neck or an oblique sinus, (2) next to that are 2 apophyses, one on each side. These descending obliquely outwards, and becoming flat, meet in a point, fig. 14, (5), whence ascending obliquely inward, this production is joined to another small round one, like the manubrium malleoli, 4½ lines long (6). This has the fore-mentioned small excavation or half round sinus (7), which with the extremity of the stapes, I suppose to have contained the os quadrangulare, or rather orbiculare, according to the figure of the sinus.

The stapes differs much in figure from the human one. From its concave extremity it is enlarged on each side by two small slender productions, not unlike the processes of the vertebræ of some fishes, fig. 13, (22) to which is joined the basis (3), as thin almost as the scale of a fish. This was accidentally separated from its two sides, and remained in the foramen ovale, from whence I pulled it with a pin, it is concave towards the stapes, and convex toward the vestibulum.

The foramen ovale lies so hid and obliquely in the side of the cavitas tympani, that it could not be delineated in its true dimensions. Near it is another hole, oblong and sharp at both ends, both which give an entry into the vestibulum.

The vestibulum is of an irregular figure, fig. 17, (a); it is for the most part 3 lines from the one side to the other, and perforated by 8 orifices, viz. 5 for the canals of the labyrinth, fig. 16, 17, (a) 1 for the cochlea, fig. 17, (h), and 2 for the fenestræ (b,c).

The cochlea is a long cavity consisting of 3 gyres or meanders, fig. 18, (def). Its orifice, where it proceeds from the vestibulum, is but small; but it afterwards widens, so that the first course of this cavity is a third part larger than the second (e), and proportionally the third is less than the other two (f), till it terminates in an orifice (g) situated in the top, for receiving a branch of the soft portion of the nervus auditorius, which accompanies and passes along all its gyres.

The hardness and solidity of the bone, from which it may be justly called os petrosum in this subject, was such that I could not exactly trace the three canals or ducts of the labyrinth, so as to give a true idea of the manner of their several turnings. But Valsalva's figures of the human ear directed me so exactly, that I easily found out the several orifices, and opened them so far as to find out their situation and true dimensions, by introducing a hog's bristle, then cutting it off and stretching it out to the scale. Thus after laying open the two foramina which gave an inlet to the vestibulum, I soon perceived the several orifices, which in so large a subject were pretty conspicuous. I first turned to the one hand and discovered the duct of the cochlea; this I pursued all along the protuberance, fig. 10, (d); in doing of which I laid wholly open the lesser duct of the labyrinth, fig. 16, 17, (d). Then turning up the other side of the bone, I traced the soft portion of the nervus auditorius, divided into two branches, one distributed into the cochlea, and the other to the labyrinth. In filing the bone a little further, I opened a small part of the middle duct, and soon discovered the ductus major; after which I measured their several lengths as is said.

The labyrinth then consists of three lineæ semilunares or incurvated ducts, whereof the major lies in that part of the processus petrosus, which regards the seat of the brain (b). This is 20 lines or 1 inch 8 lines long. The medius ductus, one part whereof regards the orifice of the cochlea, and the other is common with the major for the space of 3 lines; (e) this is 15 lines or 1 inch 3 lines long; and the minor, which regards the cavitas tympani, has one orifice which is near to the medius, where it approaches the cochlea, and the other near to the orifice of the major; this is 1 inch long.

The seventh pair of nerves, called in general the nervus auditorius, enters the processus petrosus, and is divided into the hard and soft portions, as in other animals. In this subject I find one canule entering the bone from the sides of the orifice for the carotid artery, about 3 lines diameter (e) (h), from thence running forward for the space of 1 inch 4 lines, then bending downwards 1 inch, till it meets with the orifice at the sides of the meatus auditorius, by which it pierces the skull, and passes outward. This canule, after it has entered the processus petrosus for the space of 8 lines, communicates with the orifice, which usually enters the aforesaid process from the base of the skull; and both these orifices, after they have accompanied one another about 5 lines, are separated, and the soft portion penetrates the bone at two places, as is said.

Explication of the Figures.

Fig. 8, pl. 9, represents the bony part of the meatus auditorius of the right ear; a the external orifice of the meatus auditorius; b the processus petrosus; c the orifice where the nervus auditorius enters; d the meatus auditorius; e a part of the laminæ which proceed from it on each side, by which the cellules between the two tables of the skull are formed, those situated above the meatus being removed; f part of the inner table of the skull.

Fig. 9, represents part of the meatus auditorius opened, with other parts of the inner ear; a the ragged part of the bone, from whence the os petrosum was separated; b the processus petrosus opened; c the crena for the membrana tympani; d the honeycomb cavity of the tympanum; e its inner cavity of a smooth surface; f its semicircular or undulated lines; g the orifice of the aqueduct; h the orifice of the hard portion of the nerve.

Fig. 10, represents the lower surface of the os petrosum, as it was separated from above the tympanum and other parts of the inner ear; aa the ragged margin of the bone; bb the upper part of the cavitas tympani; c the foramen ovale; d the protuberance in which the labyrinth and cochlea are lodged; e the orifice of the hard portion of the nervus auditorius.

Fig. 11, represents the malleolus alone in its true dimensions; 1 the protuberant head; 2 the semicircular sinus between it and the margin; 3 the sinus which receives the head of the incus; 4 the angle below the sinus for the head of the incus; 5 the angle where the manubrium malleoli begins; 6 the manubrium malleoli.

Fig. 12, represents the incus; 1 the head of the incus; 2 the sinus or neck of the incus; 3 two apophyses; 4 a long protuberance with the sinus for the os quadrangulare at its extremity.

Fig. 13, represents the stapes; 1 the small part of the stapes, where it is articulated with the incus, with a sinus at its extremity, being the other half of the cavity for the os quadrangulare; 2 2 two small portions of the stapes, where it is articulated with the basis; 3 the basis of the stapes separated; 4 the whole stapes.

Fig. 14, the malleolus and incus joined together, with their lower side turned up; 1 the malleolus;

2 its articulation with the incus; 3 the incus; 4 the manubrium malleoli; 5 a point of the incus, framed by the other two productions; 6 the long protuberance of the incus; 7 the sinus in the extremity of its long production.

Fig. 15, the malleolus, incus, and stapes, articulated together; 1 the incus; 2 the malleolus; 3 the stapes where it shuts the foramen ovale.

Fig. 16 represents the upper part of the lineæ semilunares, or that side which is towards the passage of the nervus auditorius; a the five extremities cut off; b the linea semilunaris major; c the semilunaris media; d the minor; e the common canule between the major and media.

Fig. 17 represents the cochlea and labyrinth together; a the vestibulum; b the foramen ovale; c the foramen oblongum; d the linea semilunaris minor, which is towards the cavitas tympani; e the common canule to the major and media; f the major; g the media; h the cochlea.

Fig. 18 represents the cochlea; a the vestibulum; b the third gyre or turning; c the orifice; d the first gyre or turning opened; e the second turning; g the orifice at the top of the cochlea.

Observations of the Transit of the Body and Shadow of Jupiter's Fourth Satellite over the Disks of the Planet. By the Rev. Mr. James Pound, F. R. S. N° 359, p. 900.

Feb. 16, 1719, at $6\frac{1}{4}^h$, through a short tube, we saw all the four satellites, the three outermost on the east side of Jupiter, and the innermost near the western limb approaching to an eclipse. The fourth at that time was about half a semidiameter of Jupiter from the eastern limb. It then proved cloudy till about 8^h, at which time, through the long glass, we could see only the second and third satellites, the first being behind Jupiter in the shadow, and the fourth entered on the disk. We saw at this time a dark spot, a little northward of the great northern zone, and near the eastern limb, where the satellite was to enter on the disk, which we took for the shadow of the satellite. The clouds then again intercepted our view, till $8^h\ 53^m$ mean time, when the first satellite was lately emerged out of the shadow, and the spot advanced so far, that we perceived it would arrive at the middle of Jupiter near 2 hours sooner than the shadow ought to have done by our computation; but not imagining that this dark spot could be any thing else but the shadow, we concluded there had been some error in the calculation, which we thought to re-examine afterwards. On this presumption we left off observing till $9^h\ 35^m$, at which time we were surprised to see a notch in the limb of Jupiter, near the place where the former spot entered. This last appearance agreeing well with the time that the shadow of the satellite ought to have entered the disk, soon made us alter our former opinion, and conjecture that this, and not the other spot, was the said shadow. At $9^h\ 39^m$ the notch vanishing, a round black spot appeared within the limb, but in contact with it. At $9^h\ 45^m$ we judged the first spot, and at $11^h\ 45^m$ the second, to be in the middle of Jupiter.

At $11^h\ 50^m$ the first spot touched the limb, being within the disk; soon after which the limb in that place seemed a little protuberant. At $12^h\ 5^m$ appeared the fourth satellite just come out of the disk, and touching the limb in the place where the protuberancy was. At $12^h\ 7^m$ we could perceive the satellite separated from the limb. At $13^h\ 56^m$ the second black spot, still within the disk, just touched the western limb; soon after which there appeared a notch in this part of the limb, as it did on the other at the coming on of this spot. At $14^h\ 6^m$ the spot was all gone off, and the limb appeared clear and entire. The first spot, when in the middle of Jupiter, was almost as black as the second when near the limb, but somewhat less and a little more northerly.

At the time that the first spot was in the middle of the disk, the three innermost satellites appeared to the east of Jupiter; the first having lately emerged out of the shadow; the second being almost at its greatest distance; and the third having passed the axis of the shadow about 12 hours before, and appearing at this time about 3 diameters of Jupiter from his limb. The times that these spots arrived at the middle of the disk are agreeable to the times found by calculation, in which the fourth satellite and its shadow ought to have appeared there. From all which it is plain, that the first of these spots was the fourth satellite itself, and the second its shadow.

We have seen the first and second satellites appearing not as dark spots, but as bright ones, somewhat different from the light of Jupiter, for some little time after they entered his disk, but as they approached nearer the middle, we lost sight of them. And we have frequently observed that the same satellites appear brighter at some times than at others; and that when one of them has shone with its utmost splendour, the light of another has been considerably diminished. From whence it is very probable, not only that the satellites revolve on their proper axis, but also that some parts of their surfaces do very faintly, if at all, reflect the solar rays to us.

All which has for some time past been observed and noticed by Messrs. Cassini and Miraldi, as may be seen in the Memoirs of the Academie Royale, for the years 1707 and 1714.

On the Situation of the ancient Carteia. By John Conduit, Esq. F. R. S. N° 359, p. 903.

About 4 English miles N. W. from Gibraltar, at the end of the Bay, there are considerable ruins. The place is at present called Rocadillo, and consists of a few huts, and a modern square tower, which appears to have been raised on the foundation of a much greater pile. The walls of the old city are very easy to be traced. They seem to have been about 2 English miles in circumference, and built on the brow of a rising ground. The space within is covered with ruins, among which are a great many pieces of very fine marble well wrought; and innumerable fragments of vessels of that kind of red earthen ware, which Ambrosio Morales in the first chapter of his Discurso de las Antiguedades de las Ciudades de Espanna, lays down for a certain mark of a Roman city, and takes to have been a composition of the clay of Saguntum, often mentioned among the Romans.

There are remains of a rude semicircular building, raised on arches, which descends gradually into an area, and seems to have been a kind of theatre. I brought away with me a marble pedestal of a statue, dug up near the square tower, and containing the following letters finely cut, VARIA MARCE. I have a considerable number of medals, found among these ruins; most of them have a caput turritum, with CARTEIA in very legible characters. The reverse is generally a fish, a neptune, or a rudder.

The Spaniards who live about the ruins, say they are the remains of a city of the Gentiles called Cartago. The corruption of Carteia into a name so much more talked of, might easily happen in an oral tradition of so many years; and I cannot help thinking that, where other circumstances concur, an account delivered down from father to son is an evidence not to be slighted, in matters of so much obscurity.

I have some medals that were dug up at Rocadillo, with the head and club of Hercules on them. On the reverse are tunny fishes, which according to Strabo and Pliny abounded formerly near Carteia, and are still taken in great quantities near the shore of the east sea, at a small distance from Rocadillo.

Bernardo Aldrete, an author of such weight, that Bochart does not disdain to copy him on several occasions, in the 2d book and 2d chapter of his Antiguedades de Espanna, accounts for the addition of eia to Cartha; which in the Syriac and Chaldean signifies Pulcher, Formosus, and was affixed to the name of this city to distinguish it from the Cartha in Syria, mentioned in the 21st chapter and 34th verse of Joshua.

By all accounts, the Phœnicians founded most of the cities on this coast, and probably Carteia was one of their earliest settlements; for it lies very near Africa, in a most inviting situation, having on one side a Bay, and on the other a river, which waters a rich country. Its height gave it strength and a very beautiful prospect; circumstances which seem to justify Aldretes's interpretation of the latter part of its name.

In the itinerary of Antoninus, it is Calpe-carteiam, not tanquam duæ urbes diversæ, as Casaubon intimates in his notes on the third book of Strabo, for then it would be Calpen-carteiam. Probably Calpe-carteia is for carteia ad calpen, to distinguish it from the other Carteia in Celtiberia, mentioned in the 21st book and 5th chapter of Livy.

I am very much surprised that Mariana, and several others, should take the present Gibraltar to have been the ancient Heraclea; when neither Pliny, who resided so long in those parts, Mela who was born there, nor any ancient geographer or historian that I have met with, makes the least mention of such a city thereabouts, except Strabo; and he places it 40 stadia from Calpe, at the foot of which Gibraltar is situated. The Spanish historians give good ground to believe there was no town upon that mountain till the Moors invaded Spain under Tariff, who gave it the name it has retained ever since.

A Letter of M. l'Abbé Conti, F.R.S. to the late M. Leibnitz, dated at London, in March 1716, *concerning the Dispute about the Invention of the Method of Fluxions, or Differential Method*; *with M. Leibnitz's Answer.* N° 359, p. 923. *Translated from the French.*

I have hitherto deferred answering your letter, as I wished to accompany my answer with that of Mr. Newton made to l'Apostille, here added. I shall not enter into particulars as to your dispute with Mr. Keill, or rather with Mr. Newton. I can only relate historically what I have seen, or what I have read, to form a proper judgment of the matter.

I have read, says the Abbé, with the greatest attention, and impartiality, the Commercium Epistolicum, and its extract published in Phil. Trans. N° 342. I have seen the original letters of the Commercium in the custody of the Royal Society, a short letter (dated March 17, 1693, and printed at the end of Raphson's history of fluxions) in your own hand-writing to Sir Isaac Newton, and the old manuscript, entitled, Analysis per series numero terminorum infinitas, sent by Sir Isaac Newton to Dr. Barrow, and published by Mr. Jones: from all which I conclude, that retrenching every thing that is foreign to the dispute, the only question seems to be, whether Sir Isaac Newton had the method of fluxions, or the infinitesimal calculus, before you, or you before Sir Isaac Newton. It is true, you were the first who published it; but you acknowledged, that Sir Isaac Newton had given considerable hints of it in the letters he wrote to Mr. Oldenburg and others, as is shown at great length in the Commercium, and its extract. What are your answers then to all this? This is what the public still wants, in order to form an exact judgment of the affair.

Your friends expect your answer with great impatience, and they think you cannot dispense with returning an answer, if not to Mr. Keill, at least to Sir

Isaac Newton himself, who bids you defiance in express terms, as you shall see by his letter.

I would fain see you at a good understanding with each other. The public reaps no advantage from disputes, but loses irretrievably, for several ages, all the discoveries of which it is deprived by such disputes.

His Majesty was pleased to desire, I should inform him of all that passed between Sir Isaac Newton and you; I have done it to the best of my power, and I could wish it were with success for both.

Your problem was very easily resolved in a little time by several geometricians, both at London and Oxford. The solution is general, extending to all sorts of curves, either geometrical or mechanical. The problem is proposed a little equivocally; and I believe Mr. De Moivre is not mistaken, in saying, that we must necessarily fix the idea of a series of curves. For instance, supposing they have the same subtangent for the same abscissa; this will agree not only with the conic sections, but also with a vast many other curves, both geometrical and mechanical. And other suppositions may still be made for fixing this idea.

I shall take another opportunity to speak of Sir Isaac Newton's philosophy. We must previously agree on the method of philosophising, and carefully distinguish between Sir Isaac Newton's philosophy, and the consequences several people draw from it without the least foundation. They ascribe to this great man several things, he by no means admits, as he has made appear to the French gentlemen who came to London, on account of the great eclipse.

N. B. M. l'Abbé Conti also spent some hours in looking over the old letters and letter-books, kept in the archives of the Royal Society, in order to see if he could find any thing, which made either for M. Leibnitz, or against Sir Isaac Newton, and had been omitted in the Commercium epistolicum Collinii et aliorum; but he could find nothing of that kind.

M. Leibnitz returned the following answer to M. l'Abbé Conti, in a letter dated at Hanover the 14th of April 1716.

I have answered the letter you honoured me with, and at the same time that Mr. Newton wrote to you; and have sent the whole to M. Remond at Paris, who will not fail to convey it to you. I have taken this way, in order to have indifferent persons, and such as understand our dispute, as evidences in the affair; and M. Remond will also communicate it to others. I have likewise sent him a duplicate of your letter and Sir Isaac Newton's: after which you may judge if the chicanery of some of your new friends embarrasses me much.

As to the problem, of which some among them would solve particular cases, in order to fix, as they pretend, its idea; it is likely they will pitch upon such as are easy; for there are such in transcendental, as well as in common curves: but the question is to obtain a general solution: this is no new problem; M. Jo. Bernoulli proposed it in the Acta Erudit. for the month of May 1697, p. 211. And as M. Fatio despised what we had done; the proposition was again repeated in the Acta for May 1700, p. 204. It may still serve to make some people sensible whether they have made such advances as we in methods, and till they can arrive at a general solution, they may try their skill in fixing the ideas in a particular case, which I here send you: its solution is by the same M. Bernoulli: and be so good as not to give in too readily to the insinuations of such as oppose us, when they would persuade you they found no difficulty in our problem.

A problem containing a particular case of the general problem about finding a series of curves, each of which is perpendicular to another series of curves.

Upon a right line AG (fig. 1, plate 10) as an axis, and from the point A having constructed any number of curves, as ABD, of such a nature, that the radius osculi BO, drawn from each point of each curve, for instance from B, be cut by the axis AG in the point C in a constant given ratio, so that BO may be to BC, as M to N. Now the trajectorial curves ENF, are to be constructed, cutting the former curves ABD at right angles.

Thus far this letter, M. Leibnitz first proposed the general problem to M. l'Abbé Conti in words to the following purpose: To find a line BCD, as in fig. 2, that cuts at right angles, all the curves of a determinate series of the same kind, ex. gr. all the hyperbolas AB, AC, AD, having the same vertex and the same centre; and that by a general method. And in the Acta Erudit. for October, 1698, p. 470, 471, he calls the curves in this determinate series, curvas ordinatim datas et positione datas et positione ordinatim datas. And by all this the series of curves to be cut is given, and nothing more is to be found, than the other series which is to cut it at right angles. But M. Leibnitz being told that his problem was solved, he changed it into a new one, of finding both the series to be cut, and the other series which is to cut it. And the particular problem proposed in this letter is a special case, not of the general problem first proposed, as it ought to have been, but of this new double problem. And the first part of this double problem (viz. by any given property of a series of curves to find the curves) is a problem more difficult than the former, and of which a general solution is not yet given. Mr. Leibnitz, in a letter to Mr. John Bernoulli, dated Dec. 16, 1694, and published in the Acta Eruditorum for October 1698, p. 471, set down his solution of the problem, when the given series of curves is defined by a finite equation, expressing the relation between the absciss and ordinate. The same solution holds when the equation is a converging series, or when the property of the curve to be cut can be reduced to such an equation, by the Analysis per Series numero terminorum infinitas. But Mr. Leibnitz was for solving the problem without converging series.

Pars reliqua Dissertationis de Potentiâ Cordis. Authore Jacobo Jurin, M.D. et R. S. S. N° 359, p. 929. [See p. 375 of this vol.]

A New Universal Method of describing all Kinds of Curves by means of Right Lines and Angles only. By Colin Maclaurin, Profes. of Math. in the New College of Aberdeen. N° 359, p. 939. *Translated from the Latin.*

As the great Newton has not extended his method of describing curves to those of the 3d order which are without a double point, nor to those of a higher order wanting a multiple point; and pronounces their description to be counted among the more difficult problems in geometry; I hope the following method will not be unacceptable to geometricians, by which geometrical curves of any order are constructed, though they may be without a punctum duplex or multiplex.

1. Lines of the first order are only right lines themselves, which can meet one another only in one point. Lines of the 2d order are the conic sections, which cannot be cut by a right line in more than 2 points. And all these may be thus constructed, according to lemma 21, lib. 1, of Newton's Principia: Let two given angles MCR, LSN (fig. 3, pl. 10) move round two given points C and S, so that Q the concourse of the legs CM, SL, may always describe the indefinite right line AE given in position; then the concourse P of the other legs CR and SN will describe a line of the 2d order, or a conic section.

2. Let the angle MCR move as before about the given point C (fig. 4); and the given angle LNQ have its angular point N always carried along the given right line AE, so that the leg QN may always pass through the given point S. Then, 1st, if the concourse Q of the legs CR and SN be drawn along the infinite line AB, the concourse of the legs CM and NL will describe a curve line of the 3d order having a double point at C. 2dly, Other things as before, if the concourse of the legs CM and NL (fig. 5) be drawn along the indefinite line AB; then the concourse P of the legs CR and SN will describe a curve of the 3d order having a double point at S.

Example of Case 1. Let the angles MCR, LNS be right ones (fig. 6), and AE, DB, CS be parallels; also let SA and SD be perpendicular to AE and DB respectively; and let SD = 2SA. Then if SD be less than CS, the curve described according to the first case, will be a parabola with a node and an oval, of the 68th species of Newton's curves. But if SD = CS, the oval will vanish, and the node become a cusp, and the curve described will be the Neilian or semicubical parabola. And if SD be greater than CS, the curve will be a punctated bell-formed parabola, of the 69th species.

3. Let the given angles RMT, KNL (fig. 7) move in such a manner, that the points M and N may run over the indefinite lines BM and DN respectively; and let the legs RM and KN always pass through the given points C and S. Now, 1st, if the concourse Q of the legs MT and NL be drawn along the indefinite line AQ; then the concourse P of the legs MR, NR, will describe a line of the 4th order, having 2 double points, the one in C and the other in S. But, 2dly, if the concourse MR and NK (fig. 8) be drawn along the indefinite line AQ; then the concourse of the legs MT and NL will describe a line of the 4th order, having no double point.

4. Now in the first case of this construction, if the lines CMR, SNK (fig. 7) coincide together with CS; then the points C and S become simple, and the curve will be of the 3d order, without a double point. For example, let BM, AQ, DN (fig. 9) be parallel lines, and all perpendicular to CS; and let the angles RMT, KNL be right ones; then if a curve be described according to the rule of the first case, the legs CMR and SNK will coincide with CS; and by this construction may be described Newton's curves 10, 11, 20, 21, 40, according to the various positions of the points C and S in respect of the three lines BM, AQ, DN; and all these species will be without a double point.

5. Now lines of the 4th order, which have a triple point, may be thus constructed: Let AQ, BN, DM (fig. 10) be three lines given in position; also let the angles QCT, SNM, NML be given and invariable; and let the points N and M be carried along the lines BN and DM, so that the leg NQ may always pass through the given point S; also let QCT revolve about C, so that the concourse of the legs CK, SN may pass along a third line AQ: then the concourse of the legs CT, ML, will describe a line of the 4th order, having a triple point in C.

6. I have shown how lines of the 4th order may be described, which have a triple point, or two double points. Others having only one double point, may be conveniently described thus: Let AQ, BN, DM (fig. 11) as before be three lines given in position, and SNK, SML, RCT three given angles; also let the points N, M, S be always in the same right line; and let the points N and M as before move along the lines BN and DM: then if the concourse of the legs CR, NK be drawn along the indefinite line AQ, the concourse of the legs CT, ML will describe a line of the 4th order, having one double point only in C. And these last two propositions give us new methods for describing lines of the 3d

order, both those that have double points, and those that have none: but in this short specimen of our method, these must be omitted.

7. Let the lines and angles remain as in prop. 3: but let the concourse of the lines MT, NK (fig. 12) be now drawn along the indefinite line AQ; then the concourse of the legs MR and NL will describe a line of the 5th order, having a quadruple point in S. I have also other methods of describing curves of the 5th order, which have a triple or double point, or two double points, or none but simple points: but these above may suffice to show the simplicity and universality of the method. But it must be observed, that in particular and simpler circumstances of the lines and angles, sometimes a curve line will pass into another of an order inferior to that explained in the proposition. Indeed all the propositions afford particular methods of describing some curves of every inferior order.

8. *General Prop.* Let there be taken at pleasure any number n of right lines any how situated in the same plane, as BN, ER, FT: also let any number m of other lines be taken at pleasure, as DM, GL, HK, &c: and let the angles CNR, NRT, RTQ, &c, as also the angles SML, MLK, LKQ, &c, be invariable, while the angular points N, R, T, M, L, K move along the indefinite lines BN, ER, FT, DM, GL, HK; and let the concourse of the legs TQ and KQ be drawn along the indefinite line AQ: to find the order of the curve which shall be described by the concourse of the leg SM with any one of the lines CN, NR, RT, TQ, &c, for instance, with the line RT.

In the series of lines CN, NR, RT, TQ, &c, let s denote the number of the line RT, by the concourse of which with SM the curve is to be described, from the line CN inclusively; which in this case is the third, or $s = 3$: then will the curve be of an order expressed by the number $sm + s + n + 1$. Hence in the case denoted by the figure, since $s = m = n = 3$, the curve will be of the 16th order.

In these descriptions we have only postulated that right lines and angles should be given. But often the more complex curves are easier described by means of simpler ones; and I have investigated propositions of this kind not less universal than these. But I omit them at present, with the demonstrations of these, as too prolix, though I may publish them hereafter.

An Account of an ancient Roman Inscription lately found at Caerleon upon Usk. By Mr. Wm. Rice, Rector of that Place. With some Conjectures thereon, by the Rev. Dr. John Harris, S.T.P. and R. S. S. N° 359, p. 945.

A person being at plough in a close near the bank of the river Usk, which the ancients called Isca, (which glides by us about a quarter of a mile off from Caerleon, and in sight of the town) came across a stone, and finding letters on it, he took it up whole; it is about a yard in length, and about three quarters broad. The inscription is as follows:

D M

G. VALERIVS. G. F GALERIA. VICTOR LVGDVNI. SIG. LEG. II AVG STIP. XVII. ANNOR XLV. CV RA. AGINT. AMNIO. PERPITVO. B

This inscription confirms what others have found hereabouts; and what Camden and other historians show us, viz. That the second Roman legion, called Augusta, brought into Britain by Claudius Cæsar under the conduct of Vespasian, was placed here at Isca or Caer Legion, by Julius Frontinus, to awe the Silures: and that general obtained several victories over them and their neighbours in several places hereabouts.

There seems to be nothing of moment or of difficulty in this inscription; but Victor Lugduni: which as I think we have no ground from history to refer to Lyons in France, so I guess that expression may be thus accounted for. The river Lugg is famous in the neighbouring parts; and as Dynas or Dyn has signified a town in the ancient British language; and that Dun also expresses a hill or down as we still call it, which I think is derived from the British also, probably Lugduni here may express some town or hill near the river Lugg; and since there is a place called to this day Luckton, on the side of the river Lugg in Herefordshire, perhaps that may bid fair to be the very place where Valerius obtained the victory perpetuated by this inscription.

Of the Maxima and Minima in the Motions of the Celestial Bodies. By Mr. Abr. Demoivre. N° 360, p. 952. *Translated from the Latin.*

Before Kepler astronomers conceived the planets revolved in circular orbits. But that author was the inventor of the theory now used, viz. that the celestial bodies move round the sun, placed in the common focus of the elliptic orbits, by this law, that areas proportional to the times are described by radii drawn to the sun. But it requires the most sublime geometry to show by what cause this is performed, and that it could not be otherwise. This glory was reserved for the celebrated Newton.

Treading in his steps, Mr. Demoivre has given some corollaries, in N° 352 of these Transactions, which are ready theorems, by which are determined the velocities or moments, both of the real and apparent motion about the sun, as also of the approach or recess to or from the sun, at any given point of given orbits. Then, further to improve the theory of the planetary system, by means of the same theorems he has deduced the moments of the said moments, and shown in what points of the orbits are the greatest changes of these velocities, and that by very neat and easy solutions.

Let ABP, fig. 14, pl. 10, be the elliptical orbit of a planet, AP the transverse axis, CB the semiconjugate, S the sun, Q the other focus of the ellipse. Through S draw SM parallel to CB: then M will be the point where the sun's distance increases or decreases the quickest, and $SM = AC - \frac{SC^2}{AC}$.

And if SL be taken a mean proportional between the semi-axes AC, CB, then L will be the place of the greatest equation of the centre, as they call it; or where the angular motion is equal to the mean motion. If the excentricity be not greater than what it is in most of the planets, then $BL = \frac{1}{4}BM$ nearly: and $SL = \sqrt{\sqrt{AC^4 - AC^2\,SC^2}}$.

If there be required the point N, in which the real motion in the curve changes the quickest, the problem is a solid one. For $2NS = 4AC - 2NQ$ is to $3NQ - AC$ as $AC^2 - CS^2 = CB^2$ is to NQ^2: and therefore, putting $AC = a$, $CB = c$, and $NQ = y$, we shall have the equation $y^3 - 2ayy + \frac{3}{2}ccy - \frac{1}{2}acc = 0$; which being resolved, it will give y or NQ the distance of the required point N from the other focus. But in orbits that are but little excentric, as those of the planets, if there be made $CD = SQ$, and $AK = AD$, then the remaining part of the axis PK will be $=$ NS the sun's distance from N very nearly. But if the orbit be parabolical, SN will be to SP as 5 to 4, and the angle $NSP = 53° 8'$ nearly, its sine being $\frac{4}{5}$ of the radius.

But the point O, in which is the greatest acceleration of the apparent or angular motion of the descending planet, or the greatest retardation of the ascending, will be obtained in this manner: In AC take $CG = \frac{1}{6}AC$, and make CSF an angle of 30 degrees, draw SF, and take CE equal to it, also take $GH = GE$: then if SO be made $=$ PH, the point O will be the place of greatest change of the angular motion of the planet, revolving in the elliptical orbit ABOP: for in that place of the orbit, the second differences of the equations of the centre of the planet will be found the greatest: and $SO = \frac{7}{6}AC - \sqrt{\frac{1}{36}AC^2 + \frac{1}{3}SQ^2}$. But when the orbit is parabolical, as in the comets, take SO to SP as 8 to 7, then the angle OSP will be $41° 24'\frac{1}{2}$, or its sine is to radius as $\frac{1}{4}\sqrt{7}$ to 1.

Lastly, the direction of the tangent of the orbit will change with the least velocity in the point R, if SR be taken $= \frac{2}{3}AB$. If the excentricity SC be less than $\frac{1}{3}$ PC, this minimum does not take place, but this velocity with which the tangent revolves is always decreasing, as far as to the aphelion; as it is in the motions of all the planets. Neither does it obtain in a parabolic orbit, because of its axis being produced in infinitum.

All these things are demonstrated from the foregoing theorems of Mr. Demoivre, by the precepts in the doctrine of maxima and minima.

An Apology against M. John Bernoulli, Math. Profes. Basil. By Dr. Brook Taylor, R. S. S. N° 360, p. 955. *Translated from the Latin.*

In an " Epistle for an eminent mathematician, Act. Leips. 1716, among others, I am accused of plagiarism, as if I arrogated to myself the inventions of M. John Bernoulli and others. Let them produce their examples, and then they shall have an answer. I have indeed treated of many things in common with others; but I have by no means used other men's inventions as my own. I have everywhere used my own analysis, except in the problem of the isoperimeters; so that I have no ways defrauded others. They should have named the authors, from whom I have taken my methods. I have so great a veneration for the illustrious names of Huygens, Hospital, Varignon, Leibnitz, &c. that I may have erred on the contrary side, and been wanting to myself, having always thought it an honour to quote such men as these. There may have been some little negligence in the matter, that being wholly intent on things of importance, I omitted some little historical matters. What problems I have treated of in common with Bernoulli, are on the funicularia, on the centre of oscillation, and on isoperimeters. In the two former of these I have used my own analysis entirely. In the isoperimeters used that of the author James Bernoulli, a man very deserving in the mathematics, to whom I now pay his due honour. My solution of the problem on the centre of oscillation, was communicated to my friends from the beginning of the year 1712, as can be witnessed by the letters of Dr. Keill. And my book was in the possession of the Royal Society, and communicated to our mathematicians, from April 1714: which I think necessary to mention here, lest John Bernoulli should claim that solution also to himself. His two solutions were published the same year; the one of them so perfectly agreeing with mine in the principles, that it might be said they were both invented by the same person. The matter of the isoperimeters was invented by James Bernoulli, as hinted above; and his solution, with the analysis, is in the Leipsic Journal for 1701. His brother John's analysis is in the Memoirs of the Paris Acad. for 1706; and another solution is in my book. Jo. Bernoulli has lately published a commentary on the same subject, in the Leipsic Acts of 1718. There, lest he should seem to be doing the same thing over again, he spitefully endeavours to detract both from my solution and his brother's, objecting prolixity to the latter, and obscurity to mine. He promises great things by those new undertakings; that by means of a certain principle of the law of uniformity, which he pretends no person has hitherto observed, he will complete the whole matter with very little trouble, and almost without calculation: but I know not by what fatality, in this affair of the isoperimeters, he never finds the gods propitious: for first, that former analysis of his, from beginning to end, consists of one continued blunder:

secondly, his boasted principle of uniformity, which he so boldly affirms no one has observed, has before been noticed by myself, in some examples in p. 113 of my book. Lastly, the analysis he here exhibits as a new one, is only that of his brother. For it is the precepts that constitute the analysis; according to which the calculation is afterwards performed; which is not the analysis itself, but only its instrument. The precepts being once laid down, every one easily performs the calculation, each in his own way, some more diffusely, others closer or neater, each as his genius directs. It must be allowed that Jo. Bernoulli has made the calculation more neat and elegant; but he has done it by his brother's analysis, not his own. And doubtless his brother, had he been now living, would have illustrated the matter quite as well. We have said that all the precepts, which form the analysis, are his brother's. For, that he considers a small arch of the required curve as composed of three small elementary right lines, is only owing to his brother, as he himself has confessed: that from the given length of that small arc, he seeks the ratio of the differences of the ordinates, in his lemmas, is from his brother: that he seeks the same ratio over again, by supposing the small nascent area, composed of what he calls the functions, to be either a maximum or minimum, is from his brother: lastly, that from that double expression of that same ratio he obtains the equation, by which the nature of the required curve is determined, is from his brother. But these are the things that constitute the solution, which therefore is wholly his brother's.

After then giving some instances of John Bernoulli's erroneous and absurd calculations in differentials or fluxions, Dr. Taylor concludes: now perhaps it might be inquired, by what right he pretends to the first rank in the sublimer analysis, with so stubborn an ambition; so that no one can make any advances in it, but he must be presently accused of having penetrated into Bernoulli's profound science. How does it appear to be true, what has lately been affirmed by some one, that the rules in the treatise "Analyse des Infiniment Petis," were first derived from Bernoulli? That the praise generally given to the Marquis de l'Hospital, must now be transferred to his preceptor? &c.

An Account of the Skeleton of a large Animal impressed on Stone. By Dr. William Stukely, F. R. S.* N° 360, p. 963.

At Elston, near Newark in Nottinghamshire, was discovered an almost

* Dr. Stukely was bred to physic and took the degree of M.D. at Cambridge in 1719; but the year following he relinquished the medical for the clerical profession, being ordained in 1720; soon after which he was presented to the living of All Saints in Stamford; some years afterwards he had the living of Somerly, near Grantham; and at length the rectory of St. George the Martyr, in London. Among his medical writings may be mentioned his Dissertation on the Spleen, and a Tract

entire skeleton (as represented fig. 3, pl. 11) of a large animal, impressed on a very hard blue clay stone; the same as, and undoubtedly came from, the neighbouring quarries about Fulbeck, on the western cliff of the long tract of hills extending quite through the adjacent county of Lincoln. It lay, time out of mind, at the side of a well near the parsonage-house, where it had served for a landing-place to those that drew water; but on removal, the under-side exhibited this unusual form. Where the remaining part of the stone may be, which contained the upper-part and continuation of the skeleton, is now utterly unknown: but I am persuaded it cannot be reckoned human, but seems to be a crocodile or porpoise. There are 16 vertebræ of the back and loins, very plain and distinct, with their processes and intermediate cartilages; 9 whole o- partial ribs of the left-side; the os sacrum, the ileum in situ, and two thighr bones displaced a little; the beginnings of the tibia and fibula of the right-leg; on one corner there seem to be the vestigia of a foot with 4 of the 5 toes, and a little way off an entire toe, now left perfect in the stone: there are no less than 11 joints of the tail, and the cartilages between them of a white colour distinguishable from the rest. We should impose on our senses, to question, whether these be the real relics of an animal; for the very bones themselves are now to be seen as plainly, as if preserved in an Egyptian mummy. The Royal Society had lately a draught of a crocodile, though a small one, found after the like manner inclosed in stone, from a quarry in the mountains of Upper Germany. I suppose the same reason accounts for both, and all the rest of this kind of fossils.

It is remarkable that all the stone-pits, about the same part of the country, abound with prodigious quantities of shells, and the like, and the greatest part of the substance of the stone is a composition of them. There are many accounts of them in the Transactions, and this stone has many shells of different kinds in it. Sir Hans Sloane has a fish-skeleton, found near this place. If we look on a map of the country, and observe the Lincolnshire Alps, how they run 50 miles north and south, and on the west side are steep and rocky, we may see the reason why these quarries should be so stocked with shells; for it

on the Gout; and among his theological productions, a sermon or two. But these publications, however respectable, would not alone have procured him much celebrity. His name is perpetuated by labours of a different kind, viz. by his antiquarian researches; the chief of which are Itinerarium Curiosum; an Account of Stonehenge and Abury; Palæographia Sacra; Palæographia Britannica; and the History of Carausius. Besides the above communication, there are various papers by Dr. S. on antiquities, on earthquakes, on a fire-ball, &c. inserted in the 35th, 45th, 46th, 47th, and 48th, vols. of the Phil. Trans.

Dr. S. was born at Holbeach, in Lincolnshire, in 1687, and died of the palsy in London, in 1765, aged 78.

is reasonable to suppose, that on the retiring of the waters of the deluge from the superficies of this country, into the eastern seas, these heavy bodies were intercepted by this cliff, which has retained such vast quantities of them ever since: while those that fell on common mold are mostly rotten, and now lost.

Sir Isaac Newton's doctrine of the attraction of the particles of matter, according to the quantity of its solidity, proximity, and surface, especially that it is infinitely greater in the point of contact, on which depends its cohesion and all the varieties of physical action, will easily direct us to a notion of petrifaction. We learn how a proper degree of heat or cold, moisture, motion, rest and time, promote this principle, from the common experiments of crystallization and freezing even before the fire, and in many chemical mixtures. Whence we may know how stone grows in quarries gradually, not by any fancied vegetation, though there is something like it in corals, but generally by apposition of parts, as is perceived in the floors of subterraneous grots and caverns. So that we have no reason to doubt, but what was clay, sand, or earth, 3000 years ago, may now be stone or marble, according as the above-mentioned causes occur. This will convince us that the now barren and rocky plains of the countries of Syria, India, and Arabia, are owing to natural causes, because the same is observable of the famous countries of Greece and Africa, warm regions, and so renowned for fertility in ancient authors. So that there may be some possibility in the opinion of those who think that in many ages the whole face of the globe may become one great rock. Dr. Plot, in his Natural History of Oxfordshire, gives an account of a tumulus, now a perfect mount of stone: and on St. Vincent's Rock, near Bristol, are fortifications now become solid cliffs. About 6 years since, I was shown many human bones taken from whole skeletons, with British beads, chains, iron rings, brass bits of bridles, and the like, which were dug up in a quarry at Blankney in Lincolnshire; which probably was plain mold when these old bodies of the Britons were interred; and since then I saw many human bones and armour, with Roman coins, fibulæ, &c. found in a stone pit in the park at Hunstanton in Norfolk, which were supposed to have been buried in the earth after a battle. Whence we may judge it a vulgar error, in the ruins of old castles and walls to admire the tenacity of the mortar, and to praise our ancestors for an art which we suppose now lost; when doubtless the strength of the cement is owing to length of time: and in future ages the same judgment may be formed of our modern buildings.

From all these instances, I infer the ancient state of these cliffs, where this skeleton was found, and shells are daily found, intimately mixed in the substance of the stone, to have formerly been of a softer consistence, capable of admitting them into its bowels, and immuring them as part of itself; and that earth which is now manageable by the plough, may possibly in time assume the same density, at least not far below the surface; for in this very cliff the upper strata are still clay, becoming the harder the deeper.

What animal this has been, for want of a natural history of skeletons, it is impossible to determine; but we generally find them to be amphibious or marine animals. Why such, rather than many others, should chance to be thus entombed, may be owing to their being able to live much longer than terrestrial animals in that collection of waters, even till they began to abate and fall away into their destined receptacles; so that while the bodies of the rest, soon perished, were corrupted, and their bones separated and dispersed much earlier; this skeleton, with others of the like kind, fell entire into the fissures of this bed of clay, which has since turned into stone, and afforded this noble monument and pregnant proof of that general inundation.

Observations on the Strata in the Coal-Mines of Mendip, in Somersetshire. By John Strachey, Esq. N° 360, p. 968.

Suppose fig. 1, pl. 11, to represent the section of a coal country, and to take in about 4 miles, from the north-west to south-east, which may be applied to the veins of coal as they lie at Faringdon-Gourny, and at Bishop-Sutton, near Stowy, in the parish of Chew-Magna in Somersetshire.

In searching for coal, they first look for the crop, which is really coal, though very friable and tender, and sometimes appears to the day, as they term it; or else for the cliff, which is a dark or blackish rock, and always keeps its regular course as the coal does, lying obliquely over it. For all coal lies shelving, like the tile of a house, not perpendicular nor horizontal, unless it be broken by a ridge, which is a parting of clay, stone, or rubble; as if the veins were disjointed and broken by some violent shock, so as to let in rubble &c. between them. The obliquity or pitch, as they term it, in all the works hereabout, is about 22 inches in a fathom; and when it rises to the land, it is called the crop, but in the north basseting. In the works near Stowy, and at Faringdon, it rises to the north-west, and pitches to the south-east; but the farther they work to the south-west, the pitch inclines to the south; and è contra, when they work towards the north-east. So likewise they observe as they work to the south-west, when they meet with a ridge, it causes the coal to trap up, that is, being cut off by the ridge, they find it over their heads, when they are through the ridge: but on the contrary, when they work through a ridge to the north-east, they say it traps down, that is, they find it under their feet.

Coal is generally dug in valleys or low grounds. The surface in these parts is mostly a red soil, which, under the first or second spit, degenerates into malm or loom, and often yields a rock of reddish firestone, till you come to 4, 5, and often to 12 or 14 fathom depth, when it changes gradually to a grey, then to a dark or blackish rock, which they call the coal clives. These always lie shelving and regular as the coal does. But in these parts they never meet with firestone over the coal, as at Newcastle and in Staffordshire. These clives vary much in hardness, in some places being little harder than malm or loom, in others so hard that they split them with gunpowder: so likewise they vary in colour, the top inclining to red or grey, but the nearer to coal the blacker they grow; and wherever they are met with, coal is sure to be found under them, though not always worth the digging.

The first or uppermost vein, at Sutton, is called the stinking vein. It is hard coal, fit for mechanic uses, but of a sulphureous smell. From $5\frac{1}{2}$ fathom, to 7 fathom under this, lies another vein, which from certain lumps of stone mixed with it, like a caput mortuum, not inflammable, called catshead, they call the cathead vein. About the same depth under this again, lies the three coal vein, so called, because it is divided into 3 different coals; between the first and second coal is a stone of a foot, in some places 2 feet thick; but the middle and third coal seem placed loose on each other, without being separated by a different matter. These 3 veins are sometimes worked in the same pit: but the next vein is generally wrought in a separate pit; for though it lies the like depth under the other, the cliff between them is hard and subject to water: I have therefore represented a pit sunk through the 3 upper veins at A, and another sunk on the three coal veins only at B. So if they sink on any of the lower veins, they go more to the north-west.

Next under the three coal veins is the peaw vein, so denominated because the coal is figured with eyes resembling a peacock's tail, gilt with gold, which bird, in this country dialect, is called a peaw. The cliff over this vein is variegated with cockle-shells and fern branches; and this is always an indication of this vein, which, as before hinted, is always searched for about 15 fathom to the north-west of the former.

Under this again, between 5 and 6 fathom, lies the Smiths' coal vein, about a yard thick; and near the same depth under that again, the shelly vein: and under that a vein of 10 inches thick, which being little valued, has not been wrought to any purpose.

Some say there is also another under the last, but that has not been proved within the memory of man. At Faringdon they have the same veins, which agree in all respects with those of Bishop-Sutton. But as Faringdon lies 4 miles south-east from Bishop-Sutton, so, in the regular course, they would lie a mile and $\frac{1}{3}$ deeper than those at Sutton. But as in fact they are dug near the same depth, there must be a trap, or several traps down, which in all must amount to that depth between these works. Between Faringdon and High-Littleton the same veins seem to retain their regular course; but at Littleton their undermost and deepest vein is the best coal, which at Faringdon proves small.

On the other hand, in the parish of Stanton-Drew, to the north-east of the coal-works at Sutton, about a mile distant, and in the true course with those at Sutton, the same veins are found again. But here they wind a little, and their course or drift runs almost north, and they dip to the east; which winding is attributed to ridges, which the workmen have met with on both sides, and have occasioned them to discontinue the work that way. At Stanton they have little of the red earth or malm on the surface, but come immediately to an iron-grit, or grey tile-stone, which is a forerunner of the coal-clives: in all other matters they agree with the works near Stowy.

In the same parish of Stanton-Drew, a little to the eastward, they have another coal-work, but the veins are in all respects different from the former. Their drift or course is to the 11 o'clock sun, as they term it; they pitch to the 5 o'clock morning, and rise to land; consequently to the 5 o'clock evening sun. There are several veins, but as yet only 3 are thought worth working. The uppermost, about 3 feet thick, is a small lime-coal. The next is about 3 fathom under it, and about $2\frac{1}{2}$ feet thick, fit for culinary uses: the undermost is about the like depth under the former, only 10 inches thick, but good hard coal.

At Clutton, about 2 miles from these latter, in the same drift, viz. almost to the south-east and by south, these last veins appear again. The surface here is red, and so continues to 10, and sometimes to 14 fathom, and in other respects they agree with the last-mentioned works at Stanton-Drew.

At Burnet, Queen-Charlton, and Brisleton, there are 4 veins, which pitch to the north nearly, and consequently the drift lies almost east and west. The surface is red land, generally to the depth of 4 or 5 fathom. The uppermost is from 3 to 6 feet thick at Brisleton, but less at Charlton and Burnet. The next, called pot-vein, is 6 fathom under the former, 18 inches thick, all hard coal. 3dly, The trench-vein, 7 fathom under the other, which is from $2\frac{1}{2}$ to 3 feet thick, all solid coal. 4thly, Rock-vein, always distinguished by a rock of paving-stone, called penant, lying over it, the rock sometimes 20 feet thick, or more, and therefore this vein is never wrought in the same pit with the former vein, but about 200 yards more to the south, or to land, as they term it. It is 7 fathom under the former.

The different veins of coal and earth, in the coal-works in these parts, all agree in the oblique situation of the veins; and every vein has its cliff or clives lying over it, in the same oblique manner. They all pitch or rise about 22 inches in a fathom, and almost all have the same strata of earth, malm, and rock over them, but differ in respect to their course or drift, as also in thickness, goodness, and use.

Now as coal is here generally dug in valleys, so the hills, which interfere between the several works, seem also to observe a regular course in the strata of stone and earth found in their bowels: for in these hills, we find on the summits a stony arable, mixed with a spongy yellowish earth and clay: under which are quarries of lyas, in several beds, to about 8 or 10 feet deep; and 6 feet under that, through yellowish loom, is a blue clay, inclinable to marle, which is about a yard thick: under this is another yard of whitish loom; and then a deep blue marle, soft, fat, and soapy, 6 feet thick; only at about 2 feet thick, it is parted by a marchasite about 6 inches thick. But I must defer the farther description of these, and some lead-mines, to another opportunity; only it is to be noted, that these beds of stone and marle, different from coal, lie all horizontal.

Some Instances of the very great and speedy Vegetation of Turnips. By the Rev. Dr. J. Theoph. Desaguliers, F. R. S. N° 360, p. 974.

At Sutton Coldfield in Warwickshire, a peaty ground near a pool, of which it was formerly a part, was sowed with turnip-seed on the 2d of July, 1702. In less than 3 days time the turnips were seen above ground. In 3 weeks the roots were as large as walnuts. In less than 5 weeks after the sowing, the gardener drew great quantities of turnips to sell, being as large as great apples. At the end of 6 weeks, viz. on the 12th of August, a large turnip was taken up, which, together with its top and long descending part of the root, weighed above 2 lb. 14 oz. At the same time also was weighed 1 oz. of the same sort of turnip-seed; and afterwards 1000 of the grains were counted singly out of the ounce so weighed, and the rest of the ounce was divided into heaps, as near as could be guessed, equal to the 1000 seeds first severed and laid together; and it was found that the whole ounce contained above 14600 single grains; which number multiplied by 46, the number of ounces that the turnip weighed, produces 671,600, viz. the number of single grains of seed required to equal the weight of the turnip. From whence may be gathered that, on supposition that the increase of the turnip was all along uniform and equal, from the time it was sowed, the grain of seed which it sprung from, weighing when it was sowed but $\frac{1}{14800}$ of an ounce, was increased in weight according to the following proportions, viz.

In 6 weeks time		671,600	times its own weight.
every	week	$111,933\frac{1}{3}$	
	day	$15,990\frac{1}{2}$	
	hour	$666\frac{1}{4}$	
	minute of an hour	11	

Some time after, another ounce of the same sort of seed was exactly weighed, and the grains were found to be in number 14673.

Another turnip of the same crop was taken up on the 21st of October, which weighed above $10\frac{1}{2}$ lb.; which unusual and truly wonderful bulk it acquired by increasing the weight of the seed it was raised from, 15 times in every minute of an hour, from the sowing to the drawing it. Besides, the gardener neglected to thin his turnips in due time, otherwise probably their growth had been more considerable.

At another time in two other sorts of turnip-seed, it was found by counting that 1 oz. of one sort contained 14702 grains; and 1 oz. of the other sort 14905 grains.

It is credibly reported, that of late years turnips have been pretty frequently found growing in several counties of this kingdom that have weighed above twice as much; one of which was seen at Birmingham about the year 1710.

An Account of Experiments made with Mons. Villette's Burning Concave, in June 1718. *By the Rev. Dr. J. Harris, F.R.S. and Dr. J. T. Desaguliers, F.R.S.* N° 360, p. 976.

This mirror is a concave, 47 inches wide, and ground to a sphere of 76 inches radius; so that its focus is about 38 inches distant from the vertex of the glass. The metal, of which it is made, is a mixture of copper, tin, and tin-glass, and its reflection has something of a yellow cast. The concave surface has scarcely any flaws, and those very small; but the convex side, which is also polished, has some holes in it.

Having held several bodies in the focus of this mirror, we observed what happened to them while exposed to this great heat; and with a half second pendulum noted the time in which any material change happened to them. The experiments were made from 9 till 12 in the morning, as follow:

N° 1. A red piece of a Roman patera, which began to melt in 3 seconds, was ready to drop in 100.—2. Another black piece melted at 4, and was ready to drop in 64 seconds.—3. Chalk taken out of an echinus spatagus filled with chalk only, fled away in 23 seconds.—4. A fossil shell calcined in 7 seconds, and did no more in 64.—5. A piece of Pompey's pillar at Alexandria was vitrified in the black part in 50 seconds, and in the white part in 54.—6. Copper ore, which had no metal in it visible, vitrified in 8 seconds.—7. Slag, or cinder of the ancient iron-work said to have been wrought by the Saxons, was ready to run in 29½ seconds.

Here the glass, growing hot, burned with much less force.

8. Iron-ore fled at first, but melted in 24 seconds.—9. Talk began to calcine at 40 seconds, and held in the focus 64.—10. Calculus humanus in 2 seconds was calcined, and only dropped off in 60.—11. An anonymous fish's tooth melted in 32½ seconds.—12. The asbestos seemed condensed a little in 28 seconds; but it was now something cloudy; Mons. Villette says, that the glass usually calcines it.—13. A golden marchasite broke in pieces, and began to melt in about 30 seconds.—14. A silver sixpence melted in 7½ seconds.—15. A King William's copper halfpenny melted in 20 seconds, and ran with a hole in it in 31.—16. A King George's halfpenny melted in 16 seconds, and ran in 34.—17. Tin melted in 3 seconds.—18. Cast iron in 16 seconds.—19. Slate melted in 3 seconds, and had a hole in 6.—20. Thin tile melted in 4 seconds, and had a hole and was vitrified through in 80.—21. Bone calcined in 4 seconds, and vitrified in 33.—22. An emerald was melted into a substance like a turquois stone.—23. A diamond, weighing 4 grains, lost $\frac{7}{8}$ of its weight.

An Account of the extraordinary Meteor seen all over England, on the 19th of March, 1718-9. By Edm. Halley, LL.D. and Sec. to the R. S. N° 360, p. 978.

This wonderful luminous meteor, which was seen in the heavens on the 19th of March, as it was matter of surprise and astonishment to the vulgar spectator, so it afforded no less subject of inquiry and entertainment to the speculative and curious in physical matters; some of its phænomena being exceedingly hard to account for, according to the notions hitherto received by our naturalists; such are its very great height thereof above the earth, the vast quantity of its matter, the extreme velocity with which it moved, and the prodigious explosions heard at so great a distance, whose sound, attended with a very sensible tremour of the subject air, was certainly propagated through a medium extremely rare, and next to a vacuum.

In N° 341 of these Transactions,* I have collected what I could find of such meteors, and since, turning over the Ephemerides of Kepler, I accidentally hit upon another, prior to all those there described, and which was seen all over

* See p. 99, &c. of this volume.

Germany, Nov. 7, O. S. 1623, and in Austria also was heard to burst with an explosion like thunder. Yet neither this, nor any of the other hitherto described, seem to come up in any circumstance to this late appearance; of which I am in hopes to give a satisfactory account, being enabled thereto by the numerous accounts communicated to the Royal Society from most parts of the kingdom. Some of the most perfect descriptions we have received are the following.

First, Our very worthy vice-president, Sir Hans Sloane, being abroad at that time, happened to have his eyes turned towards it, at its very first eruption; of which he gave the following account: that walking in the streets in London, at about a quarter after 8 at night, he was surprised to see a sudden great light, far exceeding that of the moon, which shone very bright. He turned to the westward, where the light was, which he apprehended at first to be artificial fire-works or rockets. The first place he observed it in was about the Pleiades northerly, whence it moved after the manner of a falling star, but more slowly, in a seeming direct line, descending a little beyond and below the stars in Orion's Belt, then in the S. W. The long stream appeared to be branched about the middle, and the meteor in its way turned pear-fashioned or tapering upwards. At the lower end it came at last to be larger and spherical, though it was not so large as the full moon. Its colour was whitish, with an eye of blue, of a most vivid dazzling lustre, which seemed in brightness very nearly to resemble, if not surpass that of the body of the sun in a clear day. This brightness obliged him to turn his eyes several times from it, as well when it was a stream as when it was pear-fashioned and a globe. It seemed to move in about half a minute, or less, about the length of 20°, and to go out about as much above the horizon. There was left behind it, where it had passed, a track of a cloudy or faint reddish yellow colour, such as red-hot iron or glowing coals have, which continued more than a minute, seemed to sparkle, and kept its place without falling. This track was interrupted, or had a chasm towards its upper end, at about two-thirds of its length. He did not hear any noise it made; but the place where the globe of light had been continued for some time after it was extinct, of the same reddish yellow colour with the stream, and at first some sparks seemed to issue from it, such as come from red-hot iron beaten on an anvil.

All the other accounts of the phenomenon, in London, agree in this, that the splendour was little inferior to that of the sun; that within doors the candles gave no manner of light, and in the streets not only all the stars disappeared, but the moon, then 9 days old, and high near the meridian, the sky being very clear, was so far effaced as to be scarcely seen, at least not to cast a shade, even where the beams of the meteor were intercepted by the houses; so that for some few seconds of time, in all respects it resembled perfect day.

The time when this happened was generally reckoned at a quarter past 8; but by the more accurate account of the Rev. Mr. Pound, who only saw the light, agreeing with what has been sent us from the Parisian observatory, it appears to have been at $8^h\ 8^m$ apparent time at London. And the sun being then in $9\frac{1}{2}°$ of Aries, the right ascension of the mid-heaven was $130° 45'$, by which the position of the sphere of fixed stars is given. Hence the lucida pleiadum will be found at that time to have been $25\frac{1}{4}°$ high, in an azimuth 6° to the northward of the west, and consequently the arch the meteor moved in, was inclined to the horizon with an angle of about 27°, having its node or intersection with it, nearly south-south-west, as will be more evident by what follows.

At Oxford, 5 minutes earlier, Mr. John Whiteside, R. S. Soc. Keeper of the Ashmolean Museum, and very skilful in both mathematical and physical matters, immediately after the extinction of the meteor, made haste out to see what it might be; and well considering the situation of the track it had left in the sky, found it to have passed about $1\frac{1}{2}°$ above the preceding shoulder of Orion, and about $3\frac{1}{2}°$ above the middle of his Belt, where there appeared a luminous nubecula of a reddish light, being a dilatation of the track, seeming to have been occasioned by some explosion there; and by what he could learn from those that saw it, it was thereabout that it broke out, and first began to efface the stars. Hence it proceeded as to sense in an arch of a great circle, and passing in the middle between the tail of Lepus, Bayer's θ and β in the fore-foot of Canis Major, it terminated about ξ in the breast of the same, nearly in 95° of right ascension, with 23° south declination, and at the place of its extinction there remained a large whitish nebula, much broader, and of a stronger light, than the rest of the track, which he took for a certain indication of a very great explosion made there. By computation it will be found that the angle this track made with the horizon of Oxford was nearly 40°, and its intersection due S.S.W.; and that the place of its extinction was about 9° above the horizon in the azimuth of 32° to the west.

At Worcester, Mr. Nicolas Fatio, a person greatly skilled in astronomical affairs, saw this meteor descend obliquely towards the south, making an angle with the horizon of about 65°, and intersecting it about S.S.W. $\frac{1}{2}$ S. The tract left all Orion and Canis Major to the westward, and divided the distance between Sirius and Procyon, so as to be almost twice as far from Procyon as

Sirius. The time here was 1 minute before 8, this city being about 9^m of time to the west of London, and consequently the right ascension of the mid-heaven $128\frac{1}{2}$ degrees.

Now the situation of the three cities London, Oxford, and Worcester, being nearly on the same W.N.W. point, on which the track of the meteor had its greatest altitude above the horizon, equal to the angle of its visible way; if we suppose it at London to have been 27° high, and at the same time at Worcester to be 65° high, in the plane of the vertical circle passing through London and Worcester; supposing likewise the distance between them to be 90 geographical miles, or one degree and half of an arch of a great circle of the earth, we shall, by an easy trigonometrical calculus, find the perpendicular height to have been 64 such miles; and the point over which it was then perpendicular to have been 30 such miles W.N.W. from Worcester; and the geographical mile to the English statute mile being nearly as 23 to 20, this height will be no less than $73\frac{1}{2}$ English miles; the place directly under it will be found to be about Presteign, on the confines of Hereford and Radnorshires. The Oxford observation nearly agrees in the same conclusion.

This altitude being added to the semidiameter of the earth as radius, becomes the secant of 11°: so that the meteor might be seen above the horizon, in all places not more than 220 leagues distant from it. Whence it will not be strange that it should be seen over all parts of the islands of Great Britain and Ireland, over all Holland, and the hither parts of Germany, France, and Spain, at one and the same instant of time.

This suggests a considerable use that might be made of these momentaneous phænomena, for determining the geographical longitudes of places. For if, in any two places, two observers, by help of pendulum clocks duly corrected by celestial observation, exactly note at what hour, minute, and second, such a meteor as this explodes and is extinguished, the difference of those times will be the difference of longitude of the two places, as is well known.

Having thus fixed one point in the line of its motion, let us now consider what course the meteor took from thence. And first at the town of Kirkby-Stephen, on the borders of Yorkshire and Westmoreland, in a meridian very little to the westward of Worcester, but about $2\frac{1}{2}^\circ$ more to the north, it was observed to break out as from a dusky cloud, directly under the moon, and from thence to descend, nearly in a perpendicular, almost to the horizon. Now the moon, being at that time in the 3d degree of Leo, was about half an hour past the meridian, and consequently much about a point to the west, or S. b. W.; and the situation of Presteign from Kirby-Steven being sufficiently near on the same point, it follows that the direction of the track of the meteor was according to the great circle passing over those two places.

And this is further confirmed by the observation of Sam. Cruwys, Esq. Reg. S. S. who at Tiverton, about 12 geographical miles nearly due north from Exeter, observed the first explosion of this meteor exactly in his zenith, as he was assured by applying his eye to the side of his door, which he took to be perpendicular, and looking upwards: and from thence he saw it descend to the southwards directly in the same azimuth, without declining either to the right or left. Hence it is plain, that the track likewise passed over this place, which by our best maps is found to lie in a line with Presteign and Kirby-Steven; so that we shall take it for granted that this was the very course it held.

On this supposition, that the first explosion, attended with the reddish nubecula, was directly over Tiverton, we have the Oxford observation to compare with it, in order to determine more nicely the perpendicular altitude there. At Oxford this nubecula was found to be $3\frac{1}{2}^{\circ}$ above the middle star of Orion's girdle, at 8^{h} 3^{m}, and was therefore $26\frac{1}{2}^{\circ}$ above the horizon; and the distance between Oxford and Tiverton being 1° $55'$, or 115 geographical miles, it will be, as the sine of 61° $35'$ is to the sine of 63° $30'$, so is the semidiameter of the earth, or $3437\frac{3}{4}$ such miles, to 3498 miles, the distance of the meteor from the centre of the earth; from which deducting the semidiameter, there remains $60\frac{1}{4}$ geographical miles, for the height of the meteor above Tiverton. And this is confirmed by the observation of the Rev. Mr. Wm. Derham, who at Windsor saw the aforesaid nubecula about 2° above the most southern of the seven stars in the shield of Orion; that is (the time being 8^{h} 6^{m}) in the altitude of $23\frac{1}{2}^{\circ}$: whence, the distance between Tiverton and Windsor being 150 measured miles, or 130 geographical, by a like proportion we shall find the same height of the meteor 60 such miles, wanting only one quarter. So that in a round number we may conclude it to have been just 60 geographic or 69 statute miles above the earth's surface. It is impossible to come at a precise determination of this matter; by reason of the coarseness and inaccuracy of the data, which were only the notes of persons under the surprise of the suddenness of the light, and no ways pretending to exactness; however, such as they are, they abundantly evince the height to have exceeded 60 English miles, though some would have it to be no more than 38 or 40.

I was unwilling to leave off, till I had pitched on some hypothesis that might subject the motion of this meteor to a calculus; that the curious might be able to compute its visible way, either in respect of the horizon, or among the fixed stars: this I found might be done with tolerable exactness, supposing that it moved in the arch of a circle concentric with the earth, and

60 geogr. miles without it; and that the point of the first explosion was over the lat. of 50° 40′, and 3° 40′ to the west of London; and that of the last extinction over lat. 47° 40′, with 4° 50′ west longitude; the time being fixed to 8 minutes past 8 at London. Hence it will be easy, by a trigonometrical process, to obtain the visible altitude and azimuth of the meteor at either of its explosions, as seen from any place whose longitude and latitude is known; and from the time given, the points in the sphere of stars answering to those azimuths and altitudes are readily deduced. And let those who contend for a much less height of this meteor, try if they can on such their supposition reconcile the several phænomena with one another, and with the observation of the Rev. Mr. Wm. Ella, rector of Rampton in Nottinghamshire, between Gainsborough and Redford, which for its exactness I must not omit. Here, at $8^h\ 5^m$, the meteor was seen to pass precisely in the middle between Sirius and the fore-foot of Canis Major, moving obliquely to the southward, in a line whose direction seemed to be from the middle between the two shoulders of Orion; the latitude of the place being nearly 53° 20′, and long. west from London 0° 45′. Let them try how they can account for its being seen 5° high at Aberdeen in Scotland, and near as much at Peterhead, half a degree more northerly: and then they will be better able to judge whether it did not exceed the reputed limits of our atmosphere. Lastly, if the apparent altitude of the meteor at Paris was not $5\frac{1}{2}$°, but 11°, on the W. b. N. point, when it must have been in its greatest lustre, there will be no pretence to bring it lower than I have made it, especially if it be allowed to have followed the track I have assigned it, over Presteign, Cardiff, Minehead, Tiverton, and Brest in Bretany.

Allowing this to have been the path it moved in, it would be easy to assign the real magnitude and velocity of this meteor, if the several accounts of its apparent diameter, and of the time of its passage from one of its explosions to the other, were consistent among themselves. But some of them making its visible appearance nearly equal to the sun's, which in the opinion of many it far exceeded, we may suppose with the least that, at the time when it first broke out over Tiverton, its diameter was half a degree; and its horizontal distance being 150 geogr. miles from London, and its altitude 60, the hypothenusal, or real distance from the eye, will be more than 160 such miles; to which radius the subtense of half a degree will be above an English mile and a half, being about 2800 yards nearly. After the same manner it is difficult to assign its due velocity, while some make it half, others less than a quarter, of a minute, in passing from its first explosion to its last extinction: but the distance it moved in that time being about 3°, or 180 geogr. miles, we may modestly compute it to have run above 300 such miles in a minute; which is a swiftness wholly incredible,* and such, that if a heavy body were projected horizontally with the same, it would not descend by its gravity to the earth, but would rather fly off, and move round its centre in a perpetual orbit like that of the moon.

Of several accidents that were reported to have attended its passage, some were the effect of pure fancy; such as the hearing* it hiss as it went along, as if it had been very near at hand: some imagined they felt the warmth of its beams; and others thought they were scalded* by it. But what is certain, and no way to be disputed, is the wonderful noise* that followed its explosion. All accounts from Devon and Cornwall, and the neighbouring counties, are unanimous, that there was heard there, as it were the report of a very great cannon, or rather of a broad-side,* at some distance, which was soon followed by a rattling noise, as if many small-arms had been promiscuously discharged. What was peculiar to this sound was, that it was attended with an uncommon tremour of the air, and every where in those counties, very sensibly shook the glass-windows and doors in the houses, and according to some, even the houses themselves, beyond the usual effect of cannon, though near; and Mr. Cruwys at Tiverton, on this occasion, lost a looking-glass, which being loose in its frame, fell out on the shock, and was broken. We do yet know the extent of this prodigious sound, which was heard, against the then easterly wind, in the neighbourhood of London; and by the learned Dr. Tabor, who distinctly heard it beyond Lewes in Sussex: so that I cannot help thinking, that such a meteor as this might have occasioned that famous ode of Horace: Parcus deorum cultor, &c.

——— Namque Diespiter
Igni corusco nubila dividens,
Plerumque; per purum tonantes
Egit equos volucremque currum,
Quo bruta tellus, &c. Concutitur.———

But whether the report heard near Lewes was of that explosion right over Devonshire, or rather of that latter and much greater at the extinction over Bretany, I shall not undertake to determine, till we have some further accounts from France, whence hitherto we have only had, that at Paris the time of the appearance was at 17 minutes past 8.

It remains to attempt something towards a solution of the uncommon phænomena of this meteor; and by comparing them with things more familiar to us, to show at least how they might possibly be effected. And first the unusual and continued heats of the last summer in these parts of the world, may

* All these circumstances appear to be neither incredible nor imaginary. See the notes in p. 108, &c. of this volume.

well be supposed to have excited an extraordinary quantity of vapour of all sorts; of which the aqueous and most others, soon condensed by cold, and wanting a certain degree of specific gravity in the air to buoy them up, ascend but to a small height, and are quickly returned in rain, dews, &c. whereas the inflammable sulphureous vapours, by an innate levity, have a sort of vis centrifuga, and not only have no need of the air to support them, but being agitated by heat, will ascend in vacuo Boileano, and sublime to the top of the receiver, when most other fumes fall instantly down, and lie like water at the bottom. By this we may comprehend how the matter of the meteor might have been raised from a large tract of the earth's surface, and ascend far above the reputed limits of the atmosphere; where, being disengaged from all other particles, by that principle of nature that congregates homogenia visible in so many instances, its atoms might in length of time coalesce and run together, as we see salts shoot in water, and gradually contracting themselves into a narrower compass, might lie like a train of gunpowder in the ether, till catching fire by some internal ferment, as we find the damps in mines frequently do, the flame would be communicated to its continued parts, and so run on like a train fired.

This may explain how it came to move with so inconceivable a velocity: for if a continued train of powder were no larger than a barrel, it is not easy to say how very fast the fire would fly along it; much less can we imagine the rapidity of the accension of these more inflammable vapours, lying in a train of so vast a thickness. If this were the case, as it is highly probable, it was not a globe of fire that ran along, but a successive kindling of new matter: and as some parts of the earth might emit these vapours more copiously than others, this train might in some parts thereof, be much denser and larger than in others, which might occasion several smaller explosions, as the fire ran along it, besides the great ones which were like the blowing up of magazines. Thus we may account for the rattling noise like small-arms, heard after the great bounce on the explosion over Tiverton; the continuance of which for some time, argues that its sound came from distances that increased.

What may be said to the propagation of the sound through a medium, according to the received theory of the air above 300000 times rarer than what we breath, and next to a vacuum, I must confess I know not. Hitherto we have concluded the air to be the vehicle of sound; and in our artificial vacuum we find it greatly diminished: but we have this only instance of the effect of an explosion of a mile or two diameter, the immensity of which may perhaps compensate for the extreme tenuity of the medium.*

* See the note on Dr. Halley's former article on such meteors, p. 100, &c. of this volume.

An Observation of the End of the Total Lunar Eclipse, on the 5th of March 1718, taken near the Cape of Good Hope; serving to determine its Longitude. By E. Halley, R. S. Sec. N° 361, p. 992.

It is now more than 30 years since I had a dispute with some of the French Geographers about the longitude of the Cape of Good Hope, said to have been determined by the religious missionaries sent to China in the year 1685. By an emersion of the first satellite of Jupiter, they determined that Cape to be $1^h\ 11^m$ or $17\frac{3}{4}°$ more easterly than Paris, that is 20° from London: which for the reasons I then gave, I concluded could not be more than 17°. See Phil. Trans. N° 185. Very lately I have met with an observation which I believe will determine the controversy in my favour: for I had accidentally a journal of an officer of the ship Emperor put into my hands, who in his return from India, on the 5th of March 1718, observed the end of a lunar eclipse, when the visible altitude of the moon's centre was 13° 25′, he being then in the lat. of 34° 23′ south, and as they found afterwards, just 180 leagues to the eastwards of Cape Bonne Esperance. By calculation I find that in that latitude the moon had that height at $7^h\ 17\frac{1}{2}^m$ P.M. and by comparing this eclipse with that we observed with great exactness on Feb. 11, 1682, which agrees perfectly well with our numbers, I conclude the middle of this to have been at London at $3^h\ 48^m$. To which adding $1^h\ 46^m$ for the semiduration, the end will be found to have been at London at $5^h\ 34^m$. The ship was therefore in a meridian 26° to the eastwards of London: but she was at that time 180 leagues to the eastwards of the Cape, which distance in that latitude gives 11° of longitude; this therefore being deducted from the longitude of the ship, leaves just 15°, or one hour, for the difference of meridians between London and the Cape. So that by this account the Cape is yet nearer our meridian than I had formerly made it, and near 6° nearer than M. de la Hire places it in his Tables.

On this occasion it may not be amiss to insert an observation or two I procured to be made at the Cape, by Mr. Alex. Brown, a Scotch gentleman, who went to reside in India on our company's account. He carried with him a very good brass quadrant, of above 2-foot radius, and at the Dutch settlement at Table Bay, having rectified his pendulum-clock by correspondent altitudes, on the 4th of August 1694. at $5^h\ 59^m$ manè, the distance of the bright limb of the moon from the right shoulder of Orion was observed to be 25° 3′. And the next morning Aug. 5, at $5^h\ 21^m\ 12^s$, the same limb was distant from Procyon 25° 57′, and at $5^h\ 36^m\ 48^s$ from the Lucida Arietis 58° 29′.

It were much to be wished that the moon had, either of these mornings, been accurately observed at Greenwich or Paris, or at some place in Europe, whose longitude from them is well known. But that failing us, I had recourse to the period of the lunar motions, which is performed in 18 years and 10 or 11 days, after which the errors of our lunar computations return very nearly the same; and I found among my own old observations, one that answered well with that of the 4th of August, viz. Anno 1676, July 23, 13^h 11^m 35^s, at Oxford, I observed the moon to apply to the star in Medio Collo Tauri, by Bayer marked A. The star at that time was distant from the southern and nearest cusp of the moon by the micrometer $20' 32''$, and at 13^h 17^m 15^s; when it seemed to immerge on the bright limb of the moon, it was distant from the northern cusp $23' 20''$; but this less certain by reason of the hazy air. The star at that time was in ♉ $28° 56'$, with $1° 13' 20''$ north lat. by which I found that our lunar tables, founded on Sir Isaac Newton's correct theory of her motion, gave her place at that time only 2 minutes too slow; which error being allowed on the 4th of August 1694, the result was, that 5^h 59^m at Cape Bonne Esperance, was at London 4^h 53^m; whence the difference of longitude $16\frac{1}{4}°$, is sufficiently near what we had before determined.*

Jacobi Keill, M.D. De Viribus Cordis Epistola. N° 361, p. 995.

A reply to Dr. Jurin's paper on the propulsive force of the heart; a translation of which is omitted for the reasons before assigned at p. 375 of this volume.

An Account of some Experiments relating to the Specific Gravity of Human Blood. By James Jurin, M.D. and F.R.S. N° 364, p. 1000.

It is well known, from the observations of Mr. Leuwenhoeck and others, that human blood consists of red globular particles, swimming in a pellucid lympha, or serum. Which two different substances, though of unequal specific gravities, yet so long as they continue to circulate in the veins and arteries, are prevented from separating by their motion and warmth. But when the blood comes to stagnate and cool in a porringer, the globular particles, uniting together by their attracive power, and sinking by their weight, which is greater than that of the serum, form the coagulum, or crassamentum, at the bottom, the serum swimming above it.

This always is the case, when the crassamentum is at liberty to subside: but it often happens that, either by its adhesion to the sides of the vessel, or by the bubbles of air, which the blood gathers on falling into the porringer,

* It is now known that the more accurate number is 18° 23'.

and which stick to its surface, the crassamentum is kept from sinking, and seems to float on the top of the serum.

These accidents seem to have given the first occasion to that opinion, which I think has been generally entertained by those who have written on this subject, namely, that the globular part of the blood is specifically lighter than the serum, in which it swims.

But what has so fully established this persuasion, is the authority of the late excellent Mr. Boyle, who, among the many valuable and curious experiments he has given in his natural history of human blood, has left the following ones on this subject; viz. the specific gravity of serum of human blood was found by weighing a piece of sealing wax first in serum, and afterwards in water, to be to the specific gravity of water, as 1024 to 1000. In a second experiment, which for greater accuracy was made with an instrument contrived on purpose, the specific gravity of serum was found to be to that of water, as 1194 to 1000. In a third experiment, made by the same instrument, and with serum from the blood of another person, its specific gravity appeared to be 1186. The medium between these last two experiments is 1190, which has since been universally received for the specific gravity of serum of human blood, the first experiment being declared by Mr. Boyle himself to be less exactly made than the others. The specific gravity of human blood was found by Mr. Boyle, to be to that of water, as 1040 to 1000; though on account of difficulties, by him mentioned, he was far from being satisfied with this experiment, and recommended the thing to further trials.

However, these experiments having hitherto past uncontroverted, and it appearing from them, that the specific gravity of serum was greater than that of blood, in the proportion of 1190 to 1040, or of 8 to 7 nearly; it followed, that the blood globules were specifically lighter than the serum, and that in a very great degree, considering the small proportion that the bulk of the crassamentum was found to bear to that of the serum, from other experiments.

Hence it was conjectured, that these globules were thin vesicles filled with an aërial substance: and this opinion seemed to receive a great confirmation, on its being observed, in viewing the circulation by a microscope, that a blood globule, in passing through a very narrow vessel, would change its shape from a globular to an oval form, and would recover its former figure, as soon as it was got through the narrow passage; which appearance seemed to be naturally accounted for from the elasticity of the included aura.

Upon this conjecture have been built a great many solutions of the phænomena observable in the animal economy, and its disorders; the authors of which have been led into this mistake by the natural consequence of a matter of fact, for the truth of which they had so great an authority. But that the globular part of the blood is specifically heavier than the serum, will appear from the following experiments.

Exper. 1. I have several times cut off a small part of the crassamentum, when by its adhesion to the sides of the porringer it has seemed to swim on the surface of the serum, and have put it into another vessel filled with serum: on which it has immediately sunk to the bottom.

Exper. 2. When the coagulum has been buoyed up in the serum by the bubbles of air adhering to its surface, I have separated a small part of it, where those bubbles have been thickest, and put it into a glass of serum, in which it has swum, as before. Then setting the glass upon the air-pump, those bubbles burst after one another, as the receiver was exhausting; and the air being again let into the receiver, the lump of crassamentum sunk to the bottom of the glass.

Exper. 3. I have often placed a drop of serum on a clean glass before a microscope, in which I had dissolved a very small quantity of blood, and observed, that when the glass was held in a perpendicular position, the blood-globules subsided to the bottom of the drop; and inverting the glass, the globules again descended through the serum to the bottom. I had the same success with a small quantity of serum and blood in a capillary tube. And the same thing has been long since observed by Mr. Leuwenhoeck.

These experiments undeniably prove, that the crassamentum, or globular part of the blood, is specifically heavier than the serum; and consequently it is by no means probable, that the blood globules are vesicles filled with air, or any other fluid lighter than serum. And that they are not filled with any sort of elastic fluid will appear from the following experiment.

Exper. 4. In a small quantity of serum of human blood, I dissolved so much blood, as that the globules might not lie too thick together, to hinder their being seen distinctly. Then having lodged a small drop of this liquor on the inside of a thin glass tube, I fitted the tube on to the air-pump, and placed a microscope by it, so that I could see the blood-globules through the tube. I then caused the tube to be exhausted, keeping my eye on the globules all the time, in order to observe whether they dilated themselves as the air was withdrawn; but could not perceive the least alteration, they appearing exactly of the same size in the vacuum, as they had done before. Whereas had they been filled with an elastic fluid, they would either have burst, or have been dilated to at least 70 or 80 times their former magnitude. The stop-cock being afterwards turned, and the air suffered to re-enter the tube, the blood-globules still retained the same size as in vacuo.

To prevent the objections, which may arise for want of experiments made in the same manner with Mr. Boyle's, as well as for the satisfaction of the curious, who may be desirous to know the true specific gravities of serum and blood, I shall proceed to demonstrate them by hydrostatical experiments.

Exper. 5.—November, having suffered a quantity of my own blood to stand about 24 hours in the porringer, and then drawing off the serum carefully with a small siphon into a convenient glass, I found, by the hydrostatical balance, its specific gravity to be to that of water, as 1029,8 to 1000.

Exper. 6.—February, I examined the serum from the blood of another person in the same manner, and found its specific gravity to be 1028,6.

Exper. 7, 8, 9.—In April, I procured three several quantities of serum from the blood of different persons. The first of these was of a deep colour, in clining something to red, and a little turbid: its specific gravity was 1029,7. The second was likewise a little turbid, and of a pale whitish colour: and its specific gravity was 1030,2. The third was perfectly clear, and of the colour of canary: and its specific gravity was 1030.

Though these five several experiments were all carefully made, and with a balance of great accuracy, yet for further satisfaction I make the following after another manner.

Exper. 10.—In Jan. I drew off all the serum from 5 or 6 several porringers containing the blood of different persons. This I found to be a little tinged with blood, which was occasioned by my being obliged to draw it off pretty near to the bottom of the porringers, in order to obtain a quantity sufficient for my purpose. For this reason I suffered it to stand about two days, in which time the globular part of the blood was entirely precipitated to the bottom, and the serum was become perfectly fine and transparent. I then drew it off with a siphon into a glass phial with a narrow neck, which I filled to a certain mark made in the neck for that purpose. I then placed the phial in a pair of nice scales, in which I had a counterpoise for the weight of the phial, and found that quantity of serum to weigh 2284¼ grains. Then pouring out the serum, I filled the phial with common water to the same mark, and found the weight of the water to be 2219 grains. From which it follows, that the specific gravity of this serum was 1029,4.

Exper. 11.—In July I procured a quantity of blood taken from the temporal artery, from which I drew off the serum the next day, and weighing it in the same manner, found its specific gravity to be 1028,8.

These experiments agree so nearly together, that the little difference between them may very well be attributed to that which is between the serum of different persons; or to the variations occasioned by heat and cold in the several seasons of the year in which they were made. So that from them we may safely determine the specific gravity of serum of human blood at a medium to be 1029,5, or in a round number 1030; from which the greatest variation in any of these experiments is little more than one in 1000; whereas the difference between Mr. Boyle's experiments and mine amounts to 160 in 1000.

Exper. 12.—In April, in order to find the specific gravity of human blood, which, by reason of its tenacity, and sudden alterations on standing, cannot be determined by the hydrostatical balance; I took a narrow-necked phial, and filled it to a mark, with blood poured immediately out of the porringer, as soon as the person was blooded. This being weighed, as I had done the serum before, its specific gravity was found to be 1051.

Exper. 13.—In August, having filled the same phial with the blood of another person, running immediately out of the vein through a funnel, its specific gravity was found to be 1053. Suffering this to stand till it was cold, I found the blood was sunk a small matter below the mark in the neck of the phial. This being filled up with the water, which in so small a quantity could make no sensible difference from blood, I found the specific gravity of cold blood to be 1055.

Exper. 14.—In August, the last experiment being repeated in the same manner, the specific gravity of cold blood was again found to be 1055.

Exper. 15.—In July, the arterial blood, from which the serum was afterwards drawn off for the 11th experiment, being weighed in the same manner, its specific gravity was 1052,5.

As this arterial blood and its serum, differ no more in specific gravity from venal blood and serum, than the several portions of these do from each other, it is plain, that the difference in this respect between arterial and venal blood, is wholly inconsiderable.

In the 13th experiment it was observed, that the blood altered its specific gravity on cooling from 1053 to 1055; from which we may infer, that if the blood used in the 12th experiment had been suffered to stand till it was cold, its specific gravity would have been 1053: therefore, taking a medium among the four last experiments, we may allow the specific gravity of cold human blood to be 1054.

The difference of 14 parts in 1000, between this and the specific gravity determined by Mr. Boyle, is easily accounted for, if we consider, that that gentleman did not make use of a vessel with a narrow neck, as plainly appears from the circumstances mentioned in his experiment; and consequently a small error in the height of the liquor would make a considerable alteration in the specific gravity.

Since therefore the specific gravity of human blood is 1054, and that of its serum 1030, it is plain, that blood is heavier than serum by about one part in 43. From which it follows, that the globular part of the blood is specifically heavier than the serum, since the globular part being separated from the blood, leaves the remainder, or the serum, specifically lighter than the entire mass.

But in order to determine the exact specific gravity of the blood globules, it is first necessary to know the proportion which the whole quantity of the crassamentum contained in blood bears to the serum. To this end Mr. Boyle has given two several observations of the weights of the crassamentum and serum, after they have separated one from another in the porringer. But besides the difficulty of making this experiment with any tolerable exactness, it is to be considered, that there is a great deal of serum contained in the interstices of the globules, that compose the crassamentum.

This difficulty however is in some measure answered by two other experiments, which Mr. Boyle made for this purpose, after the following manner. He put a quantity of the crassamentum, already separated from the serum, into an alembic, and distilled off the remaining serum to dryness, but without drawing off the oil, or volatile salt; after which he weighed the distilled liquor, and the dry mass left behind.

By comparing these experiments with the two former, it will be found that the entire weight of serum contained in blood, is nearly $\frac{13}{15}$ of the whole, and consequently the weight of the dried crassamentum is only $\frac{2}{15}$ths of the blood.

But for further satisfaction, an analysis was made at my desire with a large quantity of blood, amounting to 4lbs. 14 oz. by that ingenious and skilful chemist, Mr. John Brown. From this was obtained, with a very gentle heat, 2lbs. 14 oz. 6 dr. of a phlegmatic liquor, which had scarcely any thing of the fœtid scent usual in the distillation of animal substances; and its specific gravity was nearly the same with that of common water, being only 1000,8. This being mixed with a strong solution of alum, scarcely afforded any coagulum; but exhibited a considerable one on mixture with a solution of Roman vitriol.

The distillation being continued with the same heat, we had 7 oz more of phlegm considerably impregnated with volatile salt, as was manifest from the smell. The specific gravity of this was 1007; and having mixed it with tinctura martis optima, solution of alum, and of Roman vitriol, a large coagulum was precipitated. In distilling these, there were lost by evaporation, 2 oz. 2 dr.

The third portion of liquor, being raised with a stronger fire, amounted to 7 oz. 6 dr. This was reddish, and turbid, and so strongly charged with volatile salts, that it might very well deserve the name of spirit. Its specific gravity was 1080,1.

Besides these, we had 7 dr. of volatile salt, 1 oz. of oil, and 8 oz. 4 dr. of caput mortuum, which still retained some small remainder of the oil, as was manifest from its taking fire at the flame of a candle. In this latter part of the operation was lost 3 oz. 7 dr.

Upon making due allowance for the difference between the specific gravities of the 3 first portions of liquor and that of serum, as also for what was lost in the two several parts of the operation, which we may reasonably conclude to have been of a specific gravity nearly the same with that of the liquor drawn off, it will be found, that the quantity of serum contained in this mass of blood, was about $\frac{15}{17}$ of the whole weight, and consequently that the quantity of crassamentum was $\frac{2}{17}$ of the same weight.

If we calculate therefore on this supposition, that the weight of the globular part of the blood is $\frac{2}{17}$ of the whole, we shall find the specific gravity of a blood globule to be to that of water as 1277 to 1000. If we follow the proportion of $\frac{2}{15}$, which results from Mr. Boyle's experiments, the specific gravity of a blood globule will be 1242.

But this computation seems to be a great deal too large: for we cannot be certain that the whole quantity of aqueous liquor was raised from the serum of the blood. On the contrary, it is more than probable, that a considerable part of it was afforded by the blood globules themselves; especially in the latter part of the operation, when their texture must have been broken and dissolved by the strong fire that was used. To prove this, we need only consider the condition of the dried crassamentum, after the phlegm is drawn off, being now a hard and brittle substance: whereas the globules in their natural state are soft and yielding. For which reasons it may perhaps be more satisfactory, if we attempt to find the quantity of the globular part of the blood after another manner.

It appears therefore from Mr. Boyle's observations, that the quantity of serum, which may be poured off from the crassamentum, is about one half of the whole mass. The remaining crassamentum consists of the blood globules, and a quantity of serum filling up the interstices between them; which, if the globules keep their spherical form, may easily be found by the principles of common geometry, to be nearly one half of the bulk of the crassamentum: but if the globules by their pressure against each other change their figure, the quantity of serum will be something less.

If this quantity of serum, lying between the blood globules, be added to that poured off, it appears that the serum contained in blood, is about $\frac{3}{4}$ of the whole bulk, and consequently that the blood globules make about $\frac{1}{4}$ of the whole. From which we shall find the specific gravity of the blood globules, to be to that of water, as 1126 to 1000.

If we suppose the blood globules to make $\frac{1}{6}$, $\frac{1}{5}$, $\frac{1}{3}$, or $\frac{1}{2}$ of the whole bulk, their specific gravity will be respectively 1174, 1150, 1102, or 1078. So that on any of these suppositions, the specific gravity of the blood globules will be considerably greater than that of the serum, and consequently they cannot be supposed to be vesicles filled with an aërial substance.

It will therefore perhaps be asked, what do they really consist of? In order to come to a solution of this question, it may be proper to take notice, that blood is composed of phlegm, oil, volatile and fixed salts, and earth.* For as to the spirit, we consider it with Mr. Boyle, to consist of the phlegm and volatile salt united together. That the serum, on a chemical analysis, exhibits a great deal of the first of these, and the others in a very small quantity. That, on the contrary, the crassamentum yields much less phlegm, but the other principles much more copiously than the serum.

From which data we may safely conclude, that the crassamentum, or globular part of the blood, consists of some phlegm united with the oil and salts, and a small quantity of earth.

But what is the exact proportion of these several principles to each other; what alterations are produced in the body by a change of this proportion; how, and in what part these globules are formed; by what means they preserve their figure, without dissolving in the serum, or uniting with each other; what variations are made in their specific gravities by heat and cold; and what are the effects of those variations; are questions not very easy to be solved, and yet of so much importance to the animal economy, that it were greatly to be wished, we had a number of data sufficient to determine them.

P. S. Since this paper was sent to the press, I made the following experiments, which serving to confirm the method last used, for finding the specific gravity of the blood globules, it may not be improper to relate them.

In August, I took a lump of the crassamentum, and washed it gently in fair water, to free it from the loose globules, which precipitating out of the serum after the coagulum is formed, do not unite into one body with it. I then laid it on a spongy brown paper, to drain off the superfluous moisture. After which, weighing it first in air, and then in water, I found its specific gravity to be $1083\frac{1}{4}$. Another lump of the same crassamentum being weighed in the same manner, its specific gravity was 1082,9.

In September I found the specific gravity of another piece of crassamentum

* See note respecting the component parts of the blood at p. 685, vol. ii. of this Abridgment.

to be 1082,1. A second piece from the blood of a different person gave 1086,1. A third from the same person gave 1086,6.

Hence it follows that the specific gravity of the blood globules, is at least 1084, which is the medium among these five experiments.

But if we allow one half of the bulk of the crassamentum to consist of serum, filling up the spaces between the blood globules, we shall find their specific gravity to be 1138.

From this we must make a small abatement, because some part of the serum must have been squeezed out from between the globules, by their yielding to each other's pressure, when the lump of crassamentum lay upon the paper: and this will reduce their specific gravity sufficiently near to 1126, as we had before determined it.

An Account of the Sunk Island in the River Humber, some Years since recovered from the Sea. By John Chamberlayne, Esq. F. R. S. N° 361, p. 1014.

This island goes by the name of the Sunk Island, so called I suppose from the sinking marsh ground about it. As for its original, several old people here can remember when there appeared nothing of it, but a waste and barren sand; and that only at low-water, when for the space of a few hours it showed its head, and then was buried again till the next tide's retreat: thus it continued until the year 1666, when it began to maintain its ground against the attacks of the waves; about which time it began to be rescued wholly from future danger, by the care and industry of Colonel Gilby, who having a lease or gift of it from the crown, raised banks about the rising grounds of it, and so defending it from the encroachments of the water, it became firm and solid; and in a short time afforded good pasturage for sheep and other cattle.

This island is now about 9 miles in circumference, within the banks, which seem to render it impregnable against all future attacks of the sea, and is of a very fat and fertile soil, affords good grass, corn and hay, and is replenished with numerous flocks of sheep, which are of a larger size and finer wool than those in Holderness, from which it is divided by about 2 miles in water, and from Lincolnshire by about 4. It is stored with vast numbers of rabbits, which seem innumerable; their skins are counted the finest in England, and are of a dark mouse colour, shagged, and soft as silk. There are also cows and horses feeding constantly in the place, with great plenty of wild fowl. There are however only three families that live constantly upon the place.

The yearly income of the proprietor Mr. Gilby, amounts to about 800l. It lies nearer to the diocese of York, by at least 2 miles, than to that of Lincoln, being 2 miles south of Holderness, in the River Humber, and 4 miles north of Lincolnshire, &c.

A Way for Myopes to use Telescopes without Eye-glasses, By the Rev. J. T. Desaguliers, LL.D. and F. R. S. N° 361, p. 1017.

Myopes may use telescopes without eye-glasses; an object-glass alone being as useful to them as a combination of glasses, and sometimes more so. *Lemma* 1.—What is required of a telescope is to give large and distinct vision, that is, to make the object, as in Galilæo's telescope, or its image, as in the telescopes made up of convex lens, appear under a great angle, and to have all the rays of those pencils that enter the eye, meet in a point on the retina of the eye, on their respective axes.

Fig. 2, pl. xi, represents the combination of two convex lens for the astronomical or inverting telescopes; where the above-mentioned requisites are obtained. AB is the object, supposed at a vast distance from the objective lens LL, so that rays coming from the extremity A of the object, will fall on the lens LL, in the same manner as if they were parallel to their axis AX, and after passing the glass unite at a, where they project the image of the point A; from whence diverging, they fall on the eye-glass ll, and having passed through it, go on parallel to each other, and enter the cornea of a common eye E, which unites those parallel rays on its retina RRR at α, where the image of a is projected. The same may be said of the rays that come from B, and after their several refractions through the two glasses, and the coats and humours of the eye, meet on the retina at β, where they project the distinct image of the point b. The rays that come from all the points of the object AB, being affected after the same manner, give a distinct image of those points on the retina, and therefore the object appears distinct.

The object will also appear magnified in the same proportion as the angle lcl = to bMa, under which its image is seen, is greater than the angle ACB, under which the object AB would be seen by the naked eye; as is more at large demonstrated by dioptrical writers.

Lemma 2.—If parallel rays fall on the cornea of a myopes, or short-sighted person, they will unite in the eye before they come to the retina, and the farther from it the more convex the eye is; but if the rays which fall on the cornea diverge in proportion to the too great convexity of the eye, as from D, such rays will be so refracted by the coats and humours of the eye, as to meet in one point on the retina RR, see fig. 4 and 5. Where I have neglected the refraction of the rays passing out of the crystalline K into the vitreous humour V, as I do in the other cases.

This lemma is also demonstrated by dioptrical writers.

Lemma 3.—If two pencils of rays, all parallel to the axis, as a, fig. 6, fall on different parts of the cornea, at the greatest distance from each other that can be allowed for those rays to enter the pupil pp; after entering the aqueous humour, their axes will converge, and meet either in the vitreous or crystalline humour, according to the convexity of the cornea through which they passed, and diverge again before they come to the retina; the rays of each pencil converging on their respective axes, to the place where the said axes cross one another.

Demonstration.—The axes aca, $\alpha c \alpha$, falling obliquely on the cornea at c, c, and entering from air into the aqueous humour, will be refracted towards the perpendicular to k; where striking more directly on the crystalline, they will go on to α, a, on the retina RRRR, decassating at v within the vitreous humour. The other rays r, r, ϱ, ϱ, after their refraction in the aqueous humour, fall more obliquely on the crystalline, and therefore are refracted again so as to meet at v, where the axes also meet, and thence go on to the retina RRRR.

Lemma 4.—But if the axes of the above-mentioned pencils are parallel, the rays that accompany them diverging from a point so near the eye, that the divergence may be proportionable to the too great convexity of the eye; then only the axes will meet in the eye before they come to the retina, by lemma 3; but the other rays will not unite on their respective axes, till they come to the retina, by lemma 2.

Proposition.—I suppose the eye of the myopes so convex that he can see no farther than a common eye, with the eye-glass of a telescope before it: then the eye of the myopes, being in the place of the eye-glass, will receive the rays diverging from the several points of the image, projected by the object-glass in its focus, in such a manner, that after their several refractions they will meet in respective points on the retina; and the axes of the pencils which come from the extremities of the object, will, in the eye, make the angle BVA = to bca, fig. 7, under which the image ab is seen, by lemma 4. The cornea and aqueous humour here supply the place of the eye-glass, and the crystalline and vitreous humours that of a common eye; wherein R is the retina, v the vitreous humour, and kk the crystalline humour; and the image ba is supposed to be brought down from fig. 2, which represents the astronomic telescope; the too great convexity of the eye here being in the place of an eye-glass.

I have also found out a way for the presbytæ to make use of an object-glass, by placing their eye nearer the lens than its focus, by so much as their eye is flatter than a common eye, so as to make, as it were, the telescope of Galilæo;

the flat eye serving as a common eye armed with a concave lens. I have so fixed the telescope, as to make a presbyta read a small print at a great distance.

New and accurate Tables for readily computing the Eclipses of Jupiter's first Satellite, by Addition only. By the Rev. Mr. James Pound, F. R. S. N° 361, p. 1021.

The tables of this satellite are here omitted, being superseded by other more modern tables of greater accuracy, in the present treatises on astronomy.

Account of several Experiments and Observations on the Production of Silk Worms, and of their Silk in England. By Mr. Henry Barham, F. R. S. N° 362, p. 1036.

Experiments made in Chelsea Park, in the months of May, June, and July, 1719.

April 27, I received a small parcel of silk-worm's eggs from Languedoc.

May 6, early in the morning I found them hatched of themselves, the wind shifting in the night from east-northerly to the west-southerly, changing the air of a sudden to warm, two days before the change of the moon.

After feeding and managing them according to art, through the whole course of their four sicknesses, they were come to their state of perfection, being then as thick as a man's little finger, and from 4 to 5 inches long, of a yellowish colour, and when held against the light, they might be seen through as you may an egg, being of the same colour and consistence, filled with the matter that forms the silk. This is a certain sign that they will begin to spin in 24 hours, or less. They then forsake their food, being very voracious before, and hunt about for a convenient place to fix their first hold-fasts, for supporting the balls or cones they are to make, which they do in a very surprising and mathematical manner, by means of a gummy substance that ties all together; and when the loose furzy substance is taken off, and some of the silk is wound off, the remainder is so smooth and compact, shining like sattin, that they are used for artificial flowers, and esteemed the best of any thing yet known for that purpose. I weighed many hundreds of these silk-balls or cones, which I found to weigh from 35 to 40 grains, with their aurelias or crysalids within them.

June 27, they began to spin, having been hatched 7 weeks and 3 days; and in 4 or 5 days finished their laborious and curious work; but their balls were not fit to be removed till 8 or 10 days.

I found that an ounce of silk-balls would make about a drachm of fine silk. But to be more certain, I weighed out 12 lb. of silk-balls at 4 times, and told the balls in every 3 lb. as follows, viz.

The first 3 lb. contained 812 balls—the second 3 lb. contained 842 balls—the third 3 lb. contained 797 balls—the fourth 3 lb. contained 868. So that the whole 12 lb. weight contained 3319 balls.

Which when wound off, was found to yield and make 1 lb. and 1 oz. or 17 oz. of fine silk, and about 7 oz. of coarse refuse unwound, in all $1\frac{1}{2}$ lb. of Avoirdupois weight, or 2 lb. Troy; which is as great or greater making or yielding as in any part of the world, and the silk as fine. I showed it to a noted silk broker, who said it was Italian silk, not knowing it was made in England, and worth about 20 shillings per lb.

Now on this experiment finding that 3319 silk-balls would make 1 lb. and 1 oz. of fine silk. I was desirous to know what quantity of silk might be expected from the worms hatched from 1 oz. of eggs. For this I made use of the following method: by often weighing and telling I found that 100 eggs weighed but 1 grain; so that if 1 grain contains 100, a scruple must contain 2000, and a drachm 6000, and an ounce, at 8 drachms to the ounce, must contain 48000 eggs. Now if every egg hatch a worm, and every worm makes a silk-ball, there must be from 1 oz. 48000 silk-balls; and if 3319 balls will make 1 lb. and 1 oz. of fine silk, then 48000 silk-balls will make 15 lb. and 6 oz. of Avoirdupois weight, in fine silk, or 18 lb. and 8 oz. of Troy weight. And in the same proportion 1 lb. of silk-worms' eggs will produce worms sufficient to make above 180 lb. of silk. But allowing for casualties, and supposing but 12 lb. of fine silk made from the worms and their silk-balls, produced from 1 oz. of silk-worms' eggs; it will be found much to exceed most countries, according to Augustino Gallo's computation; for he says, that in the southern parts of France, viz. Languedoc and Provence, they make only 7 or 8 lb. of silk from silk-worms hatched from 1 oz. of eggs; and in Brescia in Italy, only 8, 9, or 10 lb. of silk from 1 oz; only in Calabria, where the silk-worms and their eggs are larger, they make 11 or 12 lb. of silk from 1 oz. of eggs.

Viro Celeberrimo, Richardo Mead, M. D. Coll. Med. Lond. et S. R. S. S. P. D. Jacobus Jurin, M. D. et R. S. S. N° 362, p. 1039.

Dr. Jurin's rejoinder to Dr. Keill, of which it seemed unnecessary to insert a translation, for the reasons before-mentioned at p. 375 of the present vol.

Methodus Differentialis Newtoniana illustrata. Authore Jacobo Stirling, è Coll. Balliol. Oxon.* N° 362, p. 1050.

This paper on Newton's Differential Method, by that ingenious mathematician, Mr. James Stirling, may be seen, more complete and enlarged, in the latter part of the author's treatise, "Methodus Differentialis, sive tractatus de Summatione et Interpolatione Serierum Infinitarum;" first published in the year 1730, and again in 1764; also an English translation of it, by a Mr. Francis Holliday, in 1749.

An Account of some Experiments to find how much the Resistance of the Air retards falling Bodies. By J. T. Desaguliers, LL.D et F. R. S. N° 362, p. 1071.

I took 12 balls, 6 of which were solid leaden globes of about 2 inches diameter; 3 hollow glass balls of about 5 inches diameter; and 3 light pasteboard hollow globes of about the same diameter; and having carried them to the upper gallery in the lantern, on the dome of St. Paul's church, I let them fall down by two at a time in the following manner:

First, a leaden ball and a glass ball; 2dly, a leaden ball and a glass ball; 3dly, a leaden ball and a glass ball. Then I let fall, in the same manner, the 3 other leaden balls, each with a pasteboard ball.

After that, having the leaden and pasteboard balls brought up again, I repeated the experiment twice more with a leaden and pasteboard ball; then I made the experiment twice more with a pasteboard ball alone, to see how long it would be in falling.

On the whole it appeared, that the leaden balls were a very little more than $4\frac{1}{2}$ seconds in falling; the two largest of the glass balls 6 seconds; and the pasteboard balls $6\frac{1}{2}$ seconds.

The height of the gallery, from whence the bodies fell, was 272 feet above the pavement of the church, then covered with boards, on which they fell.

The times of the falls were taken two ways above, viz. with a wheel chronometer, which measures a small part of time accurately, nearer than to a quarter of a second, made and contrived by Mr. George Graham, and with a half-second pendulum; and the differences of time between the fall of the leaden balls and the other balls were taken below, by the president, Martin Folkes,

* This very respectable mathematician was agent for the Scotch Mine Company, Leadhills. He died the 5th of December, 1770.

Esq. F. R. S. and another person, who all agreed in their observations of the time, which they made each with a half-second pendulum.

The following table shows the marks, weights, and diameters, of the several balls, in 3 columns.

Leaden balls.	Troy weight. lb.	oz.	dr.	Diam in in. & dec.	Pasteboard balls.	Troy weight. lb.	oz.	dr.	Diam. in in. & dec.	Glass balls.	Troy weight. lb.	oz.	dr.	Diam. in inc. & dec.
1 C	2	1	$\frac{1}{2}$	2.1	A	0	3	6	5.5	D	0	3	$13\frac{1}{2}$	3.9
2 C	1	11	4	1.99	B	0	1	14	5.1	E	0	5	$3\frac{1}{2}$	5.42
3 C	1	11	12	2.0	C	0	1	17	5.1	F	0	6	$0\frac{1}{2}$	5.55
4 C	1	11	12	2.0										
5 C	1	11	12	2.0										
6 C	1	10	0	1.98										

N. B. The polar and equatorial diameters of the glass balls being different, I have set down a mean diameter for each of them; the true diameters are thus, of D 4 and 3.8, of E 5.6 and 5.25, of F 5.7 and 5.4 inches.

By Galileo's theory, the lead, which was $4\frac{1}{2}^s$ in falling, must fall 4 feet the first $\frac{1}{2}^s$, or 16 feet the first second, which amounts to 324 feet in $4\frac{1}{2}^s$. But since the sound of the ball, as it struck the bottom, by which we reckoned our time, had 272 feet to move, we must abate a quarter of a second nearly, supposing sound to move 1 mile in $4\frac{1}{2}^s$, which will take away 35 feet, that the body must have fallen in the last quarter of a second, and reduce the number of feet to 289; so that the lead will have only fallen 17 feet short of the theory, which must be attributed to the resistance of the air.

The large glass ball in the 6 seconds of its fall, would in a vacuum go through 576 feet; but taking away the last quarter of a second, or 47 feet, for the motion of sound, it must only fall 529 feet in vacuo. Now since it fell only 272, there have been 257 feet taken off from the fall by the air's resistance.

Likewise the pasteboard ball, in $6\frac{1}{2}$ seconds, must have fallen 676 feet; but deducting the last quarter of a second, or 51 feet, for the motion of the sound, there remains only 625 feet for its fall in vacuo. But as it fell only 272 feet, we must allow a retardment of 353 feet for the resistance of the air.

At a medium we may call the weight of the glass ball 5 oz. Troy, and its diameter $5\frac{1}{2}$ inches; and the weight of the pasteboard ball 2 oz. Troy, and a little more than 5 inches diameter. All the lead balls fell within about a foot of each other, and made an impression in the boards of about one-third of their depth. The barometer stood at 30.1 inches, and the mercury was very convex, and therefore inclined to rise still.

A further Account of Experiments made for the same Purpose, viz. the Resistance of the Air. By the same. N° 362, p. 1075.

Having found by the former experiments that thin glass balls, and even balls of pasted paper, were too heavy to make so considerable a difference between the time of their fall and the fall of leaden balls, that it might be easily observed; I contrived a way to make dried hog's bladders perfectly round, by blowing them, when moist, within a strong spherical box of lignum vitæ, and letting them dry in the box before I took them out, which I did by opening the box that screwed in the middle, and had a hole in the pole of one of its hemispheres to let the bladder pass through, in order to tie it after blowing; and some few small holes all over the box, that in blowing no air might be confined between the inside of the box and the bladder, so as to hinder it from taking a spherical figure. Besides, I took off the ends of the ureters, the fat and a great deal of the upper coats of the bladders, before I blowed them in the box to render them still lighter.

The bladders I used were some of the thinnest I could find ready blown at a druggists, which I moistened in water, taking care to leave none in the inside. I chose those rather than fresh ones, which in drying would have stuck so fast to the inside of the box, that it would scarcely have been possible to have got them out without tearing.

Having prepared 5 bladders as above, I took them up to the upper gallery in the lantern on the top of the cupola in St. Paul's church, and there by a contrivance, which I shall describe, I let them fall by one at a time, together with a leaden ball of about 2 inches diameter, and weighing 2 lb. Troy; and I took notice of the time of the fall of each bladder, knowing by former experiments that the balls are about $4\frac{1}{4}$ seconds, or a little longer time, in falling the same height, which is 272 feet.

The following table, consisting of 5 columns, gives in the first the marks of the bladders; in the next their diameters; in the 3d their weights in grains Troy; in the 4th the times of their fall in second minutes of time; and in the 5th the difference of time between the falls of the leads and of each bladder, taken below by the president, Dr. Halley, Dr. Jurin, Martin Folkes, Esq. and Mr. George Graham, the clock-maker. The time was taken above with Mr. Graham's chronometer, and below with the same instrument, and three half-second pendulums, all which agreed very well together.

The experiments having been made twice over, the table is twice set down:

and those experiments in which the bladders fell straight down, and the most regularly, have this (*) mark before them.

Marks.	Diameters in Inches.	Weight in Grains Troy.	Time of the whole Fall.	Diff. between the Lead and Bladder.
A	5,3	128	$19\frac{3}{8}$s	$14\frac{2}{8}$ Seconds.
*B	5,193	156	$17\frac{1}{4}$	$12\frac{3}{4}$
C	5,33	$137\frac{1}{2}$	$18\frac{3}{4}$	$14\frac{4}{8}$
D	5,26	$97\frac{1}{2}$	$22\frac{1}{8}$	$17\frac{6}{8}$
*E	5,02	$99\frac{1}{8}$	$21\frac{5}{8}$	17
*A			19	$14\frac{1}{2}$
B			$18\frac{5}{8}$	$14\frac{1}{4}$
*C			$18\frac{3}{8}$	14
D			24	$19\frac{1}{8}$
E			$21\frac{1}{4}$	$16\frac{6}{8}$

A pail of water thrown down, met with such a resistance in falling 272 feet through the air, that it was all turned into drops like rain.

A Letter from Mr. Joseph Williamson, Watchmaker, asserting his Right to the curious and useful Invention of making Clocks to keep Time with the Sun's Apparent Motion. N° 363, p. 1080.

Having been informed of a French book lately published, wherein the author speaks of making clocks to agree with the sun's apparent motion; and supposes that it was a thing never thought of by any before himself: the following is a short account of what I have performed in that matter myself.

And in the first place I must take notice of the copy of a letter in this book, written by one P. Kresa a Jesuit, to one Mr. Williamson, clockmaker to his Imperial Majesty, of a clock found in the late king Charles II of Spain's cabinet, about the year 1699 or 1700; which shows both equal and apparent time according to the tables of equation; and which went 400 days without winding up. This I am well satisfied is a clock of my own making; for about 6 years before that time, I made one for Mr. Daniel Quare, which agrees with the description he gives of it, and went 400 days. This clock Mr. Quare sold, soon after it was made, to go to the said king Charles II of Spain: and it was made so, that if the pendulum was adjusted to the sun's mean motion, the hands would show equal time on two fixed circles, on one the hour, and on the other the minute. But there were two other moveable circles of the same kind, that moved forwards and backwards, as the time of the year required; on which the same hands showed apparent time likewise according to the equation table. This method the author asserts he knew of, and applied the same motion to pocket watches 12 or 14 years since; which I own I never did; being well satisfied that watches with springs and balances are very unfit to show the minute difference, as it increases and decreases, between equal and apparent time.

Soon after this clock was sent to Spain, I made others for Mr. Quare, which showed apparent time by lengthening and shortening the pendulum, in lifting it up and letting it down again, by a roller somewhat in the form of an ellipsis, through a slit in a piece of brass, which the spring at the top of the pendulum went through. By this means every vibration of the pendulum would agree to a second of time with the sun's apparent motion; that roller which lifted up the pendulum, and let it down again, being continually moving about all the year; so that it may seem very strange that this author never heard of it, so many years after they were made: for one of those, and not the first, made with the rising and setting of the sun, Mr. Quare sold to the late king William, and it was set up at Hampton-Court in his life-time, where it hath been ever since. This contrivance of lengthening and shortening the pendulum, I thought of several years before I made any of them. Since then, I have made others for Mr. Quare likewise, which showed the difference between equal and apparent time according to the equation tables, by a hand moving both ways from the top of a circle; on one side showing how much a clock keeping equal time ought to be faster than the sun, on the other side how much slower.

So that I think I may justly claim the greatest right to this contrivance of making clocks to go with apparent time; and I have never yet heard of any such clock sold in England, but what was of my own making, though I have made them so long.

An Account of some new Experiments, relating to the Action of Glass Tubes on Water and Quicksilver. By James Jurin, M. D. Reg. Soc. et Coll. Med. Lond. Soc. N° 363, p. 1083.

In the Phil. Trans. N° 355, I asserted, that the suspension of water in a capillary tube, was owing to the attraction of a small annular surface on the inside of the tube, which touched the upper part of the water. Among the several experiments made to prove this assertion, was that of a glass funnel of several inches diameter, having its small end drawn out into a very fine tube; which being inverted and filled with water, the whole quantity of water contained was sustained above the level by the attraction of that narrow annulus of glass, with which the upper surface of the water was in contact.

Soon after that discourse was printed, a book was published by a very learned and ingenious member of this society, in which that experiment was accounted for in the following manner.

If there be a funnel, as ABC, fig. 1, pl. 12, full of water, of which the wide end stands in a vessel of water BC, and the top of the funnel A ends in a capillary tube open at A; the whole water will be sustained: viz. the column Aa by the attraction of the circle of glass within the tube immediately above it; and all the rest of the columns of water, as Ff, Dd, Ee, Gg, &c. in some measure by the attraction of the parts of the glass above them, as F, D, E, G: and that the small columns or threads of water, Dd and Ee, do not slide down to Ff and Gg, and so go quite down, seems to be owing to their cohesion with the column Aa, which is sustained by the capillary tube A: for if you break off the said tube at DE, the whole water will presently sink down.

As this solution is very different from what I had before given, and the reputation of that gentleman, whose great knowledge in experimental philosophy is generally known, was sufficient to give weight to any of his opinions, I thought myself under an obligation to examine his account of the experiment, in order either to demonstrate its insufficiency, or to retract my own solution. Accordingly at the next meeting of the society, I produced the following experiment.

The funnel, AFGBC, fig. 2, whose lower part BCFG, was cylindrical to a considerable height, and the top drawn out into a fine tube at A, being filled with water to the height BF, so that the surface of the water FG did not reach to the arched part of the funnel, I touched the end A with a wetted finger, by which a small quantity of water being insinuated into the capillary tube at A, the water contained in the funnel was suspended above the level of the water in the cistern DE, as in the former experiment.

In this experiment it is manifest, that the little columns, into which we may suppose the cylinder of water, FGBC, to be divided, are no way sustained by the attraction of the arched part of the glass above them, since they have no contact with it. Nor is there any such middle column of water, which, by its contact with the tube at top, is both sustained itself, and helps to support the columns about it; on the supposition of which two particulars that gentleman's solution was founded.

This experiment may be thus accounted for. The cylinder of water FGBC, by its weight balances a part of the pressure of the atmosphere, which is incumbent on the water in the cistern, and endeavours to force that cylinder upwards. The rest of that pressure is balanced by the spring of the air, AFG, which is included between the cylinder of water FGBC, and the little column of water in the capillary A. But, as this air by its spring presses equally every way, it must balance as much of the pressure of the atmosphere on the small column of water at A, as it does of that on the water in the cistern. The remainder of the pressure of the atmosphere on the column of water at A, is sustained by the force with which that column adheres to the capillary tube, which therefore exactly balances the weight of the cylinder of water FGBC, and is the real, though not the immediate, cause of its suspension.

The experiment succeeds in the same manner when a column of quicksilver is raised into the funnel, instead of the column of water FGBC, the top of the tube being touched with a wet finger as before. But then the height of the quicksilver in the funnel must be as much less than that of the water, as its specific gravity is greater.

I proceed now to examine whether the experiments in N° 355 would succeed in vacuo; and whether water could be suspended in a wide tube by means of a capillary at top, at a greater height, than what it can be raised to by the pressure of the atmosphere.

In order to this, I boiled some water, and afterwards drew out its air by means of the air-pump; then those experiments all succeeded in the exhausted receiver in the same manner as in the open air. The 13th experiment in particular was made with a tube of about 35 inches in length, and a quarter of an inch diameter, the top of it being drawn out into a fine capillary. Which being filled with water freed of its air, as before mentioned, the whole quantity continued suspended in the exhausted receiver. This plainly shows, that the success of that experiment does not depend on the pressure of the air, since the small quantity of air left in the receiver was by no means capable of sustaining the water at so great a height; and consequently that the height, at which water may be suspended in this manner, is not limited by that pressure.

But here I must not omit taking notice of a considerable difficulty, which presents itself to those who attentively consider this experiment. In order to make which the better appear, it will be proper to observe what happens, when a simple capillary tube is filled with water freed of air, and inclosed in the exhausted receiver. In this case, the whole column of water contained in the tube ACB, fig. 3, is suspended by the attraction of the annulus at the top of the tube, A. And though that annulus does not immediately act on any part of the water, except what is either contiguous to it, or so near as to be within the sphere of its attraction, which extends but to a very small distance; yet it is impossible that any other part of the water, as for instance that at C, should part from the water above it and sink down, because its descent is opposed by the attraction of the contiguous annulus at C. For this, being equal to the upper annulus at A, is capable of sustaining a column of water of the length AB, and consequently is more than sufficient for supporting the column of water below it, CB. From which it is plain, that no part of the water contained in the tube can possibly descend, unless the upper part, assisted by the weight of the water below it, be sufficient to overcome the attraction of the annulus of glass at A.

But in such a compound tube as that made use of in our experiment, fig. 4, ACB, the case is very different; and it does not easily appear, why in a vacuum any part of the water in the wider part of the tube, as for example at c, should not leave that which is above it, and descend; since the annulus at c is by much too wide to sustain a column of water of so great a length as CB.

The best answer I can give to this difficulty is, that the cohesion between the water contained in the capillary and that below it, is sufficient to balance the weight of the column suspended. But how far this cohesion may depend on the pressure of a medium subtle enough to penetrate the receiver, is worthy of consideration. For though such a medium will pervade the pores of the water, as well as those of the glass, yet it will act with its entire pressure on all the solid particles, if I may so call them, of the surface of the water in the cistern, whereas so many of the solid particles of the water in the tube, as happen to lie directly under the solid particles of the water above them, will thereby be secured from this pressure; and consequently there will be a less pressure of this medium on any surface of the water in the tube below the capillary, than on an equal surface of the water in the cistern. So that the column of water suspended in the tube may be sustained by the difference between those two pressures. This explication seems to be favoured by the following experiments, which may all be accounted for in the same manner, though I shall soon mention another cause, which contributes to the success of the first and second.

The first is the famous experiment of the suspension of mercury, freed of air, to the height of 70 or 75 inches, in the Torricellian tube, in the open air: to which we may add the sustaining of mercury, likewise cleared of air, within the exhausted receiver, as related by Mons. Papin, in his Continuation du Digesteur. The next are the experiments made by M. Huygens, and described in Phil. Trans. N° 86, on the cohering of polished plates with a considerable force in the exhausted receiver; as also of the running of water and mercury, when cleared of air, through a siphon of unequal legs in the vacuum: all which he accounts for by the same principle, and much in the same manner as we have used for explaining the experiment above.

As to the existence of such a medium, I shall only refer to what has been said by our illustrious president in the queries at the latter end of the last edition of his Optics: and as I have lately exhibited before the society some experiments on quicksilver, which were exactly the reverse of those made by Dr. Taylor, the late Mr. Hauksbee, and myself, upon water; by which I am now enabled to throw this whole affair into a little system by itself; I shall lay it down in the following propositions, the proof of which is contained in the experiments annexed.

PROP. I. *The Particles of Water attract one another.*—This, I think, is now universally acknowledged, and therefore needs no demonstration; the sphericity of the drops of rain, and the running of two drops of water into one another on their contact, manifestly proving it.

PROP. 2. *The Particles of Quicksilver attract one another.*—This is likewise manifest from the spherical figure, into which a drop of mercury forms itself on a table; and from two of them immediately running together as soon as they come to touch.

PROP. 3. *Water is attracted by Glass.*—This plainly appears from all the experiments on this subject.

PROP. 4. *Quicksilver is attracted by Glass.*—*Exper.* 1. If a small globule of quicksilver be laid on a clean paper, and be touched with a piece of clean glass; on drawing the glass gently away, the quicksilver adheres to it, and is drawn along with it. And if the glass be lifted up from the paper, the quicksilver will be taken up by it, in the same manner as a piece of iron is drawn up by the loadstone, and will adhere to the glass by a plain surface of a considerable breadth, in proportion to the bulk of the drop, as manifestly appears by an ordinate microscope. Then if the glass be held a little obliquely, the drop of mercury will roll slowly on its axis along the under side of the glass, till it comes to the end, where it will be suspended as before.

Exper. 2. If a pretty large drop of mercury be laid on a paper, and two pieces of glass be made to touch it, one on each side; on drawing the glasses gently from each other, the drop of mercury will adhere to them both, and will be visibly drawn out from a globular to an oval shape; the longer axis passing through the middle of those surfaces in which the drop touches the glasses.

PROP. 5. *The Particles of Water are more strongly attracted by Glass, than by each other.*—This manifestly appears from the rising of water in small tubes above the level. For when the water begins to rise into a capillary tube, all the particles of water, which touch the small annulus at the bottom of the tube, must have quitted the contact of the other water, and have risen contrary to their gravity, to come into contact with the glass. After the same manner the other experiments of Dr. Taylor, Mr. Hauksbee, and myself, on this subject, are easily explicable. For on a careful examination, it will be found in them all, that some parts of the water quit the contact of the other water, and join themselves to the glass.

PROP. 6. *The Particles of Quicksilver are more strongly attracted by each other, than by Glass.* *Exper.* 1.—If a small tube as AB, fig. 5, open at both ends, be dipped into a glass vessel filled with mercury, and be held close to the side of the vessel, that the rise of the mercury within it may appear; the mercury will partly enter into the tube, but will stand within it at some depth, as CE, below the surface of the quicksilver in the vessel, CD; and this depth will always be reciprocally as the diameter of the tube.

In this experiment a column of quicksilver of the height CE endeavours to force the mercury higher into the tube; and as glass has been already proved to attract quicksilver, the attraction of the annular surface on the inside of the tube, which is contiguous to the upper part of the mercury, will also conspire to further its ascent. What opposes the ascent of the quicksilver, is the power by which that part of it, which endeavours to rise into the glass, is drawn back by the attraction of the other mercury, with which it is in contact laterally; and this not only balances the attraction of the glass, but also the weight of the column of mercury of the height CE, and consequently this attraction is considerably stronger than the attraction of the glass.

The cause therefore that suspends the weight of the column of mercury CE, being the difference between the attraction of the annular surface of the tube at E, and that of an equal surface of the quicksilver in the cistern, from which the mercury that endeavours to rise into the tube, must recede, in order to unite itself to such an annulus of the glass, will always be proportional to that annular surface, or to the diameter of the tube. And since the column sustained must be proportional to the cause that suspends it, that column must also be as the diameter of the tube. But the column suspended is as the square of the diameter of the tube and the height CE conjointly; from which it follows, that the height CE must be as the diameter of the tube reciprocally, as it is found to be by experiment.

The experiment of the ascent of water above the level in a capillary tube is just the reverse of this.

Exper. 2.—Quicksilver being poured into the inverted siphon ACB, fig. 6 having one of its legs AC narrower than the other CB; the height CE, at which the mercury stands in the wider leg CB, is greater than the height CD, at which it stands in the narrower leg CA. On the contrary, water stands higher in the narrower leg, than in the wider.

Exper. 3.—ABCD, fig. 7, represents a rectangular plane of glass, being one side of a wooden box. On the inside of this is another glass plane of the same size, which at the end AC is pressed close to the former, and opens to a small angle at the opposite end BD. When mercury is poured into this box to any height as CE, it insinuates itself between the two glass planes, and rising to different heights between the glasses where the opening is greater or less, it forms the common hyperbola CGF; one of whose asymptotes EF is the line on which the surface of the mercury in the box touches the inner glass; the other is the line AC, in which the planes are joined. This hyperbola being carefully examined by Mr. Hauksbee and myself, the rectangle EHG, wherever taken, proved always of an equal quantity, to as great an accuracy as could be expected, when the planes were opened to any considerable angle: but when the opening was very small, the inequalities of the planes, though the best I could procure, bearing a greater proportion than before to the distance between them, occasioned a sensible variation. Which I take to be the reason why the ordinates found by the late Mr. Hauksbee, in examining the curve produced in a contrary situation, on dipping two glass planes so joined into spirit of wine, do not answer to those of the hyperbola.

Exper. 4.—AB, fig. 8, is a perpendicular section through two glass planes joined at A, and opened to a small angle at B. C represents a pretty large drop of mercury, the larger the better, which, being made to descend as far as C, by holding the planes in an erect position, with the end A downwards, retires from the contact of the planes to D, on inclining the planes towards a horizontal situation; and the distance CD becomes greater or less, as the planes are more or less inclined towards the horizon.

A drop of any oily or watery liquor moves the contrary way; as has been shown by the late Mr. Hauksbee.

Exper. 5.—AB, fig. 9, is a tube open at both ends, and a foot or two in length, having its lower part drawn out into a fine capillary at B. This tube being filled with mercury, the whole column of quicksilver will be sustained in it, provided the capillary tube at B be sufficiently small. But if the mercury in the end B be suffered to touch any other mercury, it runs all out of the tube. If, without letting it touch any other mercury, a small part of the end B be broken off, the mercury will run out, till it comes to some lesser height as BC, at which it will again stop, the height BC being nearly in a reciprocal proportion to the diameter of the small end of the tube. The 7th experiment in N° 355 is the reverse of this.

Exper. 6.—Fig. 10 is the same in substance with the former, but made with a large glass funnel AB, instead of a tube. The reverse of this in water is the 13th experiment in the same N°.

In all these experiments it is easily seen, that the effect is owing to the difference between the two attractions, by which mercury tends to glass and to its own body; these being always opposed to each other; so that a particular explication is no way necessary. But perhaps it may save some little trouble to the reader, to remove the following objection, which will readily occur to him.

In the experiments brought to demonstrate the 4th proposition, the globule of mercury adheres to the glass in a plane surface, which cannot be done with out increasing the surface of the globule, and consequently removing some of its particles from the contact of each other. If therefore they tend more strongly to each other than to the glass, why do they not recede from the glass, and assume a figure perfectly spherical, that they may all have the greatest possible contact with each other?

To this we may answer, that the power by which mercury is attracted, either by glass or by other mercury, is proportional to the attracting surface; and therefore, though, cæteris paribus, the tendency of mercury to glass is not so strong as its tendency to other mercury, yet in this case a much greater number of mercurial particles coming into contact with the glass, than what recede from the contact of each other, it is no wonder that the attraction of the glass prevails, and causes the globule to adhere to it. For the number of mercurial particles which lose their contact with the other mercury, is no more than what makes up the difference of surface, which arises by changing the figure of the drop: whereas the particles which by this means come to adhere to the glass, are all those that constitute the plane surface in which the globule touches it.

Which consideration ought likewise to be applied to the suspension of quicksilver in glass tubes, either at extraordinary heights in the open air, or at lesser heights in a vacuum, as above-mentioned. For the top of the tube being spherical, or nearly so, it will be found that the contact of the mercury with the extremity of the tube, is to the contact with other mercury, which would be gained by its leaving the top of the tube and descending a very small space, in a ratio infinitely great; and consequently that the contact of the mercury with the top of the tube is one cause of its suspension.

Corol. 1.—From this proposition it appears, that in a barometer made with a narrow tube, the quicksilver will never stand at so great a height as in a wider. Which accounts for the phenomenon so often mentioned by Mons. De la Hire, in the History of the Royal Academy of Sciences at Paris, that in the barometer which he constantly made use of for his annual observations, the quicksilver did not rise so high, as in another he kept by him, by about 3 lines and a half, which is near a third of an inch our measure: for he tells us that the tube of his barometer is very small. So that there is no need to have recourse to any peculiarity, either in the quicksilver or the glass of which that tube was made; or to an unperceived remnant of air left in the tube, from some of which causes that effect and some others of the same kind were imagined to proceed.

Corol. 2.—In a barometer made with a small tube, the mercury will rise and fall irregularly. For, as the height of the mercury depends partly on the diameter of that part of the tube that touches the upper surface of the mercury, it is plain, that the unavoidable inequalities in the diameter of the tube will be more considerable, in respect to the whole diameter; and consequently will affect the height of the mercury more in a small tube than in a wider. And this I take to be the reason why it is so very difficult, not to say impossible, to make two barometers which shall exactly agree in the height of the quicksilver, in all constitutions of the air, especially if the tubes be very narrow. This irregularity is still more considerable in the pendent barometer, in which the quicksilver moves through a large space, in order to make a small alteration in the length of the column suspended: the same consideration is easily extended to those levels, that depend on the rising of mercury to the same height in the opposite legs of a bent tube; an instrument of which kind has been lately offered for the service of the public. And as the effect is just contrary in levels made with water or spirit of wine, due regard ought to be had to this property in the construction of those instruments, by making the tubes sufficiently wide, in order to diminish the error as much as possible.

An Account of a wonderful Fall of Water from a Spout, on the Moors in Lancashire. By Dr. Rd. Richardson. N° 363, p. 1097.

This remarkable spout fell on Emott-moor, near Coln in Lancashire, June 3, 1718, about 10 in the morning. Several persons who were digging peat near the place where this accident happened, on a sudden were so terrified with an unusual noise in the air, that they left their work and ran home, which was about a mile from the place: but to their great surprise they were intercepted by water; for a small brook in the way was risen above 6 feet perpendicular in a few minutes time, and had overflown the bridge. There was no rain at that time on Emott-moor, only a mist, which is very frequent on those high mountains in summer. There was a great darkness in the place where the water fell, without either thunder or lightning. The meadows at Wicolae were so much flooded, that the like had not been seen in several years before, though it was there a very bright day.

I went to view the place where the water fell; though I believed this inundation might proceed from an eruption of water out of the side of the mountain; such being not unfrequent, where lead or coal have been dug, but neither have ever been sought for here. On approaching the place, I was struck with unspeakable horror, the ground was torn up to the very rock, where the water fell, which was above 7 feet deep, and a deep gulph made for above half a mile, and vast heaps of earth cast up on each side of it, some pieces remaining yet above 20 feet over, and 6 or 7 feet thick. About 10 acres of ground were destroyed by this flood. The first breach, where the water fell, is about 60 feet over, and no appearance of any eruption, the ground being firm about it, and no cavity appearing. The ground on each side the gulph was so shaken, that large chasms appeared at above 30 feet distance, which a few days after I observed the shepherds were filling up, lest their sheep should fall into them.

An Account of the Phænomena of a very extraordinary Aurora Borealis, seen at London on Nov. 10, 1719, *both Morning and Evening. By Dr. Edmund Halley, R. S. Secr.* N° 363, p. 1099.

On Tuesday, Nov. 10, 1719, about 5 in the morning, I perceived certain white streaks in the sky, nearly perpendicular; which while I considered them, seemed instantly to vanish, and soon after others came as instantaneously in their room. I began to imagine that this was likely to be some part of the phænomena of the aurora borealis; only there appeared nothing like that luminous arch which we have of late so often seen in the north; till looking up towards the zenith, I perceived an entire canopy of such kind of white striæ, seeming to descend from a white circle of faint clouds about 7 or 8 degrees in diameter, which circle sometimes would vanish on a sudden, and as suddenly be renewed. I observed that the centre of this place of concourse was not precisely in the zenith, but rather 14° to the south of it; which I was well enabled to estimate by a star, which on each return showed itself about the centre of the circle. This star is the 33d of the Great Bear in Tycho's Catalogue, whose distance from the pole at this time is $52\frac{1}{2}$ degrees, and which about half an hour past 5 that morning passed the meridian; so that those rays centered very nearly on the meridian itself. It was a very entertaining sight, till such time as the day-break began to obscure these lights, which were but faint, though sufficiently distinguishable. None of them came lower than to about 30 or 40 degrees of altitude, and seemed not to have ascended from the horizon. The sky was perfectly serene and calm, which seems to be one of the concomitant circumstances attending the aurora borealis, of which this was certainly a species. For the night following a strange streaming of lights was seen in the air, which I attended from 9½ to 11, when a fog came on so thick, as to put an end to my prospect. But during that whole time there ascended out of the E. N. E. and N. E. a continued succession of whitish striæ, arising from below; and after changing as it were into a sort of luminous smoke, past over head with an incredible swiftness, not inferior to that of lightning; and as it passed, in some part of its passage, seemed as it were gilded, or rather as if the smoke had been strongly illuminated by a blaze of fire below. Some of the striæ would begin high in the air, and a whole set of them subordinate to each other, like organ pipes, would present themselves with more rapidity than if a curtain had been drawn from before them; some of which would die away where they first appeared, and others change into a luminous smoke, and pass on to the westwards with an immense swiftness. And I am of opinion, that had it not been for the moon, then 10 days old, and very bright, this for the time would have been reckoned as considerable an appearance as that of the 6th of March, 1716.

An Account of the same Appearance, seen at Cruwys Morchard in Devonshire. Being part of a Letter to Sam. Cruwys, Esq. R. S. S. and by him communicated to the Royal Society. N° 363, p. 1101.

A further Account of the same Appearance as seen at Dublin. N° 363, p. 1104.

An Account of another very considerable Aurora Borealis observed at Streatham in Surry. By Mr. Thomas Hearne. N° 363, p. 1107.

It is deemed quite sufficient to print only the titles of these last three accounts, as they contain nothing new, the appearances being the same as in Dr. Halley's account preceding, expressed only in words a little varied from his.

Nuperæ Observationes Astronomicæ cum Regia Societate communicatæ. N° 363, p. 1109.

The astronomical observations were made by the Rev. Mr. Pound. They describe two or three lunar eclipses; but they chiefly relate to observations of the near appulses of the planets to certain fixed stars, or distances from them And are not of sufficient importance to be reprinted on this occasion.

END OF VOLUME THIRTIETH OF THE ORIGINAL.

Some Remarks on a late Essay of Mr. Cassini, wherein he proposes to find, by Observation, the Parallax and Magnitude of Sirius. By Edmund Halley, LL.D. R.S.S. N° 364, p. 1. Anno 1720. *Vol. XXXI.*

In the Memoirs of the Royal Academy of Paris, for the year 1717, but lately published, there is a remarkable Essay, by Mr. Cassini, concerning the annual parallax of the fixed stars, and particularly of Sirius; in which he determines the diameter of Sirius to be as much larger than that of the sun, as the sun's is than that of the earth, which he supposes to be 100 times: and the distance from the sun to the earth being certainly about 100 diameters of the sun, it will follow, that the globe of Sirius must have its diameter equal to the distance between the earth and sun.

To prove this, he says he made use of an excellent telescope, of 34 French feet, or 36 English, leaving an aperture of only an inch and half, to take off the spurious rays of the star, which then appeared round, and sufficiently well defined; and comparing its body with that of Jupiter, which he says was then 50″ diameter, he found that the apparent diameter of Jupiter was 10 times greater than that of the star, which by consequence was seen under an angle of about 5″; which is his first position.

He then says, that to make the observations of the parallax of this star with all the exactness possible, he employed a telescope of 3 feet, in a copper tube, having fixed in the common focus of the two glasses, 4 threads crossing each other in the centre, under angles of 45°. This tube he firmly fixed to the plain of a mural arch, which had been for above 30 years immoveably cemented to the wall of the Royal Observatory, to which he chose to fix it, because of its solidity, and less liable to shake; and that after having stood 30 years, there was no fear of its settling any further in the space of one year; besides, that it was easy to perceive if any such alteration should happen to it.

Having therefore fixed his 3 feet tube as above, so that, about the beginning of April, 1714, New Stile, the star, being exactly in the meridian, passed over the centre of the tube, he observed that on the 20th of April the star touched the horizontal thread with its under edge, being apparently all above it, in the inverting tube, but really below. On May 15 and June 6, it passed again by the centre. On June 27 it appeared a little under, and on July 9 it was found to touch the under part of the thread. October 5 it again passed by the centre; but on December 29 it touched the upper part of the thread. January 18, 1715, being the coldest day of that winter, it passed exactly by the centre; and on March 27 and April 1, it almost touched the upper side of the horizontal thread, from which it seemed a little separated. But on June 7 it passed a little under the centre; and on June 29 the sun being then in conjunction with Sirius, it passed under the thread, so as to touch it with its upper edge. Whence it appears, that in the space of the whole year, there had been no other variation of the meridian altitude of Sirius, than the breadth of the thread, which appeared equal to the diameter of the star, which he takes to be 5, or at most 6 seconds.

Supposing this to be so, he then shows that the whole diameter of the annual orbit is to the distance of Sirius as the sine of 6″ to the sine of 39° 33′ the latitude of the star; whence the aforesaid immense magnitude of its body is a necessary consequence.

But before this obtain a full assent, it may not perhaps be amiss to inquire whether the supposed visible diameter of Sirius, were not an optic fallacy, occasioned by the great contraction of the aperture of the object glass: for we all know that the diameters of Aldebaran and Spica Virginis are so small, that when they happen to immerge on the dark limb of the moon, they are so far from losing their light gradually, as they must do were they of any sensible magnitude, that they vanish at once with their utmost lustre; and emerge likewise in a moment, not small at first, but at once appear with their full light, even though the emersion happen very near the cusp; where, if they were 4″ in diameter, they would be many seconds of time in getting entirely separated from the limb. But the contrary appears to all those that have observed the occultations of these bright stars. And though Sirius be larger than either of them, yet he is much less than two of them; and consequently his diameter to theirs is less than the square root of 2 to 1, or than 14 to 10; whence, in Mr. Cassini's excellent 36-feet glass, those stars ought to be about 4″ in diameter; and they would undoubtedly appear so, if viewed after the same manner; whereas we are aliundè certain, that they are less than one single second in diameter. The great strength of their native light, forming the resemblance of a body, when it is nothing else but the spissitude of their rays.

As to the other part of the argument, that the alteration in the declination of Sirius, on the score of the access of the earth in December, and its recess in June, amounts to 6″; I can only remark, that, besides that a radius of 3 feet, as it seems that made use of was no more, is somewhat too small for so extremely nice an observation, 6″ being subtended by the 1000th part of an inch, some of the observations before recited plainly show, that the refraction of the medium intermixed with those differences that might be occasioned by the parallax.

But the principal objection against the conclusion of this argument, seems to be, that the meridian altitude of Sirius at Paris being under 25 degrees, the ordinary refraction of the star is 1′ 55″, or 115″; and the barometer rising and falling above 2 inches in 30, shows that the density of the air, on that score, may be a 15th part more at one time than another. Whence the refractions being always proportional to the density of the medium, as we have all seen it often demonstrated by Mr. Hauksbee, both in vacuo, and in a doubly and trebly condensed air, it is plain that in that altitude the refraction of a star may differ about 7 or 8 seconds, or the 15th part of 115″, which is more than the whole parallax supposed to have been observed.

It were to be wished that Mr. Cassini would please to try this matter by the Lucida Lyræ, instead of Sirius, which, though somewhat less than him, is as near to the Solstitial Colure, and has much greater latitude, being only 28° from the pole of the ecliptic, whence its parallax would be so much greater; and being at Paris within 10° of the zenith, the grand objection of the difference of refraction would be almost wholly removed.

An Account of the external Maxillary, and other Salivary Glands: also of the Insertions of all the Lymphatics, both above and below the Subclavians, into the Veins. By Richard Hale, M.D. F.R.S. N° 364, p. 5.

The external maxillary glands in brutes are of the conglomerate kind. They lie externally, laterally (lengthwise) on the lower jaw, partly under the depressor labiorum, and partly under the buccinator. A strong membrane intervenes between these glands and the jaw on one side, and between them and the buccal glands on the other side. They are more or less red, like the pancreas, according to the quantity of blood that remains in them; otherwise their substance is white.

These glands receive arteries from the external carotids, veins from the external jugulars, and nerves from the third branch of the par quintum.

The number of excretory ducts from these glands is not always the same in the same species of animals. In cows generally 14 are discovered by the probe. Their orifices are valvular, about 4 times less than their ducts. Every duct is about half an inch from the next. Those in the middle of the glands are largest, because the glands are there broadest and thickest. The ducts do not communicate with each other, nor with the buccal. Every duct is formed of lesser ducts united, which rise from the lobules, through the whole substance of the glands, which constitute each distinct lobe, and has the same structure as the pancreatic duct. Each lobe is depressed on its sides, where it is joined to other lobes; and between the lobes many buccal glands are interspersed.

In calves, seldom more than 6 or 7 ducts admit any probe; when the animal grows older, the ducts appear more plain and open.

In sheep 6 excretory ducts are always found in each external maxillary gland.

In dogs and cats, &c. these ducts are fewer, in proportion to the smallness of the glands. It is observable, that these ducts in dogs open obliquely towards the mouth, by which the saliva may be better mixed with the food in mastication.

Dr. Wharton, cap. 21, first mentions the external maxillary glands. What he says of them is applicable only to their appearance in human beings, where they are of the conglobate kind, and very small, unless in scrophulous and venereal cases. It is plain that he had not seen them in brutes; for in his figures, which were drawn from brutes, no notice is taken of these glands. He describes them as very small, and calls them emunctories of the nerves, which was the notion in his time concerning the use of the conglobate glands; and the saliva was said è nervoso genere profundi.

Steno, (Obs. Anat. p. 14) justly blames Blasius for ascribing to the external maxillaries an excretory duct opening into the mouth, like the common one from the parotid gland. Yet Steno, otherwise very accurate, does not truly describe these glands, nor distinguish them from the buccal, though they are as distinct from the buccal, as the sublinguals are from the internal maxillaries. Steno divides his buccal into 3 parts. The large ducts in a line rise from the external maxillaries; and how distinct these glands are from the buccal appears plainly in fig. 17, &c.

The external maxillaries differ from the buccal in size, figure, structure, the particular number of ducts, in colour, &c. The buccal, labial, internal maxillary, and sublingual glands, are of a yellow colour; besides, the buccal are separated from the external maxillaries by a strong membrane. Indeed many of the excretory ducts of the buccal glands open near the ducts of the maxillaries, from whence Steno confounded these glands, but they do so round his own ducts from the parotids; and some ducts from like glands open near the sublinguals, as also about Nuck's ducts, in which places the buccal ducts are most numerous.

In short, there is a very great number of excretory ducts dispersed all over the membrane that invests the mouth, fauces, &c. which rise from glands that lie under this internal membrane. These glands are more numerous in some parts than others, and receive different names, according to the part they belong to, as labial, buccal, palatine, &c. But these are small glandules, with one excretory duct, and though they separate saliva like the large conglomerate glands, parotids, maxillaries, &c. yet they differ from these in constructure, one common excretory duct, &c. Whereas the external maxillaries differ from all the other glands of the mouth, viz. by many ways from the buccal, besides their colour; in which particular, they are also distinguished from the internal maxillary and sublingual glands; they differ also from these as well as from the parotids, in having a great number of common excretory ducts. This number of excretory ducts was not observed by Steno, nor did he know that these ducts in the same line, were the excretory ducts of large conglomerate glands, like the parotids, distinct from the buccal.

Bartholine, p. 542, mentions the external maxillary glands, but does not describe them. Nuck, Adenol. p. 5, n. 11, only gives them a place in his Catalogue of Glands, but takes no further notice of them, though he writes a book, Sialog. p. 15, 158, chiefly about a new salival duct rising from a gland found in no animal besides a dog.

Mr. Cowper had never seen these external maxillary glands, as appears by a letter of his, now by me, written above 20 years since, in answer to one I sent him on the first discovery of these glands. The external maxillaries in men, of the conglobate kind, are marked g, in the first figure of his Myotomia Reformata.

The ducts of the external maxillary glands are opposite to the orifices of Steno's ducts; from which glands and ducts, as also from the buccal, labial, and gingival glands, the saliva flows from all parts of the mouth without the teeth. From Wharton's and the sublingual ducts, from the tonsils, fauces, fretum Stenonis, gingival, lingual, and palatine glands, the saliva is derived from the upper and lower, former and hinder parts of the mouth within the teeth.

What has been said of these salivary glands, &c. will be best understood by the following figures, drawn in October 1697, at Trin. Coll. Oxon. by Mr. Burghers, and have been lately compared with the parts themselves in cows, calves, &c. These figures are part of many more taken from preparations at the same time.

The insertions of all the lymphatic vessels into the veins can be discovered only in few subjects; and no figure has yet been given of them.

These figures show the course of the lympha, both below and above the subclavian veins in men, and axillary veins in dogs. The lympha below the receptaculum chyli is conveyed from all the inferior parts by a great number of small lymphatic vessels, which uniting with others obliquely above the valves, become larger in proportion, till at length they constitute two large trunks near the emulgents, which are the pedunculi or beginnings of the receptaculum chyli. The lympha from the parts above the subclavian veins is derived in like manner from lesser lymphatics, to the common ducts here delineated.

I know Pecquet has given a figure of the thoracic duct in a dog, which is double from the receptacle and is inserted by four branches into each axillary vein. I believe with Bartholine, Barth. p. 616, 620, who has borrowed this figure from Pecquet, that such an insertion is a lusus naturæ. For though the thoracic duct may be double, and is sometimes divided into two parts near the subclavian veins, yet generally it is single, the lympha from all parts on both sides the body being carried by proper lymphæducts into one common thoracic duct, that conveys this liquor, together with the chyle from the lacteals, into the left subclavian vein, by one, three, or more branches. For there is as great a variety in the number of these branches as in the places of their insertion.

Mr. Cowper injected the thoracic duct in a human being, and gave a figure of that preparation in his book of Anatomy. But this figure is imperfect, and the insertion of the thoracic duct so ill drawn, that little can be learned by it. However, no anatomist has given any figure that shows the insertions of the lymphatics from both arms and both sides of the head, &c. above the subclavian veins, which appear so plain in the following figures.

Fig. 11, pl. 12, represents the passages or vessels, by which the chyle and lymph pass into the veins of a dog; 12, 12, are the lymphatics that bring lymph from the thighs and lower parts; 13, 13, lateral lymphatics arising from the groin, testicles, and neighbouring parts; 14, the receptacle of the chyle; 15, an indenture in the receptacle, through which passes one tendon of the diaphragm; 16, lymphatics from a neighbouring gland; 17 some lymphatics from the diaphragm; 18, an artery that serves the loins, and runs through a division of the receptacle; 19, the pancreas Asellii; 20, the vasa lactea 2di generis; 21, the beginning of the ductus thoracicus; 22, some divarications of the ductus; 23, the continuation of the duct, and its progress; 24, the aorta descendens.

N. B. The arteries at 18 and 24, by their pulsation, and the tendon at 15, much promote the ascent of the chyle and lymph.

Fig. 12, at 25, represents a common divarication of the duct; 26, a lymphatic from some neighbouring gland; 27, a double lymphatic from the secondary gland 42 in fig. 13; 28, that part of the thoracic duct, where both its branches, and the lymphatics from the left side of the head and left fore-leg meet; 29, the lymphatics from the left side of the head and left fore leg united, they lie on the inside of the vein; 30, a lymphatic with a pin in it from a neighbouring gland, perhaps the thymus; 31, a lymphatic from the neck, &c. it is divided and enters the jugular by two distinct branches under the sacculus 43; 32, the lymphatic from the right side of the head; 33, the lymphatic from the right fore leg; 34, the large sacculus, or receptacle of the lymph, on the right side, that receives all the lymph on that side, and conveys it into the jugular; 35, the descending cava; 36, the mammary vein, which is sometimes single; 37, the subclavian veins; 38, the vertebral vein; 39, the axillary veins; 40, the jugular veins; 41, the right internal jugular, not injected; 42, a small secondary lymphatic gland on the back part of the top of the thorax; 43, the sacculus, that receives all the chyle and lymph from the whole body, excepting 30, 31, 32, 33, 34, and discharges it into the vein; at least we know of no other lymphatics that any where else enter the veins; 44, a lymphatic, or membrane, for it was not injected, that joins 29 to the largest branch of the thoracic duct.

Fig. 13, represents the upper part of fig. 12 reversed, the duct, &c. being turned up, that the insertion, both sacculi, &c. may be better discovered. This figure is to be explained by the preceding, and has only from 42 to 44 more figures than the upper part of fig. 11.

N. B. In this subject, the chyle and lymph are emptied into the jugulars, and not into the axillary veins; they are sometimes emptied partly into the jugular, and partly into the axillary, or subclavian vein. In men, generally into the subclavian.

Fig. 14, represents part of the left cheek of an ox, separated from the lower jaw-bone, with the external maxillary glands, its ducts, &c. 1, 2, 3, &c. to 14, are bristles inserted into the ducts of the external maxillary gland lll; these ducts open sloping into the mouth, for the better mixture of the saliva with the food; 15, the duct 3 injected with wax, to discover its division and size, in respect of the orifice; 16, a lobule of the maxillary gland; its excretory duct is filled with wax, and ends at 15; 17, the duct 1 laid bare and opened to show its large cavity, &c.; AA, part of the muscles and fat, &c. belonging to the lower jaw; BB part of the internal membrane that invests the mouth; abcd, bristles in those ducts of the buccal glands, nn, that I could pass any into; eee, those orifices of the buccal glandules, that were too small to admit bristles; kkk, the papillæ on the inside of the mouth; lll, the lobes that constitute the external maxillary gland; mmm, the orifices of the labial glandules pp, that were too small for passing bristles; nnn, buccal glandules interspersed between the lobules of the maxillary gland; nnn near rrr, part of the buccal glandules, where they appear thickest, and are raised to discover the ducts rrr, running under them; ppp, the labial glandules like the buccal: Mr. Cowper, in fig. 4, letters them H, H; rrr, the ducts marked 6 to 14, as they appear under the glandules nn.

N. B. The same numbers and letters express the same things in the following figures.

Fig. 15, exhibits part of the left jaw-bone and cheek of a sheep, where the bristles 1, 2, 3, &c. show the constant number of excretory ducts from the external maxillary gland in these animals.

Fig. 16, shows part of the right cheek of a large dog, taken from the lower jaw-bone; f, the orifice of Steno's salival duct; g, the orifice of Nuck's duct, which rises as a papilla on the membrane BB; h, Nuck's new duct, not found in men, oxen, or sheep, but in dogs, their orbit not being entirely bony; i, Nuck's gland; ooo, the orifices of some excretory ducts, belonging to the external maxillary gland, that were too straight for the admission of bristles; qq, the teeth. In this subject they are the teeth of the upper jaw, near the scond of which, the orifice of Nuck's duct appears.

Fig. 17, shows the back part, next the cutis, of the external maxillary gland of the same dog, as it is beset with the buccal glandules.

Fig. 18, explains the external maxillary gland in the right cheek of a calf. In this subject I could only probe two ducts, 3, &c. would not admit bristles.

On the Plague at Constantinople. By Emanuel Timone, M. D. Communicated by the Author, who had practised Physic for many Years at Constantinople, to the British Envoy, Sir Robert Sutton; and with his Permission transmitted to the Royal Society by R. Hale, M. D. Translated and abridged from the Latin. N° 364, p. 14.

It is proved by historical documents, as well as by daily observation, that the plague is brought from Egypt to Constantinople. Here it is fostered and retained; and although this city is scarcely ever free from the seminia of a former pestilence, yet a new fomes of contagion is every now and then imported. It is for the most part suppressed by a severe degree of winter cold. Nevertheless, some few instances of infection occur even during the winter and spring; in summer the disorder increases, and in the autumn it rages with the utmost fury. The cool north-east winds (the Etesian winds) which set in at stated times during the summer have no effect in checking the contagion; which, on the contrary, is stopped in the summer season by the hot south winds, if they continue for some time. As for the symptoms of the plague, as it appears at Constantinople, they correspond exactly with those of the plague at Nimeguen in 1636 and 1637, as described by Diemerbroeck. It has, however, been observed, that here and there a horse, a dog, or a cat, has been seized with the bubonary pestilence, and has died. The common people, and especially the poorer sort, not only of the Turks but also of the Christians*

* Greeks? for it is afterwards mentioned that the Christians do use precautions.

and Jews, believing the plague to be sent as a punishment from heaven, take no measures whatever to avoid the contagion; precautions against which are only observed by the more civilized classes of society, and particularly the Christians.

The following are the symptoms as enumerated by the before-mentioned author, Diemerbroeck, cap. 7; namely, fever, buboes, carbuncles, exanthemata, head-ach, phrenitis, drowsiness in some, wakefulness in others, anxiety, debility or great prostration of strength, dull or muddy appearance of the eyes, (visus turbulentus,*) palpitation of the heart, dryness of the tongue, vomiting, hiccup, worms, diarrhœa, bleeding at the nose, menstruorum profluvium, bloody urine, spitting of blood, pains of the side, liver, kidneys, and other parts. To these I add a weariness and soreness of the limbs, shiverings sometimes followed by heat, but more frequently not, nausea without vomiting, vertigo, trembling of the hands from the very beginning of the disorder. Of these symptoms there is not one which is inseparable from the disorder, not even buboes, carbuncles, and exanthemata. In many instances there is no fever. Hence it may be established as a general rule, that whenever a disorder is accompanied with buboes, carbuncles, &c. we may with certainty pronounce it to be the plague; but that although such symptoms be wanting we cannot with certainty pronounce to the contrary. Thus many who are seized with the plague experience merely a slight shivering, not so much as in a common cold; and for several days none of the characteristic symptoms show themselves, but at length they burst forth all at once. Some after taking the infection only feel a degree of languor; they are capable of walking about, and going through their usual occupations without inconvenience; but on the third or fourth day they suddenly drop down, and die on the spot. And that they do not die of apoplexy is proved by the black spots which are found upon their bodies after death; to which add the circumstances of their having been previously exposed to the contagion, and of their having afterwards communicated the infection to others. On the other hand, many who have buboes and car-

* Visus turbidus, Diemerb. cap 7. The appearance of the eyes and countenance has been variously described by writers on the plague. According to Dr. Russel it somewhat resembles the dull fixed eye observable in the last stages of malignant fevers; but the dulness is different, muddiness and lustre being strangely blended together. It continues with little alteration in the remissions, and even where the patient appears sensible and composed. It does not increase in the febrile exacerbations, but the eyes then acquire a redness, that adds wildness to the look; which abating or going off in the remissions, the muddiness remains behind. It was this which chiefly contributed to that confusion of countenance which Dr. Russell has not attempted to describe, but which, he says, enabled him, after some practice, to pronounce with tolerable certainty, whether the disease was or was not the plague, though not independent of other symptoms. He adds, that when this muddiness disappeared or abated, it was constantly a favourable sign.

buncles upon them, walk about as if they had nothing the matter with them, and get well. The greater number, however, are affected with fever, and are extremely ill. Vomiting and diarrhœa, with sudden loss of strength, whether accompanied or not accompanied with fever, denote an attack of the plague; and the more so, if to these symptoms there be added a pain in the emunctories or glands. A red coloured pimple, of the size of half a vetch, and containing pus at its apex, is of a malignant nature, and soon enlarges into a livid carbuncle. These carbuncles appear indiscriminately on all parts of the body, not excepting the lips, tongue, bulbs of the eyes, glans penis, &c. Buboes appear only in the emunctories. Small hard glandular tumors about the neck are malignant. Exanthemata* are always fatal. It is a good sign if the buboes suppurate quickly. It is not a bad practice to open them with a lancet, when they do not come to perfect maturation. Many who have been cured by the dispersion of a bubo, feel for a year or two afterwards a dull pain in the part where they had the bubo, if they go to places infected with the plague. In some constitutions the plague remains dormant for several days, and then comes into action. If a person who is recovering from the plague commits any great error in diet before the 40th day, and a fresh bubo appears, he dies. It is very unusual for a person who has been perfectly recovered from an attack of the plague to have it a second time during the same year. A person who had lived in an infected house for some months without taking the plague, was at length seized with it. Old men, Diemerbroeck, cap. 4, for the most escape infection; young persons, on the contrary, are very liable to take it. Foreigners are more susceptible of it than the native inhabitants. Of all nations the Armenians are the least liable to infection. They eat very little animal food, but are much addicted to the use of onions, leeks, garlick, and wine. It is not safe to eat pork during the plague. Nothing predisposes more to the taking of infection than passions of the mind, and particularly grief and fear. Persons affected with the venereal disease, indifferenter se habent ad contagium;† but their buboes when they suppurate, degenerate into fistulous ulcers. Houses which are kept clean and neat are not so readily infected as those that are dirty. Cachectic

* Viz. petechiæ, maculæ nigræ, &c.

† Forestus, de Peste Delphensi, relates that persons affected with the venereal disease were exempt from the plague which raged at Delft in 1557. In the London plague of 1665 there were many, according to Hodges (Loimolog. sect. iv,) who, from a persuasion that the syphilitic virus would preserve them from the contagion of the plague, sought the most abandoned prostitutes purposely to give themselves the venereal infection; but, adds the last-mentioned author, the majority of them found no security in this abhorred practice; the pestilential contagion was soon superadded to the syphilitic poison; and their indiscretion only served to accelerate their death. The observations of Desgenettes, chief physician to the French army which invaded Egypt, coincide in this respect with those of Hodges.

subjects, and persons labouring under the jaundice and various other chronic disorders, either entirely escape infection, or if they take the plague, they have it favourably. On the contrary, it is particularly fatal to persons of a florid complexion and robust constitution.

Fumigations produced from juniper berries,* pitch, and sulphur, are useful, if they are continued day and night, so as to penetrate the whole house from top to bottom. Smelling to vinegar is thought to be of some service; it is very useful to anoint the nostrils with oil of amber. A moderate indulgence in wine, with cheerfulness and a good diet, are among the best preservatives. Bleeding is deemed improper, especially when resorted to late. In many instances it was found impossible to close the orifice made by the lancet, so as to stop the efflux of blood; and from the scarifications made in the operation of cupping, the blood has been observed to flow profusely for several hours, that is, even unto death. Gentle emetics do no good, and strong vomits do a great deal of harm. Purgatives are fatal. Sweating and alexipharmics are the only means of relief. The theriaca Veneta is very generally prescribed in this city. The lapis bezoar, although genuine, is of very little use. The Jews give acids, the Armenians and Greeks wine and brandy, the Christians rigidly abstain from animal food, including flesh-broths, for many days; nor is this diet despised by the Turks. Many, in whom a small tubercle, without any lividness, has made its appearance, by abstaining from animal food, have been able to walk about, like persons in health, for a week; but afterwards on taking to animal food again, have very soon died. Crude opium and all preparations of opium are useful, not only with a preservative but likewise with a curative intention. The most esteemed remedy among the Turkish grandees is the oleum naphthæ or pale-coloured petroleum (petroleum albicans) taken in the quantity of two drachms in any vehicle ad libitum. It agrees in its properties with camphor. A certain practitioner is said to have cured many by bleeding them on the first day of the disorder, even unto deliquium, and afterwards giving them a draught of the strongest vinegar, with a drachm of dragon's blood, and a drachm of Armenian bole dissolved therein. How bleeding produces its effects I leave to the consideration of others; but it is certainly a very excellent remedy.† It is useful to rub the skin with bruised garlick, on the breast and back nearest the

* The fumes produced from juniper berries, and other odoriferous substances, would seem to be useless, if not hurtful, on such occasions; hence certain acid vapours, and particularly those which are disengaged either from common salt or from nitre, in the manner described by Guyton Morveau, and Dr. Carmichael Smyth, are now universally employed in their stead.

† The author here speaks of bleeding on the first day of the attack, for he has before said that late bleeding is improper. The observations made by European physicians, attached to the forces sent out to Egypt, are not in favour of bleeding even in the beginning of this disorder.

situation of the heart, for several hours, until it becomes red and inflamed. It is also useful to apply a slice of hellebore root to the emunctories. Chickens and pigeons, either alive or cut into halves, have been applied with good effect to buboes and carbuncles, as well as to the region of the heart; they should be frequently renewed and not kept on the parts affected longer than half an hour. Oil of amber and extract of juniper have proved beneficial. Every part of Diemerbroeck's method of treatment is applicable to the plague at Constantinople. This author has certainly written well on the plague, if we except his laboured disquisition into the cause of this disorder. Barbette may also be consulted with advantage.

In 1712 the plague at Constantinople spread with increasing prevalency at the end of May,* and arrived at its height towards the end of July. A person whom I employed to make observations, counted above 90 dead bodies in one day. The Etesian winds blew strongly; afterwards the wind changed to the south. The first week after this change in the wind, viz. to the south, he counted only about 40 dead bodies per diem; the second week about 30; the third week not so many as 20; which last is not more than the ordinary number of daily deaths at that time of the year, in healthy seasons. Thus, in that year, 1712, the plague ceased in the autumn; whereas it generally rages with the greatest violence at that season, increasing and gathering strength in the middle, or towards the end of the summer. In other places, too, it has been observed that a plague which begins in the spring ceases in the summer. The plague, be it ever so violent, always ceases in Egypt after the summer solstice; and this in some measure holds good with regard to Smyrna, the isle of Chios, and even the Straits of the Hellespont. [The author then points out the different qualities of the Etesian and the south winds. The former, the Etesian or north-east winds, blowing across the Euxine sea, are, he says, not only loaded with moisture, but are moreover impregnated with nitrous particles, which he supposes to favour the spreading of the contagion; whereas the latter, the south winds, which blow from Egypt, are extremely rarefied and destitute of saline particles, and thus, he thinks, are suited to check the progress of the contagion.] After considering whether the blood be coagulated or dissolved in the plague, the author proceeds to state, that as the solids and fluids are variously affected according to the diversity of constitution, so as to give rise to various and even opposite symptoms, it follows that the plague is not to be cured by any one remedy; but that great judgment is required on the part of the physician to adapt the treatment to the diversities of constitutions and

* It began, as appears from the preceding part of the narrative, in the spring.

symptoms. That generally, however, the most cordial medicines should be exhibited as early as possible, and in large doses; that all evacuations, excepting that of perspiration, should be avoided; that, when they come on spontaneously, they should be checked by the remedies adapted to each; thus a diarrhœa has been stopped by an astringent clyster, to which some theriaca was added. That, where watchfulness takes place, the spirits should be calmed by the exhibition of opiates; that, on the other hand, where stupor and languor prevail, the spirits should be roused by volatile and camphorated medicines; that a due consistence of the blood should be preserved in the advanced stage of the disorder by the use of acids and the astringent earths; that the poison should be diverted to the surface of the body, &c.

An Account of a Luminous Appearance in the Air at Dublin. By Philip Percival, Esq. N° 364, p. 21.

Jan. 12, 1719-20, was observed an odd appearance in the sky about 10 o'clock. But it had nothing very remarkable till about half an hour after 11, when I was called out to see it, by the servants, who had been looking at it about half a quarter of an hour, and said it looked just like fire. But it appeared at first to me in long streams of light, of a round body, as at A, fig. 15, pl. 10, and very bright, though some were coloured, as at Aa. They came before the wind, which was then west, as near as I could guess, there not being a cloud in the sky, and the brightest moon I have known. We had rain about 5, but at 6 o'clock the night was clear. The streams of light, AA, moved very slowly, there being but little wind, but as they moved they joined, and, swelling out in the middle, formed themselves into the figure bbB, continuing to advance slowly in that shape for about a minute, when the two ends bb, approaching near each other, as described by the pricked lines, the advanced part B ran suddenly back, and joining itself with the ends bb, formed itself into the figure C, quivering in the upper part, and darting down perpendicularly in sharp points, as at DDD; and its colour, from a bright light, changed into the colours of a rainbow, but much fainter. It continued this way about a minute, and then the sharp points DDD, gathering themselves up into C, it changed again into a square sheet of light, as at E, and swelled out at F, as before at B; then advancing leisurely, it repeated the same scene as before, till it seemed at a great distance to disperse itself into small thin light clouds. I was very particular in observing it, and the next morning drew it, and I think very exactly. The beginning of it was very like the aurora borealis, which has been very frequent here this winter.

Of the Infinity of the Sphere of fixed Stars. By Edmund Halley, LL.D. F.R.S. N° 364, p. 22.

The system of the world, as it is now understood, is taken to occupy the whole abyss of space, and to be as such actually infinite; and the appearance of the sphere of fixed stars, still discovering smaller and smaller ones, as we apply better telescopes, seems to confirm this doctrine. And indeed, were the whole system finite, though never so extended, it would still occupy no part of the infinitum of space, which necessarily and evidently exists; whence the whole would be surrounded on all sides with an infinite inane, and the superficial stars would gravitate towards those near the centre, and with an accelerated motion run into them, and in process of time coalesce and unite with them into one. And, supposing time enough, this would be a necessary consequence. But if the whole be infinite, all the parts of it would be nearly in æquilibrio, and consequently each fixed star, being drawn by contrary powers, would keep its place; or move, till such time, as, from such an æquilibrium, it found its resting place; on which account, some perhaps may think the infinity of the sphere of fixed stars no very precarious postulate.

But to this I find two objections, which are rather of a metaphysical than physical nature. And first, this supposes, as its consequent, that the number of fixed stars is not only indefinite, but actually more than any finite number; which seems absurd in terminis, all number being composed of units, and no two points or centres being at a distance more than finite. But to this it may be answered, that by the same argument we may conclude against the possibility of eternal duration, because no number of days, or years, or ages, can compleat it. Another argument I have heard urged is, that if the number of fixed stars were more than finite, the whole superficies of their apparent sphere would be luminous, for that those shining bodies would be more in number than there are seconds of a degree in the area of the whole spherical surface, which I think cannot be denied. But if we suppose all the fixed stars to be as far from one another as the nearest of them is from the sun; that is, if we may suppose the sun to be one of them, at a greater distance their disks and light will be diminished in the proportion of squares, and the space to contain them will be increased in the same proportion; so that in each spherical surface the number of stars it might contain, will be as the biquadrate of their distances. Put then the distances immensely great, as we are well assured they cannot but be, and from thence by an obvious calculus, it will be found, that as the light of the fixed stars diminishes, the intervals between them decrease in a less proportion, the one being as the distances, and the other as their squares, reciprocally. Add to this, that the more remote stars, and those far short of the remotest, vanish even in the nicest telescopes, by reason of their extreme minuteness; so that, though it were true, that some such stars are in such a place, yet their beams, aided by any help yet known, are not sufficient to move our sense; after the same manner as a small telescopical fixed star is by no means perceivable to the naked eye.

Of the Number, Order, and Light of the fixed Stars. By the same. N° 364, p. 24.

At the last meeting of the society, I adventured to propose some arguments, that seemed to me to evince the infinity of the sphere of fixed stars, as occupying the whole abyss of space, or the τὸ πᾶν, which at present is generally understood to be necessarily infinite; and thence I laid down what may seem a very metaphysical paradox, viz. that the number of fixed stars must then be more than any finite number, and some of them more than at a finite distance from others. This seems to involve a contradiction; but it is not the only one that occurs to those who have undertaken freely to consider the nature of infinite, to which perhaps the very narrow limits of human capacity cannot attain.

Since then, I have attentively examined what might be the consequence of an hypothesis, that the sun being one of the fixed stars, all the rest were as far distant from one another, as they are from us; and by a due calculation I find, that there cannot, on that supposition, be more than 13 points in the surface of a sphere, as far distant from its centre, as they are from one another: and I believe it would be hard to find how to place 13 globes of equal magnitude, so as to touch one in the centre; for the 12 angles of the icosaedron are from one another very little more distant than from its centre; that is, the side of the triangular base of that solid is very little more than the semidiameter of the circumscribed sphere, it being to it nearly as 21 to 20; so that it is plain that somewhat more than 12 equal spheres may be posited about a middle one; but the spherical angles or inclinations of the planes of these figures being incommensurable with the 360 degrees of the circle, there will be several interstices left, between some of the 12, but not such as to receive in any part the 13th sphere.

Hence it is no very improbable conjecture, that the number of the fixed stars of the first magnitude is so small, because this superior appearance of light arises from their nearness; those that are less showing themselves so small by reason of their greater distance. Now there are in all only 16 fixed stars, in the whole number of them, that can indisputably be accounted of the first magnitude; of which 4 are Extra Zodiacum; viz. Capella, Arcturus, Lucida Lyræ, and Lucida Aquilæ, to the north; 4 in the way of the moon and planets, viz. Palilicium, Cor Leonis, Spica, and Cor Scorpii; and 5 to the southward, that are seen in England, viz. the foot and right shoulder of Orion, Sirius, Procyon, and Fomalhaut; and there are 3 more that never rise in our horizon, viz. Canopus, Acharnâr, and the foot of the Centaur. But that they exceed the number 13, may easily be accounted for from the different magnitudes that may be in the stars themselves; and perhaps some of them may be much nearer to one another than they are to us; this excess of number being found singly in the signs of Gemini and Cancer. And indeed within 45 degrees of longitude, or $\frac{1}{8}$ of the whole, there are no less than 5 of these 16 to be seen. If therefore the number of them be supposed 13, omitting niceties in a matter of such irregularity, at twice the distance from the sun there may be placed 4 times as many, or 52; which, with the same allowance, would nearly represent the number of the stars we find to be of the 2d magnitude: so 9×13, or 117, for those at 3 times the distance: and at 10 times the distance 100×13 or 1300 stars; which distance may perhaps diminish the light of any of the stars of the first magnitude to that of the 6th, it being only the 100th part of what, at their present distance, they appear with. But if, since we have room enough for it, we should suppose the sphere continued to 10 times the last, or 100 times the first distance, the number of stars would be 130,000, and they would appear but with the 10000th part of the light of a first magnitude star, as we now see it. This is so small a pulse of light, that it may well be questioned, whether the eye, assisted with any artificial help, can be made sensible of it. But 100 times the distance of a star we see, is still finite: from whence I leave those that please to consider it attentively to draw the conclusion.

An Account of the Method of making Sugar from the Juice of the Maple Tree in New England. By Paul Dudley, Esq. F. R. S. N° 364, p. 27.

Maple sugar is made of the juice of upland maple,* or maple trees that grow on the highlands. You box the tree, as it is called, i. e. make a hole with an axe, or chissel, into the side of the tree, within a foot of the ground; the box may hold about a pint, and therefore it must shelve inwards, or towards the bottom of the tree; the tree is also barked above the box, to direct the juice to the box.

* Acer saccharinum. Linn.

The tree is also tapped with a small gimblet below the box, so as to draw the liquor off. When we have pierced or tapped the tree, or box, we put in a reed, or pipe, or a bit of cedar scored with a channel, and place a bowl, tray, or small cask at the foot of the tree, to receive the liquor, and so tend the vessels as they are full.

The liquor is boiled in a pot, kettle, or copper. Ten gallons will make somewhat better than 1lb. of sugar. It becomes sugar by the thin part evaporating in the boiling, which is continued till it is as thick as treacle. Ten gallons must boil till it comes to a pint and half. A kettle of 20 gallons will be near 16 hours in boiling before it can be reduced to 3 pints: a good fire may do it sooner.

When taken off, it must be kept almost continually stirring, in order to make it sugar: otherwise it will candy as hard as a rock. Some put in a little beef suet, as large as a walnut, when taken off the fire, to make it turn the better to sugar, and to prevent its candying; but it will do without. A good large tree will yield 20 gallons. The season of the year is from the beginning of February to the beginning of April.

Mr. Dudley in a following letter says, that he has nothing to add to his chapter of maple sugar, but that the physicians esteem it not only as good for common use as the West India sugar, but to exceed all other for its medicinal virtue.

An Account of a Boy who lived a considerable Time without Food. By Patrick Blair, M. D. F. R. S. N° 364, p. 28.

This account, is of a boy, of 15 years of age, said to have lived 3 years without eating or drinking; during which time he had several severe fevers, with sometimes the loss of the use of his limbs, and one while of his speech. After the 3 years he gradually recovered tolerable health, excepting the use of one of his limbs, and taking extremely little food.

A Discourse concerning a Method of discovering the Virtues of Plants by their external Structure. By the same. N° 364, p. 30.

I cannot sufficiently admire the judiciousness and sagacity of the ancients, who, without any of those means used by the moderns, have handed down to us such an account of the virtues of those plants, which are more particularly useful in physic, that all the laborious endeavours of their inquisitive successors, have never been able to outdo them. It must have been a long course of experience, which enabled Dioscorides and Theophrastus to collect such a lasting catalogue of the virtues of plants, as scarcely any thing has been added to it even to this day. The Royal Academy at Paris has been at great pains to find out the virtues of plants by chemical analysis, and several other experiments; of which we have the abstracts in Tournefort's Histoire des Plantes aux environs de Paris, and Tauvry's Traité des Medicaments: but these laborious endeavours only serve to confirm what the ancients advanced, without any new discovery. For Tournefort, after having made the experiments with the tournesol and blue paper, and given an exact account of the several active chemical principles observed in different plants, usually concludes, ainsi il n'est pas surprenant s'il a de telles vertues, therefore it is not surprising if it is endowed with such virtues; which is nothing but giving a reason why the ancients believed they were good for such a distemper.

The means used by our ancestors to discover the virtues of plants, and their use in the several diseases, as they were the most simple, so they are most beneficial at this very time. It seems they narrowly considered their external appearance, and concluded, if such a plant partake of such virtues, such another so very like to it, must be endowed with the same: for example, apium and fæniculum have the same manner of flowering; both produce their seed after the same manner; their roots are both alike, being long, white, straight, carnous, &c. therefore since a long course of experience, delivered down by tradition, shows, that if such a plant has such virtues, such another like to it must have the same. Thus we find apium, fæniculum, petroselinum, all joined together, and prescribed as the opening roots in the dispensatory.

This induced that expert botanist and diligent inquirer into the Materia Medica, Dr. Herman,* to lay down these general maxims, quæcunque flore et semine conveniunt easdem possident virtutes: and omnia semina striata sunt carminativa.

The late ingenious and accurate naturalist, Mr. James Petiver, observes, that the plantæ umbelliferæ, galeatæ, verticillatæ, tetrapetalæ, siliquosæ and siliculosæ, have generally a tendency to the same virtue and use. And in a letter to me he observes, that the plantæ flore stamineo, which he calls blink flowers; such as hops, nettles, docks, sorrels, betes, blites, spinage, oraches, bonus henricus, or English mercury, and kali minus album, are all good sallads, raw, or boiled; as also the leguminosæ, or pea kind; such as pease, beans, phaseoli, are good nutritive food for men; and the tares, trefoils, medicæ, loti, and saintfoins, are good pabulum or fodder for beasts: to these he adds the frumentaceæ or cereales; as the wheat, rye, and oats, in Europe, and the maiz, millet, panic, and sorgum, in the Indies, make good bread; and that from barley and rice we have good fermented and spirituous liquors. To these

* Professor of botany and materia medica at Leyden.

he adds, that the iris, or flag-kind, in foreign parts, afford drugs, of no mean virtue and use; such as ginger, galingal, turmeric, zedoary, casumuniar, and cardamoms. The laurus, or bay-kind, has some noble attendants of the same tribe with itself; as cinnamon, cassia lignea, malabathrum, folium indicum, and the camphire tree.

In answer to his, I added, that all the pappescentes et lactescentes, such as the sonchus, dens leonis, hieracium, lactuca, cichoreum, endivia, tragopogon, and scorzonera, have the same virtues, and serve for the same uses, both in the kitchen and shops. All the asperifoliæ, such as borago, and buglossum, are those which are called coolers in a greater or less degree; for some are astringent, as consolida, others narcotic, as cynoglossum. All the galeatæ and labiatæ, for the most part consist of subtile particles, and are therefore cephalics; as lavendula, rosmarinus, majorana, &c.; mentha, pulegium, melissa, are hysterics. Attenuaters and inciders, as salvia, horminum, &c. A fourth sort somewhat astringent, as bugula, lamium, &c. So that by having an idea of the virtues of a majorana, mentha, salvia, lamium, we come to know the virtues of all of the same tribe. All the papavers are narcotic. The esulæ and tithymali are cathartic; though both these are lactescent, yet they differ from those which are pappescent also. All the malvæ are chiefly emollient; the pentaphyllous kind are astringent; as are also the plantains: the corymbiferous kind, are either stomachics, hysterics, or vermifuges. The gentian bitters, stomachics, hysterics, and febrifuges. The pomiferæ scandentes, as cucumbers, melons, &c. are coolers; but some are cathartic, as cucumis sylvestris, and colocynthis. The convolvuli, as mechoacanna, &c. are purgative; to which jalappa, both in flower and fruit, is near of kin. Digitalis and gratiola, are emetic and purgative. The squamous and bulbous roots are emollient, and more or less acrid. Thus allium, cepa, porrum, unboiled, are hot, diuretic, and lithontriptic. All the seda are coolers.

Thus at the first view, without knowing the characteristics so nicely as botanists do, but only exactly observing the facies externa of the plant, when the virtue of one species is known, the virtues of all of the same tribe may be guessed at, if not fully determined.

The next simple method of the ancients, to discover the virtues of plants, seems to have been the taste and smell. Thus, apium and petroselinum have a like taste; therefore they are to be prescribed together. The seeds of fænicu-lum and anisum have much the same taste and smell; and therefore both of them must be carminative, or expellers of wind, &c. They had likewise recourse to the temperament and qualities; such as hot and dry, cold and moist, in the 1st, 2d, 3d, and 4th degrees. But since the taste is not always the same in one person, and that different persons have different sensations; this, as being too much subjected to the different tempers and imaginations of people, is deservedly exploded.

I have lately composed a compendious scheme of all the plants used in physic; in which, that it might be less liable to objection, and not seem to introduce any innovation in the distribution, I have not so strictly observed the making their characteristic notes and virtues agree, as the distributing them according to their operations.

The first distribution, is by joining together all those which are prescribed under one title in the shops; such as the opening roots, emollient and capillary herbs, cordial flowers, hot and cold, greater and less seeds. In this I have not kept to the dispensatory catalogue; but have added several of the same tribe, that I might give a specimen of what is proposed concerning the virtues and characters. Thus, I have added cuminum and meum to fæniculum; laurus alexandrina and hippoglossum to ruscus; alcea to malva and althea; bonus henricus, atriplex, &c. to beta, under the title of oleraceous emollients; lingua cervina, polypodium, &c. to the capillary herbs; and so on in the cordial flowers, and in the hot and cold seeds.

I have, 2dly, distributed the plants into such as are altering and evacuating. The altering are divided into those that consist of gross, and such as are said to consist of tenuious and subtile particles. Those consisting of gross particles, are astringent. Such as prevent abortion and ruptures, stoppers of the fluxus menstruus immodicus, fluor albus, diarrhœa, dysentery; good in burnings, bruises, cancers, spitting of blood. Gross medicines are narcotics, vulnerary, good for scrophulous tumors, squinancy, and are coolers.

Plants consisting of subtile particles are aperient; such are all opthalmics, arthritics, nephritics, lithontriptics, diuretics, hydropics. They are also pectoral, anti-apoplectic, paralytic, hysteric, hypochondriac, promoters of the birth, febrifuges, scorbutics, stomachics, vermifuges.

The evacuating medicines are emetic, or such as work upward; or laxative and purgative, such as work downwards. The nutritive medicines are the plantæ cereales and leguminosæ.

It is here to be noted, that I have not inserted any plant in this table, but such as are natives of Britain, or such as are cultivated in British gardens; and to render it still more useful, I have added such particular parts as are used in the shops; viz. the root, herbs, leaves, tops, flowers, fruit, nuts, bark and wood.

Having thus reduced within a small compass the most considerable virtues of plants, both general and specific, and shown the most easy, simple, and natural method of discovering them, I would not be so far misunderstood, as if I were averse from using other experiments in finding them out. On the contrary, I could heartily recommend another method, hitherto much neglected, and which I am convinced would be of great use, if accurately gone about; and that is, their infusion in different liquors, in order to find out the proper menstruum for extracting their more useful parts.

Every physician is sensible that there are several simples, and these specific too, which adhibited in substance, are of great efficacy; whereas, if their contexture is dissolved, their parts can never be so reunited as to produce the same effect. Thus, cortex peruvianus is never so effectual, as when given in powder. That there are others which will communicate their useful particles, when infused, to one liquor, and not to another; and that the same substance will impregnate two liquors differently, according to the different menstruums. That expert chemist, Mr. Lemery, advises to infuse opium in water and spirits of wine, separately; and afterwards to mix both infusions together, in order to make the laudanum or extract; wisely considering, that the water will be impregnated by the more soluble saline particles, whereas the spirit will only imbibe the more resinous; for water is the proper menstruum for a saline substance, which will not dissolve in spirits of wine; this rather hardening and preserving it from being dissolved, either by air or water. Thus the most convenient way to preserve the volatile salts of animals, is to keep them in brandy; and every one knows, that water immediately dissolves sugar, which brandy will not do. Therefore senna will impart its purgative quality to water or ale, having its saline particles more disengaged; but the purgative virtue of jalap, consisting in its resin, requires wine or brandy for the menstruum or dissolvent.

Therefore, in my humble opinion, a most proper way to find out the virtues of plants, is to have recourse to the proper menstruums. A simple may be infused in rain-water, snow-water, or pure fountain water; if its texture is loose, and it abound with saline particles, those pure elements will be impregnated by it; but if the texture be more compact, firm and solid, if its particles are more fixed, then mineral waters are best; or by the addition of a proportional quantity of the fixed salt of a plant, a proper menstruum may be prepared. And next to the exhibiting of bitters in substance, such as wormwood, gentian, and camomile flowers, this is the most convenient way of administering them: not but their tincture extracted by brandy or wine may do very well: but since they abound very much with a fixed salt, a great deal of their virtue may be communicated to a less spirituous liquor, when a more spirituous will not extract it. The proper means to know which menstruum will best extract the more useful parts of any simple, or rather suspend its more solid particles, is to use the hydrostatical balance; when weighing the menstruum before infusion, and after the matter has been infused for some time, it will soon be observed by the augmentation of the weight, how far the menstruum is impregnated, and which is the most proper dissolvent. The proper method of exhibiting the fixed simples, if not in substance, is by decoction, infusion, or tincture. (N. B. It is called infusion, when the menstruum is either water, ale, or wine; but a tincture, when brandy is employed.) And the best way to obtain the useful particles of volatile, tenuious, or subtile substances, is by distillation. These may indeed be proper ingredients for an infusion or tincture. But there are a great many fixed substances as improper for distillation, as the volatile are improper for extracts. Thus I have shown the means of finding out the virtues of plants, without dissolving their texture: but if any have a mind rather to do it by the chemical analysis, this is not to dissuade them.

An Account of a Book, entitled Geometria Organica, sive Descriptio Linearum Curvarum Universalis. Auctore Colino Maclaurin, R.S.S. N° 364, p. 38.

The design of this treatise, is to examine the various methods proposed by mathematicians, for describing geometric curves; and at the same time to demonstrate a new one, far more general than any hitherto published; founded on those theorems proposed by our illustrious president, at the end of his enumeration of the lines of the third order.

The great improvements that have been made by most of the other modern geometricians, have related chiefly to the lines of the infinite order; they have been so fond of applying their new methods to mechanic and exponential curves, (which ought to give place to those that are more strictly geometrical) that they have neglected to cultivate geometry after the most regular manner. The writers on these subjects commonly rise at once, from considering the lines of the second order, or conic sections, to those of the infinite order, overlooking all the intermediate ranks. And hence it was, that all the orders of geometric curves lay unregarded, without the known limits of geometry, besides the first two, and a few of the superior curves that had been considered with some particular views, till that great author, by enumerating the lines of the third order, enlarged the bounds of geometry, and enriched it with almost 70 new curves. Their properties, which he has given, and the manner of describing such of them as have a punctum duplex, have almost brought them on a level with the lines of the second order; which alone had long usurped the place in geometry.

After this great example, it is attempted in this treatise, to give a universal description of all geometric lines of the third, or any order whatever. But as the higher kinds can be described only by means of the inferior sorts, some of these must be postulated to describe those: and as straight lines are the simplest and most easily described, and are always the same, that is, of one sort, therefore it was thought proper to investigate the use which they alone might be of, for describing lines of all the higher orders, in the first part of this treatise; an abstract of which has been published in the Transactions, N° 359. I shall only add, that besides the method of describing the curves, the manner of determining their asymptotes and species is also demonstrated; and the more simple curves of every order are particularly considered as examples of the method. In the first section, the lines of the second order are considered; in the second, those of the third order, that have a punctum duplex; in the 3d section, the lines of the fourth order, and those of the third order that have no punctum duplex. In the last section there are many various methods of describing the lines of any order.

In the second part, the curves of the inferior orders are used for describing those of the higher kinds. In the first section, the theorems published by Sir Isaac Newton at the end of the enumeration of the lines of the third order are demonstrated. In the second section, curves are substituted instead of straight lines, in all the propositions of the first part. From one of these propositions, lines of the 1024th order may be described by making angles move on 7 conic sections; and by 3 conic sections more, lines may be described above the 11,000th order. Lastly, these theorems are applied to show how the more complex of the infinite order may be described from the more simple.

In the third section, some other methods of describing curves are considered, that are not so general as the preceding, but give sometimes more simple methods of describing some few lines of the superior orders. Particularly the epicycloids described by the motion of any curve, whether geometric or not, on another equal to it, are easily constructed, and several infinite series of them rectified or measured by arches of more simple curves. In this section, several other descriptions of curves are treated of, that have been proposed by others. In the last section, to show the use of curves in natural philosophy, two of the most eminent problems in mathematical philosophy are solved. In the first, the centripetal force, by which a body describes any curve, is investigated after an easy manner; and a simple construction of all those curves which a body would describe, if projected with the velocity it might acquire by falling from an infinite height, in any hypothesis of gravity, is demonstrated. In the second, it is found, that if any body describe a curve in a resisting medium, the resistance is always as the moment or fluxion of a quantity, that expresses the ratio of the centripetal force, to that force by which it would describe the curve in vacuo, multiplied by the fluxion of the curve. It is also demonstrated, that if a body describe any curve in a resisting medium, which in vacuo could have been described by a centripetal force, proportional to any power of the distance, the density of that medium will be reciprocally as the part of the tangent intercepted between the point of contact, and a line perpendicular to the radius at the centre of the forces. This theorem is applied to several curves; and then the 10th Prop. of the 2d Book of the Principia, and all its examples, are demonstrated from it. These propositions are treated of here, not only because they show the use of curves in philosophy, but because more simple ideas of the de scriptions of some curves may be drawn from them, than from any other method; and because this is the method by which nature herself describes curve lines.

The whole is concluded with an attempt to draw a line of any given order, through any given number of points, that is sufficient to determine the curve. Thus if a curve of the order 2m is to be described through as many points, as determine a line of the order m, and 3 more points, each of which are nodes, formed by the concourse of as many arches of the curve, as there are units in m; then the curve is determined, and a method how to describe it is demonstrated. This, and some other theorems relating to the number of points that determine curves, and the manner of describing them through these points, conclude this part.

A Case in Surgery, which is commonly mistaken for a Fracture of the Patella. By Mr. Deverel, Surgeon at Bristol. N° 365, p. 44.

One Richard Burt was thrown from his horse, and in the fall received such a hurt in one of his knees, as made him incapable of remounting: he felt something crack in that knee, as he expressed it, before it touched the ground. On examining the part, I found, as I then thought, the ends of the fractured bone drawn above 4 fingers distance from each other: but on a stricter examination of the parts, I found the patella, which was drawn upwards by the extensors of the leg, retained its natural figure, and that the hardness which was felt below, was the end of the torn ligament that ties it to the tibia. The ends of the ligament were brought as near as possible, and kept so about 3 weeks without any very remarkable accident. He then began to walk, which was a little too soon, causing some pain, and loosening the cicatrix, which made it the longer before it was perfectly firm; however he now walks without any perceptible lameness. I have met with two others in the same case; the one does not walk so well as she used, though not for want of all the care and circumspection imaginable; for it is hardly to be expected that 1 in 10, to whom this accident happens, should ever go right, it being next to an impossibility that the ends of the torn ligament should be so exactly placed and retained as not to lie over each other.

On the Antiquity of the Venereal Disease. By Mr. William Becket, Surgeon, F.R.S. N° 365, p. 47.

My principal design in this letter, is to prove that the venereal disease was frequently known among us some centuries before the siege of Naples; but I shall first endeavour to refute the opinion of those persons, who believe it to have had its rise there, if any such remain of those who have read my former letter.* It is true that several modern authors have asserted it; but I shall make it appear to be an error as inconsiderately, and hastily received, as started by some chimerical author; who, because several writers about that time, observing the disease to begin in the pudenda, separated it from another, with which it was before confounded, must likewise take upon him to assert its being a new distemper, and to assign a certain time and place for its rise. Now one might with good reason expect, that if the disease had its origin there, it must have been so certainly known, that there could have been no doubtful opinions about it; but that the physicians, who resided in or near the place, and those more especially who interested themselves so as to write of it, must all of them have agreed on the certainty of a thing, the truth of which was so easily attainable. But on the contrary, Nicholas Leonicenus, the first Italian physician who wrote on this disease, and who lived at the time when Naples was besieged, is so far from acknowledging it to have had its rise there, from the French soldiers' conversation with the Italian women, and so little did he know of its true cause, that he does not even allow it to be the consequence of impure embraces. It was likewise about this time, that Pope Alexander the 6th engaged Gaspar Torella to write of this distemper; and this author is so far from allowing it to have had its origin there, that he tells us the astrologers were of opinion, that it proceeded from I know not what particular constellations. Neither does Sebastianus Aquilanus, who lived at that time, allow it to be any other than an ancient disease; or Antonius Scanarolius, who wrote in 1498, which was but 4 or 5 years after that siege. Nor do several other authors, then living, say one word about this Neapolitan story. But it seems Ulricus de Hutten, a German knight, who was no physician, positively affirms this disease to have had its rise there; but how he should come to know this, who lived at

* Page 368 of this volume of these Abridgments.

such a distance from the place, and they who were physicians, residing as it were upon the spot, be ignorant of it, will be as much credited, as his following inconsistent relation, which will sufficiently prove how little care he took to be apprised of the truth of what he wrote. This very author tells us this disease was unknown till the year 1493, or thereabouts; that he himself had it, when he was a child, and so consequently that it was hereditary, or from the nurse. He wrote his book on this distemper at Mentz, where it was printed by John Scheffer in 4to, in the year 1519. Now if we allow him to be but 27 years of age when he wrote, (for he cannot be supposed to be less, who before this took upon him to cure his father of the venereal disease, without the assistance of any physician or surgeon,) he must have had the distemper upon him, according to his own account, before ever it was in being.

If I have in my former letter, sufficiently proved, that the first degree of the venereal disease was very common among us some centuries before it is said to have been known in Europe; there will be no reason to suppose we were at that time in any measure strangers to it, when it came to be confirmed; more especially, when we consider the methods of treatment in those times, which consisting principally in topical applications, many patients could not possibly escape having it confirmed on them. Now when it was in this confirmed state, the writers of those early times considered it as a new disease, and not a consequence of any disorder before contracted, because they were not apprised that the first symptoms being removed, and the disease to appearance cured, it should afterwards discover itself in such a manner, as should not seem to have the least analogy with the symptoms that first attacked a part, which had been for a considerable time free from any disorder. But because the symptoms are the only true characteristics, by which we can infallibly know one disease from another, it may be expected, that I produce sufficient authorities, to show that they were all known and described by ancient physical and surgical writers, just as they appear in the venereal disease at this day. I have sufficiently made it appear in my former letter, that the first degree of this disease was anciently known in England by the name of the brenning, or burning; and that it was the same thing with what we now call a clap. The symptoms, which are usually its concomitants, are the phimosis and paraphimosis, both which are accurately described, and proper remedies for them set down, by the before-mentioned John Arden, Esq. in another manuscript of his, curiously written on vellum, and beautifully illuminated. The imprudent method of cure of this first degree of the venereal malady, is sometimes attended with a caruncle in the urethra, which was a disease very common here anciently: for, not to mention other early writers, the abovementioned author gives the case of a certain rector, who had a substance like a wart growing in the penis, which in another place he says frequently happens; and of another, which had such an excrescence as large as a small strawberry, which, says he, proceeded from the corrupted matter that remained in the urethra.

And indeed there is not any symptom of the venereal disease, that I find so often mentioned as this of the caruncle, insomuch that it seems to have been more common in those early times than at this day. But this must be certainly owing to the smooth and oily remedies they were continually injecting, which, by their relaxing and softening the fibres of the part, must necessarily dispose the contexture of small blood vessels, lodged at the bottom of the little ulcerations, to fill with nutritious juices, and to extend themselves so as to form such fungous excrescences; and so solicitous were they for removing these inconveniences, that they made use of several ways by corrosives and other methods, to accomplish this end; and a very early writer among us, has given a methodical and curious tract on this subject, where he recommends removing them by the medicated candle, (bougie) which we use at this day, and lays down divers other instructions in relation to it; which makes it perhaps the best discourse on this subject that was ever yet written. The same author takes notice of those obstinate ulcers, which happen on the glans and the neighbouring parts, now called chancres; and the great trouble our ancient surgeons found in attempting their cure, sufficiently shows them to have had their origin from a venereal infection.

Our early writers are very full in their accounts of these several symptoms of the venereal malady, and of others, when the disease was in a more confirmed state, to which they appropriated particular names, perhaps more significant and expressive than those imposed by modern authors. Thus for instance, the buboes in the groins they called dorsers, for which I have given a reason before; and the venereal nodes on the shin bones they termed the boonhawe, which gives us a perfect idea, not only of the part affected, but after what manner it was diseased; for the old English word, hawe, signified a swelling of any part. Thus for instance, a little swelling on the cornea, was anciently called the hawe in the eye; and the swelling that frequently happens on the finger, on one side the nail, was called the whitehawe, and afterwards whitflaw or whitlow. The process the last mentioned author recommends, for the cure of the boon or bonehawe, is to use a plaster, which had a hole cut in the middle to circumscribe it, and applying a caustic of unslacked lime, and black soap incorporated together, which, with plaster and bandage, was to be secured on the part 4 hours, or longer, if that was not found sufficient; after this he proceeds to separate the slough, &c. This practice of his seems to have been found out by accident. For, he tells us, when he was a young practitioner, having applied both the natural and artificial arsenic to the leg of a patient, it so mortified the flesh, as surprised him; but by proper digestives, the eschar coming off, and leaving the bone bare, he scraped it with an instrument for several days, and dressed it with incarnatives, designing to have ingendered flesh on it; but this proving unsuccessful, he continued to scrape it, till he observed it move under the instrument, after which having separated it, he found the sore covered with new flesh, and that the bone was 4 inches in length, 2 in breadth, and very thick, on the removal of which the patient was soon cured.

Thus it is probable, that this observation of this great man led our predecessors to practise the very same method; and at this day in our hospitals we treat the venereal nodes on the shins exactly as is here described, where we observe the same appearances, he so long before noticed; and it is not to be doubted, but the boonhawe and our venereal nodes are the same disease. By the appearance of some of the last of the abovementioned symptoms, we infallibly judge the patient has had the infection upon him a considerable time, and that the disease is making its gradual advances, to the corrupting and destroying of the whole frame of the body. That this was the conclusion of the miseries of those persons, that gave themselves up to the embraces of lewd women, in those early times, as well as now, I cannot better prove than by those remarkable instances in a MS. written and collected by one Tho. Gascoigne, a doctor of divinity, in Lincoln College, Oxon, whose words are, "Novi enim ego magister Thomas Gascoigne, licet indignus, sacræ theologiæ doctor, qui hæc scripsi et collegi, diversos viros, qui mortui fuerunt ex putrefactione membrorum suorum genitalium et corporis sui; quæ corruptio et putrefactio, ut ipsi dixerunt, causata fuit per exercitium copulæ carnalis cum mulieribus. Magnus enim dux in Angliâ, scil. J. de Gaunt, mortuus est ex tali putrefactione membrorum genitalium, et corporis sui, causatâ per frequentationem mulierum. Magnus enim fornicator fuit, ut in toto regno Angliæ divulgabatur, et ante mortem suam jacens sic infirmus in lecto, eandem putrefactionem Regi Angliæ Ricardo secundo ostendit, cum idem rex eundem ducem in suâ infirmitate visitavit; et dixit mihi qui ista novit unus fidelis sacræ theologiæ baccalaureus, Willus etiam longe vir maturæ ætatis et de civitat. Londonii, mortuus est ex tali putrefactione membrorum suorum genitalium et corporis sui, causatâ per copulam carnalem cum mulieribus, ut ipsemet pluries confessus est ante mortem suam, quum manu sua propria eleemosynas distribuit ut ego novi anno Dni. 1430."

Now it is plain that those instances mentioned from Arden, or these from

Gascoigne, who was then Chancellor of Oxford, could be no other than venereal cases. Certain it is, no disease was ever known to be gotten by the carnal conversation of women, which first attacked the genitals, causing a corruption and putrefaction of them, and afterward of the whole frame of the body, but that which is venereal. For nothing is more commonly known at this day, than that after the venereal connection with an impure woman, the penis is the part where the scene is first laid for the succeeding tragical appearances; and there, and in the neighbouring parts, do the symptoms of the disease as its retainers, always first assemble; till the malignant poison taints the blood and other juices; which being conveyed over the whole frame of the human fabric, if not checked, soon brings about its total corruption.

What I have further to add in relation to this is, since we do not find that the disease mentioned by Gascoigne was distinguished by any particular name, and that great numbers must unavoidably die of the venereal malady at that time, from the imperfect knowledge of those who had the treatment of the first degrees of it, it must necessarily follow, that when the whole frame of the body had received a taint from the venereal poison, so as to occasion its breaking out in scabs and ulcers, almost all over its surface, it must generally be called by the name of some particular disease, whose appearances had somewhat of an affinity to it. Now if we examine the nature of all the diseases that attack the human body, we shall not find the venereal malady, when it arrives at this state, to bear a greater similitude to any than the leprosy, as it is described by the ancients: nay, so great was the analogy between these diseases, that Sebastianus Aquilanus has endeavoured to prove from Galen, Avicenna, Pliny, &c. that the pox is only one species of the leprosy; and Jacobus Cataneus, a writer almost as early as the rise of the name of the pox, tells us it is not only possible there may be a transition from one of these diseases to the other; but that he saw 2 persons in whom the pox was changed into the leprosy; that is, from having large pocks or pustules on the surface of their bodies, from whence the pox is denominated, to have become ulcerous or scabby. This particular state of the disease anciently put the surgeons to a great deal of trouble: for finding that these ulcers were of a very obstinate nature, they were obliged to make use of great numbers of remedies, in order to conquer their bad disposition. But they observed that they were all useless, unless mercury was joined with them. Now the dressing each particular ulcer being so very tedious, they ordered the patients to daub the ointments over the parts which were ulcerated; which done they were wrapt in linen cloths, till the next dressing: but after a few days they were extremely surprised, to find their mouths began to be sore, and that they spit very profusely; and they tell us to their astonishment, that in a little time the sores became healed, and the patients cured. And by this accident it was the method of salivating by unction was first discovered, which is in so much use at this day.

Now although the foreign authorities beforementioned, might be considered as sufficient to prove how our ancestors blended these 2 diseases together; yet I shall prove from our own writers, long before those, that though the pox was, not only among us, but in distant nations, anciently confounded with the leprosy: yet so exact were our writers in their observations of the infectious nature of one species of that disease, and describing the symptoms, as was sufficient to lead any person to distinguish between them. I shall therefore first inquire into the manner how the leprosy was sometimes said to be gotten in those early times, and then examine the symptoms of the disease that attacked the patient.

John Gadisden, a very learned and famous English physician, who flourished about the year 1340, in an excellent work of his, entitled Rosa Anglica, speaking de infectione ex coitu leprosi, vel leprosæ, says as follows: Primo notandum quod ille qui timet de excoriatione et arsura virgæ post coitum, statim lavet virgam cum aqua mixta aceto, vel cum urina propria, et nihil mali habebit; and in another place speaking, de ulcere virgæ, he says, sed si quis vult membrum ab omni corruptione servare, cum a muliere recedit, quam forte habet suspectam de immunditie, lavet illud cum aqua frigida mixta cum aceto, vel urina propria, intra vel extra præputium. Likewise, still speaking of the leprosy, he recommends a decoction of plantain and roses in wine, to be used by the woman, immediately after the venereal encounter; by which he says she will be secure. From hence it is evident that some of their leprous women, as they called them, were capable of communicating an infectious malady to those who had carnal conversation with them: which proves, that the pudenda of the women must be diseased: For as much as we are absolutely assured infections of that nature only happen when a sound part comes to an immediate contact with a diseased one; the symptoms always first displaying themselves in those parts through which the virulency is first conveyed. Now in a true leprosy we never meet with the mention of any disorder in those parts, which, if there be not, must absolutely secure the person from having that disease communicated to him by coition with leprous women; but it proves there was a disease among them, which was not the leprosy, though it went by that name; and that this could be no other than venereal, because it was infectious: for there is no other disease that is capable of being communicated this way but the venereal disease, seeing the pudenda are only in that distemper so diseased as to become capable of communicating their contagion.

The learned Gilbertus Anglicus, who flourished about the year 1360, reasons concerning the manner, how it is possible a man should be infected by a leprous woman: where if we allow him to call the malignant matter, which is lodged in the vagina, the semen fœmininum, we shall find that he accurately describes the very first venereal infection, by part of the virulent matter, being received into the urethra; from whence, by the communication of the veins and arteries, it is conveyed into the whole body, after which, says he, ensues its total corruption.

Let us now examine the symptoms of one sort of their leprosy; for it must be necessarily divided into different species, when another distemper was blended with it, in which we observe such a diversity of appearances; and this I shall the rather do in this place, because it will furnish us with the next succession of symptoms after those already mentioned, as the venereal ozænas, the ulcers of the throat, the hoarseness, the proof of its being communicable from the nurse to the child, by hereditary succession, &c. All which we find to be true in the venereal disease at this day.

Our countryman Bartholomew Glanvile, who flourished about the year 1360, in his book de Proprietatibus Rerum, translated by John Trevisa, vicar of Barkley, in 1398, tells us, "some leprous persons have redde pymples and whelkes in the face, out of whome oftenne runne blood and matter: in such the noses swellen and ben grete, the vertue of smellynge faylyth, and the brethe stynkyth ryght fowle." In another place the same author speaks of "unclene spotyd glemy and quyttery, the nosethrilles ben stopyl, the wasen of the voys is rough, and the voyce is horse and the heere falls." Among the causes of this sort of leprosy, he reckons lying in the sheets after them; easing nature after them; and others which the first writers on the pox thought capable of communicating that contagion: also, says he, "it comyth of fleshly lyking by a woman, after that a leprous man hathe laye by her; also it comyth of fader and moder; and so thys contagyon passyth into the chylde, as it ware by lawe of herytage. And also when a chylde is fedde wyth corrupte mylke of a leprouse nouryce. He adds, by whatever cause it comes, you are not to hope for cure if it be confyrmyd; but it may be somewhat hidde and lett that it distroye so soone."

Thus we see how our author, under the name of one species of the leprosy, gives a summary of the symptoms of the pox, and the several ways by which it is at this time communicated. Now when these 2 diseases were anciently blended together, and passed under the name of the leprosy only, it must be the real cause why that disease seemed to be so rife formerly; for two distempers, passing under one name, must necessarily make it more noticed, and much more frequent; not but that the far greater number of those who were formerly said to be leprous, were really venereal, seems to be very evident; for since that disease has been separated from the leprosy, it has drawn off such vast numbers, that the leprosy is become as it were a perfect stranger to us.

Those that are acquainted with our English history, well know the great provision that was anciently made throughout all England for leprous persons, insomuch that there was scarcely a considerable town but had a lazar-house for such patients. In a register belonging to one of these houses, I find there were in Henry the 8th's time, six of them near London, (viz.) at Knight's-bridge, Hammersmith, Highgate, Kingsland, the Lock, and at Mile-end; but about 40 years before, I find only four mentioned; and in 1452, in the will of Ralph Holland, Merchant-Taylor, registered in the Prerogative Office, mention is made of only three, which, with his legacies to them, are as follow. Item lego Leprosis de Lokes, extra Barram Sti Georgii 20s. Item lego Leprosis de Hackenay (which is that at Kingsland) 20s. Item lego Leprosis Sti Egidii extra Barram de Holborn 40s. From which it is worth while to observe, that the Lock beyond St. George's church, and that at Kingsland, are at this time applied to no other use than for the entertainment and cure of such as have the venereal malady.

Some of our learned antiquaries have been much at a loss for the cause why the leprosy should be so common in those early times, and so little known among us now: but I believe the reason will be impossible to be assigned, unless we allow, according to the proofs which I have already brought, that the venereal disease was so blended with it, as to make up the number of the diseased. It seems to have been the same thing with those in France, as with us: for the author of the history of that kingdom, which was lately published here in two volumes in octavo, tells us that the house of the fathers of the mission of St. Lazarus, was formerly an hospital for leprous people; but that disease having ceased in this last age, since the pox has been distinguished from it, these lazar houses have been converted to other uses; and it may not be foreign to my purpose to notice, that the writ de Leproso amovendo, contained in the register of writs was, according to Coke upon Littleton, to prevent leprous persons associating themselves with their neighbours, who appear to be so by their voice and their sores; and the putrefaction of their flesh: and by their stench. The method then to prevent this noisome and filthy distemper, the leprosy, after the usual method, was castration. It is certain that eunuchs are rarely or never troubled with the leprosy, according to M. le Prestre, a counsellor in the parliament of Paris, (Centur. 1. Cap. 6. de separatione ex causa Luis Venereæ,) whose words are to this effect. "Antipathy resists the poison of the leprosy: hence eunuchs, and such as are of a soft, cold, and effeminate nature, are seldom or never infected with the leprosy; and such as are in danger of it, may, according to the opinion of physicians, be castrated." And Mezeray says, he has read in the life of Philip the August, that some men had such apprehensions of the leprosy, (that shameful and nasty distemper) that to preserve themselves from it, they made themselves eunuchs. Now it is highly probable that those persons that submitted to such a painful operation, having before observed, that those who gave themselves up to a free and unrestrained use of women, fell at length under such unhappy circumstances; and so found the only measures to preserve themselves from it, was to be disabled for such engagements; which sufficiently proves that this species of the leprosy was infectious; and, for the reasons before assigned, could be no other than venereal; for how the true leprosy should be prevented by such means, will be, I believe, impossible for any person to determine.

There yet remains one very considerable symptom of the venereal malady to take notice of, because it is considered the most remarkable in that disease, which is the falling of the nose; but since it has been already proved, that this disease, when it had arrived to such a pitch, as to discover itself by those direful symptoms as are the immediate forerunners of this, was by the ancients confounded with the leprosy, and called by that name, it must be among the symptoms of that disease we are the most likely to meet with it, if any such thing as the falling of the nose was known among them. Now the most likely method of coming to a certain knowledge of the infallible symptoms of the leprosy of the ancients, in its more confirmed state, is to consult the examinations those unhappy persons were obliged to undergo, before they were debarred the conversation of human society, and committed to close confinement: but this being a thing some ages since laid aside, no author that I know of having the particular history of it, and somewhat of it being absolutely necessary in this design, I shall do it as briefly as I can from what remains I have met with in records, and other scattered papers.

First then, after the persons appointed to examine the diseased, had comforted them, by telling them that this distemper might prove a spiritual advantage; and if they were found to be leprous, it was to be considered as their purgatory in this world, and though they were denied the world, they were chosen of God; the person was then to swear to answer truly to all such questions as they should be asked; but the examiners were very cautious in their inquiries, lest a person that was not really leprous should be committed, which they esteemed an almost unpardonable crime; they considered the signs as univocal, which properly belonged to that disease, or equivocal, which might belong to another, and did not on the appearance of one or two signs, determine the person to be a lazar; and this I find to be the case of the wife of John Nightingale, Esq. of Burntwood in Essex, who in the reign of Edward the 4th, Anno 1468, being reported to be a lazar, and that she conversed with persons in public and private places, and did not, according to custom, retire herself, but refused so to do, was accordingly examined by William Hattecliff, Roger Marcall, and Dominicus de Serego, the king's physicians; but they, upon strict inquiry, adjudged her not to be leprous, by reason the appearances of the disease were not sufficient. Some of the questions put to the leprous persons, as they called them, which will more fully confirm what I have before advanced, I shall now give as I transcribed them from an ancient book of surgery, as follows: "yf there were any of his lygnage that he knew to be lazares, and especially their faders and moders; for by any other of their kynred they ought not to be lazares, then ought ye to enquire yf he hath had the company of any lepress woman, and if any lazare had medled with her afore him; and lately because of the infect matter and contagyous filth that she had received of him. Also his nostrills be wyde outward, narrow within and gnawn. Also yf his lips and gummes are foul stynking and coroded. Also yf his voice be horse, and as he speaketh in the nose." Now the signs here mentioned were accounted univocal, and which made the examiners principally determine the persons to be leprous; but what determinations any one would immediately give from such symptoms now, no person is surely ignorant of. But even these certain appearances would not always satisfy some persons, if we may believe Fælix Platerus, in his Medicinal and Surgical Observations, lib. 3, who tells us, that some did not account them to be so, till they had a horrible aspect, were hoarse, and noses fell. Likewise in the Examen Leprosorum, printed in the De Chirurgia Scriptores Optimi, the author, speaking of the signs of the leprosy relating to the nose, begins thus, "Si nares exterius secundum exteriorem partem ingrossentur, et interius constringantur, et coarctentur. Secundo si appereat cartilaginis in medio corrosio, et casus ejus significat lepram incurabilem." And the above-mentioned John Gadisden, in his chapter de Lepra, says as follows: "Signa confirmationis etiam incurabiliter sunt corrosio cartilaginis quæ est inter foramina et casus ejusdem."

Thus I have proved, that we had a distemper among us some centuries before the venereal disease is said to have been known in Europe, which was called the burning; that this burning was infectious, and that it was the first degree of the venereal disease; that this being common at that time, from their method of treatment, the pox must be unavoidable; that it had exactly the same appearances it has now, though they were generally called by different names; that the ancients confounded it with the leprosy; that the vast numbers of leprous persons among us before the venereal disease was distinguished from it, and the small number we observe at this time, is a flagrant proof of the former: that in describing the symptoms of the leprosy, they give us those of the venereal malady; and, by mentioning how it is communicated, they describe the ways by which the pox is communicated at this day; that such remedies were by them recommended to prevent the first attack of the leprosy, as are at this time in use to prevent the first symptoms of the pox; and that the falling of the nose, which has been accounted the most remarkable symptom of the venereal disease, was commonly observed in what they called the leprosy in former ages.

An Account of the great Meteor which appeared on the 6th of March, 1715. *By the late Rev. Mr. Roger Cotes.* N° 365, p. 66.

The appearance of the meteor was very nearly the same here at Cambridge as in Yorkshire, excepting that the triangular streams of light were not so permanent, and the point to which they all converged was about 20° from the zenith, its azimuth lying between the south and the east at about 10° from the south, towards which point of the compass the wind tended. I am told that some streams were seen to shoot forth immediately after sun-set, and that they did not perfectly cease till about 3 or 4 in the morning.

It was after 7 before I had notice of this uncommon sight. At first I saw only two or three of the triangular streams towards the north and north-west; these were not of long duration, but were succeeded by others which appeared and vanished again by turns, rising from, and ascending up to places in the heavens, of very different altitudes above the horizon. From the time I began to view them, they continued to ascend more and more copiously, being propagated still farther and farther from the north towards the west and east, and directed always to the heads of Gemini, till at length, when they seemed almost to meet at the point of convergence, they began to ascend up towards it from the southern parts also, and all around it; insomuch that at a quarter after 7 we had a perfect canopy of rays over us. The bottom of this canopy did no where reach down to the horizon; for near the north, where it descended most, its altutude was about 10 or 15 degrees; and near the south, where it descended least, its altitude was about 40 degrees. It remained in this state about 2 minutes, during which time, we saw several colours, some fainter and more permanent, others brighter, but quickly vanishing. Thus in the west I observed the rays to be tinged for some considerable time with an obscure and heavy red; and in one of the brightest streams, at another time, there suddenly broke out a very vivid red, which was instantly and gradually succeeded by the other prismatic colours, all vanishing in about a second of time. These colours affected the sense so strongly, that I thought them to be more intense than those of the brightest rainbow I had ever seen. A small time before the appearance lost its perfection, we were surprised to observe a shaking and trembling of the streams, chiefly in their upper parts, during which their convergence was confounded, and the whole heaven seemed to be in a convulsion. At the same time I could perceive waves of light towards the north, which moved upwards, and in their motion crossed the streams, lying parallel to the horizon. Their breadth seemed to be about a degree, their length about 90 degrees, and I can compare them to nothing better than to those slender waves on the surface of stagnant water made by throwing in a small stone.

About 7 or 8 years since, I saw another meteor. Along the horizon in the north, there lay a white and luminous, and seemingly a dense matter, in the form of a cloud, represented by a b c d, fig. 1, pl. 13, its length, a b, was about 10 or 15 degrees. From this there arose, directly upwards, pointed streams of the like luminous and white matter, which yet did not appear in any part of it to be so dense as the former, and which became gradually more and more rare in its upper parts, so as to vanish almost insensibly at the points. There was some little difference in the height of these streams, but they generally ascended up to about 4 degrees above the horizon. They were very numerous, and contiguous to each other, and seemed to be composed of very slender parallel filaments or rays. Sometimes a fire or flame would break out in the cloud, abcd, and move along it in a direction parallel to the horizon; and during this motion a pointed stream directly over the fire seemed to run along with it, and to pass by the other more fixed streams, to which it always kept itself parallel.

I am persuaded that the late appearance was of the same kind with this. For let AB, fig. 2, represent the plane of the horizon, C the place of the spectator, EF, a fund of vapours or exhalations at a considerable height, diffused every way into a large and spacious plane, parallel to the horizon. This fund of mixed matter by fermentation will emit streams from itself, such as EG, FH, &c. which, if the wind be perfectly still, will ascend perpendicularly upwards; if it be boisterous and irregular, they will be blended and confounded together; but if it be very gentle and uniform, as it was at the time of this appearance, they will be inclined towards the point of the horizon, which is opposite to that from which the wind blows. Now if ADB represent the concave of the heavens, and a line, CD, be drawn parallel to the columns EG, FG, &c. it is certain, by the rules of perspective, that these columns will appear on that concave to converge all around towards the point D; thus the column EG will seem to arise from the point e, to ascend up to g, and to take up the space eg; and in like manner the arch fh will be the projection of the column FH. From hence it is evident, that the reason why the triangular streams ascended at first only from the northern parts of the heavens, was this: the fund of matter EF was not yet arrived by its motion to the line CD. After it had passed that line, it is plain they must appear to ascend from all quarters. A great number of columns being therefore disposed to emit light, at the same time, caused that perfect canopy described above. The reason why that canopy descended lower in the north than in the south, was this: the shining columns which had not yet passed the line CD, were more numerous and more remote from it than those which had passed it; for if the point E be farther distant from CD than the point F, the arch Ae must needs be less than the arch Bf. An irregular gust of wind blowing on and shaking the columns I suppose was the cause of that trembling which appeared in the triangular streams, and the cause also which destroyed that fine appearance of the canopy. The slender circular waves, seen at the same time, might also be explained from the same cause. I need not detain you any longer by endeavouring to make out some other particulars of this unusual appearance, I fear I have been already too tedious. However I will not omit to mention a very easy contrivance, by which the thing may be tolerably well represented to view. Take a hoop, and round about it fasten several straight sticks, parallel to each other, but all inclined to the plane of the hoop, hold this plane parallel to the horizon, and in that posture move it with the sticks over a candle; then the shadows of the sticks on the ceiling of your room will converge to a point not directly over the candle, as they would have done had the sticks been perpendicular to the plane of the hoop, but to the point in which a line drawn from the candle parallel to the sticks shall intersect the plane of the ceiling.

A Letter of Dr. John Quincy to the late learned Mr. Sam. Moreland, F. R. S. concerning the Operation of Medicines. N° 365, p. 71.

There is nothing in this letter of sufficient importance for republication.

An Account of an extraordinary Cramp and Fistula. By Dr. Steigerthall, F. R. S. N° 365, p. 79.

John Henry Oizmann, aged 31 years, was 15 years of age when the following misfortune befel him.

He felt a spasm or cramp in his left hip, and the lower part of his leg. As this pain seized him pretty often, a surgeon applied several plasters to the place, but without any relief. After many fruitless efforts, the surgeon, to try whether the patient had any feeling in his leg, which to outward appearance was become very brown, made about 37 incisions over the whole leg, of which the patient was not at all sensible, unless when the instrument happened to grate on the bone, the periosteum being as yet sound. The leg, however, became daily blacker, and the pain continued both in the periosteum and in all the bodies of the upper and lower part of the leg. At last a black circle was seen round about the muscles of the hip, as an indication of an approaching mortification. This circle appeared so visibly, as if it had been cut off with a knife from the other part. It has ever since spread itself, and come to such a head, that without any other help and cure the flesh began gradually to rot away from the bones; and at last quite fell away from the upper part of the leg, which has preserved its soundness. After this, nothing was seen but the bare tendons or sinews hanging down like so many strings or cords. There also remained a piece of the inferior muscles of the hip, fastened to the upper part. At last the tendons, becoming dry, consumed away, and after all, the leg itself, I mean the os femoris, wholly dropped off in such a manner, that there remained about 4 inches between the bones and the flesh, hanging loosely down from them. The flesh is at last grown up to the bones, and without any help has fastened itself to them. And in this sound part the patient feels a great pain, whenever the weather proves tempestuous. It is remarkable that at the same time he perceives also a swelling in tarsus of the right foot, the matter of which discharges itself through the toes, and is of so corrosive a nature, that it has consumed all the toes except the little one. The surgeon has at last healed up this wound; but there is as yet but little feeling or warmth in the foot.

This man was married about 7 years since to a woman, whose bodily constitution is almost as remarkable. She is now in the 41st year of her age. In her younger years she had the misfortune to be gored by a wild boar under the short ribs of the left side. This wound became fistulous, and what food she eats discharges itself half concocted through this aperture, so that you can distinguish what sort of food it was; however she has, notwithstanding this, her daily evacuations per anum.

An Experiment before the Royal Society, to show, by a new Proof, that Bodies of the same Bulk do not contain equal quantities of Matter; and therefore that there is an interspersed Vacuum. By J. T. Desaguliers. N° 365, p. 81.

I took 3 lb. of mercury, which by measure filled three times a small glass jar exactly full, and poured it into a thin Florence flask; then having poured the same quantity of water, that is, three of the same jars full, into another such flask, I set both the flasks in a pail, and poured boiling water about them, keeping the flask of water down by force, that it might be as low in the hot water as the mercury. After the fluids in the flasks had received a sufficient degree of heat from the water, which was round the flasks, for the space of 5 minutes, I took the flasks out of the hot water, and putting that which held the water into a cylindric vessel, that had 3 pints of cold water in it, and at the same time plunging the flask with mercury into another cylindric vessel, containing also 3 pints of cold water, I observed which of the cold waters was most heated, in the following manner.

A small thermometer being held in the first vessel of cold water, so as to have its ball covered with the water, on putting in the flask of warm water, the spirit rose 2 degrees; then putting the thermometer into the water where the flask that had the mercury was, the spirit rose 3 degrees higher. The thermometer being again put into the first vessel fell 4 degrees, and afterwards again into the last, it rose almost 3 degrees.

This shows that more heat is communicated by warm mercury than by an equal bulk of water equally warmed; and therefore that there is more matter in the mercury; but how much more matter there is in the mercury, is not determined by this experiment alone.

N. B. The warm mercury and the warm water were not poured into the cold, but only communicated their heat through the flasks.

An Account of a Contagion among the Cattle in the Venetian Territories, in Autumn 1711. *By Dr. Peter Antony Michelotti.* N° 365, p. 83.

Dr. Michelotti, being in the Venetian territories about October 1711, took that opportunity of making a particular inquiry into the circumstances of the mortality that then raged among the black cattle; and was himself an eye-witness of the greatest part of the facts contained in this account, and received the rest on the spot from persons of integrity and credit.

Almost all the sick cattle refused every kind of food and drink; they hung their heads, had shiverings in their skin and in their limbs, they breathed with difficulty, and their expiration in particular was attended with a sort of rattling noise; they were so feeble that they could scarcely go or stand upon their legs. Some few of them eat a little and drank very much, others had fluxes of excrements variously coloured, of a very offensive smell, and frequently tinged with blood; many of them had their heads and their bellies swelled in such a manner, that, on clapping them with the hand on their paunches, or along the vertebræ of the loins, they sounded like a dry bladder when full blown. In some the urine was very turbid, in others of a bright flame colour. In comparing the pulses of the sound cattle with those of the diseased, he found the latter to be quicker and weaker. There was but little heat perceivable by the touch in any of them; their tongues were soft and moist, but their breath was exceedingly offensive. Besides these particulars, he was informed by those who attended the sick cattle, and by other persons worthy of credit, that in some of these beasts they had observed crude tumors in several parts of the body, as also watery pustules, and disorderly motions of the head, with dry, black, and fissured tongues; that in others there were tumors that came to maturation, with putrid matter issuing from the mouth and nostrils, worms in the fæces and in the eyes, bloody sweats, and shedding of the hair.

In comparing the flesh of the cattle dead of this distemper, with that of others killed for the market, he found the muscles in the former, lying immediately under the skin, to be something livid. Having opened the three cavities of the body, he applied himself with the utmost diligence to examine the brain with its membranes; the trachæa, œsophagus, lungs, heart with its auricles, the vena cava, aorta, and diaphragm; the liver, spleen, and other parts of the lower belly; in all which there was no discernible difference, either as to figure, size, contents, situation, or connection, with the neighbouring parts, from what was observed in sound cattle killed by the butcher, except the particulars hereafter mentioned. The blood found in the ventricles of the heart, in the pulmonary vessels, in the aorta and cava, though still warm, was considerably blackish, and almost coagulated. In opening the upper and middle cavity, the scent was offensive, but tolerable enough; whereas that proceeding from the lower belly, was quite intolerable. In some few carcases the viscera differed from their natural state, with regard to their size, their consistence, their contents, colour, and smell. In several the paunch was found very much contracted and dried, and containing a hard substance. In others the lungs were swelled and livid, the liver tumefied, and the brain watery and putrid.

Having ordered several of the cattle to be blooded, he found the blood not to issue out of the vessels in a continued stream, as usual, but with a broken and interrupted flux, one part of it not immediately succeeding another. Having caused the blood to be received in proper vessels, and suffered it to stand for some time, he found it entirely coagulated, without any separation of the serum, and attached to the sides of the vessels, with a reticular pellicle on the surface exposed to the air. All the cattle that were blooded, being 18 in number, died in a few days after the bleeding, one only excepted, in which the vein was opened on its being first taken ill.

Having enumerated all the symptoms of the distemper, the author concludes from the whole, that the sickness among the cattle was a malignant pestilential fever, killing almost all those that were infected with it.

The immediate cause of this he takes to be a præternatural thickness of the blood, occasioned by a beginning coagulation of those parts which constitute the crassamentum; by which the globules of the blood and the particles of the serum were imprisoned in a sort of reticulum, formed by the union of the fibres of the blood.

The occasional cause of this sickness he deduces from the cold and wetness of the season, which prevailed all the preceding year, from October 1710 to November 1711. Which observation is worthy of remark, since the season preceding the mortality among the cattle here in England was remarkably dry, and yet the symptoms of the distemper agreed with those observed in Italy, as may appear from the account given by the learned Mr. Bates, surgeon to his majesty's household, in the Phil. Trans. N° 358.

Account of a Case in which a portion of the Colon was propendent from a wound of the Abdomen for 14 Years. Communicated by Abr. Vater, Phil. et Med. Doct. Professor of Anatomy, &c. at Wittemberg.* N° 366, p. 89. *Translated from the Latin.*

George Deppe, of Halberstadt, aged 34, in consequence of a wound received in the left hypochondrium, in the battle of Ramillies, in 1706, has ever since (viz. for 14 years) had a large portion of the colon, a span in length, hanging out of the abdomen. The wounded and protruded intestine is inverted; and being united in the middle, it exhibits 2 orifices, the upper orifice

* Abr. Vater was born in 1684 at Wittemberg, in which university his father Christian Vater held a professorship, and was dean of the faculty. After taking his degree of M. D. the son, author of the above communication, travelled through various parts of Germany, as well as into Holland and the Netherlands; from whence he passed over to England, where he became acquainted with Hauksbee, Sir Hans Sloane, and other distinguished members of the R. S. of which he was some years after elected a member. On revisiting Holland, he formed an intimate friendship with Ruysch, under whom he perfected himself in anatomy, and particularly in the art of making injected preparations. He returned to Wittemberg in 1711, after an absence of 2 years. Here, in course of time, he was appointed successively to the professorships of medicine, anatomy, botany, &c. He died in 1744. Dr. Abraham Vater contributed, in no small degree, to the progress of anatomical and pathological knowledge in his native country, not only by his lectures and valuable collection of injected and other preparations; but further by various tracts and dissertations which he at different times published. After his death there was found among his papers a description, illustrated by engravings, of his anatomical cabinet, which was edited with a preface by Heister, under the title of Abr. Vateri Museum Anatomicum Proprium, 4to. 1750.

communicating with the small intestines, and discharging the alvine fæces, and the lower orifice with the rectum, so that a clyster injected per anum is returned by it. In the inner surface, now turned outwards, are seen a vast number of small glands, like warts, of a whitish grey colour, exhibiting a most curious appearance, and bleeding on being rubbed or touched roughly. The protruded portion of the intestine is never wholly drawn into the abdomen; a part of it however retires when the stomach is empty; on the contrary, when the stomach is full, and especially when the person holds his breath, it is thrust out to a greater extent. This person uses, without inconvenience, the coldest water, and in winter-time even snow and ice mixed together, for cleansing the protruded gut; which moreover bears exposure to the coldest air; except that it is thereby contracted and hardened, and rendered of a somewhat paler colour. He can eat all sorts of food; but raw fruit and garden-stuff pass off undigested; and this is the case also with broths that are taken without solid food.

Wittemberg, Oct. 1, 1720.

Observations on the Bones and the Periosteum. By Mr. Leuwenhoeck, F. R. S. N° 366, p. 91.

I have found, by frequent observations on the bones, that their superficial part consisted of an inconceivable number of small vessels, and some few of a larger size, which last, when they came to the surface of the bone, appeared to be clothed with either a membrane, or a bony substance, that was perfectly transparent. I once discovered, in a small portion of a shin-bone, 4 or 5 vessels of such a size, that a single filament of silk might have been drawn through their aperture. One of these appeared to consist of 2 openings, each seemingly provided with a valve, disposed in such a manner as to let out what was contained in the vessel, but to suffer nothing to go in.

As for that matter which issues out from the bone, and is carried into the periosteum, I have discovered the source of it to be the spongy or cellular substance on the inside of the bone, which is the repository for the marrow. This spongy substance consists of long particles closely united and linked together, which are composed of an infinite number of small vessels, some running lengthwise, and others taking their course towards the sides of the bony particles; which last, notwithstanding their great number of apertures, are yet ex ceedingly hard; some of them lying parallel, and others perpendicular to the length of the bone

Those particles, that lie perpendicular to the length of the bone, have vessels proceeding from their extremities; and from their sides, where they do not lie close together, proceed other vessels, that compose the cortex, or superficial part of the bone. And those bony particles that lie parallel to the length of the bone, send out vessels from their sides, that issue out through the side of the bone. It is impossible to conceive the prodigious number of small vessels, of which the cortical part of the bone consists; which in some places lies no thicker on the spongy part of the bone than a thick hair of a man's head, though in other places it has 3 or 4 times that thickness.

The periosteum is united to the cortex of the bone, not only on the outside, but even by entering in many places into the very substance of the bone, and is joined to it by the vessels, which issue out from the bone, in such a manner, that sometimes one cannot determine which is the bone, and which belongs to the membrane investing it, both appearing in the microscope to consist alike of exceedingly small vessels.

Fig. 3, pl. 13, is a representation of a small part of the bone, with the periosteum adhering to it; in which ABCDEF represents the bony part; BGHIE the periosteum, the thickness of which is designed by BG, or IE; though in other places of the bone, and even at no greater distance than 2 or 3 hairs breadth, it is twice or thrice as thick: all the small vessels in the periosteum are represented by so many dots or points; but in other places, where I had several times seen the membrane of twice this thickness, the upper half of it has appeared to be of a different make from the under part; for as much as in the upper part I could discover not only those vessels that had been cut transversely, and which consequently were represented by so many points, but likewise a great number of other vessels running lengthwise along the membrane, as is represented by LOPQNM, in fig. 4.

I am fully persuaded, that the part represented by BGHIE, fig. 3, is not entirely membranous, but that some part of it is really bony. If we cut through the periosteum so deep as to divide the part of the bone marked with the letters ABCDEF, in the same figure, we find the same appearance of pores in the bony substance, which are no other than the transverse sections of small vessels; and besides these, there are other vessels running lengthwise in the bone. And we find just the same in those transparent parts, that lie between the bony particles, which are represented thicker between BCDE, than they appeared to me.

It is my opinion, that the use of these bony particles, is to convey an oleaginous liquor into the periosteum, and from thence, by the intervention of the other membranes, into all parts of the body, when in a healthful state.

In another place, I saw a great number of vessels rising from a greater depth within the bone, which drew closer together, so as to compose small fasciculi, before they entered the periosteum, in which they separated from each other, and dispersed themselves again. It is difficult to determine whether these vessels bring any liquor into the bone, or carry it out; but I rather think for the latter purpose.

Having placed another piece of bone before the microscope, with the periosteum adhering to it, I could discover a great number of vessels, cut through lengthwise, as they ran along the periosteum, and others cut through transversely, and appeared as so many points, as is represented in fig. 4, by KLOPQNA, where the bony part is marked KLMNA, in which, though no pores or vessels are here represented, yet it is full of openings. That part represented by LOPQNM, we must not take to be entirely membranous; for I am of opinion, that that part of it lying next the bone, and which is represented by LMN, is of a bony substance.

I had another small piece of bone lying before a microscope, of which I caused a part to be represented by RSWXTV, fig. 5, in which RSTV is the bone, and SWXT the periosteum, which in this place was no thicker than a thick hair of a man's beard, but in another part of the same bone at a small distance, it was full 4 times that thickness.

I placed another piece of bone before a microscope in such a manner, that the bone did not appear, but only the periosteum and the muscular fibres, which were cut through transversely, and appeared to be surrounded by the fibrillæ of the periosteum, as is represented by YZCDAB, fig. 6, where YZAB is the periosteum, and ZCDA are the fleshy fibres cut through transversely. This piece of bone was taken from one of the ribs of a fat ox, and I was surprised to find, that in this place, as I cut longwise through the rib, I could not discover any particles of the marrow, whereas in other parts the rib abounded with them.

Notwithstanding the great number of observations I have made on the bones, and the membrane that surrounds them, commonly called the periosteum, I have never been able to satisfy myself entirely about them. I still imagined, that the part of the periosteum which immediately covers the bone, and is strictly united to it, must have a degree of hardness approaching to that of the bone, and that at a small distance from the bone, the periosteum must have a softness and flexibility like that of the carneous and adipose membranes.

I had kept 4 pieces of ribs of an ox, full 2 months, which were now grown very dry. From one of these I tore off the periosteum, which I found stuck much harder to the bone than I could have imagined, and I observed, that a great many particles of this membrane were left on adhering to the bone. I did this with design to make some observations on the superficial part of the bone, which is not near so hard as those bony particles that lie a little deeper. From this bone I cut off some very thin slices, both lengthwise and transversely, one of which I placed before a miscroscope, and caused it to be delineated. This piece is represented by ABKC, fig. 7, having been cut off transversely, and as thin as possible, from the rib, with part of the periosteum, as from K to C, still adhering to it, and another part of it torn off from the bone, as noted by BKD, only that in some places the bone and membrane are still united by vessels torn out of their places, that run from one to the other. In this figure DEFC represents the periosteum, and the part EGHIF is something lying upon it, which I could not tell what to make of, though it appeared to be membranous.

I had likewise some very thin slices shaven off from the rib both of an ox, and of a calf, from which I tore off the periosteum entirely, or at least as much as possible; after which I caused the edge of the bone it had stuck to, to be represented by the crooked line, LMN, fig. 8.

In fig. 9, OPQ represents the edge of another small slice of bone, from which the periosteum has been torn off; by which appearance it should seem, that the union of the periosteum with the bone is so firm and strong, that in separating it, some of the superficial particles of the bone are torn off with it. I have likewise discovered some vessels running along within the marrow-bone of the shank of an ox, that seemed to be blood-vessels.

Since now it appears from our observations, which have been made with great diligence and care upon bones of all kinds, that the bones do for the most part consist of exceedingly small vessels, which vessels arise from the inner hollow, or spongy part of the bone, and passing through the superficial or cortical substance, enter the periosteum, and are from thence continued, even into the utmost parts of the body; we may from hence reasonably conclude, that in a healthful body, as there is a constant supply of an oily substance conveyed into the bones, so this is again constantly carried out from the bones by means of these vessels, into all parts of the body, even to the extremities of the fingers. As an evident proof of this, let any one lay the ends of his fingers on a clean and bright pewter plate, and he shall find the pewter appear soiled in the place where he has touched it; for in reality this soil is nothing else, but some oleaginous particles discharged from the ends of his fingers. There is indeed something of a watery substance mixed with the oily particles, but this soon evaporates, and leaves the oily particles on the plate.

Of a præternatural Tumor on the Loins of an Infant, attended with a Cloven Spine. By Dr. Rutty, F. R. S. N° 366, p. 98.

Mr. Ruysch, in an observation on the spina bifida, takes notice that other writers have described it to be cloven into 2 equal parts lengthwise; whereas out of 10 subjects he had an opportunity to examine, not one proved to be in that manner, the body of the vertebræ being entire, and the acute processes only divided.

The spine now treated of comes near this description. It was that of a female infant 6 days old; whose mother, when 7 weeks gone with child, on a fright occasioned by her husband's falling from a horse and very much bruising his loins, gave the embryo this injury; but notwithstanding, she went out her time, and the child was full grown.

There appeared on the region of the loins, in the same place where the father received his hurt, a tumor about the size of a small turnip, with a broad basis, around which the skin was discoloured, as by an ecchymosis, but it grew immediately from thence pellucid, like a vesicle raised by cantharides, and continued so throughout, except just at its apex, where was a substance like a fungus. This vesicle was filled with a liquor, which in scent and colour resembled urine: insomuch, that on strictly examining the linen stained by what issued from hence, with that from the pudenda, we could perceive no sensible difference, and concluded there was a communication between the left kidney and the orifice, into which the surgeon's probe passed obliquely upwards about an inch. On opening the body, the kidneys, contrary to expectation, were perfect, and did not any way communicate with the outward orifice.

But on clearing away the fungous substance, which took up all the sulcus or hollow of the spine, we found where the perforation tended; a long probe easily passing up the channel, which contains the medulla spinalis. Throughout this fungus were dispersed a great many terminations of small nerves, from whence distilled this seeming urinous liquor, which occasioned the tumor: the rest of the medulla was more compact, and filled the cavity of the spine; though in some subjects it has been wasted to such a degree, that by blowing into the orifice, the dura mater may be inflated. The coat of the tumor consisted at its basis of the cuticula, cutis, and membrana adiposa: the first two of which presently terminating, the cuticula only was continued; immediately under which appeared the muscles and fungus abovementioned.

These tumors constantly attend the spina bifida: so that when any of them present themselves on the loins or back of a new-born infant, we may pronounce this to be cause; but we may be positive in it, if the child cannot move its lower limbs; the want of which motion is an infallible indication, that the medulla spinalis reaches no farther than the swelling, by which the nerves are not distributed to those parts. They appear differently in different subjects. In some the whole tumor is opaque; which proceeds from small filaments of nerves propagated in great numbers throughout its coat, and not from the thickness of the skin, as Ruysch will have it. In others it is pellucid; and then the medulla terminates at once at the aperture of the spine, and does not shoot out into any ramifications. This before us is composed of both species, the greater part of which was pellucid; the less, viz. the apex, opaque.

The back-bone itself is not cleft, but has its vertebræ, with their other processes entire, and is only defective in its spines or acute ones. But that portion of the vertebræ, which should make an acute angle, from whence their spinal processes naturally arise, in order to form the specus or passage for the marrow, instead of that, gapes and lies almost in a straight line on each side; whereby the medulla is defrauded of its usual guard from external injuries. This defect begins at the third vertebra of the loins, and is continued to the end of the os sacrum.

As the case before us is a vitium conformationis, owing to the mother's imagination; so the same sometimes is occasioned by matter lodging on the spine, and eroding the vertebræ by its acrimony: but then the bone is carious, whereas in these præternatural cases, there remain no such footsteps.

These cases are incurable, and must in a little time kill the patient. But it is almost immediate death to open the tumor; which every surgeon will naturally do, that has not seen or read concerning it.

On the change of Colour in Grapes and Jessamine. By Mr. Henry Cane. N° 366, p. 102.

About 6 years since I planted against a wall, a cutting from a Muscadine vine, on an eastern aspect, where it has the sun from its rise till half an hour after 12. The soil is a stiff clay, but to make it work the better, I meliorated it, by mixing some rubbish of the foundation of an old brick wall, where it now grows. Two years since, it shot out at both ends about 22 inches of a side, before it came to a joint; that on the right was a very luxurious exuberant branch, as large as the body of the tree, the other side not half so thick, and the leaves on the right were as large again as those on the left-hand, and I fancy the largest that were ever seen. The right-hand bears a very large and good black grape, and large bunches; the left-hand very good white grapes, and I had last year more bunches of the white than of the black; and whereas in all vines bearing black and blue grapes, the leaves die red, these died white on the black side as well as the other. Last January I pruned the tree again, but tacked up more of the right-hand (being black) than I did on the left, for which reason I had this year a great many more of the black than I had of the white, and they ripened for the season of the year very well. I gathered the last about 8 days since, (Oct. 23,) and the leaves die white this year also, being the second year of bearing.

In April 1692, having a small plant of the common white jessamine, which stood in the ground, and was no thicker than a tobacco-pipe, I cut it off at two joints above the ground, and grafted it with a cutting of the yellow striped. It took, and shot a small weak shoot; but in a month or 5 weeks after, it was blighted, and I perceived it had killed the graft, and some part of the stock below, so I took my knife and cut it to the quick, which was near the next knot or joint to the ground, and let it stand, thinking to graft it again at spring, as before, but forgot it till the season was passed. At length I saw it had broke out at the next joint with several shoots of the yellow and green striped; also a strong shoot from the root, of yellow and green striped. After a while I took it up with mold to the root, and put it in a pot, and it flourished all the summer, and for 2 or 3 years after; when for want of shifting the pot in time, it was matted so to the bottom and sides of the pot, that it died.

An Account of some new Electrical Experiments. By Mr. Stephen Gray. N° 366, p. 104.

Having often observed in the electrical experiments made with a glass tube, and a down feather tied to the end of a small stick, that after its fibres had been drawn towards the tube, when that has been withdrawn, most of them would be drawn to the stick, as if it had been an electric body, or as if there had been some electricity communicated to the stick or feather; this put me upon thinking, whether if a feather were drawn through my fingers, it might not produce the same effect, by acquiring some degree of electricity. This succeeded accordingly on my first trial, the small downy fibres of the feather next the quill being drawn by my finger when held near it: and sometimes the upper part of the feather, with its stem, would be attracted also; but not always with the same success. I then tried whether hair might not have the same property, by taking one from my wig, and drawing it 3 or 4 times between my thumb and fore-finger, and soon found it would come to my finger at the distance of half an inch; and soon after I found that the fine hair of a dog's ear was strongly electrical; for on taking the ear and drawing it through my fingers, great numbers of them would be attracted to my fingers at once. I next tried threads of silk, of several colours and finenesses, which I found to be all electrical.

Having succeeded so well in these, I proceeded to larger quantities of the same materials, as pieces of ribband, both of coarse and fine silk, of several colours; and found that by taking a piece of either of these, of about half a yard long, and by holding the end in one hand, and drawing it through my other hand between my thumb and fingers, it would acquire an electricity, so that if the hand were held near its lower end, it would be attracted by it at the distance of 5 or 6 inches.

After this I tried several other bodies; as, linen of several sorts, viz. Holland, muslin, &c. and woollen, as of several sorts of cloth and other stuffs of the same materials. From these I proceeded to paper, both white and brown, finding them, after they had been well heated before rubbing, to emit copiously their electric effluvia. The next body that I found the same property in, was thin shavings of wood; I have only as yet tried the fir shavings, which are strongly electrical. The last three substances which I found to have the same property, are leather, parchment, and those thin guts in which leaf-gold is beaten.

All these bodies will not only, by their electricity, be drawn to the hand, or any other solid body that is near them; but they will, as other electric bodies do, draw all small bodies to them, and that to the distance sometimes of 8 or 10 inches. Heating them by the fire before rubbing very much increases their force.

There is another property in some of these bodies, which is common to glass, that when they are rubbed in the dark, there is a light follows the fingers through which they are drawn; this holds both in silk and linen, but is strongest in pieces of white pressing papers, which are much the same with card-paper; this not only yields a light as above, but when the fingers are held near it, there proceeds a light from them with a crackling noise like that produced by a glass tube, though not at so great a distance from the fingers. To perform this, the paper, before rubbing, must be heated as hot as the fingers can well bear.

A down feather being tied to the end of a fine thread of raw silk, and the other end to a small stick, which was fixed to a foot, that it might stand upright on the table; there was taken a piece of brown paper, which by the foregoing method was made to be strongly electrical, which being held near the feather, it came to the paper, and I carried it with the same till it came near the perpendicular of the stick; then lifting up my hand till the paper was got beyond the feather, the thread was extended and stood upright in the air, as if it had been a piece of wire, though the feather was near an inch distant from the paper. If the finger were held near the feather in this position, the greatest part of the fibres next the paper would be repelled, when at the same time if a finger were held to the fibres that were more remote from the paper, they would be drawn by it.

I repeated this experiment without the feather, viz. by a single thread of silk only, of about 5 or 6 inches long, which was made to stand extended upright as abovementioned, without touching the paper; then placing my finger near the end, it would avoid, or was repelled by it; but on placing my finger at about the same distance from a part of the thread, that was about 2 inches from the end, it was then attracted by it.

The several bodies here found to be electrical, are, 1. Feathers. 2. Hair. 3. Silk. 4. Linen. 5. Woollen, 6. Paper. 7. Leather. 8. Wood. 9. Parchment. 10. Ox-guts, in which leaf-gold is beaten.

A Letter to Dr. Halley, R. S. S. in answer to some Objections made to the History of the Antiquity of the Venereal Disease. By Mr. Beckett, Surgeon, F. R. S. N° 366, p. 108.

I find there have been two objections made against what I have advanced on the antiquity of the venereal disease. The first is, that the venereal disease so well known among us now, and the leprosy of former ages, could not be the same disease, because the leprosy is not to be conquered by salivation, which the other generally very readily yields to. In answer to this, I am to observe, that the leprosy, which we have among us at this time, affects only the surface of the body, the skin generally appears scaly, with a certain deep red colour, or small sores upon removing the scales, and sometimes a scabbiness, with a redness of the skin, which affects different parts of the body. I have known both the cheeks only affected, both the arms for the breadth of the palm of the hand, sometimes the breast, the legs, and other parts. But this may continue on the patient during his life, as it frequently does, and never make any further progress; which shows it to be a cuticular disease. In these cases, on salivating, the scales generally fall off, the redness disappears, and the cure will seem to be completed: but in a month or two, the same inconveniences generally attend them as before. But we ought not to conclude, that because our leprosy will but rarely be cured by salivation, and the pox generally will, that many of those persons the ancients judged to be leprous, were not really venereal: for their leprosy, as they called it, was a quite different disease from ours. Had there been any proof brought that persons had been salivated in their leprosy, and failed of cure, it would have determined the case: but on the contrary, we are assured by the learned Dr. Pitcairn, in his Dissertation on the Ingress of the Lues Venerea, that the leprosy, before the Neapolitan disease was talked of, was cured by mercury, and now since it changed its name, it is no longer heard of. Thus we find that their leprosy and our venereal disease would be cured by the same method; but their leprosy and ours, being absolutely different diseases, we by no means ought to expect the success, from the same process of cure, should be the same. I dare be positive that nobody ever observed our leprosy to be attended with falling of the hair, hoarseness of the voice, the patient speaking as though he spoke through the nose, consumption of the flesh, ulcers all over the body, corruption of the fleshy parts, and of the bones themselves, filthy ulcers of the throat, corrosion and falling of the nose, all which are reckoned as symptoms of their leprosy: on the contrary, ours is a mild and almost inoffensive disease, which a person may be affected with during his life, and never become worse; whereas the other, by displaying itself under the symptoms before enumerated, brings the patient to the most miserable end; besides, their disease was got by coition as their authors assure us, but in our leprosy, a diseased husband may cohabit with his wife as long as he lives, and he shall never be able either by coition, or the immediate contact of the diseased parts with those that are sound, to communicate any malady. Had what our predecessors called the leprosy been the same disease we call by that name now, they had not been so solicitous of making such large provision for them, or shutting them up from human society; for one of our leprous persons might have been among them, and nobody have known he laboured under any infirmity at all.

Hence it is evident that the disease so common among them, was entirely different from our leprosy, the appearances of which bear no manner of analogy with the former: it is from the symptoms of the disease, and the manner of its being received, that we generally know one disease from another; but the symptoms of most of their leprous persons, and the manner in which the disease was received, will be found in no other disease that attacks the human body, but in the venereal disease only: for here they so exactly agree, that we must in a manner do violence to our own reason, if we deny them to be the same.

The second objection was long ago falsely asserted by Dr. Fuller, the historian, that the leprosy was brought into England from the Holy War, by some of our countrymen, and that the disease was altogether unknown among us before. This, as I take it, does not so immediately concern me, since all I take upon me to prove is, that what they called the leprosy, is not the same disease that we call by that name now, but another. However, I shall in a few words make it appear that this objection is likewise groundless, by observing that the first Englishmen that went over to the Holy War made their first voyage in the year 1096, as our historians generally agree, and that some of them returned in 1098, two years after that expedition. But it is certain, that we had the leprosy among us before: for Wharton, de Episcopis Londinensibus, and other historians assure us, that Hugo de Orivalle, one of the bishops of London, died here of the leprosy in the year 1084; which proves that our countrymen did not bring that disease first from the Holy War, because we had it among us before. The account William of Malmesbury gives of this bishop's disease, is as follows. Is post paucos ordinationis annos in morbum incurabilem incidit. Siquidem regia valetudo totum corpus ejus purulentis ulceribus occupans ad pudendum remedium transmisit. Nam credens asserentibus unicum fore subsidium si vasa humorum receptacula, verenda scilicet, exsecantur, non abnuit. Itaque et opprobrium spadonis tulit episcopus, et nullum invenit remedium, quoad vixit leprosus. Now it is highly probable, that had this been a new disease the bishop died of, the mention of it as such, would not have escaped our historian; but on the contrary it seems to have been anciently known among us, because the remedy made use of for it was so, it having been recommended by Ætius, and other physical writers several hundred years before this time; and I think it is very plain that the cutting off the testicles, and with them the vessels formed for the receiving the humours as expressed in the former case, was by them thought to be of peculiar service, because it is probable that observing the disease to begin in these and the neighbouring parts, they thought the very minera morbi would by this means be destroyed, and the disease either cured, or the spreading of it prevented.

An Experiment to compare the Paris Weights with the English. By the Rev. J. T. Desaguliers, LL.D. F.R.S. N° 366, p. 112.

Finding the accounts we have of the French weights different in different books, I sent to a curious gentleman for some Paris weights exact to the standard weights at the Chatelet; and found on trial, that the Paris ounce, which contains 576 of their grains, is equal to 476 of our grains Troy: from which experiment all the other proportions may be deduced. Note,

The French pound contains 16 ounces—the ounce 8 drachms, or 576 Paris grains—the drachm 3 deniers—the denier 24 grains.

Some Remarks on the Method of observing the Differences of Right Ascension and Declination by Cross Hairs in a Telescope. By Dr. Edm. Halley, R. S. S. N° 366, p. 113.

Those that are curious in observing the heavenly motions, and particularly myself, whose business it is, are greatly obliged to the late Signior Cassini for his thought of applying threads at half right angles in the common focus of a telescope, to determine the differences of right ascension and declination of any two stars, whose situation is such, that by their diurnal motion they follow each other through the aperture of the telescope, so fixed as that the first of them may pass over the centre of the glass, and move exactly along one of the threads, while the interval of time between its transit, and that of the following star, is exactly measured by a pendulum clock, well adjusted to the mean motion of the sun, or else to the revolution of the fixed stars, by which the difference of right ascension is given; as is the difference of declination, by the time the following star takes to pass from one diagonal thread to the other. By this manner of observing, Dr. Pound, and his nephew Mr. Bradley did, in the last opposition of the sun and Mars, demonstrate the extreme minuteness of the sun's parallax, and that, by many repeated trials, it was not more than 12″, nor less than 9″. But considering that, in October next, Mars would be again in opposition to the sun, about the 10th degree of Taurus, but would not come very near any fixed star in Mr. Flamsteed's catalogue; I was solicitous to see if there were any telescopic stars to which he would very nearly approach; and on the 28th of February last, the heavens being very serene and clear in the evening, and Venus having nearly the declination in which Mars will move in October next, I fixed my telescope on her, at $7^h\ 28^m$, equal time, and noted the moment she passed over the centre of my glass, or rather the common intersection of the 4 cross hairs; and in half an hour's time I noted 8 very conspicuous stars, 4 of which being within the compass of one degree, fell very nearly in the said way of Mars; and from the intervals of time I then observed, with their difference of declination from Venus, I determined their right ascensions and declinations, as well as her place from my tables, which by observation I found at this time needed no correction; all of them falling between the 9th and 10th degree of Taurus, with very little latitude. But what confirmed me that all was right was, that on Tuesday last, March 21, Mercury appearing very fair, and newly past his greatest elongation, I found by Senex's zodiac that he was nearly in the same parallel that Venus had before described; and though the brightness of the crepusculum effaced the smaller stars, yet in a quarter of an hour I had one past $10'\frac{1}{4}$ more southerly than the planet, which in less than 3^m of time was succeeded by another, which was only 1′ more northerly than the former; when after an interval of about 14 minutes of time, in which I was surprised to find the sky so void of stars, the 4 beforementioned stars passed successively over my glass, with the same interval of time in which I had seen them follow each other on the 28th of February: on which I was desirous to try whether, if the place of Mercury in my tables were assumed, the same right ascensions and declinations of those stars would be deduced from him, as from Venus: and to my great satisfaction, I found, on trial by an exact calculus, that I had the same right ascensions now as before, in none of the 4 differing quite half a minute: so that these stars may securely be added to the catalogue, and the appulse of Mars to them be observed in very long telescopes, in October next, to the further ascertaining the immense distance between the sun and earth.

Hence it will also appear that our mercurial numbers are, at least at this time, and in this part of his orb, not less exact than those of Venus. And whereas this planet scarcely ever appears with us out of the sun's beams, and always low, and therefore under great refraction; this way of observing takes off all the uncertainty thence accruing; and when once the zodiac shall be completed with the stars that are wanting to fill up the vacant places, it will be easy at any time, by this method, to observe Mercury or a Comet within the sun's beams, with the same certainty as if it were remote, and out of the neighbourhood of the horizon, where the different vapours near the earth render the appearances of the stars somewhat dubious, on account of the irregular refractions.

A Proposal for measuring the Height of Places, by Mr. Patrick's Barometer, in which the Scale is greatly enlarged. By the same. N° 366, p. 116.

Since Torricelli first found that the Mercury in an inverted tube was in æquilibrio with the whole column of air over it; and that the weight of the incumbent column was various, according to the different dispositions of the air, in respect of serene fair weather, and of rainy, windy, or otherwise tempestuous weather; there have been several attempts and contrivances to make its minute variations more sensible. And first the wheel barometer was thought of; which certainly shows these variations with great exactness; but it is only proper for a fixed station, and not easy to be removed; which circumstance is necessary for the principal use this instrument is applicable to, and which I would recommend it for.

The next thought for this purpose, was that of Mr. Hubin, described in Phil. Trans. N° 184, who returning the tube of the barometer, as an inverted syphon, made a large dilatation in the ascending leg, where the Mercury ascended, as its altitude in the other part abated, and è contra: over this he drew out a narrow glass cane, which he filled with a tinged spirit, and which being about 15 times lighter than Mercury, would ascend about 15 times as much as the Mercury in the barometer, fell. This, besides that the spirit would dilate and contract itself with heat and cold, had the inconvenience of the former, not to be easily removed without great danger of disorder and breaking, by reason of the smallness of the tube in which the spirit was to rise and fall.

This was succeeded by Dr. Hook's marine barometer, made of two thermometers, the one the common sealed weather-glass, having no communication with the outward air, where the temper, as to heat and cold, was shown by the swelling or shrinking of the included spirit; the other the old thermometer made with an inverted bolt-head, having in the globular part included air somewhat rarer than the ambient, so as to make the liquor, which was to rise and fall in the shank of the bolt-head, always to stand above the surface of the cistern, into which its end was immersed. This showed the heat of the air by its own dilatation; but at the same time the different pressure of the atmosphere mixed with it, so that the graduation of these two thermometers being adjusted to any given height of the Mercury, they would at all times, when the Mercury was at that height, both show the same degree of heat: but at other times, when the weight of the air was different, that difference would show itself by the disagreement of the degree of heat indicated by them. This will be better understood from N° 269, of the Transactions, where I have described this instrument at large. This, though of admirable use at sea, to give timely notice of approaching bad weather, labours under the objection that it supposes the tubes of the thermometers to be exact cylinders, or of equal diameters throughout; and also that, on account of heat and cold, the air and spirit have a proportional dilatation and contraction; the first of which I take to be very hard to be found in ordinary glass canes, and the other I fear still wants to be made out by authentic experiments.

The last contrivance for this purpose, is that of Mr. Patrick, who stiles himself the Torricellian operator, by filling a small glass cane about 5 feet long, and somewhat, but as little as may be, tapering upwards toward the close end of the cane; then inverting it, without a stagnant cistern of Mercury, so much of the Mercury, as exceeds the length of the column the atmosphere can then support, will drop off, and leave its length equal to the then present height of the common barometer. Now when the barometer rises, this length in the cane becomes greater by the Mercury's being pressed up into the upper and narrower part of the tube; and when it falls, on the contrary, it settles down into the wider part, and becomes shorter, being always the same in quantity. By this means, as the angle of the concave cone of glass, of which this tube consists, is smaller, the different situation of the Mercury will, on the alteration of the air's pressure, be nicely shown by very large and distinct divisions.

Now the use to which I would apply this contrivance of the barometer, is to measure by it the different levels of places, too remote to be come at by the ordinary instruments for levelling, with the certainty that may be desired. For this purpose, let there be provided two small glass canes, as near as can be similar, being very little taper or smaller at the closed end, so that being inverted, the Mercury may be suspended in them at the height it ought to have at the time of the experiment. Let that height be duly noted, and then ascending the monument, or some such edifice, where the ascent may be exactly measured, let the scales annexed be divided into parts by the descent of the Mercury, at every 10 feet, in both the pendent barometers, which I conceive may be so chosen as to make the divisions very distinct and sensible. These being thus prepared, when it is desired to take the level of two distant places, let one instrument be fixed in the lower place at a time when the Mercury has the same height as when they were first inverted and graduated; and let the other be carried to the higher place, where it will be found to stand at that division which answers to the elevation of that place above the other, which had before been found by measure in ascending the monument. Thus may 90 feet ascent, which makes only one 10th of an inch of Mercury, be represented by 2 or 3 inches, or a space capable of being divided into 90 parts: whereas, if the distance of the two places be 20 miles, a minute of a degree is equal to above 30 feet; and by the usual sights, whether telescope or otherwise, of the water levels, I fear it will be very hard to convey a true level without a greater error than one minute in the whole.

Observations on the Variation of the Needle made in the Baltic, Anno 1720. *By Mr. William Sanderson.* N° 366, p. 120.

Wednesday, June 1, 1720, being at anchor near Revel, in 58° 58′ north lat. the magnetical amplitude at sun-set was west, 64° 30′ north, and the true amplitude was west, 49° 37′ north; therefore the variation north, 14° 53′ west.

Saturday, July 23, at the isle Gottsand, in 58° 21′ north lat. at sun-set, the magnetical amplitude was west, 49° 50′ north, and the true amplitude west, 35° north, which gives the variation north, 14° 50′ west.

The difference of longitudes of the two foresaid places by the dead reckoning is 1° 50′.

October the 24th at Bornholme, in 56° lat. at sun-rising, the magnetical amplitude was east, 43° 15′ south, and the true was east, 28° 31′, which gives the variation north, 14° 44′ west.

A large quantity of Alkaline Salt produced by burning Rotten Wood. By Mr. Robie, of Harward College, in Cambridge, New England. N° 366, p. 121.

A white oak tree, about 2 feet diameter, is in Cambridge, of so wonderful a nature, that though about a third part of it was decayed, and seemed really to be rotten wood, yet this decayed part in burning would turn almost wholly into a good white alkali, and it would run down into hard lumps, white and clean. On tasting the lumps, it was found to be salt, and very strong. Being dissolved in clean water, and decanted and evaporated without any filtration, we produced a very clean white salt, exceeding in strength and whiteness any to be had at the shops.

Now though alkalis may be extracted from common ashes, yet what was peculiar in this is, 1. That while it was burning the wood itself would melt, and run down into hard lumps of salt, and none of the wood that was sound would do this, but only that which was decayed; and what was most decayed, would yield the greatest quantity of salt.

2. Whereas all other alkalis of wood made thus by incineration are blackish at first, and a lixivium made of them, though often filtered, will still be tinged with a brown colour, occasioned by a kind of coal or ashes so inclosed, or closely united to the alkali in burning, as not easily to be separated by filtration, though often repeated; yet this alkali was very white, even before solution, and when dissolved, the lixivium was not in the least tinged, but clear like pure water, only a very small quantity of ashes subsided to the bottom of the vessel in which the solution was made. The lixivium thence decanted needed no filtration, but when boiled up to dryness, the salt remained fine and white.

3. That in burning this wood, as the heat of the fire became more intense, the wood, as it were, melted and clodded together in great lumps, and visibly bubbled and boiled, with a hissing noise, like the frying of fat in a pan.

4. That whereas the weight of the alkaline salt produced from other wood, in the common way of incineration, is very inconsiderable, in proportion to the weight of the wood producing it; yet this salt nearly equalled in weight the wood from whence it was taken.

5. Whereas the ashes of other wood are never so replete with salt, as that salt can be seen, or in the least cause the ashes to lump or clod together; yet the whole of this would gather into hard and solid lumps of white salt, as easily to be distinguished from ashes, though whiter, as the purest salt of tartar made with nitre would be.

6. That though from other rotten wood much less of an alkali can be pro-

duced than from sound wood, yet here it is quite contrary, the decayed part of this tree yielding in quantity as aforesaid, and the other, or sound part, yielding no more than other wood.

Having given an account of this strange and unusual production, we shall give our thoughts respecting the solution; which we should not attempt, but that being on the spot we have examined the tree, and considered what, by the marks found on it, has, in all probability, happened to it; and therefore suppose ourselves, in some measure, capable of giving as true or truer judgment concerning it than wiser and more ingenious men can be, who have not had those advantages. All which we do with humility and modesty submit to you. In all probability it was struck with lightning many years since, being torn from the top of its trunk to the bottom, on that side which is now decayed, and which yielded the aforesaid salt, there being a channel from top to bottom, about 5 inches wide, as we suppose at first, which the length of time had closed. And under this bark, the wood next to it was black, supposed to be caused by the lightning.

Hence we conjectured, that the wood having been thus exposed to the air and water for so long a time, this was the occasion of its becoming defective in that part; and that the lightning having penetrated the wood, had so altered and disposed the parts and pores of it, the figure and texture of the parts appearing very different from other rotten wood, to attract, receive in, and retain the nitrous salt of the air, which through so long a space of time could not but be in great abundance. Just as salt of tartar, or other alkalis, being exposed to the air for some considerable time, will be wholly reduced to a nitrous salt, as Glauber says, and its quantity also increased very considerably. Not that the lightning had so calcined the wood as to reduce it to a perfect salt, but yet, by penetrating, it had calcined it in such a measure, as to give it a like property or disposition of attracting the nitrous salt of the air, as alkalis of wood that have been fully calcined.

Now if it should be objected, that the nitre in this wood, being volatile, would fly away in burning the wood, we answer, that though nitre cannot be fixed, and reduced to an alkaline salt, by calcining it per se; yet it may be so by the addition of the powder of charcoal. And here we suppose the wood to be so altered by lightning, in which this nitrous salt was lodged, as served instead of coal in the burning of it.

An Account of a Fœtus that continued 46 Years in the Mother's Body. By Dr. Steigertahl, F.R.S. N° 367, p. 126.

Anna Mullern, of the village of Leinzelle, near Gemund in Suabia, of a dry and thin constitution, but otherwise healthful and robust, died at the age of 94, after she had lived a widow 40 years.

Now 46 years before her death she declared herself to be with child, and had all the usual signs of pregnancy. At the end of her reckoning the waters came away, and she was taken with the pains of labour, which continued on her about 7 weeks, and then went off on using some medicines given her by a surgeon. Some time after she recovered her perfect health, except only that her belly continued swelled, and that now and then, on any exercise, she felt a little pain in the lower part of it. She was after this twice brought to bed, the first time of a son, and afterwards of a daughter. But still she was firmly persuaded that she was not yet delivered of what she first went with, and desired her body to be opened after her death. Which being done accordingly, there was found within her a hard mass of the form and size of a large ninepin bowl; but the surgeon had not the precaution to observe whether it lay in the uterus or without it, and for want of better instruments, he broke it open with the blow of a hatchet. This ball, with the contents of it, are represented in figures 10 and 11 of plate 13.

In fig. 10, A shows a part of the integument, which adhered to a spongy fleshy substance; which at first seemed to be a mass of cartilage, but was afterwards found to be entirely bony; BBB CCC is the membranous part, which was bloody; DDDD the opening made by the hatchet; EEEEE another part of the integument, appearing entirely bony, with several prominences; F a contusion occasioned by the rude manner of opening it, where there appeared some putrefied membranes.

In fig. 11, AAA show the integument, or substance inclosing the fœtus; B the fœtus; C a depression, or hollowness on the right cheek; α the nose turning up; β the mouth flatted, but not so wide as it is here represented; γ the eye closed up; δ the ear; DD the arms, of which the right was the larger, and the two joints of that thumb were plainly to be seen; E the protuberance of the knee; F part of the funiculus umbilicalis torn, but still adhering to the navel; GG part of the same funiculus, fastened to the bony part of the integument; H the breast; I the mark of an incision into the left side, where the flesh appeared red, but was dry, and looked like smoked beef.

Fig. 12 represents two ribs from the left side, of their natural substance, colour, and size; AA the part joining to the vertebræ; BB to the sternum.

This piece is preserved in the cabinet of rarities of the Duke of Wirtemberg.

Dr. Camerarius, professor at Tubingen, in a letter upon this subject, takes notice, that the surgeon found this mass in a cavity on the woman's left side, and that it adhered to the membranes of that cavity by the intervention of a spongy fleshy substance. From which particular, and the woman having had two children during the time that this large mass lay in her, he conjectures that it was not lodged in the womb, but in the left Falloppian tube, which by this means had been very much dilated and thickened in its substance.

Observations on the Membranes inclosing the Fasciculi of Fibres, into which a Muscle is divided. By Mr. Leuwenhoeck, F. R. S. N° 367, p. 129.

In cutting off several thin slices from a piece of beef, whenever I cut the fleshy fibres though transversely, I could plainly discover the membrane, as it is commonly called, which runs between and envelopes the fleshy fibres, and especially the larger fasciculi of them, as they run lengthwise along the muscle. Between these fasciculi the membrane is of a considerable thickness, but spreads out every way into ramifications exceedingly small. This membrane is composed of an inconceivable number of very minute vessels, plainly to be discerned, not only where the membrane appeared of some considerable breadth, but even where it was not so broad as a single muscular fibre; but how far this held I could not determine, as these small ramifications of the membrane again spread themselves into other ramifications so exceedingly fine, especially where they inclosed the single muscular fibres, that they were in a manner invisible even through my best microscopes.

The very small vessels which compose this membrane, as it is called, are doubtless framed to convey some nutritious juices, yet they are so small, that the globules of blood cannot pass through them.

ABCD, fig. 13, pl. 13, represents a small piece of the membrane, which with the adjoining fleshy parts, is cut through transversely, and as it was impossible to draw the extraordinary number of vessels which composed it, on account of their exceeding minuteness, they are represented only by points; EFG and HI represent the carnous fibres cut through transversely along with the membranes. These carnous fibres, when moist, lay so close to each other, that the space between EFG and HI was quite filled up; but when dried, the fibres were so shrunk, that one might observe such spaces between them, as in the figure.

Now as we see, on the drying of the membranes AFG and DEG, with the muscular fibres between them, what a number of small ramifications proceed from the membranes, as is here represented between the muscular fibres, we must not imagine that these ramifications proceed only from the points here represented, but that they are continued the whole length of the fibres, and subdividing into still finer ramifications, they inclose every single fibre in the whole muscle.

Among several pieces of flesh, where the carnous fibres were cut transversely, I happened on one piece with its branches so plain, that the membranes and fibres looked like so many boughs of trees, with the leaves on them, as represented at fig. 14, by KLMN, where M shows the membrane torn off from another, as also how many branches it runs into, and the many fibres it covers.

All these carnous fibres, with the membranes, lay very compact together, when cut off from the piece of flesh, as also when laid on the glass, and moistened; but as the moisture dried away, they shrunk again, in the manner here represented, and though the designer could plainly distinguish the small vessels which were cut through, the largest of which appeared at M, fig. 14, yet he was obliged to mark them only with points. Here we may observe, that all the carnous fibres, having been closely tied together by the said membranes, by which they were enveloped, which are nothing but a congeries of vessels, could not be separated from each other on drying, but by tearing those membranes asunder.

The carnous fibres, along with the membrane at KLMN, do not take up so much room, but that a grain of sand may cover it, and yet one might very distinctly observe, in some of those carnous fibres, the parts of which they were composed.

I pursued this observation in the flesh of a whale, of which I had kept two pieces by me for about 7 or 8 years, of about a span long, and 2 inches thick; from these I cut several slices transversely, but found that the carnous fibres so cut through easily separated from each other, so that I could not find my account in this, but thought that the membranes were rotten. I therefore cut off the outside with a table knife, and then with a very sharp knife cut the inner part into very fine slices; and there I found the excrements of mites, which were very small but globular, and some of them as small as I had ever seen before; and thus I found these excrements every where, especially where the membranes were thickest; then viewing the parts where the membranes were thinnest, I discovered in the membranes the aforesaid vessels, and that in as great a number as I had seen them in the ox's flesh, and as distinctly as one can see the holes in a thimble with the naked eye.

After the former discoveries I had made concerning the circulation of the blood, particularly that the blood-vessels had no terminations, I began to consider how the fat particles could be formed, since I did not think that they were separated from the blood and came out of the blood-vessels. But having now plainly discovered, that these membranes were nothing but very small vessels, and believing that they were formed for no other end, but to transport nutriment, as also that there was no circulation in these vessels, I imagined that the matter called fat, was brought into them; which, when there was too great a supply of nutriment, so that it could not be forced farther on, must be driven out of these vessels; for, all the particles of fat I have as yet observed, are inclosed in small films.

This origin of the fat is to me much more credible than that it should be forced out of the blood-vessels; and yet how these fat particles, which consist of small globules, and those out of still smaller globules, are formed, I cannot as yet determine; as also where these vessels, which constitute these membranes, have their origin, and how this fat is conveyed into them.

I had in my drawer a piece of ox's flesh, which I believe had lain there about 4 years, wrapped up in a paper; which piece I found in some places to be covered with a membrane; from this I cut off several small slices along with the membrane; and I found that near the membrane there lay about 16 or 18 nervous fibrils, which, in the drying of the flesh, were so squeezed together, that they were almost twice as long as broad. In some of them I saw very distinctly those vessels which are in the nerves.

These nervous fibrils were inclosed in a kind of half round, separating them from the muscular fibres, and which consisted of a row of small tendinous fibrils, each about twice as thick as a hair of a man's beard. Without these tendinous fibrils lay the muscular fibres, which had been cut through transversely; and in this part of the half round there were several apertures, which seemed in the microscope to be large enough for hemp-seed to pass through them, which might well be taken for vessels, but that there lay so many of them together. But considering that the nerves are commonly covered with fat particles, I concluded that these apertures were no vessels, but mere fat particles, which I found to be true on cutting them through, and discovered that the inner fat was eaten out by the mites, which had left only the husks, or cortices, of the fat globules behind; which cortices I never had as yet been able to discover, because they would, by any heat, melt away as fast as the inner fat itself.

Observations on the Vessels in several kinds of Wood, and on the Muscular Fibres of different Animals. By the same. N° 367, p. 134.

I procured a piece of reddish wood, brought from the island of Amboyna, in the East Indies, and of which cabinets are made, sawed off at the end of a board, as also some of the chips, in order to observe the vessels in it; and cutting the wood through all manner of ways, I found that in one place it appeared whitish, at a small distance red, and in another place blackish. On cutting it transversely, I saw the orifices of the ascending vessels, which ran along the length of the wood, and which appeared of such a size in the microscope, that one would have judged a pea might pass through them. Where the wood looked reddish, I found these large vessels filled with a substance of a fine red colour; so that I imagined these great vessels carried a red sap into the horizontal vessels, which appeared so very numerous, and so thick together, that they caused the wood to appear of the same colour with the red substance contained in them.

I afterwards cut off some very thin slices transversely from this wood, and putting them into a China cup, I poured some hot water on them, and suffered them to lie in it for some time; then viewing them with a microscope, I observed that the red substance was extracted by the water, and no red colour was now to be found in any of the vessels.

What seemed most surprising was, that cutting through the wood lengthwise, as I frequently did, I observed it to be of a fine red colour for a hair's breadth, and a hair's breadth farther it appeared white; and the ascending vessels seemed to be smaller where the wood was red than where it was white; which narrowness of the red vessels I judged to proceed from the sap contained in them.

In viewing the ascending vessels in oak, I found some other vessels which entered into their sides, and appeared like so many small round holes, especially where the horizontal vessels lay, which I judged to be united to the ascending vessels by means of those small orifices, and thereby to discharge part of their sap into them.

Taking a small twig of an oak, which in 7 years growth was about the thickness of a finger, I cut it through lengthwise, both of the ascending and horizontal vessels, which last I saw lying in great numbers very close together, and proceeding directly from the pith of the twig.

I have likewise made some observations on fir wood, in which the ascending vessels consist of so very fine and thin a substance, that they exhibit a very delightful spectacle in the microscope. In these ascending vessels I imagined that I saw some globules, with a small opening in their middle, which seemed to be of a closer and denser substance than the rest of the wood. But I afterwards found myself mistaken, and that these supposed globules were nothing else but the orifices, by which the ascending and horizontal vessels were united together, and through which the sap was carried from the one to the other.

From these observations I turned my thoughts to the fleshy fibres of animals, and began to consider that, since the Author of nature usually observes the same frame and structure in a great variety of his creatures, perhaps the fine membranes, with which every muscular fibre is invested, and which are provided with an innumerable multitude of small vessels, might carry nourishment in the same manner through every carnous fibre in a healthful body. And these small vessels were found to be only the 4th part of a blood globule.

I took also part of the flesh of a whale, which I had kept some years by me, and cut it into very thin slices directly across the fibres, and having moistened these thin slices with fair water, I placed them upon several glasses, and before several microscopes, when I observed that what I had formerly taken for small threads or filaments, were in reality exceedingly small vessels. I then cut part of the whale's flesh lengthwise, in order to discover the vessels, which convey the nutritious juice out of the membranes into the muscular fibres, which vessels then appeared to me in great plenty and very distinct.

I afterwards took another piece of the flesh of an ox, which I cut through transversely, and looking upon it with some of my best microscopes, I could plainly see, that how small soever these fibres were, they were still vascular, for I could see the light through the apertures of these vessels, as I had done before in those of a whale; but if I happened to cut the fibres ever so little obliquely, instead of cutting them directly across their length, the light was not to be seen through them.

I had in a drawer the hind quarter of a mouse, which had lain there some years; from the largest muscle of which, I cut off transversely some small slices, as thin as possibly I could. Then placing these before my microscope, I not only saw, that the carnous fibres were of the same thickness with those of an ox, but I could also see the apertures of the vessels composing the carnous fibres, as plainly as in the flesh of a whale; the vessels in the muscular fibres of a whale are indeed 6 times more in number than in those of an ox, or a mouse, but then the fibre of a whale is also 6 times as thick as the other.

Experiments relating to the Resistance of Fluids. By the Rev. J. T. Desaguliers, LL. D. F.R.S. N° 367, p. 142.

I took a ball of gold, an inch in diameter, that had a little stem of the same metal, with a place on it to fasten a string to; and having suspended it by a silken thread too strong to lengthen by stretching, I made the distance between the centre of the ball and the point of suspension, equal to 12.5 inches; then causing the ball to vibrate in a trough full of water, which had an upright piece of wood in the middle of one side, with pins or keys from which the ball hung, that the centre of suspension might always be in the same place, I observed by looking from a pin on one side of the trough to a mark made opposite to it on the other side, whereabouts the string of the pendulum, just above the surface of the water in which the ball was quite immersed, went to, after 14 vibrations; and by another pin and opposite mark, also observed where it went to, after 28 vibrations. Taking out the water, I filled the trough with mercury, the length of the pendulum, point of suspension and all other things remaining as before: then letting go the ball in the mercury from the same place whence it was let down when the trough was full of water, which was marked by a string stretched across to prevent mistakes, after one whole vibration, it came very little short of the same mark as it had come to in water after 14 vibrations; and when it vibrated twice in mercury, it came to the same place it had done after between 26 and 28 vibrations in water; and this it did exactly several times.

Afterwards filling an upright copper pipe, of 4 inches diameter, with mercury, to the height of 3 feet 10 inches, and suspending the golden ball in it by a short string about an inch long, so as to have the ball just immersed under the middle of the surface of the mercury; I caused it to be let down suddenly, and observing how long it was in falling down to the bottom of the tube, I found that the experiment was disturbed by the ball's striking against the sides of the tube, which retarded the fall of the ball, and the more so the oftener the ball struck: when the ball was least retarded, by not striking at all, it was only $2\frac{1}{2}$ seconds in falling, which must be taken as the true time of the fall of the ball through 46 inches of quicksilver.

I also repeated the other experiments, making the golden pendulum 39.2 inches long, from the point of suspension to the centre of the ball, so as to make it vibrate but once in a second, and then I found that it would vibrate 5 or 6 times in the mercury before the vibrations became so small as not to be observed; and then the first vibration in the mercury ended very near where the 14th in water had done; the second in mercury ended where the 27th in water had done; and observing the third vibration in mercury, it ended exactly at the mark where the 40th in water ended; and this was observed by several persons as well as myself. The specific gravity of the mercury was, by trial, found to be to water, as 13.44 to 1. As to the golden ball, which had varnish and cement on it to keep the mercury from sinking into it, I found it to weigh as follows, viz. in mercury 498 grains, in water 2424 grains, but in air 2577 grains.

An Account of the Poison Wood Tree, in New England. By the Hon. Paul Dudley, F.R.S.* N° 367, p. 145.

The poison-wood-tree grows only in swamps, or low wet grounds, and is something like a small ash, but more like a sumach, and therefore is by some

* Rhus Vernix. Lin.

called the swamp sumach; for the twigs, leaves, and shape, are exactly like the sumach, and it likewise bears a dry berry.

It never grows thicker than a man's leg, nor taller than alder, but spreads much, and several together, especially about the stump or roots of one that is cut down. As it is of quick growth, so it does not last long. The inside of the wood is yellow, and very full of juice, as glutinous as honey or turpentine; the wood itself has a very strong unsavory smell; but the juice stinks as bad as carrion.

This tree poisons two ways, either by the touching or handling of it, or by the smell; for its scent, when cut down in the woods, or on the fire, has poisoned persons to a very great degree. One of my neighbours was blind for above a week together, with only handling it; and a gentleman in the country, sitting by his fire-side in the winter, was swelled for several days with the smoke or flame of some poisonous wood that was in the fire. 2. It has this effect only on some particular persons and constitutions; for I have seen my own brother not only handle, but chew it without any harm at all. And so by the same fire one shall be poisoned, and another not at all affected. 3. But this sort of poison is never mortal, and will go off in a few days of itself, like the sting of a bee; but generally the person applies plantain water, or sallet-oil and cream. 4. As to its operation, within a few hours after the person is poisoned, he feels an itching pain that provokes a scratching, which is followed by an inflammation and swelling; sometimes a man's legs only have been poisoned, and have run with water.

A further Account of the same Tree. By William Sherard, LL.D. R. S. S. N° 367, p. 147.

The poison-tree grows to the size of alder. I never saw the leaf; the wood is as cold as ice; when laid on the fire, of 5 or 6 persons sitting by it, some will swoon, faint, or yawn, continuing so for some days, others but a few hours, and others of the company not at all. I handle, cut and burn it with impunity; and so it is with several others, I suppose, according to their several constitutions. It was never known to kill any body, but only to do hurt to some persons.

Mr. Catesby, in Carolina, calls it a water shrub, of which he never saw leaf or flower. It is a species of toxicodendron, though not named by Dr. Tournefort in his Institutions, p. 610: but I believe it to be Arbor Americana alatis foliis, succo lacteo venenata. Pluknet. Almag. 45. Tab. 145, fig. 1, which is a species of toxicodendron, that grew formerly in Chelsea gardens. In its manner of growing, and alated leaves, it very much resembles the sumach or rhus: the fruit is a white roundish dry berry, growing in clusters, so like that of toxicodendron triphyllon folio sinuato, pubescente, Inst. R. Herb. 611. Hederæ trifoliæ Canadensi affinis planta: arbor venenata quorundam H. R. Paris. as scarcely to be distinguished from it.

An Account of a Method lately practised in New-England, for discovering where the Bees Hive in the Woods, in Order to get their Honey. By the same Mr. Dudley. N° 367, p. 148.

The hunter, in a clear sun-shiny day, takes a plate or trencher, with a little sugar, honey or molosses spread on it, and when got into the woods, sets it down on a rock or stump in the woods: this the bees soon scent and find out; for it is generally supposed a bee will scent honey or wax above a mile's distance. The hunter secures in a box, one or more of the bees as they fill themselves, and after a little time, lets one of them go, observing very carefully the course the bee steers; for after he rises in the air, he flies directly, or on a straight course to the tree where the hive is.

For this purpose, the hunter carries with him his pocket compass, his rule, and other implements, with a sheet of paper, and sets down the course, suppose it be west; by this he is sure the tree must be somewhere in a west line from where he is; but still he wants to know the exact distance from his station. To determine this, he makes an off-set either south or north, suppose north, 100 rods; he then takes out another bee, and lets it go, observing its course also very carefully; for this being loaded, will, as the first, after having mounted a convenient height, fly directly to the hive. This second course the hunter finds, for instance, to be south, 54° west. There then remains nothing but to find out where the two courses intersect, or the distance from B to A, or from C to A, as in fig. 15, pl. 13, for there the honey-tree is situated. But if the course of the second bee from C had been south-west and by south, viz. to D, then the hive-tree must have been there, for there the lines are found to intersect.

The foundation of all this is the straight or direct motion of bees, when bound home with their honey; and this is found to be certain by the observation and experience of the hunters every year, and especially of late years, since this mathematical way of finding honey in the woods has been used with such success.

Note, that all the bees we have in our gardens, or in the woods, and which now are in great numbers, are the produce of such as were brought in hives from England near 100 years since, and not the natural produce of this part of America; for the first planters of New England never observed a bee in the woods, till many years after the country was settled. And in further proof, the Aborigines, the Indians, have no word in their language for a bee, as they have for all animals whatever proper to the country; and therefore for many years they called a bee by the name of Englishman's fly.

Our people formerly used to find out honey in the woods, by surprising and following one bee after another by the eye, till at length they found out where the bees hived. It is remarkable, that when the bees swarm, they never go to the northward, but move southward, or inclining that way. It is also observed, that when one bee goes home from the sugar-plate, he returns with a considerable number from the hive.

Some Propositions concerning the Parabolic Motion of Projectiles, written in 1710. *By Brook Taylor, LL.D. F.R.S.* N° 367, p. 151. *Translated from the Latin.*

PROP. I. *The Force and Direction of Gravity being given; the Path of a Body, projected in a non-resisting Medium, is in a Parabola.*—For let the body be projected from the place A, fig. 1, pl. 14, in the direction AB, and let its path be the curve ACD. At any point in it C, draw CB in the direction of gravity; then the motion of the projectile in AC is resolved into the parts AB, BC, of which AB arises from the uniform motion of projection, and BC from the accelerating force of gravity. Therefore the line AB is proportional to the time from the beginning of the motion at A, but BC is in the duplicate ratio of the same time, as Galileo formerly demonstrated, and therefore in the duplicate ratio of AB. Since then BC is in the duplicate ratio of AB, it follows that the curve ACD is a parabola. Q. E. D.

PROP. II. *The Velocity of the Projectile, in any Point of its Path, is the same as that acquired by a Body falling through an Altitude equal to the 4th Part of the Parameter of the Parabola at that Point.*—For let ACD, fig. 2, be the path. To any point in it A, draw the tangent AB, and the diameter AE. Take AB equal to half the parameter to the vertex A, and draw BC parallel to AE, meeting the curve in C, and to the point C draw the tangent CG, meeting AB in F and AE produced in G. Then, from the nature of the parabola, AG and CB will be equal, and therefore also AF and FB: and since AB is equal to half the parameter at the point A, BC will be the 4th part of the same parameter, and therefore equal to BF. Draw bc very near and parallel to BC, meeting the parabola in c, also draw cβ parallel to Bb, and meeting bc in β. Then since the space cc, and therefore also βc, is supposed very small, the velocities with which they are described will be nearly equable; therefore the spaces cc, Bb or cβ, being described in the same time, will be as the velocities with which they are described; and again the velocities will be as the spaces. Let the points c and c coincide, and then these ratios will be accurate. But in that case, because of the similar triangles cβc, FBC, it is cβ : βc :: FB : BC; therefore the velocities with which Bb and βc are described, are as FB and BC, that is, they are equal. But the velocity by which Bb is described, is that with which the projectile moves at the point A, and the other velocity by which βc is described, is that which a body acquires by falling through the altitude BC, the 4th part of the parameter at the point A. Therefore the velocity of the projectile in any point A, is equal to the velocity which a body can acquire by falling from a height equal to a 4th part of the parameter at that point. Q. E. D.

PROP. III. *Having given the Velocity and Direction of Projection; to find the Trajectory of the projected Body.*—1. Let the body be projected from the place A, fig. 3, in the direction AB. Draw AC in the direction of gravity, viz. perpendicular to the horizon, and of that length which is due to the velocity of projection at A. Draw AF = AC, making the angle BAF = the angle BAC. Draw CD perp. to AC, that is parallel to the horizon, meeting FD, parallel to AC, in D. Bisect FD in E: then EF will be the axis, and E the principal vertex of the parabola described by the projectile. Hence the trajectory will be described by the known properties of the parabola. Q. E. F.

For AC is the 4th part of the parameter at the point A. Hence the rest appears from conics.

2. At any point G of the trajectory, draw GH parallel to AC, and meeting CD in H; then HG is the altitude due to the velocity of the projectile at G, that is, through which a body by falling can gain that velocity. Q. E. F.

This also appears from Prop. 2, and from conics.

Scholium. If to the points A and C, fig. 2, there be drawn the tangents AB and CG, meeting the perpendiculars to the horizon, CB and AG, in B and G; then the velocities at A and C will be to each other, as the intercepted parts, AB, CG, of the tangents.

PROP. IV. *From One Experiment made, to find the Velocity of Projection.*—Let the body be projected from the point A, fig. 2, in any direction AB, and observe the point struck C. In the direction of gravity draw CB, meeting AB in B, and take L a 3d proportional to CB and AB. Then will the 4th part of L be the altitude due to the projectile velocity at A. Q. E. I.

For L is the parameter of the trajectory at A; hence the solution appears from Prop. 2.

Scholium. The experiment will be conveniently made, by erecting AG perpendicular to the horizon, drawing the direction AB bisecting the angle GAC, the line AC being parallel to the horizon. For in this case the required altitude is equal to half the distance AC.

PROP. V. *Given the Direction and Velocity of Projection; to find the Intersection of the Trajectory with a Line passing through the Point of Projection.*—Let the body be projected from the point A, fig. 4, in the direction AB. Raise AC opposite to the direction of gravity, and equal to the height due to the projectile velocity, and draw CE perp. to AC. Make AF = AC, and forming the angle BAF = the angle BAC. Let AK be the line whose meeting with the trajectory is required. Draw FI perp. to AK, and meeting CE in D. Take DE = CD, and draw EK perp. to CE: then will K be the point required.

For, in FI produced take If = FI, and draw fA, fE, FK, FK. Because FIA is a right angle, and If = FI, hence fA is also = FA. But by construc. AF is = AC, and ACD is a right angle. Therefore the points C, F, f are in the circle described with the centre A, and is touched by the line CD. Therefore DF, DC, Df are continual proportionals. But DE is = CD by constr. therefore DF, DE, Df are continual proportionals; and therefore, because of the common angle at D, the triangles FED, fED are similar, and the angle FED = the angle FfE. Therefore the three points F, E, f are in the circle which DE touches at E. But because FI is = If, and FIK is a right angle, the centre of that circle is in the line IK; and because DEK is a right angle, that centre is also in EK. Therefore K is the centre of that circle, and conseq. FK is = EK. Now, by Prop. 3, F is the focus of the trajectory, and CA the 4th part of the parameter at the point A. Hence, since CE is perp. to AC and KE, and FK = EK, the point K will be in the trajectory, by conics. Q. E. D.

PROP. VI. *Having the same Things given; to find the Meeting of the Trajectory with any Line given in position.*—Let the body be projected from the point A, fig. 5, in the direction AB; and let GH be the line to meet the trajectory. Draw CA in the direction of gravity, and equal to the height due to the given projectile velocity; also draw AF = AC, and making the angle BAF = the angle BAC; and draw CE perp. to AC. Draw FI meeting GH at right angles in I, and CE in D; and take If = FI. Take DE a mean proportional between DF and Df; lastly, drawing EK perp. to CE, then K will be the point required. Q. E. I.

Joining Ef, this is demonstrated in the same way as the foregoing.

Scholium.—Because the point E may be taken on both sides of the point D, there are two points, K, k, where GH meets the trajectory.

PROP. VII. *Given the Projectile velocity; to find the Direction to cause the trajectory to pass through a Given Point.*—Let the body be projected from A, fig. 4, and let K be the point through which the trajectory must pass. Draw CA in the direction of gravity, and equal to the altitude due to the given projectile velocity. Draw CE perp. to AC, and KE perp. to CE. With the centres A and K, and radii AC and KE, describe two circles meeting in F. Draw AF, and bisect the angle CAF by the line AB. Then will AB be the direction sought. Q. E. I.

For CA is the 4th part of the parameter at the point A, by prop. 2; and, by construction, AF = AC, and KF = KE; therefore F is the focus of a parabola passing through A and K. And the same will touch AB in A, because of the equal angles FAB, CAB. Therefore the body projected from the point A, in the direction AB, with the velocity due to the altitude AC, will pass through the point K. Q. E. D.

N. B. With the centres A, K, and radii AC, KE, of the described circles, there will be two concourses, F, f; and bisecting the angles FAC, fAC, there will be two directions, which will cause the trajectory to pass through the given point K.

PROP. VIII. *Given the Projectile Direction; to find the Velocity, to cause the trajectory to pass through a Given Point.*—Let the body be projected from A, fig. 6, in the direction AB, and K the given point to be passed through. Draw AK, which bisect in C, and draw BC in the direction of gravity; also join BK. Draw AD, KE parallel to CB; and draw AF, KF, making the angle FAB = DAB, and the angle FKB = EKB. Then will AF be equal to the altitude due to the velocity sought. Q. E. F.

For because BC is in the direction of gravity, it is a diameter of the parabola; and because AC is = CK, BC is the diameter to the ordinate AK. Hence, since AB is a tangent to the parabola at A, KB will be a tangent also at K. Hence, because AD is in the direction of the diameters, and the angle FAB is = DAB, therefore AF passes through the focus of the parabola. In like manner KF passes through the focus. Therefore F is the focus of the parabola, and FA the 4th part of the parameter at the point A, which is thence equal to the altitude due to the velocity sought.

PROP. IX. *To find the Least Velocity, and the Direction belonging to that velocity, to cause the projectile to pass through a Given Point.*—Let A be the point of projection, fig. 7, and K the given point to be passed through. Draw CA, EK in the direction of gravity; and joining AK, bisect the angles CAK, EKA, by the lines AB, KB, intersecting in B. Draw BC perp. to AC; then AC will be the altitude due to the velocity sought, and AB the corresponding direction.

For draw BF perp. to AFK, and CB meeting KE in E. Because of the equal angles BAC, BAF, also BKE, BKF, and the right angles C, E, F, it will be AC = AF, and KE = KF. Hence the points A and K are in a parabola touching AB in A, and whose parameter is equal to 4AC, the focus being F. Therefore a body projected from A, in the direction AB, with the velocity due to the altitude CA, will describe the said parabola, by prop. 2. I say also that is the least velocity, or CA the 4th part of the least of all the parameters, by which the parabola can be described, to pass through the points A, K.

For, if possible, in CA take the less altitude cA, which may be the 4th part of the parameter at A. Draw ce perp. to cA, meeting KE in e, and with the centre A and radius Ac, describe a circle meeting AK in f. Because cA is said to be the 4th part of the parameter at A, the focus of the parabola will be some point p, in the circumference of the circle cpf, described with the centre A and radius Ac. If then K be a point in that parabola, pK will be = eK. But FK is = EK. Hence, since eK is less than EK, pK will also be less than FK. But pK is greater than fK, and fK greater than FK, because fA is less than FA by hypothesis; hence pK is greater than FK. But it was said that pK is less than FK; which is absurd. Therefore no parabola can be described through A and K, that can have a less parameter than in the solution. Q. E. D.

PROP. X. *Given the Projectile Velocity; to find the Direction which shall cause the body to range to the Greatest Distance on a Given Plane; and to determine that distance.*—Let the given plane be AK, fig. 8, and AK the greatest distance sought. Draw CA in the direction of gravity, and equal to the 4th part of the parameter at the point A. Then bisect the angle CAK with the line AB, which will be the direction of the projectile sought. Draw CB perp. to CA, making BE = CB. Then draw EK parallel to CA, so will AK be the greatest distance required.

For with the centre A and radius AC describe a circle, meeting AK in F; and draw BF, BK. Because the angles BAC, BAF are equal, by construction, and AF = AC, then will BF = BC = BE by constr. also the angles at F are right: hence KF = KE. Therefore the points A, K are in a parabola whose focus is F, and which touches AB in A, because of the equal angles BAC, BAF, and having the 4th part of the parameter at A equal to AC. Therefore the body projected from A, in the direction AB, with the velocity due to the altitude CA, will pass through the point K, by prop. 2. Q. E. D.

I say also, that AK is the greatest distance, to which the body can be projected from A, with the same velocity.—For, if possible, let a parabola be described with the same parameter, passing at a greater distance k, from the point

A. Draw Bk, and ke parallel to KE, meeting CE in e. Because BF = BE, and KF = KE, the angle KBF = KBE. But the angle kBF is greater than kBe; hence kF is greater than ke. But because AC is the 4th part of the parameter at A, the focus of the parabola will be somewhere in the circumference of the circle, described with the centre A and radius AC. Let that focus be p, and draw pk. Then because pk is greater than Fk, it will also be greater than ke. But as the parabola passes through k, pk ought to be equal to ke. Therefore neither can a parabola be drawn to pass through a point k more distant than K, nor can the body be projected to a distance greater than AK. Q. E. D.

PROP. XI. *Given the same; to find the Locus of the point* K, *or to describe the curve, which touches all the Parabolas described with the same vertex* A *and the same parameter.*—Let the given vertex be A, fig. 9, and in the direction of gravity draw CA equal to the 4th part of the given parameter. Then describe a parabola, whose principal vertex shall be C, and its focus A; and it will be the curve sought.

For draw any line AK, in which take AF = AC, and draw CB perp. to CA, and let K be the point found in the preceding proposition. In AC produced take cC = CA, and draw ce parallel to CE; also draw KEe parallel to AC. By the foregoing prop. KE is = KF; hence, since also AF is = AC, and cC = Ee by constr. therefore Ke is = KA; hence K is in the parabola described with the focus A and principal vertex C. Q. E. D.

And bisecting the angle AKE by the line KB, this will touch both the parabola described to the focus F through A and K, and to the focus A through K. Hence the parabolas mutually touch each other. Q. E. D.

An Account of the Moose-Deer in America. By the Hon. Paul Dudley, F. R. S.* N° 368, p. 165.

The moose is an animal thought peculiar to North America. There are two sorts, the common light grey moose, by the Indians called wampoose; these are more like the ordinary deer; they spring like them, and herd sometimes to the number of 38 together.

The other is the large, or black moose, which is the chief of the deer-kind. He has several things in common with other deer, and differs from them in several things; but in all very superior to them. The moose is shaped much like a deer, he parts the hoof, chews the cud, has no gall, and his ears are large and erect. The hair of the black moose is a dark grey, and on the ridge

* Cervus Alces. Linn.

of his back is 10 or 12 inches long, of which the Indians make good belts. He has a very short tail.

A stag-moose has been taken, which measured 10½ feet high from the withers; a quarter of his venison weighed upwards of 200 lbs. A doe or hind of the 4th year was killed near Boston, which from the nose to the tail measured between 10 and 11 feet, and wanted only an inch of 7 feet in height.

The horns of the moose, when full grown, are between 4 and 5 feet from the head to the tip; they have 7 shoots or branches to each horn, and they generally spread about 6 feet. When the horns come out of the head, they are round, like the horns of an ox; about a foot from the head, they begin to grow a palm broad, and further up still wider, of which the Indians make good ladles, that will hold a pint. When a moose goes through a thicket, or under the boughs of trees, he lays his horns back on his neck, not only that he may make his way the easier, but to cover his body from the bruise or scratch of the wood. The horns are shed every year; the doe-moose has none.

A moose does not spring, or rise in going, as an ordinary deer, but moves along sidewise, throwing out the feet, much like a horse in a racking pace. One of these large black moose, in his common walk, has been seen to step over a gate, or fence, 5 feet high. After a moose is unharboured, he will run a course of 20 or 30 miles, before he turns about, or comes to a bay; when chased, they generally take to the water; the common deer, for a short space, are swifter than a moose, but a moose soon outwinds a deer.

The flesh of the moose is excellent food, and though not so delicate as the common venison, yet it is more substantial, and will bear salting: the nose is considered as a great delicacy; I have eat several of them, and found them resemble marrow. The Indians say they can travel three times as far after a meal of moose, as after any other flesh of the forest.

The black moose are not very gregarious, being rarely found above 4 or 5 together; the young ones keep with the dam a full year. A moose calves every year, and generally brings two. She brings forth her young ones standing, and the young fall from the dam on their feet. The time of their bringing forth is generally in the month of April.

The moose, being very tall, and having short necks, do not graze on the ground, as the common deer; and if at any time they eat grass, it is the top of that which grows very high, or on steep rising ground. In the summer they feed on plants, herbs, and young shrubs, that grow on the land; but mostly, and with greatest delight, on water-plants, especially a sort of wild colts-foot and lily, that abound in the ponds, and by the sides of the rivers, and for which the moose will wade far and deep, and by the noise they make in the water, the hunters often discover them. In the winter they live on browse, or the tops of bushes and young trees, and being very tall and strong, they will bend down a tree as thick as a man's leg; and where the browse fails them, they will eat off the bark of some sorts of trees, as high as they can reach. They generally feed in the night, and lie still in the day.

The skin of the moose, when well dressed, makes excellent buff; the Indians make their snow-shoes of it: their way of dressing it is thus: after they have haired and grained the hide, they make a lather of the moose's brains in warm water; and after soaking the hide for some time, they stretch and supple it.

Some Remarks on the Allowances to be made in Astronomical Observations for the Refraction of the Air. By Dr. Edm. Halley, R. S. S. Astronomer Royal. With an accurate Table of Refractions. N° 368, p. 169.

Were the medium of our air much more in quantity, or the force of gravity much greater than it is, or in a word, were the refractive power of the air much more sensible than we find it, nothing could have been a greater impediment to discoveries in astronomy: for all objects appearing by refraction higher than really they are, till such time as the laws and quantity of that refraction had been ascertained, it would have been impossible to have been secure of the true observed place of any celestial object. But as it is so little, that none but nice instruments can perceive its effects, it was not discovered to be any at all, till Bernard Walther's time, about the year 1500; nor brought to any sort of rule till Tycho Brahe; nor ascertained, till our worthy president [Sir I. Newton] made the first accurate table of it. The curve which a beam of light describes, as it approaches the earth, being one of the most perplexed and intricate that can well be proposed, as Dr. Brook Taylor has shown in the last proposition of his Methodus Incrementorum.

By this table it follows, that the ratio of the sine of the angle of incidence, to that of the refracted angle, increasing as the beam approaches, makes a very notable difference in the place of an object near the horizon: but in objects that are much elevated, the refractions become small, and their differences scarcely exceed a second per degree; so that they are much the same, as if the incident and refracted angles were on the surface of a sphere of air of the same uniform density, close adjoining to the eye.

When therefore the stars are 20° or more elevated above the horizon, we may take it for granted, without sensible error, that the sines of the true and apparent distances from the vertex, are in the same constant ratio. Hence it will appear that the distances of all the stars are seen less than they really are, in whatever position they are taken, and that not less than a second per degree of the distance; that is, a distance of 30°, for example, is contracted at least so many seconds, and one of 60° no less than a minute, if the distances be taken by an instrument truly divided. So that when Mr. Hevelius, to show the exactness of his observations, brings 8 distances, as taken by his sextant, which exactly complete the circle, both in longitude and right ascension; the consequence is really quite opposite to his design: for if those distances were the true ones, they being all contracted by appearing through a refracting medium, the sum of the 8 differences of both longitude and right ascension, ought to fall short of a whole circle or 360°, by at least 6′; so that I am inclined to believe that the 60° of Mr. Hevelius's sextant wanted about a minute of its true quantity.

Such an allowance as this may perhaps be a proper expedient, to avoid accounting for refraction in celestial observations, provided the objects be nearly parallel to the horizon, or at a good height above it. For all distances of stars are contracted by refraction, when they are parallel to the horizon, by the same constant quantity, be they high or low, that is by about one second per degree; the chords of the arches of the real and visible distances being always in the same ratio as is the sine of the angle of incidence to that of the refracted angle.

And this is the case where the refraction of the air affects the distances of the stars the least, which distances are still more and more contracted, as they are nearer to a perpendicular situation: so that a distance, for example, of 30° loses but half a minute in a horizontal site; but if the one star be 20° high, and the other 50, it will be lessened by above 3 times as much, or by 1′ 41″. If the one be 30 and the other 60° high, the same distance will appear less than 30° by about one minute; the difference still decreasing as the objects are more elevated above the horizon. But in all cases to account for the effect of the refraction on the distances of the stars requires, besides some trigonometrical work, the help of the beforementioned table, which I here subjoin for the use of the curious, such as I long since received it from its great author; it having never yet, that I know of, been made public.

Sir I. Newton's Table of the Refractions of the Stars for their Apparent Altitudes.

Appar. Alt.	Refraction.	Appar. Alt.	Refraction.	Appar. Alt.	Refraction.
0° 0′	33′ 45″	16°	3′ 4″	46°	0′ 52″
0 15	30 24	17	2 53	47	0 50
0 30	27 35	18	2 43	48	0 48
0 45	25 11	19	2 34	49	0 47
1 0	23 7	20	2 26	50	0 45
1 15	21 20	21	2 18	51	0 44
1 30	19 46	22	2 11	52	0 42
1 45	18 22	23	2 5	53	0 40
2 0	17 8	24	1 59	54	0 39
2 30	15 2	25	1 54	55	0 38
3 0	13 20	26	1 49	56	0 36
3 30	11 57	27	1 44	57	0 35
4 0	10 48	28	1 40	58	0 34
4 30	9 50	29	1 36	59	0 32
5 0	9 2	30	1 32	60	0 31
5 30	8 21	31	1 28	61	0 30
6 0	7 45	32	1 25	62	0 28
6 30	7 14	33	1 22	63	0 27
7 0	6 47	34	1 19	64	0 26
7 30	6 22	35	1 16	65	0 25
8 0	6 0	36	1 13	66	0 24
8 30	5 40	37	1 11	67	0 23
9 0	5 22	38	1 8	68	0 22
9 30	5 6	39	1 6	69	0 21
10 0	4 52	40	1 4	70	0 20
11 0	4 27	41	1 2	71	0 19
12 0	4 5	42	1 0	72	0 18
13 0	3 47	43	0 58	73	0 17
14 0	3 31	44	0 56	74	0 16
15 0	3 17	45	0 54	75	0 15

The Variation of the Magnetical Compass, observed by Capt. Rogers, in the Pacific Ocean. With some Remarks on the same. By Dr. Halley. N° 368, p. 173.

Having lately had the opportunity of perusing Capt. Woods Rogers's original journal, who in 1709-10 traversed the great South-Sea, or Pacific Ocean, I was highly pleased to find the care he had taken to set down the variations of the magnetical compass, in his passage from the South Cape of Calefornia, to the Island of Guana, being about 7 hours or 105 degrees of longitude. This might have been long since expected from Capt. Dampier, who had three times made the tour of the world, and thrice gone this very same track.

It is to be wished that the French, who have had frequent opportunities to do it, would give us an account of the variations they have lately found in their voyages from Peru and Chili to China; and that the Spaniards would do the same for the north part of that great sea through which they return from the Manillas to New Spain. With these helps, having three points in each curve, we might be enabled with tolerable certainty to complete the system of the magnetic variations, which I was forced to leave unfinished, as to this part of the ocean, in my general chart, for want of the requisite observations.

The following is the account extracted from Capt. Rogers's Journal, where the first column gives the correct latitude of the place; the second, the longitude west from London, as estimated by reckoning; and the third, the variation, which in this whole tract is easterly.

Variations observed in the Great South Sea, from the South Cape of Calefornia to the Island of Guana or Guam, one of the Ladrones.

January 1709-10	Lat. correct every day		Long. west from London		Variation easterly	
12	22°	16′	114°	9′	3°	0′
	21	18	114	42	2	50
	20	24	115	15	2	50
15	19	25	115	45	2	50
	18	56	116	24	2	45
	18	0	117	6	2	45
	17	11	117	30	2	15
	16	32	118	5	2	0
20	15	44	118	54	1	50
	15	0	120	15	1	30
	14	49	122	5	1	10
	14	36	124	25	0	50
	14	24	126	45	0	40
25	14	14	129	5	0	45
	13	50	131	23	0	50
	13	29	132	58	1	0
	13	29	134	41	1	10
	13	22	136	48	1	15
30	13	27	139	21	1	25
	13	32	142	7	1	30
Feb. 1	13	32	144	37	1	40
	13	36	147	32	1	50
3	13	26	150	18	2	0
	13	26	153	2	2	10
5	13	26	155	19	2	25
	13	26	157	43	2	30
	13	25	160	31	2	50
	13	41	163	0	3	0
	13	41	165	18	3	20

1709-10	Lat. north corr. daily		Long. west from London		Variation easterly	
Feb. 10	13°	44′	167°	26′	3°	30′
	13	36	169	56	3	45
	13	33	172	27	4	0
	13	36	175	0	4	30
	13	32	177	21	5	20
15	13	40	179	28	6	30
	13	47	181	24	7	0
	13	54	183	22	7	30
	13	52	185	37	9	0
	13	40	187	42	10	15
20	13	28	189	49	11	0
	13	21	191	30	11	30
	13	12	193	25	12	0
	13	7	194	37	11	50
	13	10	195	51	11	0
25	13	3	197	51	10	0
	13	0	199	3	9	50
	12	57	200	16	9	30
	12	54	202	20	9	0
March 1	12	58	204	12	8	40
	13	4	206	6	8	20
	13	5	207	33	8	0
	13	5	209	4	7	50
5	13	2	211	54	7	30
	13	7	212	42	7	10
	13	7	214	7	7	0
	13	3	215	28	6	50
	13	8	217	11	6	30
10	13	16	218	27	5	40

Island of Guana in sight.

By this it appears, that at about 250 or 300 leagues west from the south-head of Calefornia, the east variation diminishes to about $\frac{3}{4}$ of a degree; that for 1300 leagues from thence, the same easterly variation gradually increases to about 12°, where it becomes greatest. And that at the isle of Guam, 500 leagues still more westerly, it is again decreased to 5° 40′.

As far as this single instance can direct us, I am apt to think, that in all that space of sea which lies to the northwards of our track, between Japon and Calefornia, there is an easterly variation, which is still greater and greater as the north latitude increases. But that to the southward of our track, and especially to the southward of the equinoctial, a westerly variation arises, of no great extent or quantity, but which is greatest about 1000 leagues west from the coasts of Peru and Chili, about the same meridians where Capt. Rogers found the east variation smallest. This is agreeable to the theory of the variation I laid down in N° 148 of these Transactions, about 40 years since; and I then expressly mentioned, in my 7th remark on the observations there cited, that there was undoubtedly such a tract of west variation in the southern parts of the South Sea, it being the necessary consequence of the site of the four magnetical poles there supposed, though at that time I wanted experiments to prove it.

An Addition to the Description of the Art of Diving or Living under Water. By Dr. Halley. N° 368, p. 177.

In N° 349 of the Phil. Trans. I sufficiently explained the method I had practised and found effectual to furnish air at any reasonable depth under water, and in any quantity desired, for the subsistence of men that shall have occasion to work on wrecks, or otherwise at the bottom, under a great pressure of water. This I did by means of the diving-bell, which, being from time to time replenished with fresh air, I had found sufficient to maintain 5 men for near 2 hours together in 10 fathom water, without the least hurt or inconvenience. But the bell not being to be moved from place to place, unless by moving the vessel from which it was suspended, this was a great impediment to the work to be done below; and therefore I bethought myself how to enable the diver to go out from the bell to a considerable distance, and to stay a sufficient time without it, with full freedom to act as occasion served. And considering that the pressure being greater on the surface of the water in the bell than on any other surface that was higher than it, the air would by a pipe pass from the bell into a cavity of air over that higher surface; I concluded, that putting on a cap of lead made weighty enough to sink empty, and in form resembling the bell itself, I might by flexible pipes, which a man might carry coiled on his arm, receive a constant stream of air from the magazine in the great bell, so long as the surface of the water in the caps was above the level of that in the bell.

Following this idea, I procured pipes to be made, which answered all that was hoped from them. They were secured against the pressure of the water, by a spiral brass wire, which kept them open from end to end, the diameter of the cavity being about $\frac{1}{6}$ part of an inch. These wires we coated with thin glove leather, curiously sewed on, and then the leather was dipped into a mixture of oil and bee's-wax hot, which, filling up the pores of the leather, made it impenetrable to water. We then drew several folds of sheep's guts over them, which when dry, we painted with a good coat of paint, and then secured the whole with another coat of leather, to keep them from fretting. The pipes were about 40 feet long, the size of a half-inch rope; the one end being fixed in the bell, at some height above the water, and the other end fastened to a cock, which opened into the cap. The use of the cock being to stop the return of the air, whenever there was occasion to stoop down, or go below the surface of the air in the bell, which was necessary as often as there was occasion to go out or return into the bell.

The diver therefore putting on his cap, and coiling his pipe on his arm, like a rope, as soon as he is discharged from the bell, opens his cock, and walks on the bottom of the sea, vearing out the coils of his pipe, which serve as a clue to direct him back again; and this I have seen practised without any ill incident attending it.

But there are two things to be remarked in this affair: first, that the weight of a man being very little more than that of his bulk in water, he cannot act with any strength, nor stand with any firmness, especially where any thing of a stream runs, without a considerable addition of weight; and therefore the leaden caps were made to weigh about half a hundred weight, to which I added a girdle of large weights of leads, of about the same weight in the whole, this being to be worn about the waist; and two clogs of lead for the feet, of about 12 lb. each. With this accession of weight I found a man could stand well in an ordinary stream, and even go against it. The other thing necessary to be provided against, was the cold of the water, which though it could not be wholly taken off, so that a man could endure it long, yet it was much eased by habits of waistcoat and drawers, made close to the body, of thick blanket stuff: this being full of water would be a little warmed by the heat of the body, and keep off the chill of new cold water coming on it.

As to seeing under water, as long as the water is not turbid, things are seen sufficiently distinct; but a small degree of thickness makes perfect night, at no great depth of water. In my leaden caps, which from their use I called caps of maintenance, I at first fixed a plain glass before the sight, but soon found that the vapour of the breath would make such a dew on the surface of the glass, that it hindered its transparency; to remedy which, I found it necessary to prolong that side of the cap that was before the eyes, and thereby enlarge the prospect of what was beneath.

An Account of an Aurora Borealis, observed at Dublin, Feb. 6, 1720-1. *By J. W.* N° 368, p. 180.

A Description of an Aurora Borealis, seen on the same Day at Cruwys-Morehard, in Devonshire. By Samuel Cruwys, Esq. F. R. S. N° 368, p. 186.

These two descriptions of the aurora borealis being of the most ordinary kind, as frequently seen, are of no use to be here retained on the present occasion.

Observations on the Muscular Fibres of Fish. By Mr. Leuwenhoeck, F. R. S. N° 368, p. 190.

It has been asserted that nature, in all her various productions, constantly observes the same course and manner of operation. To this assertion my observations by no means agree, neither those that I have made on the generation of animals, and the seeds of plants; nor yet those on the muscles and muscular fibres of different animals, for the muscles of fishes are not provided with any tendons.

After the late discoveries I had made of the small vessels in the muscular fibres of the whale, the ox, the sheep, and the mouse, I was apt to imagine, that the same fabric would hold in the muscular fibres of fish likewise; but as this could not be certainly concluded, I cut a piece of cod fish into small slices, some according to the length of the fibres, and others directly across them, and found, that when I had cut the fibres dexterously through, there appeared in the microscope as great a number of small vessels running along these fibres, as I had formerly seen in the muscular fibres of a whale.

But what appeared to me the most remarkable was, that in a great number of fibres, in which I was not able to discover any vessels running lengthwise, I observed abundance of small vessels, which seemed to proceed from the membranes encompassing the fibres. For in one fibre these vessels appeared to come out of the circumference, or circular tunicle of the fibre, and to pass on to the opposite part of the tunicle; and in another fibre cut transversely, I saw vessels arising from the circumference, and dividing into smaller branches about the middle of the fibre; all which, as far as I could perceive, ended again in the circumference of the fibre. In one fibre I saw at least 50 of these vessels running through each other.

On this discovery, I found I had been mistaken in what I had at first imagined, which was, that the vessels, which arose from the membranes, proceeded no farther than just through the tunicle of the fibre, and so discharged the fluid into the fibre for its nourishment. Whereas now I perceived that the vessels, which arose from the membrane, and entered into the fibre, did not end there, but spread themselves into smaller branches, proceeding every way from the inside to the tunicle of the fibre. Hence I imagined, that the nutritious juice might circulate in these small vessels, just as the blood does in the veins and arteries; and that what the muscular fibres received from them, might be no more than what oozed through the tunicles of these small vessels, as I have said of the small vessels in land-animals, which have no other termination than the artery coming from the heart, and the vein terminating in the heart; the artery and vein thus making one continued vessel.

Yet I could not discern, in the transverse sections of the fibres, any appearance of those vessels which run along their length, and compose the greatest part of the body of each fibre. This I imputed to the cutting of those vessels not directly across, but somewhat obliquely, by which their apertures had been closed in such a manner, that I could not perceive the least resemblance of them.

In viewing an entire muscle of a cod-fish, and the fibres of which it was composed, I found the thick end of the muscle to equal the back of an ordinary knife, and the thinner end not to exceed the thickness of a single fibre. Many of these fibres are twice as long as the thickness of the muscle, and between the muscles lie what are commonly called membranes, which are nothing else but a congeries of vessels. These vessels do not only run between the fibres, but into the very substance of every fibre, as we see, when the fibres are cut transversely. By these vessels the muscular fibres, and the entire muscles themselves are so firmly bound together, that they serve instead of tendons to each other. In like manner the muscular fibres are united to the bones, by the vessels proceeding from the bones, which vessels compose what in land-animals is called the periosteum.

Fig. 10, pl. 14, represents two muscles of a cod-fish, lying close together, as they are united to each other, and separated from the other muscles; the part ABC having been covered with the skin near the head of the fish: and I am of opinion, that the body of the cod-fish, from head to tail, consists of a continued series of such muscles. Fig. 11 represents a single muscle of the fish: where EHG shows the thickness of the muscle; and its thin edge, which is no thicker than the edge of a knife, is marked by EFG. When these muscles had lain several days on a paper, they were not yet dried so hard, but that I could split them into thin shivers, one of which is shown between the letters I and K in fig. 12, in order to show the oblique course of the fibres, which are represented by small lines.

I now turned my thoughts to the River-Fish, and particularly to the pearch; and, as I imagined that an old pearch had no greater number of muscular fibres than a young one, but only that the fibres increased in size during the growth of the fish, and that the larger these fibres were, the more plain and distinct must be the small vessels of which they were composed; I procured one, the largest I had ever seen, weighing $3\frac{1}{4}$lb. and $17\frac{1}{2}$ inches in length, Delft measure, which is the same with the Rhinland.*

I cut off four pieces from this fish in different parts, and viewing the muscular fibres both in length and breadth, I found that the fibres of this great pearch were not so thick as those of the cod-fish. On cutting them through lengthwise, I saw the apertures of the small vessels in vast numbers. I next cut some of the fibres transversely, and found them thinner in this pearch, than in a middling cod-fish, and saw the small vessels, that compose the greatest part of the bulk of the fibre, lying as close together, as ever I saw them in any kind of fish or flesh.

Fig. 13 represents a small portion of these muscular fibres of the fish, cut through transversely, after they were grown dry, and in their shrinking had been torn off from the small vessels that encompass them. The openings of the small vessels in these fibres were distinctly to be seen, but appeared in such great numbers, and were so exceedingly small, that it was impossible to represent them any otherwise than by points. In this figure are represented what we call the membranes, but which indeed are nothing else but a congeries of small vessels, which not only surround the fibres, but enter into their very substance. These, in the drying and shrinking of the object on the plate, had been torn off from the fibres, as may be seen at P, P, P.

I next put a small drop of water, about the size of a pin's head, on this small portion of fibres, into which it immediately insinuated, and swelled them to the same size as when they were first laid upon the plate: after which, they were drawn as they then appeared, but omitting the small vessels, and only showing the circumference of every fibre, as appears at fig. 14. I then split a grain of millet through the middle, and placing one half of it on the glass, near the portion of fibres represented in fig. 13, I observed that the half grain

* Nearly the same as the English.

appeared larger than the portion of fibres. By which one may easily imagine, in how small a space that number of fibres is comprehended, each of which consists of so many vessels.

I likewise made observations on the muscular fibres of a pike, a roach, char, and flounder, in each of which I found the fibres to be composed of small vessels, like those of a cod and pearch. The fibres of a sprat too were but little thinner than those of the large pearch, and the vessels of which the fibres were composed, were nearly as numerous as in the fibres of the pearch.

From these observations some persons may be apt to conclude, that the muscular fibres of land-animals are of the same thickness with those of fish. But for the satisfaction of those who have not seen the objects here spoken of, I have caused a small portion of the muscular fibres of a large ox to be delineated, as they appeared through the same microscope with the former, to show the thickness of the dried fibres, and the vessels that compose them, as is represented in fig. 15. In the transverse section of one of these fibres, were counted 25 vessels in one fibre.

On cutting some of the muscular fibres of a small smelt transversely, I placed them before a microscope, and saw not only that these fibres were twice as thick as those of an ox, but likewise that they were provided with as great a number of vessels as the fibres of other fish.

On thus observing that the muscular fibres of fishes were much larger than those of beasts, I began to consider, for what reasons there was so great a disproportion between them. But all the satisfaction I could meet with was, that as fish swam in the water, their muscular fibres need to exert very little force, in order to support their bodies in it, because they are very nearly of the same specific gravity with the element in which they swim. All the force they exert is in their progressive motion, in pursuit of their food. Whereas the muscular fibres of land-animals exercise a great force, not only in supporting and moving their own bodies, but in carrying burthens and other labour they are put to. And we must allow, that the smaller and finer the fibres are, to make a body of any determinate thickness, the stronger will be the composition, and therefore the muscles in flesh must be stronger than those of fish.

Taking out a little of the mealy substance of a boiled grey pea, I laid it before a microscope, where it appeared to consist of such like parts as are found in rats dung; every one of which parts consisted of a great number of very small particles; but could not discover any membranes enveloping those parts; from whence I concluded, that those membranes were destroyed and dissolved by the hot water. On this, I took another grey pea, which had not been boiled, and cut it into very thin slices; when I not only saw the membranes, in which the parts of the mealy substance had been inclosed, but found also that those membranes consisted entirely of a great number of very small vessels, like the membranes, as they are commonly called, which surround the muscles and muscular fibres in beasts and fish.

Observations on the Seeds of Plants. By the same. N° 368, p. 200.

Having often turned my thoughts to observe the so called membranes, in which the substance of meal or flour is inclosed, like little packets in cells or boxes, which is also the case of all kinds of beans, pease, wheat, barley and other grain; I at length, with astonishment, discovered very plainly, that what I call the membranes, were endued with an unspeakable number of little holes, through which, in many places, one might perceive the light; which holes we must suppose to be nothing else but little vessels, which had been torn or cut off, and which partly compose the membranes, which I call little cells, and which partly serve for the production of the farina, of which there are an infinite number of particles in a pea or bean; which, as small as they are, I imagine that each of those mealy particles receives its increase from a little vessel, which proceeds from the aforesaid cell; and that those vessels are imperceptible through their smallness.

These vessels, of which the little cells or cases mostly consist, are more easy to be discovered in beans and pease, than in any sort of pulse or grain; but in wheat the vessels are difficultly traced in the cells, and I have been obliged to make many observations and experiments before I could fully satisfy myself that I saw the torn or broken vessels; the reason of which is, that the little vessels, of which the cells or skins of the grains of wheat are composed, are exceeding thin and brittle. I have found also, on observing the vessels of which the cells are composed, that several of the globules in wheat were broken in pieces in the operation; and that in one of those single globules, there were other small globules inclosed. I have likewise observed that the membranes, or little cells, in barley, in which the globules or parcels of the meal are shut up, and receive their increase, are thicker and stronger than those of wheat.

Though I conclude, that almost all seeds and grains, as well as their membranes or skins, are of one and the same texture and configuration, yet for experiment sake, I took a large almond, and cut off several thin slices from it, and dug out of those slices, as well as I could, the substance that lay in the little cells, and viewing them as nicely as possible with a microscope, I observed that those cells, in which the oil of the almond was for the most part contained, consisted also of nothing but little vessels.

Often as I have viewed seeds, for several years past, with the microscope, yet I never imagined, that the little cells were endued with so many vessels, though I have frequently considered, how the intrusion of the particles of the meal, or flour, into the membranes was effected; nor should I ever have attained to it, but by continual labour in the investigation of things which are concealed from our naked eyes.

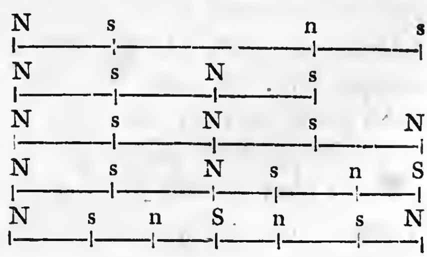
An Account of some Experiments relating to Magnetism. By Dr. Brook Taylor. N° 368, p. 204.

After having given an account of an experiment made with the large magnet in the Repository of the Royal Society, described in N° 344, the Doctor further proceeds on the same subject as follows. If it were known what point within the stone, and what point in the needle are the centres of the magnetical power, it would be easy to find the true powers of the magnet at all the distances observed. For want of that knowledge, I have computed the forces from the centre of the needle, and the extremity of the loadstone, and find, that at the distance of 9 feet, the power alters faster than as the cubes of the distances; whereas at the distances of 1 and 2 feet, the power alters nearly as their squares. To try whether the law, by which the magnetism alters, could be reduced at all distances to any one certain power of those distances, I sought those points in the needle and stone, which being used as the centres of the power, might have that property. But in that case I found the centre of the stone must be carried quite out of its figure, to make the distances large enough for this purpose. From whence it seems to appear, that the power of magnetism does not alter according to any particular power of the distances, but decreases much faster in the greater distances than it does in the near ones.

This seems to be confirmed by other experiments I made. The first experiment was thus: I made a needle $\frac{1}{4}$ of an inch long, of very fine steel-wire, a foot length of which weighed only a grain, which I lengthened by sticking a light piece of rush to it, so that I could observe the direction of the needle in all the trials with a radius of 2 inches. Instead of a magnet, I used a touched needle of steel-wire, which I set on a perpendicular to the horizontal plane I made the observations on, by means of a frame made to transport it from one place to another; the north end of the needle being placed downwards, and made a little sharp, that it might mark the paper it was set upon in every position, by pressing the top of the needle gently with the finger. The observations were made in this manner: after having taken notice of the natural direction of the small compass needle, I brought the perpendicular needle as near to it as conveniently it could, setting it in such a manner, that a line from the upright needle to the centre of the compass might be perpendicular to the compass needle. Then observing the same caution, which was convenient to make the centre of the compass serve sufficiently well to be esteemed its centre of power, I placed the upright needle at several greater distances, every time marking the place in the manner already described, and observing the variation of the compass. By this means I got a curve pretty regularly and fairly drawn by points on the paper. And by examining this curve, compared with the variations of the compass corresponding to its respective points, I found that the magnetical power decreased faster at the greater distances than at the nearer. It is of little use to be very particular in the account of the several observations. I shall only take notice, that at about $2\frac{1}{4}$ inches distance, the force did not alter so fast as the squares, and at 10 inches distance, where the variation was one degree only, it altered faster than the cubes, the index of the power being about $3\frac{1}{4}$. The needle of the compass was so short, that to suppose its centre of force to be either in the middle or at the extremity of it, would not alter the index of the powers of the distances $\frac{1}{20}$ of a unit.

I made another experiment to the same purpose, with a compass needle made of a slight piece of straw, with a small piece of steel-wire fastened to one end of it, which was always kept in the same position, being balanced between two perpendicular needles, one of which was moveable, and the other fixed. The event was much the same as in the former experiment.

Endeavouring to find the true poles or centres of the magnetical power in touched needles, I made a needle of 2 inches long, of the fine steel-wire, which I touched with the south point of a small capped loadstone, applying the point of the cap only to the extremity of the needle, without drawing it along. The needle so touched, being laid gently on the surface of a stagnant water, floated. I then applied to it successively the two ends of a touched needle, as near as I could, without letting the needles touch. The result was, that the floating needle rested under the respective poles of the other needle marked with the small letters s, n, s. So that by one touch with the loadstone, which gave the needle a north-pole at N, where it was touched, it acquired three other poles, s, n, s, which we may not therefore improperly call its consequential poles. Having discovered these consequential poles, I made some other



experiments to discover more of the nature of them, as they are described in the scheme annexed. The needles were each 2 inches long, made of the same fine steel-wire, and the letters N, or n, and S, or s, denote the character of north or south belonging to the points marked; the great letters signifying the points the loadstone was applied to, and the small letters showing the consequential poles.

There are two other experiments described in the same letter, relating to the attraction of fluids, one of which, viz. that of the hyperbola made by the surface of the water between two glass-planes, being already described in N° 336, we shall only give the account of the other, as follows: viz.

I took several very thin pieces of fir-board, and having hung them successively in a convenient manner to a nice pair of scales, I tried what weight was necessary, over and above their own, after they had been well soaked in water, to separate them at once from the surface of stagnating water. I found 50 grains to separate a surface of one inch square; and the weight in every trial being exactly proportional to the surface, I was encouraged to think the experiment well made. The distance of the under surface of the board from the surface of the stagnating water, at the time they separated, I found to be $\frac{16}{100}$ of an inch; though I believe it would be found greater, if it could be measured at a greater distance from the edge of the board than I could do it, the water rising a little before it came quite under the edge of the board.

On the Method of determining the Places of the Planets by observing their near Appulses to the Fixed Stars. By Edmund Halley, LL. D. N° 369, p. 209.

Of all the celestial observations that have hitherto been made, none are so capable of perfect exactness, as the near appulses of the moon and planets to the fixed stars; for though the places of the stars have not as yet attained an ultimate precision, yet these sorts of observations are ever good, and the places of the planets ascertained, in proportion to the correctness of the catalogues that may hereafter be made: but the ordinary number of the stars, with which the planets may be thus compared, being small, the opportunities of observing are consequently rare: whence appears the great use of a full catalogue of telescopical stars, at least within the limits of the zodiac, viz. that these opportunities may be more frequent: and wherever such observations have formerly been made on these small stars, we may be enabled to find them out, and by determining their places, to be certain of the places of the planets also: of which I have given a notable instance in finding the place of the great comet of 1680, in its first appearance, even before it had a tail visible to the naked eye, of which an account is given in N° 342 of the Transactions. And since the Royal Observatory at Greenwich has been put under my care, I have endeavoured to put myself into a condition to supply the many and great vacancies to be met with in the present zodiac; and particularly I have sought out and settled the places of two telescopic stars, to one of which Jupiter was observed to apply by Galileo, at the beginning of March 1610, New Stile, and which is the very first observation of that kind that was made with the telescope. (Nuncius Syder. p. 27, edit. prin. 1610.) On the 28th of February, one hour after sun-set, a small fixed star was in conjunction with the 4th satellite, as it since appears to have been, being then eastwards of the planet. The next day, March 1, at the same hour, the centre of ♃ was in the angle of an equilateral triangle with the 4th satellite and the star: and again, March 2, Jupiter being retrograde, had passed the conjunction of the star, and a line from the star, perpendicular to that of the satellites, fell on the first satellite then 2^m to the west of the planet, and in latitude the star was more southerly than the satellite 8 minutes. This star, by the direction of the place of Jupiter at that time, I found out, and by comparing it with others in the catalogue, having nearly the same declination, I settled its place in ♊ $13° 24\frac{1}{2}'$ to the time of the British catalogue, with $0° 25'$ south latitude.

Another remarkable observation of Saturn is recorded in Riccioli, (Astron. Reform. p. 280) said to have been made at Modena by the Marquis Malvazzo, on July 3, N. S. 1662, when the eastern ansa of Saturn touched a fixed star. By the then place of Saturn, I looked out for this star, to which Saturn is at this time very near, and after the same method I settled its place, the beginning of 1690, the epoch of the British Catalogue, in $29° 34'$ of Scorpio, with $2° 0\frac{1}{2}'$, north latitude. By this it will appear how defective the observed place of Saturn is stated in Riccioli, there being above $7'$ erred in his latitude.

Observation of a Parhelion, Oct. 26*th,* 1721. *By the same.* N° 369, p. 211.

This morning, 26th of October, being on the river coming up to London, about half past 10, the sun being then about 20° high, I observed a circle about the sun, which is by no means unusual, when the air in chilly weather, such as it is now, is replete with snowy particles; which circle was of the size it always appears in, about 23° from the sun, and faintly tinged with the colours of the Iris. When this circle happens, I always look out, to see whether any other of the phænomena that sometimes attend it did at that time appear, such as Parhelia, and other coloured circles, concentric with the sun, and sometimes, as once I saw it, eccentric; as also a white circle round the zenith, in equal altitude with the sun: but this time, the air being thickened with a hazy vapour, and the smoke of the town, I could only see to the eastward a luminous white patch, which for about 20 minutes shone through the thick air very conspicuously, of about 2° diameter, and about the same altitude with the sun: and from it, towards the sun, there seemed to proceed a long white tail, muc narrower than the mock-sun, but which I took to be a segment of the white circle which I once saw entire in London. Had the air been clear, I doubt not but much more of the phænomena of the Parhelia might this time have been observed: and I hope, that from our neighbourhood some member of the Society may furnish us with a fuller relation. But how to explain these appearances, and account for the magnitude of these circles, is what seems still wanting.

*An Account of two Mock-Suns, and an Arc of a Rainbow inverted, with a Halo, and its brightest Arc, seen at Lyndon in Rutland. By the Rev. Mr. William Whiston,** N° 369, p. 212.

About 10 o'clock in the morning, on Sunday, Oct. 22, 1721, being at Lyndon in the county of Rutland, after an aurora borealis the night before, wind

* Mr. Whiston was a very learned, but eccentric divine, and a celebrated mathematician. He was born 1667, at Norton in Leicestershire, where his father was rector. About 1686 he was entered at Clarehall, Cambridge; and in 1693 he became master of arts, and fellow of this college, and shortly after commenced one of the tutors, an employment he was soon obliged to relinquish, on account of ill health. In 1694 having taken orders, Dr. Moore, Bishop of Norwich, appointed him his chaplain, and 4 years after gave him the living of Lowestoff in Suffolk. While with Bishop Moore he published his "New Theory of the Earth, from its Original to the Consummation of all Things;" an ingenious work which brought the author much reputation, though it was refuted by Dr. John Keill.

In 1700 he was appointed Sir I. Newton's deputy, and afterward his successor, as Lucasian professor of mathematics: soon after which, his publications became very numerous, both in theology and in mathematics. Among several other works, he published the first edition of Newton's Universal Arithmetic, which had been left in the university, where the author had used it in his lectures · but this publication being made without the author's wish or consent, it has been said that Sir Isaac never forgave Mr. Whiston for doing it; though by several publications on the Newtonian philosophy, he was one of the first who rendered those principles popular and intelligible to general readers. But some of his theological writings favouring the Arian principles, he was deprived of his professorship, and expelled from the university in 1710. These measures however did not abate his zeal: he retired to London, where he continued such publications as zealously as ever. Here he long struggled against adversities, sometimes reading lectures in philosophy, astronomy, and even divinity, and receiving occasional benevolences, by pecuniary subscriptions among his friends, till his death, which happened in 1752, at 84 years of age.

Mr. Whiston's publications were very numerous, and mostly ingenious, the author having been chiefly unfortunate and blamed for his imprudent though honest zeal. In 1714 he and Mr. Ditton published their scheme for the longitude, which consisted in measuring distances by the velocity of sound, and was so ludicrously stigmatized by Swift. In 1720, he was proposed as a member to the Royal Society, by Sir Hans Sloane and Dr. Halley, but was rejected by means of the president Sir I. Newton. The above paper is the only communication of his printed in the Philosophical

W. S. W. I saw an attempt towards two mock-suns, as I had done sometimes formerly. About $\frac{1}{2}$ or $\frac{3}{4}$ of an hour after, I found the appearance complete; when two plain parhelia, or mock-suns, appeared tolerably bright and distinct; and that in the usual places, viz. in the two intersections of a strong and large portion of a halo, fig. 16, pl. 14, with an imaginary circle, parallel to the horizon, passing through the true sun. This circle I call imaginary, because it was not itself visible, as it sometimes has been at such appearances. Each parhelion had its tail of a white colour, and in direct opposition to the true sun; that towards the east was 20 or 25° long; that towards the west about 10 or 12 degrees; but both narrowest at the remote ends. The mock-suns were evidently red towards the sun, but pale or whitish at the opposite sides, as was the halo also. Looking upward, we saw an arc of a curious inverted rainbow, about the middle of the distance between the top of the halo and the vertex. This arc was as distinct in its colours as the common rainbow; and of the same breadth. The red colour was on the convex, and the blue on the concave of the arc; which seemed to be about 90° long: its centre in or near the vertex. On the top of the halo was a kind of inverted bright arc, though its bend was not plain. The lower part of the halo was among the vapours of the horizon, and not visible. The angles, as more exactly measured on Monday, near noon, when the same appearance returned again, but more faintly, were as follow: the sun's altitude $22\frac{1}{3}°$; perpendicular semidiameter of the halo $23\frac{1}{3}°$; distance of the rainbow from the top of the halo $23\frac{1}{3}°$; semidiameter of the arc of the rainbow, if our vertex be supposed its centre, 21°. The phenomenon lasted each day for an hour and a half, or two hours. What was most remarkable on Monday was, that the wind, which on Sunday had been almost insensible, was now become sensible, and changed to N. N. E. that the halo was sensibly become oval; its shorter axis parallel to the horizon; and the two mock-suns, which were then but just visible, especially that on the east, were not in the halo, but a degree or two without it, which I ascribe to the unusual shortness of the horizontal diameter; which position of the mock-suns does not appear to have been hitherto taken notice of by any, though it was now very sensible.

October 26, about 9 in the morning, as I was coming in the Northampton coach towards London, the halo returned larger and clearer than before; and the two mock-suns just attempted an appearance, as on Sunday; but the air becoming thicker and thicker towards rain, I saw them no more. I add no-

Transactions. A few years before his death, he published Memoirs of his own life and writings, which are very curious.

thing to this account, but only, that Aug. 30, before, I saw at the same place, Rutland, a remarkable halo, whose upper part had its inverted arc reddish within, and pale without, but brighter and more vivid than ever I saw before. That we had there, Sept. 11, in the evening, the lightest and most remarkable aurora borealis, with its unaccountable motions and removals, that ever I saw; excepting that original one, March 6, 17$1\frac{5}{6}$: that it was seen in Northamptonshire, at the Bath, and elsewhere: that the vertex of the columns which shot upwards, was not our vertex, but evidently 15 or 20 degrees distant towards the south; and that the wind was in Rutland north, as I observed myself; at the Bath west; and in Northamptonshire south; all at the same time, which deserves particular reflection. But if any reader expects here the solution of all these phænomena, he is to know, that as to these northern lights, Dr. Halley has communicated his thoughts to the public in the Philosophical Transactions soon after the first appearance; and I communicated mine about the same time in a small pamphlet. And as to the halos, mock-suns, inverted arcs of rainbows, and other phænomena of the like nature, Mons. Huygens has most accurately explained them in his Posthumous Works, from p. 293 to p. 366; and Sir Isaac Newton himself has touched upon them in his Optics, 1st edit. p. 134, to which the inquisitive reader may have recourse for his satisfaction. Only if any inquire further, why the northern lights have of late been so unusually frequent, I must declare, I am far from having satisfied myself.

Observations on the Generation of Plants. By Patrick Blair, M.D. F.R.S. N° 369, p. 216.

What I advanced in my Botanic Essays is now so fully confirmed by experiments made by some curious gardeners, among whom is Mr. Philip Millar, who writes me, Nov. 11, 1721,—1. That in pursuance of my advice he separated the male plants of the spinage from the female; the consequence was, that though the seeds swelled to the usual size, yet when he sowed it, it did not grow afterwards. He searched into the seed, and found it wanted the punctum vitæ, which perhaps might have been the case with Mr. Geoffroy; but if not, the female embryos might have been impregnated another way, as he experimented with 12 tulips, which he set by themselves about 6 or 7 yards from any other, and as soon as they blew, he took out the stamina so very carefully, that he scattered none of the dust, and about 2 days afterwards he saw bees working on tulips, in a bed where he did not take out the stamina, and when they came out, they were loaded with the dust on their bodies and legs: he saw them fly into the tulips, where he had taken out the stamina, and when they came out, he went and found they had left behind them sufficient to impregnate these flowers, and they bore good ripe seed: which persuades him that the farina may be carried from place to place by insects.

I am of opinion this will not suit with Mr. Morland's scheme. For though we may suppose the stamina of every flower to be loaded with a due proportion of the farina, yet this accidental conveyance of it to a neighbouring flower, may be rather less and not greater than is necessary: so that, if wanting, then those embryos which had not received the determined particle into their bosom, must be defective in bulk, or barren in growing, but here all were equally filled.

2. By a second letter, Oct. 19, 1721, Mr. Millar informs me, that he bought a parcel of Savoy seeds of a neighbour, which he sowed, and planted out the plants; but was surprised to see the production: for he had half of them red cabbages, and some white cabbages, and some Savoys with red ribs, and some neither one sort nor other, but a mixture of all sorts together in one plant. He went to the gardener to complain, who showed him that he was in the same condition, but did not know how it should come to pass, for he was sure he took special care in saving the seed. Being asked how and where he planted them for seed, he showed him them under a south-west hedge, and told him the manner in which he planted them: first, a dozen of white cabbages, then a dozen of Savoys, and then a dozen of red. On which Mr. Millar immediately thought this happened by the effluvia impregnating the uterus of each other; and it is very common for our gardeners to plant white and red cabbages together for seed, and they are as often disappointed by having a degeneracy of both kinds, which they attribute to the soil, thinking that is the cause: they send to Holland for a fresh supply of seeds, and say our soil will not continue that sort good.

This experiment is a most convincing argument for the effluvia; for did each grain of the farina enter the pistillum to its proper uterus, this mongrel kind would never be produced. For if the individual plant be in each grain of the male farina, how can it be so far dismembered, as that one part shall go to the making up of the ribs of red cabbage, and another to compose the rest of a Savoy plant.

Analogous to this, is what I lately observed in a spaniel bitch, of so good a kind, that when she became proud, care was taken to let her have good dogs. The litter she produced, consisted of puppies some piebald, like one of the dogs that had lined her, of the same shape, colour and spots; others like another; and a third partaking of both, with spots of the bitch interspersed.

This is a farther confirmation of what I have advanced, Essay 4, p. 310, where I only assert that several fœtuses partake equally of male and female; but here two males concur with one female in the composition of a 4th body, made up of all three: and one seed produces a cabbage consisting of three different species, which could never happen, did these organized animalcula, or granules of the farina, become a fœtus, or contain the folia seminalia of a plant. This methinks is sufficient to answer what the ingenious Mr. Bradley has so strenuously contended for, in his Works of Nature, p. 9, et seq.

It might be useful to consider how far this may lead us into the infinite variegations and stripes, not only in annual flowers, such as poppies, consolida regalis, and bottles, but also in perennial roots; such as auriculas, cowslips, &c. of a lower size, which is hinted by Mr. Bradley, having received that notion from the ingenious Mr. Du Bois, as I have been credibly informed; and in plants of a larger size, not of a bulbous, but carnous root, such as columbines, where there is a vast variety: and in this plant it is most especially to be observed, that though the indigenous one, from which all the other seem only to be variations, and not determinate species, be of a blue colour, consisting of 10 alternate petala, viz. 5 corniculate, and 5 plain; yet into how many other kinds of flowers it is subdivided; such as pale yellow, with bluish red, purple, dark double stripes, blue, blackish red, &c. Some with corniculate petala, and some only with plain; and how in single flowers it imitates all the colours we see pigeons endowed with. I say it is worthy of consideration, whether the farina may do this, since I do not understand there has been much art used in making these flowers break, as tulips, or to cultivate a set of breeders; but that a richer soil may produce a double flower; and a suitable loam may produce the variety of colours; the farina from several flowers may occasion the stripes, and the stamina arising from the plain petala, rather than the cornicula, pouring out the farina, may cause the flowers with the plain petala. So that were I to extend this to a great many other plants, and were there proper observations made on them, considerable improvements might be made in this doctrine of the sexes of plants. For after the flowers, we come next to the variegation of the seed of some plants, particularly the phaseoli, whose various spots and colours, and even the size too, may very much depend on the effluvia from the farina, when several kinds are sown together. For do but consider three plain colours, a white, red, and dark blue, and you may observe how many descendants, and what a variety of spots may proceed from them. The lupines also in some measure may be brought in here; and perhaps the medica cochleata, falcata, lunata, may be multiplied in its variations, after the same manner. But it is time to proceed to another experiment of my correspondent Mr. Millar.

Being persuaded to it by an ingenious gardener, Mr. Millar pulled off all the male-flowers of some melon-plants as soon as they appeared; but instead of finding, as his friend informed him, that these flowers exhausted the nourishment from the fruit; he found that, without these flowers none of the melons would grow, so that he was deprived of the fruit which he expected.

As this experiment is a plain indication of the necessity of the farina, so it confirms the use I have assigned to the leaves, viz. that by entering the capillaries of the leaves, and returning again, the nutritive particles may be more attenuated; so here, the petala of the male-flowers may serve for the same purpose; for by the largeness of the tubuli in these pomiferæ scandentes, a gross viscid sap is received, which even the leaves themselves are not sufficient to attenuate, so as to be fit for composing the more subtile part of the fruit, till by repeated circulation through the petala of the male flowers, it may be rendered fit for such a purpose. Indeed the female flowers on the top of the rudimentum fructus, may in some measure serve for this purpose. But as the male flowers are, generally speaking, more numerous than the female, so their being removed, must deprive the embryos of a very great assistance towards being perfected; I may add, that the orifices of the pedicles, when the flowers are pulled off, must lose so much of the sap, that the whole plant must be thereby so impoverished, as not to be able to bring forth the designed fruit; and all this, besides the want of the considerable supply of the farina fæcundans.

On an extraordinary Height of the Barometer. By Mr. George Graham, Watch-maker, F. R. S.* N° 369, p. 222.

On Thursday, December 21, 1721, observing the barometer much higher

* Mr. Graham was justly esteemed the most accurate and ingenious mechanist of his time. He was born at Gratwick in Cumberland, in 1675. After being a short time in London, he was engaged, on account of his promising talents, by the celebrated clock-maker Mr. Tompion. He invented several astronomical instruments, and improved those before in use, constructing them with an accuracy that had never before been attained. The second mural arch at the Royal Observatory was made for Dr. Halley under Mr. Graham's direction, and the limb divided by his own hand. He invented and made the sector with which Dr. Bradley discovered two new motions in the fixed stars. He comprised the whole planetary system within the compass of a small cabinet; from whence, as a model, all the modern orreries have been derived. He furnished the members of the French Academy, who were sent to the north to make observations to ascertain the figure of the earth, with the instruments they used on that occasion. He was many years a very useful member of the Royal Society, to which he communicated several ingenious discoveries, viz. from the 31st to the 42d vol. of the Philos. Trans., chiefly on astronomical and philosophical matters; particularly a kind of horary alteration of the magnetic needle; a quicksilver pendulum; and many curious particulars relating to

than usual, that evening, between 7 and 8 o'clock, I filled a tube with very clean quicksilver, and found the height a little to exceed 30.7$\frac{1}{2}$ inches. By 8 the next morning, a wheel-barometer, which hung in the same room, had risen $\frac{1}{10}$ of an inch higher than it was the night before, when the experiment was made; at 10 o'clock $\frac{1}{5}$ of an inch more; at which time it was at the highest, being a little above 30.8$\frac{1}{2}$ inches; for about 12 at noon it was sensibly lower, and continued falling all the rest of the day.

When the lower end of the tube was first immersed in the cistern, the quicksilver for some time adhered to the crown of the glass; but upon shaking it fell to the height above-mentioned.

A Caution to be used in examining the Specific Gravity of Solids, by weighing them in Water. By James Jurin, M.D. R.S.Secr. N° 369, p. 223.

That the experiments for finding the specific gravity of solid bodies, should be made with great exactness, if we would so far depend on them, as to draw any inferences from them in natural philosophy, a caution may be useful, which has been but little regarded: viz. that when a dry porous body is to be weighed in water, to discover its specific gravity, it is necessary to extricate the air out of all the small pores and cavities within it, that the water may have free liberty to pervade them. Unless this care be taken, it must needs happen, that the air, which possesses those small cavities, and keeps the water out, will render the solid of less weight in the water, and consequently of less apparent specific gravity than it really is.

The best way of avoiding this inconvenience, is to set the vessel of water in which the solid body is immerged, under the receiver of an air-pump, and to extract the air out of the body by that means; which will be more easily and exactly done, if the water be first heated over the fire. And where the convenience of an air-pump cannot be had, the same thing may be done almost as well, by letting the solid body continue some time in boiling water over the fire. But no solid body must ever be put into hot water, that will in any measure dissolve, or give a tincture to it.

One instance of the neglect of this caution may be seen in the accounts we have of the specific gravity of the stones taken out of human bladders, which have been commonly found to be but about one half, and some of them have been no more than $\frac{1}{4}$ part heavier than an equal bulk of water. From this

the true length of the simple pendulum; on which he continued to make experiments till almost the time of his death, which happened in 1751, at 76 years of age. He was interred in Westminster Abbey, in the same grave as his kind master Mr. Tompion.

it has been too hastily concluded, that these stones are very improperly called by that name, as not at all approaching to the specific gravity of even the lightest real stones.

Whereas it is much more reasonable to suppose, that those stones, which have been found to be so light, were such as had been a considerable time taken out of the bladder, and consequently had lost much of their weight by the evaporation of the urine, with which they had at first been saturated, and that they had afterwards been tried without the caution above-mentioned. I would therefore beg leave to recommend it to those who shall examine the specific gravity of the human calculus, that they will either try the experiment on stones fresh taken out of the bladder, or else to use the abovesaid method, to extricate the air out of their cavities. By doing this, I am confident they will meet with some calculi, as I have done, exceeding the weight of some sorts of burnt earthen-ware and alabaster, and approaching very near to that of brick, and the softer kind of paving stone. But it is not to be expected, that they should quite equal the specific gravity of stone found in the earth; because the mixture of some portion of the animal oil and volatile salt, with the stony substance of the human calculi, must needs lessen the specific gravity of the whole concrete.

I shall mention one other observation relating to this subject; which, however trivial it may seem, yet to me was very surprising, when I accidentally discovered it: viz. that the substance of all wood, as oak, fir, &c. is specifically heavier than water. To prevent being misunderstood, I must observe, that in wood and other vegetables, there are two sorts of vessels, one of which convey the sap, and the other contain only air, for which reason they are called air-vessels. When wood floats, or swims in water, this effect is not owing to the lightness of the substance of the wood, but only to its being buoyed up by the air contained in those vessels. For when the air is extracted out of them, and the water has insinuated itself in their stead, the wood will sink to the bottom. As is very easily shown in small chips, or shavings of wood, by means of the air-pump, or an infusion in boiling, or even in cold water for a sufficient time. And the same is found to succeed in the roots, stalks, leaves, and seeds of as many other vegetables as I have yet tried; cork only excepted; in which last I had no reason to expect it, considering the particular structure of that substance, as described by Dr. Hook, in his Micrographia.

Of an Ossification of the Crural Artery. By Mr. Edward Naish, Surgeon, York. N° 369, p. 226.

Mr. Consett, of Cleveland, Yorkshire, 67 years of age, who had all his life enjoyed a good state of health, sent for me on account of a mortification which had begun about a month before on one of his toes, and by gradual advances in that time had reached half way up his leg: and this without any manifest cause. In such a case, what was to be done? the gentleman saw himself dying daily by piece-meal, but heart-whole, as he expressed it, and had a pretty good pulse. I proposed amputation, as the only remedy, which he readily consented to.

The leg being taken off at the usual place, 4 inches above the mortification, 2 or 3 ounces of blood issued out from the muscular part; but on slackening the tourniquet, in order to look for, and tie the artery, to my great surprise, not one drop of blood flowed out. On feeling the extremity of the artery, I found it hard and callous; however, I secured it by a ligature, as usual, and dressed the stump. The patient, who had borne the operation with the greatest resolution, being put to bed, I was desirous to examine the leg; and having dissected the artery, with its two considerable branches as far as the tarsus, I found them for the most part ossified, that is to say, the trunk, where it was amputated, was ossified about two-thirds of its circumference. About a quarter of an inch lower, the whole was bony, leaving so small an orifice, that it would only admit of a hog's bristle. A little lower it was on one side bony, on the other membranous; then again an entire case of bone. Here and there, for the breadth of a barley-corn, there was no bone at all. I opened about 2 inches of the internal branch immediately above the malleolus, it appearing blacker than the rest; after it had been washed, I found in it 2 or 3 drops of coagulated blood; and now being expanded and dried, it is one entire lamina of bone, as thick as the shell of a pigeon's egg, and of an unequal surface. I dissected 3 ramifications of this internal branch into the foot; only one of which had a very small bit of bone in it, about half an inch from the trunk. The other great branch, that runs on the ligament that ties the fociles together, was not so much ossified.

This ossification, the completest of any I have yet heard of, was doubtless the cause of the mortification, and of the death of my patient, which followed 4 days after the amputation.

This bony shell, or lamina, was contained within the tunicles or coats of the artery. I doubt not but these cases are more common than we imagine. For when we see mortifications seize the extremities of aged people, which we commonly attribute to a decay of nature, or an extinction of the vital warmth; this I believe is often the cause. And I am the more inclined to think so from two or three parallel cases I have met in my practice.

An Account of a Rainbow seen on the Ground. Communicated by the Rev. Benj. Langwith, D.D. to Dr. Jurin, Sec. R.S. N° 369, p. 229.

Sept. 7, 1721, about 9 in the morning, I was riding with some friends over Port Mead near Oxford. The morning had been misty, and the grass was very wet with the dew. We had not been long out, before the air cleared up, and the sun began to shine very bright. We soon after observed a rainbow on the ground, whose colours were almost as lively as those of the common iris, extending for some hundreds of yards; the colours were so strong, that it might have been seen much further had it not been terminated by the bank, and hedge of the field. It continually changed its place as we moved along, as commonly happens in other rainbows. The more remarkable particulars were these.

1. That its figure was not round but oblong, being I conceive a portion of an hyperbola. 2. That the convex part was turned towards the eye, and the vertex at a small distance before us. 3. That the colours took up less space, and were much more lively, in those parts of the iris that were near us, than in those at a distance.

These phænomena may be easily accounted for, by comparing this iris DCE, fig. 16, pl. 13, with the common iris keiE formed by drops falling in the air at a small distance from the eye of the spectator H, and touching the ground with the lower part of its arch in E, the vertical point of the iris DCE. Produce the cone HkiEe, then its intersection with the plane of the horizon will give the figure of the iris DCE. Hence it follows,

1. That as the angle EHG happens to be greater, equal to, or less than 90°, the figure will be a hyperbola, parabola, or ellipsis. 2. That as the sun was about 30° high when we viewed the phænomena, the iris was a hyperbola. 3. That the arches of the same iris, consisting of colours of different refrangibility, may also in some cases be different sections of the cone. 4. That since the angle EHF is always given; from the height of the point of view HG, and the sun's altitude SLA, the dimensions of these irises are easily determined.

A Letter from Mr. Anthony Van Leuwenhoeck, F. R. S. on the Pores or Spiracula of Box-leaves, and on the Down of Peaches and Quinces. Dated Delft, Jan. 15, 1721. *Abridged from the Latin.* N° 369, p. 231.

It occurred to Mr. L. that the leaves of trees might possibly be provided with spiracula; and having in his area two plants of that sort of box which is commonly called palma ceres [palma cereris] he gathered one of the leaves, and dividing it into small pieces, he examined them by the microscope, when he saw very clearly the pores through which the perspiration or exhalation is performed. He also perceived several small hiatuses which transmitted the light. These were seen more distinctly when the pieces of the leaf were become somewhat dry. On examining the leaves of another box-shrub, both in the fresh and dried state, he saw the pores or spiracula more distinctly than he had seen them in any kinds of fruits. Having measured one of these leaves, he found its length equal to $\frac{8}{10}$ of an inch, and its breadth to $\frac{5}{10}$ of an inch. Now, let it be supposed that the figure of such a leaf is oval, then the length and the breadth being added together, the number will be 13, the half of which will be $6\frac{1}{2}$. But let it be supposed that the same leaf, after adding together the length and the breadth, is circular; and that its diameter is equal to $6\frac{1}{2}$-tenths of an inch.

Now, on placing by the side of the aforesaid leaf a hog's bristle, and viewing both together through the microscope, it appeared that 12 pores of the box-leaf, if they lay close together, were equal in length to the diameter of the hog's bristle. But 60 bristles were found equal to an inch; whence it follows, that every 10th part of an inch is equal to 6 diameters of a hog's bristle; and that the half diameter of a box-leaf is equal to $19\frac{1}{2}$ diameters of the said bristles; which $19\frac{1}{2}$ diameters being multiplied by 12, that is by the number of pores, will give 234 as the half length of the diameter of a box-leaf.

Now to calculate the contents of such a circle it must first be observed, that the proportion between the square of the diameter of each circle and the contents of the circle itself, is as 14 to 11. Hence it follows, that on one surface of a box-leaf there are 172090 pores; but as there is an equal number on the other surface, the collective number of pores (to a single leaf) by means of which perspiration and exhalation are carried on, amounts to 344180.

In the concluding part of this communication Mr. L. observes, that the downy hairs on peaches appear to be equal in number to the pores seen on [other] fruit. The down upon a quince is not less than that upon a peach.

Remarks on some Attempts made towards a Perpetual Motion. By the Rev. Dr. Desaguliers, F. R. S. N° 369, p. 234.

The wheel at Hesse Cassel, made by Orfireus, and by him called a perpetual motion, has of late been so much spoken of, on account of its phænomena, that many people have believed it to be actually a self-moving engine: and accordingly have attempted to imitate it as such. Now as a great deal of time and money is spent in those endeavours, I was willing, for the sake of those that try experiments with that view, to show that the principle, which most of them go upon, is false, and can by no means produce a perpetual motion.

They take it for granted, that if a weight descending in a wheel, at a determinate distance from the centre, does in its ascent approach nearer to it; such a weight in its descent will always preponderate, and cause an equal weight to rise, provided it comes nearer the centre in its rise; and accordingly as itself rises it will be overbalanced by another equal weight to it; therefore they endeavour by various contrivances to produce that effect, as if the consequence of it would be a perpetual motion.

But I shall show, that they mistake one particular case of a general theorem, or rather a corollary of it, for the theorem itself. The theorem is as follows:

Theor.—If one weight in its descent, does by means of any contrivance, cause another weight to ascend with a less momentum or quantity of motion than itself, it will preponderate and raise the other weight.

Cor. 1.—Therefore if the weights be equal, the descending weight must have more velocity than the ascending weight, because the momentum is made up of the weight multiplied into the velocity.

Cor. 2.—Therefore if a lever or balance, have equal weights fastened or hanging at its ends, and the brachia be ever so little unequal, that weight will preponderate which is farthest from the centre.

Scholium—This second corollary causes the mistake; because those, who think the velocity of the weight is the line it describes, expect that that weight shall be overpoised which describes the shortest line, and therefore contrive machines to cause the ascending weight to describe a shorter line than the descending weight. As for example, in the circle ADBa, fig. 17, pl. 13, the weights A and B being supposed equal, they imagine, that if, by any contrivance whatever, while the weight A describes the arc Aa, the weight B is carried in any arc, as Bb, so as to come nearer the centre in its rising, than if it went up the arc BD; the said weight shall be overpoised, and consequently, by a number of such weights, a perpetual motion will be produced.

This is attempted by several contrivances, all depending on this false principle; but I shall only mention one, represented by fig. 18, where a wheel with two parallel circumferences, has the space between them divided into cells, which being curved, will, when the wheel goes round, cause weights placed loose in the said cells, to descend on the side AAA, at the outer circumference; and on the side D to ascend in the line Bbbb, which comes nearer the centre, and touches the inner circumference. In a machine of this kind, the weights will indeed move in such a manner, if the wheel be turned round, but they will never be the cause of the wheel's going round. Such a machine is mentioned by the Marquis of Worcester, in his Century of Inventions, N° 56.

The consequence of this, and similar machines, is nothing to a perpetual motion; and the fallacy is this: the velocity of any weight is not the line which it describes in general, but the height that it rises up to, or falls from, with respect to its distance from the centre of the earth. So that when the weight in fig. 17, describes the arc Aa, its velocity is the line AC, which shows the perpendicular descent, or measures how much it is come nearer to the centre of the earth; likewise the line BC denotes the velocity of the weight B, or the height that it rises to, when it ascends in any of the arcs Bb instead of the arc BD; so that in this case, whether the weight B in its ascent be brought nearer the centre or not, it loses no velocity, which it ought to do, in order to be raised up by the weight A. Nay, the weight in rising nearer the centre of a wheel, may not only not lose of its velocity, but be made to gain velocity, in proportion to the velocity of its counterpoising weights, that descend in the circumference of the opposite side of the wheel; for if we consider two radii of the wheel, one of which is horizontal, and the other, fastened to and moving with it, inclined under the horizon in an angle of 60°, fig. 19, and by the descent of the end B of the radius BC, the radius CD by its motion causes the weight at D to rise up the line PP, which is in a plane that stops the said weight from rising in the curve DA, that weight will gain velocity, and in the beginning of its rise it will have twice the velocity of the weight at B; and consequently, instead of being raised will overpoise, if it be equal to the last-mentioned weight. And this velocity will be so much the greater, in proportion as the angle ACD is greater, or as the plane Pp, along which the weight D must rise, is nearer the centre. Indeed if the weight at B, fig. 17, could by any means be lifted up to β, and move in the arc βb, the end would be answered; because then the velocity would be diminished, and become βc.

Experiment.—Take the lever BCD, fig. 19, whose brachia are equal in length, bent in an angle of 120° at C, and moveable about that point as its centre; in this case, a weight of 2 lb. hanging at the end B of the horizontal part of the lever, will keep in equilibrio a weight of 4 lb. hanging at the end D. But if a weight of 1 lb. be laid on the end D of the lever, so that in the motion of D along the arc PA, this weight is made to rise up against the plane PP, which divides in half the line AC equal to CB, the said weight will keep in equilibrio 2 lb. at B, as having twice the velocity, when the lever begins to move. This will be evident, if you let the weight 4 hang at D, while the weight 1 lies above it; for if you then move the lever, the weight 1 will rise 4 times as fast as the weight 4.

A Method for rowing Men of War in a Calm. By M. Du Quet. N° 369, p. 239.

To perfect the art of navigation, two things seem principally wanting, viz. an easy method for finding the longitude at sea, and a way to give a vessel its course in a calm. I flatter myself I have found the last, and hope to make it appear by reason and experiment, that a man of war may make a league an hour in a calm, by means of revolving oars, which are easily applied to the sides of the ship, without occasioning any incumbrance.

A body floats on water, when it weighs less than the volume of water, whose place it takes up; and it sinks more or less in the water, only in proportion as its volume is more or less increased. A body lying in still water is as it were in equilibrio; the least effort gives it motion, and makes it lose that equilibrium. If the effort be continued, though ever so little, the motion it communicates will be very sensible. How great soever the weight of the body be, when once it is in motion, it will always continue so, if nothing hinders it.

On these principles, I consider the motion a vessel receives by means of oars, and the application of hands that set it a-going. The impetus of the hand, applied at one end of the oar, and the resistance the water makes against the other end, are both impressed on the point where the oar rests on the vessel. This point is like the fulcrum of the common lever, which always bears the sum of the weights at both ends, besides the weight of the lever itself; so that the greater the effort is at one of the oars, and the resistance at the other, so much the greater is the impression, which the point or fulcrum receives, in order to its being put in motion. A galley, with two oars only, would go as fast as it does with the usual number, provided the same number of hands were applied with equal vigour to the two oars, and the oars were strong and broad enough to make the necessary resistance; because then the fulcrum of the two oars would receive as much impression as all the fulcra of the common oars taken together.

This consideration put me at first on contriving a way, how to apply a greater number of hands to the common inclined oars; but, after several trials, I threw them aside, and made use of perpendicular ones: because the first only skim the water, and when the sea is rough, and the waves run high, they do not take water often, and so become useless. For in this case the rowers are tripped up, for want of meeting a resistance.

This inconvenience is avoided by the revolving oars; because they take the water perpendicularly, and enter far enough not to miss it: and if the water should happen to evade the stroke, the rowers would not be so incommoded;

because they would be supported at every vibration, which is only of three feet. Besides, in the use of inclined oars, more than half the time is lost in raising and recovering the oar, before they give the stroke; which makes the vessel move by fits and jerks, so that the people aboard feel every stroke of the oars when they play; whereas the revolving oars always move equally, and succeed each other without loss of time; which makes the vessel move uniformly, without affecting those who are aboard.

It is to be observed too, that a galley built on purpose for the use of inclined oars, would not be so proper as another vessel for perpendicular oars; because the galley has a considerable length, and but little height above the water.

The author then states a comparative experiment between his galley with the machine oars, and a common galley, in which their appears to be very little difference between them, either way.

On another experiment, performed by M. Chazelles, a member of the Academy of Sciences, it is stated that the experiment made of the new machine, though defective by reason of the difference with respect both to the crew and the vessels, yet it leaves room to expect a considerable advantage from this invention, in giving the ship way; for though the common galley should keep up with the machine galley at their first setting out, with an equal number of hands; the machine galley will get the better at long run, when the other crews are so fatigued, as to be obliged to row by turns. For here the men will hold out a longer time, their action not being so great, nor so violent. Besides, having only 200 men employed, and being equally manned with the other galley, fresh hands may be supplied, and so they will continue to go at the same rate: for in case of need, the marines may be employed in this service; which they will perform with as little reluctance, or trouble, as they work at the capstan.

The reason of this increase of velocity appears plain, if we consider the difference between the common way of rowing, and that by perpendicular oars: the last is done by an uninterrupted application of force, in the same direction; the other acts by jerks. And, of the three parts of action that are employed, in order to give the strokes; one in raising the oar out of the water, the second in advancing the hands forwards, and the third in pressing against the water; only the last turns to account: and that still loses something of its efficacy; for the crew, by their falling back all together, make the vessel plunge, and render its motion oblique, which contributes very much to its decay.

These are not the only defects of the common oars; for, in order to augment their force, the number is to be increased, and consequently the vessel must have a greater length; by which means it is rendered weaker, and less able to resist the force of the sea. Besides, the vessel must be low-built, and uncovered, and so more exposed to the beating in of the waves, by reason they are obliged to proportion the length of the oar to the strength and size of the men. And though the crew should be under some cover, as they are in a galeass; an opening must be left for the oars to play, by which the waves may beat in.

Both these inconveniencies are avoided by the perpendicular oars; because the addition of force may be obtained, by only applying more hands to the machine; so that with two or three machines on a side, there will be more or less force, in proportion to the number of men employed, and the length of the vessel may be lessened at discretion. And to guard against the sea, another deck may be made, shut close on all sides, even where the axis of the machine passes through.

The chief objections against this invention, seem to me sufficiently obviated by M. Du Quet's Memoir: but though the whole of what is objected should indeed prove, that a vessel made for sailing, as the common galley, would be so incumbered with the machines, as to make the use of sails impracticable; yet if it still holds true, that she will move faster, as appears both by reason and fact, it must be allowed, that a vessel might be so commodiously constructed to carry these machines, as to go as fast as a galley in a calm, and better endure the weather when under sail.

Such a vessel would have several advantages above a galley, both in sailing, and in fight; not to mention the conveniencies of lodging the crew. She may put off to sea any where, and so avoid the dangers attending the coast-winds, which galleys find to be a-head as soon as they have doubled certain capes. With respect to fight, she may mount cannon fore and aft, and on each side; and even mortar-pieces. In time of battle, she would take and maintain her post without assistance, either at the head, or the rear of the enemy's line, and there make use of her bombs: besides the advantages of towing off other vessels from their danger in a calm, and of boarding, or making off from the enemy. And this holds in ships of any rate; provided the length of the oars, the breadth of the pallets, and the strength of the handspikes be proportionable. And the moving force will always be in proportion to the strength and number of the men employed, and not to the number of machines, as in the common oars, which too are impracticable in ships above the fourth rate, by reason of their great length, which will be disproportionate to the ordinary bulk of a man.

By this means the crew will be free from the fatigue of towing, and the vessel will move incomparably faster than if it was towed; because the chaloups which tow, are subject to the inconveniencies of the common way of towing, by losing two thirds of the time; and besides, they cannot act all together: and the vessel that is towed, pulling them back after the oar has made its stroke, they have so much of the space to regain by the next stroke. Besides, the cable by which they tow, sinking into the water by its own gravity, the resistance the water makes to its return, is to be overbalanced; all which circumstances together considerably diminish the towing force.

M. de Chazelles might have added, that the chaloups that tow, are in close fight liable to be sunk by the enemy's cannon, and are exposed to the waves by having so little height above water.

M. Arnoult was ordered to examine the new oars; and he made his report to the court, that the officers of the galleys found, that they interfered with the use of the sails in a galley, but might be of use in other vessels and bomb-ketches.

Part of a Letter from the Rev. Mr. Rowlands, to the Rev. Mr. Derham, F.R.S. Concerning the stocking of the River Mene with Oysters. N° 369, p. 250.

The river Mene, that divides Anglesea from Caernarvonshire, has at present the bottom of its channel, for some miles in length, all bedded with good oysters, in such plenty, that in the season several boats are daily employed to dredge them up, and have done so these 8 or 9 years last past, to their great profit; but what I recommend as observable, is, that about 24 years ago, we have good assurance that there were none to be found on that bottom: but that a gentleman about that time, caused 3 or 400 large oysters to be dropped into the channel, just under his land; from the spat or seed of which, it is most probable, the flux and reflux of tides dispersing it, all the bottom at length, where small stones and a large cultch received the sperm, became covered with oysters. And what favours this conjecture that they are a brood of oysters begun at that time, is, that at the first finding, they appeared young and small, but have since yearly increased in bulk and plenty, though prodigious quantities of them have been taken up.

END OF VOLUME THIRTY-FIRST OF THE ORIGINAL.

The Longitude of Buenos Aires, determined from an Observation made there by Pere Feuillèe. By Edm. Halley, LL. D. F.R.S. N° 370, p. 2. *Vol. XXXII.*

I have, as occasion offered, collected such celestial observations as might be of use to determine the longitudes of places on the sea coasts of the world; in order to get as near as possible the outline, or true figure of the earth, without which our geography of the inlands must necessarily be very insufficient. The Memoirs of the Royal Academy of Paris, afford a good number of observations of this kind, and among the rest, there is one made at Buenos Aires on the river of Plate, in South America, by Pere Feuillèe, in his voyage to Peru: who, in the Memoirs for the year 1711, is said to have observed at that place on the 19th of August, 1708, the immersion of the star in the southern foot of Virgo, marked λ by Bayer, behind the obscure limb of the moon. Being desirous to see what longitude might be deduced from this observation, I soon found that there was a fault in the day, and also in the star; for λ of Virgo was then nearly in 3 degrees of Scorpio, and the moon would not be there till the next day, Monday the 20th of August; and the latitude of λ being about half a degree north, the moon at that longitude would be about 3 degrees more southerly than the star, and consequently far from eclipsing it; for at that time the descending node was in the very beginning of Libra. Hence I concluded it must be some other star, that Pere Feuillèe observed eclipsed by the moon. The day was certainly the 20th, and not the 19th of August, as was evident by the place of the moon; but as to the star, it was neither in the Tychonic catalogue, nor yet in that more copious British catalogue of Mr. Flamsteed; but turning over that of Hevelius, I found a star whose situation agreed well with the observation, and was undoubtedly the star that was seen to immerge behind the moon: the place Mr. Hevelius gives it, allowing the precession of the equinox, was then ♍ $1^\circ\ 56'\frac{1}{4}$, with $2^\circ\ 51'\frac{1}{4}$ south lat. It remained then for me to be assured of the place of this star, and accordingly on the 21st and 24th of December last, I got such observations by help of the circumjacent stars, that I was assured the place of the star, which is a fair one, of the 5th magnitude, was at that time, ♍ $1^\circ\ 58'\ 40''$, with $2^\circ\ 54'\frac{1}{3}$, south lat. being above $2'$ in long. and $3'$ in lat. more than Hevelius gives it. The hour of this occultation is set down precisely $7^h\ 5'\ 38''$ at Buenos Aires, the lat. of the place being $34^4\ 35'$ south. Whence the altitude of the moon there was then $42^\circ\ 48'$, and the parallactic angle $76^\circ\ 38'$, and the parallax in long. $40'\ 11''$ to the west, and in lat. $9'\ 33''$ to the north. So that the moon's observed place, corrected by parallax, was ♍ $2^\circ\ 28'\ 4''$, with south lat. $2^\circ\ 52'\frac{1}{2}$. To this place, by the Calculus of those numbers, I have fitted to our president's theory of the moon, the moon will be found to have arrived August the $\frac{9}{20}$ at $10^h\ 57^m\ 36^s$ apparent time at London. But at Buenos Aires it was then computed but $7^h\ 5^m\ 38^s$; whence the difference of longitude, resulting from this observation, is $3^h\ 52^m$ or 58°, more westerly than London. I have twice repeated the calculation, to be sure to avoid error, and by comparing my chart of the variation with the longitude thus found, it appears that in this case a ship at sea using those tables, and that chart, would by an observation of this occultation have fallen with greater exactness on the coast of America, than by any reckoning can be pretended to be done.

A Description of an Engine to raise Water by the Help of Quicksilver, invented by the late Mr. Joshua Haskins, and improved by J. T. Desaguliers, LL.D. F.R.S. N° 370, p. 5.

Mr. Haskins finding that all hydraulic engines, working with pumps, lose a great deal of water, always giving less than the number of strokes ought to give according to the contents of the barrels; and that when the pistons are new leathered to prevent that loss, the friction is much increased, and the engines are subject to jerks, which in great works often disorder them for a great while, by breaking some of the parts; contrived a new way of raising water without any friction of solids; making use of quicksilver instead of leather, to keep the air or water from slipping by the sides of the pistons in the barrels where they work; hoping thus to prevent all the abovesaid inconveniencies, and also to have water engines less liable to be out of order than any yet made.

The first experiment he made with an engine that he set up at my house about two years since, the description of which is as follows:

In fig. 1, pl. 15, dddd represent a lignum vitæ plug or piston, which Mr. Haskins called a plunger, about 6 feet long, made heavy enough with lead at top to sink into mercury, which is beforehand poured into the barrel D1 D2 up to mm. The chain E1 E2, joined to the piston and the power that moves it, being let down till the piston comes to D2, the mercury rises to the same height in the barrel, and in the receiver R, which it fills, namely to nn, as appears in the figure. Then drawing up the piston till its bottom is come to mm, the mercury coming out of the receiver down to oo makes a vacuum, and the weight of the atmosphere causes the water to rise up through the sucking pipe A1 A2, and valve V, into the receiver where the mercury was before. On letting down the piston again, the mercury rises into the receiver, and drives up the water through the elbow B, the forcing valve u, and so up the forcing pipe a1 a2: but when once the forcing pipe, which here was 46 feet high, is full, before any mercury can enter into the receiver, and force any water out at the top of the pipe a1, the mercury between the piston and barrel must rise up to qq, near $3\frac{1}{2}$ feet above the bottom of the receiver; and as it continues to rise up to pp, the water is thrown out with a velocity proportionable to the height that the mercury is raised above the 14th part of the height of the water. Now though the friction of solids is here avoided, it is plain that the mercury must move from mm to qq without raising any water; and that it can only force in going from qq to pp, and only suck in falling from oo to mm: and unless the piston is stopped a little while when at lowest, the water will not have time to run out: so likewise the piston must be stopped when at highest, that the receiver may have time to fill.

Mr. Haskins likewise proposed another way, represented in fig. 2; where the same letters represent the same parts, only here the barrel is moveable by the two chains E1 E2; and instead of a solid piston, the hollow cylinder c1 cc is fixed, and the mercury moving up and down in the lower part of it, sucks and forces the water through the elbow. The figure represents the engine sucking, by means of the mercury hanging from oo to mm. In order to force, before any water can be driven out, the mercury in the inner cylinder must descend from oo to mm, and rise up to pp between that cylinder and the barrel; so that here also a great deal of time is lost; besides the great quantity of mercury used, which is very expensive; because as much mercury is moved every stroke as the water raised.

These difficulties very much puzzled Mr. Haskins, and quite discouraged some other persons who had got the secret of the invention, and were setting up against him. But when I had considered the matter a little, though I had not time to contrive a machine for it, I told him, that a little mercury might be made to raise a great quantity of water, and there should not be such a loss of time as in his engines; but that I would have him find it out, before I assisted him farther. In a little time he found out the contrivance represented in fig. 4; and afterwards that of fig. 3; which last was what I had thought of: and both these were also found out by the late Mr. William Vreem, who was an excellent mechanic.

In fig. 3, the barrel is moved as in fig. 2, but the plug ddd taking up a great deal of space, there is occasion for no more mercury than what will make a concave cylinder or shell up to pp, between the barrel D1 D2, and the hanging cylinder c1 c2 cc, when the stroke is made for forcing; and a concave cylinder between the plug and c1 c2 cc, when the suction is made.

In fig. 4, the barrel with a third cylinder dddd, instead of the plug of fig. 3, is lifted up and down every stroke, and the water passes through dddd, the mercury making a shell sometimes between the middle and inner cylinder, as in the suction; and sometimes between the barrel and the middle cylinder, as in the forcing stroke.

Mr. Haskins had contrived such a machine as is represented by this 4th figure, and bespoke the several parts before he died; and therefore when I was desired by his assignees to direct the setting up the machine, I was obliged to make use of the pieces already made, in order to save the expence of a new engine: and now the whole put together with some alterations, make the engine represented by fig. 5, as it is set up at my house in Westminster, and by the force of one man, raises a hogshead of water in little more than a minute and a half to the height of 27 feet. All the fault of the machine of fig. 5 is, that the pendulum handle Ff is too long, and the bottom of the middle cylinder c ought to be just in the middle of the height to which the water is to be raised, supposing three copper cylinders to be as they are here: if likewise the barrel D1 D2 worked under the forcing pipe, the lift would be easier. Therefore I describe the machine with the small alteration represented in fig. 6.

The sucking and forcing pipe and valves are marked with the same letters as in the other figures; and the chains E1 E2 must be supposed to hang from such pullies, and to be moved by such a pendulum as in fig. 5. The barrel D1 D2, otherwise called the outer cylinder, and represented by the same letters in fig. 7, has within it another cylinder, called the inner cylinder or plug, as dddd fig. 7, between which two cylinders a certain quantity of mercury is poured in, and the hanging cylinder c coming down into the mercury, a stroke of 13 inches may be made by the motion of the barrel, which, in going down sucks by making a vacuum in c, and in going up forces the water out of the top of the forcing pipe, performing the office of a common piston; only that instead of leather to make it tight to the cylinder c, there is always a thin shell of quicksilver, either between the middle cylinder c and the inner one, dddd fig. 7, as happens when the suction is made, or between the middle and outer cylinder, as happens in lifting up the barrel to force. In the suction, the mercury is higher in the inner shell than in the outer, by a height equal to little more than $\frac{1}{14}$ part of the height of the barrel above the water to be raised: and in forcing, it is higher in the outer shell than in the inner, by little more than $\frac{1}{14}$ of the height of the column of water to be forced. And therefore if the water is not required to be raised above 64 feet, the barrel should move so as to make the middle of its stroke at the height of 30 feet, or at the middle of the way from the water to be raised, to the delivery at top.

The 7th figure, drawn by a larger scale, represents the three cylinders, which are here made of copper in their just proportions: and for the sake of those that would consider this matter fully, I have here given their lengths, diameters within and without, and thickness.

	Outer Cylinder or Barrel, D1 D2.	Middle or hanging Cylinder, in which the Stroke is made C1 C2 cc.	Inner Cylinder or plug closed at top by a Cap, and moving up and down with the Barrel to which it is joined at bottom. dddd.
	Inches	Inches	Inches
Length	30	29.0	31.2
Diameter within	6.74	6.35	6.03
Thickness	0.10	0.08	0.13
Diameter without	6.94	6.51	6.29

Here BB represents part of the elbow of fig. 5, or of the forcing pipe of fig. 6. But as the spaces between the cylinders are so small, as not to be visible even in a large draught made by a scale; I have here given three more draughts of the three cylinders, where the height is according to the scale of the 7th figure, but the diameters of the middle and inner cylinders are made less than they are in the engine, to make the space between, where the mercury rises and falls, visible; and the cylinders themselves are represented by single lines.

The quantity of mercury used in this engine, is $36\frac{1}{2}$ pounds, which being poured in between the outer and inner cylinder, rises up to the height of 16 inches.

When the barrel is pulled up, as in fig. 9, so as to have the middle cylinder within an inch of the bottom of the barrel; the mercury on both sides the middle cylinder will rise up to the height of 23.1 inches, that is about 2 inches below the cup D1, to the line qq.

When the barrel is going down, to fill the sucking pipe and middle cylinder c, the mercury in the inner shell will be 25 inches high, and only 13 in the outer shell, fig. 9, where the shaded part represents the mercury.

At the end of the sucking stroke the mercury is up to the top of the inner cylinder, and scarcely an inch in the outer shell. Fig. 8.

In raising the piston from forcing to sucking, the first $1\frac{1}{4}$ inch drives the mercury out of the inner shell, and raises it in the outer 13.28 inches.

The depth of an inch of water in the middle cylinder, above the inner one or plug, is equal to a space in the outer shell of 13.28 inches, and $\frac{1}{4}$ inch is equal to the same height in the inner shell.

Therefore, when the mercury is equally high in both shells, a motion of $\frac{1}{4}$ inch of the barrel will charge for suction. That is, on letting down the barrel only $\frac{1}{4}$ inch, the pressure of the atmosphere in the outer shell will raise the mercury in the inner, 13.28 inches, at the same time, that it pushes up the water from the well $13\frac{1}{2}$ feet high, into the sucking pipe. And when all the pipes are full, if the mercury be equally high in both shells, on raising the barrel one inch, the mercury will rise 13.28 inches in the outer shell; which I call charging for forcing; because in continuing to raise the barrel, the forcing valve immediately rises, and the water comes out at top during the rest of the stroke, which is 12 inches, and delivers 1.6 gallon of water, wine measure.

Fig. 10 represents the forcing stroke half way up; with the mercury 17 inches in the outer shell, 4 inches in the inner, and the whole space at bottom under the middle cylinder 7 inches.

From this it appears, that in the whole stroke of 13 inches in length, there is only $\frac{1}{4}$ inch lost, to charge for suction, and in the next stroke, which is likewise of 13 inches, there is only one inch lost, to charge for forcing; so that in a motion of 26 inches, there is but $1\frac{1}{4}$ inch, or about $\frac{1}{20}$ part ineffectual. But this is owing to the too large space of the outer shell, which contains 4 times more than the inner one, because the cylinders were only hammered, and not turned; for if the outer space had been no larger than the inner, then $\frac{1}{4}$ inch of the stroke would have charged for forcing; so that only $\frac{1}{2}$ inch in 26, or $\frac{1}{52}$ part of the whole stroke would have been ineffectual; and in that case, $\frac{2}{5}$ of the quantity of mercury, or a little more than 12 pounds, would have been sufficient.

There may still less mercury be used, if the middle cylinder be made of plate iron turned on the outside, and bored within, the outer cylinder bored, and the inner one turned; so that if the work be well performed, 8 or 10 pounds of mercury will be sufficient in this engine, though the bore of the middle cylinder, or diameter of the column of water which is raised, be of 6.35 inches. If the bore of the said cylinder were only 3 inches, less than 3 pounds of mercury would suffice, and less than 6 if there were two barrels, in order to keep a constant stream through a pipe of almost the same diameter. This will very much lessen the expence of mercury, which would otherwise be an objection against this engine; and by making the inner and outer cylinder of hard wood, as box, or lignum vitæ, the cost of the engine may still be reduced. But if the engine be very large, cast iron bored will be proper for the outer cylinder, and cast iron turned on the outside for the inner cylinder or plug, and hammered iron bored and turned for the middle cylinder.

There is an objection, which seems at first to take off the intended advantage of this engine, which is this, viz. that instead of the friction of the leather of a piston, when we lift up our barrel to force, the resistance, that the mercury finds to rise in the outer shell, is at least as great as the friction that we avoid. Now that resistance is never greater than the weight of a concave cylinder of mercury, whose height is the greatest to which the mercury rises in the said shell, and the base is the area of the shell itself. This weight in our engine is equal to 57.5 pounds, and therefore one would think it greater than the resistance made by the friction of a piston. But if it be considered, that in the descent of the barrel for sucking, the mercury shifts immediately into the inner shell, rising to the same height, and still keeping the same base; the aforesaid weight of 57.5 pounds helps down the barrel, and facilitates the overcoming of the force of the atmosphere, consequently the weight of the mercury, being balanced, is no hindrance, whether you work with a single or a double barrel.

There remains then only the hindrance by loss of time in the beginning of any stroke: but I have showed that to be but $\frac{1}{52}$ part of the stroke. I have found that the best engines now in use generally lose near $\frac{1}{5}$ of the water that they ought to give, according to their number of strokes. And Mr. Henry Beighton, an ingenious member of this Society, having a great many times measured the water that is raised by engines in mines, found that some engines lost $\frac{1}{4}$, and none ever lost less than $\frac{1}{5}$, of what they ought to give, according to the number of the strokes in their pumps, whatever auxiliary powers they were moved with.

There is indeed another objection, but scarcely worth notice; which is, that some particles of mercury will mix with the water that is raised, and make it unwholesome; but nobody that considers specific gravity, will imagine any such thing. However, to satisfy those that might still apprehend it, it is to be observed, that none of the water that is raised comes near the mercury: for in the cylinder C, and part of the elbow B, fig. 5, there is always above the mercury a certain quantity of water, that rises and falls with the barrel, and never goes into the forcing pipe. The same happens also in the machine of fig. 6; for the water having once run into the cylinder C, all that is raised afterwards, comes through the forcing valve, without coming down to the mercury.

Provided care be taken to make the barrel with its plug tight, I do not see that this machine will want repair in a long time, except some of the auxiliary powers be out of order, which do not relate to this invention. The numbers given will serve to examine the truth of what I have asserted concerning the motion of the mercury: and from them one may make tables to serve to proportion these engines for raising any quantity of water to any height, according to the power to be applied.

Account of the coming off of the Scapula and Head of the Os Humeri, on a Mortification. By Mr. Peter Derante, Surgeon at Waterford. N° 370, p. 15.

Nov. 5, 1713, John Fletcher, on board the Neptune of Liverpool, had the misfortune to break the radius and ulna of his left arm, and their ends burst through the skin. He was immediately dressed by the surgeon of the ship with the common astringents and bandages. About five days after, I was sent for to see the patient; when on taking off the dressings, I found it black and insensible from his fingers to his shoulder, and therefore advised the extirpation of it immediately; but his surgeon opposed it: however, I scarified it in several places, very deep, and then applied a warm dressing. Next day the ship put to sea, and the patient was sent to Waterford, and committed to my care. As soon as I could get my apparatus ready, I cut off his arm as high as possible; I then cauterized the stump, which was perfectly mortified as high as the acromion. Next day I perceived the mortification spreading toward the lower angle of the scapula: I then rubbed the edges of the mortification with armed probes dipped in a solution of argentum vivum in aq. fort. which completely answered my intention: for from that time the mortification spread no further. Next dressing I scarified and cauterized all the mortified part, and dressed it secundum artem. I continued this method for 17 or 18 days, and then the sloughs began to separate and cast off daily. Some time after the scapula began to part from the os humeri and clavicula; and at length came out entirely. The stump of the os humeri still adhered to the pectoralis and latissimus dorsi; but in a little time it also separated and came away, without any hæmorrhage succeeding. I was afterwards obliged to cut off part of the clavicula, before I could cicatrize the wound, which was soon afterwards accomplished.

*An Account of a great Number of Hydatids found in the Abdomen. Communicated by John Thorpe, M. D. F. R. S.** N° 370, p. 17. *An Extract from the Latin.*

In this communication an account is given of a person, aged 58, who was affected with an enormous swelling of the abdomen, resembling ascites; yet from the appearance of the urine, the absence of œdema of the legs, and the freedom of respiration, it was doubtful whether the distension was occasioned by water or not. For the space of 6 months various remedies were employed (without success) to remove the swelling; which at length increased to such a

* This account was transmitted to Dr. Thorpe from Paris, by Barthol. Anhorn, de Hartniss.

degree, that it was judged necessary to resort to the operation of paracentesis. Accordingly the perforation of the abdomen was made in the usual way; but it was found that the morbid contents were of so gelatinous a consistence that they would not pass through the cannula. It was therefore necessary to dilate the perforation with a lancet; after which there was evacuated a large quantity* of a dirty coloured fluid, thicker than the white of an egg, and containing a vast number of white lumps of various shapes (viz. spherical, triangular, vermicular, &c.) and from the size of a filbert to that of a pigeon's egg, and even larger. These white lumps gave a resistance when pressed with the finger, and were evidently included within a membrane or pellicle. When broken, there flowed from them a white liquor resembling chyle. One large lump, which looked like a portion of the omentum, and was an inch in thickness, was wholly membranaceous. The number of lumps, large and small, amounted to 7000 or 8000.

It is added, that on the 16th day after the operation, the patient was doing well.

An Account of some Experiments made with the Bile of Persons dead of the Plague at Marseilles, with what appeared on the Dissection of the Bodies; also some Experiments made with the Bile of Persons dead of other Diseases. By Dr. Deidier,† Professor of Physic at Montpellier. N° 370, p. 20.

Experiments made upon the Bile of Persons dead of the Plague. Exper. I.—The human bile, taken from the gall-bladder of the bodies of those that died of the plague at Marseilles, was always found to be of a black and greenish colour. It became constantly of a lasting grass green on mixing spirit of vitriol with it; and always very yellow when mixed with oil of tartar per deliquium, or the alcalous fixed salt of the same, dissolved in a sufficient quantity

* There were evacuated on the 1st day of the operation 6 mensuræ of gelatinous matter; on the 2d day, when the aperture was further dilated, the same quantity. And there continued to come away more or less of the said gelatinous matter for the space of 13 days; but how much in the whole is not mentioned.

† Dr. Deidier was fond of starting hypotheses calculated rather to amuse than convince. Thus, in his Tractatus de Lue Venereâ, he maintained that the venereal disease was produced by animalcula, and that mercury effected a cure by destroying them. In his observations on the plague, he represented the pestilential contagion to be of an acid nature, and that the blood was coagulated by it, &c. &c. The experiments which he made with the bile of persons dead of the plague, present, it must be confessed, some curious results; but they do not appear to be of the smallest use, in a pathological or practical view.—Besides the tracts already mentioned, the following are among this author's principal works: viz. Institutiones Medicinæ Theoreticæ; Traité des Tumeurs; Consultations et Observations Medicinales; and Matiere Medicale.

of water. These 2 colours, green and yellow, have continued for whole months. The bile became of a black colour, like ink, but soon fading by the affusion of spirit of nitre.

2d. The bile, taken from the gall-bladder of the bodies of those that died of the plague, having been poured into a wound, made on purpose in different dogs, rendered them presently melancholy, drowsy, and indifferent about food. All these animals died in 3 or 4 days, with the essential marks of the true plague; as buboes, carbuncles, and gangrenous inflammations in the viscera, in the same manner as in the human bodies from whence the bile was taken.

3d. A drachm of the said pestiferous bile having been mixed with 2 oz. of fountain water, made lukewarm, and injected into the jugular vein of several dogs, rendered them presently drowsy, and killed them in 4 hours, with gangrenous inflammations, the heart stuffed full of black thick blood, the liver swelled, and the gall-bladder full of green bile.

4th. The same quantity of bile, injected by the crural vein, produced in the dogs a heaviness in about an hour. They had such a strong loathing of their food, that they would not eat or drink the least matter after the injection was made. They frequently made water, especially if they were stirred. On the 3d day there appeared considerable tumors under the axilla, and on their thighs, about 3 inches from the wound. The wound turned to a gangrene, and the animal usually died on the 4th day, with all the signs of the plague.

5th. A dog, at the hospital at the Mail, in Marseilles, who followed the surgeons when they went to dress the sick, used greedily to swallow the corrupted glands, and the dressings full of pus, taken off the plague-sores; he licked up the blood that he found spilt on the ground in the infirmary; and this he did for about 3 months; and yet he was always well, gay, brisk, full of play, and familiar with all comers. We injected into the right crural vein of this dog about a drachm of the pestiferous bile, mixed with 2 ounces of warm water. He died the 4th day, as the others did, with a bubo on the wounded thigh; on which likewise there were two carbuncles, and the wound gangrened. What was remarkable in this dog was, that after the injection, both when he was living, and after he was opened, when dead, he had a very stinking smell, which we did not observe in any of the others. He had also a considerable hæmorrhage from the wound, the night before he died, having struggled hard to escape out of his confinement.

6th. The 2d of May having injected about a drachm of human bile, taken from persons dead of the plague, mixed with 2 ounces of warm water, into the crural vein of a dog; he was presently drowsy, refused his food, and died about the 3d or 4th day after the injection, with all the inward and outward signs of the plague that the others had.

7th. May 6, we collected the bile of this dog dead of the plague, and injected it by the crural vein into the blood of another dog, who, presently after the injection, had convulsive motions all over him, which were followed by a lethargic heaviness. On the 2d day after the injection, there appeared a carbuncle on the great pectoral muscle on the right side. The 3d day there arose a considerable bubo on the thigh; and the dog died the same day. On opening the body, we found the forepart of the breast all gangrened under the teguments; and the viscera were full of black clotted blood, as in all the former. The outer surface of the lungs was all purple; the heart was swelled as large again as usual, with the 4 cavities full of black clotted blood. The animal lived 3 days after the injection, without eating or drinking.

May 10, we injected the bile of this 2d dog into the crural vein of a 3d, who was soon seized with violent convulsions, and various convulsive motions, for about half a ¼ of an hour. When he recovered from these, he appeared dull, and sleepy; and he vomited with violent strainings. The vomiting was followed with a hickup. He eat a little boiled meat, having fasted a good while before the injection was made; but he vomited it up 2 hours after. He died the 3d day, with the same symptoms of the plague that the other dogs had.

The State of the Bodies of Persons dead of the Plague, from which the Bile was taken for the Experiments beforementioned.—1st body. A soldier 25 years of age, of a strong robust constitution, having a flattish bubo on the hollow of the right groin, died delirious. On opening his body, the heart was of an extraordinary size, stuffed with black clotted blood. His lungs covered with a livid purple, and adhering a little to the pleura: the liver was double the natural size, and stuffed with a thick blood: the gall-bladder was full of a black and greenish bile: the dura and pia mater, by their blackness, seemed to have been seized with a gangrenous inflammation: the inner substance of the brain was sprinkled over with an infinite number of small livid spots.

2d. Mary Pisanne, aged 30 years, of a sanguine habit, had a bubo under her right arm-pit, with a delirium that was followed by a mortal sleepiness. On opening her body, the lungs were in their natural state: the heart was of a prodigious size, full of black coagulated blood, with the left auricle livid and gangrenous: the liver, which was much enlarged, was all covered with purple; and the gall-bladder filled with a black and greenish bile.

3d. Peter Moulard, of a tender, feeble constitution, about 14 years of age, had a bubo under the hollow of the right groin, very deep, and never came well out: he became delirious with convulsions, in which he died. On opening his body, the heart was double the size naturally, containing a black thick blood: his lungs were besprinkled with livid spots: the gall-bladder full of a black and greenish bile.

4th. Jean Raynaud, a cook, about 25 years of age, of a melancholy temperament, had the whole habit of his body covered with a purple livid colour, and a bubo under his left axilla; and he died in a delirious phrenzy. We found in his body 2 abscesses, one between the teguments and the left great pectoral muscle, and the other in the breast between the sternum and the mediastinum: his heart was very large, filled with black thick blood: the right auricle was 3 inches broad: the left was in its natural state: his lungs were covered with small livid spots, remaining soft and pliant, without any hardness in their substance. The liver was larger and harder than ordinary, and was also full of livid purple spots; and the like were found in the substance of his brain, all the vessels of which were filled with black thick blood.

5th. Jaques Audibert, about 35 years of age, of a melancholy complexion, 4 months after he had been cured of the plague, the mark of which was a bubo in the fold of the right groin, which came well to a suppuration, was attacked afresh with three carbuncles; one in the middle of the arm, and the other two in the fore-arm. He had but little fever, with some small sickness at stomach; but a delirium, coming of a sudden, carried him off. On opening his body, we found the heart of a prodigious size: the right auricle being 5 inches broad; and the left 3: a little imposthume on the body of the aorta: the lungs were covered with livid spots; and the liver appeared gangrenous: the gall-bladder was of a very black colour: the duodenum and the rectum were inflamed.

6th. Venture Cajole, about 40 years of age, of a melancholy temperament, died without any outward eruption on the 3d day, of a violent fever, with a sleepiness. We found in the body the mediastinum torn towards the upper part: the pericardium was of a livid colour: the heart larger than in its natural state, by the swelling of its ventricles full of black thick blood, as in the others: the liver also was very large, and of a livid colour, with a carbuncular pustule on the side of the gall-bladder; and this was filled with very black bile.

7th. Margueritte Bachaire, 18 years of age, of a lively vigorous constitution, having 2 carbuncular pustules on the middle and inside of the thigh, with a sharp pain in the head, died delirious. We found, in her body, the coverings of the brain of a blackish red: the cortical part of a livid colour, and the medullary sprinkled with a few black spots: the heart of a prodigious size, full of a thick black blood: the liver also very large, and the gall-bladder very full of a black and green bile. There were several livid spots on the surface of the intestines.

8th. Louise Belingere, 20 years of age, having a bubo in each fold of the groin, died very suddenly, without any dangerous symptoms. We found in her body, the heart all covered with a livid purple, much larger than natural, filled with thick black blood, having a polypus in each ventricle: the lungs were in their natural state: the liver of a prodigious size; and the gall-bladder full of bile, of a deep green colour.

9th. One Rampeau, a peasant, about 20 years of age, of a sanguine robust constitution, having had a carbuncular parotid for 8 days, accompanied with a burning fever, was carried to the hospital the 2d of May, where he died on the 5th. We found the outer part of the left side of the lungs covered with a livid purple: the heart was double the natural size, having scarcely any blood in the ventricles, their cavities being filled with a large polypus, that on the right side having dilated the auricle to the breadth of 4 inches: the liver also was larger than ordinary; and the gall-bladder was full of a black and green bile.

The following Experiments were made at Montpelier, in the Hospital of St. Eloy, during Sept. Oct. and Nov. in company with Mons. Fizes, M. D. and Messrs. Duli and Morel, Surgeons of the said Hospital. N° 370, p. 28.

A soldier, between the age of 20 and 25, of a lively temper, being sick in the hospital of St. Eloy of an ordinary malignant fever, died about the 15th day by a defluxion on his breast. His lungs were found very much blown up, filling all the cavity of the breast, and adhering to the pleura. Having remarked that the bile in the gall-bladder was of a bright grass green colour, we reserved it for the following experiments.

This bile, mingled with 4 oz. of warm water, was part injected into the jugular vein of a dog, and a compress, soaked in the rest of the liquor, was applied to the rest of the wound. The dog soon appeared heavy and sleepy, and would neither eat nor drink; but the 3d day he eat and drank willingly. The compress falling off the 4th day, the wound was diminished one half, and was healed by degrees, and the dog became perfectly well.

Exper. 8.—A peasant, of about 50 or 60 years old, of a melancholy temperament, had been near a month in the hospital with an ordinary malignant fever, being alternately delirious and sleepy. After his death, the bile was found extremely thick, and black as ink, and in great quantity. We put about a drachm in a wound made for that purpose on the outer part of the right thigh of a dog, thrusting in pledgets, dipped in the said bile diluted into the wound. There did not appear any change in the dog. We made him swallow some of the same bile, without losing his appetite: and seeing he was like to do well, we left the wound to itself, which healed in 15 days, only by the dog's licking it.

9th. We took a pledget, soaked in as much of this bile diluted as it could take up, and applied it to a fresh wound made in the inside of the right thigh of a dog. The pledget was fastened within the skin by some needles. This application produced no considerable alteration in the dog; he neither appeared sleepy nor stomachless; but licked his sore readily enough; and after the pledget was fallen off, the wound healed as in the foregoing experiment.

10th. About a drachm of the same black bile, mixed with warm water, was injected into the jugular vein of another dog: with which he was not incommoded, but was as brisk as before the injection, only he appeared very thirsty, and drank with greediness. The next morning we found the wound black and dry, and the dog becoming surly, bit one of the assistants. The two ligatures made for the injection were taken away, without any blood running out. We applied a dossil, charged with the ordinary digestive, and kept on by a bandage; and about 4 hours after the dressing, we found the dog dead, having lived 23 hours after the injection. Having opened him, we found that his heart beat still with violence, and after the beating ceased, there was no blood either in the ventricles or the auricles. This liquor crouded together in the great vessels, appeared of a lively red, and very fluid, without any of those concretions that we constantly observed in all the bodies that died of the plague. Here appeared neither internal nor external signs of the plague.

11th. An inhabitant of Montpelier, aged about 30 or 35, very fat and robust, of a sanguine complexion, having had a fall on the pavement, received a simple wound on the upper part of his forehead on the right side. This being neglected, brought on an erysipelas all over his face, which was accompanied with a swelling of the left parotid. This appeared and disappeared thrice from morning till night. The erysipelas came suddenly on; he grew delirious, and died after 15 or 20 days illness, reckoning from the fall. On the opening of the body, we found a quantity of water between the skull and the dura mater. The brain, which was firmer than ordinary, was a little red, and part of the pia mater covering the hinder part of that viscus, appeared inflamed. There was about half a septier of water, of a yellowish colour, shed in the cavity of the breast. The great right lobe of the lungs was a little hard on the upper part: the heart had a polypose concretion in each ventricle: we found likewise 2 French pints of limpid water in the lower belly. All the fat of his body was yellow: the liver appeared a little swelled: and the gall-bladder almost empty, not having above 2 drachms of yellow bile in it. That bile of this body mixed with 2 ounces of warm water, was injected into the crural vein of a dog. The animal eat and drank heartily after the injection, and did not appear at all incommoded. The wound bleeding much, we were obliged to fill it with astringent powders, kept in by a pledget and a convenient bandage. Twenty-four hours after, the dressing was taken away, and the wound appeared black and dry. The dog licking it, it suppurated the next day, and afterwards became red and well coloured; and the wound was lessened one half in 8 days; during which time the dog appeared in perfect health.

12th. Eight days after the foregoing experiment, the same dog was killed by about half a drachm of powder of Hungarian vitriol, dissolved in a spoonful of warm water, which we injected into the jugular vein. He died on the spot, with universal convulsions. His heart was found full of grumous blood, reduced to a kind of thick pap, but without any clots: the bile was yellow, and in small quantity. Not being able to inject it into the crural vein of another dog, because the vein was too small, we contented ourselves with dipping 2 compresses in this bile, which we applied and kept under the skin, by 2 wounds made on purpose in this 2d dog: no notable change happened. We observed in these 2 dogs no signs, either internal or external, of the plague.

Montpelier, Dec. 4, 1721.

The Method of Inoculating the Small-Pox in New England. Communicated by Henry Newman, Esq. of the Middle Temple. N° 370, p. 33.

Two incisions are usually made in the arms, where issues are made, but somewhat larger than for them; sometimes in one arm, and one leg. 2. Into these are put bits of lint, (the patient at the same time turning his face another way, and guarding his nostrils) which have been dipped in some of the variolous matter taken in a phial, from the pustules of one that has the small-pox of the best sort, and just turning upon him; and so covered down with a plaster of diachylon. 3. Yet we find the variolous matter taken from those that have the inoculated small-pox, altogether as agreeable and effectual as any other. And so we do also what is taken from those that have the confluent sort.

4. Within 24 hours, we throw away the lint, and the sores are dressed once or twice every 24 hours, with warmed cabbage leaves. 5. The patient continues to conduct himself as at other times; only not exposing himself to the injuries of the weather, if that be at all tempestuous.

6. About the 7th day the patient feels the usual symptoms of the small-pox coming on; and he is now managed as in an ordinary putrid fever. If he cannot hold up, he goes to bed; if his head ach too much, we put the common poultice to his feet; if he be very sick at the stomach, we give him a gentle

vomit; indeed we commonly do these things almost of course, whether we find the patient want them or not. And we reckon the sooner we do these things the better. If the fever be too high, in some constitutions, we bleed a little: and finally, to hasten the eruption, we put on a couple of blisters.

7. On or about the 3d day from the decumbiture, the eruption begins. The number of the pustules is not alike in all, in some they are very few, in others they amount to 100, and even several hundreds; frequently to more than what the accounts from the Levant say is usual there.

8. The eruption being made, all illness ceases; except perhaps a little of the vapours in those that are troubled with them. And there is nothing more to do, but to keep warm, drink proper teas, take gruel, milk pottage, panada bread, butter, and almost any thing equally simple and innocent. 9. Usually the patient sits up every day, and entertains his friends, and ventures upon a glass of wine with them. If he be too intent on hard reading and study, we take him off.

10. Sometimes, though the patient be on other accounts easy enough, yet he cannot sleep for several nights together. In this case we do not give him anodynes or opiates, because we find, that such as have taken these things in the small-pox, are generally pestered with miserable biles, after being recovered. So their sleep is let come on of itself, as their strength is coming on.

11. On the 7th day the pustules usually come to their maturity; and soon after they go off, as those of the small-pox in the distinct sort usually do.

12. The patient gets abroad quickly, and is most sensibly stronger, and in better health than he was before. The transplantation has been given to women in child-bed, 8 or 9 days after their delivery; and they have got earlier out of their child-bed, and in better circumstances, than ever in their lives. Those that have had ugly ulcers long running on them, have had them healed up by this transplantation. Some very feeble, crazy, consumptive people, on this transplantation, have grown hearty, and got rid of their former maladies.

13. The sores of the incision seem to dry a little in 3 or 4 days of the feverish preparation for eruption. After this there is a plentiful discharge at them. The discharge may continue a little while after the patient is quite well on other accounts; but the sores will soon enough dry up of themselves; though the later the better, as we think. If they happen to be inflamed, or otherwise troublesome, we presently treat them as we do any ordinary sores.

Concerning the Inoculation of the Small-Pox at Halifax in Yorkshire. By Dr. Nettleton. N° 370, p. 35.

Having often found with grief, how little the assistance of art could avail in many cases of the small-pox. I was induced to try the method of incision or inoculation, which came so well recommended by several physicians from Turkey, and which had also been lately practised in London. And the following is an account of the process, and the success.

In December 1721 I first began to put this method in practice, and finding it succeed beyond my expectation in the first instance, I was encouraged to repeat it; and afterwards several persons, seeing with how much ease these got through the distemper, were desirous to have the same done to themselves or their children; so that there are now upwards of 40 here, who have received the small-pox by incision; who are all got well, and are now in very good health; only one patient dying.

What was done by way of preparation, was chiefly purging with rhubarb for children, and sometimes vomiting or bleeding for grown persons; and many have had no preparation at all. But I always found, that those whose bodies were well prepared, by such proper methods as their different ages or constitutions seem to require, had more favourable symptoms than others in like circumstances, where that was omitted.

The method, which I always took in the operation, was to make two incisions, one in the arm, and another in the opposite leg. It is not material, as to raising the distemper, whether the incisions be large or small; but I commonly found, that when they were made pretty large, the quantity of matter discharged afterwards at those places was greater; and the more plentiful that discharge, the more easy the rest of the symptoms generally are, and they are also by this means the best secured from any inconvenience which might follow after the small-pox are gone off.

At first I collected some of the matter from the pustules of one who had the small-pox of the natural sort, into a shell or phial, and infused 2 or 3 drops of it into the wound; but finding it to be very troublesome and difficult to get any quantity of the matter, and observing also, that the least imaginable will be sufficient for the purpose, I commonly take small pledgets of cotton, and ripping the pustules, when they are ripe, with the point of a lancet, roll the pledgets over them, till they have imbibed some of the moisture. I put one of these on each wound, and cover it with any common plaster till the next day, when I commonly take away both the cotton and the plaster, leaving the wound to itself, only covering it with a slight linen roller, to defend it from the air. I have sometimes rubbed the pledget only once over the wound, without binding it on, which I found to answer the end as well; and from some other observations I have made, I have been surprised to see the small-pox produced this way, when I was very well assured the quantity of matter received into the vessels, could not amount to the 100th part of a grain.

The persons inoculated have not been confined to any regimen, but only to be kept moderately warm; and those who were grown up to live very temperate and regular, to keep their minds easy and composed, and to use proper means to drive away all fear and concern. Some have been obliged, from the time of the incision, to abstain from flesh and all strong liquors; but I found afterwards, that the eruption did not proceed so well, when they were obliged to live too low. Perhaps in warmer climates, where they are not so much accustomed to live upon flesh, such abstinence may be necessary; but here I find it best to let them eat and drink as usual, though something more sparingly, till the fever begins to rise; and then, but not before, we enjoin such a regimen as is usual in like cases.

The first thing that occurred after the incision, was the inflammation of the wounds, which commonly happened about the 4th day, when they began to appear very red round about, and to grow a little sore and painful; in about 2 days more they began to digest and run. In some they begin to run sooner, and the quantity discharged is much greater than in others. I generally found, that in those who discharged most this way, the fever was more slight, and the small-pox fewer, though I have known some do very well when these places have only appeared very red, but have scarcely run any thing at all, as it usually happens, when the incision is made so superficial as not to cut through the skin.

About the 7th day the symptoms of the fever begin to come on, which are the very same, that we always observe in the small-pox of the distinct kind, in the natural way. A quick pulse, great heat and thirst, pain in the head and back, and about the region of the stomach, vomiting, doziness, startings, and sometimes convulsions. All were not seized with all these symptoms, nor in the same degree or continuance; some began on the 7th day, and continued ill, without any remission, till after the 11th; many not till the 8th or 9th day; and the fever in these was more moderate, with great intermissions; and some have scarcely had any illness at all. During all this time the places of incision continued to be very sore, and swell very much, so as to appear very large and deep, and to discharge a great deal of matter.

On the 10th day the small-pox most commonly appeared, sometimes on the 9th, and sometimes not till the 11th: but I never found that any difference of age, constitution, or any other cause ever made them vary above 1 day from the 10th. The number was very different, in some not above 10 or 20, most frequently from 50 to 200, and some have had more than could well be numbered, but never of the confluent sort. Their appearance was the same with those of the distinct kind; they commonly came out very round and florid, and many times rose as large as any I have observed of the natural sort, going off with a yellow crust or scab as usual; though it sometimes happens, especially when the sores discharge a very great quantity of matter, that they are both few in number, and do not rise to any bulk; but having made their appearance for 4 or 5 days they waste insensibly away.

After the small-pox come out, the feverish symptoms gradually abate; and when the eruption is completed, they usually cease, without any second fever, or any further trouble in any respect.

While the pustules were rising, and for some time after they were gone, the sores continued to swell and to run very much, the longer they did so the better; but they never failed to heal up of themselves after a certain time.

I very rarely saw occasion for any medicines in the course of the distemper, only sometimes, when the symptoms ran very high, I gave a gentle anodyne, to be repeated as occasion should require, and once or twice I thought it necessary to blister, and to use such medicines as are found to be most serviceable in the small-pox of the natural sort. After the pustules are gone away, they have always been purged twice or thrice, and sometimes let blood, which is all that has been usually done. But though the practice may seem to be very easy, yet it is an affair of such a nature as to require the utmost care, and I presume it will never be undertaken without the advice of physicians to direct a proper method of preparation before the incision is made, as well as a just regimen afterwards, to watch every symptom, and lend nature all proper assistance, whenever it shall be requisite. Where this is done, it will seldom fail of being attended with happy success.

It has happened in one instance or two, that the symptoms in the distemper have been worse than usual, and some few, after the small pox were gone off, have been subject to other indispositions.

Whether those slight indispositions, which some have been subject to afterward, were owing to the incision, I have not been able to judge; but I presume what they have endured in the course of the distemper, and what has followed after, is not to be compared with what is undergone in the common way, by those who are thought to come off very well; and if this method was more generally practised, probably some means would be found out to prevent even these subsequent disorders, which are no more frequent, nor near so bad, as those which follow the natural sort.

In two instances the inoculation had no effect, the reason of which, in one, was because the child had the small-pox before, as the parents believed, but the distemper had been so favourable, as to leave it doubtful. In the other, the matter was taken when the pustules were withered, and almost gone, and that little moisture which they contained I suppose had lost its virtue; the boy on whom it was used was no way affected; the places of incision did not at all inflame, nor swell as usual, nor did any pustules appear; but about a month after he was seized with the distemper in the ordinary way, and did very well.

Some of those who have been inoculated, that are grown up, have afterwards attended others in the small-pox, and it has often happened that in families where some children have been inoculated, others have been afterwards seized in the natural way, and they have lain together in the same bed all the time; but we have not yet found, that ever any had the distemper twice; neither is there any reason to suppose it possible, there being no difference that can be observed between the natural and artificial sort, but only that in the latter the pustules are commonly fewer in number, and all the rest of the symptoms in the same proportion more favourable. There is one observation which I have made, though I would not yet lay any great stress on it, that in families where any have been inoculated, those who have been afterwards seized, never had an ill sort of small-pox, but always recovered very well.

A further Account of inoculating the Small-pox. By the same. N° 370, p. 49.

I believe all other persons who have seen any thing of this practice are in the same sentiment with us, and there is no doubt, but in a few years the world will acknowledge the service which the Royal Society have done to mankind, in first revealing to this part of Europe a thing so beneficial as it will certainly prove; for though some few unfortunate accidents may sometimes happen, yet those will be very rare in comparison of the many sad and disastrous events which this distemper has been, and ever will be very fruitful of, while it is left to rage in its full force and violence.

I doubt not but when you have collected a sufficient number of observations for it, you will be able to demonstrate, that the hazard in this method is very inconsiderable, in proportion to that in the ordinary way by accidental contagion; so small, that it ought not to deter any one from making use of it. In order to satisfy myself what proportion the number of those that die of the small-pox might bear to the whole number seized with the distemper in the natural way, I have made some inquiry, as follows: In Halifax, since the beginning of last winter, 276 have had the small-pox, and out of that number 43 have died. In Rochdale, a small neighbouring market town, 177 have had the distemper, and 38 have died. In Leeds 792 have had the small-pox, and 189 have died. It is to be noted, that in this town the small-pox have been more favourable this season than usual, and in Leeds they have been more than usually mortal. But on a medium in these three towns there have died nearly 22 out of every hundred, which is above a 5th part of all that have been infected in the natural way.

I have in these accounts confined myself to the limits of the towns. The numbers that have had the small-pox in the country round about, is vastly greater; but the proportion of those that die is much the same. I have made the inquiry in several country villages, in some I found the proportion to be greater, in others less, but in the main it is nearly the same.

Observations of the Variation of the Magnetic Needle, on board the Royal African Packet, in 1721. *By Capt. Cornwall.* N° 371, p. 55.

N. B. The meridional distance is reckoned from St. Jago.

Anno 1721.		Latitudes.	Meridian distance.	Longitude.	Variation.
Aug.	24	9° 8′ s.	9° 23′ w.	9° 25′ w.	2° 13′ E
ditto	26	11 12 s.	10 46 w.	10 50 w.	4 30 E
ditto	27	11 34 s.	11 28 w.	11 41 w.	4 29 E
ditto	28	12 32 s.	11 31 w.	11 43 w.	4 27 E
ditto	31	15 46 s.	10 53 w.	11 6 w.	6 10 E
Sept.	2	16 26 s.	8 25 w.	8 30 w.	7 16 E
ditto	5	18 45 s.	9 31 w.	9 39 w.	6 17 E
ditto	6	19 47 s.	9 10 w.	10 0 w.	8 6 E
ditto	17	28 43 s.	1 7 w.	1 9 E.	5 53 E
ditto	22	31 33 s.	3 41 E.	3 56 E.	4 10 E
ditto	27	33 30 s.	11 29 E.	12 57 E.	0 11 W
ditto	30	32 40 s.	19 6 E.	12 1 E.	3 0 W
Oct.	1	32 53 s.	21 18 E.	24 59 E.	5 41 W
ditto	3	32 30 s.	25 33 E.	30 0 E.	7 47 W
ditto	5	32 28 s.	30 37 E.	35 52 E.	8 44 W
ditto	6	31 22 s.	31 40 E.	37 7 E.	10 57 W
ditto	7	31 11 s.	32 4 E.	37 47 E.	11 20 W

Observations on the coast of Africa.

Oct.	13	26° 17′ s.	35° 35′ E.	41° 41′ E.	14° 30 W
ditto	19	19 41 s.			12 22 W
ditto	21	17 4 s.			14 29 W

Anno 1721.	Latitudes.	Meridian distance.	Longitude.	Variation.
Oct. 25	13 56 s.			14 48 w
Nov. 4	10 57 s.			13 11 w
ditto 7	8 19 s.			15 14 w
ditto 29	5 0 s in Cabenda-Bay			14 33 w

From Cabenda to London, meridian distance from thence.

Dec. 9	3° 25′ s.	11° 38′ w.	11° 43′ w.	11° 32′ w
ditto 14	3 30 s.	21 18 w.	21 24 w	
ditto 20	0 30 s.	30 41 w.	30 46 w.	1 5 w
Jan. 1722 1	10 50 N.	39 8 w.	39 16 w.	1 1 E
ditto 6	17 15 N.	43 21 w.	43 29 w.	1 41 E

Remarks on an Experiment, by which it has been attempted to show the Falsity of the common Opinion on the Force of Bodies in Motion. In a Letter to Dr. Mead. By Henry Pemberton, M. D. R. S. S.* N° 371, p. 57.

Perusing the learned Polenus's Tract de Castellis, I have found in it several curious experiments, among which I reckon that of letting globes of equal

* Dr. Pemberton, a learned physician and mathematician, was born at London, in 1694. He was also a skilful mechanist, and readily performed with his own hands several mechanical operations. After studying grammar at a school, and the higher classics under Mr. John Ward, afterwards professor of rhetoric at Gresham college, he went to Leyden, to attend the lectures of the celebrated Boerhaave, to qualify himself for the profession of medicine. Here also, as well as in England, he constantly mixed, with his professional studies, those of the best mathematical authors, whom he contemplated with great effect. From hence he went to Paris, to perfect himself in the practice of anatomy, to which he readily attained, being naturally dexterous in all manual operations. Having obtained his main object, he returned to London, enriched also with other branches of scientific knowledge, and a choice collection of mathematical books, both ancient and modern, from the sale of the valuable library of the Abbé Gallois, which took place during his stay in Paris. After his return he assiduously attended St. Thomas's hospital, to acquire the London practice of physic, though he seldom afterwards practised, owing to his delicate state of health. In 1719 he returned to Leyden, to take his degree of M.D. where he was kindly entertained by his friend Dr. Boerhaave. After his return to London, he became more intimately acquainted with Dr. Mead, Sir I. Newton, and other eminent men, with whom he afterwards cultivated the most friendly connexions. Hence he was useful in assisting Sir I. Newton in preparing a new edition of his Principia, in writing an account of his philosophical discoveries, in bringing forward Mr. Robins, and writing some pieces printed in the 2d vol. of that gentleman's collection of tracts, in Dr. Mead's Treatise on the Plague, and in his edition of Cowper on the Muscles, &c. Being chosen professor of physic in Gresham college, he undertook to give a course of lectures on chemistry, which was improved every time he exhibited it, and was published in 1771, by his friend Dr. James Wilson. In this situation too, at the request of the college of physicians, he revised and reformed their pharmacopœia, in a new and much improved edition. After a long and laborious life spent in improving science, and assisting its cultivators, Dr. Pemberton died in 1771, at 77 years of age.

magnitude, but of different weights, fall on a yielding substance, as tallow, wax, clay, or the like, from heights reciprocally proportional to the weights of the globes. This experiment engaged in particular my attention, as it is brought with design to overturn one of the first principles established in natural philosophy; for I can by no means admit of the deduction drawn from it, viz. that because the globes make in this experiment equal impressions in the yielding substance, therefore they strike upon it with equal force; by which it is attempted to prove the assertion of Mr. Leibnitz, that the force of the same body in descending is proportional to the height from whence it falls; or, in all motions, proportional to the square of the velocity, and not to the velocity itself, as is commonly thought. On the contrary, I think that this very experiment proves the great unreasonableness of Mr. Leibnitz's notion.

It is surprising that so careful a writer as Polenus appears to be, from the accuracy shown in his experiments, should not rather suspect his reasoning in an intricate case, than thus contradict a principle in philosophy, that has been directly proved by a multitude of experiments, in particular by those Sir Isaac Newton recommends for that purpose, Princip. p. 19. Certainly this experiment of Polenus is much more fit to inform us of the law by which these yielding substances resist the motion of bodies striking them, than to show the forces with which bodies strike; for whatever those forces be, the effects must be very different, according to the difference there may be in the rule observed by such resistance.

Now this experiment shows, that if two globes in motion bear against equal

Besides the doctor's writings above-mentioned, he wrote numerous other pieces; as, 1. Epistola ad Amicum de Cotesii inventis; demonstrating Cotes's celebrated theorem, and showing how his theorems by ratios and logarithms may be done by the circle and hyperbola. 2. Observations on Poetry, especially the epic, occasioned by Glover's Leonidas. 3. A plan of a free state, with a king at the head: not published. 4. Account of the ancient ode, printed in the preface to West's Pindar. 5. On the Dispute about Fluxions; in the 2d vol. of Robins's works. 6. On the Alteration of the Style and Calendar. 7. On reducing the Weights and Measures to one standard. 8. A Dissertation on Eclipses. 9. On the Loci Plani, &c. His numerous communications to the Royal Society, on a variety of interesting subjects, extend from the 32d to the 62d vol. of the Philos. Trans.

After his death, many valuable pieces were found among his papers, viz. A short History of Trigonometry, from Menelaus to Napier. A comment on an English translation of Newton's Principia. Demonstrations of the spherics and spherical projections, enough to compose a treatise on those subjects. A Dissertation on Archimedes's Screw. Improvements in Gauging. In a given latitude to find the point of the Ecliptic that ascends the slowest. To find when the Oblique Ascension differs most from the arch to which it belongs. On the principles of Mercator's and Middle-latitude sailing. To find the Heliacal Rising of a Star. To compute the Moon's Parallax. To determine the Course of a Comet in a Parabolic Orbit. And others, all neatly performed. On the whole, Dr. Pemberton appears to have been a clear and industrious author, but his writings are too diffuse and laboured.

portions of the yielding substance, the opposition that substance makes to the motion of the globes, will be the same in both, however different the velocities be, with which they move. This I shall demonstrate as follows.

Let A and B be two globes, equal in magnitude, but of different weights, which are equally immersed into a yielding substance. Suppose the velocities with which they move in their present situation, to be reciprocally in the subduplicate ratio of the weights of the globes; that is, let the ratio of the weight of the globe A to the weight of the globe B, be duplicate of the ratio of the velocity of the globe B, to the velocity of the globe A. Since therefore the ratio of the quantity of motion in the globe A, or of the force with which it moves, to the quantity of motion in the globe B, or to the force with which that globe moves, is compounded of the ratio of the weight of the globe A, to the weight of the globe B, and of the ratio of the velocity of the globe A, to the velocity of the other globe B, the force with which the globe A moves, is to the force with which the globe B moves, as the velocity of this globe B, to the velocity of the other globe A. But if the same opposition be made to the motion of the globes when they bear upon equal portions of the yielding substance, the effect of that opposition, while the globes enter farther into the substance by equal spaces, will be proportional to the time in which the globes are moving those spaces, or in which the opposition is made, if we consider those spaces while nascent or in their first origin; the effect therefore of this opposition will be reciprocally proportional to the velocity of each globe; namely, the momentaneous loss of force in the globe A, will be to the momentaneous loss of force in the globe B, as the velocity of the globe B, to the velocity of the globe A; and the whole force of the globe A has been found to bear the same ratio to the whole force of the globe B, consequently these globes, while they penetrate equal spaces into the substance, lose parts of their force which bear the same proportion to the whole; and therefore, if their velocities be at any time reciprocally in the subduplicate ratio of their weights, so that the forces or degrees of motion, with which they move, be reciprocally proportional to their velocities, the forces with which they press into the yielding substance, at equal indentures made in the substance, will continue in the same proportion; and therefore, on the theory of resistance here supposed, when the whole force and motion of both these globes is entirely lost, they will be plunged into the substance at equal depths.

Now whereas in the experiment of Polenus, the globes, falling from heights reciprocally proportional to their weights, strike the yielding substance with velocities reciprocally in the subduplicate proportion of their weights, and the effect is in all cases found to be what is here deduced from the theory of resistance I have proposed, it is a sufficient confirmation of the truth of this theory.

Only here I ought to observe, that I have supposed the globes to be stopped by the whole resistance of the substance they move against, though in strictness they are stopped only by the excess of that resistance above the action of gravity upon them. But I have neglected the consideration of the action of gravity, that being but small in proportion to the resistance, as will appear from the globes being much more speedily stopped by this resistance, than they would be by the action of gravity, if its force were applied upwards; for by that force alone, the globes would not be stopped, till they had measured spaces equal to the heights above the resisting substance from whence they fell; which heights bear a great proportion to the depths the globes in this experiment are immersed into the yielding substance, as I have found upon trial.

Hence therefore we may see, that the very method of reasoning, which being applied erroneously, is supposed to prove Mr. Leibnitz's sentiment concerning the force of bodies in motion, will, when justly used, confirm the other opinion in relation to that matter.

Thus may this experiment be made use of to invalidate that very opinion it is brought to support. But another use may likewise be made of it; for it will serve to illustrate what Sir Isaac Newton has more than once hinted, that the resistance of fluids, which arises from the tenacity of their parts, decreases in a less proportion than the velocity of the resisted body decreases;* for as this resistance bears a great analogy to the resistance of the yielding substances we have been here treating of, so we have found that the resistance of these substances does not much depend on the velocity of the body, against which the resistance is applied.

Postscript.†—About a week after I had sent you the letter, containing my observations on Polenus's experiment, I had the good fortune to hear an excellent and learned friend of yours, to whom you had been pleased to show my letter, give a very curious and weighty argument to confirm Sir Isaac Newton's sentiment in relation to the resistance of fluids, which I had deduced from the above-mentioned experiment; and as this very much pleased me, I shall here endeavour to send you an account of it in the following manner:

Suppose pieces of fine silk, or the like thin substance, extended in parallel planes, and fixed at small distances from each other. Suppose then a globe to

* Vid. Philos. Nat. Princip Math. prop. 52, lib. 2, in Schol. Opticks. qu. 28, p. 339, 340.—Orig.

† This postscript it seems was by Sir I. Newton himself; as stated by Dr. James Wilson, in his Life of Dr. Pemberton, prefixed to an edition of our author's Course of Chemistry, in 24 lectures, printed in 1771.

strike perpendicularly against the middle of the outermost of the silks, and by breaking through them to lose part of its motion. If the pieces of silk be of equal strength, the same degree of force will be required to break each of them; but the time in which each piece of silk resists will be so much shorter as the globe is swifter, and the loss of motion in the globe consequent upon its breaking through each silk, and surmounting the resistance thereof, will be proportional to the time in which the silk opposes itself to the globe's motion; insomuch that the globe by the resistance of any one piece of silk will lose so much less of its motion as it is swifter. But on the other hand, by how much swifter the globe moves, so many more of the silks it will break through in a given space of time; whence the number of the silks which oppose themselves to the motion of the globe in a given time, being reciprocally proportional to the effect of each silk upon the globe, the resistance made to the globe by these silks, or the loss of motion the globe undergoes by them in a given time, will be always the same.

Now if the tenacity of the parts of fluids observes the same rule as the cohesion of the parts of these silks; namely, that a certain degree of force is required to separate and disunite the adhering particles, the resistance arising from the tenacity of fluids must observe the same rule as the resistance of the silks. And therefore, in a given time the loss of motion a body undergoes in a fluid by the tenacity of its parts, will, in all degrees of velocity, be the same; or in other words, that part of the resistance of fluids which arises from the cohesion of their parts, will be uniform.

An Account of the Falls of the River Niagara, taken at Albany, Oct. 10, 1721, *from M. Borassaw, a French Native of Canada. By the Hon. Paul Dudley, F.R.S.* N° 371, p. 69.

The falls of Niagara are formed by a vast ledge or precipice of solid rock, lying across the whole breadth of the river, a little before it empties itself into, or forms the lake Ontario.

M. Borassaw says, that in spring 1722, the governor of Canada ordered his own son, with three other officers, to survey the Niagara, and take the exact height of the cataract, which they accordingly did with a stone of half a hundred weight, and a large cod-line, and found it on a perpendicular no more than 26 fathoms, vingt et six brass.

This differs very much from the account Father Hennepin has given of that cataract; for he makes it 100 fathoms, and our modern maps from him, as I suppose, mark it at 600 feet; but I believe Hennepin never measured it, and there is no guessing at such things.

When I objected Hennepin's account of those falls to M. Borassaw, he replied, that accordingly every body had depended on it as right, until the late survey. On further discourse he acknowledged, that below the cataract, for a great way, there were numbers of small ledges or stairs across the river, that lowered it still more and more, till you come to a level; so that if all the descents be put together, he does not know but the difference of the water above the falls and the level below, may come up to father Hennepin; but the strict and proper cataract on a perpendicular is no more than 26 fathoms, or 156 feet, which yet is a prodigious thing, and what the world I suppose cannot parallel, considering the size of the river, being near a quarter of an English mile broad, and very deep water.

Several other things M. Borassaw set me right in, as to the falls of the Niagara. Particularly it has been said, that the cataract makes such a prodigious noise, that people cannot hear each other speak at some miles distance; whereas he affirms, that you may converse together close by it. I have also heard it positively asserted, that the shoot of the river, when it comes to the precipice, was with such force, that men and horse might march under the body of the river without being wet; this also he utterly denies, and says, the water falls in a manner right down.

What he observed further to me was, that the mist or shower which the falls make, is so extraordinary, as to be seen at 5 leagues distance, and rises as high as the common clouds. In this brume or cloud, when the sun shines, you have always a glorious rainbow. That the river itself, which is there called the river Niagara, is much narrower at the falls than either above or below; and that from below there is no coming nearer the falls by water than about 6 English miles, the torrent is so rapid, and having such terrible whirlpools.

He confirms Father Hennepin's and Mr. Kellug's account of the large trouts of those lakes, and solemnly affirmed there was one taken lately, that weighed 86 lb. which I am the rather inclined to believe, on the general rule, that fish are according to the waters. To confirm which, a very worthy minister affirmed, that he saw a pike taken in Canada river, and carried on a pole between two men, that measured 5 feet 10 inches in length, and proportionably thick.

I myself saw a cataract, 3 leagues above Albany, in the province of New York, on Schenectada river, called the Cohoes, which they count much of there, and yet it is not above 40 or 50 feet perpendicular. From these falls also there rises a misty cloud, which descends like small rain, which, when the sun shines gives a handsome small rainbow, that moves as you move, according to the angle of vision. The river at the Cohoes is 40 or 50 rods broad, but then it is very shallow water, for in a dry season the whole river runs in a channel of not more than 15 feet wide.

In my journey to Albany, 20 miles to the eastward of Hudson's river, near the middle of a long rising hill, I met with a brisk noisy brook, sufficient to serve a water mill; and having observed nothing of it at the beginning of the hill, I turned about, and followed the course of the brook, till at length I found it come to an end, being absorbed, and sinking into the ground, thence either passing through subterraneous passages, or soaked up by the sand; and though it be common in other parts of the world for brooks and even rivers thus to be lost, yet this is the first of the sort I have heard of, or met with, in this country.

A Letter from Mr. Leuwenhoeck, F. R. S. concerning the Muscular Fibres in several Animals, and the Magnetic Quality acquired by Iron, on standing for a long time in the same Position. N° 371, p. 72.

On viewing a portion of the flesh of a fat ox, as also the muscular fibres of a cod-fish, and of a perch, the fibres being cut transversely, I could see in them very distinctly the great number of small vessels, that ran along the length of each fibre. And I have seen the same in the muscular fibres of the hinder leg of a mouse, cut through transversely, and which are found of the same size as the fibres of the ox.

As to the small fibrils, mentioned in a former N°, that help to suspend the testicles of a ram, I forgot to mention, that each of these consists of exceedingly small vessels, which run parallel to its length.

The iron cross, which is supposed to have stood on the steeple of the New Church here about 200 years, having been lately taken down to be repaired, and being informed that a piece of iron that has stood a long time in one situation would acquire a magnetic quality; on which I desired a workman to procure me a piece of that cross, who accordingly brought me a bit of it, of about a span long, and a quarter of an inch thick, which I applied both to a working needle, and the needle of the compass, but without any effect on the one or the other.

Some time after, the same workman brought me some other pieces, looking like rusty iron, which he had broken off from the bottom of the cross, where it had been fastened by four cross pieces bound down with iron, to an erect piece of timber 9 inches square, and covered with lead in such a manner, that no wet could get to it. This seeming rusty iron would take up several needles hanging by each other, and appeared to have a stronger magnetic virtue than two load-stones, which I had then in the house, and it was so hard that no file would touch it.*

An Account of the Manner of bending Planks in his Majesty's Yards at Deptford, &c. by a Sand-heat, invented by Captain Cumberland. By Robert Cay, Esq. N° 371, p. 75.

The place, where the planks lie to be softened in the stove, is between two brick-walls, of such a length, height, and distance from each other, as suffice to admit the largest, or to hold a good number of the smaller sort: the bottom is of thick iron plates, supported by strong bars; under the middle of which, are two fire-places, whose flews carry the flame towards the ends.

The planks are laid in sand; the lowest about 6 or 8 inches above the iron-plates; they are well covered with the sand, and boards laid over all, to keep in the heat. The sand is moistened with warm water, for which purpose they have a cauldron adjoining to the stove; and if the timber be large, and intended to be very much bent, so that it must lie long in the stove, they water the sand again, once in 8 or 10 hours. When it is judged to be soft enough, the sand is removed; and the workmen carry away their respective planks, to the several places, where they are to be used; and having first nailed a thin board on the outside, to preserve the plank from bruises, they fix one part in its proper place, and bring the others to, by any power they can most conveniently apply. This work seems to be performed with great ease.

This method excels that of burning the planks over an open fire, in several respects: particularly, that no part of the wood is destroyed, but remains of the same dimensions; at least very nearly; a plank of the breadth of 16 inches being said not to alter above $\frac{1}{20}$ part of an inch. The edges of the plank are preserved; and consequently the work must be much firmer, and the caulking last longer. The extraordinary softness of the wood, while warm, makes it easily bend to any figure necessary in ship-building, which it holds very well, if they have occasion to take it off again after it is cold: whereas the plank bent by burning, would start when loosened; and could only be fixed to the timbers by such a force, as was able to overcome the resistance occasioned by the spring of the plank. It likewise adapts itself very readily to the surface of the timbers, if they happen to be uneven.

The gun deck-clamps in a ship of the second rate, which are very large

* It is extraordinary that the latter piece of the iron was found to be magnetical and the former not. On this account the case is doubtful. It is to be suspected that the workman had imposed on the old gentleman, by getting the latter piece magnetized before he brought it.

planks, were bent and twisted in so peculiar a manner, as they never could by any other method bend them into that form, but used to cut them into shape. The whole operation is performed with much less trouble to the carpenters, as well as at less expence; and they hope the wood will be more durable; as it is evident, from the deep tincture the sand receives, that a considerable quantity of sap comes out of the oak, while in the stove: and a large plank was observed to weigh some pounds less, when it was taken out.

A plank five inches thick requires five or six hours to make it fit for bending; and the time requisite for others, seems to be in a duplicate proportion to their thickness.

A Letter from the Rev. Mr. James Field, Rector of St. John's in Antigua, concerning two Cases of Wounds in the Stomach, to Mr. John Douglas, Surgeon, F. R. S. N° 371, p. 78.

A lusty young negro returning home about noon, went into his house, where seeing some ripe plantains, he eat of them heartily; his father-in-law, about 60 years of age, coming to the same house soon after, and finding the young fellow had taken his plantains, with his knife gave him a most desperate wound in the upper region of the belly, a vast gash being made in the stomach, insomuch that the plantains, he had eaten, burst through the wound, which was made straight up and down.

The old man immediately fled; and the youth's companions, hearing what was done, pursued the old man with bills in their hands: the old man perceiving that they gained ground of him, and suspecting their design was to kill him, pulled out the same knife with which he had stabbed the other, and gave himself as desperate a wound as he had given him, and in the upper region of the belly, his stomach being likewise seen; only with this difference, that this last wound was transverse, or from left to right, but the first directly up and down: the old man was carried home and laid in the same house where the other lay.

This accident happened about noon, and the surgeon came not to dress them till between 4 and 5: he stitched up both their stomachs and bellies, only leaving in each a small hole for suppuration. A fever seized each of them; the old man was in most danger; the fever held them about a fortnight, the wounds were brought to a good digestion, and in about a month's time the young fellow went abroad; but the old man lay somewhat longer. They were both perfectly cured, and have been very well ever since, though it is above 15 years since this accident happened.

Account of an Imposthumation in the Stomach. By Mr. Atkinson, Surgeon in Whitechapel. N° 371, p. 80.

I had a patient who had a large tumour on the upper part of her belly. It was hard and painful, but did not alter the natural colour of the skin, and had been three months in coming. I applied a warm gum plaster to it, which in about 2 weeks brought it to a suppuration. I then applied a caustic, about the size of a shilling; and when the eschar fell off, I saw a solid kind of substance appear in the orifice; I laid hold of it with my forceps, and pulled it gently towards me, on which there thrust forcibly out a quantity of it, that nearly filled my hand; so I dressed it. Next dressing, the same substance appeared again, which on her straining, forced out near twice as much as before. I concluded this was the omentum, in which opinion I was confirmed by some other surgeons I showed it to. I was still in doubt whether the stomach was concerned in this case, till the next removal of the dressings, when there spurted out above half a pint of ale in a full stream, being part of a pint she had drank a little before. I now concluded the case mortal; however I ordered her to keep her bed, to lie constantly on her back, and feed on things of easy digestion. The greatest part of what she eat or drank came through the ulcer for 8 or 10 days, so that I had no hopes of ever curing it; yet, contrary to my expectation; in about 6 weeks she was perfectly cured.

An Account of the Quantity of Resin in the Cortex Eleutheriæ. By Mr. John Brown, Chymist, F.R.S. N° 371, p. 81.

Dr. Douglas having given account (from the history of the Royal Academy at Paris) of the cortex eleutheriæ; and among other things having said of it, that M. Boulduc had, from one ounce of the bark, by means of spirit of wine, obtained 5 drachms of resinous extract, there remaining 3 drachms of fæces; and that gentleman's account of some of the properties of this bark being founded on the quantity of resin supposed to be contained in it, I proposed to Dr. Douglas, and some other gentlemen of the society, (who agreed with me in believing that scarcely any part of any plant whatever would yield that quantity of resinous extract) to try the experiment, which was performed in the following manner.

I took 2 oz. of picked bark, and digested it in rectified spirit of wine, which was often decanted and fresh put on, till the bark would yield no more tincture. The impregnated spirit being evaporated by a very gentle heat;

there were left 2 drachms of resinous extract; the remains of the bark dried, weighed 1 oz. $2\frac{1}{2}$ drs. the loss this way is $3\frac{1}{2}$ drs.

I boiled these remains in several waters till they would no longer tinge the water; which being evaporated, yielded $1\frac{1}{2}$ drs. of extracts; the remains of this dried, weighed 1 oz. and $\frac{1}{2}$ dr.; the loss by this method is half a drachm.

I took 2 oz. more of picked bark, and boiled it in several waters, till the bark gave no more colour; and then by evaporating of the water, had 2 drs. of extract. The remains being dried, weighed 1 oz. 6 drs.; here the loss was not any thing, except so much as might answer in weight to the quantity of the menstruum left in the extract, which allowance must likewise be made in the other extracts.

I digested the remains in rectified spirit of wine till they no longer tinged the spirit, and by a very gentle evaporation, I found remaining 1 dr. of resinous extract. What was left, when dried, weighed 1 oz. 2 drs. in this the loss was $2\frac{1}{2}$ drs.

The difference in the quantity of extract, obtained by these two different methods, is only half a drachm; and the medium between them, on putting together the several extracts made with spirit of wine and water, is in the whole only $3\frac{1}{4}$ drs. But the extract made with spirit of wine alone, is no more than 2 drs. from 2 oz. of the cortex, instead of 10 drs. which it ought to have yielded according to Mons. Boulduc.

An Account of the new Method of Cutting for the Stone. By J. Douglas, Surgeon, F.R.S. N° 371, p. 83.

After making some experiments on dead bodies, I was convinced that the stone might be extracted the high way, with much less trouble than commonly; and I was persuaded that the wound would heal again, by the great number of authentic instances we have of accidental wounds in the same place being speedily and firmly cured. I therefore resolved to make the experiment on the first patient I could meet with, which I could not procure till Dec. 1719, and then I proceeded as follows:

The patient was placed flat on his back, on a table, with a pillow under his head; then his wrists and ancles were fastened together with straps. I then ordered one assistant to his head, another to each of his shoulders, two to the penis, one of which was to manage the ligature, and the other the prepuce, and one to each knee, to hold them as fast and firm as possible. The patient and assistants being thus placed, the operation consists of three parts.

1st. In filling the bladder, which is done thus: pass the catheter; then draw out the stillet, and order the ligature assistant to cast the ligature, with a skein of silk, round the penis, above the glans. Then fix the key to the head of the catheter, to keep it steady, while you screw on the syringe; then screw the second part of the sucking-pipe to the first. Then order the penis ligature to be straightened, and the prepuce assistant to gather the prepuce up about the catheter, and to hold it as close as possible. Order the water, being a little warmer than milk, to be clapped under the sucking-pipe, then draw up the water into the syringe, and thrust it into the bladder at leisure, and repeat it till the bladder is so full that you can perceive its tumour through the abdomen, at which time you will also observe the penis above, and the prepuce below the ligature, very much swelled, and the patient in a great deal of pain, which is a certain sign that there is enough injected; then withdraw the syringe and catheter together, taking particular care that the penis assistants straighten their gripe, lest the water should follow the catheter, which would undo all.

2dly. In making the incision, which is done thus: order the penis assistants to turn the penis towards the anus, that so their hands may be the more out of the way; then take the first knife, and cut at leisure, and with a steady hand, from near the upper part of the tumour of the bladder, or lower, according to the computed size of the stone, down to the os pubis, and exactly in the middle; when you have got a little more than half way through the abdominal muscles, take the second knife, clap its back on the middle of the os pubis, then run its point down towards the sphincter, till you get into the cavity of the bladder, which is discovered by the issuing out of the water; then run the knife along very quickly towards the fund of the bladder, as far as is necessary.

3dly. In extracting the stone, which is done thus: before withdrawing the knife, introduce the fore and middle fingers of your left hand, between the knife and the os pubis, into the bladder; then withdraw the knife, and thrust the fore and middle fingers of your right hand into the anus, and raise the stone up towards the wound, and so you will easily catch hold of it, though ever so small, with your fingers which are in the bladder; then draw it out with the smallest end foremost. Then introduce your fingers again, to see if there are any more stones, which are to be drawn out as before.

Then take a needle and thread, and make one stitch through the skin, in the middle of the wound, and tie it pretty close, then undo the straps and carry the patient to bed.

The patient being put to bed, I laid a pledget armed with balsam over the wound, and a bit of sticking plaster over that. Then embrocating all the abdomen, scrotum and penis, with warm ol. chamæmel. I applied over the dressing and all the abdomen an emollient poultice, spread almost an inch thick on soft flannel; then turning a swath, a little broader than the patient's hand, once round him, I pinned it on the poultice cloth, just tight enough to keep it on. As soon as he was dressed, I gave him an opiate, for nothing is so proper as rest; such as this ℞ Aq. Cinnam. Hord. ℥ii. Laud. Liq. Gutt. xv. Syr. de Mecon. ℥ii. which may be increased or diminished, as the case requires.

Next evening I took off the poultice and dressing, and cut the stitch; then fomented the wound and all the abdomen with stupes wrung out of Aq. Calc. and fresh urine, as warm as he could bear it; then dressed the wound as before. I then rubbed all the scrotum, penis, and groins with unguentum album, to prevent their being scalded by the urine, which flows from the wound.

The wound must be dressed twice a day at least till you have a plentiful digestion. After every dressing, the ointment and oil are used, as before directed.

When the urine begins to come the right way, it pains and scalds them, much after the same manner, as when they had the stone, which is caused by the contraction of the urethra, that has been so long useless, but it never lasts above a day or two, and then they make water with the same ease and freedom as any other person. They ought not to be forced to go to stool under 6 or 7 days, unless there is some particular reason for it; because straining to go to stool injures the wound. Nor ought they ever to be taken up, except to get the bed made, till the urine comes all the right way; because it makes them sick and faint; and consequently hinders the cure of the wound. Cold is to be avoided as the pest, because it puts them to a great deal of pain either to stifle it, or to cough out. If a flexible catheter could be passed, and kept in the passage without pain, it would very much hasten the cure of the wound.

Several other patients were also cut, and treated the same way, with good success.

This operation may be performed with equal success on females, when the stone is large; but when small, the common way of extracting them is very good.

An Account of a Parhelion seen in Ireland. By Arthur Dobbs, Esq. of Castle Dobbs, County of Antrim. N° 372, p. 89.

March 22, 1721-22, about half an hour after 5 in the afternoon, I saw a parhelion, the sun near West, about an hour high, the wind and course of the clouds, about N. and by E. the sky in several places obscured with light clouds, and the sun entering into one somewhat more watery, yet so as to distinguish his disk. At first appeared below the sun, breaking out of the cloud, such rays as are usually seen in an evening in a sky interspersed with clouds. In a little time appeared at the same height with the sun, a luminous spot, about 4 times the size of the sun's disk, and about 30° distant from the sun to the southward, which was covered with the lively shades of red and yellow on the side next the sun, and increased in splendor, so as scarcely to be borne by the naked eye, till it exceeded the brightness of the sun, which was then under a thin cloud, so as easily to show his disk. After this had appeared about 3 or 4 minutes, finding it to be a real parhelion, I began to look about for the halo, they generally appear in; and as I observed some rays resembling a glory to point upwards from the sun, I saw in those rays at the same distance, being about 30° perpendicularly above the sun, the colours of the halo appearing as in the luminous spot; but instead of finding it, as I expected, in a circle surrounding the sun, it was inverted, yet not circular, but making an obtuse angle, the point towards the sun. I then looked to the northward of the sun, and as the cloud, which was thicker on that side, moved southwardly, a luminous spot began to appear at the same distance from the sun as the other, and in the same parallel of altitude, which had the same colours towards the sun, and increased in brightness, but not equal to the brightness of the other spot, yet was as luminous as the sun then appeared: this spot was very little larger than the sun's disk. As the cloud moved on, till it came to about 60° to the southward of the sun, and 30° from the spot, at an equal height there appeared another spot, tinged with the colours of the rainbow. The whole appearance lasted a quarter of an hour. The reason of my not seeing the halos, which generally appear with them, was, that there was a good deal of clear sky above the sun, and the cloud was too thick below it.

Concerning the Particles of Fat. By Mr. Leuwenhoeck, F.R.S. N° 372, p. 93.

I have formerly said, that the matter which we call meal or flour, in wheat, rye, barley, oats, and in all sorts of beans, is shut up as it were in little cells, separated from each other by thin membranes, which are thinnest in wheat. And as, in the inquiry into what is called the periosteum of an ox or sheep, I have often broke in pieces the fat particles, and viewed through a microscope the broken particles, so I have likewise placed a few of the fat globules on a clean glass plate, and afterward held it over a coal fire, or the flame of a candle, till they were all melted and reduced into a liquid matter; so that not only the fat, inclosed in the skin of the fat globules, but the skin itself was reduced to a fluid matter; then viewing it with attention, I perceived, when the melted fat was cold, that there were different matters inclosed in the fat globules; as there appeared an inconceivable great number of exceedingly small coagulated particles, and the rest of the parts, of which the fat was composed, lay in one smooth and even substance.

By the microscope, I saw that the fat particles had such dents in them, as I have shown, are in the globules of flour of wheat: from which I am confirmed more than before in my opinion, that the fat globules might be separated intirely, or in part, from the skin with which they are surrounded, by opening the dents, without breaking the skin. I took off the thin membranes, which encompassed the fat particles, and viewing them with a microscope, observed that the fat particles had imprinted a roundish figure on the membranes, inclining to a hexangular shape; but in other parts they were of an oval figure.

I took a flat fish, called plaise, and took off the fat which adhered to the vessels, or bones, and viewed it with a microscope, and observed, that the fat particles were of several sizes; and some were so small, that I judged that 50 of the least were no larger than one great fat globule; and I also saw that many of the fat globules had such a dent in them, as we find in the meal or flour of those little white beans, which we call French or kidney-beans.

Concerning a new Island lately raised out of the Sea, near Tercera. By Tho. Forster, Esq. N° 372, p. 100.

John Robinson, master of a small pink-snow, from Piscataqua in New-England, arrived at Tercera,* Dec. 10, 1720; near which island he saw a fire break out of the sea. Dec. 18, we got under sail at 12 o'clock at night, and stood from Angras, S. E. The next day at 2 in the afternoon, we made an island, all fire and smoke; and continued our course till the ashes fell on our deck, like hail or snow, all night. We bore from it, the fire and smoke roaring like thunder, or great guns. At day-break we stood towards it again: at 12 o'clock we had a good observation, 2 leagues south from it. We sailed round it, and so near, that the fire and matter it threw out, had like to have done us damage: but a small gale, at S. E. sprung up, and carried us clear, to our great joy. The breeze was accompanied with a small shower of rain, which caused a great dust to fall on our deck. With this breeze we stood away for Tercera. The Governor informed us that the fire broke out Nov. 20, 1720, in the night, and that its prodigious noise caused an earthquake, which

* One of the largest of the Azores, or western islands.

shattered many houses in the town of Angra, and places adjacent. Prodigious quantities of pumice stones and half-broiled fish were found floating on the sea, for many leagues round the island, and abundance of sea-birds hovering about it.

An acquaintance of mine informed me, that in his passage from Cadiz to London, the latter end of April 1721, he observed the sea from Cape Finistere, almost to the chops of the channel, to be covered with pumice-stones, some of which he gave me.

Concerning the Effects of a violent Shower of Rain in Yorkshire. By Mr. Ralph Thoresby, F. R. S. N° 372, p. 101.

The effects of a violent shower of rain at Ripponden, near Halifax, were so surprising, that I wrote to a gentleman in those parts for an account that might be depended on; and particularly desired to know, whether there was not an eruption of waters out of the hills, as the late Mr. Townley wrote me there was out of Pendle-hill, in that at Star-bottom mentioned in the Philos. Trans. N° 245: but all the account I can learn of this is, that what they call the dashing of two great watery clouds on the hills, occasioned the inundation; whatever was the more immediate cause, the effects were dismal, and so sudden, that though it was in the day-time, the poor people could not save their lives. This calamity happened May 18, 1722, between the hours of 3 and 5, when the beck was raised more than 2 yards in perpendicular height, above what was ever known before. Several houses, 4 mills, some say 6, 9 stone-bridges, and 10 or 11 of wood, are broken down, and the wheels, dams, and sluices, of most of the mills that are left standing, broken and damaged; and a great deal of cloth gone. Fifteen persons were drowned.

The rapidity of the torrent was so violent, that it broke down the north-side of Ripponden chapel, and carried off most of the seats. It tore up the dead out of their graves. It swept away all the corn-land, as deep as the plough had gone. Some persons saved themselves by forcing a way out of the roofs of their houses, and sitting upon the ridges till the floods abated.

Concerning a new Experiment made with the Blood of a Person dead of the Plague. By Mons. Couzier. Communicated by Dr. Woodward, F. R. S. N° 372, p. 103.

On the 1st of April I took a quantity of blood from the body of a person dead of the plague, and mixed it with warm water, then I attempted to inject it into the crural vein of a dog; but the end of the syringe being too large to enter the vein, the experiment did not succeed.

On this I laid some of the infected blood on the wound, and covered it with a dressing, which the dog got off in the night. I found the next morning that the dog had licked his wound, and that he refused his food. Towards night he began to moan, and gave signs of an approaching death. The next morning I found him dead, the wound being considerably swelled and gangrened, and the edges round the swelling likewise gangrened.

On opening the body, we found the liver something larger than usual, with spots of a livid purple, as in the bodies of persons dead of the plague. In the stomach was found a quantity of black coagulated blood, of the size of a hen's egg. This in all likelihood was what he had swallowed on licking the wound. The heart was very large, with a black grumous blood in the ventricles, and the auricles were turned blackish and gangrenous.

Extract of a Letter from Dr. Deidier, concerning an Experiment made with the Bile of Persons dead of the Plague. Communicated by Dr. Woodward. N° 372, p. 105.

We caused two dogs to swallow a pretty large quantity of the bile taken from the bodies of persons dead of the plague. On this the dogs appeared heavy and melancholy, refused their food, and made water very often, especially when they were any ways disturbed. Their urine was thick and very fetid, and their gross excrements were tinged with the black and greenish bile which they had swallowed. But in a few days those accidents went off, and the dogs recovered their perfect health.

Solution of the Problem, of finding Curves, which may cut each other in a Given Angle. N° 372, p. 106.

This solution is anonymous: but it seems it was given by Dr. Henry Pemberton, as we are informed by Dr. James Wilson, in his account of the life of Dr. Pemberton, prefixed to his edition of the Doctor's Lectures on Chemistry at Gresham college.

This was the noted problem proposed by M. Leibnitz and M. John Bernoulli, to the English mathematicians, which had before occasioned several controversial writings. But as we have already inserted some other solutions of it in these Abridgments, viz. by Sir I. Newton, Dr. Taylor, &c. it is quite unnecessary to abstract the present one also.

An Account of a Book, entitled, Harmonia Mensurarum, sive Analysis et Synthesis per Rationum et Angulorum mensuras promotæ: accedunt alia Opuscula Mathematica: per Rogerum Cotesium. Edit et auxit Robertus Smith. Cantab. 1722, *4to.* N° 372, p. 139.

The book consists of three parts. In the first, called Logometria, the author's chief design is to show how that sort of problems, which are usually reduced to the quadrature of the hyperbola and ellipsis, may be reduced to the measures of ratios and angles; and afterwards be solved more readily by the canons of logarithms and sines and tangents. He defines the measures of ratios to be quantities of any kind, whose magnitudes are analogous to the magnitudes of the ratios to be measured. In this sense any canon of logarithms is a system of numeral measures of the ratios of the absolute numbers to an unit: the parts of the asymptote of the logistic line, intercepted between its ordinates, are a system of linear measures of the ratios of those ordinates: the areas of an hyperbola, intercepted between its ordinates to the asymptote, are a system of plane measures of the ratios of those ordinates: and since there may be infinite systems of measures, according as various kinds of quantities are made use of, such as numbers, time, velocity, and the like; or according as the measures of any one system may be all increased or diminished in any given proportion; in such variety much confusion may possibly arise as to the kind and absolute magnitudes of particular measures, which happen to fall under consideration. Our author very happily removes this difficulty; by showing that the nature of the subject points out the measure of a certain immutable ratio for a modulus in all systems, by which to determine the kind and absolute magnitudes of all other measures in each system.

The first proposition is to find the measure of any proposed ratio. This he considers in a way so simple and general, as naturally leads to the notion and definition of a modulus; namely, that it is an invariable quantity in each system, which bears the same proportion to the increment of the measure of any proposed ratio, as the increasing term of the ratio bears to its own increment. He then shows that the measure of any given ratio, is as the modulus of the system from whence it is taken: and that the modulus in every system, is always equal to the measure of a certain determinate and immutable ratio, which he therefore calls the ratio modularis. He shows that this ratio is expressed by these numbers 2.7182818 &c. to 1, or by 1 to 0.3678794 &c. So that in Briggs's canon, the logarithm of this ratio is the modulus of that system: in the logistic line, the given subtangent is the modulus of that system: in the hyperbola, the given parallelogram, contained by an ordinate to the asymptote and the absciss from the centre, is the modulus of that system: and in other systems, the modulus is generally some remarkable quantity. In the second proposition he gives a concise uncommon method for calculating Briggs's Canon of Logarithms; with rules for finding intermediate logarithms and numbers, even beyond the limits of the canon. In the 3d proposition he constructs any system of measures by a canon of logarithms; not only when the measure of some one ratio is given, but also without that datum, by seeking the modulus of the system by the rule abovementioned. In the 4th, 5th, and 6th propositions, he squares the hyperbola, describes the logistic line and æquiangular spiral by a canon of logarithms, and shows some curious uses of these propositions in their scholia. Take an easy example of the logometrical method, in the common problem for finding the density of the atmosphere. Supposing gravity uniform, every one knows, that if altitudes are taken in any arithmetical progression, the densities of the air in those altitudes will be in a geometrical progression; that is, the altitudes are the measures of the ratios of the densities below and in those altitudes, and so the difference of any two altitudes is the measure of the ratio of the densities in those altitudes. Now to determine the absolute or real magnitude of these measures, the author shows, a priori, that the modulus of the system, is the altitude of the atmosphere, when reduced every where to the same density as below. The modulus therefore is given, as bearing the same proportion to the altitude of the mercury in the barometer, as the specific gravity of mercury does to the specific gravity of air, and consequently the whole system is given. For since, in all systems, the measures of the same ratios are analogous among themselves; the logarithm of the ratio of the air's density in any two altitudes, will be to the modulus of the canon, that is, to the logarithm of the ratio modularis defined above, as the difference of those altitudes, is to the aforesaid given altitude of the homogeneous atmosphere.

He concludes the logometria with a general scholium, containing great variety of elegant constructions both logometrical and trigonometrical; such as give the length of curves, either geometrical or mechanical; their areas and centres of gravity; the solids generated from them, and the surfaces of these solids; with several curious problems in natural philosophy, concerning the attraction of bodies, the density and resistance of fluids, and the trajectories of planets. Several of these problems have two cases; one constructed by the measure of a ratio, and the other by the measure of an angle. The great affinity and beautiful harmony of the measures in these cases, have given occasion to the title of the book. The measures of angles are defined, just as the measures of ratios, to be quantities of any kind, whose magnitudes are analogous to the magnitudes of the angles. Such may be the arcs or sectors of any circle, or any other quantities of time, velocity, or resistance, analogous to the magnitudes of the angles. Every system of these measures has likewise its modulus homogeneous to the measures in that system, and may be computed by the trigonometrical canon of sines and tangents, just as the measures of ratios by the canon of logarithms; for the given modulus in each system, bears the same proportion to the measure of any given angle, as the radius of a circle bears to an arc which subtends that angle, or the same as this constant number of degrees 57.2957795130 bears to the number of degrees in the said angle.

Why the author takes his principles to be so general, will further appear by an instance or two. In the problem already mentioned he measures the ratio of the air's densities in any altitudes, by the altitudes themselves, making use of the altitude of a uniform atmosphere for the modulus. So likewise when he considers the velocities acquired, and the spaces described in given times, by a body projected upwards or downwards in a resisting medium with any given velocity; he shows, that the times of descent, added to a given time, are the measures of ratios, to a given modulus of time, whose terms are the sum and difference of the ultimate velocity and the present velocities that are acquired: that the times of ascent, taken from a given time, are the measures of angles, to a given modulus of time, whose radius is to their tangents, in the ratio of the ultimate velocity to the present velocities: and lastly, that the spaces described in descent or ascent, are the measures of ratios to a given modulus of space, whose terms are the absolute accelerating and retarding forces arising from gravity and resistance, taken together at the beginning and end of those spaces.

This general account may suffice to illustrate what I am going to say; that since the magnitudes of ratios, as well as their terms, may be expounded by quantities of any kind, the mathematician is at liberty on all occasions to chuse those which are fittest for his purpose; and such are they without doubt, that are put into his hand by the conditions of the problem. He may indeed represent these quantities by an hyperbola, or any other logometrical system, were not his purpose answered with greater simplicity by the very system itself, which occurs in each particular problem. And the same may be said for the systems of angular measures, instead of recurring on all occasions to elliptical or circular areas.

As to the convenience of calculating from our author's constructions, he shows that the measures of any ratios or angles, are always computed in the same uniform way; by taking from the tables the logarithm of the ratio, or the number of degrees in the angle, and then by finding a fourth proportional to three given quantities; for that will be the measure required. The simplest hyperbolic area may indeed be squared by the same operation taught in the author's 4th proposition; but the simplest elliptic area requires somewhat more: those that are more complex in both kinds, which generally happens, require an additional trouble to reduce them to the simplest: to square them by infinite series is still more operose, and does not answer the end of geometry. On the whole therefore it may deserve to be considered, for what purposes should problems be always constructed by conic areas, unless it be to please or assist the imagination. The design of theoretical geometry differs from problematical; the former consists in the discovery and contemplation of the properties and relations of figures for the sake of naked truth; but the design of the latter is to do something proposed, and is best executed by the least apparatus of the former.

The logometria was first published by the author himself, in the Philos. Trans. of the year 1714, N° 338. But his logometrical and trigonometrical theorems abovementioned, were not published till after his decease. These theorems make the 2d part of the book, and are calculated to give the fluents of fluxions, reduced to 18 forms, by measures of ratios and angles; in such a manner, that any person may perfectly comprehend their construction and use, though altogether unacquainted with curvilinear figures, as expressed by equations. And this circumstance also renders their application to the analysis and construction of problems extremely easy. Of this kind the author has given a great many choice examples, both in abstract and physical problems; which make up the third and last part of the book.

The author, a little before his decease has informed us (in a letter of May 5, 1716, written to his friend Mr. Jones, "That geometers had not yet promoted the inverse method of fluxions, by conic areas, or by measures of ratios and angles, so far as it is capable of being promoted by those methods. There is an infinite field (says he) still reserved, which it has been my fortune to find an entrance into. Not to keep you longer in suspense, I have found out a general and beautiful method by measures of ratios and angles, for the fluent of any quantity which can come under this form $\frac{d\dot{z}z^{\theta n + \frac{\delta}{\lambda}n - 1}}{e + fz^n}$, in which d,e,f are any constant quantities, z the variable, n any index, θ any whole number affirmative or negative, $\frac{\delta}{\lambda}$ any fraction whatever. The fluents of this form, which have hitherto been considered, are $\frac{d\dot{z}z^{\theta n - 1}}{e + fz^n}$ & $\frac{d\dot{z}z^{\theta n + \frac{1}{2}n - 1}}{e + fz^n}$: these you remember are Sir Isaac Newton's first two, and from these all his others are easily deduced. And as his irrational forms of the quadratic kind are derived from the rational, so from my general rational form I deduce irrational ones of all kinds. For instance, if $\frac{\delta}{\lambda}$ represent any affirmative or negative fraction, the fluent of any quantity of this form

$d\dot{z}z^{\theta n - 1} \times \overline{e + fz^n}|^{\frac{\delta}{\lambda}}$, or of this $d\dot{z}z^{\theta n - 1} \times \overline{\frac{e + fz^n}{g + zh^n}}|^{\frac{\delta}{\lambda}}$, and so of some others, depends upon the measures of ratios and angles. Mr. Leibnitz, in the Leipsic Acts of 1702, p. 218 and 219, has very rashly undertaken to demonstrate, that the fluent of $\frac{\dot{x}}{x^4 + a^4}$ cannot be expressed by measures of ratios and angles; and he swaggers upon the occasion (according to his usual vanity) as having by this demonstration determined a question of the greatest moment. Then he goes on thus; as the fluent of $\frac{\dot{x}}{x + a}$ depends upon the measure of a ratio, and the fluent of $\frac{\dot{x}}{xx + aa}$ upon the measure of an angle; so he had more than once expressed his wishes, that the progression may be continued, and it be determined to what problem the fluents of $\frac{\dot{x}}{x^4 + a^4}$, $\frac{\dot{x}}{x^8 + a^8}$, &c. may be referred. His desire is answered in my general solution, which contains an infinite number of such progressions. I can go yet further, and show him how by measures of ratios and angles, without any exception or limitation, the fluent of this general quantity $\frac{d\dot{z}z^{\theta n + \frac{\delta}{\lambda}n - 1}}{e + fz^n + gz^{2n}}$ or even this $\frac{d\dot{z}z^{\theta n + \frac{\delta}{\lambda}n - 1}}{e + fz^n + gz^{2n} + hz^{3n}}$ may be had; where θ, as before, represents any integer, and the denominator λ of the fraction $\frac{\delta}{\lambda}$, represents any number in this series, 2, 4, 8, 16, 32, &c. any whole number being denoted by its numerator δ. In truth I am inclined to believe, that Mr. Leibnitz's grand question ought to be determined the contrary way; and that it will be found at last, that the fluent of any rational fluxion whatever, does depend upon the measures of ratios and angles, excepting those which may be had in finite terms even without introducing measures."

Dr. Taylor, knowing by this letter what the author had done, was pleased to propose the invention of the fluents of the last two fluxions, as a problem to the mathematicians in foreign parts. Mr. Bernoulli, in the Leipsic Acts of 1719, p. 256, showed accordingly how they are reducible to conic areas. The editor has published the author's own solution by measures of ratios and angles; and on this foundation has constructed new tables of logometrical and trigonometrical theorems. for the fluents of fluxions, reduced to 94 forms, part rational and part irrational. He has also added general notes on the chief difficulties in the book, with a method of composing synthetical demonstrations of logometrical and trigonometrical constructions, illustrated by various examples.

The first treatise in the miscellaneous works, is concerning the estimation of errors in mixt mathematics. It consists of 28 theorems, to determine the proportions among the least contemporary variations of the sides and angles of plane and spherical triangles, while any two of them remain invariable. An example will show their great use in astronomy. The time of the day or night is frequently to be determined by the altitude of some star. Let it then be proposed to find the error that may arise in the time, from any given error in taking the altitude. By applying the 22d theorem to the triangle formed by the complements of the star's altitude and declination, and by the complement of the pole's elevation, the author shows, that the variation of the angle at the pole, and consequently the error in time, will be as the error in the altitude directly, as the sine complement of the pole's elevation inversely, and as the sine of the star's azimuth from the meridian inversely. Consequently, if the error in the altitude be given, under a given elevation of the pole, the error in time will be reciprocally as the sine of the azimuth contained by the meridian and the vertical which the star is in. This error therefore will be the same, whatever be the altitude of the star in the same vertical; and will be least when the vertical is at right angles to the meridian. But will be absolutely the least in the same circumstance, if the observer be under the equator. In which case, if the error in the altitude be one minute, the error in the time will be 4 seconds. If the observer recedes from the equator, towards either pole, the error will be increased in the proportion of the radius to the sine complement of the latitude: so that in the latitude of 45°, it will be $5\frac{2}{3}$ seconds; and in the latitudes of 50 and 55, it will be $6\frac{2}{9}$ and $6\frac{37}{38}$ seconds respectively. If the star be in any other vertical, oblique to the meridian, the error will still be increased in the proportion of the radius to the sine of that oblique angle. Lastly, if the error in the altitude be either more or less than one minute, the error in time will be more or less in the same proportion. Much after the same manner may the limits of errors be computed in other cases, which arise from the inaccuracy of observations, and from hence the most convenient opportunities for observing are also determined.

The second treatise, is concerning the differential method. The author, having written it before he had seen Sir Isaac Newton's treatise on that subject, has handled it after a manner somewhat different.

The title of the 3d treatise, is Canonotechnia, or concerning the construction of tables by differences. It consists of 10 propositions, most admirably contrived for expeditious computation of intermediate terms in any given series. The last proposition, which contains a general solution of the whole design, is this, datis seriei cujuscunque terminis aliquot æquidistantibus, quorum intervalla secanda sunt in æquales quotlibetcunq; partes, propositum sit invenire terminos interserendos.

The book concludes with three small tracts, concerning the descent of bodies, the motion of pendulums in the cycloid, and the motion of projectiles, composed in a very natural and easy manner.

The author has written some other pieces, yet unpublished, which the editor has given an account of in his preface to the book.

Observations on a Fœtus, and the Parts of Generation of a Sheep. By Mr. Leuwenhoeck, F. R. S. N° 373, p. 151.

A ewe, which within 2 years had twice lambed, happened to be covered by a young ram, about 20 weeks old; about 5 days after the ewe was killed, and out of her belly was cut 28 lb. of fat. But observing, on opening her, that the uterus was four times larger than ordinary, the butcher brought it, with the ovaria, to Mr. L. assuring him, that it was not yet quite 5 days since the young ram had covered the sheep, and that there was no other ram thereabouts.

Mr. L. began first to try to penetrate into the uterus from the vagina, with the point of a small pair of scissars; but found it so close that he could not; he therefore cut a piece off from the uterus, out of which ran a clear water, and within it lay the fœtus with all its coverings. Spreading this on the backside of a China tea-dish, and finding that it still contained more water, he made a small incision to drain it, and to let it dry, to observe it the better. He could plainly see the vertebræ of the neck and back, as also the joints of its short tail; he thought likewise that he saw the eyes. But when it was quite dry, he could not observe its backbone so well as before, when it was as yet moist, though the painter, who made the draught, and had sharper and younger eyes, saw the bones of the back very distinctly. The design in drying it, was to cut it in small slices, the better to observe the inner parts: for it was so extremely soft and tender when moist, that with the least touch its parts would be disordered and confounded. He therefore cut the fœtus into 15 slices, and observed them with a microscope, but could not be very certain about what he saw; though he imagined he saw the intestines, the bladder, and the heart; but he saw and observed, with a great deal of pleasure, that two blood-vessels lay near together in the brain, and how they were spread into branches.

Some persons might expect, that he should have looked for the extremities of the blood-vessels; but these have no termination, as he has frequently said. Besides, they become gradually so exquisitely fine, that the blood which passes through them, can exhibit no red colour to our eyes; so that there is no tracing them when entering into the vessels that return the blood back to the heart, except in living animals, where one may see the blood enter into the returning vessels. Before the butcher gave him the uterus, he squeezed it between his fingers, and said he could feel nothing in it; and this he had probably done several times, by which means he tore off the vessels by which the fœtus was fastened to the uterus; which might be the occasion that, on opening the uterus, the fœtus with its coverings came forth so easily.

Observations on the Callus of the Hands and Feet. By the same. N° 373, p. 156.

In September 1719, Mr. Leuwenhoeck feeling an acute pain in one of his feet, at the joint between the foot and the little toe, which he imagined to proceed from the more than usual thickness of the callus or hard skin on that part; he caused his servant, partly with his nails and partly with a penknife, to take off that hard skin, and let it fall upon a blue paper. This callus was composed of little scaly shivers, lying over each other, and the whole piece was as large as a small nail of a man's hand.

He viewed these shivers through a microscope, but could not satisfy himself, because they lay so irregularly on each other. Then taking a little bit of it, he laid it on a clean glass plate, steeped it in pure rain-water, and gently dividing it with a piece of a quill, he was amazed to see into what a vast number of particles it separated, and that with as much readiness as if they had never been joined.

Afterwards he took two or three of the said particles, several of which were of the figure of a weaver's shuttle, broad in the middle and pointed at each end, with a line in the middle, like those upon the uppermost or outside skin of fruits, or of our bodies, but generally irregular; they were very thick in proportion to their size; having laid two or three of the particles on a clean glass, and put to them a drop of water as large as a coarse grain of sand, and divided them; on viewing the divided particles through a microscope, he was astonished at the prodigious number of exceedingly small particles that occurred to the sight, and which were of the same figure as beforesaid.

Since these observations concerning the friction or rubbing of his hands, Mr. L. took more notice of it, when washing and drying them, than formerly; and was amazed at the numerous particles that daily separate themselves from the hands, and grow on them again; and at the particular provision made for producing these particles in the palms of our hands and bottom of our feet; whereas we do not by far meet with such a quantity of particles constantly produced in other parts of the body; for if we observe those who work much with the back of their hands, we shall not meet with any of that hard skin we have been speaking of, but only a kind of tumour, or rising, as the dry-sheerers, or those who dress cloth, have on their left hands. In short, the manner of the production of these small particles will be a mystery to us, though our hands and feet must be fortified with such a matter, to enable them to support all that force and pressure, which they are obliged to bear.

Of the Reducing Rational Algebraic Fractions to Simpler Fractions; and of Summing any Terms of Series at equal Distances from each other. By Mr. Abraham Demoivre. N° 373, p. 162.

This paper by Mr. Demoivre was afterwards enlarged and improved, and made the 1st and 2d books of his Miscellanea Analytica, to which it is proper to refer for the more perfect state of the paper.

A Defence of the Dissertation of Running Water, published in the Philos. Trans. N° 355, *against the Animadversions of Sig. Pet. Ant. Michelotti. By Dr. Jurin, Sec. R. S.* N° 373, p. 179. *Translated from the Latin.*

S. Michelotti's animadversions in his book De Separatione Fluidorum in Corpore Humano, on a dissertation of Dr. Jurin's on the Motion of Running Waters, published in Philos. Trans. N° 355, are partly owing to his not thoroughly understanding the drift of that dissertation, and partly that some things are not put in so clear a light as they are capable of: to obviate which, the Dr. first explains, what is to be understood by the motion of water running out at a hole in the bottom of a vessel; for, there is a wide difference between the motion, or the quantity of motion of water, running out at a hole in a vessel, which motion is in a compound ratio of the quantity of water running out at the hole in any given time, and of the velocity with which it runs out, and between the motion of the whole quantity of water or cataract of water, descending within the vessel towards the hole, and immediately about to flow out; this motion being in a ratio of the sum of all the products of each particle of water, constituting the cataract, multiplied into their respective velocities. The Doctor observing that one of these motions was often taken for the other, chose to illustrate the latter in his said dissertation, and bring it to a calculation, and apply it to the fluids in animal bodies.

Since this motion then was what the Doctor always meant by the motion of running waters, as plainly appears by all his propositions, he thought he might justly allege, that this motion had not hitherto been defined by any one, as far as he knew; no mathematician having even so much as hinted at it; the Doctor is therefore surprised, that neither Michelotti nor John Bernoulli were aware that in the preface to that dissertation, so often cited and so much censured by Michelotti, he did not so much as mention the velocity with which water runs out at a hole, much less the velocity determined by Bernoulli.

In order to define the said motion, the Doctor needed no other than his third general theorem; but since he thought the property of the Newtonian hyperbolic curve, in which Sir Isaac Newton forms the cataract of the descending water, not unworthy the consideration of geometricians, he would by the bye premise some things about it, as taken from prop. 36, lib. 2, Princip. Mathem. Philos. Nat.

For, it is plain that such a cataract should be formed by water descending freely, and accelerated in the manner of all other heavy bodies, without any other water surrounding it. And even if the cataract of water be surrounded with a hollow crust of ice, exactly answering to its figure, and by reason of its extreme polish making no resistance to it; the cataract of water will not in the least press on the ice, but only touch it and descend quite freely; whence no alteration will be produced either in the figure, or velocity of the descending cataract. But if the circumambient ice be dissolved, there is no manner of occasion for such a strong battery as Michelotti, p. 128, 129, 130, and likewise John Bernoulli, have raised to break down the Doctor's slender cataract; since Sir Isaac Newton himself has quite dissolved it, when he says, Princip. p. 304, Liquescat jam glacies in vase, &c.

The Doctor does not deny, but that there is some difference between the case as laid down by Sir Isaac Newton, and by himself; for the cylinder of ice which the former supposes to descend with a given uniform velocity, and to dissolve as soon as it touches the surface of the water, contained in the vessel, that the vessel may be always kept equally full, the Doctor has omitted, and instead of it has supposed an infinite surface of water, that by that means he might represent the whole solid or hyperbolic cataract: yet this position alters nothing, either in the velocity or motion of the running water.

What S. Michelotti says, p. 127, that the Doctor begs the question; and a little lower, that the question therefore ceases, and that the whole demonstration becomes an hypothesis; this the Doctor does not understand: for in loc. citat. the question was not about the velocity of the effluent water, nor was there any demonstration adduced to prove that velocity; but the only thing the

Doctor intended was, from that velocity to deduce the equation of the Newtonian hyperbolic curve; for the Doctor had already determined the velocity of the effluent water, or rather assumed it, viz. by supposing what Sir Isaac Newton had done, viz. that water by the force of gravity falls freely, and in falling is accelerated.

Here the Doctor takes notice of an error, Michelotti, p. 112, 113, would rashly fasten on Sir Isaac Newton, Huygens, and Mr. Keill, viz. that they have supposed the force, by which the whole motion of the effluent water may be produced, equal to the weight of a cylindrical column of water, whose base is the hole, and height double that of the water contained in the vessel; this Sir Isaac Newton had briefly but perspicuously demonstrated in the second corol. of the aforesaid prop. And another demonstration of it might be deduced from considering the entire hyperbolic cataract, which is equal to this cylinder, and whose entire weight is spent on the descent of the water; but that is unnecessary, since the same thing follows very evidently from Bernoulli's own proposition, which Michelotti so often commends and so strenuously defends. This will easily appear, if, laying aside for a while the consideration of the column of water, incumbent on the hole, and making the calculation, he would, from the mass of water running out at the hole in any given time, and from the velocity with which Bernoulli has determined water to run out, determine the motion of the water, and then find the weight which, by the force of gravity, falling freely in the same given time, may produce the same quantity of motion; he will find this weight equal to that of double the column of water incumbent on the hole, just as Sir Isaac Newton had determined it in that corol. But the same weight, suspended at one arm of a balance, will be kept in equilibrio by the impetus of the water, at its very first efflux out of the hole, impinging in a continued stream on the other equal arm of the balance, and falling down immediately after the impulse, which will easily appear on making the calculation.

Dr. Jurin would here also remove a prejudice which Michelotti, p. 113, and others labour under, Sir Isaac Newton had demonstrated, prop. 37, lib. 2, Princip. first edit. that water runs out at a hole in the bottom of a vessel with that velocity, with which it might rise to half the height of the water contained in the vessel. Experience is said to contradict this, by which it is discovered, that the effluent water rises to the whole height; and Sir Isaac Newton himself in the solution of the same problem, prop. 36, lib. 2, the second edit. ascribes to the water that velocity with which it might rise to its whole height; consequently he seems to contradict himself. But if this matter be accurately and judiciously considered, it will be found to agree very well both with Sir

Isaac Newton's first and second solutions, and likewise with experience; for, in his second solution, he supposes the jet of water narrower in diameter at a small distance from the hole, than in the hole itself, in the ratio of 21 to 25; therefore the section of the jet at that distance, is to the hole itself, as 21×21 to 25×25, that is, as 1 to $\sqrt{2}$ nearly. And since the same quantity of water flows in a given time, either through the section of the hole, or that of the contracted jet, and consequently the velocities of the water in these sections are reciprocally as the sections themselves, the velocity of the water in the hole will be to the velocity of that in the contracted jet, as 1 to $\sqrt{2}$; consequently if the velocity in the contracted jet be such, as that the water can rise to the whole height of that contained in the vessel, the velocity of the water will not be greater in the hole itself, than what can raise it to half the height; these two solutions therefore are consistent with each other and experience; for if by either of the solutions, from the given velocity with which water is supposed to pass either through a hole, or contracted jet, by calculation the quantity of water to run out be found; the same will be found to agree nearly with the quantity of water discovered by experiments; and even Sir Isaac Newton's own experiment, taking a hole whose diameter is $\frac{5}{8}$ parts of an inch, agrees with this calculation; as also several other experiments with holes of smaller dia meters, made at London; it is true, the experiments made by the accurate Poleni differ somewhat from these, but yet they give a less quantity of water than according to this calculation, and never a greater, because probably the vessels were narrower in proportion to the size of the holes.

There still remains one animadversion more, or rather scruple, p. 101, 102, arising hence, that in corol. 17, theor. 3, of the abovementioned dissertation, Dr. Jurin had supposed the motion or impetus of the blood to be greater in all the capillary arteries taken together than in the aorta itself; to explain this, Michelotti would fasten on the Doctor an hypothesis of a greater density of blood in the capillary arteries than in the aorta. Dr. Jurin disowns any such hypothesis, having deduced the corol. from the foregoing theor. which treats of the motion of water running through any full pipe; whence it appears, that the blood is no otherwise considered in his corollaries than as it is fluid, and resembles water. But Michelotti's scruple appears to proceed from his taking the impetus of the blood to be the quantity of its motion, produced by multiplying the velocity into the mass, running through in a given time; which is quite different from Dr. Jurin's motion or impetus, he having in that theorem supposed it equal to the motion of a mass of water, which runs out of a pipe in any given time, and whose velocity is such, as in the same time to run over a space equal to the length of the pipe. The aforesaid corol. easily flows from this theorem; since in a given time an equal mass of blood runs through the aorta and capillary arteries, and the length of the tube, consisting of the aorta and capillary arteries, is greater than the aorta alone. This the Doctor has the rather observed, because not only Michelotti, but other mathematical writers, in treating of forces, which either put into motion the liquor, contained in pipes filled with it, or stop its efflux, consider only the mass and velocity of the effluent fluid, without regarding the length of the pipes; for, cæteris paribus, a fluid is with greater difficulty, either thrown out of a full pipe, or stopped in its efflux, the longer the pipe is; since the whole mass of fluid contained in the pipe must be put into motion before any part can flow out at the orifice; as also the entire mass be necessarily stopped, to hinder the efflux of any part just ready for it.

'The principles of Bernoulli's demonstration of the velocity of water running out of a hole of a full vessel, are, that the lowest drop of the liquor, or that immediately incumbent on the hole of the vessel, is considered as pressed on, or, as he calls it, animated by a certain accelerating gravity, which is to the natural gravity, as the height of the water, or of the whole liquor, incumbent on the hole of the vessel, to the height of the small drop; that is, as the absolute weight of the column of water, insisting on the hole of the vessel, to the absolute weight of the drop; for thus, nothing remains but to find how great a velocity the drop, animated by that greater gravity, may acquire, when it falls through a line equal to its height; that is, after it has got quite out of the hole; for it is pressed on by the whole column of water, consequently animated by the greater gravity, so long as any of the drop, which he supposes a small solid column, remains above the hole.'

The weakness of this foundation appears thus; since Bernoulli makes use of nothing to animate, as he calls it, the lowest drop with the aforesaid accelerating gravity, but pression alone, or the weight of the column of water insisting on the hole: let all the water, surrounding that column, be supposed to be frozen, and the column to fall without any resistance along the smooth surface of the ice; then as long as the hole is shut up, the small drop next the hole will be pressed on by the whole weight of the incumbent column of water, in the manner Bernoulli supposes. Now let the hole be opened, and a free exit be given to the water. What will then be the consequence? will the lowest drop be urged or animated by the accelerating gravity, which is to the natural gravity, as the height of the whole water incumbent on the hole, to the height of the drop? by no means; but it will be urged only by its own natural accelerating gravity; for, as soon as the lowest drop begins to descend, though with an infinitely small velocity, it will no longer be urged by the weight of the incumbent column of water; for, it is impossible for the column of water to press on the subjacent drop without being hindered in its descent by that drop; but it is not hindered, because it does not endeavour to descend with greater velocity than the lowest drop tends downwards by its own force of gravity; for the column and drop descend equally; so that the drop will neither quit the column, nor receive any force or pression from it.

The Doctor therefore thinks Bernoulli's demonstration falls to the ground; his mistake seems to be owing to his not adverting to the difference between a body pressed on by an incumbent weight, when that weight is only urged by the natural accelerating force of gravity, and a body impelled, or animated, as he calls it, by the accelerating force of gravity, preternaturally increased: in the latter case, the body will descend with a greater velocity than what can be produced by the natural force of gravity, according to Bernoulli himself; but in the former case, however, the body pressed upon, while it is at rest, may be urged by the incumbent weight; yet as soon as it begins to descend, it will do so entirely with the same degree of velocity, as if it were not pressed on by any incumbent weight.

To illustrate this by an example: suppose a solid column, consisting of 100 pieces of gold, laid upon each other, at rest on a table, and the lowest piece pressed on by the weight of the rest; now if a hole be made in the table, under the pieces, that the undermost may slip through; as soon as it begins to fall, it is immediately freed from the weight of the incumbent pieces; and then the undermost piece, and all the rest will descend, with the same velocity as if there were only that lowest piece on the table.

If from the velocity with which, according to Bernoulli, water runs out at a hole, and from the mass of water, as determined by that velocity running out in any given time, any one would determine its motion, he would find it twice greater than what could be produced by the force of gravity, from the weight of the column of water incumbent on the hole.

The Doctor recommends the two following experiments, in order with more certainty to determine the controversy, either to be tried a-new, or at least diligently considered; the one is Sir Isaac Newton's, described p. 305, Princip. second edit. viz. From the mass of water, running out in a given time, to find the velocity with which it passes through the hole; the other is Mariotte's, in his book De Mouvement des Eaux, part 2, disc. 3, regl. 1, and made with a cylindrical pipe, open at both ends, its lower part turned upwards, and full of water; whence it may be easily estimated, whether the first drops of effluent water can rise to so great a height, as Bernoulli's demonstration requires.

On a remarkable Instance of the Infection of the Small-pox. By Dr. Jurin, Sec. R. S. N° 373, p. 191.

A young gentleman, ill of the small-pox, of that sort called the coherent, or the middle between the distinct and the confluent kind, on Wednesday, Oct. 3, 1722, being the 6th day from the eruption, became delirious in the night, and got out of bed in spite of the opposition of two nurses that attended him, and seizing one of his nurses by the neck with his bare arms, he pressed her forehead against his naked breast, then covered with the small-pox in the state of maturation, and held her for some time in that manner. She was heated by striving with him, and in struggling to get loose, she was sensible that she bruised and broke some of the pustules with her forehead. The woman was about 40 years of age, of a clear, florid, sanguine complexion; she said she had had the small-pox, when she was about 7 or 8 years of age, and had been pretty full of them, though I saw no marks on her face. On Friday morning the small-pox began to appear on her forehead, and increased by degrees to between 50 and 60; she had likewise a small number of pustules on the back part and sides of her neck, where the gentleman had grasped her with his naked arms; but had none, as she told me, on any other part of her body. The lower part of her face was perfectly clear, and those on her forehead were chiefly confined to the middle and most prominent part of it, which had been pressed against the gentleman's breast. They rose gradually, and came to maturity, in the same manner as the small-pox of the milder coherent kind use to do, with a great inflammation and swelling of her forehead, and the adjoining part of her face, especially between the eye-brows, where a small cluster of the pustules were seated, insomuch that on Tuesday the 9th of October, her right eye was quite closed up, and the left almost in the same condition; but all this time she had no fever, sickness, or other symptom of the small-pox, except this eruption, and the inflammation and pain that attended it. That night she caused a blister to be applied to her neck, on which she recovered the sight of her eye the next day, being the 6th from the eruption, when the pustules were turning, and beginning to scab. The scabs agreed with those of the milder coherent small-pox in their appearance and duration. I saw her hitherto every day, as likewise at several times after this, and particularly on Monday, Oct. 22, which was the 18th day from the eruption of the pustules, when she had still some small part of the scabs remaining on her forehead.

In this instance it is worthy of remark: 1. That this woman, though she had had the small-pox before, was yet infected again by the immediate and close application of the variolous matter to her skin, when her body was heated with exercise. Which seems to prove, that such an application is more effectual to give the infection than the bare morbid effluvia, arising from the body of the sick person, and received into the sound one by inspiration; for that she received no infection by inspiration is plain, from the appearing of the small pox upon those parts only where there had been such an application and contact. From which it appears very probable, that a person who has already had the small-pox, as the man inoculated by Mr. Tanner in St. Thomas's Hospital, may possibly receive it again in some slight degree by inoculation; that being still a more close and immediate application of the variolous matter to the blood and juices of the sound person, than when it is applied only by contact to the skin whole and unwounded.

2. That the infection communicated to this woman not being universal, as appears from her having no fever, or sickness, or general eruption of the pustules all over her body, but only on the parts infected by immediate contact, no argument can be drawn from hence, for a person's being liable to undergo the small-pox a second time, so as to have the usual symptoms of that disease, and a general eruption of the pustules, but rather the contrary.

3. That the time in which this infection showed itself, by the appearance of the pustules, is very different from that observed on inoculation; the first appearing in about a day and a half; whereas in the latter case, the eruption generally shows itself on the 10th day, or not above a day sooner or later, as appears from the accurate and curious observations of Dr. Nettleton. Which difference is what ought in reason to be expected, since in one case the infection went no farther than the parts affected by immediate contact; whereas in the other it must be propagated through the mass of blood to all parts of the body.

An Account of two Observations on the Cataract of the Eye. By Sig. Antonio Benevoli, Master Surgeon in the Hospital of S. Maria Nuova in Florence. N° 373, p. 194.

On the 13th of July, 1720, S. Benevoli had couched a German soldier of cataracts in both his eyes, who immediately after the operation recovered the sight of them, and continued to see till his death, which happened by an acute illness on the 6th of April, 1722. On this, S. Benevoli took the eyes out of their orbits, in order to examine whether the cataracts, which this soldier had been couched of, consisted of a membranous pellicle, as some writers maintain; or, as others pretend, of a preternatural opacity in the crystalline humour. Proceeding immediately to the dissection of the left eye, on a careful and very exact examination of all its contents, he could not find any such thing as a pellicle within it, but discovered a small yellowish body at the bottom of the bulb of the eye, of a lenticular shape, without adhesion to any of the other parts, which appeared to be the crystalline humour, become opaque, and rather less than its natural size, having two or three small dents, or impressions, made in its circumference, which it had received from the needle during the operation of couching.

The next day he examined the right eye in the same manner, in the presence of several eminent physicians and surgeons, and found in it the crystalline become opaque, and depressed in the same manner as the former, to the bottom of the eye, still bearing the marks of the needle evidently on it, but could find no pellicle within the eye. S. Benevoli further relates, that having formerly made some experiments on the eyes of dead subjects, at Bologna, in company with Dr. Valsalva, he had introduced the needle into the eye in the same place, and in the same manner, as is commonly practised in the operation of couching, and having afterwards dissected the same eyes, he had always found that the needle had passed into the eye on the backside of the crystalline humour, so that it had been impossible to bring the needle forward from thence into that part of the aqueous humour, which is seated between the uvea and the crystalline humour, in order to depress a pellicle seated there, according to the common opinion, unless he would have passed his needle through the body of the crystalline.

This curious author likewise observes, that the aforesaid space, between the uvea and the crystalline humour, is so very narrow, that though he finds it not impossible to introduce a needle into that space, yet there is by no means room enough to turn the needle up and down in all directions, with the freedom used in couching cataracts, without wounding either the uvea, or the crystalline.

Lastly, S. Benevoli observes, that in his practice of couching cataracts for many years, having generally couched about 12 or 14 in a year, he had always found, that he worked on a hard and resisting substance, which being tenderly touched by the needle, would vibrate and fluctuate backwards and forwards, and would sometimes return against the needle with a sensible impetus, which by no means agrees with the common notion of the cataract's consisting in a pellicle or membranous substance.

An Observation of a Solar Eclipse at Greenwich, Nov. 27, 1722, p. m. By Dr. Halley, LL.D. R.S.S. Astron. Regal, and Savilian Prof. Astron. at Oxford. N° 374, p. 197. *Translated from the Latin.*

At $1^h\ 28^m\ 58^s$ the eclipse began.
3 43 25 the end, but rather doubtful.
3 43 45 the eclipse certainly ended.

The same Eclipse observed in Fleet Street, London. By Mr. George Graham, F. R. S. N° 374, p. 198.

P. M. $1^h\ 28^m\ 38^s$ beginning. Apparent time.
2 29 34 by estimation the cuspes parallel to the horizon.
3 43 22 the end.
2 14 44 the duration.
Quantity eclipsed 5 $\frac{716}{1000}$ digits.

I had very correct observations both of the sun and stars, the 26, 27, and 28th, for determining the exact time by my clock.

For some minutes before the eclipse began, I observed the sun with a telescope of 12 feet, furnished with a micrometer; keeping that part of the limb in the middle of the glass, where I expected the moon first to touch, and in less than 4 seconds of time, from the moment I judged the eclipse began, it was so considerably advanced, that I cannot doubt of having the beginning to less than 3 seconds. I believe the exact time of ending was within the same limit, though the undulation of the limb was then much greater than at the beginning. The parts eclipsed, measured with the micrometer, at the time of the greatest obscuration, were 927 such parts as the sun's vertical diameter contained 1946; which was taken a little before the beginning of the eclipse.

By this observation the beginning differed not $2\frac{1}{2}^m$, and the end not $\frac{1}{2}$ a minute from Dr. Halley's computation, which he sent me the day before. And if his computation, which was made for Greenwich, had been reduced to the meridian of London, the difference would have been still less.

The same eclipse was observed by Mr. Hawkins at Wakefield, in Yorkshire, to begin at $1^h\ 21^m$ p. m. and to end at $3^h\ 30^m\ 3^s$. The sun's diameter was obscured somewhat more than 5 digits.

On the Particles and Structure of Diamonds. By M. Leuwenhoeck. N° 374, p. 199. *Translated from the Latin.*

After M. Leuwenhoeck had discovered, that some metals, and even grains of sand, consisted of very small particles of the same matter, he turned his thoughts to consider a diamond, as whether it also consisted of the same sort of particles, that might be observed with the microscope.

Therefore viewing, with a microscope, a small diamond, he could observe with the naked eye a great many particles in the unpolished and dark part of it; and he found, that it consisted of small particles: but not being satisfied with these observations, he resolved to break a diamond into pieces, to consider these the better.

Laying, therefore, a diamond upon a hammer, he struck it twice with another hammer, by which it was broken into 4 or 5 pieces: with which not being yet satisfied, he had a mind to break the diamond into still smaller bits, and accordingly he folded up a piece, larger than the rest, in a double piece of paper, that he might not lose any of it.

Here M. L. was surprised at the hardness of a diamond; which, though often struck with considerable force, broke only into 4 or 5 pieces, without any small bits.

On placing the last mentioned pieces before a microscope, he viewed them, and found that they all consisted of very small particles: and on exposing them to the rays of the sun, he observed a kind of little flame break out from them, and larger than any he had ever observed.

He observed with his naked eye one small piece that had its flat and square crack directly exposed to the sun; and as far as he could judge with his naked eye, 3 or 4 hairs of a man's beard in breadth. Such a quantity of sparkling flames issued from this piece of diamond, that he judged them upwards of 400: a few of those flames lay closer to each other, and were larger than the rest: whence he concluded, that the particles of the diamond were larger in that place, and more regularly disposed than the other particles.

Afterwards he viewed another piece of diamond, about the same size as the former, which was also directly exposed to the rays of the sun; and he found it consisted of the same number of very small particles: from the half of that little piece there likewise arose the same sparkling flames, but smaller; and in the other half a kind of waving flame was observed, with a continual coruscation, like a faint lightning.

Also, after withdrawing these little pieces out of the sun's rays, various appearances still presented themselves to his eyes; among other things, a little flame seemed to dart aloft from each particle of the diamond.

Further, he had 9 small pieces of diamond lying before his microscope; and in 7 of them he observed these particles, which shot forth the sparkling flames as before mentioned; and in two others he could likewise observe those particles, of which he supposes a diamond is composed; but they had their planes turned towards the sun in such a manner, that he could distinctly observe several particles at the same time.

It was an agreeable sight to behold so many appearances of sparkling flames, most of them of a bright flame-colour, and some greenish: M. L. was surprised to observe at the extremity of some of the flames such a motion and vibration in the air, as if they were become so weak in that place, as not to be distinguished. He was most of all surprised, that fire shot forth every way out of such a particle of diamond, flashing faintly, and like lightning at a distance; and this he observed several times. M. L. not only himself viewed with his microscope the said piece of diamond, but he showed it to another person, who affirmed, that the appearances exactly agreed with M. L.'s description, and that he was surprised at the unusual sight.

In another piece of diamond, the lamellæ could be distinguished, of which it is composed; and about $\frac{1}{3}$ part of it consisted of so regular a pentagon, as if it were artificially cut, only that a very small diamond was fastened to it, that covered about the fourth part of the pentagon; and he could plainly see that it also consisted of lamellæ, or particles resembling lamellæ.

Some of the pieces of the diamond, viewed with a microscope, exhibited a very agreeable sight; and M. L. showed them to others, who were highly pleased to observe such a variety of parts in one single piece of diamond; especially that the lamellæ, of which diamonds are composed, could be very distinctly observed in two small pieces; viz. when these lamellæ lay lengthwise before the eye.

Afterwards M. L. turned his thoughts to examine a hexagonal piece of mountain crystal, whose length was about 2 fingers breadth, and its thickness that of the little finger. He broke this crystal into several pieces, which he placed before his microscopes, to examine whether they were composed of lamellæ laid on each other, in the same manner as the said diamonds acquire their bulk: but after repeated trials, he could not find even the least lamellæ in it. But he generally found, in the 6 sides of all the pieces of crystal he had, small transverse lines, some a little higher than the others.

On the different Refrangibility of coloured Light. By the Rev. J. T. Desaguliers, LL. D. F. R. S. N° 374, p. 206.

Sir Isaac Newton, in his Optics, B. 1, Prop. 1, Exp. 2, relates an experiment made with a card, or paper, painted red on one half and blue on the other; which being enlightened by a candle, the image, by the interposition of a lens, is so projected on a white paper, held on the other side of the lens, that the place where the blue half appears distinct, or as the opticians term it, the distinct base of the image of the blue half, is much nearer to the lens than the place of the image of the red half. And this is made apparent, by seeing on one of these images the representation of the black threads, wrapped round the card, while they are not visible on the other. This is fully described in the place abovementioned; but yet a gentleman* abroad has called the experiment in question, and denied the matter of fact, saying, that he could not make it succeed, but proposes an experiment of his own, to disprove the different refrangibility of the rays.

On this I was desired to make the experiment over again, before the Royal Society, which succeeded well. But because that care must be taken in making it, I shall mention all the particulars observed in the performance; which, if duly put in practice, will make the experiment always succeed.

I painted one half of the card RB, fig. 17, pl. 14, as B, with ultramarine, made deeper with a small mixture of indigo, and the other half R, I painted over with cinnabar heightened with a little carmine; so that the line that separated the red from the blue, was perpendicular to the long sides of the cord. I then wrapped a black silk four times together, over the middle of each painted part of the card, as in fig. 18. On a square trencher, fig. 19, painted black, and suspended vertically against a wall, I fixed my coloured card with a pin; and the room being made very dark, I enlightened the card with a strong light thrown on it from a dark lantern, having two convex glasses in it; then setting up the lens LL, represented by fig. 20, in such a manner, that its axis passed perpendicularly through the image of the card, and at the distance of 9 feet from it, the image of the card being received on a white paper, at the distance of 9 feet on the other side of the lens, at B, the blue half appeared distinct, with the image of the black silk going vertically along its plain, while no appearance of the black silk was perceivable on the red half. Then removing the paper about 2 inches, to R, the red half of the image had a black-line very plain upon it, while it was invisible on the blue

* Act. Erudit. Lips. Supplem. tom. 8, §. 3, p. 130, 131.

half. This was more evident, when a strong image of the candle was successively thrown on that half of the card, whose image was under examination. When the paper was held in the middle between R and B, the black line on each colour was visible, but indistinct.

N. B. Care must be taken that the colours be deep, because having accidentally rubbed off some of the blue, the whiteness of the card under it, made its image fly out farther, almost as far as that of the red.

Concerning the Inoculation of the Small Pox, and the Mortality of that Distemper in the natural Way. By Dr. Nettleton, Physician at Halifax.* N° 374, p. 209.

Two propositions are advanced by the favourers of inoculation, concerning which the public seems to require more full satisfaction: viz. "That the distemper raised by inoculation is really the small pox; and that it is much more mild and favourable, and far less mortal, than the natural sort."

The former of these is not so much disputed now, as it was at first, when this method was introduced, nor can it be made a doubt of by any one, who has seen those that have been inoculated, and has also been much conversant in the natural small pox. There is usually no manner of difference to be observed between the one sort and the other, when the number of pustules is nearly the same; but in both there are almost infinite degrees of the distemper, according to the difference of that number. All the variation that can be perceived of the ingrafted small pox from the natural, is, that in the former the pustules are commonly fewer in number, and all the rest of the symptoms are in the same proportion more favourable. They exactly resemble what we call the distinct sort: the symptoms before the eruption are the very same, and when the pustules begin to rise, their appearance is the same, as well as their periods of maturation and declension; they are at first of the same florid, rosy colour, and when fully ripe, of as fair a yellow. They commonly rise as round and as large as the other, and when they are very numerous, the inflammation and swelling of the face comes on at the usual time, and is followed by the swelling of the hands and feet, and only once I observed a salivation, though the pustules were distinct. In the natural small pox, when the pustules are

* Although the vaccine inoculation, a discovery which constitutes a new æra in medicine, and which it cannot be doubted will transmit the name of Jenner with undiminished lustre to future ages, has now superseded the inoculation of the small pox; yet this and the following papers are reprinted entire, as they exhibit a valuable set of facts and arguments relative to a subject which will ever be interesting in an historical point of view, and may even be made to furnish data for comparative illustrations of the superior advantages of the vaccine over the variolous inoculation.

very few, we sometimes observe that they do not rise to so great a bulk, neither do they ripen so fully, nor continue so long as usual; and it is the same in the way of inoculation. In short, as this distemper is raised by an ingraftment from the small pox, as it has the very same appearance, and as it is capable of producing the same by infection, there seems to be no room to doubt of its being the true and genuine small pox. And if that be allowed, it will follow from thence, as a corollary, that "Those, who have been inoculated, are in no more danger of receiving the distemper again, than those who have had it in the ordinary way." And this is also so far confirmed by experience. We are very ready to own, that the operation may sometimes fail: those gentlemen, who first communicated to the R. S. some account of this practice from Turkey, did both of them intimate so much; though I believe that will but rarely happen. In one instance here, I observed no eruption at all, neither did the wounds inflame and swell any more than would have followed from a common incision, which made me conclude, that what was applied had not taken effect, and indeed the reason of it was very well known to me. In 3 others, though the wounds did inflame, and swell, and discharge considerably, yet the eruptions were so imperfect, as to leave me a little in doubt: but 2 of these have since been sufficiently tried, by being constantly with those who had the small pox, without receiving any infection; which makes me inclined to believe they will always be secure from any danger of it. As to all the rest, no one, who saw them, did in the least question, but that they had the true small pox.

As to the latter proposition, "That the ingrafted small pox is far less dangerous than the natural:" the truth of this, I suppose, can only be found by making a comparison, so far as our experience will extend. In order to this, I have annexed an account taken in this and several other towns, where the small pox has been epidemical this last year, how many have had the small pox, and how many out of that number have died.

	Have had the Small Pox.	Died.
In Halifax	276	43
In a part of the parish of Halifax, stretching towards Bradford	297	59
In another part of the same parish	268	28
In Bradford	129	36
In Leeds	792	189
In Wakefield	418	57
In Rochdale	177	38
In Ashton under Line, a small market-town in Lancashire, including 2 neighbouring villages	279	56
In Macclesfield	302	37
In Stockport	287	73
In Hatherfield	180	20
Total	3405	636

I am very sensible you will require a great number of observations, before you can draw any certain conclusions. I would only remark, that it appears from these accounts, that this last year, in this part of the kingdom, almost 19 out of every 100, or near $\frac{1}{5}$ of those who have had the natural small-pox, have died; whereas out of 61 which have been inoculated hereabouts, not one has died; for as to the case of Mr. John Symson's daughter, which would have made the 62d, I leave it out of my account, and I will refer it to any impartial judgment whether I may not justly do so. (Phil. Trans. N° 370.) The facts are open to every one's inquiry, and whoever will give himself the trouble, may be satisfied as to the truth of them.

Halifax, Dec. 16, 1722.

A Comparison between the Danger of the Natural Small Pox, and of that given by Inoculation. By James Jurin, M. D. R. S. Secret. N° 374, p. 213.

We have seen for some considerable time past, above 100 persons per week in this city and suburbs, taking one week with another, carried off by this disease; a consideration certainly that ought to dispose us to enter into any measures, by which we may reasonably hope to put some stop to the progress of so fatal a distemper. To this purpose, the method of inoculation, which has lately been introduced among us, is strongly recommended on the one hand, and has been opposed with a great deal of warmth and zeal on the other.

I have no inclination to enter into this controversy; it is in better and abler hands: but, as the point in dispute is of the utmost importance to mankind, I heartily wish that, without passion, prejudice, or private views, it may be fairly and maturely examined. In order to which, if the following extracts and computations, concerning the comparative danger of the inoculated and natural small-pox, may be of any use, I shall think my labour well bestowed.

The number of persons, who have had the small-pox by inoculation here in England, is, by the best information I have been able to collect, as follows:

Inoculated by	Dr. Nettleton	61
	Claudius Amyand, Esq. serjeant surgeon	17
	Mr. Maitland, surgeon	57
	Dr. Dover	4
	Mr. Weymish, surgeon	3
	The Rev. Mr Johnson	3
	Dr. Brady, at Portsmouth	4
	Mr. Smith, surgeon, and Mr. Dymer, apothecary, at Chichester	13
	Mr. Waller, apothecary at Gosport	3
	A woman at Leicester	8
	Dr. Williams at Haverford-West	6
	Two other persons near the same place	2
	Dr. French, at Bristol	1
	In all	182

Out of this number, the opposers of inoculation affirm, that 2 persons died of the inoculated small-pox; the favourers of this practice maintain, that their death was occasioned by other causes. If, to avoid dispute, these 2 be allowed to have died of inoculation, we must estimate the hazard of dying of the inoculated small-pox, as far as can be collected from our own experience, to be that of 2 out of 182, or one out of 91.

The Rev. Mr. Mather, in a letter dated March 10, 1721, from Boston in New England, gives an account, that of near 300 inoculated there, 5 or 6 died on it or after it, but by other diseases and accidents; chiefly from having taken the infection in the common way by inspiration, before it could be given them in this way of transplantation.

If we allow 5 out of these 300 to have died of the small-pox by inoculation, notwithstanding what Mr. Mather has said of their dying by other accidents or diseases: the hazard of inoculation will thence be determined to be that of 1 in about 60. But here it must be observed, that by all the accounts from New England, the operators there appear not to have been so cautious in the choice of their subjects, as here in England. For Mr. Mather tells us, that the persons inoculated were young and old, from 1 year to 70, weak and strong; and by other relations we are informed, that women with child, and others even in childbed, underwent the operation. Apparently the greatness of the danger they were in, from the infection in the natural way, which then raged among them with the utmost fury, made them the more adventurous.

Now to form an estimate of the hazard, which all mankind, one with another, are under of dying of the natural small-pox, that, by comparing this with the hazard of inoculation, the public may be enabled to form a judgment, whether the practice of inoculation tends to the preservation of mankind, by lessening the danger to which they are otherwise liable. With this view I have consulted the yearly bills of mortality, as far back as the year 1667, being the year after the plague and the fire of London, comprehending to the present time the space of 56 years, from 42 of which I have given extracts in the 2 following tables.

The first of these takes in the first 20 years, distinguishing for every year the total number of burials, and likewise the number that died of the small-pox, in 2 separate columns. The 3d column shows how many died of the small-pox out of every 1000 that were buried; and the 4th column represents the proportion between those that died of the small-pox, and the whole number of burials, by the nearest vulgar fraction, having always 1 for the numerator.

The 2d table gives the last 22 years, after the same manner, and at the bottom of each table is given the total number for each series of years, and likewise the number that died each year, taken at a medium, one year with another: by which it appears, that the proportion between the number of those that die of the small-pox, and the whole number of burials, is very nearly the same, on an average for each series of years.

The 14 intermediate years, between 1686 and 1701, are omitted, because in the bills for those years, the accounts of the small-pox and measles are not distinguished, as in the preceding and following years, but are joined together in one article, so that from them no certain account can be drawn of the number of persons that died of the small-pox.

TABLE I.

Years.	Total No. of Burials	Died of the Small Pox. In all.	In 1000.	In Prop.
1667	15842	1196	75	$\frac{1}{33}$
1668	17278	1987	115	$\frac{1}{9}$
1669	19432	951	49	$\frac{1}{20}$
1670	20198	1465	73	$\frac{1}{14}$
1671	15729	696	44	$\frac{1}{23}$
1672	18230	1116	61	$\frac{1}{16}$
1673	17504	853	49	$\frac{1}{21}$
1674	21201	2507	118	$\frac{1}{8}$
1675	17244	997	58	$\frac{1}{17}$
1676	18732	359	19	$\frac{1}{52}$
1677	19067	1678	88	$\frac{1}{11}$
1678	20678	1798	87	$\frac{1}{12}$
1679	21730	1967	91	$\frac{1}{11}$
1680	21053	689	33	$\frac{1}{31}$
1681	23971	2982	125	$\frac{1}{8}$
1682	20691	1408	68	$\frac{1}{15}$
1683	20587	2096	102	$\frac{1}{10}$
1684	23202	156	7	$\frac{1}{149}$
1685	23222	2496	107	$\frac{1}{9}$
1686	22609	1062	47	$\frac{1}{21}$
20 Years.	398200	28459	$71\frac{1}{2}$	$\frac{1}{14}$
Each year at a medium.	19910	1423	$71\frac{1}{2}$	$\frac{1}{14}$

TABLE II.

Years.	Total No of Burials	Died of the Small Pox. In all.	In 1000.	In Prop.
1701	20471	1095	53	$\frac{1}{19}$
1702	19481	311	16	$\frac{1}{63}$
1703	20720	898	43	$\frac{1}{23}$
1704	22684	1501	66	$\frac{1}{15}$
1705	22097	1095	50	$\frac{1}{20}$
1706	19847	721	36	$\frac{1}{28}$
1707	21600	1078	50	$\frac{1}{20}$
1708	21291	1687	79	$\frac{1}{13}$
1709	21800	1024	47	$\frac{1}{21}$
1710	24620	3138	127	$\frac{1}{8}$
1711	19833	915	46	$\frac{1}{22}$
1712	21198	1943	92	$\frac{1}{11}$
1713	21057	1614	77	$\frac{1}{13}$
1714	26569	2810	106	$\frac{1}{9}$
1715	22232	1057	48	$\frac{1}{21}$
1716	24436	2427	99	$\frac{1}{10}$
1717	23446	2211	94	$\frac{1}{11}$
1718	26523	1884	71	$\frac{1}{14}$
1719	28347	3229	114	$\frac{1}{9}$
1720	25454	1440	57	$\frac{1}{18}$
1721	26142	2375	91	$\frac{1}{11}$
1722	25750	2167	84	$\frac{1}{12}$
22 Years.	505598	36620	72	$\frac{1}{14}$
Each year at a medium.	22982	1665	72	$\frac{1}{14}$
42 Years.	903798	65079	72	$\frac{1}{14}$
Each year in 42 at a medium.	21519	1550	72	$\frac{1}{14}$

By these tables it appears, that upwards of 7 per cent. or somewhat more than a 14th part of mankind, die of the small-pox; and consequently the hazard of dying of that distemper, to every individual born into the world, is at least that of 1 in 14. And that this hazard increases after the birth, as the child advances in age, will appear from what follows.

From this estimate it is demonstrable, that in the case of persons actually having the small-pox, the hazard that they run, one with another, of dying of that distemper, is greater than that of 1 in 14; or, which is the same thing, there must be fewer than 13, that recover, for 1 that dies of the small-pox. For since $\frac{1}{14}$ part of mankind die of the small-pox, and the other 13 parts die of other diseases; if these 13 have all had the small-pox, and recovered from it before they fell ill of those other diseases of which they died, then just 13 will have recovered from the small-pox, for one that dies of that distemper: but, as it is notorious, that great numbers, especially of young children, die of other diseases, without ever having the small-pox, it is plain, that fewer than 13 must recover from this distemper, for 1 that dies of it.

To determine exactly how many of these 13 parts of mankind, die without having the small-pox, is a very difficult task: but it is easy to see, that a considerable deduction is to be made from them. In the first place, the 2 articles of stillborn and abortive children, which are put into the yearly bills, as part of the number of burials, are unquestionably to be deducted.

With these 2, may be joined the following heads, which, by the best information I can procure, comprehend only very young children, or at most not above 1 or 2 years of age. Overlaid, chrysoms and infants, convulsions, horseshoehead, headmoldshot, teeth, water in the head, worms, rickets, livergrown, chin-cough, and hooping-cough, which articles in the yearly bills for 22 years last past, amount at a medium to 386 in each 1000, of the whole number of burials.

It is true indeed, that probably some small part of these must have gone through the small-pox, and therefore ought not to be deducted out of the account: but then on the other hand, as it is certain that of the remaining $\frac{614}{1000}$ of mankind, that are above 1 or 2 years of age, there are great numbers that never have the small-pox, it will I presume be judged to be no unequal supposition, if I suppose all that are contained under the heads abovementioned, to have missed that distemper, when by way of compensation, I allow all the remainder of mankind to undergo it; which concession is so large, that it will abundantly make up for what I assume too much in the former supposition.

Allowing therefore, that out of every 1000 children that are born, 386 die under 1 or 2 years of age, without having the small-pox, and 72 do some time or other die of that distemper; it follows, that the hazard of dying of it, to the remainder of mankind, above 1 or 2 years of age, who are all supposed to undergo that disease sooner or later, is that of 72 out of 614, or nearly 2 out of 17: so that no more than between 7 and 8 can recover from that distemper, for 1 that dies of it. And if any considerable part of the aforesaid remainder of mankind, more than is allowed for above, escape having the small-pox, then the proportion of those that recover from it, will be still smaller.

This consideration shows the fallacy of one plausible argument, that has been often made use of on occasion of the present disputes about inoculation: which is, that whatever be the danger of dying of the small-pox, to those that actually have that disease, yet as great numbers of persons never have the small-pox at all, this danger is what any particular person may never be in; and therefore it will be madness to undergo the hazard of inoculation, be it great or small, in order to prevent a disease which possibly may never befall one.

For if 2 parts in 17 of all mankind, that are above 1 or 2 years of age, must sooner or later die of the small-pox, it is plain, that how many parts soever of these 17 are supposed to escape that distemper, the mortality among the remainder, who undergo it, must in proportion be so much the greater. As for instance, if 7 parts escape having the small-pox, and 10 undergo it, then 2 out of 10, or 1 out of 5, that have the small-pox, must die of that disease.

And as it can never be known, whether any particular person be one of those that are to have the small-pox, his hazard of dying of that distemper, being made up of the hazard of having it, and the hazard of dying of it, if he has it, will be exactly the same, namely, that of 2 in 17, or 1 in 8 or 9, whether the proportion of mankind that escape having the small-pox, be great or small.

But as what has been said concerning the hazard of the natural small-pox, is taken from an account of 42 years; whereas the hazard of inoculation is estimated only from what has happened in the space of about 18 months, since which time it had its first rise among us; it will perhaps be asked by some persons, why we do not likewise make the estimate of the hazard of the natural small pox, from the last 2 years alone, without running back into so great a number of years, before inoculation was begun?

To which we answer, that the proportion of those that die of the small-pox, varies so much in different years, as appears from the tables above, that it was impossible to come at any certainty in this point, from the consideration of the last 2 years alone: and if any one suspects us of partiality in proceeding after the manner we have done, he need only cast his eyes on the 2d table, where he will find, that the mortality of the natural small-pox, for the last 2 years, has considerably exceeded the medium we have determined, by taking in 42 years.

There is another method, which, if put in practice in several large towns, or parishes, and for a sufficient number of years, would enable us to come at a nearer and still more certain computation of the proportion between those that recover, and those that die of the small-pox: which is, to send a careful person once a year, from house to house, to inquire what persons have had the small-pox, and how many have died of it, in the preceding year. This has been done by Dr. Nettleton the last year, at several towns in Yorkshire, &c. and the same was done at Chichester for the same year, to the 15th of October last, by a person of credit, whose account was communicated to me by my learned and ingenious friend, Dr. Whitaker. Such another account has been transmitted to me from Haverfordwest, in South Wales, by the learned Dr. Perrot Williams, physician in that place. The sum of these accounts is as follows:

	Sick of the Small-pox.		Died.
Several towns in Yorkshire................	3405	..	636
Chichester............................	994	..	168
Haverfordwest..........................	227	..	52
Total	4626	..	856

From which it appears, that, on a medium between these accounts, there died of the small-pox almost 19 per cent. or nearly 1 in 5, of persons of all ages that underwent that distemper. Which is the more to be remarked, as out of 82 persons, who had the small-pox by inoculation the same year, and in the neighbourhood of the same places, not 1 miscarried.

Mr. Mather observes, in his letter mentioned above, that out of more than 5000 persons, that had the small-pox at Boston in New England, within little more than half a year, near 900 died, which is more than 1 in 6; and this account added to those from Yorkshire, Chichester, and Wales, reduces the proportion of those that die of the small-pox, to somewhat more than 18 per cent. so that the hazard of dying of that distemper, to those who are taken ill of it, is that of 1 in between 5 and 6, or something above 2 in 11.

The result therefore of these computations is, that if the same proportions should still continue, as have hitherto been determined by observation, we must expect, that of all the children that are born, there will some time or other, die of the small-pox, 1 in 14. That, of persons of all ages taken ill of the natural small-pox, there will die of that distemper, 1 in 5 or 6, or 2 in 11.— That of persons of all ages inoculated, without regard to the healthiness or unhealthiness of the subject, as practised in New England, there will die 1 in 60 —That of persons inoculated with the same caution in the choice of the subjects, as has been used by the several operators one with another, here in England, (if we allow in the 2 disputed cases abovementioned, that the persons died of the inoculated small pox) there will die 1 in 91.

But if those 2 persons be allowed to have died of other accidents or diseases, then we shall have reason to think, as far as any judgment can be made from our own experience here in England, that none at all will die of inoculation, provided that proper caution be used; as we are informed is the case in Turkey: where out of many thousands, that, in the space of about 40 years past, have been inoculated in and about Constantinople, by one Greek woman, who still continues that practice, notwithstanding her extreme old age, not so much as 1 person has miscarried, as I am assured by the ingenious Dr. le Duc, a native of Constantinople, who was himself inoculated there under the care of his father, an eminent physician in that city.

P. S. Since this paper was drawn up and communicated to the R. S. the following account of the success of inoculation in and about Boston, in New England, was procured at my desire, by Dr. Nesbitt, from Capt. John Osborne, who resided in that town and neighbourhood during the whole time of that practice. I think proper to insert it here, as it confirms the extract given above from Mr. Mather's relation, and is a more particular account of the matter of fact, than any that I have yet seen.

In May, 1721, the small-pox was brought into the town of Boston; in June it began to spread pretty much; and in the month of July it was got into most parts of the town, and a considerable number of people died of it. At this time inoculation was first put in practice by Dr. Boyleston, who then performed it on his own child and a negro-servant, who both did well; notwithstanding which, this attempt gave great uneasiness to the neighbours. However the practice went on, to the number of about 40 persons, one of which was a woman of about 40 or 45 years of age, who got well over the small-pox, but had been before troubled with hysteric fits, of which she died some little time after. When about 70 persons had passed under the operation, myself and wife, who had hitherto been at a place called Roxbury, a mile from Boston, went into town and received the small-pox by inoculation. We had it with all the gentleness and moderation that was possible, neither of us having 100 pustules, or being sensible of any fever worth mentioning: so that we did not find it necessary to keep our beds for it.

In August the small-pox in the natural way proved more mortal, and inoculation made a greater progress, the people continuing to come into the practice of it. A 2d person that died after inoculation, was an apothecary's housekeeper, that was out of town, till an Indian maid got the distemper in the same house, and removed, and died. On which this woman coming to town, her master undertook to perform the operation upon her, which by the way was the first and last that he ever performed; and on the 3d day after the inoculation, the small-pox came out upon her very full; from which it was plain, that she had taken the infection before, in the common way.

The 3d person that died after being inoculated, was a gentleman who lodged in the same house with my wife and self at Roxbury, who was under great and extreme infirmity of body, that we feared he would have lived but a short time under it. His friends much persuaded him to make use of inoculation, believing that it would have carried off his illness; but when he made the experiment, he had not strength to go through with it. He was about 45 years of age.

His sister was the 4th person that died on this operation. She was about 40 years of age, of great indisposition of body, and weak, as was her brother.

The 5th, that died on inoculation, was a woman servant in a house, where the whole family, to the number of 8, were inoculated at the same time. She lay in a cold upper room during her illness, and was much neglected, the whole family being down together, so that she died merely for want of a little attendance. This was in the town of Roxbury, where 13 men, masters of families, got the small-pox, and all died; which inclined the people to make use of inoculation, having before been much against it, and 43 men were inoculated there, who all did well. The minister of the town was the first, who put it in practice there, much against the mind of his people at first, though afterwards they were very well pleased with it, seeing with what great success it was attended; and then whole families came into it, and underwent the operation. There were in all at least 280 persons inoculated, that I knew of, and I suppose there might be about 20 or 30 more, but of those I can give no certain account.

Of the Section of an Angle. By Mr. Abraham Demoivre. N° 374, p. 228. *Translated from the Latin.*

The beginning of the year 1707, I fell upon a method by which a given equation of these forms,

$$\text{viz. } ny + \frac{nn-1}{2.3}\text{A}y^3 + \frac{nn-9}{4.5}\text{B}y^5 + \frac{nn-25}{6.7}\text{C}y^7 \text{ \&c.} = a,$$

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

(where A, B, C, &c. represent the coefficients of the preceding terms) may have its root determined in the following manner: viz.

Putting $a + \sqrt{aa + 1} = v$ in the 1st case, and $a + \sqrt{aa - 1} = v$ in the 2d. Then will $y = \frac{1}{2}\sqrt[n]{v} - \frac{1}{2}\sqrt[n]{\frac{1}{v}}$ in the 1st case; and $y = \frac{1}{2}\sqrt[n]{v} + \frac{1}{2}\sqrt[n]{\frac{1}{v}}$ in the 2d. Which solutions were inserted in N° 309, of that year.

Now by what artifice these formulas were discovered, will clearly appear from the demonstration of the following theorem.

Let x denote the versed sine of any arc, and t that of another, the radius being 1; and let the former arc be to the latter, as 1 to n: then, assuming the two equations, which may be called know, viz.

$$1 - 2z^n + z^{2n} = -2z^n t, \text{ and } 1 - 2z + z^2 = -2zx;$$

by expunging z, there will arise the equation by which the relation between x and t will be determined.

Corol. 1.—If the latter arc be a semicircle, the equations will be

$$1 + z^n = 0, \text{ and } 1 - 2z + z^2 = -2zx:$$

from which if z be expunged, there will arise an equation, by which will be determined the versed sines of the arcs, which are to the semicircle, taken once, or thrice, or 5 times, &c. as 1 to n.

Corol. 2.—If the latter arc be the whole circle, the equations will be

$$1 - z^n = 0, \text{ and } 1 - 2z + z^2 = -2zx:$$

from whence expunging z, there will arise an equation, by which are determined the versed sines of the arcs, which are to the circumference, taken once, twice, thrice, 4 times, &c. as 1 to n.

Corol. 3.—If the latter arc be of 60°, the equations will be

$$1 - z^n + z^{2n} = 0, \text{ and } 1 - 2z + z^2 = -zx:$$

from which, by expunging z, there will arise an equation, by which are determined the versed sines of the arcs, which are to the arc of 60°, multiplied by 1, 7, 13, 19, 25, &c. or by 5, 11, 17, 23, 29, &c. as 1 to n.

If the latter arc be 120°, the equations will be

$$1 - z^n + z^{2n} = 0, \text{ and } 1 - 2z + z^2 = -2z:$$

from which if z be expunged, there will arise the equation, by which are determined the versed sines of the arcs, which are to the arc of 120°, multiplied by 1, 4, 7, 10, 13, &c. or by 2, 5, 8, 11, 14, &c. as 1 to n.

An Account of a new sort of Molosses, made of Apples; and of the degenerating of Smelts. By the Hon. Paul Dudley, F. R. S. N° 374, p. 231.

The apple, that produces the molosses, is a summer-sweeting, of a middling size, pleasant to the taste, and full of juice, so that 7 bushels will make a barrel of cyder. The manner of making it is thus; you grind and press the apples, and then boil the juice in a copper till $\frac{3}{4}$ of it is wasted, which will be done in about 6 hours gentle boiling; and then it comes to be of the sweetness and consistency of molosses.

This new molosses answers all the purposes of that made of the sweet cane imported from beyond sea. It serves not only for food and brewing, but is of great use also in the preserving of cyder; 2 quarts of it put into a barrel of racked cyder, will both preserve and give it a very agreeable colour.

Our country farmers run much upon planting orchards of these sweetings, for fattening their swine, and assure me it makes the best sort of pork. And I know the cyder made of them to be better than that of other fruit for taste, colour, and keeping.

Two short miles from my house we have a fine pond, of half a mile over, having little or no communication with the sea. An ingenious man, about 60 years since, for an experiment, took a pail of large smelts from the river, and put them into this pond, where they have increased abundantly, but are degenerated to a very small sort; for our river smelts I suppose are full as large as those of the Thames, some of them I know will weigh $2\frac{1}{2}$ oz. whereas these small ones will not weigh 5 pennyw. We reckon the pond smelt eats much better than the other; they are also very transparent, and of a beautiful shining pearl colour.

Roxbury, New England, Oct. 25, 1722.

Observations on the Eclipse of the Moon, June 18, 1722; *and the Longitude of Port Royal in Jamaica determined by it. By Dr. Halley, F. R. S.* N° 375, p. 235.

The eclipse of the moon, which happened in June, 1722, was so far hid by the cloudy sky, that neither myself, nor any of our astronomical friends, in or about London, could furnish an observation fit to be laid before the Society. But having been well observed at Jamaica, by Capt. Candler, commander of his Majesty's ship Launceston, and at Berlin, by Mr. Christfried Kirck, astronomer of the Royal Academy of Sciences there, I thought it not amiss to prefix to their accounts what little I was able to note concerning it.

June 18, in the morning, having perfectly rectified my clock so as to show the apparent time, neither the transit of the moon over the meridian, nor the beginning of the eclipse which soon followed, could be seen through the very thick cloud. At $13^h\ 12^m$ apparent time, a small particle of the moon's body was seen through a very small hiatus in the cloud, by which glimpse I could only be assured that the eclipse was not yet total. At $13^h\ 29^m$, by such another view, I was satisfied that it was now become total; but in a moment, it again disappeared, till $14^h\ 49^m\ 10^s$, when the cloud beginning to break, I got time to measure with the micrometer, the lucid parts, now recovered in the moon's diameter, which I found 14′, though this not so well as I could wish, by reason of a thinner sort of cloud which perpetually intercurred, and rendered the edge of the shadow somewhat dubious.

At $15^h\ 15^m$ the moon was pretty well got out of the thick cloud, but being very low, and the day-light become strong, she shone very faintly, and the shadow became worse and worse defined.

From $15^h\ 26^m$ to $15^h\ 27^m$ I doubted of the end, and am confident it did not exceed the 27th minute. It ended opposite the north part of the palus mæotis of Hevelius, much about the middle of the western or right-hand limb of the moon, she being then very near setting.

Capt. Barth. Candler, being then at Port Royal, in Jamaica, had much better fortune, and a serene sky from the beginning to the end, who having used due care to be assured of his times by altitudes taken with an instrument of 3 feet radius, was pleased to send us the result of his observation as follows:

The eclipse began	6^h	59^m	10^s
Immersion	8	7	50
Emersion	9	11	0
The end	10	19	40
Whence the middle	8	39	25

And supposing the eclipse to have ended at Greenwich, at $15^h\ 26\frac{1}{2}^m$, the difference of longitude between Port Royal and Greenwich, will be $5^h\ 6^m\ 50^s$, or $5^h\ 6\frac{1}{2}^m$ from London, that is $76^\circ\ 37\frac{1}{2}'$.

Mr. Kirck, being in a more easterly meridian, could see nothing of the emersion, but as carefully noted the time of the beginning and immersion, as he observed them at Berlin, viz. the beginning of the eclipse at $12^h\ 59^m\ 55^s$, and the immersion at $14^h\ 8^m\ 8^s$. Now by comparing several observations made at both places, we have formerly concluded Berlin to be 54^m of time, or $13\frac{1}{2}^\circ$ of longitude, more easterly than London; therefore at London it began at $12^h\ 5^m\ 55^s$, and immerged at $13^h\ 14^m\ 8^s$, that is, the beginning was later here than at Jamaica $5^h\ 6^m\ 45^s$, and the immersion later $5^h\ 6^m\ 18^s$, punctually agreeing with what resulted from my own observation of the end, as abovesaid; and sufficiently with what I had long since determined from observations sent me from Jamaica by my old astronomical friend Mr. Charles Boucher.

The Longitude of Carthagena in America. By the same. N° 375, p. 237.

Having lately received a packet of observations from Carthagena in America, made by Colonel Don Juan de Herrera, chief engineer of that city, I find among them one immersion of the first satellite of Jupiter into his shadow, observed there by a telescope of 17½ feet, on April 9, O. S. 1722, at $15^h\ 58^m\ 44^s$ apparent time; and two emersions of the same, viz. July 5, $11^h\ 23^m\ 41^s$, and July 21, $9^h\ 42^m\ 17^s$, O. S. all which tally with observations made at Wansted by the Rev. Dr. Pound and Mr. Bradley, who observed there the very next eclipses to all the three; viz. the immersion by a 15-foot tube, on April 11, $15^h\ 28^m\ 40^s$ equal time, or $15^h\ 30^m\ 25^s$ apparent time. And the first emersion, July 7, $10^h\ 59^m\ 28^s$ equal time by the reflector, and 18^s after, or $10^h\ 59^m\ 46^s$ by the 15-foot glass, that is, $10^h\ 54^m\ 12^s$ apparent time. The other was observed at Wansted, July 23, $9^h\ 19^m\ 10^s$ equal time, both by the reflector and 15-foot glass, viz. at $9^h\ 13^m\ 35^s$ apparent time. Subtract from each of these one period of this satellite, or $1^d\ 18^h\ 28^m\ 36^s$, and April 9, $15^h\ 58^m\ 44^s$ at Carthagena will be $21^h\ 1^m\ 49^s$ of the same day at Wansted, and the difference of meridians $5^h\ 3^m\ 5^s$. Likewise by the first emersion, July 5, $11^h\ 23^m\ 41^s$ at Carthagena, was at Wansted $16^h\ 25^m\ 36^s$ of that day, whence the difference of meridians $5^h\ 1^m\ 55^s$. But by the last emersion, July 21, $9^h\ 42^m\ 17^s$ at Carthagena, was $14^h\ 44^m\ 59^s$ at Wansted; whence Wansted is $5^h\ 2^m\ 42^s$ more easterly than Carthagena; and taking the medium of all three, $5^h\ 2^m\ 34^s$, or 75° 38′, may be taken for the true difference of longitude, that is, 75½ from London, which compared with Capt. Candler's observation of the late lunar eclipse, shows Carthagena to be about 20 leagues to the eastwards of Port Royal in Jamaica.

Observations on a Comet seen at Berlin, from the 18*th of January to the* 5*th of February,* 1718, *N. S. By Christfried Kirch.* N° 375, p. 238. *Translated from the Latin.*

Here M. Kirch hints, that the observations of this comet, published in Nov. Liter. Lipsien. are not accurate; for, on the 23d of January in the morning, the comet formed an isosceles triangle with θ and φ of Cassiopeia, and not with δ and φ; and in the evening, the φ of Perseus, the comet, and the θ of Cassiopeia were as to sense in a straight line.

M. Kirch observed the comet from the 18th of January to the 5th of February: the following table exhibits its places by observations, at 10 in the evening, when it could be seen.

	Longitude.			Latitude.			Longitude.			Latitude.	
18 Jan..	27°	26′	Cancer..	69°	18′ s.	30 Jan..	3°	4′	Taur..	28°	23½′ s.
21 Jan..	16	25½	Taurus..	48	42 s.	31 Jan..	2	43	Taur..	27	40 s.
23 Jan..	9	28½	Taurus..	39	45 s.	1 Feb..	2	25	Taur..	27	1 s.
26 Jan..	5	25½	Taurus..	32	55 s.	2 Feb..	2	10	Taur..	26	22 s.
27 Jan..	4	41	Taurus..	31	24 s.	5 Feb..	1	39	Taur.	24	53 s.
28 Jan..	4	4	Taurus..	30	13 s.						

The path of the comet passed above the back of the Ursa Minor, near the pole star, through the legs and knees of Cepheus, Cassiopeia, and Andromeda; its descending node was in $21\frac{1}{2}°$ of Aries, with some mutation; the angle of the cometic orbit and ecliptic was about $69\frac{1}{2}°$, with some variation too; the path of the comet was almost $2°$ from the pole of the world, and intersected the equator in $20\frac{1}{4}°$ from the equinoctial point; its perigæum was in $6° 6'$ of Virgo, with $62° 7'$ N. lat. The comet was in its perigæum on the 18th of January $3^h 9^m$ in the morning; its diurnal motion in its orbit was $22° 8'$ in the perigæum, viz. 12 hours before and 12 hours after the perigæum, but on the last days of its appearing $32'$. Supposing the earth at rest, and the comet moving in a right line, the motion of the comet was 391 such parts, as the least distance of the comet from the earth is 1000. M. Kirch could determine nothing with certainty about the parallax of the comet, only that it was a great deal higher than the moon; and he conjectures, with some probability, that it moved within the orbits of the planets; nay, that in its perigæum it was much nearer to us than the sphere of Mars. For, suppose the semidiameter of the earth's orbit be 10,000 parts, the diurnal motion of Mars will be 139 or 140 such parts. If we suppose the comet to have been in the orbit of Mars, with $62° 7'$ lat. and $22° 8'$ diurnal motion, its velocity would be 2847 parts, supposing at the same time it was in opposition to the sun; but since the difference of long. of the sun and comet in its perigæum was only $141° 40'$, the diurnal motion of the comet becomes 3200 parts, and the proportion of the motion of the comet to the motion of Mars as 23 to 1; wherefore he collects, that the comet moved within the sphere of Mars. But should any one suppose the comet to have moved within the orbit of Saturn, it should then have a velocity, which would be to the velocity of Saturn, as 600 to 1, and that in one day it would run over a greater space than the earth does in half a year; not to mention the diameter of the comet, which should be no less than 3 diameters of the sun.

M. Kirch, comparing this comet with others, found that observed by Regiomontanus, in January and February 1472, or 1475, moved in a tract not very different from it; for it passed through Ursa Minor, the thighs of Cepheus, the breast or neck of Cassiopeia, and the girdle of Andromeda; and its greatest velocity in a day was $40°$. Another comet was observed in 1556, whose nodes Camerarius placed in $11°$ of Libra and Aries, and which passed near the feet of Ursa Minor, through Cepheus, above Cassiopeia, and through the upper parts of Andromeda, with a very swift motion in its perigæum. And if Regiomontanus observed a comet in 1475, which astronomers very much doubt, there would be a surprising coincidence between these three comets; for, the interval between the former and middle comet would be 81 years, and between the middle and last comet 162 years, so that the revolution of the comet would be 81 years, and the history of other comets would also agree very well with these.

Concerning the Appearance of several Arches of Colours, contiguous to the inner Edge of the common Rainbow, observed at Petworth in Sussex, by the Rev. Dr. Langwith. N° 375, p. 241.

When the primary rainbow has been very vivid, I have observed in it, more than once, a second series of colours within, contiguous to the first, but far weaker, and sometimes a faint appearance even of a third. These increase the rainbow to a breadth much exceeding what has hitherto been determined by calculation.

I have since observed something of the same nature, though not in the same degree of perfection, with the above. On March 22, 1722, a little before 6 in the evening, wind at N. W. by W. we had here a lively, distinct, primary rainbow, the inner and purple colour of which had a far greater mixture of red in it than I could ever observe in Sir Isaac Newton's oblong spectrum. Under this was a space, of a breadth considerably less than that of the limbus of the rainbow, in which I could not distinguish any colours; still lower was a faint interrupted arch of red, inclining to purple, which appeared and vanished several times, while I was observing it.

March 27, 1722, in the evening, about a quarter before 6, wind S. W. we had one of the finest rainbows I ever beheld. The first series of colours was as usual, only the purple had a far greater mixture of red in it than I had ever seen in the prismatic purple; under this was a coloured arch, in which the green was so predominant, that I could not distinguish either the yellow or the blue: still lower was an arch of purple, like the former, highly saturated with red, under which I could not distinguish any more colours.

The order of the colours in this compounded rainbow was, you see, red, yellow, green, blue, a mixture of purple and red, green, or rather a mixture of yellow, green, and blue, a mixture of purple and red.

August 21, about half an hour past 5 in the evening, weather temperate, wind at N. E. the appearance was as follows, viz. The colours of the primary rainbow were as usual, only the purple very much inclining to red, and well defined; under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more dusky green; under this were alternately two arches of reddish purple and two of green; under all a faint appearance of another arch of purple, which vanished and returned several times so quick, that we could not steadily fix our eyes upon it. Thus the order of the colours was, 1. Red, orange colour, yellow, green, light blue, deep blue, purple. 2. Light green, dark green, purple. 3. Green, purple. 4. Green, faint vanishing purple.

There are two things which well deserve to be taken notice of, as they may perhaps direct us in some measure to the solution of this curious phenomenon. The 1st is, that the breadth of the first series so far exceeded that of any of the rest, that it was equal to them all taken together. The 2d is, that I have never observed these inner orders of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this so very often, that I can hardly look upon it to be accidental; and if it should prove true in general, it will bring the disquisition into a narrow compass; for it will shew that this effect depends on some property which the drops retain while they are in the upper part of the air, but lose as they come lower, and are more mixed with each other.

On the abovementioned Appearance in the Rainbow, with some other Reflections on the same Subject. By Henry Pemberton, M.D. R.S.S. N° 375, p. 245.

Let AB, fig. 1, pl. 16, represent a drop of rain, B the point from whence the rays of any determinate species being reflected to C, and afterwards emerging in the line CD, proceed to the eye, and cause the appearance of that colour in the rainbow, which appertains to this species. It is observed by Sir Isaac Newton, Optics, book 2, part 4, that in the reflection of light, besides what is reflected regularly, some small part of it is irregularly scattered every way. So that from the point B, besides the rays that are regularly reflected from B to C, some scattered rays will return in other lines, as in BE, BF, BG, BH, on each side the line BC. Further, it must be noted from Newton, Optics, part 2, prop. 12, that the rays of light, in their passage from one superficies of a refracting medium to the other, undergo alternate fits of easy transmission and reflection, succeeding each other at equal intervals; insomuch that if they reach the farther superficies in one sort of those fits, they shall be transmitted; if in the other kind of them, they shall rather be reflected back. Whence the rays that proceed from B to C, and emerge in the line CD, being in a fit of easy transmission, the scattered rays that fall at a small distance without these on either side, suppose the rays, that pass in the lines BE, BG, shall fall on the surface in a fit of easy reflection, and shall not emerge; but the scattered rays that pass at some distance without these last, shall arrive at the surface of the drop in a fit of easy transmission, and break through that surface. Suppose these rays to pass in the lines BF, BH; the former of which rays shall have had one fit more of easy transmission, and the latter one fit less, than the rays that pass from B to C. Now both these rays when they go out of the drop, will proceed by the refraction of the water in the lines FI, HK, which will be inclined almost equally to the rays incident on the drop that come from the sun, but the angles of their inclination will be less than the angle in which the rays emerging in the line CD are inclined to those incident rays. And after the same manner, rays scattered from the point B, at a certain distance without these, will emerge out of the drop, while the intermediate rays are intercepted; and these emergent rays will be inclined to the rays incident on the drop, in angles still less than the angles in which the rays FI and HK are inclined to them; and without these rays will emerge other rays, that shall be inclined to the incident rays in angles yet less. Now by this means will be formed of every kind of rays, besides the principal arch which goes to the formation of the rainbow, other arches, within every one of the principal, of the same colour, though much more faint; and this for divers successions, as long as these weak lights, which in every arch grow more and more obscure, shall continue visible. Now as the arches produced by each colour will be variously mixed together, the diversity of colours observed by Dr. Langwith may well arise from them.

The precise distances between the principal arch of each respective colour, and these fainter correspondent arches, depend on the magnitude of the drops of rain. In particular, the smallest drops will make the secondary arches of each species at the greatest distance from their respective principal, and from each other. Whence, as the drops of rain increase in falling, these arches near the horizon, by their great nearness to their respective principal arches, become invisible.

Of refracting circles in two porisms.—I shall here set down two propositions, which I have formerly considered, relating to this subject. For the greater brevity I shall deliver them under the form of porisms; as, in my opinion, the ancients called all propositions treated by analysis only.

PROP. 1. *In a given refracting Circle, whose refracting Power is given, the Ray is given in Position, which, passing Parallel to a given Diameter of the Circle, is refracted by that Circle to a Point given in its Circumference.*—Let ABCD, fig. 2, be the given circle, the given diameter AC, and given point G; and let the ray EF, parallel to AC, be refracted to G. I say EF is given in position.

Produce EF to H, and draw the diameter FI, drawing also IKH, IG. Then is HFI the angle of incidence, and GFI the refracted angle; so that IH being perpendicular to FH, and IG perpendicular to FG, IH is to IG as the sine of the angle of incidence to the sine of the refracted angle, and the ratio of IH to IG is given, as also the ratio of IK to IG. Therefore IK being perpendicular to AC, the point I is in a conic section given in position, whose axis is perpendicular to AC, and one of its foci is the point G. (See Papp. l. 7. prop. 238. Milnes Conic. part. 4. prop. 9.) Consequently the points I and F are given, and lastly the ray EF given in position.

Determination. It is evident, that this conic section may either cut the circle in two points, touch it in one point, or fall wholly without it. Therefore let the section touch the circle in the point I, fig. 3, and let IL touch both the section and the circle in the same point I. Then GL being joined, the angle under IGL, on account of the conic section, is a right one; so that FGL is one continued right line, and IF is to IL as FG to GI; also, M being the centre of the circle, MI to IL, or FH to HI, as FG to 2GI, because MI is to IF as GI to 2GI. Hence by permutation FH is to FG as HI to 2GI; that is, as the sine of the angle of incidence to twice the sine of the refracted angle.

Further, FH being to HI as FG to 2GI, the square of FH will be to the square of HI, as the square of FG to 4 times the square of GI. Therefore, by composition, as the square of FH to the square of FI or of AC, so is the square of FG to the square of FI together with 3 times the square of GI, and so likewise is the excess of the square of FG above the square of FH, which equals the excess of the square of IH above the square of IG, to 3 times the square of GI; for as one antecedent to one consequent, so is the difference of the antecedents to the difference of the consequents. Hence in the last place, the square of half FH will be to the square of AM, as the excess of the square of IH above the square of IG to 3 times the square of IG, or as the excess of the square of the sine of incidence above the square of the sine of refraction, to 3 times the square of the sine of refraction.

Another Determination.—Draw the diameter GO, fig. 4, and the tangent OP, meeting GF produced in Q: then the angle under IFG is equal to the angle under OGF, the angle under FIL equal to that under GOQ, both being right, and FI is equal to GO; whence the triangles GOQ, FIL are similar and equal; so that GQ is equal to FL, and the point F in an hyperbola passing through G, whose asymptotes are AC and OP. (Apoll. Conic. l. 2, prop. 8.)

PROP. 2. *A refracting Circle and its refracting Power being given, the Ray is given in Position, which passing Parallel to a given Diameter of the Circle, after its Refraction, is so reflected from the farther Surface of the Circle, as to be inclined to its incident Course in a given Angle.*—Let ABCD, fig. 5, be the given circle; let AC be the given diameter, EF the incident ray parallel to it, which, being refracted into the line FG, shall be so reflected from the point G in the line GH, that EF and HG being produced, till they meet in I, the angle under EIH shall be given.

Let K be the centre of the circle, and KF, KG be joined; let the semidiameter LK be parallel to the refracted ray FG; and, MK being taken to the semidiameter of the circle in the ratio of the sine of incidence to the sine of refraction, let LM be joined; and lastly, make the angle under KMN equal to half the given angle under EIH. Then, if FG be produced to O, FO shall be to KO, as the sine of the angle of incidence to the sine of the refracted angle, that is as MK to KL; so that KL being parallel to FO, and the angle under MKL equal to that under FOK, the angle under MLK shall be equal to that under FKO, and the angle under KML equal to that under KFO equal to that under FGK, or half that under FGH; whence the angle under KMN being equal to half the angle under FIH, the residuary angle under NML will be equal to half the angle under IFG or to half that under MKL. Therefore LC being drawn, the angle under LMN will be equal to that under MCL; and in the last place, if MC be divided into two equal parts in P, and PQR be drawn parallel to CL, the angle under QMR will be equal to that under RPM, and the triangles QMR, MPR similar, so that the rectangle under PRQ shall be equal to the square of MR. Whence RL being equal to MR, the point L shall be in an equilateral hyperbola, touching the line MN in the point M, and having the point P for its centre. (Apoll. Conic. lib. 1, prop. 37, compared with lib. 7, prop. 23.) But this hyperbola is given in position, and consequently the point L, the angle under MLK, and the equal angle under CKF will be given, and therefore the ray EF is given in position.

Determination.—Let the hyperbola touch the circle in the point L, fig. 6, and let their common tangent be LS; draw LT parallel to MN, so as to be ordinately applied in the hyperbola to the diameter CM. Whence LS touching the hyperbola in L, PT will be to TL as TL to TS, (Apoll. Conic. lib. 1, prop. 37, compared with lib. 7, prop. 23,) and the angle under TSL equal to that under TLP, but as the angle under SCL is equal to that under NML, the same is equal to the angle under TLM; therefore the angle under SLC is equal to the angle under MLP. Further, ML being produced to V, and VC joined, the angle under LVC is equal to that under SLC, because LS touches the circle in L; hence the angles under LVC and under MLP are equal, LP, VC are parallel, and MP being equal to PC, ML is equal to LV; and KW being let fall perpendicular to LV, MW is equal to 3LW. But now if the incident ray EF be produced to X, the angle under MLK being equal to that under CKF, or to that under EFK, FX shall be equal to LV, equal to 2LW; and the angle under KML being equal to that under KFG; since KW is perpendicular to MW, FG shall be to 2MW as MK to KF, or as the sine of incidence to the sine of refraction: whence MW being equal to 3LW, FX shall be to FG as the sine of incidence to 3 times the sine of refraction.

Also, MW being equal to 3LW, the square of MW will be equal to 9 times the square of LW, and the rectangle under VML, or the rectangle under CMA, that is, the excess of the square of KM above the square of KA, will be equal to 8 times the square of LW; therefore the square of LW, or the square of half FX, will be to the square of KL, or of KA, as the excess of the square of KM above the square of KA, to 8 times the square of KA, that is, as the excess of the square of the sine of incidence, above the sine of refraction, to 8 times the square of the sine of refraction.

Another Determination.—Draw AY parallel to MN, fig. 7, and AZ parallel to MV: then is the angle under YAZ, equal to that under LMN, which is equal to that under LCA; whence the arches AL, YZ are equal; but the arches AL, VZ are likewise equal, because LV, AZ are parallel, therefore YV being joined, and LΓ drawn perpendicular to AC, the chord VY shall be the double of LΓ; but VΔ being likewise let fall perpendicular to AC, because MV is the double of ML, VΔ shall be the double of LΓ; and therefore VΔ and VY shall be equal; whence the point V shall be in a parabola, whose focus is the point Y, its axis perpendicular to AC, and the latus rectum, belonging to that axis, equal to twice the perpendicular let fall from Y upon AC. (Vide de la Hire Sect. Conic. lib. 8, prop. 1, 3.) But if KV be joined, the angle under LKV is equal to twice the complement to a right angle of the angle under KLV, which is equal to the angle of incidence, and exceeds the refracted angle by the angle under AKL.

The determinations of these two propositions, have relation to the first and second rainbow; those of the first proposition respecting the interior, and those of the second the exterior. The first determinations of these two propositions assign the angles, under which each rainbow will appear in any given refracting power of the transparent substance, by which they are produced; the latter determinations of these propositions teach how to find the refracting power of the substance, from the angles under which the rainbows appear; the angle under CMG, in the determinations of the first proposition, being half the angle which measures the distance of the interior bow from the point opposite to the sun; and in the determinations of the second proposition, the angle under CMN is half the complement to a right angle of half the angle that measures the distance of the exterior bow, from the point opposite to the sun. But whereas these latter determinations require solid geometry, it may not be amiss here to show how they may be reduced to calculation, seeing the observation of these angles, as Dr. Halley has already remarked, (Phil. Trans. N° 267,) affords no inconvenient method of finding the refracting power of any fluid, or indeed of any transparent substance, if it be formed into a spherical or cylindrical figure. For this purpose therefore I have found, that in the latter determination of the first proposition, if the sine of the angle under CMG, fig. 4, be denoted by a, the tangent of the complement of this angle to a right one be denoted by b, and the secant of this complement by c; the root of this equation $z^3 - 3aaz = 2aa \times \overline{2c - a}$ will exceed the sine of the angle under FMA, that is the sine of the angle of incidence, by the sine of the angle under CMG; and the sine of the angle under FMO, which is double the refracted angle, will be the root of this equation $x^3 + 3aax = 4aab$; this angle being acute, when the tangent of the angle under CMG is less than half the radius, or when the angle itself is less than 26° 33′ 54″ 11‴, and when this tangent is more than half the radius, the angle under OMF is obtuse.

The roots of these cubic equations are found by seeking the first of two mean proportionals, between each of the versed sines appertaining to the arches CG, AG, and the sine of those arches, counting from the versed sines; for the sum of these two mean proportionals is the root of the former equation, and the difference between them the root of the latter; as may be collected from Cardan's rules.

And hence also, if the first and last of the 5 mean proportionals, between the sine and cosine of half the angle under CMG be found, twice the sum of the squares of these mean proportionals applied to the radius, exceeds the sine of the angle of incidence, by the sine of the angle under CMG; and twice the difference of the squares of the same mean proportionals, applied to the radius, is equal to the sine of double the refracted angle. Further, this double of the refracted angle exceeds the angle of incidence, by the angle under CMG.

In the latter determination of the 2d proposition, draw KY, fig. 8, and AY being parallel to MN, the angle under CKY will be equal to twice the angle under CMN, that is equal to the complement of half the distance of the exterior rainbow from the point opposite to the sun. Then putting a for the radius AK, and b for the sine of the angle under CKY, the sine of the angle under AKV will be the root of this equation $y^4 + 4by^3 - 8aaby + 4aabb = 0$. But the angle of incidence and refraction may also be found as follows:

Let two mean proportionals between the radius and the sine of the angle under CKY be found; then take the angle, whose cosine is the first of these mean proportionals, counting from the radius; and also the angle whose sine, together with the second mean proportional, shall be to the radius, as the cosine of the angle under CKY, to the sine of the angle before found. The sum of these three angles, is double the complement to a right one of the angle under AKL, the angle under KML, or the refracted angle, being equal to half the sum of this angle under AKL and the angle under CKY: as in the last place the angle under KLV, that is the angle of incidence, equal to the sum of the angles under KML and under MKL.

I need not observe, that the geometrical methods of deducing these angles of incidence and refraction from the angle measuring the distance of each rainbow from the point opposite to the sun, afford very expeditious mechanical constructions.

On the Method of procuring the Small Pox, in South Wales. By Perrot Williams, M. D. N° 375, p. 262.

However new the method of communicating the small pox may appear in this kingdom; yet it has been commonly practised by the inhabitants of Pembrokeshire in Wales, time out of mind, though by another name, viz. that of buying the disease, as I have been long since informed by several who procured the distemper by that means. There is a married woman in the neighbourhood of this place, who practised it on her daughter, about a year and a half since, by which means she had the small pox favourably, and is now in perfect health, though she has ever since, without reserve, conversed with such as have had that distemper.

To procure the distemper, they either rub the matter taken from the pustules when ripe, on several parts of the skin of the arms, &c. or prick those parts with pins, or the like, first infected with the same matter. And though they omit the necessary evacuations, such as purging, &c. yet I am informed they generally come off well enough; and I cannot hear of one instance of their having the small pox a second time.

One Mr. Owen, a gentleman of this country told me, that above 20 years since when at school, he and several of his schoolfellows, infected themselves at the same time, from the same person, and that not one of them miscarried. The method he used was this: having rubbed the skin off his left hand with the back of his penknife, till the blood began to appear, he applied the variolous matter to that part; which by degrees growing inflamed, about a week afterwards he fell into the small pox. I have since conversed with several others, who made the like experiments on themselves, 20 years since: who all positively affirm, that they never had the small pox a second time.

Haverford West, Sept. 28, 1722.

A further Account of procuring the Small Pox. By the same. N° 375, p. 264.

Mr. Owen was about 15 years of age, when he made the experiment on himself; and it cannot be doubted had the genuine small pox, the signs of them on his face, and the mark on his hand, where he applied the matter, being still so very visible, as to put that matter beyond dispute. I can aver, that within the compass of 20 years last past, I have been so often assured of the truth of the practice, not by children, but grown persons of undoubted credit, that I am entirely satisfied it has been an immemorial custom in these parts; and not only practised by boys at school, but also by many others of both sexes more advanced in years, and consequently capable of distinguishing the small pox from other distempers. There are now living, in this town and neighbourhood, 5 or 6 persons, who undoubtedly had that distemper after taking the aforesaid method to infect themselves; one of whom, a young woman aged 23, told me that, about 8 or 9 years ago, in order to infect herself, she held 20 pocky scabs, taken from one towards the latter end of the distemper, in the hollow of her hand, a considerable time; that about 10 or 12 days after, she sickened, and had upwards of 30 large pustules in her face, and other parts; and that she has since freely conversed with such as have had the small pox on them.

To make it appear that inoculation is a sufficient preservative against receiving the small pox a second time; about 6 weeks since, I caused my two sons, who had been inoculated this last summer, not only to see, but even to handle a child, dying of a most malignant sort of small pox; and yet they continue in perfect health.

On a very exact inquiry I find, that out of 227 who have had the small pox in the natural way, in this town and a neighbouring parish, since the beginning of June last, 52 have died.

Haverford West, Feb. 2, 1723.

A further Account of buying the Small Pox. By Mr. Richard Wright, Surgeon at Haverford West. N° 375, p. 267.

In Wales the custom commonly called buying the small pox, on a strict inquiry, I find to be a common practice, and of very long standing; being assured by persons of unquestionable veracity, and of advanced age, that they have had the small pox communicated to themselves this way, when about 16 or 17 years of age, being then very capable of distinguishing that distemper from any other; and that they have parted with the matter contained in the pustules to others, which produced the same effects. Two large villages near the harbour of Milford, are more famous for this custom than any other, viz. St. Ishmael's and Marloes. The old inhabitants of these villages say, that it has been a common practice with them time out of mind; and, what was more remarkable, one William Allen, of St. Ishmael's, 90 years of age, who died about 6 months since, declared to some persons of good sense and integrity, that this practice was used all his time; that he very well remembered his mother's telling him, that it was a common practice all her time, and that she got the small pox that way.

These, together with the many other informations, I have met with from almost all parts of the county, confirm me in the belief of its being a very antient and frequent practice, among the common people; and to prove that this method is still continued among us, one Joan Jones, a midwife, 70 years of age, solemnly declares, that about 54 years since, having then the small pox, one Margaret Brown, about 12 or 13 years of age, bought the small pox of her; that the said Margaret Brown was seized with the small pox a few days afterwards; that the said Margaret Brown had not had the small pox a 2d time. She further says, that she has known this way of procuring the small pox practised from time to time, above 50 years; that it has been lately used in her neighbourhood, and that she knows of only one dying of the said distemper, when communicated after the method aforesaid, which accident happened within these 2 years last past; the person who miscarried, a young woman about 20 years of age, having procured the distemper from a man then dying of a very malignant small pox. Further, that hundreds in this country have had the small pox this way, is certain; and it cannot produce one single instance of their ever having them a second time.

Haverford West, Feb. 15, 1723.

Experiments, to prove that the Force of moving Bodies, is proportionable to their Velocities: or rather that the Momentum of moving Bodies is to be found by multiplying the Masses into the Velocities. By the Rev. John Theophilus Desaguliers, LL. D. F. R. S. N° 375, p. 269.

M. Leibnitz, I believe, was the first that opposed the received opinion, concerning the quantity of the force of moving bodies; by saying, that it was to be estimated by multiplying the mass of the bodies, not by their velocity, but by the square of it. But, instead of showing any paralogism, in the mathematical demonstrations, made use of to prove the proposition, or any mistakes in the reasonings from the experiments made to confirm it, he uses other mediums to prove his assertions; (Acta Erudit. ad ann. 1686, p. 162;) and without any regard to what others had said on that subject, brings new arguments, which Dr. Clarke has fully answered, in his 5th letter to him. Messieurs John Bernoulli, Wolfius, Herman and others, have followed and defended Leibnitz's opinion, and in the same manner, so that what is an answer to him, is so to them.

Poleni, professor at Padua, has acted after the same manner in the experimental way, making some experiments to defend Leibnitz's opinion, without having shown those to be false which are made use of to prove the contrary; (Ptolenus de Castellis, p. 56, 57, &c.) and now lately, an ingenious professor abroad, who was of the opinion commonly received, and in his writings had demonstrated it in the usual way, (Gravesande Introductio, Vol. I. N° 132,) confirming it with the common experiments made in that case, happening to make some experiments like those of Poleni, has drawn conclusions from them, to show the force of moving bodies to be proportionable to the square of their velocity; and being wholly come over to that opinion, endeavours to deduce it from physical principles.

As there can hardly be said any thing new, or better than has been said, to show the force to be proportionable to the mass multiplied into the velocity; I only repeat here the substance of what others have said, and make some old experiments over again; but then I consider some circumstances that perhaps have been overlooked, and at last, by a new experiment, endeavour to show what has led into an error some of those who defend the new opinion.

If a man with a certain force can move a weight of 50lb. through a space of 4 feet, in a determinate time; it is certain he must employ twice that force to move 100lb. through the same space in the same time. But if he uses only the same force, he will move the 100lb. weight only 2 feet in the same time. For as the 100lb. weight contains two 50lb. weights, if each of them has 2 degrees of velocity given to it, it will exactly require the same force that would give one of them 4 degrees of velocity; hence it appears, that the force is proportionable to the mass multiplied into the velocity.

Exper. 1. Let the balance AB, fig. 9, pl. 16, whose fulcrum, or centre of motion, is at C, be so divided, that the brachium AC be but the 4th part of the brachium CB; then 100lb. at A, will keep in equilibrio a weight of 25lb. at B, where it will have a velocity 4 times greater than that of the weight at A. For, not only when the balance is horizontal, there will be an equilibrium, but when the balance is put in motion, it will return to an equilibrium in a horizontal position; the equal and contrary forces, applied at each, destroying each other. Whereas, if the forces were as the mass multiplied into the square of the velocity, the 25lb. weight should have been suspended at D, only twice as far from C, as the weight at A. And in general, let the make of the engine be what it will, let the mechanical powers be combined in any manner, when two heavy bodies, by means of the machine, act on each other in different directions, if their velocities be reciprocally as their masses, they will destroy each other's force, and come to rest.

As this is true in respect of mechanical powers, so is it also in respect to the shock or blow given by falling bodies. A heavy body, falling with an accelerated motion, goes through a space of 1 foot in a quarter of a second, and acquires a velocity, which would carry it 2 feet in the same time with a uniform motion; the same body falls through a space of 4 feet in half a second, and acquires a velocity that would, with a uniform motion, carry it 8 feet in half a second. Therefore, as the time of the fall through a space of 4 feet is twice the time of a fall through 1 foot, the velocity in the latter case is double that of the first, and consequently the blow, that the body will give, will be double.

Exper. 2. Let the weight P of 1lb. fig. 10, be placed in the scale suspended at the end A, of the balance AB, which bears on the gnomon, or iron supporter, khi. Then if the weight C be let fall from D, or 1 foot, it will by its stroke on the end of the beam B, raise up the opposite end A with the weight P, so high, that the spring gh will fly from the button i, which kept it straight and upright before the shock. If the weight P be of 2lb. it cannot be raised by the fall of C from any height less than F or 4 feet; whereas, if the force of the shock was proportionable to the space, without any regard to the time, as Leibnitz and his followers have affirmed, P ought to be raised, when C falls only from E, or 2 feet; which never happens; or, if the stroke was proportionable to the mass multiplied into the square of the velocity, when C falls from F, then P might weigh 4lb. whereas the experiment will never succeed under those circumstances.

Exper. 3. If, in order to avoid friction, instead of a blow struck on the end B, by the falling body, the body C be fastened to a pretty long string tied to the button m, as at c, and first lifted up 1 foot, and then let fall; so that in falling 1 foot, it may pull down B, and lift up A with the weight P of 1lb. whenever P is 2lb. C must fall from a height greater than f or 4 feet, otherwise it will not raise the brachium A, especially if it be let fall between e and f.

Exper. 4. I took the weight C of 17 ounces Troy, which was a round ball of lead, with a hole through the middle; and having passed the string NX through it, before it was fastened to the hook X, I placed the whole machine in such a manner, that the string being stretched by the weight N, went through the hole of the weight C, and also through the hole of the brachium B, upon which c lay, without touching the sides of the hole, either in the weight or balance; then having put such a weight P in the opposite scale, as c falling from the height of one inch, was able to raise high enough, to let loose the spring gh from the button i; I added to P another weight equal to it, and then letting fall c along the string that guided it, from a height of 2 inches, then of 3, and then exactly of 4, it would not raise the double weight P to the former height, but falling from 5 inches, or a little higher, it raised it up.

Exper. 5. Leaving every thing else as before, I changed the weight c, for another leaden ball of twice the weight, which falling from one inch, raised the double weight P to the usual height; then changing the weight P in any proportion, whatever height was required for the heaviest ball c, or c2, to fall from, in order to raise the weight at P; more than 4 times the height was re quired for the first ball c, to raise the same weight so high as to let loose the spring.

Exper. 6. I tried the experiment with the weight c hanging at the string mc, as in exper. 3, and a fall from a height of 5, or near 5 inches, was required to raise double the weight in the opposite scale, that a fall from 1 inch would raise; only here the height above 4 inches was not so great as in the former experiment, the friction being something less. Then I suspended the great ball c, or c 2, by the string mc, and when by falling 1 inch it raised the weight P, the little weight c could not produce the same effect, without falling from a greater height than 4 inches.

It is here to be observed, that which way soever these experiments are tried, the objections rising from the friction do no way serve to confirm the new opinion, because they show that, on account of the friction, the heights must be something more than in a duplicate proportion of the velocities, but never less, to give a blow with the same body in proportion to the velocity.

That the momentum of bodies is in proportion to the mass multiplied into the velocity, is almost evidently shown from the congress of elastic bodies, as has been demonstrated by Newton in his Principia, in the Corollaries to his Laws of Motion. I had often tried the experiments there mentioned with balls of ivory and balls of glass, and some of them with two balls of steel, of 2 ounces each, and found every thing answer, allowing for the want of perfect elasticity in the bodies. But now on this occasion, as the objections to the received opinion were renewed, I was willing to repeat the experiments with the utmost accuracy; and therefore, as ivory balls are not equally dense in all their parts, and glass balls break after two or three strokes; I caused balls to be nicely turned of steel, and made as hard, that is, as elastic, as possible, and their weights were precisely as follows: two balls of 12 ounces Troy each, one of 6 ounces, one of 3, one of 2, and one of 8 dwt. Then making pendulums of these balls, and hanging them on the machine contrived by Mariotte for the congress of bodies, and lately improved by Dr. Gravesande, I measured $57\frac{1}{4}$ inches between the centre of suspension and the centre of gravity of the balls, and then every degree of the circle they described in their oscillation was 1 inch, and the degrees being marked on a line of chords on a brass ruler above the balls, by their strings successively covering the cross lines of division, the degrees that the balls fell from, and those to which they rose, were very discernable to an eye placed at a convenient distance.

Exper. 7. I took the two balls 12, and removing each from the lowest point of their equal and respective circles, up to 4 inches, or 4 degrees, I let them fall so that they met at bottom, and were both reflected again to 4, the place from whence they fell.

Exper. 8. Every thing else being as before, instead of one of the balls 12, I took the ball 6, then letting 6 go from 8°, and 12 from 4, after reflection 12 was driven up again to 4, as before.

Exper. 9. The ball 3 falling from 16°, met the ball 12 that fell still from 4, and after reflection 12 went up again to 4.

Exper. 10. The ball 2 falling from 6°, and 12 from 1°, 12 was reflected to 1; and when 2 fell from 12°, and the ball 12 from 2, the 12 was reflected to 2.

Exper. 11. The ball of 8 dwt. which weighed only $\frac{1}{30}$ of the ball 12, falling from 15 inches or degrees, raised up 12, that fell from half a degree, to the same place again.

In all these experiments, the error, or want of perfect reflection, was greater in the little balls than in the large ones, on account of their going through a greater arc of a circle, by which they deviated more from a cycloid than the great ones; as also on account of the resistance of the air, which must be greater because of the little balls going through a greater arc, moving with more velocity, being suspended by a string as thick as that of the great ones, and having more surface in proportion to their weight. But all the errors do not bring the phænomena any thing near what they ought to be, if the force of the bodies was as the square of their velocities multiplied into their masses, for then the ball 12 would have been driven to heights very different from what it rose up to.

In the 8th experiment, the ball 12 should have risen to near $5\frac{3}{4}$ inches, for the ball 6 falling with the velocity 8, must have had its force $= 8 \times 8 \times 6 = 384$; and then, that the ball 12 might have the same force or quantity of motion, it must rise near to 5,7 because $5{,}7 \times 5{,}7 \times 12 = 389{,}88$.

In the 9th, 12 should have risen to 8; for the ball 3 must have had its force $= 16 \times 16 \times 3 = 768$; and if 12 received its whole force it must have risen to 8, because $8 \times 8 \times 12 = 768$.

In the second part of the 10th experiment, 12 should have risen to near 5, because $12 \times 12 \times 2 = 288$, and $5 \times 5 \times 12$ is but 300.

In the 11th, the ball 12, 30 times heavier than the little one, must have gone to $2\frac{3}{4}$ inches, because the momentum of the little ball being $= 15 \times 15 \times 1 = 225$, that of the ball 12 must be $= 2{,}75 \times 2{,}75 \times 12 = 226$ &c.

It may be here alleged, that one ought to subtract the momentum, with which the great ball comes upon the little one; but that will not mend the matter much, though indeed the difference will be less. For,

In the 8th experiment, if we subtract $4 \times 4 \times 12, = 192$, from 389,88, there will remain 197,88, and the ball 12 will go but to 4; but then in experiment 9, if we subtract the same N° 192 from 768, we shall have 576, which would carry 12 to near 7 degrees, because $7 \times 7 \times 12 = 588$.

In the 10th experiment, there is only 48 to be subtracted; and in the 11th only 15; and therefore the velocity of 12 will very much fall short of what is agreeable to the new opinion.

After the experiments made, and what has been said, till these consequences are overthrown, no notice ought to be taken of any objections, or new experiments. But to give the objectors all possible satisfaction, I shall, in another paper, endeavour to show the fallacies of the arguments, and solve the phænomena of the experiments made; showing, both by reason and experiment, that the facts ought to be as they are, in consequence of the received opinion and laws of resistance.

A Catalogue of 50 Plants lately presented to the R. S. by the Company of Apothecaries of London. N° 376, p. 279.

The Company of Apothecaries of London, having in the year 1673, established a physic-garden, which they afterwards furnished with a great variety of plants, for the improvement of their members in the knowledge of botany; Sir Hans Sloane, Bart. in order to encourage and promote an undertaking so serviceable to the public, generously granted to the company the inheritance of the said garden, being part of his estate and manor of Chelsea, on condition that it be for ever kept up and maintained by the company as a physic-garden; and as an evidence of its being so maintained, he directed and obliged the company, in consideration of the said grant, to present yearly for ever to the R. S. at one of their weekly meetings, 50 specimens of plants, that have grown in the said garden the preceding year, which are all to be specifically distinct from each other, until the number of 2000 plants be completed.

Accordingly the company lately presented to the R. S. by the hands of Mr. Rand, and Mr. Meres, 50 specimens of plants, for the last year 1722; which specimens, together with those that are to follow them in subsequent years, will, by order of the R. S. be carefully preserved for the satisfaction of such curious persons, as may desire to have recourse to them. Then follows the catalogue.

Animadversions on some Experiments relating to the Force of moving Bodies; with two new Experiments on the same Subject. By the Rev. Dr. Desaguliers, F. R. S. N° 376, p. 285.

In the preceding N° I demonstrated, by reason and experiments, that the momentum, or force of moving bodies, is always proportionable to their mass multiplied into their velocity; which is the opinion of most mathematicians and philosophers. I now come to consider the experiments that have led some ingenious men into an error, in regard to this proposition. Poleni gives an account of his experiments relating to this matter, in these words: "I took a vessel, that had in it congealed tallow 6 inches deep, and fixed it to a level floor, in such manner, that the surface of the tallow, which was flat, should every where be equally distant from the floor. I had caused to be made two balls of equal size, the one of lead, the other of brass, the last of which was a little hollow in the middle, that it might weigh but 1 lb. while the other weighed 2. Suspending these balls from the ceiling by threads, in such a manner, that the lighter ball hung over the surface of the tallow, from twice the height that the heavier ball did; I cut the threads, and the balls falling perpendicularly on the tallow, by their fall made pits in it, that were precisely equal: the ball of 1 lb. from the beginning of its fall, till it came to rest, going through a space expressed by the number 2, produced an effect equal to that which the 2 lb. ball produced, in falling through a space expressed by the number one. It follows therefore, that we may look upon it as a settled truth, that the active forces (vires vivas) of falling bodies, are equal, when their proper weights are in a reciprocal ratio of the spaces which the said bodies describe by their fall. And because these spaces are in the same ratio, as the squares of the numbers expressing the velocities; it appears by the experiment, that the active force (vim vivam) of the falling body, is that which is made up of the body itself, multiplied into the space described in the fall, or into the square of the number that expresses the velocity of the body, at the end of the motion. This experiment I made several times, changing the balls, the distances, and the body on which they fell; as for example, making use of clay, or of soft wax: and the effects were constantly the same; which made me easily conclude, that there was always the same reason in nature for this phenomenon."

Thus far Poleni, whose mistake lies in this; that he estimates the force of the stroke of the falling balls, by the depth of the impression made in the tallow, clay, wax, or any yielding substance. But we must consider, that when two bodies move with equal forces, but different velocities, that which moves the swiftest, must make the deepest impression, while the slowest body communicates its motion to the clay round about, and therefore does not strike in so deep as the swifter body, which puts in motion few parts of the clay, besides those that are before it, and which parts have so much less time to oppose this body's motion, as its velocity is greater than the other's. To make this plainer, let us suppose a door half open, and moving very freely on its hinges; if a pistol be fired against it, the ball will go through the door without moving it out of its place; but if we take a large weight of lead, and throw it against the same door, with the same force as the pistol bullet moved, the door will be removed from its position, and carried out of the place on its hinges by the stroke; because in the first case, the motion of the ball is communicated but to a few parts of the door, and in the last it is diffused all over it. Nay, the door will be moved by the stroke, even though there should be a prominent part in the lead, that should be no larger than a pistol-bullet, in order to strike the door on no more of its surface than the bullet had done.

For illustrating this further I contrived the following Experiment.—I caused a machine to be made, as represented in fig. 21, pl. 14, consisting of a wooden base AB, which could be set horizontal by means of 3 screws, as SS. On this board, or base, stood upright 2 parallel boards, about 4 inches wide, and 4 inches asunder, with the elbow-piece EF sliding behind one of them, so as to raise its upper end F to any height desired. Between these boards, square frames of wood GG &c. with paper extended on them, could slide in, to the number of 6, in a horizontal position. These paper diaphragms being thus placed, I suspended an ivory ball of about one inch and a half diameter, weighing something more than an ounce and a half, by a short thread, under F, so that its centre of gravity hung 4 feet over the first diaphragm; then cutting the thread, the ball fell on the paper, and by its perpendicular stroke broke through that diaphragm, and the 3 next under it. Then putting so much lead into the ball, which was made hollow for that purpose, as to make it weigh twice as much as it did before; and bringing down F, to let it fall but from one foot, it broke through only 2 diaphragms by its fall. Making the experiment several times with different heights, but still keeping the proportion in height of 4 to 1, when the balls were as 1 and 2, the heavy and slowest ball broke through but half the number of papers. It happened indeed sometimes, that there was some little difference, when the papers were not equally strong, or equally stretched, but the swiftest ball always broke through more papers than the slow one.

Now though this experiment at first seems to confirm Poleni's theory; yet, when duly weighed, it proves no such thing. For the lighter ball does not break through more papers, because it has more force, or a greater quantity of motion, but because each diaphragm has but half the time to resist the ball, that falls with a double velocity, and therefore their resistance being as the time, as many more of them must be broken by the swift ball, as by the slow one.

P. S. To all the objectors, that allow the force of moving bodies, and their quantity of motion to be the same, what has been said in this and my former paper, seems to be a full answer; but as there are now some philosophers, who distinguish that force from the quantity of motion, I am obliged to say something more for clearing up that point.

If I understand them right, they call vis viva a force, whose effect is sensible, as the force of gravity, when it accelerates bodies in their fall; and vis mortua a force, which being destroyed, produces no sensible effect, as the force of gravity acting upon a weight in one scale of balance, when the weight cannot descend by reason of a counterpoise in the other scale. But certainly no man, that considers the thing attentively, would make that distinction. However, since Poleni allows that the quantity of motion in bodies, is as the mass multiplied into the velocity, or MV; but says, that the force, with which they act, which he distinguishes by the name of vis viva, is as the mass multiplied into the square of the velocity, or MVV: I have made the following experiment to show his notion to be inconsistent; though all the phænomena of unequal weights applied to a statera, so as to make an equilibrium, might serve for that purpose, if it had not been objected, that the particular construction of the machine hindered it from agreeing with the supposed theorem, that the force is as the matter multiplied into the square of the velocity.

Exper. Let two balls, A and B, fig. 22, be joined by a string, which going through the smooth hole C of an even table, and under the pulley P, suspends a weight W. It is plain, that on letting go the balls A and B, from the places A and B, they will move towards C with the same force, because each of them will be drawn towards C by half the force of the weight W, whether the balls be equal, or unequal.

1. The balls being of 2 ounces each, of ivory, were, at the same instant of time, let loose from A and B, each distant 12 inches from C, and both came

to C at the same time. Here the equal forces will agree with the product of the masses into the velocities, or into the squares of the velocities; because A × 12 = B × 12, as well as A × 144 is equal to B × 144.

2. If A be taken of 4 ounces weight, and let go from D, or 6 inches, while B, still equal to 2, moves from 12 inches; both bodies will again meet at C: therefore here the equal forces must be expressed by the masses into the velocities, and not into their squares; for though A × 6 be equal to B × 12, or 4 × 6 = 2 × 12, yet A × 6 × 6, or 144 is but half of B × 12 × 12, or 288. Whereas if the forces had been as Poleni affirms, B should have been let loose only from 8,4 inches.

3. When A is 6 ounces, it is let loose only from E, or 4 inches, to meet at C with B, let loose from 12; for then A × 4 = B × 12, while A × 4 × 4, or 96, is 3 times less than B × 12 × 12, or 288. So that according to Poleni, B must have been let loose from 7; but in that case it comes sooner to C than A.

N. B. The weight W must be greater than the weight of both balls, lest the friction of the table should spoil the experiment.

An Account of an Experiment, made to ascertain the Proportion of the Expansion of the Liquor in the Thermometer, with Regard to the Degrees of heat. By Brook Taylor, LL.D., R.S.S. N° 376, p. 291.

It has been generally supposed, though not proved, that the expansion of the liquor in the thermometer, is proportional to the increase of heat. To determine this matter with certainty, I made the following experiment.

I provided a good linseed oil thermometer, which I marked with small divisions, not equal in length, but equal according to the capacity of the tube in the several parts of it, as all thermometers ought to be graduated. I likewise provided 2 vessels of thin tin, of the same shape, and equal in capacity, containing each about a gallon. Then, observing in every trial, that the vessels were cold, before the water was put in them, as also that the vessel I measured the hot water with, was well heated with it, I successively filled the vessels with one, two, three, &c. parts of hot boiling water, and the rest cold; and at last with all the water boiling hot; and in every case I immersed the thermometer into the water, and observed to what mark it rose, making each trial in both vessels for the greater accuracy. And having first observed where the thermometer stood in cold water, I found that its rising from that mark, or the expansion of the oil, was accurately proportional to the quantity of hot water in the mixture, that is, to the degree of heat.

An Account of the Rattlesnake. By the Hon. Paul Dudley, F. R. S. N° 376, p. 292.

The rattlesnake is reckoned by the aborigines, to be the most terrible of all snakes, and the master of the serpent-kind; and it is most certain, that both men and beasts are more afraid of them, than of other snakes; and while the common snake avoids a man, this will never turn out of the way.

There are three sorts of this snake, distinguished by their colour, viz. a yellowish green, a deep ash colour, and a black satin.

The eye of this creature has something so singular and terrible, that there is no looking stedfastly on him; one is apt, almost, to think they are possessed by some demon. A rattlesnake creeps with his head close to the ground, and is very slow in moving, so that a man may easily get out of his way. His leaping and jumping to do mischief, is no more than extending, or uncoiling himself; for they do not remove their whole body, as other creatures do, when they leap; so that a man is in no danger from them, if his distance be more than their length; neither can they do any harm when they are in their ordinary motion, till they first coil and then extend or uncoil themselves; but they both are done in a moment's time. When a rattlesnake rests, or sleeps, he is coiled, and they are observed to be exceedingly sleepy. The tail is composed of joints, that overlap each other, somewhat like a lobster's tail; and their striking against each other, forms that noise which is so terrible to man and beast. The fiercest noise is observed to be in clear fair weather, for when it is rainy, they make none at all. One other circumstance of their rattling has been observed, viz. that if a single snake be surprised and rattles, and there happen to be others near him, they all take the alarm, and rattle in like manner.

I dare not answer for the truth of every story I have heard, of their charming, or power of fascination; yet I am abundantly satisfied from many witnesses, both English and Indian, that a rattlesnake will charm both squirrels and birds from a tree into his mouth.* A man of undoubted probity sometime since told me, that as he was in the woods, he observed a squirrel in great distress, dancing from one bough to another, and making a lamentable noise, till at last he came down the tree, and ran behind a log: the person going to see what was become of him, spied a great snake, that had swallowed him. And I am the rather confirmed in this relation, because my own brother, being in the woods, opened one of these snakes, and found 2 stripped squirrels in his

* Respecting the supposed fascinating power of the rattlesnake and other serpents, see a memoir by Professor Barton, inserted in the Trans. of the Phil. Society at Philadelphia.

belly, and both of them head foremost. When they charm, they make a hoarse noise with their mouths, and a soft rattle with their tails, the eye at the same time fixed on the prey.

Their general food consists of toads, frogs, crickets, grashoppers, and other insects, but principally of ground mice; and the rattlesnake again serves for food to bears, and even our hogs will eat them without harm.

They are viviparous, and bring forth generally about 12, and in the month of June. A friend of mine in the country, being desirous to discover the nature and manner of the generation of the rattlesnake, gave me the following account, viz. About the middle of May, the time when the rattlesnakes first come abroad, he took and opened one of them, and in the matrix found 12 small globes, as large as a common marble, in colour like the yolk of an egg; in 3 or 4 days more, he took and opened another, and then plainly perceived a white speck in the centre of the yellow globe; in 3 or 4 days more, he dissected a 3d, and discovered the head of a snake; and in a few days after that, 3 quarters of a snake were formed, lying round in a coil. In the latter end of June, he killed an old one, and took out perfect live snakes of 6 inches long. In September, when the old ones take their young in, and carry them to their dens, they are not quite a foot long. They couple in August, and are then most dangerous.

They are generally from 3 to 5 feet long, and do not commonly exceed 20 rattles. They shed, or throw off their skins every year, sometime in the month of June, and turn it inside out when they throw it off. It has also been observed, that the skin covers not only the body, but the head and eyes.

They generally den among the rocks in great numbers together; the time of their retiring is about the middle of September, and they do not come abroad till the middle of May, when the hunters watch them, as they come out a sunning, and kill them by hundreds.

Roxbury, New-England, Oct. 25, 1722.

Some Observations upon Vipers; on occasion of the foregoing Relation. By C. J. Sprengell, M.D., F.R.S. N° 376, p. 296.

At Milan I found a viper-catcher, who seldom was without 60 or more living vipers, kept together in a back room open at the top; he had them from all parts of Italy, and sold them dead or alive, according to the uses they were designed for. He having got one day a female viper big with young, gave me notice to see her manage her prey. We catched some mice, and throwing them in one at a time, among all that number of vipers, which were rather above 60, none of them in the least concerned himself about the mouse till the last-mentioned pregnant female viper and the mouse interchanged eyes; on this the mouse startled; but the viper raised her head, and turned her neck into a perfect bow, the mouth open, the tongue playing, the eyes all on fire, and the tail erect. The mouse seemed soon recovered of his fright, would take a turn or two, and sometimes more, pretty briskly, round the viper, and giving now and then a squeak, would run swiftly into the chops of the viper, where it gradually sunk down the gullet. All this while the viper never stirred out of its place, but lay in a ring. It is remarkable that no viper will feed, when confined, except a female viper impregnated.

The same I saw at Brussels, where a soldier had caught a large viper big with young. The house, where I and some of my companions lodged, was near the fish-market, where my landlord had a sow, and 5 small pigs of 9 or 10 days old. We got one of the pigs, which we caused to be bit by the viper in the tail, and in 4 minutes time chopped off the tail, the pig appearing to be sick and dizzy, and the remaining part of the tail being swelled; but I believe the bleeding saved it, for the next morning it was well again. The same happened to another pig, which we had got bit in the fore foot, and staying 7 minutes after the bite, cut off his leg about 2 inches above the bite. After these 2, we took the other 3, and had them bit in several places, whereof 2 died that night, and the 3d recovered, we having given it, about 5 or 6 minutes afterwards, 10 grains of emetic tartar. This I tried afterwards upon dogs bit by vipers, and I found that they all recovered on the emetic tartar.

Observations on the Figures of Snow. By the Rev. Benj. Langwith, D. D. Rector of Petworth. N° 376, p. 298.

On Jan. 30, 1723, a little after 9 in the morning, weather cold, wind south-westerly, but not very high, barometer above 30 inches, I saw that pretty phenomenon of the star-like snow, and though on comparing my observations afterwards with those of Descartes, Dr. Grew, Mr. Morton, I find I have but little to add on the subject; yet as I observed the progress of nature, in this sort of crystallization, with a great deal of pleasure, I hope it will not be disagreeable to you to receive an account of it.

I shall begin with the most simple figures A and B, pl. 16, fig. 11, of which the former is a roundish pellet of ice; the second, a small oblong body, with parallel sides, which is often as fine as a hair. Of this latter kind the flakes of snow chiefly consist; and though they look white to the eye, yet when viewed with a small magnifier of a microscope, they appear like so many transparent needles of ice thrown together, without any order.

The next figure is c, in which the pellet has shot out 6 of those small bodies of equal length, and set at equal angles; of this kind I saw a considerable number.

The next step in the crystallization is D, in which those bodies are lengthened, and have shot out a great many more from their sides, at equal angles, but unequal lengths, growing continually shorter and shorter, till they terminate in a point. I measured some of these, and found them to be about one quarter of an inch in breadth. I saw but very few of them in perfection, for the collateral shoots were so exquisitely fine, as to be liable to be broken in their fall, or confounded together by the least degree of heat.

Of the next kind, E, I saw a very great number, which being examined by the microscope, plainly appeared to be nothing but the former in disorder. The edges of these were in general very irregular, but some of them happened to be so indented, as to look like the jagged leaves of plants.

The next kind, F, had twelve points regularly disposed, and probably might consist of two of the former so joined together, as to cut their angles equally.

Perhaps also those Mr. Morton describes, as consisting of radii, which, instead of terminating in a point, grow larger, as they advance from the centre, might be formed from two of the kind, C, so joined at the centre, as to cut each others angles unequally; for in the progsess of the crystallization, these radii would quickly unite.

Lastly, that sort which Descartes compares to roses, and of which he has given a figure in his Treatise of Meteors, may be nothing but the kind E, when the points are rounded off by being gently thawed.

I saw but very few figures of 12 points, and those mostly imperfect in one respect or other.

Observations for four Years on the Aurora Borealis, made at Lyn-regis in Norfolk. N° 376, p. 300. *Abridged from the Latin.*

These observations were made in the years 1718, 19, 20, 21, in the winter and spring months of these years, and were mostly of the usual kind. The more extraordinary ones, were 1st, that of Sept. 5, 1718, about 10 in the evening, when the appearance in the north was as shown in fig. 12, pl. 16. The next night, Sept. 6, between 8 and 10 o'clock, several columns of light were observed like those represented at aa in the same figure, but not so bright as the pyramids observed the night before, which were carried towards the east, but these toward the west. Dec. 19, between 8 and 9, several rays of light, with the aurora borealis, rose out of a seeming black cloud, though it was really no cloud, as the stars shone plainly through it, and the moon shone bright. The motions of the streams of light were very surprising, and extending to the zenith.

March 27, 1719, it was observed with various oblique radiations, as represented fig. 13.

Jan. 31, 1720, from 7 till 10, it was very high, and extended over half the heavens from east to west, and giving light enough to read by.

Jan. 6, 1721, between 7 and 8 o'clock, it was observed with pyramidal coruscations issuing on all hands from the zenith, as from a centre, and almost resembling an umbrella.

An Account of a Catadioptric, or Reflecting Telescope, made by John Hadley, Esq. F.R.S. With the Description of a Machine contrived by him for the applying it to use.* N° 376, p. 303.

The instrument consists of a metalline speculum, about 6 inches in diameter. The radius of the sphere, on which its concave surface was ground, is 10 feet $5\frac{1}{4}$ inches, and consequently its focal length is $62\frac{5}{8}$ inches The back has a hollow screw made at its centre to receive the end of a handle, which is screwed on, whenever the metal is to be moved, to avoid sullying its polished surface by handling.

This object metal A, fig. 1, pl. 17, is placed in one end of an octangular tube, BB, about 6 feet long, and something wider than what is sufficient to receive the metal, died black on the inside. About 6 or 7 inches in length of the three uppermost sides of the tube C, toward that end at which the metal is placed, are separated from the rest, and open with two hinges, to make room for the metal to be put in and taken out. The end of the tube is closed by an octangular piece of board D, which has an opening d, about $\frac{4}{5}$ of an inch broad, from the top down to a little below the centre, to give room for the beforementioned handle, when the object metal is lifted into or out of the tube;

* We have been able to find but very few biographical memorandums of this very respectable member of the Royal Society. On May 27, 1731, we find him offering to the Society an instrument he had invented for discovering the longitude. And again, in Sept. 1732, we find a memorandum, that by the assistance of Dr. Halley's exact calculation of the moon's motion, he had effectually discovered the longitude. His communications to the Royal Society are contained in the volumes of the Philos. Trans. from vol. 32 to 39, inclusively. He has been chiefly celebrated by the invention of his reflecting instrument for taking angles, called the Hadley's quadrant, offered to the Royal Society in 1731, by which his name is rendered immortal. Mr. Hadley died Feb. 15, 1744.

at other times it is closed with a sliding shutter. The metal is placed so, as to have its axis coincide with that of the tube, by the means of three small buttons fixed to the inside of the tube, having their hinder ends all in the same plane, to which this axis is perpendicular. Two of these appear at aa, the third, being at the middle of the bottom of the tube, is not seen. The foreside of the metal rests against these buttons in three points of its circumference, nearly equidistant from each other, and is held to them by three screws, one of which appears at b, which run through the octangular board at the end of the tube, and bear against the back of the metal in three points, which directly answer those three on the foreside, with just so much force as is requisite to keep it steady in its place.

The oval plane is composed of a plate of the same metal with the great speculum, about $\frac{1}{15}$ or $\frac{1}{16}$ of an inch in thickness, soldered on the back to another of brass. Its breadth is something less than half an inch, and is in proportion to its length as 1 to $\sqrt{2}$. At one end of the oval, the brass plate projects a little beyond the other, and has a screw cut through it in that part, as also another directly against the centre of the foreside. The other end is cyphered away on the backside, that it may intercept as few as possible of the rays, in their passage towards the object metal. The two screw holes in the back serve to fix this oval A, fig. 12, pl. 15*, to a brass arm B, which is fastened at the other end into a slider EE, fig. 11 and 12. This slider is of an equal thickness with the side of the tube, and has a groove GG, fig. 13, cut for it in that side, parallel to the axis, and long enough to give room for its motion, to set the two specula at the different distances, which the several eye-glasses require. It rests on the inside against two thin ledges, fastened within the tube along the sides of the groove. On the outside it is kept in its place by a sliding shutter, not expressed in the figure. In the middle it has a cylindric cavity D, fig. 12, whose axis is exactly perpendicular to its inner and outer surfaces. Each of the boxes, in which the eye-glasses are contained, is fitted to this cavity. The beforementioned brass arm is fixed into the inside of this slider, towards the end farthest from the object metal; it rises perpendicular for about 2 inches, and is made flat, so as to turn one edge to the rays which come from the object. About b, it is bent forwards and flatted the other way, so that when the back of the oval plane is held flat to it, by the two screws cc, the axis of the cylindric cavity may fall on the centre of its foreside, inclined to its surface in an angle of something less than 45°. This angle is brought to be exact by two very small screws ii, whose threads take hold in the flatted end of the brass arm, and their points bearing against the back of the oval, raise one end of it a little

* At line 21 of the opposite page, for fig. 1, pl. 17, read fig. 11, pl. 15.

from the flat of the arm. The specula are set at their due distance, by turning a long screw cc, for which there is a nut lodged in the slider at g; the screw is kept from moving backward or forward, when turned, by a brass plate, F, which is to be fixed to the flat end of the side of the tube, and taken off at pleasure. Each of the eye-glass boxes, H, has a screw on the outer end, to fasten to it a bowl or dish, I, to receive the ball of the eye, and guard it from external light.

On the top of the tube, on two small pedestals, is fixed a common dioptric telescope, H, fig. 11, about 18 inches long, its axis parallel to that of the tube; and having two hairs placed in the common focus of its object and eye-glasses, crossing each other in its axis.

There are three convex eye-glasses belonging to the instrument. The first, or shallowest, has its focal distance of about $\frac{1}{3}$ of an inch; the second of $\frac{3}{10}$; and the deepest of $\frac{11}{40}$, or something less. When the first of these is used with the instrument, it magnifies about 188 or 190 times in diameter; with the second, about 208; and with the third, 228 or 230. Each of these glasses has placed, in that focus nearest the oval, a circle to determine the part of the object seen at one view; and in the other focus toward the eye, a brass plate with a little hole in the middle, to let no light pass to the eye from the inside of the tube, but what comes from the oval. Besides these three convex, there are two concave eye-glasses, with which it magnifies about 200 and 220 times; and also a set of three convex, which turn it into a day telescope, magnifying about 125 times. The aperture is limited by a circle of card, or pasteboard, placed before the object metal in the tube. To vary the aperture, there are three of these circles, and the apertures allowed by them are $5\frac{1}{2}$ inches, 5 inches, and $4\frac{1}{2}$, though for some objects the whole metal may be left open.

The engine made use of to direct the tube to any object, consists of a strong plank, FF, fig. 11 and 13, about 14 inches wide, and $2\frac{1}{2}$ feet or 3 feet long, which serves as a foundation for the whole. Near one end of this plank is placed an upright foursided box, III, fig. 11 and 13, about 2 feet high, narrower at the back next the end of the plank than before; its two sides are mortised both into the plank below, aa, fig. 13, and into the top of the box above, dd, the back and fore part are fastened to the edges of the sides with wood-screws. The top has a circular hole cut in it, something above 3 inches in diameter, whose centre is about 3 inches distant from the outside of the back, and at an equal distance from the two sides. This hole gives passage to a turning pillar B, in the bottom of which there is fixed an iron pivot, c, to turn in a thick brass plate lodged in the plank, b. The upper end of the pillar rises about an inch and a half above the top of the box, and is mortised into a strong head, K, fig. 11 and 13, about 8 inches in length, and 4 or 5 in breadth and thickness. This head carries two cheeks, LL, about 13 or 14 inches in height, their hinder edges, towards the lower end, extending 5 inches beyond the axis of the pillar backward. Along the back of these cheeks, at equal distances above each other, there are notches, tending obliquely downwards, and answering each other in each cheek, to receive the pivots of a crooked iron axis, C, fig. 13, on which the tube is placed. The notches are made at different heights, to keep the eye-glass at a proper height for the eye, in different elevations of the object above the horizon. The figure of the axis answers that of the three under sides of the tube. The axis of the tube lies about $2\frac{1}{2}$ inches higher than the axis of the motion on these pivots, and the centre of gravity, when the object metal is in, is about 3 inches backwarder. To keep the tube from slipping back, when its fore end is raised, it has two buttons fixed to it, which rest against the fore part of the axis.

To keep the pillar from touching any of the sides of the round hole, in which it turns, a cylindrical sector, containing about 65° or 70°, and about an inch in height, is cut out on the back part of the pillar, near the upper end D. In the square angle of this cavity is fixed a thin steel plate, bent across the middle to the same angle. The internal angular edge, between the two parts of this plate, lies in the axis of the pillar, and turns on the hardened edge of a wedgelike iron, f, whose base, or board part, is fastened with two strong screws on the top of the box, directly behind the round hole beforementioned.

The upper parts of the cheeks are strengthened by two brackets, GG, leaving room between them for the bottom of the tube to touch the upper edge of the fore part of the head. The hinder part of the head is also hollowed, in the manner represented in the third figure.

The head on its fore part carries a flat arm, M, fig. 11, about 27 inches long, a little taper towards the farther end, where it is 4 inches broad. This is strengthened by a narrow slip, glued edgewise along the middle underneath, O, and also by a brace or stay, N, reaching from the turning pillar to within 9 inches of the end of the arm. The stay passes through a transverse opening cut in the fore part of the box, P, which is long enough to allow room for a sufficient motion of the pillar round its axis.

On the other end of the bottom plank, transversely to its length, is erected a board about 12 inches wide, and 26 or 27 high, Q, the top of it reaching within an inch and a half of the under side of the arm. This board is held firm in its position by a spur, R, part of its upper end on the outside is pared off toward the edges, to form it into the segment of a cylinder, whose axis coincides with that of the pillar. Its use is to support a rest, SS, on which the end of the flat arm moves backward and forward. This rest being applied transversely to the outer part of the upright board, where it is made cylindrical, is bent into the same figure, by means of four screw-pins, two of which passing through each end of this, and of another piece of the same length, T, but something narrower, placed over against it on the inside of the board, by their nuts draw them together, so as to grasp the end of the upright board between them; the upper edge of the rest being first shot with a plane very straight and smooth. To render the motion of the arm along the rest smooth and easy, it has two rollers lodged in a box fixed near the end, on its underside V, to roll on the edge of the rest, when the end of the arm is moved along i. One of the rollers is placed near each edge of the arm, and their axes lie in lines passing through the axis of the turning pillar. The rest is kept up to them, with a proper degree of force, by two screws, ww, which run in two plugs, xx, fastened on the sides of the upright board, and bear against the under sides of two pieces fixed on the inside of the rest.

The motion of the tube is governed by two brass pegs, Y and Z. The first of these, Y, is placed about 10 or 11 inches from the end of the arm, and has a line wound round it, which passing under a small pulley, f, fixed in a vertical position near the end of the arm, is fastened to a staple on the under side of the tube g. This line, by the turning of the peg, brings the fore end of the tube to its due elevation, being acted against by the excess of weight in the hinder end of the tube, when the metal is in it, which is equivalent to about 2 pound at g, where the line is fastened. In great elevations of the object above the horizon, the line is not carried so far as the point g; but is fastened a little above the pulley, to a light square stick, h, having at one end a hook, by which it takes hold of the staple g. This is done, that the springiness of the line may not continue a vibrating motion in the tube, when any thing happens to shake the instrument, and make the object appear to tremble. The lower part of the stick rests against the end of the arm, and by its slight friction contributes to the same effect.

The other peg, Z, is so placed, that it may be conveniently reached by one hand of the observer, while the other is employed about the peg Y; it regulates the horizontal motion of the tube, by means of a line, which being wound about the peg at one end, passes by another small pulley placed close by the side of the aforementioned one in a horizontal position, not to be seen in the figure, and is hung on a pin driven into the little head K. It is acted against by two springs, m and n, fig. 13, placed in the box, III, one on each side of the turning pillar; that on the right hand, m, draws the right side of the pillar forward, by a very strong line, which being fastened to the head of the spring, passes round the back part of the pillar to a pin, at P, by which it is strained to its due strength. The spring on the left hand, n, draws the left side of the pillar backwards in the same manner. These pins are placed on the pillar a little higher than the tops of the springs, that being drawn a little downwards, as well as turned round its axis, the pivot in its bottom may not be raised out of the hole in the brass plate, when the rest bears hard against the rollers at the end of the arm. Each of these springs draws with a force equal to about 18 or 20 lb. weight, when the end of the arm is carried close to the small head, k, fig. 11, and consequently, the semidiameter of the pillar being an inch and half, and the distance of that head from the axis about 28 or 29 inches, the end of the arm will be carried by the united forces of both the springs, towards the other end of the rest, with a force equivalent to the weight of about 2 lb. Each of the pegs, Y and Z, turns in a hole made in a piece of wood, l, fastened to the under side of the arm; and the pieces being slit with a saw from one end through the hole, and about half an inch beyond it, the separated parts are drawn together by a screw, m, till the end of the peg is griped between them with a due degree of force. By these pegs, with the help of the telescope H, the tube is easily directed to any object, and made to accompany a celestial one in its diurnal motion, while the end of the arm moves the whole length of the rest.

If it be desired, that when the object is found, the turning of one peg should carry the tube along with the motion of the heavens, so as to keep the object always in sight; this may easily be effected in various manners.

The open air has commonly an undulating motion in its parts, especially in the day time, which occasions the rays of light to deflect a little from the straight lines, in which they ought to move, in order to render the species perfectly distinct. The effect of this, though insensible to the naked eye, or even through a small telescope, becomes considerable, when the object is very much magnified. The instrument, when tried at an object inclosed, so as to secure it from this inconvenience, seems to bear an aperture of $5\frac{1}{2}$ inches, with the deepest of the forementioned eye-glasses, as well as the common telescopes do the usual charge and aperture given to them, except that in these the objects appear a little brighter.

Fig. 11, represents the instrument placed on the machine, to be applied to use. Fig. 12, represents the inside of the slider, with the rest of the apparatus belonging to the oval plane and eye-glass. Fig. 13, represents the hinder part of the machine, the back, and one side of the box, being taken away, to show the turning pillar and springs on the inside. Fig. 14, represents Saturn, as it appeared in June, 1720, by this telescope.

Dissection of a Person aged 109, *at Zurich, Feb.* 2, 1723. *By John James Scheuchzer of Zurich, M. D. Professor of Mathematics, and F.R.S.* N° 376, p. 313. *An Abstract from the Latin.*

John Leonhard Vopper, a Grison, was born May 1, 1614. In 1634, while working in a mine, he was buried by the falling in of a vein of ore, for the space of 33 hours; but was at length extricated, almost dead from the violent pressure on the abdomen. This accident was followed by an incontinence of urine. In 1637 he travelled into Hungary, Turkey, and the Holy Land, returning by Venice. After 1639 he engaged in the military life, and served in the Duke of Lorrain's army in the Milanese; in 1663 he served in Portugal; in 1682 he was at the siege of Vienna; afterwards at the siege of Landau; and at the battle of Hochstadt; yet, notwithstanding the hardships and dangers inseparable from the various situations of life in which he was placed, he attained the advanced age of 109 years and 3 months.

On opening the body after death, the following appearances were observed:

In the cavity of the abdomen there was found a small quantity of serum tinged with blood. Almost all the small intestines were inflamed; the duodenum, in particular, was exceedingly distended, and its inner surface was in a gangrenous state. The omentum was so much wasted, that it could scarcely be traced. The pancreas was contracted; the liver was in a sound state; the gall bladder full of bile; the ductus choledochus turgid with bile; and all the adjoining parts of the intestines and mesentery were tinged green with bile, of which there seemed to have been an extravasation; for its passage into the duodenum could not be traced. Near the pylorus, in the upper part of the stomach, there was a sort of flatulent tumor, larger than a walnut. The kidnies were in a sound state. The exterior membrane of the spleen exhibited a curious appearance, being beset with spots of a snowy whiteness, and of various sizes, resembling at first sight pustules of the small pox in a state of maturity; but proving, on further examination, to be of a cartilaginous hardness, and rising up a little above the surface of the rest of the membrane. [Here the author takes occasion to remark, that in consequence of the rigid and con tracted state of the fibres in advanced age, they possess very little sensibility; whence it happened that this person made no complaints of pain, though he died of a violent inflammation of the intestines, which in a young subject would have been accompanied with the greatest tortures.]

The opening of the thorax was a matter of much labour, owing to the ossified state of the cartilages. The surface of the lungs was thickly beset with green spots, and in both sides of the chest there were adhesions of the posterior part of the lungs to the pleura costalis. The pericardium contained a great quantity of serum, of which there was also a small effusion in the cavity of the thorax. The heart was large; its auricles were preternaturally distended, and both these and the ventricles were filled with coagulated blood. At the places of their insertion into the heart, the arteries were, if not ossified, at least cartilaginous. The semilunar valves were nearly of a cartilaginous hardness. The aorta descendens was exceedingly large, its diameter being twice that of the œsophagus. A number of indurated lenticular glands were seen on the inner surface of the œsophagus.

On examining the head, the substance of the cranium was found unusually dense, and the sutures nearly obliterated. The dura mater was twice the usual thickness, and resembled leather. The pia mater was easily separated from the brain, in consequence of an effusion of serum. The ventricles were full of serum; of which fluid there was also a considerable quantity collected at the basis of the brain. The plexus choroides was beset with glands of the size of peas, filled with coagulated lymph. The septum pellucidum was very conspicuous. The remainder of the substance of the brain was more flaccid than usual.

It is added, that the aforesaid Vopper had told many people that his father had 3 children after he was turned of 100; and that few of his ancestors had died before they were 100 years old. He himself had a daughter who was baptized at Diessenhoffen on the 18th of Aug. 1707.

An Account of the Coati Mondi of Brasil. By Dr. George Mackenzie. N° 377, p. 317.

The Coati Mondi of Brasil is seldom or never brought alive into Europe; yet 2 of them were found in Capt. Green's ship, a pirate; one of which died in my custody of a wound it received in the thigh, which I caused to be dissected; an account of which, compared with that which the Parisian academists published, is now sent. They differ in several particulars, most of which may proceed from the difference of sex, theirs being a male, and ours a female.

Theirs was $6\frac{1}{2}$ inches from the end of the snout to the hinder part of the head, ours was only 4; theirs was 16 inches from the occiput to the beginning of the tail, ours was 10; theirs from the insertion of the tail to the end was 13 inches, ours 12; theirs from the top of the back to the extremity of the fore feet was 10 inches, ours was 7; theirs from the top of the back to the extremity of the hinder feet was 12 inches, ours 8; the snout of theirs was very long and moveable, like that of a hog, but straighter and longer in proportion; but ours was only 2 inches; the 4 paws had each 5 toes, the claws of which were black, long and hollow, like those of the castor; the toes of the fore-paws were a little longer than those of the hind-paws; the soles without hair; the palms and soles of these fore-paws were covered with a soft and tender skin; the sole of the hinder paw was long, having a heel, at the extremity of which there were several scales a line broad, and 5 or 6 long, in all which they perfectly agreed.

The ears were round, like those of rats, and covered at the top with very short hairs, and in this they likewise both agreed, as they did in the eyes, which were very small and beautiful, but there was some difference in the hair; for theirs was short, rough and knotty, blackish on the back and head; and the rest of the body mixed with black and red; but in ours the hair was long, in proportion to the animal, especially on the tail, and the whole was beautified with white and black circles, which made it have a most lovely aspect: but from the snout, down all the throat and belly to the top of the tail and the inside of the legs, was of a reddish colour. The tongue of both was chopped with several fissures or strokes, which made it rough to the touch. The incisores were 6 in each jaw: the canini were very large, especially those of the lower jaw; but they did not turn up like tusks as theirs did; their figure was not round, blunt, or white, like those of a dog, wolf, or lion, but sharp, by means of 3 angles, which at the extremity formed a point sharp like an awl. Their colour was greyish, and somewhat transparent: the gula was large, and cleft like a hog's; and the lower jaw, as in a hog, much shorter than the upper.

By the dissection we found in ours, as the Parisian academists did in theirs, that under the skin, and between the muscles, there was a great deal of fat, white and hard, like tallow. Theirs, being a male, had a penis provided with a bone, whose length in proportion greatly exceeded that of the bones in the penis of other animals; so we in ours, being a female, observed, that it had an exceedingly large matrix, and that the insertion of the urethra was on the right side of the vagina. The epiploon in ours, as in theirs, was very small; it had little fat, and was a complication of fibres and fillets, rather than a membrane; it was not laid on the intestines, but touched on the ventricle. In theirs was a very large spleen, but in ours we could discern none. We did not observe, more than they, any vessels in the external membrane of the ventricle, but the coronaria stomachica, which appeared as in theirs towards the upper orifice, and soon disappeared, shooting forth a few branches. The liver in ours, like theirs, was somewhat blackish, and of a substance very homogeneous, without any appearance of glands: it had 7 lobes, 2 great ones on the left side, and 5 other small ones on the right side. The pancreas in ours, as in theirs, was fastened along the duodenum, inclining more towards the right kidney than the left; but though very small in theirs, it was very large in ours. The mesentery in ours, as in theirs, was filled with a very hard fat, which inclosed and almost concealed all its vessels. The intestines in theirs were 7 feet long, and all of one thickness, having nothing to distinguish them; but in ours they were only 42 inches and a half. Theirs had no cæcum, but we found it in ours, at the upper end of the rectum. The bladder was very large; the right kidney in ours, as in theirs, was a great deal higher than the left, and covered with the lobes of the liver. The lungs in theirs had 5 lobes, 2 on the right side and 2 on the left, and the 5th in the mediastinum, which was as thin as a spider's web: but in ours there were 7 lobes, 3 on the right and 3 on the left, and the 7th in the middle. The heart in ours, as in theirs, resembled that of a dog, having the right auricle exceedingly large, and as they found a great deal of slimy matter hardened in the right ventricle, so we found in ours a polypus. The musculus crotophites passing under the zygoma was in ours, as in theirs, fastened there, being very fleshy, even to its insertion, made by a very large tendon, which was inclosed between 2 pieces of flesh, much thicker than those which are generally found in this place, and which are thought to be put there to defend and strengthen the tendon of the muscle of the temples.

The tendons in the articulations of the fore feet were very large and strong. In ours we observed 2 glands on each side of the anus, with a passage to each of them, full of a greyish fœtid matter. The orbita in ours, like theirs, was not bony throughout, but it was supplied in the upper part by a cartilaginous ligament, which joined the apophysis of the os frontis to that of the first bone in the upper jaw. The bone which separates the cerebrum from the cerebellum was like that in dogs. The dura mater in ours did not adhere to the cranium, as in theirs. The sinuses of the os frontis in ours, as in theirs, was full of matter, like a friable fat. The mammillares processus, in ours, as in theirs, were very large. In the eye both of them agreed exactly, the globe not exceeding $4\frac{1}{2}$ lines in diameter, the aperture of the lids being much larger, and the pupilla being as large as the whole globe of the eye; the crystalline contained 3 lines in breadth, and $2\frac{1}{2}$ in thickness, and was more convex inwards than outwards; this thickness of the crystalline made the two other humours to be less in quantity. The choroides was all over of the same colour, viz. of a very brown red, without any tapetum, which is hardly ever wanting in the eyes of other animals.

I believe the academicians are misinformed, in saying that they carry their tails erected, at least the tail of this was always trailing on the ground; neither can I be induced to believe that they eat their tails, for there was no part of her that she could endure less to be handled than her tail, the least touching of which would make her cry, or rather hiss like a snake; she could endure no manner of cold; for in the intervals between the times of eating, she was either beneath the bed-clothes, or on a cushion before a fire, with the heat of which she seemed to be extremely well pleased.

Her ordinary meat was buttered eggs, milk, and bread, all manner of roasted flesh, but no fish; I once tried her with a new killed partridge, which she eat of most voraciously, and for several days after she was very wild and ungovernable, which made me never afterwards try her with raw flesh. I am apt to believe their ordinary dens or habitations are under ground, in sandy banks, like rabbits; for when she was brought to the fields she would dig up the sand with her paws, with an incredible swiftness, so that had she not been chained, there had been no possibility of recovering her.

An Account of the voiding of a great number of Stones during the use of the Pyrmont Waters, by a Person who had never before been troubled with Calculous Complaints. Communicated by Ab. Vater, M. D. Professor of Anatomy and Botany, at Wittemberg, and F. R. S. Dated Jan. 6, 1723. N° 377, p. 322. *An Abstract from the Latin.*

A Pomeranian nobleman, who at that time enjoyed perfect health, was prevailed upon to accompany a friend to the Pyrmont waters. After he had been taking these waters for some days, he voided several stones inter mingendum, without any pain. This induced him to drink the waters in larger quantities; the consequence of which was, that 4 or 5 days afterwards, more than 40 came from him, without giving him any uneasiness. This circumstance encouraged him to persist in the use of these waters, which he took in excessive quantities; the consequence of which was, a suppression of urine for 3 days, for which the catheter was employed. After this he discontinued the waters by the advice of his physicians, and he flattered himself that he should not experience any further bad effects from them. But not long after, while he was travelling homewards, he was troubled with a constant desire to make water, which obliged him frequently to get out of his carriage, when pure blood would be voided, accompanied with excruciating pain. This symptom continued during the whole of his journey; but after his return home, on keeping quiet, both the bloody urine and pain ceased. But, whenever he rode out either in his carriage or on horseback, this complaint returned, and again ceased on keeping himself quiet. Sometimes, but very rarely, it would happen that a few stones would be voided without any pain, such stones being much smaller than those which came away during the use of the Pyrmont waters. After he had been afflicted with this complaint (bloody urine) for 2 years together, it was at length entirely removed by the help of physic. He then discontinued the use of medicines, thinking them no longer necessary: but from that time he has been constantly troubled with strangury. The urine, which is voided in very small quantities with extreme pain, is of a ropy quality and thick consistence, like paste made from flour and water. The patient nevertheless enjoys a good appetite, sleeps sufficiently, and feels no pain in any other part of the body.

In his reflexions upon this case, Dr. V. supposes that the stones which this patient voided, had not been formed previously to the use of the Pyrmont waters; but were produced ex croco minerali, which those waters contain.

Observations on dissecting the Body of a Person troubled with the Stone. By Dr. Perrot Williams, of Haverford West. N° 337, p. 326.

Mr. William Bowen, of the town and county of Haverford West, aged between 40 and 50, having been for about 7 years severely afflicted with the usual symptoms of the stone in the kidnies and bladder, viz. bloody urine after exercise, strangury, &c. died in May 1722. On opening the body there were found in the bladder 6 smooth oval stones, exactly of the same figure, and nearly of the same magnitude: there were also 3 cells in each kidney, the figure of each corresponding to that of the stones: the ureters were so preternaturally distended, as very easily to admit the largest of the stones to pass from the kidney to the bladder. The other viscera appeared in their natural state.

Dissection of a Man who died of the Stone in the Kidnies. By P. Hardisway, M. D. N° 337, p. 327. *Translated from the Latin.*

A man, about 70 years of age, was seized, on the 19th of October 1722, with a violent pain of the bowels, accompanied with vomiting and dysury. The pain was so excessive, that he was obliged to keep his body bent; he lay groaning before the fire day and night. On the 6th day of the attack there was a total suppression of urine. He then complained of a painful tightness about the hypochondria, as if a rope was tied round him. He afterwards complained of a distressing sensation of weight in the bladder, as if (to use his own expression) a large turnip was within it. He died on the 12th day of this attack.

On opening the body, no traces of calculi were discovered in the bladder; but in each of the kidnies there was found a remarkable rough stone, which was distributed by ramifications through the parenchyma, in such manner that it was impossible to disengage it without tearing the substance of the kidney to pieces. The largest of the branches insinuated itself into the head of the ureter, which it fitted like a stopple to a bottle. This patient was a poor man of an athletic habit of body. Before this attack he had enjoyed good health, except that during the last few years he had now and then voided some gravel.

An Account of the Depth of Rain fallen from April 1, 1722, *to April* 1, 1723. *Observed at Widdrington in Northumberland. By the Rev. Mr. Horsley.* N° 377, p. 328.

I have kept an exact account of what rain has fallen the last year in this place. Weighing the water, and reducing it from weight to depth, seemed pretty troublesome, even when done in the easiest method: to remedy this inconvenience, besides a funnel and proper receptacle for the rain, I use a cylindrical measure and gauge. The funnel is 30 inches diameter, and the cylindrical measure exactly 3 inches; the depth of the measure is 10 inches, and the gauge of the same length, with each inch divided into 10 equal parts; or, instead of a gauge, the inches and divisions may be marked on the side of the cylindrical measure. The apparatus is simple and plain, and it is easy to apprehend the design and reason of the contrivance; for the diameter of the cylindrical measure being just $\frac{1}{10}$ of that of the funnel, and the measure exactly 10 inches deep, it is plain that 10 measures of rain make an inch in depth; one measure, $\frac{1}{1000}$; one inch on the gauge, $\frac{1}{100}$ and $\frac{1}{10}$, of an inch on the gauge, $\frac{1}{1000}$, &c. By this means the depth of any particular quantity which falls may be set down with ease and exactness, and the whole at the end of each month, or every year, may be summed up without any trouble.

An Account of the Depth of Rain fallen from April 1, 1722, *to April* 1, 1723.

	Inches.
In April	1.015
In May	3.532
In June	2.570
In July	4.350
In August	2.132
In September	1.155
In October	.600
In November	2.205
In December	1.780
In January	1.225
In February	.485
In March	.195
In the whole year	21.244

Two General Propositions of Pappus of Alexandria, in which many of Euclid's Porisms are included, restored by Rob. Simson, Professor of Mathematics at Glasgow, from the Preface to the Seventh Book of Pappus's Mathematical Collections.* N° 377, p. 330.

These two propositions were afterwards incorporated into the author's large posthumous works, published in 1776 by Earl Stanhope, on the same subject, and where they may be read by lovers of this science with more advantage.

* Dr. Robert Simson, professor of mathematics in the university of Glasgow, was born at West-Kilbridge, in Ayrshire, 1687. He was entered a student of that university about 1701, where his proficiency in several branches of literature was soon remarkable. Particularly botany and other branches of natural history were his chief pursuits, which led his attention to the medical science, in which he afterwards took a doctor's degree, though be never practised. In prosecution of this object he went to Leyden, were he studied for some time under the celebrated Boerhaave.

On his return to Scotland, he was induced by his friends to relinquish medicine, and turn his views to the church. About this time however mathematics began to engage his mind, and at length even to the exclusion of his theological studies. And a prospect soon after fortunately opened of rendering this study his peculiar profession. On the resignation of Dr. Robert Sinclair, he was elected professor of mathematics by the university of Glasgow in 1711, at 24 years of age.

Soon after this appointment he came to London, to acquire more enlarged views in regard to the objects of his profession, for that purpose putting himself under the immediate direction of Mr. Ditton, a mathematician of considerable reputation, and master of the mathematical school in Christ's Hospital. Here he commenced an acquaintance with Dr. Halley, and other eminent men of the R. S. by whose advice Dr. S. early directed his attention to the restoration of the works of the ancient geometricians; yet the first specimen of his talents for this difficult study, was the above paper in the Philos. Trans. for 1723; though he proceeded from this time in compiling several other similar works, which however were not published till many years after; and he never communicated more than one other paper to the Philos. Trans. After a long life, spent in the severe study of the pure and ancient geometry, rather indeed to the neglect of the modern analysis, he died in 1768, at 81 years of age.

Dr. Simson's works published in his life time, were, 1. Conic Sections, in 1735. 2. The Loci Plani of Apollonius, in 1749. 3. Euclid's Elements, in 1756. After his death, Earl Stanhope was at the expence of a publication of several of his posthumous pieces; viz. 1. Apollonius's Determinate Section: 2. A treatise on Porisms: 3. A tract on Logarithms: 4. On the Limits of Quantities and Ratios: 5. Some Select Geometrical Problems.

Besides the tracts published in those posthumous works, Dr. Simson's manuscripts contained a great variety of geometrical propositions, and other interesting observations on different parts of the mathematics; though not in a state fit for publication. Among other things however, was an edition of the works of Pappus, in a state of considerable advancement, and which, had he lived, he probably would have published. The copy of Pappus, with all Dr. Simson's notes and explanations, it seems were, soon after his death, sent by his executor to the university of Oxford, with a view to publication; but which however it does not appear has yet been accomplished. It is true, Dr. Simson's copy contains a large collection of materials, among which to make a proper selection would probably require considerable labour, as well as judgment.

On the Magnitude of the Blood-globules, &c. By M. Leuwenhoeck, F. R. S. N° 377, p. 341. *Abstracted from the Latin.*

Dr. Jurin having explained a method of determining with certainty the diameters of minute objects; and particularly that the diameter of a blood globule was equal to the 1940th part of an inch; M. Leuwenhoeck reasoned in this manner: if the diameters of 1940 blood globules be equal to 1 inch; and spheres being in proportion as the cubes of their diameters, it follows that the cube of 1940, or 7,301,384,000 globules, are only equal in bulk to a globe of 1 inch in diameter.

Some Amendments and Additions to the Account of Things found under Ground in Lincolnshire. By Mr. Ralph Thoresby, F. R. S. N° 377, p. 344.

As in N° 279 there is some difference between the accounts of the depth of the things found; the one accounting it to be about 8 or 10 feet deep, and the other 12 or 14; it is to be observed that the depth was not measured, but only estimated according to the relator's best remembrance. But the difference may easily be accounted for by supposing, which will not be far from truth, that when the labourers first discovered the jetties and other things there, it might be about the depth of 8 or 10 feet; but the bottom of them, when they came to be all taken up, might be at the depth of about 12 or 14 feet, as in the other account.

It is also to be noted, that some judicious persons affirm, that the stones which the spectators saw at the bottom of Hammon Beck, were such as the dikers had first thrown out, on taking up the old goat, and were fallen in again. But that it was a hard and firm soil is certain; and probably that on which the famous steeple of Boston stands. See the record of the foundation of the said steeple in N° 223 of the Transactions.

The form of the shoe soles found at Spalding, was as represented fig. 14, pl. 16; each foot had its proper shoe, this being for the right foot. By some passages in history, it may probably be conjectured when those shoe soles were left there, and how long since that atterration began in that part of Lincolnshire.

In Stow's Chronicle, An. 1465, we read of a proclamation against the beaks or pikes of shoone, or boots, that they should not exceed 2 inches, on penalties there mentioned. And by other passages in history it appears, that those pikes of shoes were before that time exceedingly long, and held up by chains, that they might not hinder the wearer's walking; which chains were sometimes of silver, if not of gold, that they might be rich, as well as ornamental.

Again, in Melchior Adamus's Life of Conrad Pellican, at the bottom of p. 263, 8vo. edit. there is a passage to this purport: "At that time, viz. in 1484, the soldiers, returning from Flanders, introduced several novelties; as, party-coloured stockings, square-toed shoes, both men and women before that time wearing them piked or sharp-pointed; as also a new kind of square-toed sandals, called *pantofflen*, which superseded the use of wooden shoes, *holtzschuh:* so that these novelties became very fashionable."

Hence it appears, that it is not much above 200 years since those shoes, above described, were worn; and consequently it cannot be much longer since the earth has been raised there to the thickness beforementioned, viz. of 10 or 12 feet; and since Bicker Haven grew up to be, as at present, higher land than the country on each side of it. By which may be conjectured, what a change a century or two more may make in the out-falls of the rivers of Witham, Welland, Nyne and Ouse; and consequently the necessity of taking some other method for preventing the impending mischiefs, which threaten the navigation of the said rivers, and those who have estates and interests in the great level of the fens, and are concerned in the draining of them.

An Account of a monstrous double Birth in Lorrain. By Mons. Fevry. Communicated by Mr. Colin Mac Laurin, F. R. S. N° 377, p. 346.

On Dec. 31, 1722, M. Fevry, surgeon in ordinary to the Duke of Lorrain, went by his orders to Domp Remy la Pucelle, to see one Sebastiana Camus, aged 44, delivered on the 24th of the same month, about 8 o'clock in the evening, of 2 children joined together. There was one head, one neck, one breast, one abdomen, and 2 hands on one side; and as many parts on the other; the whole being well proportioned and plump, joining together by the belly, which was common to both; so that one of the heads was in the place where the other's feet should be, and the other head in its natural place; they had but 2 legs for them both, which seemed to arise from the transverse apophyses of the vertebræ of the loins on one side; and from the opposite region of the loins, came out a leg ending with a joint bending forwards, and at the extremity forming a small stump, like a finger, articulated by ginglymus. There was but one fundament for both, by which they voided their fæces; they had but one navel-string, and the parts proper to the female sex also single: they eat and drink with their mouths severally, and while the breast was given to one, the other cried for it: they sleep and are awake, sometimes both at the same time, sometimes separately. Each of these children were baptized: one of them was plumper than the other, which is more puny, and not so fresh-coloured. The head of the one, which was a little larger than that of the other, came first to the birth, the 2 arms lying on the breast followed next; the legs lay on the sides of the breast of the second; the opposite leg, which is single, was extricated afterwards; last of all, the arms of the 2d child, being ranged on the side of its head, made it easy for the rest to come out. The bodies of both these children made no more in bulk than that of one ordinary child.

It is observable that the mother could assign nothing that had had any relation to this event, during the time of her pregnancy.

By another account, communicated to the Royal Society, these children lived 2 months after the birth.

Observations and Experiments on the Sal Catharticum Amarum, commonly called the Epsom Salt. By Mr. John Brown, Chemist, F. R. S. N° 377, p. 348.

In this paper, and in the continuation inserted in the following number of the Transactions, it is stated that the liquor which remains after the crystallization of common salt from sea-water, and which liquor is called *bittern*, is at Lymington in Hampshire, conveyed by channels into pits made tight with clay, where it stands for some months, and there will shoot again : the liquor which remains is boiled down, till it is observed to be in a disposition to crystallize, and then is conveyed into wooden coolers lined with lead; the liquor, which will not shoot there, is boiled down after the same manner, for another crystallization. By this time the liquor seems to have altered its property, and becomes of a very pungent biting taste, and, if boiled down, will no longer shoot into crystals as before, but precipitates, during the boiling, a small grained salt; and if you for experiment sake should continue to boil down the liquor, separated from this salt, each quantity of salt thus produced, will still be more pungent than the other. If you boil down the whole quantity of this liquor, it will produce a salt, which, if exposed to the air, will run per deliquium. But as this salt is not the business of our present inquiry, it may probably be the subject of another paper. The liquor, that produces this salt, is always flung away, wherever the sal catharticum is made.

This is what, at present, I can give no other name to, than a third salt produced from the sea-water, differing, in some respects, as much from the other two, as they differ from each other.

To return to the several crystallizations, such as mentioned to be shot from the bittern ; these will be of different sizes, as to their figures, and hold some share of the third salt but now taken notice of, which makes them apt to give and dissolve; nor is their taste come yet to that simple bitter of the pure salt. These therefore are either separately, or altogether, to be flung into a copper, with as much common water as is sufficient to dissolve them, and allow of a gentle evaporation, till they are again ready to be poured into the coolers for crystallization. This generally proves to be the pure sal catharticum, thoroughly freed, as far as the experiments I have tried can be convincive, from either a sea salt, or the third salt. The liquor decanted from this shooting, may be boiled down again, for a second shooting, and after that a third; but as the liquors from these shootings are boiled away more or less, so you will sooner or later meet again with the pungent liquor, which contains the third salt, as in the former shootings from the bittern, which the pure sal catharticum is as necessarily required to be freed from, as from the common salt; a proof of which cannot be better determined than by one of the experiments to be taken notice of hereafter, viz. that with the ol. vitriol. which will certainly ferment with this salt, if the sea salt has not been well separated from it, or if it still holds some of the third salt. And when any of the crystallizations will not stand the test of this experiment, they ought to be dissolved and shot again, as before, by which means the pure salt is to be obtained. I do not mention this as a trial made use of at the salt-works, but what I have by experience found to be true. And the same experiment will serve to distinguish a sal mirabile made at these works, from that made with ol. vitrioli and common salt. The account they give of it is this. They take any quantity of coarser grained crystals, boiled from the bittern, which when dissolved and evaporated, more than they would otherwise do for making the sal catharticum, they throw into a wooden bowl, with some oil of vitriol, where it stands for 10 days, and shoots into large crystals, transparent, and like the sal mirabile: but as this salt, by this method, is not sufficiently satiated with the ol. vitriol. if they use any, so it is easily discovered by the ol. vitriol. which will readily ferment with it; whereas it has no effect on the other sal mirabile made as above.*

* The liquor, termed *bittern,* which remains after the crystallization of common salt from sea-water, consists, for the most part, of muriate of magnesia. It is therefore necessary to add the sulphuric acid in some form or other to the bittern, in order to obtain from it the sal catharticum amarum (Epsom salt) which is a sulphate of magnesia. But this circumstance is not duly noticed in Mr. Brown's communication. The late Dr. Donald Monro (Treatise on Chemistry, Vol. i. p. 200) mentions that at his desire a gentleman inquired of the manufacturers of this salt at Lymington, "whether they added any of the vitriolic [sulphuric] acid, or any substance containing it, to the bittern from which it was prepared. They said that in general they put none, though they owned that they sometimes did; but they did not mention to him the circumstances which at times induced them to make that addition." In all probability these manufacturers did not choose to explain the whole of their process.

A Collection of Geometrical Flowers, presented to the Royal Society. By Guido Grandi, Abbot of the Camaldules, and Professor of Mathematics at Pisa. N° 378, p. 355.

This handful or bouquet of geometrical roses, is a dissertation on certain curves geometrically described in a circle, of a nature more curious and fanciful, than any way useful. This paper the author afterwards enlarged in another separate treatise published in 1728, entitled, Flores Geometrici ex Rhodonearum, &c. where the subject may better be consulted in its improved state.

Concerning Observations made with Mr. Hadley's Reflecting Telescope. By the Rev. Mr. James Pound, Rector of Wanstead, F. R. S. N° 378, p. 382.

It were to be wished, that, with the particular description given in the Transact. N° 376, of the curious mechanism of that catadioptric telescope which was made by Mr. Hadley, and presented to the Royal Society, he had communicated also a full account of what observations he had made with it, that the public might have been apprised of the usefulness of an invention, worthy of its great author, Sir Isaac Newton, which, perhaps from the vain attempts made by some of putting it in practice, has lain neglected these 50 years; the time since which it was first published in the Philos. Transactions, (N° 81.)

Mr. Hadley has sufficiently shown, that this noble invention does not consist in bare theory; and it is to be hoped that he, or some other such curious persons, will in a short time find out a method, either of preserving the concave metal from tarnishing, or of clearing it easily when tarnished, or else of making a good concave speculum of glass quicksilvered on the back part. When a method for either of these shall be discovered, it is not to be doubted, but that the old dioptric telescope will be for the most part laid by, and this catoptric one will be chiefly in use among practical astronomers; as several inconveniencies and difficulties, which are unavoidable in the management of the former, especially when long, are in the latter entirely avoided.

It is no small convenience, that by means of one of these reflecting telescopes, not exceeding 5 feet in length, and which may be managed at a window within the house, celestial objects appear as much magnified, and as distinct, as they do through the common telescope, of more than 100 feet in length.

Mr. Bradley, Savilian professor of astronomy, and myself, have compared Mr. Hadley's telescope, in which the focal length of the object metal is not quite $5\frac{1}{4}$ feet, with the Hugenian telescope, the focal length of whose object-glass is 123 feet: and we find, that the former will bear such a charge, as to make it magnify the object as many times as the latter with its due charge; and that it represents objects as distinct, though not altogether so clear and bright; which may be occasioned partly by the difference of their apertures, that of the Hugenian being somewhat the larger, and partly by several little spots in the concave surface of the object metal, which did not admit of a good polish.

Notwithstanding this difference in the brightness of the objects, we were able, with this reflecting telescope, to see whatever we have hitherto discovered by the Hugenian; particularly the transits of Jupiter's satellites, and their shadows over his disk; the black list in Saturn's ring; and the edge of his shadow cast on his ring, as represented by fig. 14, pl. 15.

We have also seen with it several times the 5 satellites of Saturn; in viewing of which, this telescope had the advantage of the Hugenian, at the time when we compared them; for being in summer, and the Hugenian telescope being managed without a tube, the twilight prevented us from seeing in this, some of those small objects, which at the same time we could discern with the reflecting telescope.

Observations on the Satellites of Jupiter and Saturn, made with the same Telescope. By John Hadley, Esq. F.R.S. N° 378, p. 385.

Mr. Hadley gave the Society an account of some of the most remarkable observations he had made with his reflecting telescope, before he presented it to the Society.

In observing Jupiter's satellites, he has seen distinctly the shadows of the first and third satellites cast on the body of the planet; Mr. Folkes and Dr. Jurin, being present, affirmed, that Mr. Hadley had likewise shown them the shadow of the third satellite through the same telescope.

In observing Saturn, when that planet was about fifteen days past the opposition, he saw the shadow of the planet cast on the ring, and plainly discerned the ring to be distinguished into two parts, by a dark line, concentric to the circumference of the ring. The outer or upper part of the ring seemed to be narrower than the lower or inner part, next the body, and the dark line, which separated them, was stronger next the body, and fainter on the outer part towards the upper edge of the ring. Within the ring he discerned two belts, one of which crossed Saturn close to its inner edge, and seemed like the shadow of the ring on his body; but when he considered the situation of the sun, in respect to the ring, he found that belt could not arise from such a cause.

He says, that at times he has seen with this telescope 3 different satellites of Saturn, but could never have the good fortune to see all the 5.

Aug. 1723, Mr. Hadley adds, that he has several times seen the shadow of the first, second, and third satellites of Jupiter pass over the body of that planet; and that he has seen the first and second appear, as a bright spot on the body of Jupiter; and has been able to keep sight of them there for about a quarter of an hour, from the time of their entering on his limb.

Jupiter's satellites have of late years been so situated, with regard to the earth and Jupiter, that he has not had sufficient opportunity of observing the transit of the fourth satellite, or of its shadow.

The dark line on the ring of Saturn, parallel to its circumference, is chiefly visible on the ansæ, or extremities of the elliptic figure, in which the ring appears; but he has several times been able to trace it very near, if not quite round; particularly in May 1722, he could discern it without the northern limb of Saturn, in that part of the ring, that appeared beyond the globe of the planet. The globe of Saturn, at least towards its limb, reflects less light than the inner part of the ring, and he has sometimes distinguished it from the ring by the difference of colour.

The dusky line, which in 1720 he observed to accompany the inner edge of the ring across the disk, continues close to the same, though the breadth of the ellipse is considerably increased since that time.

An Account of an Extra-Uterine Fœtus, taken out of a Woman after Death, that had continued Five years and a half in the Body. By Robert Houstoun, M.D. N° 378, p. 387.

A woman near Newport-market, who had been married 18 years to a native of the East Indies, by whom she had 8 children, besides two miscarriages, was in August 1717, with child in a second marriage.

She was near her full time, and had felt pains for several days, which, returning by intervals, she concluded would, as usual, bring on her delivery. Her mother and the midwife, apprehending no difficulty, assured those about her, that time only was wanting.

But it was found on examination, that her womb was of no bulk to contain a child near its time; and that its neck, of an uncommon hardness, was also closed so straitly, as not to admit even of a small probe or knitting needle.

Dr. H. on this declared that her delivery was impossible; because the child was not within the womb, but between the womb and the guts: that it might be removed by a passage to be made for it, without any great pain, and with safety to the mother. He offered to undertake it, and assured them that this was the only opportunity; and that if she neglected it, it would hereafter be out of the power of art to give her the relief; she must languish till death, unless favoured by some unlikely and extraordinary accident.

This advice, however, was not complied with. A year after this, Dr. Houstoun was again desired to visit her. He found her much disordered by a growing imposthumation in her belly: he ordered her some cordial stomachics, cassia, and gentle lenitives; and they succeeded beyond his expectation: so that by aid of a regular diet, and the watchful exactness of a very tender mother, and a nurse of above 30 years experience, he restored her to such strength, that she went cheerfully abroad, and again applied herself to business.

But about 15 months from the time when he visited her first, her mother came again to intreat his assistance: she complained of great pain in the lower part of her abdomen; and he found a tumour of a conical form, projecting about an inch beneath the umbilicus: its inflammation with tension, and a feverishness attending it, so plainly indicated suppuratives, that he was not surprized to hear, in a few days, that it had broken.

He proposed to lay it open, both to give a free emission, and prevent its becoming fistulous; but she was apprehensive, that he would, as she called it, cut open her belly: so that not being able to prevail with her, he ordered an unguent, and some plasters.

The ulcer soon grew fistulous, and so continued till she died, which was on the 23d of March last, in the 41st year of her age.

For above 5 months before her death, she voided her excrements by this vent, and all the soft parts of the fœtus, with some small bones of its fingers. But the rest of the skeleton remaining entire, Dr. H. took it out of her body, together with the vagina, uterus, rectum, &c. wherein it had involved itself. She was full 9 months gone in August 1717, and she died the 23d of March, 1723, on which day Dr. H. took the skeleton out of her body.*

An Account of a Roman Inscription, found at Chichester. By Roger Gale, Esq. F. R. S. N° 397, p. 391.

This inscription, fig. 15, pl. 16, as curious as any that has yet been discovered in Britain, was found at Chichester, in digging a cellar under the cor-

* In the original, this account is accompanied with an engraving, which, however, is here purposely omitted, not only in consequence of the bad representation it exhibits, but also because the description is sufficiently intelligible to medical readers without it.

ner house of St. Martin's-lane, on the north side as it comes into North-street. It lay about 4 feet under ground, with the face upwards, which received considerable damage from the tools of the labourers as they endeavoured to raise it; for besides the defacing of several letters, what was here recovered of the stone was broke into 4 pieces: the other part of it, still wanting, is, in all probability, buried under the next house, and will not be brought to light till that happens to be rebuilt. The inscription is cut on a grey Sussex marble, of 6 Roman feet in length, as may be conjectured by measuring it from the middle of the word TEMPLUM to that end of it which is entire, and is not altogether 3 English feet from the said point. Its breadth is $2\frac{3}{4}$ of the same feet, the letters beautifully and exactly drawn, those in the first two lines 3 inches long, and the rest $2\frac{1}{4}$.

Being at Chichester in September last with Dr. Stukely, we took an accurate view of this marble, which is now fixed in the wall under a window within the house where it was found; and that we might be as sure of the true reading as possible, wherever the letters were defaced, we impressed a paper with a wet sponge into them, and by that means found those in the fifth line to have been as we have expressed them above, and not as in other copies that have been handed about of this inscription.

The only letter wanting in the first line is an N before EPTVNO, so there is no difficulty in reading that. As to the second, though it was more usual in inscriptions of this nature to express the donation by the word SACRVM only, referring to the temple or altar dedicated; yet we have so many instances in Gruter's Corpus Inscriptionum of TEMPLVM and ARAM also cut on the stones, that there is not the least occasion to say any thing further upon that point.

The third line can be no other way filled up, than as I have done it by the pricked letters: I must own, however, that I have had some scruple about the phrase of DOMVS DIVINA, the same thing as DOMVS AVGVSTA, the imperial family, which I cannot say occurs, with any certainty of the time it was used in, before the reign of Antoninus Pius, from whom, down to Constantine the Great, it is very frequently met with in inscriptions.

The third line I believe was EX AVCTORITATE. TIB. CLAVD. and the fourth COGIDVBNI. R. LEG. &c. that is, ex auctoritate Tiberii Claudii Cogidubni Regis, Legati Augusti in Britannia; for the following reasons: we are informed by Tacitus in Vita Agricolæ, that after Britain had been reduced to a Roman province by the successful arms of Aulus Plautius, and Ostorius Scapula, under the emperor Claudius, quædam civitates Cogiduno regi erant donatæ, is ad nostram usque memoriam fidissimus remansit, vetere ac jam pridem recepta populi Romani consuetudine ut haberet instrumenta servitutis

& reges. This Cogidunus seems to be the same person as Cogidubnus in our inscription, the letter B in the third syllable making little or no difference in the word, especially if pronounced soft, as it ought to be, like a V consonant.

It is so well known to have been the custom of the Roman liberti and clientes, to take the names of their patrons and benefactors, that it would be wasting of time to prove the constant usage of that practice. Now as this Cogidubnus, who probably was a petty prince of that part of the Dobuni which had submitted to Claudius, and one that continued many years faithful to him and the Romans, had received the government of some part of the island by that emperor, nothing could be more grateful in regard to Claudius, nor more honourable to himself, after he was Romanized, than to take the name of a benefactor to whom he was indebted for his kingdom, and so call himself TIBERIVS CLAVDIVS COGIDVBNVS.

The sixth line has lost at the beginning the letters COLLE, but so much remains of the word as makes it to have been indubitably; when entire, COLLEGIVM, and the following letters are an abbreviation of FABRORVM.

These colleges of artificers were very ancient at Rome, as ancient as their second king Numa Pompilius, if we may believe Plutarch, who tells us, that the people were divided by him into what we at this day call companies of tradesmen, and mentions the Τέκτονες or Fabri among them; though Florus says, that populus Romanus a Servio Tullio relatus fuit in censum, digestus in classes, curiis atque collegiis distributus. But as the power of the Romans extended itself, it carried the arts of that great people along with it, and improved the nations it subdued, by civilizing, and teaching them the use of whatever was necessary or advantageous among their conquerors, from which most wise and generous disposition, among other beneficial institutions, we find these collegia to have been established in every part of the empire, from the frequent mention of them in the inscriptions collected by Gruter, Spon, and other antiquaries.

Several sorts of workmen were included under the name of Fabri, particularly all those that were concerned in any kind of building; whence we meet with the Fabri Ferrarii, Lignarii, Tignarii, Materiarii, Navales, and others; the last named may have been the authors of dedicating this temple to Neptune, having so near a relation to the sea, from which the city of Chichester is at so small a distance, that perhaps that arm of it which still comes up within two miles of its walls, might formerly have washed them. The rest of the fraternity might very well pay the same devotion to Minerva, the goddess of all arts and sciences, and patroness of the Dædalian profession.

As no less than five letters are wanting at the beginning of the sixth line, there cannot be fewer lost at the beginning of the seventh, where the stone is more broken away than above; so that probably there were 6 when perfect. What we have left of them is only the top of an s; perhaps it was A. SACR. S. a sacris sunt; perhaps it was HONOR. S. honorati sunt: as to the former, we find these collegia had their Sacerdotes, therefore qui a sacris sunt, which is found in inscriptions, would be no improper term to express them; or it might have been SACER. S. sacerdotes sunt, since we find such mentioned in other inscriptions.

So that the vacuity in our inscription may very well have been filled up with one or other of these words, and the three next letters that follow them D. S. D. de suo dedicaverunt, will agree with either of them, and what precedes them.

The last line has been PVDENTE PVDENTINI FIL I O; but there must have been a letter or two of the prænomen at the beginning of it, unless it was shorter than the rest at that, as well as at the latter end of it; and from what has been said, the whole may be read as follows.

Neptuno & Minervæ templum pro salute domus divinæ, ex auctoritate Tiberii Claudii Cogidubni regis, legati Augusti in Britanniâ; collegium fabrorum, & qui in eo a sacris [or] honorati sunt, de suo dedicaverunt, donante aream Pudente Pudentini filio.

Chichester, by this inscription found at it, must have been a town of eminence very soon after the Romans had settled here, and in process of time seems to have been much frequented, by the Roman roads, still visible, that terminate here from Portsmouth, Midhurst, and Arundel, though, what is very strange, we have no Roman name now known for it. I once thought it might have put in its claim for Anderida, which our antiquaries have not yet agreed to fix any where, being situated very near, both to the sylva anderida, and the southern coast of the island, the two properties of that city: but Henry of Huntingdon, who lived in the time of Henry the Second, telling us, that the Saxons so destroyed Andredecester, that nunquam postea reædificata fuit, & locus tantum quasi nobilissimæ urbis transeuntibus ostenditur desolatus, it could not be Chichester; for that was not only rebuilt before his time, but was a place of such note, that when the bishops, soon after the conquest, removed their churches from small decayed towns, where several of them were then seated, in urbes celebriores, Stigand, then bishop of Selsey, settled his episcopal chair at that place.

When this inscription was dug up, there were also two walls of stone discovered close by it, 3 feet thick each, one running north, the other east, and joining in an angle, as the North-street and St. Martin's-lane now turn, which probably were part of the foundations of the temple mentioned on the marble.

De Structura Diaphragmatis. Epistola Antonii van Leuwenhoeck, R.S.S. Delphis, 31 *Maii,* 1723. N° 379, p. 400.

The diaphragm like other muscular parts, is composed of fleshy, and tendinous fibres, with interposed cellular membranes, besides blood-vessels, &c. The appearance of these fibres and interposed membranes, as exhibited by the microscope in other muscular parts, having been described and figured in some of Mr. L.'s former communications inserted in the present collection, it is deemed unnecessary, as it would only lead to repetition, to give a translation of this letter, concerning the structure of the diaphragm.

Partium Genitalium in Muliere Structura præternaturalis. Ex Epistola Johannis Huxham, M. D. ad Jacobum Jurin, R. S. Secr. Plimutho,* v° *Kalend. Octob.* 1723. N° 379, p. 408.

A. B. de parochia Lanteglass in comit. Cornubiæ, prope Fowye oppidum, annos nata xxiii nupta fuit cuidam nautæ robusto, tandemque prægnans, sibi

* Dr. John Huxham holds a distinguished rank among the medical writers of Great Britain. He practised at Plymouth, where he kept a register of the weather and of the prevailing diseases, for a series of years. The result of these labours he published under the title of Observationes de Aere et Morbis Epidemicis, forming 2 Vols. 8vo.; the first of which appeared in 1739, and the other in 1752. They contain some good descriptions of diseases, with practical remarks on the method of treatment. He afterwards published an Essay on Fevers, subjoined to which is a dissertation on the malignant ulcerous sore throat, of which Dr. Fothergill had given an account some years before. He concurs with the last-mentioned physician, in asserting that bleeding and purging are extremely prejudicial in this disorder. In this Essay on Fevers, he has described with much accuracy the symptoms of that insidious form of typhus, which he denominates *the slow nervous fever.* He admonishes strongly against the evacuating mode of treatment, recommending in its stead, the employment of " temperate, cordial, diaphoretic medicines, and a well-regulated supporting, diluting diet.' Thus he opened the way to a grand improvement in the curative treatment of fevers of this description. His tincture of bark, in which the medicinal virtues of the Peruvian drug are heightened by combination with bitters and aromatics, has long since become a standard preparation; and few persons, we imagine, will contend that, as a cordial medicine adapted to certain states of typhoid fever, the character given of it by its author, has been much over rated. In pneumonia, and other inflammatory disorders, he was in the habit of prescribing small and frequently repeated doses of antimonial wine, prepared by dissolving vitrified antimony in white wine. This he thought superior to every other preparation of antimony; but at the present time the preference is given to the vinous solution of tartarized antimony, termed in the London Pharmacopœia, vinum antimonii tartarisatum, and in the Edinburgh, vinum tartritis antimonii.

Besides the works above-mentioned, Dr. H. sent various communications to the Royal Society, which are inserted in the Philos. Trans. from Vol. 32d to Vol. 42d inclusively. These communications relate to anatomy, medicine, astronomy, meteorology, natural history, &c. They prove that he possessed a cultivated and enlarged understanding, joined with a philosophic turn of mind, and a talent for observation, which could occasionally be exercised on a great variety of subjects.

partium male conformatarum conscia, opem imploravit chirurgicam. Sibi accersitum esse voluit dominum Bonnett de Fowye, oppido, & artis chirurgicæ, et obstetriciæ peritissimum. Ille vero perspecto denudato corpore sequentia observavit:

Loco umbilici, in medio abdomine, paululum vero inferius, prominet massa quædam spongiosa, carnis quasi offam præ se ferens, abdomini transverse incumbens, magnitudinis fere ovi gallinacei, longa autem tres digitos: ex hac duo erumpunt meatus urinarii exiles, qui urinam perpetim exstillant, quam neque vel retinere vel ejaculare potest; hinc vesicam, (si quæ adsit) sphinctere caruisse concludendum. Spongiosa hæc mollisque massa, urinæ acrimonia corrosa, tactum fere refugit mollissimum, adeo ut incurvata obambulare coacta sit, ad dolorem a vestibus impressum evitandum, eamque tenuissimis linteis involvere. Hanc quidem massam funem fuisse umbilicalem male a partu abscissum, male dein curatum, existimo; ne vel minima enim umbilici, nisi hic, apparent vestigia; quid quod et urina per urachum pervium, et in duos forsan tubulos divisum vel saltem per canales duos proprios, effluit. Priori potius credo opinioni; quia dantur historiæ urinam per umbilicum in adultis etiam excretam fuisse attestantes. Vid. Hist. de l'Academie Royale des Sciences, Ann. 1701.

Hanc infra offam subintrat vaginæ foramen, ab offa distans brevissimo spatio: ex hoc effluxere catamenia; per hoc etiam gravida facta fuit mulier; haud magnam vero tempore coitus percepit voluptatem; summa etiam penis glans in hoc orificium vix fuit intromissa, multo minus ipsa virga. In hoc foramen digitum ægre introduxit chirurgus, eo scilicet animo, ut ipsum uteri collum exploraret, quod tamen ne vel tactu percepit, plane autem deprehendit membranam crassam hocce orificium ab altero inferiore, jam describendo, separantem.

Eo fere ipso loco, superius vero paululum, ubi in mulieribus rite conformatis adest fossa magna, inventum fuit foramen alterum oblongum, ne vel minimi digiti apicem altius admittens, recto intestino, uti post partum observatum fuit, pervium (quod a sectione fortasse accidit:) nullus autem hic occurrit sphincter: inferius vero rectum intestinum, more solito, cum sphinctere circumdato terminatur.

Orificium hoc oblongum ab orificio vaginæ, abdomine maxime tumente, duos saltem digitos transversos distitit, inter quæ membrana descripta intervenit ab interiore parte; hujusce vero fissuræ oblongæ quasi labiorum coalitio exterius e superiore foraminis parte.

Nulla hic clitoris, hic nulla ossium pubis adfuere vestigia, nisi, quasi apophyses breviores ex utriusque ossis ilii parte inferiore protuberantes, ossium pubis rudimenta dixisses. Hic fuit ante partum rerum status.

Die 18 Julii 1722, hora noctis 11ª advocatus est Bonnett chirurgus, ut parturienti opem ferret. Perpensis omnibus, fœtum invenit vaginæ orificium infra dilapsum, quem muliere decumbente superiora versus propellere frustra adnixus est, ob fortissimos fœtus motus et gravissimos matris dolores, cui etiam jam supervenere convulsiones, syncope, &c. Vaginæ autem orificium vix ac ne vix quidem dilatatum, ita ut illi jam jam moriendum esse fuerit omnium expectatio.

In hoc miserrimo rerum statu, misericordia et humanitate adductus chirurgus, posthabuit omnino, quid vel ignarum vulgus, vel invidus hic garriret aut hic: anceps experiendum esse remedium potius quam nullum apud se statuit, morte aliter citissimo ingruente pede.

Parentibus periculosissimum rerum statum enarravit, nil nisi a sectione expectandum, ancipitem prædixit eventum. Annuentibus hujus miserrimæ matre, et astantibus, in orificium oblongum inferius scalpellum chirurgicum introduxit, et uno ictu coalitionem labiorum hujusce orificii et membranam separantem divisit: hinc in unum coivere et orificium vaginæ et oblongum inferius. Jam digitis facilis introitus, orificium uteri internum attrectavit; dilatavit paululum, caput inde sensit infantis: quid plura? fœtus ori intruso digito, puellulam vivam, probe formatam, mirantibus maxime astantibus, demum extraxit, quæ et adhuc et viget et valet: uti etiam et ipsa mater, quanquam post puerperium febre graviter colluctata est.

Jam a partu prolapsu ipsius uteri divexatur, quo prolabente nec per horas 8, 10ve reducto, eam dein si fortius intrudas, exiliunt e meatibus urinariis descriptis rivuli duo ad pedis saltem distantiam; argumentum sane cystidis cujusdam urinam excipientis: aliter revera suspicassem meatus istos duos urinarios ipsorum ureterum fuisse orificia, hic terminantia.

Quaerat fortasse curiosior aliquis, quo forte modo gravida facta fuit muliercula nostra. Illi responderem, penis intrusionem ad prolem concipiendam haud absolute esse necessariam, seminis autem intra vaginam ejaculationem quam maxime. Vid. Hist. de l'Acad. Royale des Sciences, 1712. Videatur etiam Mauriceau.

Mulieris ejusdem Historia, ex Epistola Medici Doctissimi, Gulielmi Oliver, aa Richardum Mead, M. D. F. R. S. N° 379, p. 413.

Another account of the preceding case. The former description by Dr. Huxham being sufficiently circumstantial, it is deemed unnecessary to insert this also.

An Account of a remarkable Hæmorrhage from the Penis; in a Letter from Dr. Howman to Sir Hans Sloane, Bart. President of the College of Physicians, and Vice President of the Royal Society. N° 379, p. 418. *Translated from the Latin.*

A citizen of Norwich, aged 40, at a time when he was in perfect health, was seized with a hæmorrhage from the urethra, on the 30th of June; and again on the 31st of July. By the use of some medicine and bleeding in the arm, the hæmorrhage was stopped until the 8th of Sept.; after which it entirely ceased. It is worthy of remark, that these discharges of blood were not preceded by any pain, nor followed by any depression of spirits.

An Account of the Pits for Fullers Earth in Bedfordshire. By the Rev. Mr. B. Holloway, F.R.S. N° 379, p. 419.

Several fullers-earth pits are open at Wavendon near Woburn, where the earth is disposed in much the same manner in all of them.

From the surface, for about 6 yards in depth, there are several layers of sands, all reddish, but some lighter coloured than others, under which there is a thin stratum of red sand-stone, which they break through; and then for the depth of about 7 or 8 yards more, you have sand again, and after that come to the fullers-earth; the upper layer of which, being about a foot deep, they call the cledge; and this is by the diggers thrown by as useless, being too much mixed with the sand which covers, and has insinuated itself among it; after which they dig up earth for use, to the depth of about 8 feet more, distinguished into several layers, there being commonly about a foot and a half between one horizontal fissure and another. Of these layers of fullers-earth, the upper half, where the earth breaks itself, is tinged red, as it seems by the running of water from the sandy strata above; and this part they call the crop; between which and the cledge, abovementioned, is a thin layer of matter not an inch thick, in taste, colour, and consistency, not unlike to terra japonica. The lower half of the layers of fullers-earth, they call the wall-earth; this is untinged with that red abovementioned, and seems to be the more pure and fitter for fulling; and underneath all is a stratum of white rough stone, of about 2 feet thick, which, if they dig through, as they very seldom do, they find sand again, and then is an end of their works.

It is observable in the site of this earth, that it seems to have every where a pretty equal horizontal level; or when the sand ridges at the surface are higher, the fullers-earth lies proportionably deeper.

In these works they seldom undermine the ground; but as they dig away the earth below, others are employed to dig and carry off the surface; otherwise the matter above, being of so light a nature, would fall in and endanger the workmen; for as that stratum of sand-stone, which occurs before they come to the fullers-earth, does not lie, as in coal-pits, immediately over the matter they dig for, like a ceiling, but even in the midst of the superjacent strata of sand, and therefore can be no security if they undermine.

The perpendicular fissures are frequent, and the earth in the strata, besides its apparent distinction into layers, like all other kinds of matter, by reason of its peculiar unctuousness, or the running of the adjacent sand imperceptibly among it, breaks into pieces of all angles and sizes.

These pits lie in that ridge of sand-hills near Woburn, which near Oxford is called Shotover, on which lies Newmarket heath by Cambridge, and which extends from east to west every where, at about the distance of 8 or 10 miles from the Chiltern hills, which in Cambridgeshire are called Gog-magog; but in Bucks and Oxon, the Chiltern hills, from the chalky matter, of which they chiefly consist; which two ridges you always pass in going from London into the north, north-east, or north-west counties. After which you come into that vast vale, forming the greater part of the midland counties of Cambridge, Bedford, Bucks, Northampton, Oxford, and Gloucester.

An Invitation for making Meteorological Observations. By Dr. James Jurin, R. S. Secr. N° 379, p. 422. *Abridged from the Latin.*

Dr. Jurin states that the changes in the weather, especially when great or sudden, have much influence on the health of mankind; for which reason philosophers, even in the 17th century, invented various instruments, by which were ascertained the several degrees and changes in the weight, heat, moisture, and elasticity of the atmosphere. They endeavoured also to discover the causes of these changes, adding several observations on the weather, the face of the sky, the winds, and quantity of rain.

Were this done more generally, and the observations compared, we should have a still more perfect history of the air. In general, the sudden changes in the weather are chiefly to be attributed to the winds; hence then we should have some means of evincing the cause of the winds, and in particular to determine the truth or falsehood of Dr. Halley's opinion, in N° 181, who thinks that the ascent of the mercury in the barometer is owing to the winds blowing towards the same place from opposite points, thus collecting and accumulating the air; as, on the contrary, that its descent is caused by the winds carrying the air from the same place, towards the opposite parts, and thus exhausting it as it were.

Further to improve this part of natural history therefore, Dr. Jurin recommends the curious to mark in their diary, once a day at least, the height of the barometer and thermometer, the course and strength of the wind, the face of the heavens, the rain or snow, as also the observations with the microscope and the magnetical needle.

Dr. J. gives directions for chusing, filling, and using barometers, thermometers, and rain-gauges, &c. He recommends, for the sake of comparison, that all observations be made at the same hour of the day, noting the weight, heat, and moisture of the air, by the barometer, thermometer, and hygrometer; the point of the winds, and their strength, denoting the several degrees by the numbers 1, 2, 3, 4; the face of the heavens, a short account of the weather, with the depth of rain or dissolved snow, in inches or decimals. This last may be easily estimated by means of a funnel, 2 or 3 feet wide, with another vessel to receive the water from it, and a cylindrical measure with a gauge, divided into inches and decimal parts. The situation of the funnel should be such, that whatever wind blows, no part of the rain may be intercepted, either by the intervention of the house, or any other obstacle. The vessel to be close shut every way, that no water may evaporate, having only a small hole to receive the water from the funnel above.

At the end of every month and year, let the mean height of the barometer and thermometer in each be subjoined; as also the sum of all the depths of the rain, fallen in the whole month or year, the mean height being found by dividing the sum of all the heights by the number of the days or observations.

Such persons as may be pleased to make the observations, are desired to send copies of them for each year to the secretaries of the Royal Society, that they may be compared with the diary kept in London, by order of the Society, it being proposed, that the comparisons and inferences shall be published every year in the Philos. Trans.

Account of a Book, entitled, Adversariorum Anatomico-Medico-Chirurgicorum Decas tertia. Auctore Frederico Ruysch, M. D. Anatom. et Botan. Prof. Amstel. R. S. Soc. N° 379, p. 428.

Ruysch's works are now so generally known, that it is deemed unnecessary to reprint this account of the contents of the 3d decade of his Adversaria.

Concerning Stones voided per Anum. By Mr. David Martineau, Surgeon at Norwich. N° 380, p. 433.

These stones were voided by a poor woman per anum, on the 26th, 27th, and 28th of March, 1723, being then pregnant about the 11th or 12th week. On the 23d, she seemed in the extremity of a convulsion fit, attended with violent vomitings, which when over, she complained of great pain in her back, from her reins downward to the anus. She continued with much pain the whole day; and on the 24th, in the night, her fits returned with double force, her pains also increasing like labour pains. Glysters were attempted, but none could be thrown up, though repeated by some of the most experienced nurses; on which Mr. M. gave her a gentle draught, which she observed increased her pain with a strong tenesmus, that continued near 3 hours before the largest stone appeared; after some hours she voided 2 more stones; and the next day, at 4 hours distance, other 2. She recovered perfectly, and was delivered of a very fine live girl, on the 24th of August following. She had been frequently troubled, for 14 years past, with pains in her side and stomach, without vomitings.

The size of the stones, their seat and substance, are what seem worthy of speculation; their being all alike in colour and weight, according to their dimensions, is the reason Mr. M. broke but one.

	Wt. of the stones.			Their dimensions in	
	oz.	dwts.	gr.	circumf. inch.	length inch.
1.	2	16	12	8	$6\frac{5}{8}$
2.		8	12	$5\frac{1}{8}$	$4\frac{5}{8}$
3.		7	3	5	$3\frac{5}{8}$
4.		7	12	$4\frac{6}{8}$	$3\frac{7}{8}$
5.		5	13	$4\frac{3}{8}$	$4\frac{1}{8}$

De Globulis in Sanguine et in Vini Fœcibus. Epistola posthuma Domini Antonii a Leuwenhoeck, Societatis Regiæ Londinensis, dum viveret, Sodalis dignissimi, ad Jacobum Jurin, R. S. Secr. N° 380, p. 436.

In this and the following letter of Mr. Leuwenhoeck's there is nothing sufficiently interesting for translation. In the concluding part of the second letter Mr. L. gives an account of an experiment, in which, during an attack of what he terms a palpitation of the diaphragm, he inspired air through a glass tube filled to the height 3 transversorum digitorum with spirit of wine impregnated with nutmeg, mace, cloves, and other aromatics, whereby the inordinate action was for that time quieted; but on repeating it another time the experiment failed.

Ejusdem Viri Clarissimi ad eundem Epistola posthuma. De Generatione Animalium, et de Palpitatione Diaphragmatis.* N° 380, p. 438.

A Botanical Description of the Flower and Seed Vessel of the Plant, called Crocus Autumnalis Sativus, that produces the true English Saffron of the Shops. By Dr. James Douglas, F. R. S. N° 380, p. 441.

This plant being so well known, the botanical description given by Dr. Douglas is at present of no importance.

Some Account of Mr. Leuwenhoeck's curious Microscopes, lately presented to the Royal Society. By Martin Folkes, Esq. V. P. R. S. N° 380, p. 446.

It is more than 50 years since the late Mr. Leuwenhoeck first began his correspondence with the Royal Society, when he was recommended by Dr. De Graaf, as a person already considerable by his microscopical discoveries, made with glasses contrived by himself, and excelling even those of the famous Eustachio Divini. And as he has ever since that time applied himself, with the greatest diligence and success, to such observations, no doubt can be made of the excellency of those instruments he so long used, so much improved, and on the fullest experience so often commended in his letters; great part of which, at his decease, he thought fit to bequeath to this Society.

The legacy consists of a small Indian cabinet, in the drawers of which are 13 little boxes or cases, each containing two microscopes, handsomely fitted up in silver, all which, not only the glasses, but also the apparatus for managing them, were made by Mr. L's own hands; besides which, they seem to have been put in order in the cabinet by himself, as he designed them to be presented to the Royal Society, each microscope having had an object placed before it, and the whole being accompanied with a register of the same, in his own handwriting, being desirous the gentlemen of the Society might easily examine many of those objects, on which he had made the most considerable discoveries.

* A full account of Mr. L's discovery of animalcula in semine masculino, and of his theory of generation founded thereon, has been inserted in some of the preceding vols. of this abridgment.

The 13 cases abovementioned are numbered from 15 to 27 inclusively, corresponding to which is the register of the objects, two to every case, as follows:

N° 15. Globules of blood, from which its redness proceeds.

A thin slice of wood of the lime-tree, where the vessels conveying the sap are cut transversely.

N° 16. The eye of a gnat.

N° 17. A crooked hair, to which adheres a ring-worm, with a piece of the cuticle.

A small hair from the hand, by which it appears those hairs are not round.

N° 18. Flesh of the codfish, showing how the fibres lie oblique to the membranes.

An embryo of cochineal, taken from the egg, in which the limbs and horns are conspicuous.

N° 19. Small pipes, which compose the elephant's tooth.

Part of the crystalline humour, from the eye of a whale.

N° 20. A thread of sheep's-wool, which is broken, and appears to consist of many lesser threads.

The instrument, whence a spider spins the threads, that compose his web.

N° 21. A granade, or spark, made in striking fire.

The vessels in a tea-leaf.

N° 22. The animalcula in semine masculino, of a lamb taken from the testicle, July 24, 1702.

A piece of the tongue of a hog, full of sharp points.

N° 23. A fibre of codfish, consisting of long slender particles.

Another of the same.

N° 24. A filament, conveying nourishment to the nutmeg, cut transversely.

Another piece of the same, showing the figure of the vessels.

N° 25. Part of the tooth abovementioned, consisting of hollow pipes.

An exceedingly thin membrane, being that which covered a very small muscle.

N° 26. Vessels by which membranes receive nourishment and increase.

A bunch of hair from the insect called a hair-worm.

N° 27. The double silk, spun by the worm.

The organ of sight of a fly.

It were endless to enter into any particulars of what is to be observed in any of these objects, or indeed to give any account of Mr. L's discoveries, they are so numerous as to make up a considerable part of the Philos. Trans. and when collected together, to fill 4 pretty large volumes in quarto, which have been published by him at several times; and of such consequence, as to have opened entirely new scenes in some parts of natural philosophy, as in that famous discovery of the animalcula in semine masculino, which has given a perfectly new turn to the theory of generation, in almost all the authors that have since written on that subject.

The construction of these instruments is the same in them all, and the apparatus is very simple and convenient; they are all single microscopes, consisting each of a very small double convex glass, let into a socket, between two silver plates rivetted together, and pierced with a small hole; the object is placed on a silver point, or needle, which, by means of screws of the same metal, provided for that purpose, may be turned about, raised, or depressed, and brought nearer, or put farther from the glass, as the eye of the observer, the nature of the object, and the convenient examination of its several parts may require.

Mr. L. fixed his objects, if they were solid, to this silver point, with glue; and when they were fluid, or of such a nature as not to be commodiously viewed unless spread upon glass, he first fitted them on a little plate of talk, or excessively thin blown glass, which he afterwards glued to the needle, in the same manner as his other objects.

The observation indeed of the circulation of the blood, and some others, require a somewhat different apparatus, and such an one he had, to which he occasionally fixed these same microscopes; but as it makes no part of this cabinet, I shall omit giving any farther account of it, only taking notice that it may be seen in a letter to the Royal Society, of the 12th of January, 1689, and printed in his Arcana Naturæ Detecta, N° 69.

On the late Queen Mary's visiting Mr. L. at Delft, and viewing his curiosities with great satisfaction, he presented her with a couple of his microscopes, which it seems were of the same sort as these, and no ways differing from one of the 13 cases contained in the drawers of this cabinet.

The glasses are all exceedingly clear, and show the object very bright and distinct. Their powers of magnifying are different, as different sorts of objects may require; and as on the one hand, being all ground glasses, none of them are so small, and consequently magnify to so great a degree, as some of those drops frequently used in other microscopes; yet on the other, the distinctness of these very much exceeds what are met with in the glasses of that sort; and this was what Mr. L. ever principally proposed to himself, rejecting all those degrees of magnifying in which he could not so well obtain that end; having found in a long course of experience, that the most considerable discoveries were to be made with such glasses as, magnifying but moderately, exhibited the object with the most perfect brightness and distinctness.

The Bills of Mortality, &c. of several considerable Towns in Europe, from Christmas 1716 *to Christmas* 1717. *Extracted from the Acta Breslaviensia. By Conr. Joach. Sprengell, M. D. R. S. S. et Coll. Med. Lond. Lic.* N° 380, p. 454.

Breslaw, in Silesia.—Buried, 1485; Christened, males 584; females 576. Among the buried were, married men 226; married women 144; widows and widowers 157; bachelors 60; maidens 57; boys (under ten years) 419; girls (under ten years) 397; boys (stillborn) 37; girls (stillborn) 17.

In Vienna there died, young people 3179; old 2026;—total 5205. Among which were, of 90 years old, two; of 91, one; of 92, three; of 93, one; of 94, three; of 95, two; of 96, three; of 99, two; of 100, one; of 102, two; of 103, one; of 104, one; of 115, one. Born and christened, 4030 children.

In Dresden, Saxony, died 1908; christened 1443.

In Dantzic, died 1605; christened 2102.

In Esperies, or Eperies, alias Epperies, a town in Upper Hungary, died 132; christened 157; most of these died of the small pox.

The Bills of Mortality from Christmas 1717, *to Christmas* 1718.—Breslaw, buried 1255; christened males 598; females 554. Among the dead were, married men 238; married women 141; widows and widowers 122; bachelors 60; virgins 60; boys (under ten years) 282; girls (under ten years) 280; boys (stillborn) 47; girls (stillborn) 25.

In Vienna died, men 1432; women 1129; boys 1844; girls 1705. In all 6110.

Among the dead were found, eight persons of 90 years old; seven of 92; two of 93; three of 94; three of 95; one of 96; two of 97; two of 98; two of 99; one of 105. Christened, males 2185; females 2057; total 4242. Amongst which were 48 pair of twins, and once tergemini.

In Ratisbon died of the Lutherans, 235 persons; amongst which were 117 children, 13 young men, and 11 young women.

List for several Cities in Saxony.

	Christened.	Bastards.	Buried.	Pair Married.	Communicants.
In Dresden	1578	99	1412	501	78875
Wittemberg	286	16	317	61	13536
Leipzic	861	68	953	303	
Torgau	136	9	148	54	7917
Freyberg	340	13	373	104	24098
Stollberg	88		112	27	7314

	Christened.	Bastards.	Buried.	Pair Married.	Communicants.
Pulsnitz	73		75	39	4381
Konigsbruc	67		95	16	3351
Elstra	63		45	11	3021
Bautzen	207	5	135	55	14520
Annaberg	146	19	112	37	8426
Chemnitz	173	4	166	33	10690
Oschatz	103	3	82	20	5169
Altenburg	141		248	57	12901
Eulenberg	96	5	86	41	6194
Pirna	138	2	133	43	9164
Marienberg	91		194	18	5903
Georgenstad	182		191	36	3580
Harta, near Waldheim	40	3	29	20	3291

A List for a whole Century, of the Numbers of People Married, Christened, Buried, and Communicants, viz. from the Year 1617, *to* 1717 *inclusive, in the Electoral City of Freyberg.*

	Pair Married.	Christened.	Bastards.	Buried.	Communicants.
Total	7546	28851	582	30295	1211761.

It is to be observed, that in the years 1625, 26, 27, 32, and 33, the plague raged very much; as also, that in the year 1630, there died of the plague merely, 764, and in 1680, 103.

The following list is from Berlin, consisting of all that were born, married and buried in Prussia, in the 4 years, 1715, 1716, 1717, 1718.

		Born.	Pair Married.	Buried.
In the kingdom of Prussia	Sum.	82712	18331	47503

The next list contains the numbers of all that were born, married and buried, in the rest of the king of Prussia's dominions in Germany, &c. for the same 4 years.

		Born.	Pair Married.	Buried.
In the electorate of Brandenburg	Sum.	64533	18559	48508
In the Newmarck	Sum.	24196	7125	19001
In the duchy of Magdeburg	Sum.	31481	6490	22619
The duchy of Cleve	Sum.	29209	7907	23207
The duchy of Pomerania	Sum.	30721	8843	22182
The principality of Halberstad	Sum.	10224	2948	8359
The county of Hohenstein	Sum.	2383	625	1715

		Born.	Pair Married.	Buried.
The principality of Minden	Sum..	7322....	2237....	6352
The city of Meurs	Sum..	1769....	648....	1366
In Geldern	Sum..	4043....	986....	2130
The county of Ravensberg	Sum..	9254....	2708....	8104
The county of Tecklenberg	Sum..	1907....	637....	1813
The county of Lingen	Sum..	2730....	836....	2411
In Lauenburg and Butow	Sum..	2312....	584....	1469
In the French colonies	Sum..	1210....	317....	1203

The sum total of all that were born in 4 years	306006
.... of all that were married	81881 Pair
.... of all that were buried	217942
More born than buried, in number	88064

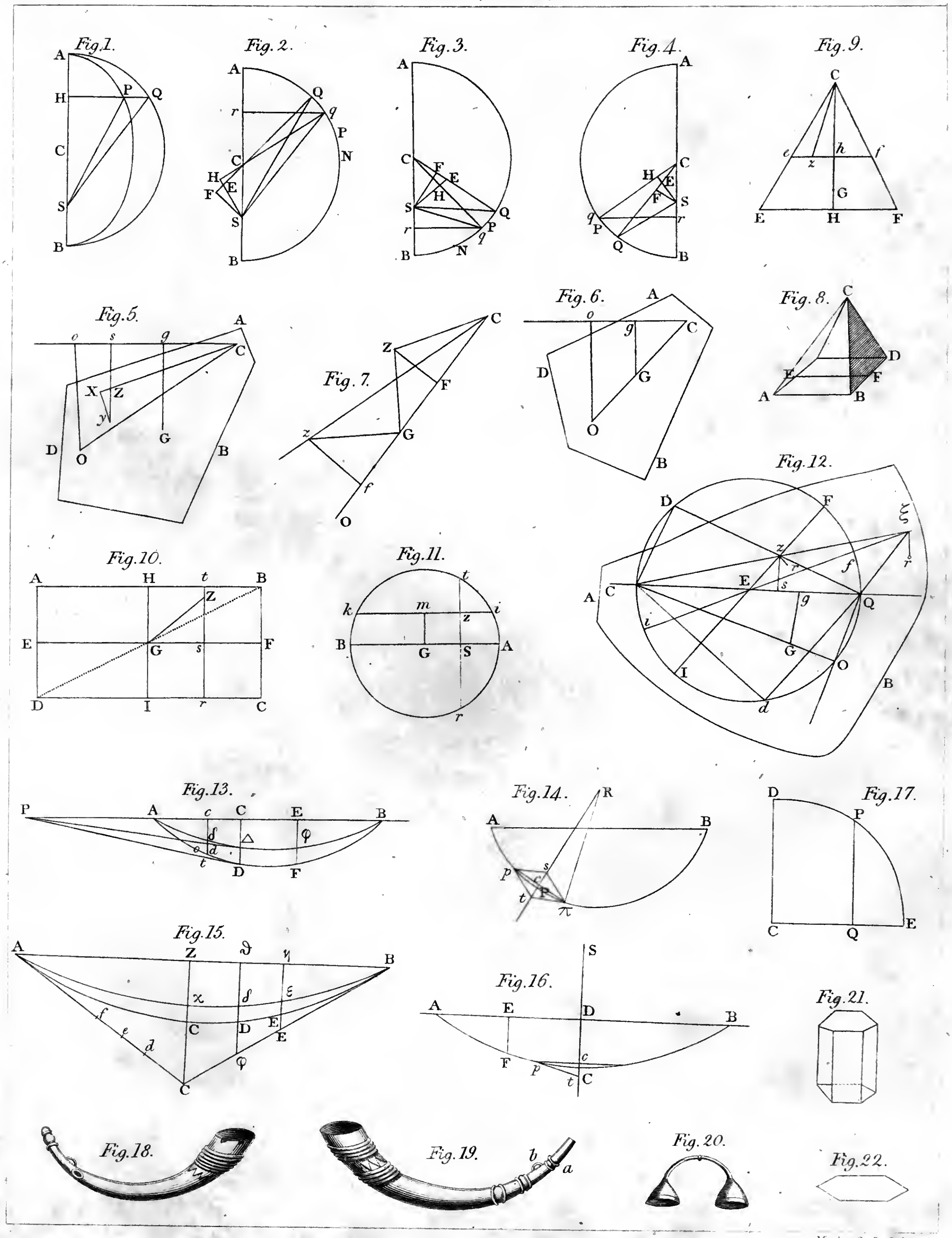
It is also to be observed, that in the year 1718, there died 84 persons above 91 years old, and 32 above 100; and, 1 died in the 116th year of his age. Besides in that very year are reckoned 2088 bastards.

In the Royal Hospital at Lisbon out of 1251 foundlings maintained by the king, without knowing their parents, there died 469, remained 782.

END OF VOLUME THIRTY-SECOND OF THE ORIGINAL.

END OF VOLUME SIXTH.

C. and R. Baldwin, Printers,
New Bridge-Street, London.



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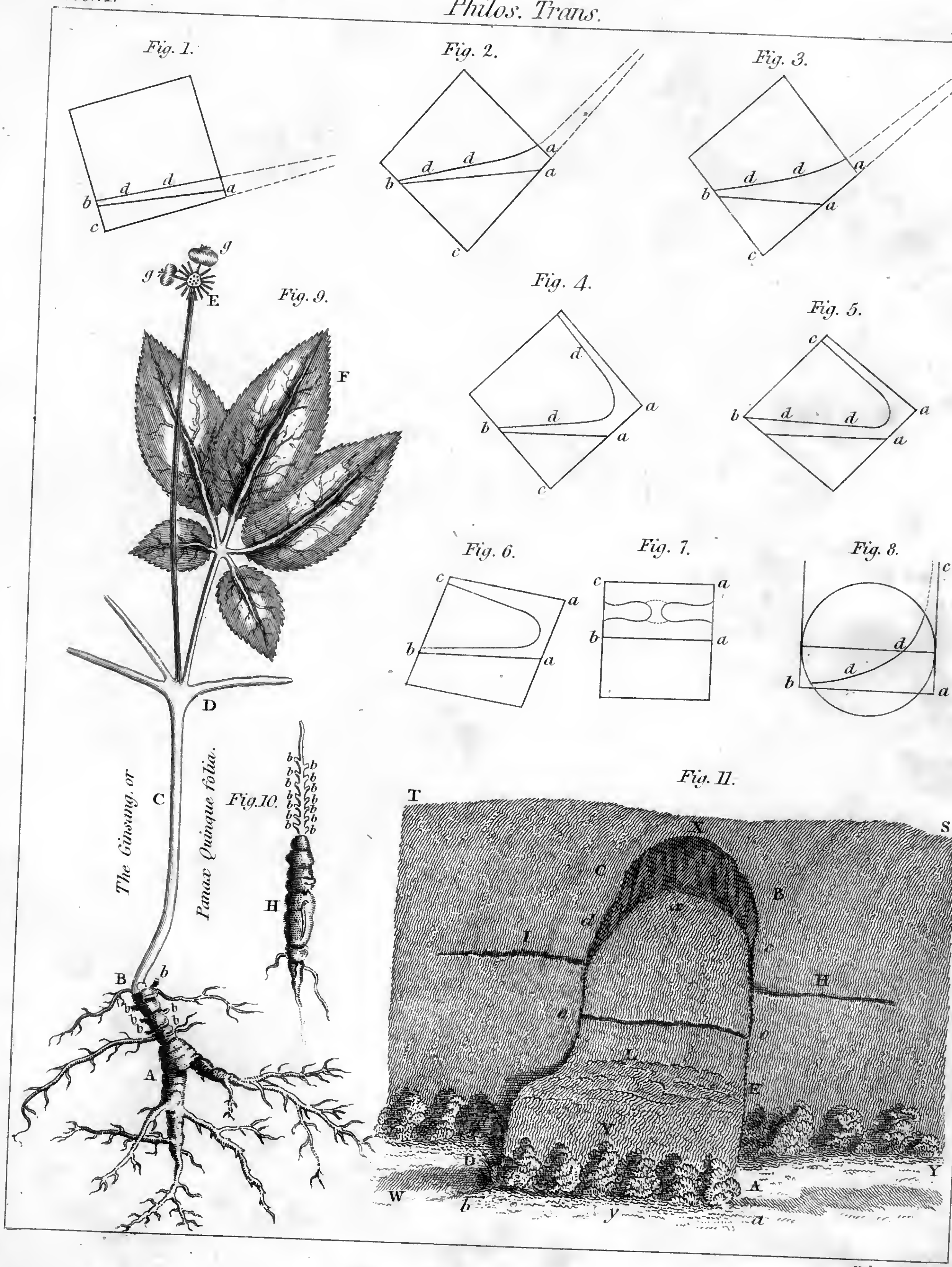

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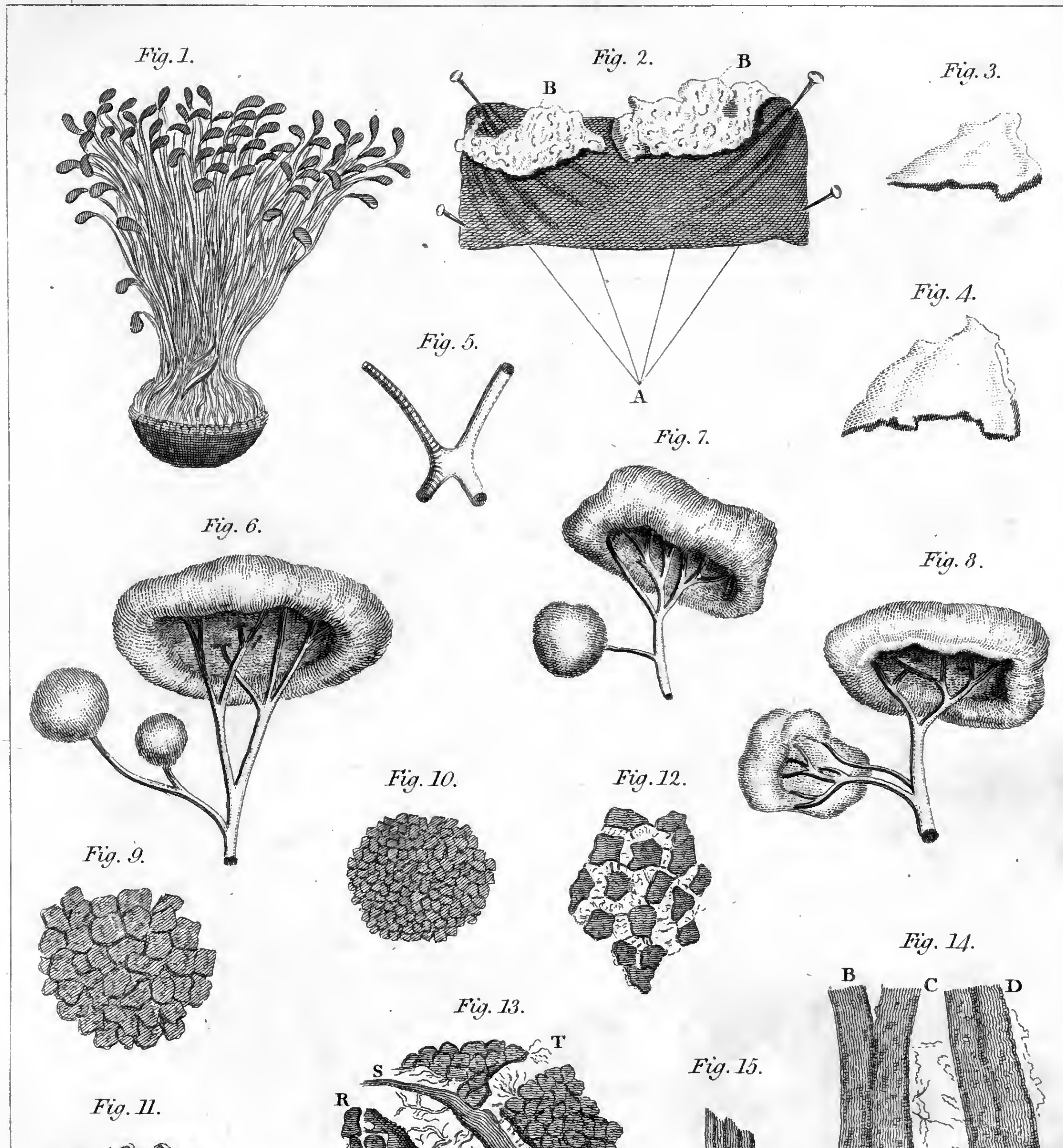

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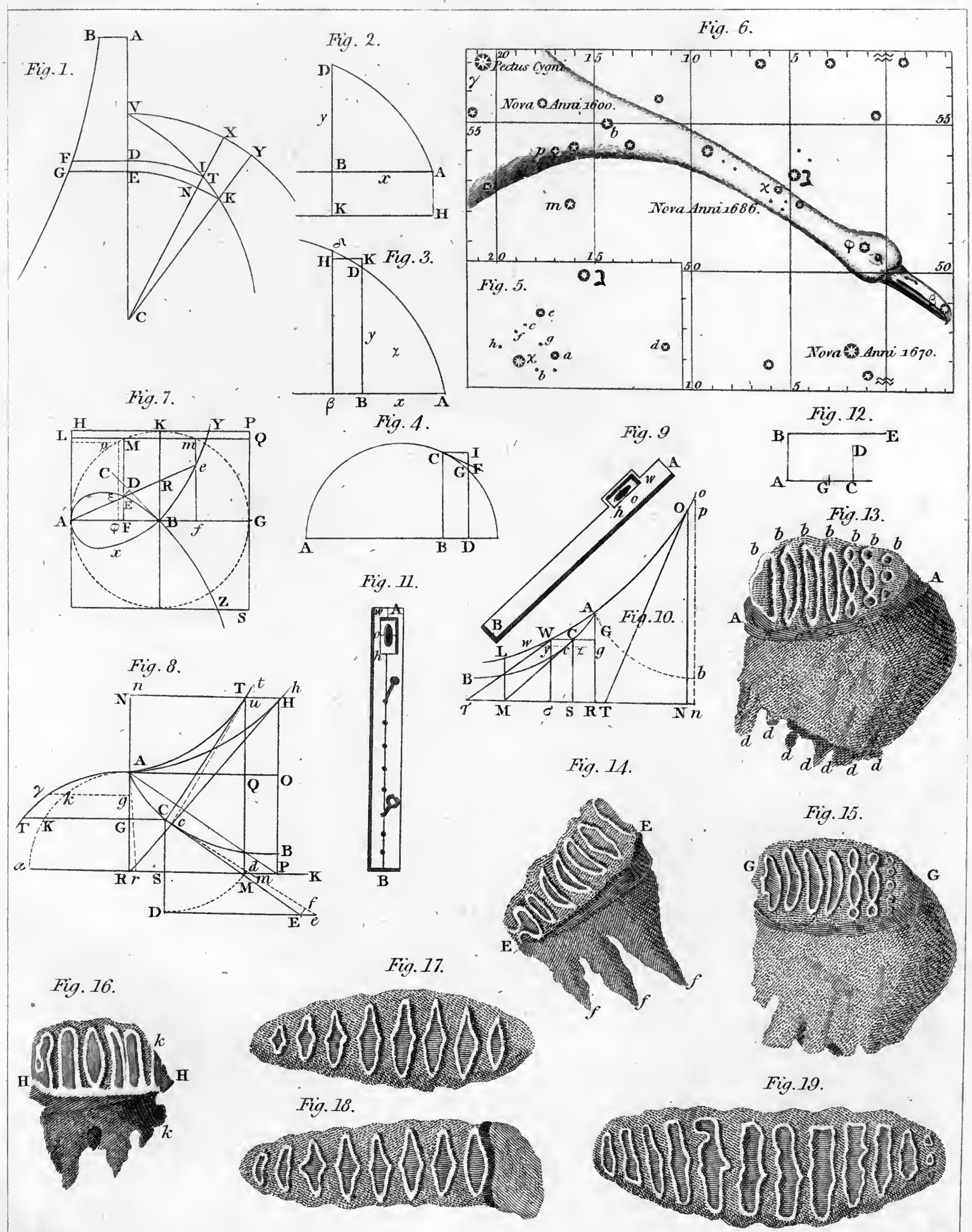

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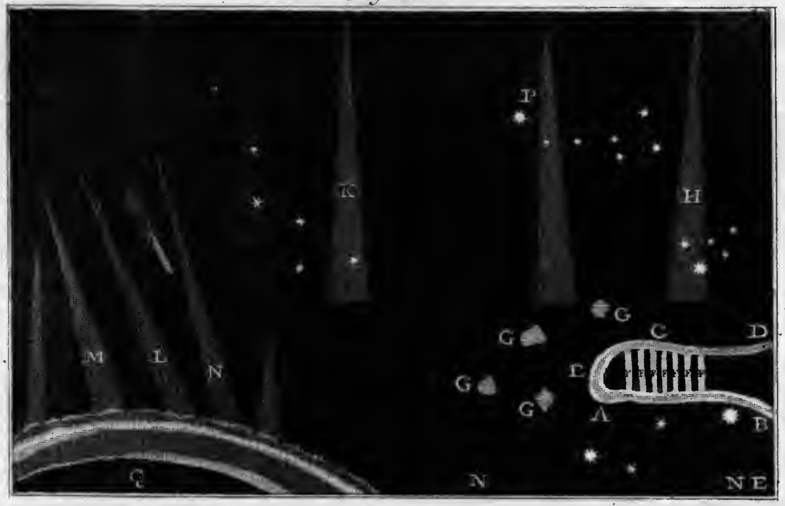

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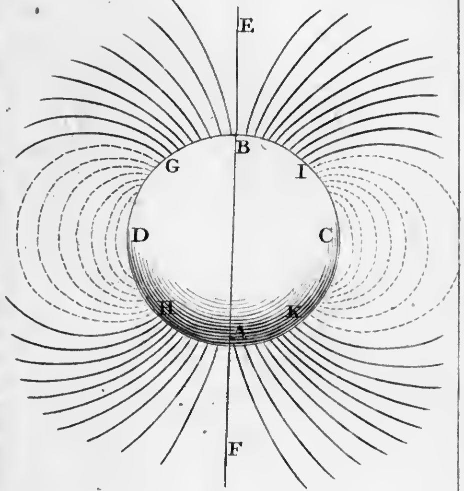

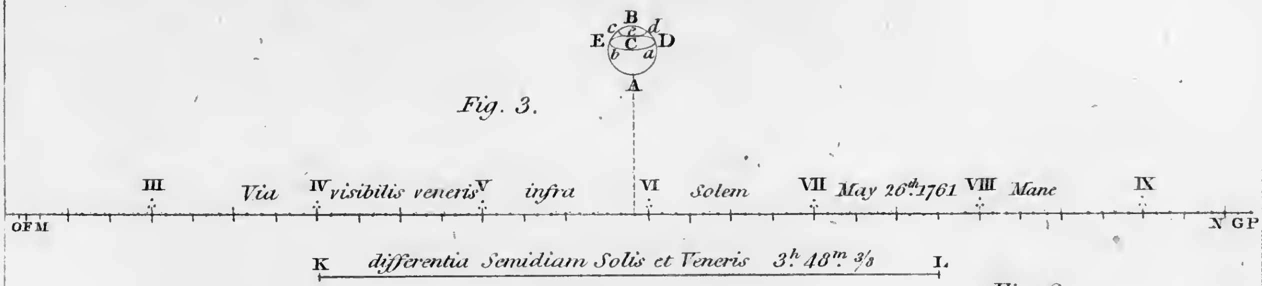

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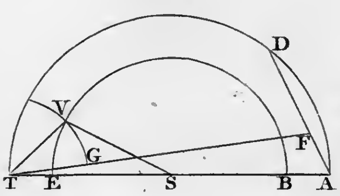

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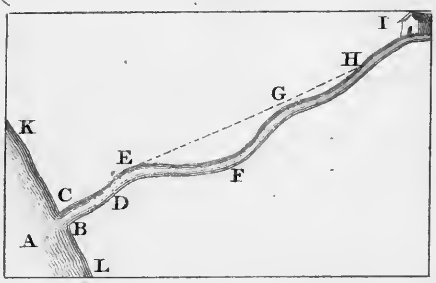

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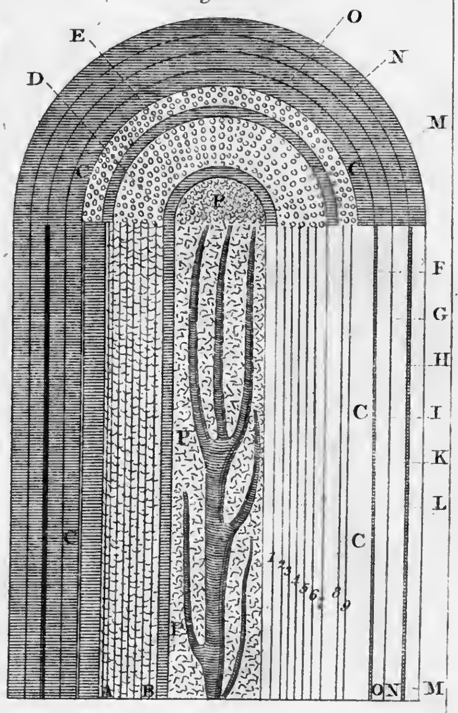

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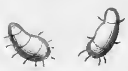

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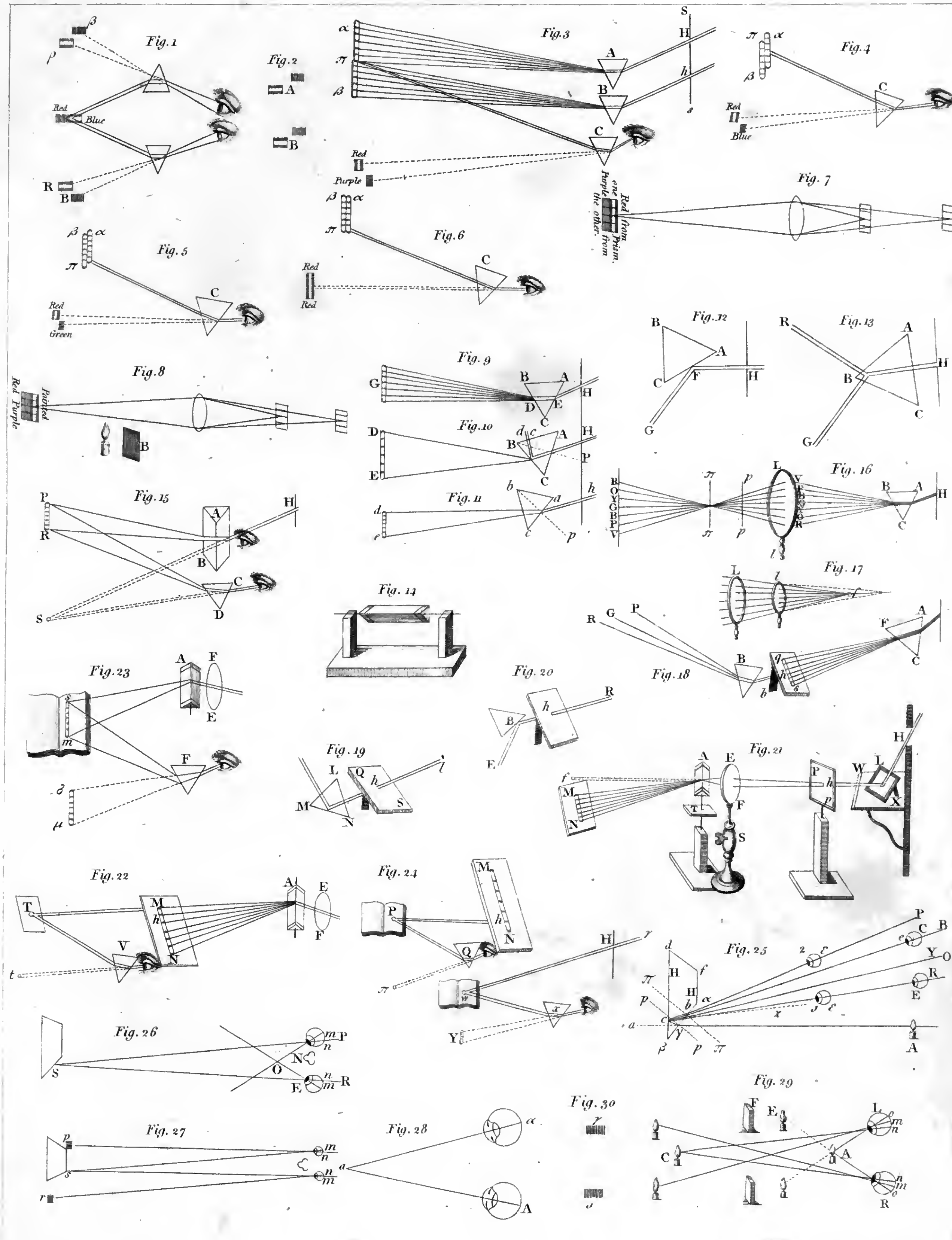

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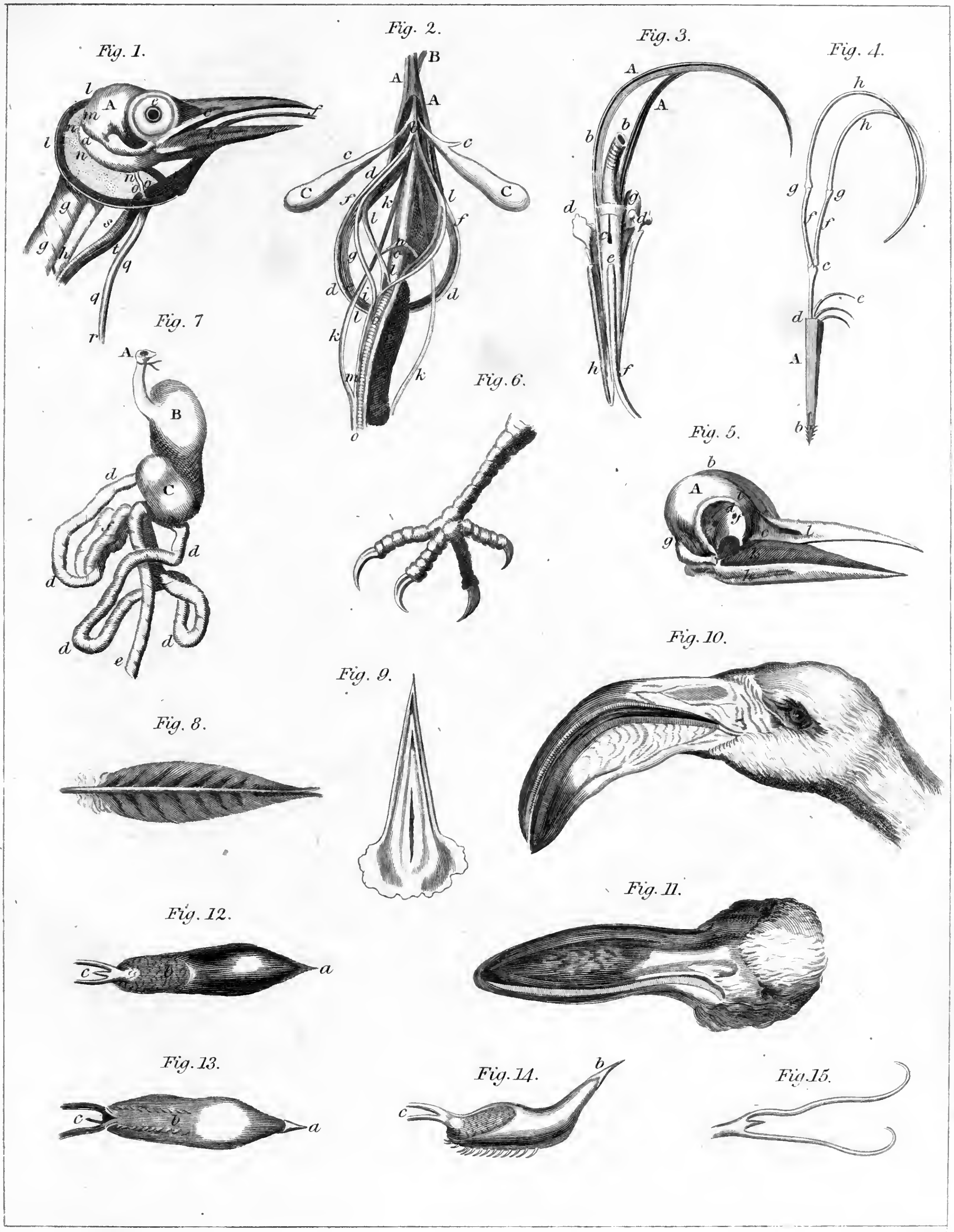

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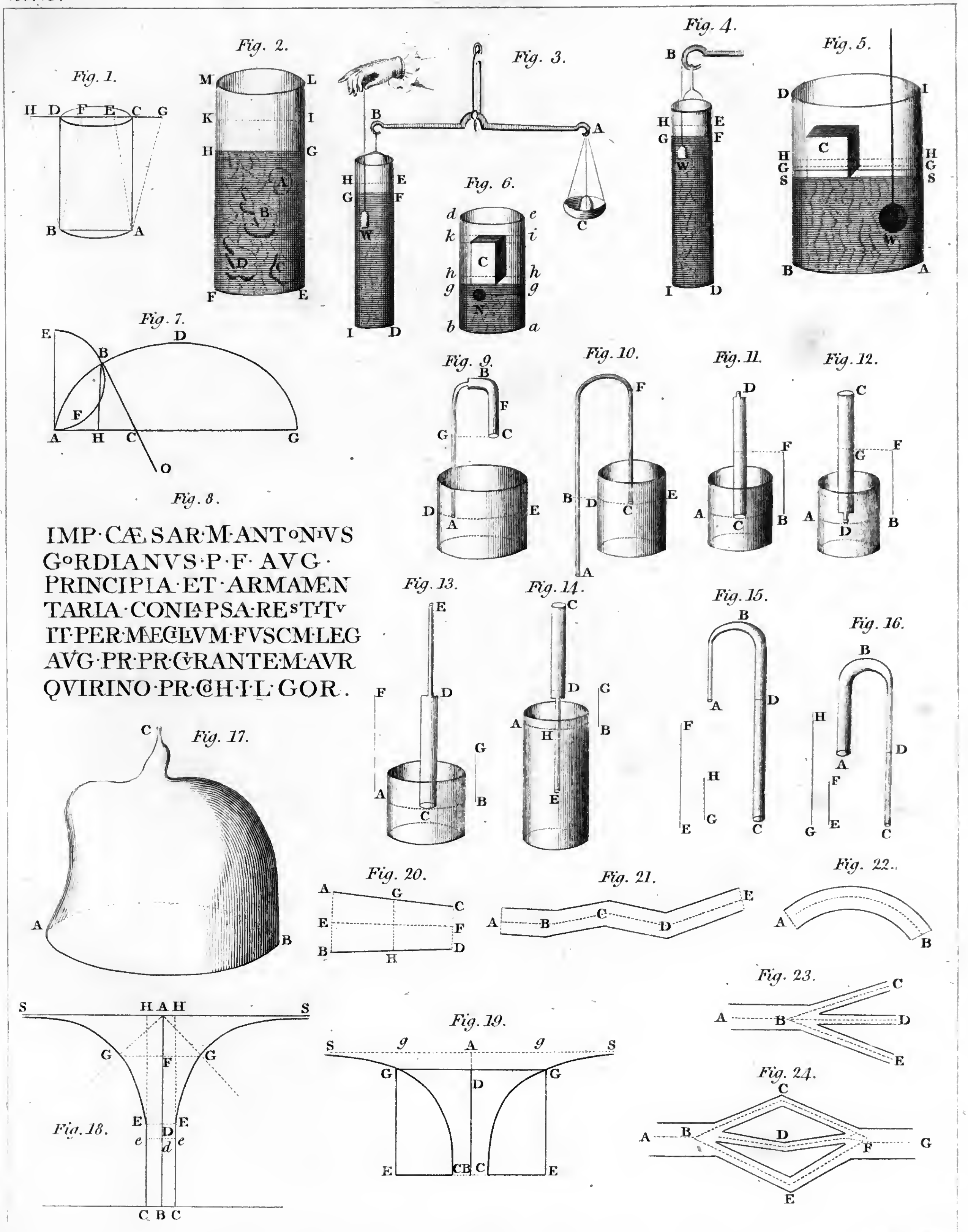

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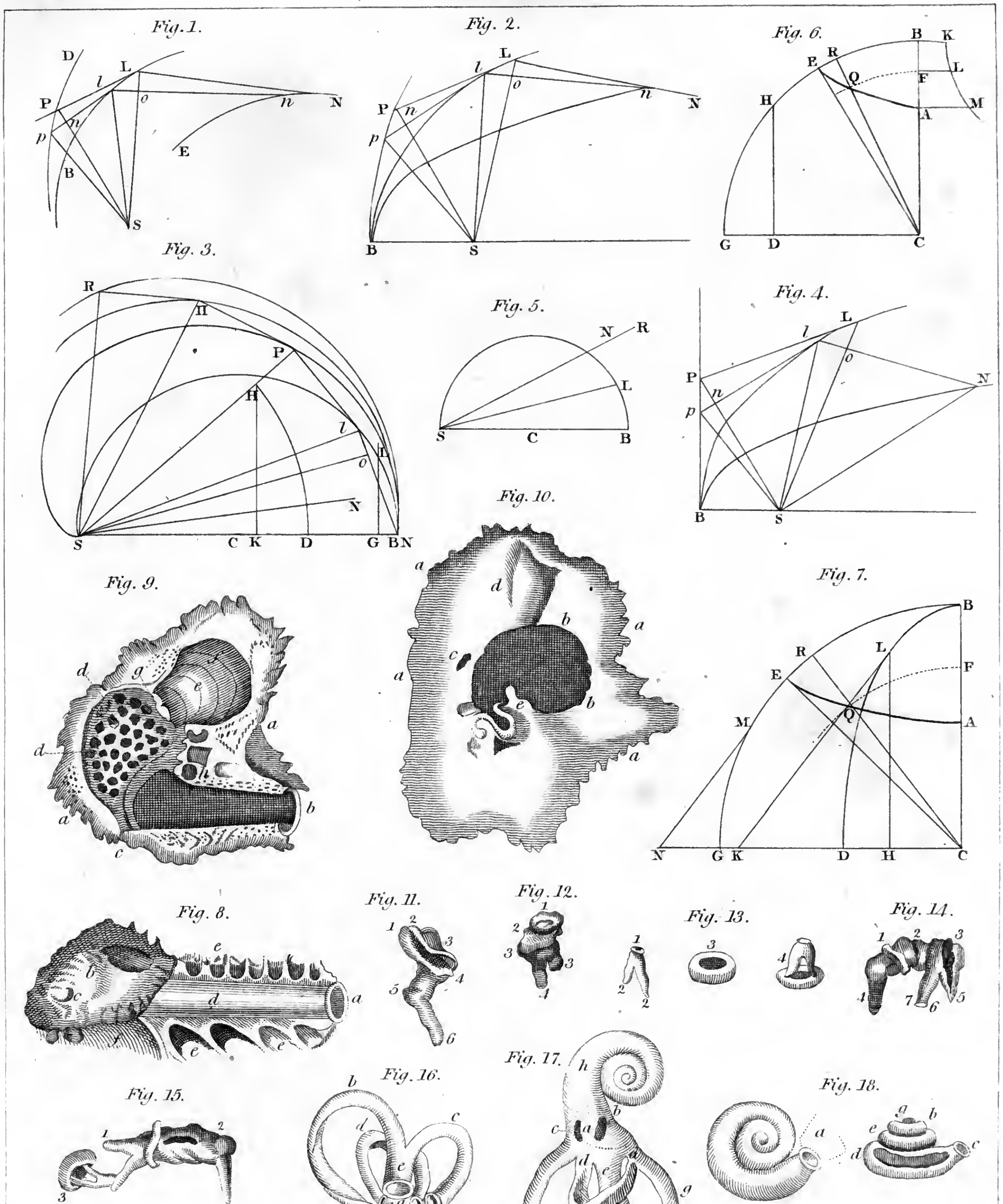

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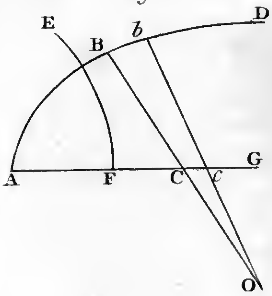

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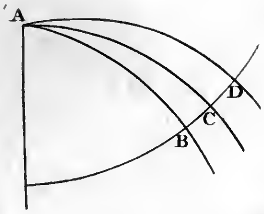

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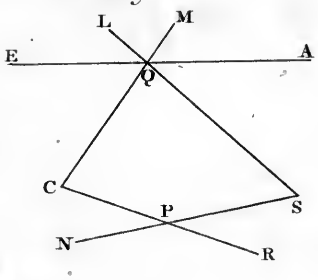

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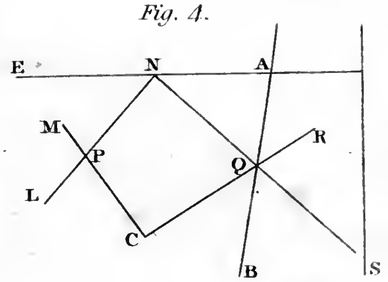

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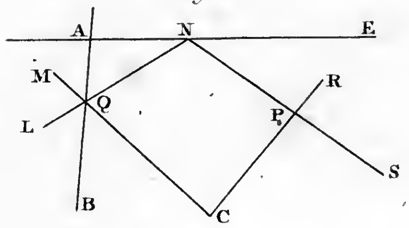

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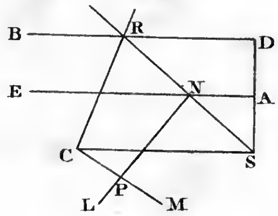

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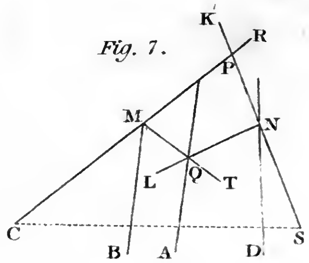

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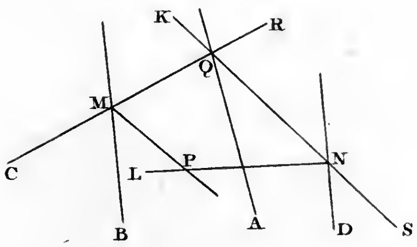

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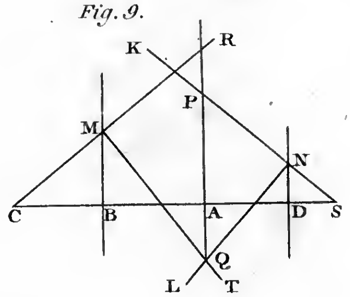

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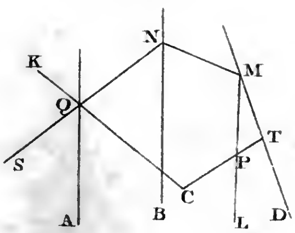

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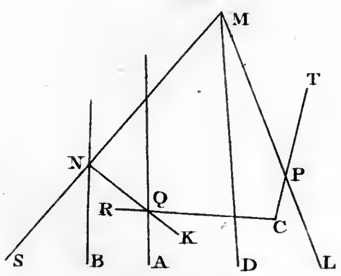

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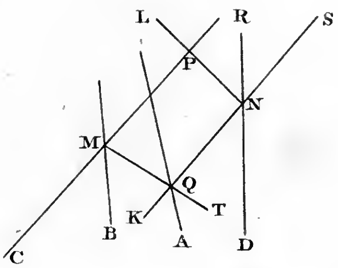

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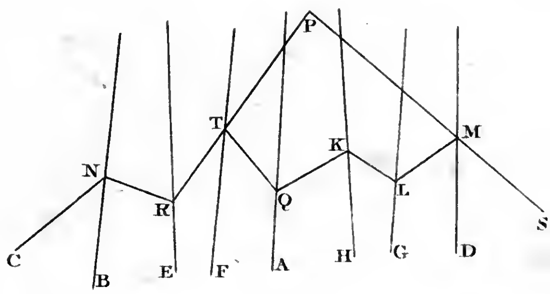

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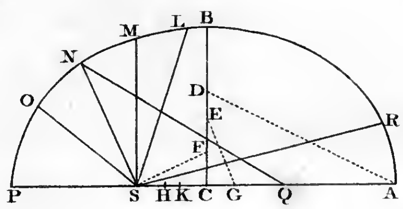

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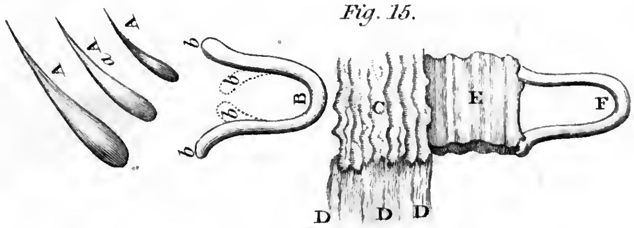

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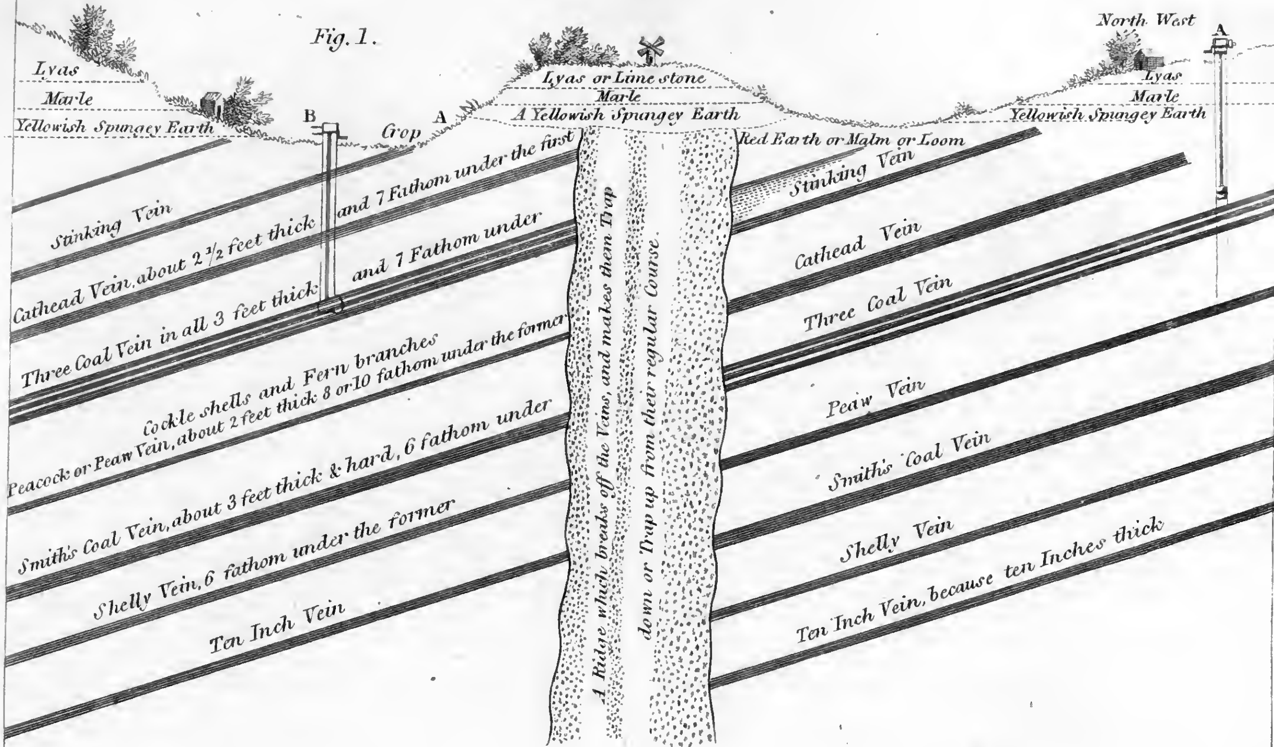

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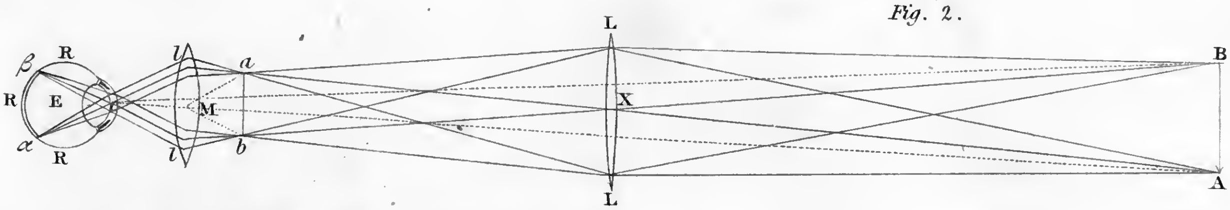

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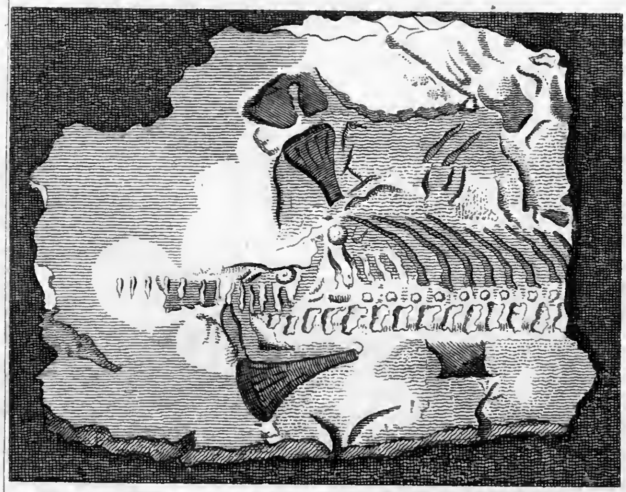


The Stone is 3 Foot long, and 2 Foot 2 Inches Broad.

Fig. 4.

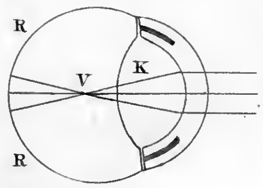


Fig. 5.

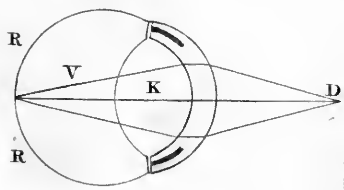


Fig. 6.

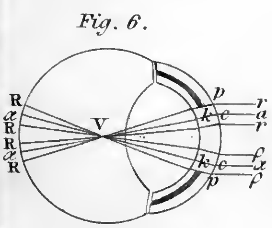
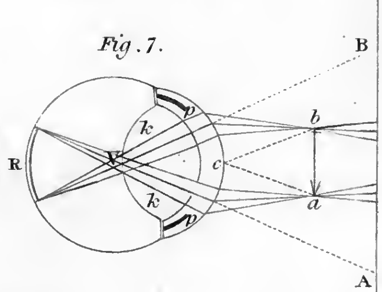


Fig. 7.



Mutlow Sc. Russell Ct.

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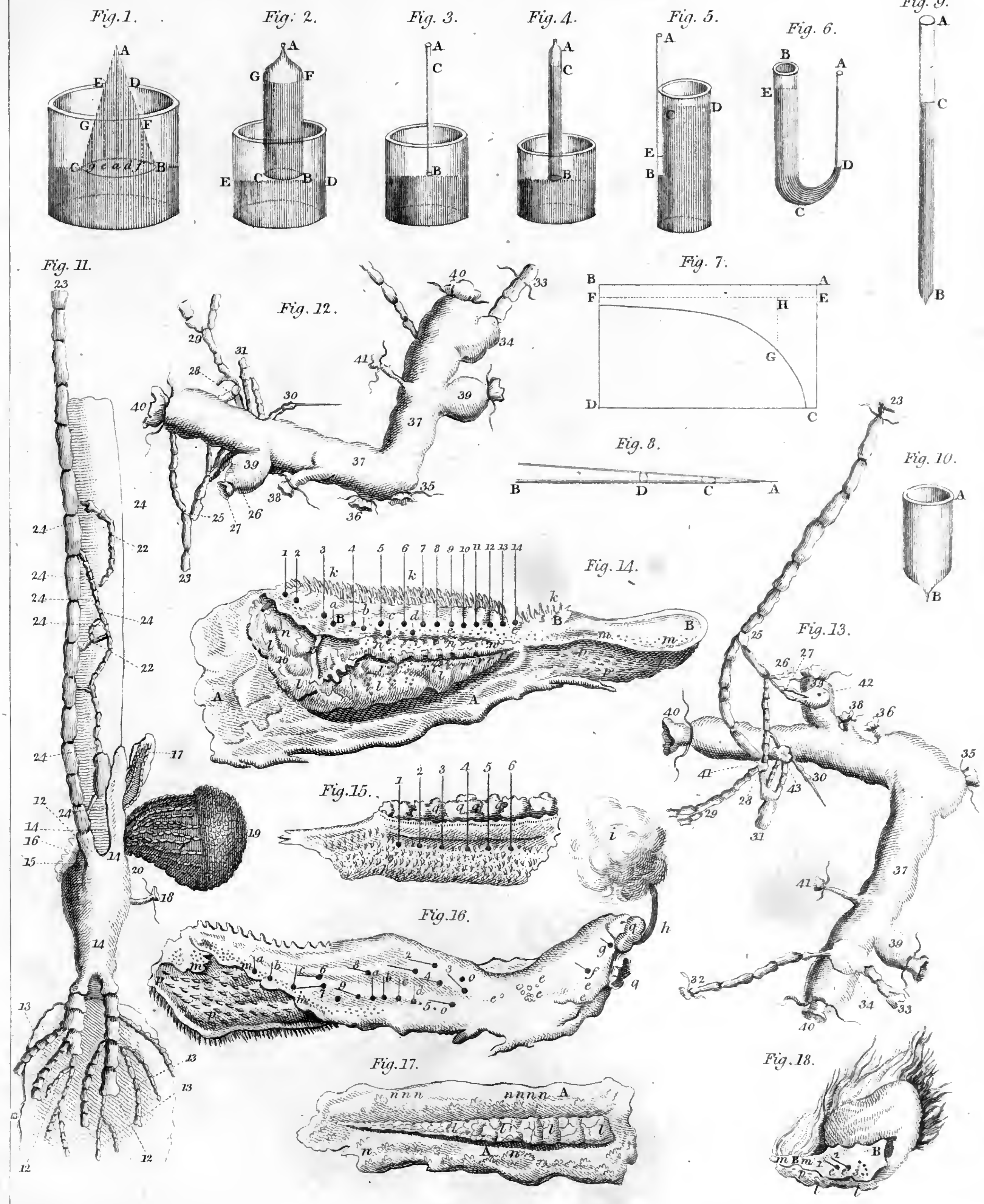

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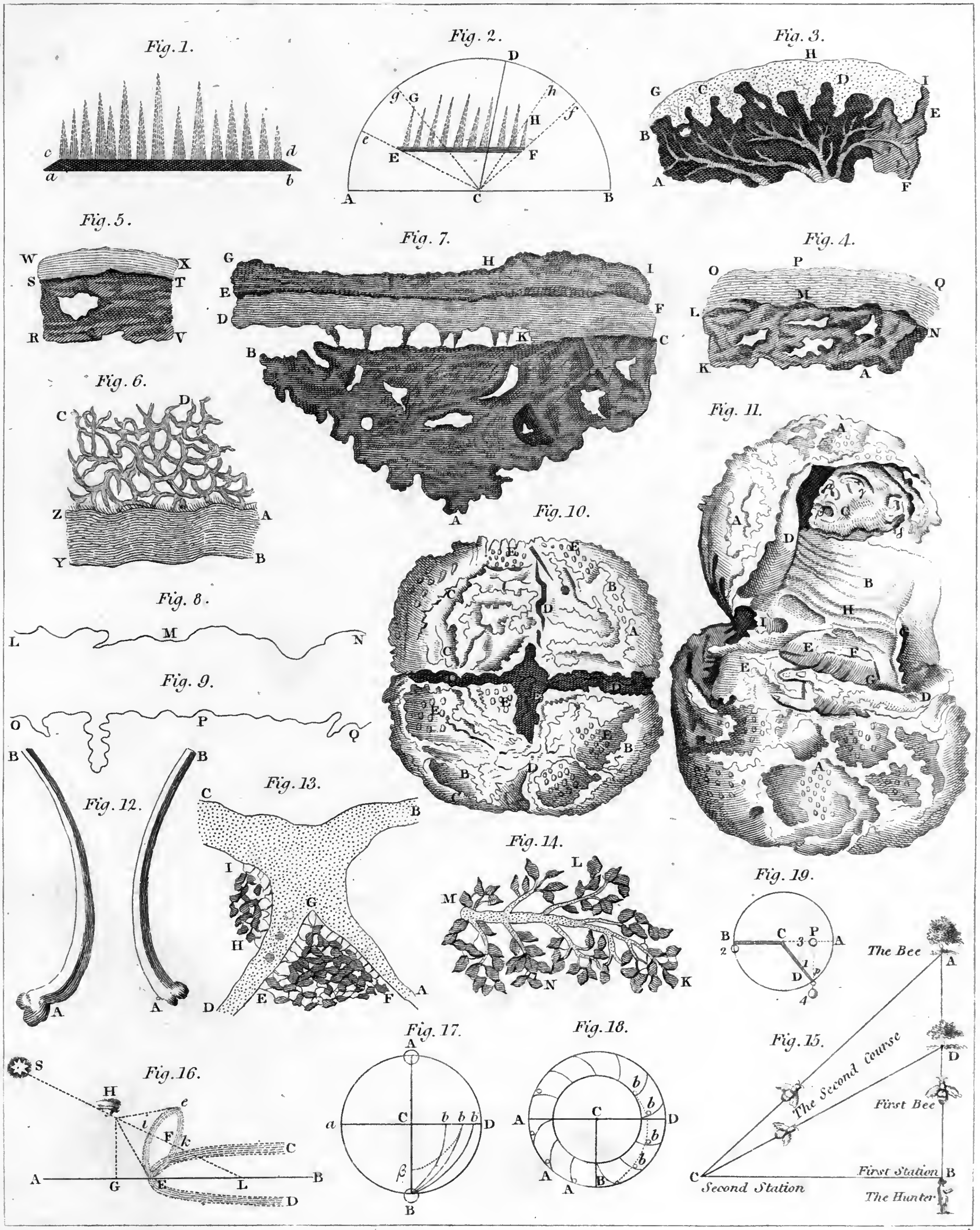

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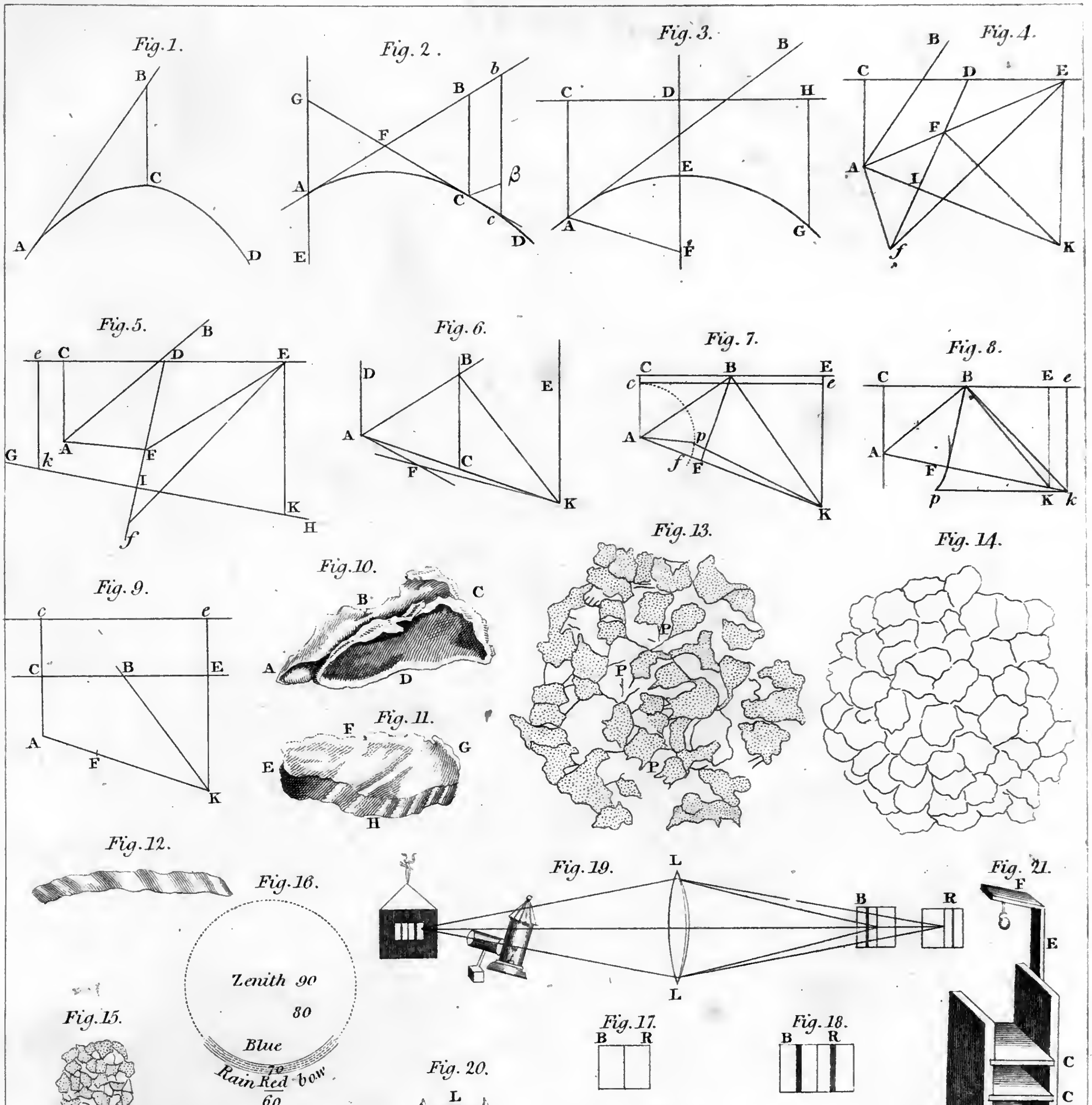

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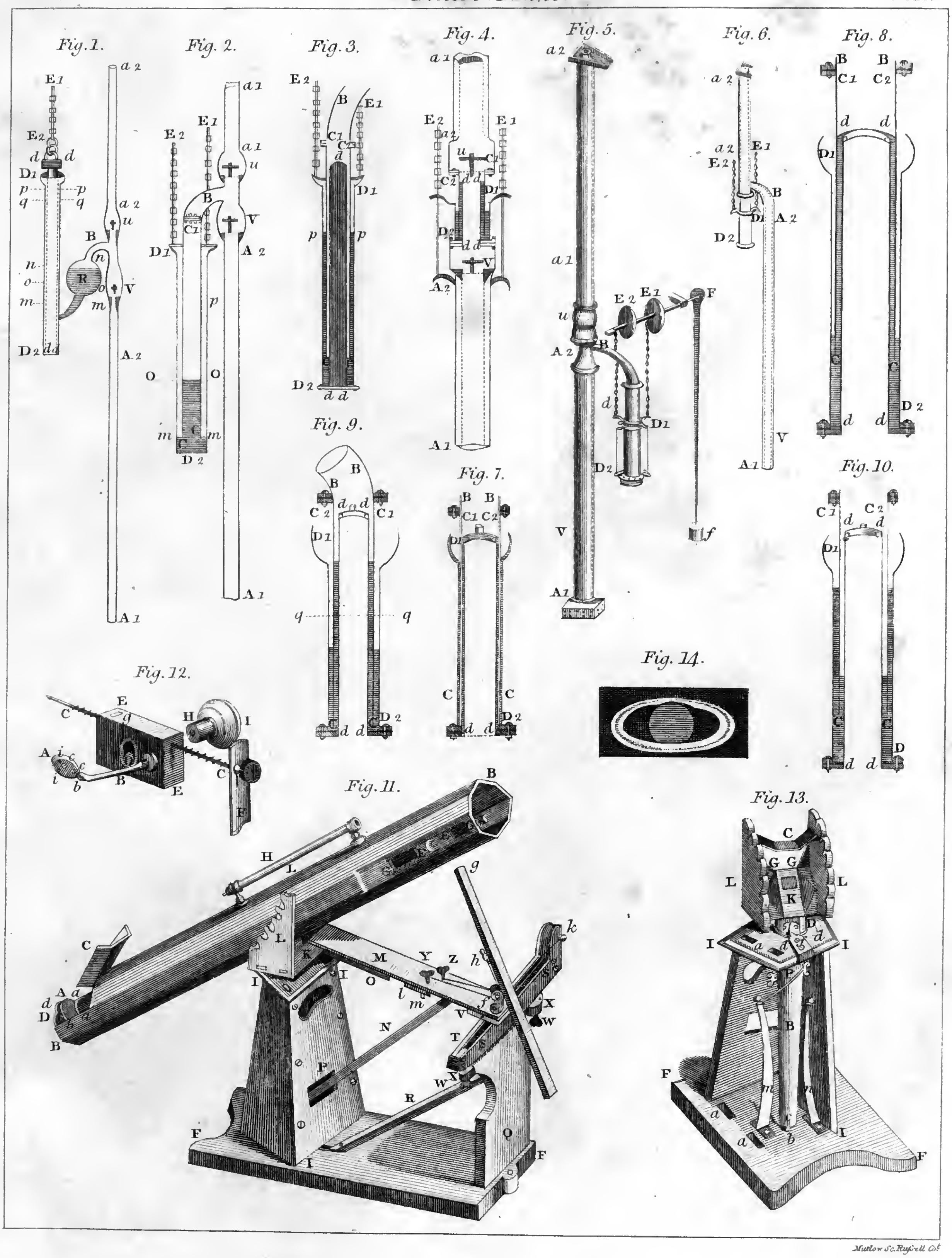

Mutlow Sc. Russell Ct.

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