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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CONTENTS OF VOLUME EIGHTH.

THE CONTENTS CLASSED UNDER GENERAL HEADS.

Class I. Mathematics.

1. *Arithmetic, Political Arithmetic.*

2. *Algebra, Analysis, Fluxions.*

3. *Geometry, Surveying.*

Class II. Mechanical Philosophy.

1. *Dynamics.*

2. *Statics.*

3. *Astronomy, Navigation, Chronology.*

4. *Projectiles, Gunnery.*

5. *Mechanics.*

6. *Hydraulics.*

7. *Pneumatics.*

8. *Optics.*

9. *Magnetism, Electricity, Thermometry.*

Class III. NATURAL HISTORY.

1. *Zoology.*

2. *Botany.*

3. *Mineralogy.*

4. *Geography and Topography.*

5. *Hydrology.*

Class IV. CHEMICAL PHILOSOPHY.

1. *Chemistry.*

2. *Meteorology.*

Class V. PHYSIOLOGY.

1. *Anatomy.*

2. *Physiology of Animals.*

3. *Physiology of Plants.*

4. *Medicine.*

5. *Surgery.*

Class VI. The Arts.

1. *Mechanical.*

2. *Chemical.*

3. *Fine.*

4. *Antiquities.*

Class VII. Bibliography; *or, Account of Books.*

Class VIII. Biography; *or, Account of Authors.*

REFERENCES TO THE PLATES IN VOLUME VIII.

THE
PHILOSOPHICAL TRANSACTIONS
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ROYAL SOCIETY OF LONDON;
ABRIDGED.

A Catalogue of the 50 Plants, from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1733, *pursuant to the Directions of Sir Hans Sloane, Bart. By Isaac Rand, Apothecary, F. R. S.* N° 436, p. 1. *Vol. XXXIX.*

THE 12th annual present of the 50 plants, making in all 600.

A Catalogue of the Eclipses of Jupiter's Satellites, for the Year 1736, *computed to the Meridian of the Royal Observatory at Greenwich. By J. Hodgson, F. R. S. Master of the Royal Mathematical School in Christ's Hospital.* N° 436, p. 5.

Mr. Hodgson's usual catalogue of these eclipses, precalculated, to compare with observations when they should be made.

The Apparent Times of such of the Immersions and Emersions of Jupiter's Satellites, as are visible at London, in the Year 1736, *with their Configurations at those Times. By Mr. Hodgson.* N° 436, p. 13.

O the same nature, and for the same use as the former.

Account of the Observations of the Eclipses of the First Satellite of Jupiter, compared with the Tables. By the same. N° 436, p. 15.

By comparing 244 eclipses of Jupiter's first satellite, observed between the years 1677 and 1731, with the Flamsteedian tables, corrected by Mr. Hodgson, he finds there are 74 that do not differ 1 minute from the tables; 127 that do not differ 2 minutes; 181 that do not differ 3 minutes; and 214 that do not differ 4 minutes; the rest not differing above $5\frac{1}{2}$ minutes.

Experiments and Observations on the Light, produced by communicating Electrical Attraction to animate or inanimate Bodies; with some of its most surprising Effects. By Mr. Stephen Gray. N° 436, p. 16.

The several new electrical discoveries made by M. Dufay, especially that important luciferous one, mentioned in a former Transaction, led Mr. Gray to make the following experiments:—

He began first with some common utensils that were at hand, as the iron poker, tongs and fire-shovel; any of these being suspended on lines of the largest sewing silk, and then the excited tube being applied first to the knob of the poker, and afterwards to the hand, the snap and pricking were felt; and the effect was the same, when the tube was first applied to the other end of the poker. He had by him a three-pronged iron instrument, made many years before; being designed for propping up the observatory table, when he observed the spots in the sun. The prongs were about $\frac{1}{4}$ an inch in diameter, two of them about 22 inches, and the third about 8 inches long, and pointed. This being laid either on cylinders of glass, or cakes of rosin and bees-wax, or on a cake of sulphur, the tube being applied to the end of any of the legs, while the hand or cheek was applied near the other, both the other legs had the same effect, as that to which the tube had been applied; but by holding his cheek near any of the points of the legs, the pricking or burning pain was much more sensibly felt, and even sometimes for several minutes after.

As to the success in repeating M. Dufay's experiment at Mr. Wheeler's, they procured silk lines strong enough to bear the weight of his foot-boy, a good stout lad: then having suspended him on the lines, the tube being applied to his feet or hands, and the finger of any one that stood by held near his hands or face, he found himself pricked or burnt, as it were, by a spark of fire, and the snapping noise was heard at the same time: but it did not succeed when they applied their hands to any part of his body through his clothes, except upon his legs, where he felt the pain through his stockings, though they were very thick ones.

They took also a large white cock, and suspending him on the lines, first alive, the effect was the same as on the boy, whether they applied their fingers to any part of his body, or their cheek to his beak, or comb or claws. The cock was then killed, and put on the lines again; when they found very little, if any difference, from the effect it had when the cock was living. They then caused the cock to be stripped of all his feathers; and the difference was still not very considerable.

They took a large surloin of beef, and suspended it on the silk-lines: then, on holding the fingers near any part of it, there was a snapping, and the fingers were pushed or pricked: but the snapping was thought not to be quite so loud, as when the experiment was made on the cock.

They caused to be made an iron rod, 4 feet long, and about half an inch diameter, blunt pointed at each end. This was suspended on the lines: then the tube being rubbed, and held near one end of the rod; and then the finger or cheek being put near either end of it, the effect was the same as when an animal was suspended on the line, with respect to the pricking pain.

At night they made the luminous part of the experiment; suspending the iron rod on the silk lines; then applying one end of the tube to one end of the rod, not only that end had a light on it; but there proceeded a light at the same time from the other end, in form of a cone, the vertex being at the end of the rod; and it plainly consisted of rays of light, diverging from the point of the rod; and the exterior rays incurvated. This light is attended with a small hissing noise; and every stroke given to the tube, causes the light to appear. The hissing seems to begin at that end of the rod next the tube; and as it comes, increases in loudness; but it is so small as not to be heard without good attention, and by those only that stand at the end of the rod from whence the said light proceeds.

Mr. Gray repeated the experiments, by laying a rod of iron on a cake of shell-lac, which was laid on a glass-vessel: and the effects were much the same as mentioned above.

Having caused to be made 3 iron rods, one 4 feet long, and two 3 feet long each; one of these was made tapering towards the ends, and pointed as that of 4 feet was; the other pointed at one end, and the other end not pointed, the diameter of the rods about half an inch; they were first forged, then filed and burnished. With these Mr. Gray made the following experiments: when any of them were laid on the brims of hollow cylinders of glass well warmed, or on cakes of rosin and bees-wax, or on those of sulphur, the phenomenon was the same, as when they had been suspended on silk lines. But now he discovered a very surprising one, viz. that after the tube had been applied, and the light seen at both ends, on going to the other end of the rod, when there was no light to be seen, and holding his hand at some distance from it, then moving his hand towards it with a pretty swift motion, there issued from that point of the rod a cone of light, as when the tube had been applied to the end: and on repeating this motion of his hand, the same phenomenon appeared for 5 or 6 times successively, only the rays were each time shorter than the former: and these lights were attended with a hissing noise. The light which appears on that end next the tube, when it is held obliquely to the axis of the rod, has its range tending towards it. All the time he is rubbing the tube, these flashes of light appear on every motion of his hand, up or down the tube; but the largest flashes are produced by the downward motion of the hand.

When several rods are laid either in a right line, or forming any angle with each other; and either touching, or are at a small distance from each other; the tube being applied to one of their ends, the farthest end of the farthest rod exhibits the same phænomena, as one single rod does.

An experiment with the rod pointed at one end only. When the tube is applied to the other end, the point exhibits the same appearance, and a like effect, as the rods that are pointed at each end; but the great end of the rod, when the hand or cheek is applied near it, gives but one single snap; though this is much louder than the greatest of those from the pointed end, and it gives a little more pain.

Having forged an iron ball, 2 inches diameter, and then turned and burnished it; having placed it on a wooden stand with a small concave on the top, in which the ball was placed: the stand being set on a cylindric glass, and the excited tube being applied near the ball, a stream of light proceeded from it, with a small hissing noise: but putting his finger or cheek near the ball, no snapping nor pain was felt; yet a very bright light appeared.

The rod, of 4 feet long, being placed on a stand, having a cross arm, with a groove in it, to receive the rod; and then the stand being placed on the glass cylinder, they were set at such a distance, as that one of the points of the rod might just touch the ball over against its centre; then going to the other end of the rod with the excited tube, he applied it as usual; when he came to the ball, the hand or cheek being near it, caused a loud snap, compared to those made by the points of the rods, and the pain of pricking or burning was more strongly felt; the light was also brighter and more contracted. The rod being then placed with its point at an inch distance from the ball, and applying the rod as before, then touching the ball with his finger, there not only appeared a light on the ball, but there also proceeded a pencil of light from the point of the rod, after the same manner as when the experiments were made with the rods only.

The following experiment was made with the 4-foot rod, and a brass plate 4 feet square. This was placed on a stand, so that the plate stood perpendicular, the stand being set on the cylindric glass; then the rod with its stand and glass was set in such a manner, that one point of it was about an inch from the centre of the plate; then the tube being applied to the other end of the rod, and after going to the plate, on striking it gently with the finger on the backside, a light appeared on the plate, and at the same time the pencil of light issued from the point of the rod; and when the hand or cheek was held near any of the angles of the plate, a light issued from thence, with a small hissing noise, and the pricking was felt, as when the experiments were made with the pointed rods.

A pewter plate being laid on the stand, which had been set on a glass cylinder, applying first the tube, and then the finger, a light appeared on the plate, and the end of the finger was pushed; and when the cheek was held near the edge of the plate, a snapping was heard, but not so loud as when the iron rods were used. On filling the plate with water, and applying the tube and finger as before, there was the same light, with the pushing of the finger and snapping, as when the experiment was made with the empty plate. When the experiment is made with water by day-light; by applying the end of the finger near the surface of the water, it appears to rise in a little hill, but on the snapping noise it falls down again, putting the water into a waving motion, near the place where the water had risen.

He then took a wooden dish, and placed it on the stand, first empty; then applying the tube and the finger near the dish, a light appeared, but no pushing of the finger, nor snapping. He then filled the dish with water, and the tube being held over the surface of it, there appeared a greater light than when the finger had been applied to the empty dish, but no snapping; till by holding the tube, after it had been well rubbed, within 2 or 3 inches of the finger that was held near the surface of the water; for then the finger was pushed, and a snapping noise was heard, as when the experiment was made with the pewter plate.

By these experiments we see, that an actual flame of fire, with an explosion, and an ebullition of cold water, may be produced by communicative electricity; and though these effects are at present but in minimis, it is probable that in time there may be found out a way to collect a greater quantity of it; and consequently to increase the force of this electric fire; which, by several of these experiments, seems to be of the same nature with that of thunder and lightning.

A General Method of describing Curves, by the Intersection of Right Lines, moving round Points in a Given Plane. By the Rev. Wm. Braikenridge. N° 436, p. 25. *Translated from the Latin.*

The following general method of describing lines of any order, by the intersection of right lines moved round poles, Mr. Braikenridge thinks is much more simple than that of Sir I. Newton, and will give a solution of many difficult problems, which he doubts if they can be found by any other principles. The author gave only one particular case of this in his Geometrical Exercitation, printed at London in 1733, not thinking it convenient to explain the whole method at that time, though he was then, he says, well acquainted with that method. It is now, he says, 3 years since he fell upon the general theorem, which he had many reasons for concealing; being determined to let 2 years at least pass, after the publication of that Exercitation, before disclosing this general method: for he doubted not, that if any others were possessed of this invention, they would, on the publication of a particular case, especially as they were provoked to it, embrace the opportunity to publish their general method, if they had really discovered one.

About 3 given points, as poles, A, B, C, fig. 1, pl. 1, in any plane, let there be turned 3 right lines, ANS, BOS, CNO, which may intersect each other in the points S, N, O; and let the two points of intersection S and N be drawn along the right lines DKS, RNK, given by position; then the remaining point O will describe a conic section; as is demonstrated in the Exercit. prop. 1.—If through the points A, B, C, be drawn the right lines AB, AC, meeting each other in A, and the right lines RK, DK, given by position, in R and M; then the figure described will pass through the 5 points B, C, K, M, R. And hence appears a new method of describing a conic section through 5 given points, much easier than any yet invented. See Exercit. prop. 3.

Let there be moved around 4 points A, B, C, D, fig. 2, as poles, in any plane, as many right lines ANS, BOS, CNO, DPO, three of which, ANS, BOS, CNO, may intersect each other in three points, S, N, O; and let the two points of intersection S, N, be drawn along the right lines dK, RK, given in position; and at the same time let the right line DPO, drawn from the 4th pole D, pass through the remaining point O, and cut the right line ANS in P: then that point P will describe a line of the 3d order: as is demonstrated prop. 11 of the exercitation.

Through the poles A, B, D, let there be drawn the right lines ABR, BDH, meeting each other in B, and the right lines KR, Kd, given by position, in R and H: then the figure described by the motion of the point P, will pass through the 5 points A, D, H, K, R, of which A will be double. Hence is deduced a method of describing a line of the 3d order through 7 given points, one of which may be double. For let A, D, H, K, P, M, R be given, fig. 3, one of which A is to be double. Through the two points H, R, and another K, let the right lines HK, RK pass; also join the points A, R, and H, D, and produce AR, HD, to meet each other in B. Then through A and the points P, M, draw the right lines APMS, AMNS, cutting the right line KR in N and N, and the right line HK in S, S; through these points S, S, to the point B, draw BS, BS; and through D, to the points P, M, draw the lines DPO, DMT, meeting BS, BS, in O and T. Draw ON, TN, produced to meet in C. Then about the points A, B, C, D, as poles, let there be revolved the lines AS, BO, CO, DO, of which the three AS, BO, CO intersect in S, N, O; and let the two S, N move along the lines HK, KR, while the line DO always passes through the remaining point O, and cuts ANS in P; then this intersection P, of the right lines AS, DO, will describe a line of the 3d order, passing through the 7 given points, A, D, H, K, M, P, R, and doubly through the given point A.

Lines of the 3d order also are more generally, but less commodiously, described after this manner, which also comprehends the former. About 5 given points, A, B, C, D, E, fig. 4, as poles, let as many right lines ANS, BOS, CNO, DPO, EPS, revolve, of which the three ANS, BOS, CNO intersect each other in the points N, S, O; let the two S, N be moved along the lines dK, KR given in position; and through one S, of the two N, S, and the remaining point O, let the lines EPS, DPO pass, being drawn through the poles E, D, and meeting in P: then this point P will describe a line of the 3d order, with a double point in the pole E.

In like manner may lines of the 4th order be described. About the 5 given joined points A, B, C, D, E, fig. 5, as poles, in any plane, let as many right lines, ANS, BQS, CNO, DPO, EPQ, be moved; of which the three ANS, BQS, CNO meet in the three points S, N, O; let the two points of intersection S, N be drawn along the lines dK, RK, given in position, while the line DPO, moveable about the 4th pole D, passes through the remaining point O, and cuts the line ANS in P; then let the line EPQ, drawn from the 5th pole E, be drawn through P, and be produced both ways to meet the lines BQS, CNO, in Q and W: then will the points Q and W describe lines of the 4th order; as is demonstrated by prop. 11 of the Exercitatio. Through the poles A, E, and B, D, let the lines AEH, BDF, pass, meeting dK, given by position, in H and F; join DE; and through the poles D and A, the line AD being drawn, meeting dK in V; from which point V let the line VB be drawn to the pole B, and cut the line DE in G. Then the figure described will pass through the 5 points B, E, G, F, H, and triply through the pole B. Through the poles A, B, let there be produced the line ABR, meeting the line KR, given by position, in R; then the curve will also pass through the points R, K.

Hence is derived a method of drawing a line of the 4th order through 9 given points, one of which is a triple point. For let B. E. F, G, H, L, M, T, Q be given, one of which B is to be triple, fig. 6. Join the points BF, FH, HE, and producing these three lines; and through the points EG, GB, let the lines EGD, BGV be drawn, of which let EGD cut BF in D, and the other line BGV cut FH in V. Then having joined V and D, and produced VD to meet HE in A, draw the line dABR through the points A, B. Then from the points B, E let the lines BQS, EPQ be inflected to the given point Q, of which let the first BQS meet FH produced in S; and through the points A, S having drawn AS, meeting EQ in P, let DPO be produced through P and D, and meet BQS in O: and note the point O. In like manner, from the same B, E, to another given point T, let the lines BTS, EPT (supply the figure) be inflected, of which let BTS meet FH in S; and having drawn AS cutting EPT in p, draw Dpz through p and D, meeting BTS in z, and mark the point z. And thus let lines be drawn from the same B, E, to the other given points M, L, and drawing lines from A and D as before, let the points so found be marked X, Y. Then through the 4 points thus found, O, Z, X, Y, and the given point B, let a conic section be described (see prop. 3, exercit.), cutting FH in the points I, K, and the line dAB in B, R. Through the points A, I draw the line AI, cutting the conic section in I and C; join the points K, R, and let this line KR be produced. Now about the 5 points A, B, C, D, E, as poles, let as many lines, AS, BS, CN, DO, EQ, revolve, of which three AS, BS, CN meet each other in N, S, O; and let the intersections N and S, of the lines AS, CN, and AS, BS, be drawn along KR and FHK, while the line DPO passes through the pole D; and the intersection O of the lines BS, CN, and cut the line AS in P; and through P and the pole E, let EPQ be produced to cut BS in Q: then this intersection Q will describe a line of the 4th order, passing through the 9 given points, B, E, F, G, H, L, M, T, Q, one of which B will be triple.

By a method not much unlike this, a line of the 4th order may be described through 8 given points, 3 of which are double; as also a line of the same order through 11 given points, 2 of which are double; with many other cases of that kind.

As to the number of points which determine a line of any order, Mr. B. says, that if n denote the number of the dimensions of a line; then $n^2 + 1$ will be the number of points through which the line may be described. For instance, a line of the 2d order through 5 points, one of the 3d order through 10, of the 4th order through 17, of the 5th order through 26 points. And hence is deduced, that if a line of the 11th order have an $n - 1$ multiple point, it may be described through $2n + 1$ points. For instance, a line of the 3d order, with a double point, (viz. $n - 1 = 2$) through 7 points, and a line of the 4th order with a triple point, through 9, &c. And generally, if p, q, r, &c. denote multiplex points, the number of which is m, a curve can be described through

$n^2 - p^2 - q^2 - r^2 + m + 1$ points, of which there are m multiplex points. Thus, a line of the 4th order, with 3 double points, may be described through 8 points: for $n = 4$, $p = q = r = 2$, $m = 3$, and $16 - 4 - 4 - 4 + 3 + 1 = 8$.

There is another method also of describing lines of the 4th order, not much different from the former, but a little more complex. About 7 poles, A, B, C, D, E, F, G, (fig. 7), let there revolve as many lines, AS, BS, CN, DS, EN, FO, GT, one of which ANS, by revolving, cuts the lines dK, RK, given by position, in the points S, N; let the lines CN, EN be drawn through one of them N, and the lines BS, DS through the other S, and meet the lines CN, EN, in the points O, T, describing conic sections as above; while the lines FO, GT, drawn from the poles F, G, pass through the same O, T, and meet in P: then the intersection P will describe a line of the 4th order, with a double point in both the poles F, G.

But not to dwell longer on these, Mr. B. now gives the following general theorem. About the poles A, B, C, D, E, F, G, H, &c. (fig. 8), whose number is n, let as many lines AS, BS, CN, DP, EQ, FW, GX, HY, &c. revolve, of which the three AS, BS, CN intersect each other in the points N, S, O; let two, S, N, be drawn along the lines dK, KR, given in position; while through the third O and the pole D, passes the line DP, cutting AS in P; and through P and the pole E draw the line EQ, cutting BS in Q; and from Q through the pole F let FQ be drawn, cutting AS in W; also through W and the pole G draw GW, cutting BS in X; and then through X and the pole H draw HY, meeting SA in Y; and so on: then the concourse Y, of the line HY, drawn from the last pole H, with either of the lines AS, BS, will describe a line of the $n - 1$ order, and have the $n - 2$ multiplex point in the pole A or B, like as it was described by the intersection of the line AS or BS. The points O, P, Q, W, X, Y, &c. will describe lines of the 2d, 3d, 4th, 5th, 6th, 7th, &c. order. But if all the poles A, B, C, D, E, F, G, H, &c. be situated in the same right line, then those points O, P, Q, W, X, Y, &c. will also describe as many right lines.

The Newtonian description of curves is also greatly promoted by this method. It is well known, that if the given angles OAN, OBN revolve about the given points A, B (fig. 9) and the intersection N, of the legs AN, BN, be drawn along the line NR, given in position; then the concourse O, of the legs AO, BO, will describe a conic section. Now let another point C be taken, about which let the line OCP be moved, which shall always pass through the intersection O of the legs AO, BO, and meet the other leg AN of the angle A in P: then the intersection P will describe a line of the 3d order, passing doubly through the pole A. In like manner, if by the intersection of the leg BN, of the angle B, a curve be described, it will be of the same order, and have a double point in the pole B. And hence also it appears, how a line of the 3d order may be described through 7 given points, one of which may be double.

Let the angles OAN, OBN, be moved as before, about the given points A, B, (fig. 10); and through the intersection O, of the legs OA, OB, let the line OCP pass, drawn from another given point C, meeting the side AN, of the angle A, in P; then through P, and a 4th given point D, draw the line DPQ, meeting the leg AO in Q; then the point Q will describe a line of the 4th order, having a triple point in the pole A.

And thus, by increasing the number of the poles A, B, C, D, &c. so that their number at length may be n, the line described will be of the same order n. But it may be noted, that if for the angle OBN, there be substituted a right line, revolved about the pole B, the description will become easier.

An Account of M. Seignette's Sal Polychrestus Rupellensis, and some other Chemical Salts. By M. Geoffroy.* N° 436, p. 37.

M. Seignette's sal polychrestus rupellensis is a soluble tartar, composed of cream or crystals of tartar, and the fixed salt of the kali of Alicant,† well depurated. This salt is very singular: for, though it be a fixed alcaline salt, it has the peculiar property of crystallizing; nor does it easily dissolve in the open air; as other fixed salts do; but on the contrary calcines in it like vitriols or Glauber's salt. Another peculiar property M. Geoffroy observed of it is, that if it be saturated with vitriolic acid, and the liquor be evaporated, there results a salt that resembles Glauber's salt, and has all the properties requisite to make M. Seignette's salt.

To produce this, take of the salt of kali [of Alicant] well depurated, 1 lb; dissolve it in water, add about 1 lb. ss. of crystals of tartar; boil the whole, to dissolve the crystals: but the exact proportion of crystals of tartar can be determined no more in this operation, than in making the soluble tartar; either because the salt of kali has retained more or less humidity in its crystallization, or because the tartar has more or less impurities in it. But if there be too much tartar in the alkaline liquor, after the fermentation is over, filtrate the liquor, and as it cools, the superfluous tartar will fall to the bottom. After the separation of the tartar from the liquor, evaporate the lixivium by a gentle fire; set it in a cool place to crystallize, and you will have very fine crystals. If the

* M. Seignette was an apothecary at Rochelle, whence this salt has been called Rochelle salt.

† The kali of Alicant is a marine plant (Salsola sativa Linn.) from which, by incineration, is obtained the fossil alkali or soda, which uniting with the superfluous acid of the crystals of tartar, forms a triple salt, compounded of tartaric acid, potass and soda, and denominated in the modern chemical nomenclature tartrite of potass and soda.

liquor be evaporated a little too much, there will be no crystals of salt formed, but the liquor will be converted into a hard transparent mass, not unlike glue. But by dissolving this mass again, it is made to crystallize, as on dissolving M. Seignette's salt.

This salt purges very well, from 1 to 2 oz. dissolved in a quart of water.

M. Geoffroy employed himself in perfecting this salt, and in examining the salt of kali, and comparing it with borax. From the salt of kali he extracted Glauber's salt, by mixing it with oil of vitriol. He next made experiments on borax. A mixture of 4 oz. of borax with 1 oz. and 1 dr. of vitriol, on sublimation, gives the sedative salt, described by M. Homberg; and the residue exposed to a strong fire, afforded Glauber's salt.* M. Geoffroy found out a method to shorten this operation: for, instead of subliming this salt, he procured it by crystallization in light foliated laminæ. This salt, whether sublimated, or crystallized, has the property of dissolving in sp. of wine; and when this sp. of wine is set on fire, its flame is green. Sp. of wine has no effect on borax; the oil of vitriol, digested with sp. of wine, communicates no greenness to its flame; it is therefore requisite that the borax be united with an acid, in order to produce this green flame.

Sedative salt, made by crystallization, crystallizes in a peculiar manner: this operation is performed with 4 oz. of borax, 1 oz. and 1 dr. of concentrated oil of vitriol, the most fixed and weighty that can be had. The borax is put into a glass retort; the oil of vitriol is poured on it; and then ½ oz. of common water. This mixture being exposed to a fire, gradually increases, after the phlegm has passed off, and even while it is passing there rise flowers, or a volatile salt, in very beautiful foliated laminæ, some of which melt by the heat of the fire. After the operation, the finest of these flowers, which are round the neck of the retort, are gathered; and those that are grey are thrown on the remaining mass; which mass is dissolved in water, filtrated and evaporated slowly. Sometimes even without evaporation, the shining talcous laminæ are to be seen in the liquor. In 24 hours the liquor is poured from these laminæ; they are washed in fair water, set to drain, and then to dry in a stove.†

If these crystals do not calcine in the stove, or in the sun, it is a sign there is nothing crystallized but the sedative salt: if they do calcine, it is a sign that there is some Glauber's salt mixed: and then this salt must be dissolved again in

* Borax being compounded of a peculiar acid termed boracic acid and soda, its own acid is expelled from the alkaline base by the vitriolic or sulphuric acid of the vitriol here employed; which unites with the said alkaline base, and forms with it Glauber's salt, i. e. sulphate of soda.

† The product here termed sedative salt, is now known to be the acid of borax.

hot water, and re-crystallized. No one before M. Geoffroy thought of extracting this salt by crystallization, being always before sublimed.

An Account of a Machine for changing the Air of the Room of sick People in a little Time, by either drawing out the foul Air, or forcing in fresh Air; or doing both successively, without opening Doors or Windows. By Dr. J. T. Desaguliers, F. R. S. N° 437, p. 41.

Fig. 1, pl. 2, represents a case, DECB, containing a wheel of 7 feet diameter, and 1 foot thick; being a cylindrical box, divided into 12 cavities, by partitions directed from the circumference towards the centre, but wanting 9 inches of reaching this, being open towards the centre, and also towards the circumference, and only closed at the circumference by the case, in which the wheel turns by means of a handle fixed to its axis A, turning in two iron forks, or half concave cylinders, of bell-metal, such as A, fixed to the upright timber or standard AE.

From the middle of the case on the other side behind A, comes out a trunk or square pipe, called the sucking-pipe; which is continued quite to the upper part of the sick person's room, whether it be near or far from the place where the machine stands, or in an upper or lower story, or above or below the machine. There is a round hole in one of the circular planes of the machine, of 18 inches diameter round the axis, just where the pipe is inserted into the case, by which the pipe communicates with all the cavities; and as the wheel is turned swiftly round, the air which comes from the sick room, is taken in at the centre of the wheel, and driven to the circumference, so as to go out with great swiftness at the blowing-pipe B, fixed to it.

As the foul air is drawn away from the sick rooms, the air in the neighbouring apartments gradually comes into the room through the smallest passages: but there is a contrivance to apply the pipes which go to the sick room to the blowing-pipe B, while the sucking-pipe receives its air only from the room where the machine stands. By this means fresh air may be driven into the sick room, after the foul has been drawn out.

This machine would be of great use in all hospitals, and in prisons: it would also serve very well to convey warm or cold air into any distant room; or even to perfume it on occasion.

Fig. 2 represents the inside of the flat of the wheel, which is farthest from the handle, and next to the sucking-pipe. 1, 2, 3, 4, represents the cavity or hole which receives the air round the axis, having about it a circular plate of iron, to hold all firm; which plate is made fast to the wood and to the iron cross containing the axis. ggg, by a pricked circle, denotes a narrow ring of thick blanketing, which (by pressing against the outside case, while it is fixed to the outside of the flat of the wheel) makes the passage into the wheel tight. HHH is another circle of blanketing, likewise fixed to the outside of the wheel, and rubbing against the case; that the air violently driven against the inner circumference of the case, may have no exit, but at the blowing-pipe at B.

On the outside of the other flat of the wheel, where the handle is fixed, is a ring of blanketing, like HHH, opposite to it, but none opposite to ggg; because the wood there is not open, but comes home close to the axis.

Fig. 3 gives a vertical section of the wheel and case, a little forward of the axis, drawn to a scale twice as large as that of the other two figures. Aa the axis, supported by the irons A, a, cylindrically hollowed, except the upper part, where a pin keeps in the axis. BD the case with the sucking-pipe sa. EA the prop for one end of the axis. 1, 2, the opening into the wheel. gg the eminence of the wood, to which is fixed the small ring of blanketing. The four black marks, one of which is near H, represent the sections of the two other rings of blanketing.

A Calculation of the Velocity of the Air moved by the new-invented Centrifugal Bellows, of 7 Feet in Diameter, and 1 Foot thick within. By J. T. Desaguliers, F. R. S. N° 437, p. 44.

When the wheel revolves on its axis, which is performed in this machine every revolution in about half a second, the air may be considered as divided into as many concentric circumferences, as there are particles of air contained between the least and the greatest circle; consequently the centrifugal forces will be as the radii, that is, in an arithmetical progression.

Let R = radius of the greatest circle .... 3.5 feet,
r = radius of the least circle 0.75,
m = radius of the middle circle .... $2.125 = \frac{1}{2}R + \frac{1}{2}r$,
v = the velocity, or space described in a second, in the middle circle, on the supposition that the wheel makes two revolutions in a second } 26.21,
s = space described in a second by the action of gravity .... } 16.1,
s = the space that a particle of air, receding from the centre, would describe in a second, by the action of the centrifugal force at the circumference of the middle circle.

Then $2m : v :: v : s$; therefore $\frac{vv}{2m} = s$, by Huygens's rule. Let G and c, express the force of gravity, and the centrifugal force at the middle circle. Since the spaces described in the same time, by the action of two forces, are as those forces, $S : s :: G : c$, and $\frac{sG}{S} = c$; then substituting in this expression, $\frac{vv}{2m}$ instead of s, we have $\frac{vvG}{2mS} = c$; and putting $\frac{1}{2}R + \frac{1}{2}r$ instead of its equal m, $\frac{vvG}{(R + r) \times S} = c$. So that the ratio of gravity to the centrifugal force, at the middle circle, is that of G to $\frac{vvG}{(R + r) \times S}$, or that of 1 to $\frac{vv}{(R + r) \times S}$; which being multiplied by the number of the revolving circles $R - r$, gives for the pressure of the column of air $R - r$, proceeding from gravity, $R - r$, and the pressure proceeding from the centrifical forces $\frac{(R - r) \times vv}{(R + r) \times S}$, where $R - r$ being a factor common to both, may be thrown out of the expression; and since the velocities produced from different pressures, are as the square roots of the pressures, the velocity gravity would give from the natural weight or pressure of $R - r$, will be to the velocity the same column would have from the pressure occasioned by the centrifugal force, as $\sqrt{1}$, or 1, to $\sqrt{\frac{vv}{(R + r) \times S}}$.

Lastly, since the velocity proceeding from the action of gravity, on a column $= R - r$, is always a known quantity, it may be called $= a$, equal in this case to 15.38 feet per second, and consequently the velocity proceeding from the centrifugal force will be $a \times \sqrt{\frac{vv}{(R + r) \times S}}$, or $av \times \sqrt{\frac{1}{(R + r) \times S}}$, or $\frac{av}{\sqrt{(R + r) \times S}}$; that is, in this machine, $\frac{15.38 \times 26.71}{\sqrt{4.25 \times 16.1}} = 49.67$ feet per second. And if we add to this, the velocity of the outer circle in the tangent of which the air escapes, which, in the supposition we made of two revolutions in a second, is 44 feet per second, we shall have $= 93.67$ feet per second.

Note.—This calculation supposes the bore of the sucking-pipe sufficient to furnish as much air as would escape, according to this velocity; but in this machine the sucking-pipe being no larger than the ajutage or blowing-pipe, the velocity proceeding from the pressure occasioned by the centrifugal force, and from the velocity in the tangent, which may be represented by a column of air of sufficient height to give the velocity of 93.67 feet, which is 145.882 feet, must be divided into two equal parts, one half employed in sucking, and the other in blowing; therefore the half of 145.882 feet, which is 72.941 feet, will represent the height of a column of air, that would occasion the same pressure with which the centrifugal force and the circular motion act in this machine; and a column of this height producing a velocity of 68.53 feet per second. This number will express the velocity with which the air is sucked into the wheel; and the same number will also express the velocity of the air out of the blower, proceeding from the centrifugal force, and the circular velocity of the outer circle, which is the real velocity of the stream of air out of the blower of this machine, viz. 68.53 feet per second, which is at the rate of a mile in about 77 seconds, or about 7 miles in 9 minutes.

The Uses of the foregoing Machine. By the same. N° 437, p. 47.

Besides the uses of this machine for sick rooms, for prisons, or large assemblies, for warming, cooling, or perfuming any chambers at a distance, it may also serve in a man of war, to take away the foul air between decks, occasioned by the number of men in the ship, and to give them fresh air in a few minutes. In every part of the vessel, every foul hole may be rendered wholesome, and even the stench and foul air from the surface of the bulge water may be carried off. Also for mines the machine must prove of excellent use; for as the damps, either fulminating, which taking fire, destroy the men and ruin the works, or arsenical, which kill by their poisonous nature, are some specifically lighter, and some specifically heavier than common air, this centrifugal wheel can in a little time drive down air through wooden trunks, or launders, of 7 inches bore, in such quantities into the deepest mines, as to cause all the light damp to come out at the top of the pit; or, by only altering two sliders, suck away all the heavy poisonous damp, while wholesome air goes down from above ground into the pit, so as to fill all the subterraneous caverns with fresh and wholesome air.

Likewise a great many of the difficulties which attend the carrying on subterraneous passages, for the conveyance of water from mines, called soughs, adits, or drifts, may be removed by the help of this wheel; for the fresh air may be driven in a very little time to the place where the men are at work, though at the distance of 2, 3, or 4 miles, and therefore also to any intermediate space; whereas the practice now is, either to make a double drift, with communications between the two for the circulation of the air, or to sink perpendicular shafts or pits, from the top of the hill over the adit: both which methods are very expensive, and inferior to the application of this machine.

The Case of a Cataleptic Woman. By Richard Reynell, Apothecary, London. N° 437, p. 49.

Ann Bullard, a servant, about 21 years of age, had been for some time irregular in her menses, and very much afflicted for the loss of a friend. July 10, 1730, she complained of a pain in her head, sickness in her stomach, with a general disorder; and took Gascoign's powder for a sweat; next morning, July 11, about 9 o'clock, she was found in bed, senseless, stiff, and void of feeling, with her eyes shut, and seemed to be dead. When Mr. R. came, he found her in a true cataleptic fit, senseless, without motion, her limbs very stiff, but warm, and not easy to be bent; but in whatever posture any limb was put, it continued in the same, whether erect or reclined; her respiration was good, but her pulse low and irregular; she had no catchings, or convulsive motions, but could not, by any means used, be brought to herself. A vein was opened in the arm, and 12 oz. of blood were taken away; she bled freely, and came a little to herself, but could not speak. Mr. R. then gave her some volatile and anti-spasmodic medicines. In a few hours she came to herself. She complained of a dizziness in her head, with a violent pain in the fore part of it, and sickness in her stomach, and was a little feverish. He gave her a vomit at 4 o'clock in the afternoon, which worked kindly, and she seemed relieved by it. About 6 in the evening another fit returned, much in the same manner as before; but she soon came out of it, and then took the medicines with the volatile salt of hartshorn, as before. Mr. R. moreover applied a large blister to her back, and two more to her arms; about 9 the same evening she had a strong convulsion fit, with catchings, grinding of the teeth, and a great tremour; neither of which she had had before: she had a stool the preceding night, but none that day. He gave her a draught with tinct. hier. at night going to rest; she continued taking the volatile medicines, &c. every 4 hours. July 12, she had been light-headed all night, with little or no rest; the blisters were dressed, which discharged plentifully, and the tincture had given her 3 stools in the night, which had made her a little faint; her pulse was low, and her water pale. Mr. R. saw her in the evening, when she had slept pretty well, with which she was refreshed; the pain in her head but little, her stomach easy, and he found her in every respect better. The medicines were continued. July 13, in the morning, Mr. R. found her head easy, her water higher coloured; she was allowed broth, and food of easy digestion, which agreed very well with her. She sat up in the afternoon, but was faint, and her head giddy; but when in bed she was better. She had no stool that day. The volatile medicines, &c. were continued with a purge the next morning. July 14, the purge worked 5 times; she eat a light dinner, and was easy; but on walking about the room, her head was giddy, and she trembled very much; but when in bed, she was better. Mr. R. gave her a draught with sp. c. c. and tinct. castor. July 15, she complained, when up, of a numbness in her legs, and a pricking in them, like what happens when the legs are what we commonly call asleep: her appetite was better, and she was in every respect mended. Mr. R. then gave her some medicines composed of valer. castor. asafœtida, &c. The blisters were kept running as long as could be; and when they were dried up, July 19, he gave her the same purge as before. July 22, she had continued very well, without any return of a fit; but on cutting an issue in her arm, she fell into a third fit, in which she continued near 2 hours; but then came to herself, and was well that evening. July 29 the purge was repeated. August 6 she complained of a pain in her head, sickness in her stomach, and some days before she had the menses, and had vomited near 1 lb. of blood, and was costive; Mr. R. then advised her to take 2 spoonfuls of tinct. sacra every, or every other night, going to bed, as she found it necessary, and 40 of the following drops: ℞ spt. c. c. opt. ʒiij. tinct. helleb. nigr. ʒv. to be taken twice a day in camomile tea. She took these medicines about 3 weeks, which answered expectation, and he left her well. He saw her about 12 months after, and she told him she had continued very well ever since.

Sennertus, Med. Pract. lib. 1. c. 30, says, that a catalepsy is so rare a case, that it is supposed hardly one physician in a hundred has seen a cataleptic patient; so that when this disease occurs, its history is carefully to be noted.

Thoughts on the Operation of the Fistula Lacrymalis. By Francis Joseph Hunauld, M. D. F. R. S. and Member of the Royal Academy of Sciences at Paris; in a Letter to Tho. Stack, M. D.* N° 437, p. 54.

Mr. H. omits giving the history of the fistula lacrymalis, of the different species of the distemper, or the various methods of treating it, as things sufficiently known; and only remarks that the intention in destroying the os unguis, and saccus lacrymalis, through which the tears naturally distil into the nose, is to procure them a new passage thither, by the hole thus artificially made. In order to keep the sides of this hole asunder, to prevent its filling up, and render the flesh, which forms its circumference, hard, and as it were callous, a tent made of prepared sponge, &c. is put into this new passage, where it is continued a month or two. Notwithstanding this precaution, it happens but too often, that the tears, instead of keeping the road prepared for them with such care, flow over the lower eye-lid, as before the operation, and occasion a weeping, which is now become past remedy.

It is easy to prove, that those very means, which are used after the operation to make the tears distil into the nose, are generally the cause of the subsequent

* M. Hunauld succeeded M. du Verney in the anatomical professorship at Paris. He wrote a treatise on the bones of the cranium, besides various papers inserted in the Memoirs of the French Academy of Sciences. He was possessed of a good collection of anatomical preparations, which was purchased by the academy after his death. This happened in 1742, when he was in his 41st year. His fort lay in osteology and the diseases of the bones.

weeping; for by filling the wound with small pledgets, and putting a tent into the hole, the orifice of the little common canal, that serves to convey the tears into the lacrymal duct, suffers a compressure, and is rendered hard, thick, and callous; by which, as its diameter is very small, it is easily stopped up. The contusion made on this small orifice, and round about it, brings on a suppuration; after which the parts coalesce, and the orifice of this small canal closes up. The pus or sanies, which in the course of the distemper flowed back, both through the common canal, and the small canals, which are a continuation of the puncta lacrymalia, has sometimes occasioned excoriations; in consequence of which there happens a regeneration of flesh during the dressings, a small matter of which is sufficient to stop up such slender ducts. Indeed those small canals, through which nothing passes for a month or two, that the dressings last, either close by their own elasticity, or their diameters are lessened by their small vessels becoming varicose. It is true, that injections are sometimes made through the puncta lacrymalia; but the propelling force of these injections overcomes those resistances, which the cause that naturally drives the tears into the puncta lacrymalia, is not in a condition to overcome.

Thus it appears, from the detail of the accidents here enumerated, and which generally happen more or less, that while the artist is endeavouring to preserve a clear passage for the tears into the nose, he labours, without designing it, to stop the entry of the upper part of their canal. Mr. H. hopes to make appear, that the best way to avoid part of these accidents, and keep open the new canal from the eye to the nose, is precisely to do nothing. This is what experience has confirmed, and what theory too, well understood, will give us a clear conception of.

It is not very easy to determine, how the tears, and the liquid that is continually found on the surface of the eye, to preserve the cleanness and transparency of the cornea, can pass through the puncta lacrymalia. It is also observed, that when we lie in bed, this liquid enters into those puncta lacrymalia, which in that position are higher than the eye, as well as into the puncta lacrymalia of the opposite eye. The ascent of liquors in capillary tubes above the level, might be proposed to explain this last fact. We might also in certain circumstances conceive the road which the tears keep, to pass from the eye into the nose, to be a syphon, the short leg of which is divided into two. It is strange that these two ideas, which strike by their simplicity, have never been offered by any one. It must be allowed however that they are not entirely sufficient to account for the phenomenon under consideration. The following rationale seems quite as simple, and more accurate.

The air present at the orifices of all the ducts, which have any communication with the trachea, is by its proper weight determined to enter them, when the resistance happens to be diminished. Thus as, during inspiration, it passes through the mouth and nostrils, so it likewise enters the puncta lacrymalia; and must necessarily carry with it, towards these puncta and their small canals, the moisture that lubricates the surface of the ball of the eye, as it mixes with it. Therefore it is easy to perceive already, that to preserve to the tears their new and artificial road into the nose, we need only commit the whole care to the continual passage of the air and tears. It is well known in good surgery, that it is very difficult, not to say impossible, to effect a reunion in a part, that serves as an emunctory to a liquor constantly flowing to it.

Now let us examine, if nature alone can stop the hole made by the operation. It will not be imagined, that from the remains of a bony lamina, so thin as the os unguis, a sufficient quantity of ossifying juice can work out to stop it up. The periosteum and saccus lacrymalis are too much lacerated, to think it possible for them to repair of themselves what they had lost. Nor will it be believed, that the membrana pituitaria can easily fill up the hole made in it. Those are the parts concerned in the operation: but even if they are granted to be more disposed to a re-production than they really are, still the air and tears will always be able to preserve themselves a passage into the nose.

Therefore, after having destroyed the saccus lacrymalis and os unguis, instead of introducing an extraneous body capable of making the orifice of the small common canal into the ductus lacrymalis become callous, and of drawing on a suppuration, the communication between the nose and eye must be left entirely disengaged, and liberty by this means be given to respiration, to make both the air alone, and the air mixed with the tears, to pass continually through it.

Also, the action of these fluids may be assisted by the application of collyriums, and by making frequent injections into the puncta lacrymalia; which, besides the common effects that may be naturally expected from them, will contribute to prevent the juice, that re-unites the wound made in the skin, from over-straitening the canal.

On the Cause of the General Trade-Winds. By Geo. Hadley, Esq. F. R. S. N° 437, p. 58.

Probably the causes of the general trade-winds have not been fully explained by any who have written on that subject, for want of more particularly and distinctly considering the share the diurnal motion of the earth has in their production. For though this has been mentioned by some among the causes of those winds, yet they have not showed how it contributes to their production;

or else have applied it to the explication of these phænomena, on insufficient principles.

That the action of the sun is the original cause of these winds, it seems all are agreed; and that it does it by causing a greater rarefaction of the air in those parts on which its rays, falling nearly perpendicular, produce a greater degree of heat there, than in other places; by which means the air becoming specifically lighter than the rest round about, the cooler air will, by its greater density and gravity, remove it out of its place, and make it rise upwards. But it seems that this rarefaction will have no other effect, than to cause the air to rush in from all parts, into the place where it is most rarefied, especially from the north and south, where the air is coolest, and not more from the east than the west, as is commonly supposed. So that, setting aside the diurnal motion of the earth, the tendency of the air would be from every side towards that part where the sun's action is most intense at the time, and so a N. W. wind be produced in the morning, and a N. E. in the afternoon, by turns, on this side of the parallel of the sun's declination, and a S. W. and S. E. on the other side.

That the perpetual motion of the air towards the west, cannot be derived merely from the action of the sun upon it, appears more evidently from this: if the earth be supposed at rest, that motion of the air will be communicated to the superficial parts, and by little and little produce a revolution of the whole the same way, except there be the same quantity of motion given the air in a contrary direction in other parts at the same time; which is hard to suppose. But if the globe of the earth had before a rotation towards the east, this by the same means must be continually retarded: and if this motion of the air be supposed to arise from any action of its parts on each other, the consequence will be the same. For this reason it seems necessary to show how these phænomena of the trade-winds may be caused, without producing any real general motion of the air westwards. This will readily be done by taking in the consideration of the diurnal motion of the earth: for, let us suppose the air in every part to keep an equal pace with the earth in its diurnal motion; in which case there will be no relative motion of the surface of the earth and air, and consequently no wind: then by the action of the sun on the parts about the equator, and the rarefaction of the air thence proceeding, let the air be drawn down thither from the N. and S. parts. The parallels continually enlarge, as they approach to the equator, and the equator exceeds the tropics, nearly in the ratio of 1000 to 917; consequently their difference in circuit is about 2083 miles, and the surface of the earth at the equator moves so much faster than the surface of the earth with its air at the tropics. From which it follows, that the air, as it moves from the tropics towards the equator, having a less velocity than the parts of the earth it arrives at, will have a relative motion contrary to that of the diurnal motion of the earth in those parts; which being combined with the motion towards the equator, a N. E. wind will be produced on this side of the equator, and a S. E. on the other side. These, as the air comes nearer the equator, will become stronger, and more and more easterly, and be due east at the equator itself, according to experience, by reason of the concourse of both currents from the N. and S. where its velocity will be at the rate of 2083 miles in the space of one rotation of the earth or natural day, and above 1 mile and $\frac{1}{3}$ in a minute of time; which is greater than the velocity of the wind is supposed to be in the greatest storm, which according to Dr. Derham's observations, is not above 1 mile in a minute. But it is to be considered, that before the air from the tropics can arrive at the equator, it must have gained some motion eastward from the surface of the earth or sea; by which its relative motion will be diminished; and in several successive circulations, may be supposed to be reduced to the strength it is found to be of.

Thus it appears the N. E. winds on this side of the equator, and the S. E. on the other side, are fully accounted for. The same principle as necessarily extends to the production of the west trade-winds without the tropics; the air rarefied by the heat of the sun, about the equatorial parts, being removed, to make room for the air from the cooler parts, must rise upwards from the earth; and as it is a fluid, it will then spread itself abroad over the other air, and so its motion in the upper regions must be to the N. and S. from the equator. Being got up at a distance from the surface of the earth, it will soon lose great part of its heat, and thus acquire density and gravity sufficient to make it approach its surface again, which may be supposed to be when arrived at those parts beyond the tropics where the westerly winds are found. Being supposed at first to have the velocity of the surface of the earth at the equator, it will have a greater velocity than the parts it now arrives at; and thus become a westerly wind, with strength proportionable to the difference of velocity, which in several rotations will be reduced to a certain degree, as before said of the easterly winds, at the equator: and thus the air will continue to circulate, and gain and lose velocity by turns from the surface of the earth or sea, as it approaches to, or recedes from, the equator. To solve the phænomena of the variations of these winds at different times of the year, and different parts of the earth, would too far extend this paper. From what has been said it follows:

1. That without the assistance of the diurnal motion of the earth, navigation, especially easterly and westerly, would be very tedious, and to make the whole circuit of the earth perhaps impracticable. 2. That the N. E. and S. E. winds within the tropics, must be compensated by as much N. W. and S. W. in other parts: and generally all winds from any one quarter, must be compen-

sated by a contrary wind some where or other; otherwise some change must be produced in the motion of the earth round its axis.

An Account of the several Earthquakes which have happened in New-England, since the first Settlement of the English in that Country, especially of the last Earthquake, Oct. 29, 1727. By Paul Dudley, Esq. F. R. S.* N° 437, p. 63.

That this country (New England) is subject to earthquakes, is certain; many instances of which have occurred since the first settlement of the English here, which now is about 100 years. The first and most considerable earthquake in our history, and which seems to have been much like the last, was June 2, 1638. This is said to have been "a great and fearful earthquake: it was heard before it came, with a rumbling noise or low murmur like distant thunder; it came from the north, and passed southward; as the noise approached near, the earth began to quake; and it came at length with such violence, as caused platters, tiles, &c. to fall down. The shock was so violent, that some persons without doors, could not stand, but were obliged to catch hold of posts, &c. In less than half an hour after, came another noise and shaking, but not so loud nor strong as the former: ships and vessels in the harbour were shaken, &c."

In 1658, there was another very great earthquake, but no particulars are related. In 1660, Jan. 31st, a great earthquake. In 1662, Jan. 26th, about 6 o'clock at night, there happened an earthquake, which shook the houses, caused the inhabitants to run out into the streets, and the tops of several chimnies fell down. About the middle of the same night was another shake; also in the morning following the earth shook again. In 1665, and in 1668, and 1669, the earth was shaken. Since which we have also had several tremors of the earth, but not very considerable, till the terrible earthquake, Oct. 29, 1727, which amazed and terrified the inhabitants from one end of the country to the other.

Gilbertus Jacchæus, in his Institutiones Physicæ, cap. Terræ Motus, distinguishes earthquakes into four species; in which he agrees with Aristotle and Pliny, with whom the first species is a shake or trembling, which they compare to the shaking fit of an ague. Our motion of the earth was not that which Aristotle and Pliny call a pulse or an intermittent knocking, but one continued shake or trembling; and therefore must be ranked under the first species, viz. a tremor or shake, without altering the position of the earth,

* See another account of this earthquake in p. 348, Vol. vi. of these Abridgments.

leaving all things in the same state, except the falling down of the tops of some chimnies, stone walls, &c. without doors; dishes and some other things within doors, &c.

That this earthquake was of the first species, is also proved from the sound that accompanied it; since tremulous and vibrating motions are proper to produce sounds. The noise that accompanied, or immediately preceded it, was very terrible and amazing. Some people took this noise to be thunder; others compared it to the rattling of coaches and carts on pavements, or frozen ground. One compared it to the shooting out of a load of stones from a cart under his window. Mr. Dudley himself, being perfectly awake, though in bed, thought at first the servants, who lodged in a garret over his chamber, were dragging along a trundle-bed: but indeed the noise that accompanies an earthquake seems to be sonus sui generis, and there is no describing it. This noise was instantly succeeded by a shake much more terrible. His house, which was large and well built, seemed to be pressed up together, as if a hundred screws had been at work to throw it down; and every thing in the house, particularly the bed, and the building itself, shook so violently, that there was great fear it would have tumbled down.

As to the degree of the shake; this will be best known from its effects. Besides some circumstances before mentioned, a country farmer said he had 40 or 50 rods of stone wall thrown down by it; another person walking abroad at the time, could hardly keep his legs: another that was riding says, that his horse stood still, and, during the shake, trembled so that he thought he would have fallen under him: some dogs barked, others howled and made strange and unusual noises. Nor was the earth only affected with this shake, but the sea also in the harbours, and the shipping were much moved by it.

The extent of the shake was felt from Boston to Kennebeck River to the eastward, and at Philadelphia to the westward, 150 leagues distant from each other on a W. S. W. and E. N. E. course nearest: and no part of the intermediate country, escaped it; the colonies of Rhode Island, Connecticut, and New York being all affected, though not equally, particularly at Philadelphia they write, it was a small shock.

A person at Boston, who had a well 36 feet deep, about 3 days before the earthquake, was surprised to find his water, which used to be very sweet and lympid, stink so that they could make no use of it; and thinking some carrion had fallen into the well, he searched the bottom, but found it clear and good, though the colour of the water was turned wheyish or pale. In about 7 days after the earthquake, the water began to mend, and in 3 days more it returned to its former sweetness and colour. It was also credibly asserted, that several springs and good watering places were some of them lowered, and others quite sunk and lost by the earthquake. A divine, in a town about 20 miles distant from Boston, said, that immediately after the earthquake, there was such a strong smell of sulphur, that the family could scarcely bear to be in the house for a considerable time that night; which is confirmed also from other places. Persons of credit also affirm, that just before, or in the time of the earthquake, they perceived flashes of light. A gentleman of probity, from Newbury, a town situate between 30 and 40 miles to the N. N. E. of Boston, writes, that at 40 rods distance from his house, there was a fissure of the earth, and near 20 cart-loads of fine sand thrown out where the ground brake, and water boiled out like a spring, and mixing with the sand, made a sort of quagmire; but at the date of his letter, which was the 21st current, the spring was become dry, and the ground closed up again. It is also said, that the ground where this sand is thrown up, and round about it for a considerable distance, is a solid clay for 20 or 30 feet deep, and nothing like sand ever to be found there before; so that the exhalation forced this great quantity of sand through a very deep stratum of clay.

Of an Extraordinary Effect of Lightning in communicating Magnetism. By Dr. Cookson of Wakefield in Yorkshire. N° 437, p. 74.

A tradesman at Wakefield in Yorkshire, having put up a great number of knives and forks in a large box, some in cases or sheaths, and others not, of different sizes, and of different manufactures, in order to be sent beyond sea; and having placed the box in the corner of a large room, there happened a sudden storm of thunder, lightning, &c. by which the corner of the room was damaged, the box split, and many of the knives and forks melted, the sheaths being untouched. The owner emptying the box on a counter where some nails lay, the persons who took up the knives, that lay on the nails, observed that the knives took up the nails. On this the whole number was tried, and found to do the same, and that, to such a degree as to take up large nails, packing-needles, and other iron things of considerable weight. Needles or other things placed on a pewter-dish, would follow the knife or fork, though held under the dish, and would move along as the knife or fork was moved; with several other odd appearances. Also, though the knives be heated red-hot, yet their power is still the same when cold.

A further Account of the extraordinary Effects of the same Lightning at Wakefield. By Dr. Cookson. N° 437, p. 75.

This storm of thunder and lightning happened the latter end of July, 1731, and not only broke the glass and iron frames of the cross-chamber windows, but at the same time split some studs in the corner of a wood-house, and passing into a room, split likewise a large deal box, which stood in the south corner of the room, where the lightning entered, and dispersed a great many dozen of knives and forks, which were put up in the box, all over the room.

On gathering up these knives and forks, some of them were melted, others snapped in sunder; others had their hafts burnt; others their sheaths either singed or burnt; others not; but what was most remarkable, on laying them on a counter where there were iron nails, rings, &c. it was observed, that when any of them were taken up, there hung a nail or ring at the end of each of them; most of them were tried, and found to do the same.

Query. The polarity of the compass has been altered by lightning, as is to be seen in the Philosophical Transactions: now how should lightning be capable of communicating such a power in this case, since it is plain that it has taken it away in another?

The Description and Use of an Arithmetical Machine invented by Christian Ludovicus Gersten, F. R. S. Professor of Mathematics at Giessen. N° 438, p. 79.

Sir Samuel Morland was, it seems, the first who undertook to perform arithmetical operations by wheel-work. To this end he invented two different machines, one for addition and subtraction, the other for multiplication, which he published in London, in the year 1673, in 12mo. He gives no more than the outward figure of the machines, and shows the method of working them. The last for multiplication, is merely an application of the Napierian bones on flat moveable disks; consequently his invention alone is not fit to perform justly all arithmetical operations.

After him the celebrated Baron de Leibnitz, the Marchese Poleni, and Mr. Leupold, attempted to perform it after different methods.

The first published his scheme in the year 1709, in the Miscellanea Berolinensia; giving however only the outer figure of the machine. Signor Poleni communicated his, but explaining at the same time its inner construction, in his Miscellanea of the same year 1709. Mr. Leupold's machine, with those of Mr. de Leibnitz and Signor Poleni, were inserted in his Theatrum Arithmetico-Geometricum, published at Leipzig in 1727, after the author's death, yet imperfect, as it is owned in the book itself.

Besides these, the French journals show that Charles Pascal invented one.

M. Gersten took the hint of his from that of Mr. de Leibnitz, which put him on thinking how the inner structure might be contrived. The structure is then described, and the mode of performing the arithmetical operations; but the whole so intricate and operose, as incapable now of exciting any attention.

Of the Figure of the Earth, and the Variation of Gravity on the Surface. By Mr. James Stirling, F. R. S. N° 438, p. 98.

The centrifugal force, arising from the diurnal rotation of the earth, depresses it at the poles, and renders it protuberant at the equator; as has been lately advanced by Sir Isaac Newton, and long ago by Polybius, according to Strabo, in the second book of his Geography. But though it be of an oblate spheroidical figure, yet the kind of that spheroid is not yet discovered; and therefore we may suppose it to be the common spheroid generated by the rotation of an ellipsis about its less axis; though by computation it appears, that it is only nearly, and not accurately such. Let us also suppose the density to be every where the same, from the centre to the surface, and the mutual gravitation of the particles towards each other to decrease in the duplicate ratio of their distances; and then the following rules will follow from the nature of the spheroid.

1. Let ADBE, fig. 5, pl. 2, be the meridian of an oblate spheroid, DE the axis, AB the diameter of the equator, and C the centre. Take any point on the surface, as F, from which draw FC to the centre, FG, perpendicular to the surface at F, meeting CB in G, and FH cutting the line CG, so that CH may be to GH as 3 to 2. Then will a body at F gravitate in the direction FH; and the mean force of gravity on the surface, will be to the excess of the gravity at the pole above that at F, as the mean diameter multiplied into the square of the radius, is to $\frac{1}{4}$ of the difference of the longest and shortest diameters multiplied into the square of the co-sine of latitude at F.

2. The decrement of gravity from the pole to the equator is proportional to the square of the co-sine of latitude; or, which comes to the same, the increment of gravity from the equator to the pole, is proportional to the sqaure of the sine of latitude. Hitherto we have considered the variation of gravity which arises from the spheroidical figure, while it does not turn round its axis; but if it does, the direction of gravity will be in the line FG, perpendicular to the surface; and its variation now arising from both the figure and centrifugal force, will be 5 times greater than what arises from the figure alone; as will appear

from the proportion of the lines FH and FG, the former being to the latter, as the whole force of gravity at F, while the spheroid is at rest, to the force with which a body descends at F, while it turns round its axis.

3. From this last article it appears, that $\frac{1}{5}$ of the variation of gravity is occasioned by the figure of the spheroid, and the remaining $\frac{4}{5}$ by the centrifugal force. And whereas the earth could not be of an oblate spheroidical figure, unless it turned round its axis, nor could it turn round its axis, without putting on that figure; therefore the diminution of gravity towards the equator, known by the experiments with pendulums, prove both the rotation and oblate spheroidical figure of the earth.

4. The mean force of gravity on the surface, is to the centrifugal force at any point F, as a rectangle under the radius and mean diameter, to a rectangle under the co-sine of latitude, and $\frac{4}{5}$ of the difference of the longest and shortest diameters. And at the equator, where the co-sine of latitude becomes equal to the radius, the mean force of gravity is to the centrifugal force, as the mean diameter to $\frac{4}{5}$ of the difference of the longest and shortest diameters. This article is found from the proportion of the lines FH and GH; the former being to the latter as the force of gravity to the centrifugal force.

5. The proportion of the diameters of the earth will be found in the following manner: the moon revolves about the earth in $27^d\ 7^h\ 43^m$, or in 39343 minutes: and her mean distance is about $59\frac{1}{2}$ semidiameters of the earth, according to La Hire's and Flamsteed's tables; but near $60\frac{1}{2}$ by Halley's tables. We shall therefore take 60 for the mean distance, till it be better known; then according to the nature of gravity, as the cube of the moon's distance is to the semidiameter of the earth, or as 216000 to unity, so is 1547870000, the square of the periodic time of the moon, to 7166, the square of the number of minutes in which another moon would revolve about the earth at the distance of its semidiameter. And as this last number is to 2062096, the square of 1436, the number of minutes in a sydereal day, so is unity to 287.7; which would show the proportion of the centrifugal force at the equator, to the mean force of gravity, by corol. 2, prop. 4, lib. 1, Princip. were it not for the action of the sun on the moon. Therefore, by corol. 17, prop. 66, lib. 1, Princip. as the square of the sydereal year is to the square of the periodic time of the moon, that is, as 179 to unity, so is 287.7 to 1.6; which being added to 287.7 makes 289.3. And therefore, as unity to 289, neglecting the fraction, which is uncertain, so is the centrifugal force at the equator to the mean force of gravity on the surface. And thence, by article 4, as 289 to $\frac{4}{5}$, so is the mean diameter to the difference of the longest and shortest; and therefore, as the axis is to the equatorial diameter, so is 2307 to 2317, or in smaller numbers, as 231 to

232, the same as Sir Isaac Newton found in a different manner; for he makes it as 230 to 231, and as 230 to 231, so is 231 to 232.004.

6. In the same manner the proportion of the diameters of any planet may be found, if it has a satellite; for instance, in Jupiter, he turns about his axis in $9^h\ 56^m$, or in 596 minutes: and his third satellite revolves about him in $7^d\ 3^h\ 42^m\ 36^s$, or in 10302.6 minutes, at the distance of 15.141 of his semidiameters. Therefore, as the cube of 15.141 to unity, so is the square of 10302.6 to 30579, the square of the number of minutes in which a satellite would revolve about him at the distance of his semidiameter; and as this last number is to 355216, the square of 596, so is unity to $11\frac{5}{8}$, or the centrifugal force at his equator, to the mean force of gravity on his surface. There is no need of correcting this number, as in the former article, because the periodic time of Jupiter round the sun is vastly greater than that of his third satellite round him. The third satellite is here chosen before any of the rest, because its greatest elongation was observed by Dr. Pound, with a micrometer adapted to a telescope 123 feet long; and he also took the diameter of Jupiter by the transit of the satellite, which is a much more exact way than with a micrometer. But as the planes of Jupiter's satellites almost coincide with the plane of his equator, the diameter, determined by the transit of the satellite, is his greatest; and the distance of the satellite, which ought to have been given in his mean diameters, is assigned in his greatest; for which reason the force of gravity already found, must be augmented in the triplicate ratio of his greatest diameter to his mean one; that is, if a represent the mean diameter, and d the difference of the longest and shortest, in the proportion of $2a + 3d$ to $2a$ very nearly. Hence, as the centrifugal force at his equator, is to the mean force of gravity on his surface, so is unity to $11\frac{5}{8} \times \frac{2a + 3d}{2a}$. And, by article 4, $11\frac{5}{8} \times \frac{2a + 3d}{2a} : 1 :: a : \frac{4}{5}d$, or $20\ aa = 186ad + 279dd$; which makes a to d, as 108 to 10; and hence the axis is to the equatorial diameter, as $108 - 5$ to $108 + 5$, or as 103 to 113; that is, as 12 to $13\frac{1}{6}$; which agrees nicely with the observations of both Dr. Pound and Mr. Bradley, made with Huygens's long telescope; the former making it as 12 to 13, and the latter as 25 to 27, which is very nearly the same. And if this theory agrees so well with observations in Jupiter, there is no doubt but it will be more exact in the earth, whose diameters are much nearer to equality.

7. By experiments made at Jamaica, Philos. Trans. N° 432, in the latitude of 18°, with a very curious clock, contrived by Mr Graham, it was found that the London pendulum went slower there by $2^m\ 6^s$, in a sydereal day, than at London. But it was found by experiments made with thermometers, that 9^s

were to be allowed for the lengthening of the pendulum by heat; and therefore it was retarded only $1^m\ 57^s$ by the decrement of gravity. So that while a pendulum of London makes 86164 vibrations, the number of seconds in a sydereal day, the same at Jamaica gives only 86047 vibrations. Therefore the force of gravity at London, is to that in the latitude of 18°, as the square of 86164, to the square of 86047; that is, very nearly as 1106 to 1103. And, by article 1 and 2, if a denote the mean diameter of the earth, d the difference of the greatest and smallest, then $a - \frac{ccd}{rr}$ will denote the force of gravity in general in any latitude, whose co-sine is to the radius as c to r; where, if instead of c there be substituted the co-sines of 51° 32′ and 18°, that is, of the latitudes of London and Jamaica, we shall have the force of gravity at the former, to that at the latter, as $a - 3870d$ to $a - 9045d$, that is, as 1106 to 1103. Hence the mean diameter of the earth, will be to the difference of the axis and equatorial diameter, as 191 to unity; and thence, by article 4, as the mean gravity on the surface, is to the centrifugal force at the equator, so is 191 to $\frac{4}{5}$, or so is 239 to unity. In order to show that this cannot be, we may observe, that when the moon's distance was supposed 60 semidiameters of the earth, as in article 5, it was found that the mean force of gravity was to the centrifugal force at the equator, as 289 to 1. But if the proportion now found be true, the moon's distance of 60 semidiameters must be augmented in the subtriplicate ratio of 289 to 239, and then it will become 64 semidiameters. In like manner, if we compute the ratio of the mean force of gravity to the centrifugal force, by presupposing the magnitude of the earth, as Sir Isaac Newton and Mr. Huygens did, we must suppose a degree to be above 80 English miles, to bring it out 239 to unity. Now whereas it is certain that the distance of the moon is about 60 semidiameters of the earth, and that a degree is less than 70 English miles; therefore, that the conclusion, which seems to follow from the Jamaica experiment, cannot be allowed to be true. And the experiments made by Richer, in the island of Cayenna, would still make a greater difference between the diameters of the earth, than those made in Jamaica. And the lengths of the Paris and London pendulums compared together, would make it greater than $\frac{1}{231}$ part of the whole, as it was found in article 5.

8. From all the experiments made with pendulums, it appears that the theory makes them longer in islands than they are found in fact. The London pendulum should be longer when compared to the Paris one, than it really is; the Jamaica pendulum, when compared to the London one, which vibrates in a greater island, should be longer than is found by experience; and the pendulum in Cayenna, a smaller island than Jamaica, should be still longer. This defect

of gravity in islands is very probably occasioned by the vicinity of a great quantity of water, which being specifically lighter than land, attracts less in proportion to its bulk. And we find by computation, that the odds in the pendulums, between theory and practice, is not greater than what may be accounted for on that supposition. We may also observe, that though the matter of the earth were entirely uniform, yet the hypothesis of its being a true spheroid is not near enough the truth to give the number of vibrations which a pendulum makes in 24 hours. And suppose the true figure were known, the inequalities of mountains and vallies, land and water, heat and cold, would never allow theory and experiments to agree. But after the French gentlemen, who are now about measuring a degree, and making experiments with pendulums in the north and south, shall have finished their design, we may expect new light in this matter.

Of the Mexican Filtering Stone. By Dr. Abraham Vater, F. R. S. &c. N° 438, p. 106. *Abridged from the Latin.*

This stone has the name filtre from its porosity, by which it suffers liquors to pass through it: and for this reason pots and mortars are made from larger pieces of it, to strain liquors, particularly water to drink: for it is thought, that the water filtered through this stone is freed from all its impurities, and becomes clearer and purer, and more wholesome. Hence these stones are highly valued in Japan, and sold at the price of gold; because the Japanese, who know nothing of the stone or any other disorder in the kidneys, and who prefer health far before all other blessings, are of opinion, that these petrified fungi have the power of prolonging life. This species of fungus, it is said, grows on the rocks in some places of the gulph of Mexico, about 100 elns under water, and spontaneously hardens and petrifies in the air. Dr. Vater does not attempt to determine the origin of the filtre stone, nor its productions, though both appear to be very suspicious, and invented only to prevent its being thought a common stone. For Lentilius writes that there are vessels made of two sorts of it; one of a dark grey colour, like the lapis scissilis from Canada, and sold at a dearer rate; and others of a tophaceous colour, of the growth of Italy. And, according to Le Clerc, in his physics, it is likewise dug up in the bishopric of Liege, and much used in Holland. Dr. Ehrhart of Memmingen, presented Dr. Vater with a choice collection of fossils, among which was a tophus very porous, found about Memmingen, and which he was assured would strongly imbibe water. For, no sooner does it touch the surface of the water, but the water ascends, and is carried quite through its porous substance, as in sugar, salt, filtering paper and sponge. This immediately suggested the hint, whether it might not be used instead of the Mexican filtre to strain water. He accordingly made a hollow in a little bit of it, and on pouring water into it, it strained very fast through the pores. He then took the tophaceous tubes of osteocolla, and stopping one extremity, he poured water on it; when it transuded very fast through its porous substance. He also recollected that he had a sponge for several years, which when he lived at the Caroline bath, he had put in a pipe that conveyed the hot waters, and by this means the sponge being incrustated with the ochre, which the hot waters carry along with them and deposit in their passage, degenerated into a tophus: he made a pit in the sponge, and filled it with water, when it ran very fast through it. On this he resolved to make trial with a tophus of the hot bath, of which he had a pretty large piece; and for this purpose he had it hollowed into a mortar, to see whether the water would pass through that dense and solid stone: and it answered his expectation; as the water strained through in the same manner as through the Mexican filtre and other tophi; but by reason of the density of the stone, slower than through more porous stones.

Hence the Dr. thought, that the tophus of the hot baths is generated from the water depositing its ochre, in flowing through the pipes, and is insensibly concreted; in like manner might the sea, beating on the rocks, deposit saline earthy particles, from whose successive concretion this stone is generated, and rather grow on the rocks, than like rock-mushrooms, spring from them. But considering the remarkable density of the hot bath tophus, through which water filtrates, he had a mind to try the same experiment with the common stone used in building. The success answered expectation: for, a mortar made of such stone served instead of the Mexican filtre, the water straining equally clear through both. The water strained in this manner acquired at first an earthy taste, which yet on repeated filtration it lost.

As to the purifying quality of these filtres, the Dr. does not deny, but that muddy and slimy waters may, by straining through such stones, become clear and pellucid; because these impurities do not dissolve in the water or intimately incorporate with it, but only float in it. But besides these, no other waters can by any means become purer, as he learned from repeated experiments, both with the filtre from Holland, and with those made from the tophus of the Caroline hot baths and common stone, on several kinds of river and spring water; and with an hydrometer examining their weight both before and after filtration, he found little or no difference.

A Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands, by Mark Catesby, F. R. S. With some Extracts out of the Seventh Set. By Dr. Mortimer, R. S. Secret. N° 438, p. 112.

This 7th set consists of the description of fishes.

A Halo observed at Rome, Aug. 11, 1732. By Sig. De Revillas. N° 438, p. 118. *From the Latin.*

From 9 o'clock in the forenoon, till 2 in the afternoon, a simple halo was observed to surround the sun. It was exactly circular, and well defined; and its breadth equal to the sun's apparent diameter. The innermost colour was red; the rest pretty dilute, and analogous to those in the rainbow, but terminating in a whitish brightness.

Concerning an Ancient Date found at Widgel-Hall in Hertfordshire. By Mr. John Cope. N° 439, p. 119.

Fig. 4, pl. 2, represents an ancient chimney-piece, as Mr. Cope was informed, found on pulling down part of Widgel-Hall in Hertfordshire. There is cut on it a date expressed part in Roman numerals, and part in Indian figures; which is the earliest instance he has met with of the Indian figures being used here in England, viz. 𝔐. 16, or 1016; that at Colchester being in the year 1090. See Philos. Trans. N° 266. The carving is very fair, the letter 𝔐 and the figure project out above a quarter of an inch. The whole chimney-piece is of English oak plank, and is now very firm, though 718 years old, and was never painted over; it is 4 feet $3\frac{1}{2}$ inches long; the part under the 16 was broken off in taking it down in August, 1733, when the house was on fire. * * *

Remarks on the foregoing Ancient Date, found at Widgel-Hall near Buntingford in Hertfordshire, on an Oaken Plank. By John Ward, Rhet. Pr. Gresh. and F. R. S. N° 439, p. 120.

April 4, 1734, a curious draught of an ancient date, carved in an oaken plank, at Widgel-Hall, the seat of Francis Gulston, Esq. was laid before the Royal Society, as the most early instance of our common figures, usually called Arabian, which had ever been observed in England. It was read 𝔐16, and thought to express the year 1016, the 𝔐 being taken for a Roman numeral, and the 16 for Arabian figures.

Doctor Wallis had, in 1683, communicated to the R. S. the draught of a mantle-tree, somewhat like this, which he saw at the parsonage-house at Helmdon in Northamptonshire. The date, which was likewise carved in mixed characters, expressed the year 𝔐133, as the Doctor read it. This being the oldest monument of that sort, which had then been discovered among us, was published first in the Philos. Trans. N° 154, and afterwards in the Doctor's Algebra, cap. 4, p. 14.

In 1700, another draught of a date at Colchester, which had been sent to Dr. Wallis by Mr. Luffkin, who copied it from the under cell of a wooden window, and read the figures 1090, being all Arabian, was printed likewise in the Transactions, N° 266, as more ancient than the former.

None earlier than these last two had yet appeared, till that from Widgel-Hall. On the sight of this, Mr. Ward thought the reading given to it looked very plausible. The mixed characters were no just objection, which Dr. Wallis had accounted for in the Helmdon date, and Mr. Ward himself observed in some manuscripts. Yet one difficulty seemed to remain, which was the want of some character in the place of hundreds. And therefore soon after going into Hertfordshire, he took that opportunity to wait upon Mr. Gulston, in order to see the original.

That gentleman afterwards informed him, by letter, that the house was always esteemed ancient: that before it was burnt, on the timbers there were several old coats of arms; some were considered as belonging to the family of the Scalers, who were possessors of Widdihale, with other estates, soon after the conquest; and at the time of the conquest it was in the possession of a considerable follower of Harold.

Widdihale, in Hertfordshire, in the time of the Conqueror was parcel of the estate of Hardwin de Scalers, as appears by Domesday Book, fol. 141. It continued in that family for several generations, till it came to Anthony Widvile, by the marriage of the daughter and heir of Scalers. But when he would not comply with Richard the Third to destroy the young Princes, all his lands were seized, and the manor continued in the crown, till Henry the Eighth granted it to George Canon and John Gill: George Gill, the son of John, marrying the daughter of George Canon, obtained the whole. In this family it continued till the beginning of the reign of James the First, when it was sold to John Goulston, Esq. whose descendants now hold it.

The piece of timber was the top of a door-way, in a timber built house, and plastered over with mortar. From the date on the plastered wall, the door had not been used at least 343 years; for on the outside was plainly to be seen the date 1390. Part of the room this was found in, was burnt too much to repair again.

On considering the characters on this plank, and those of the other two dates mentioned above, with the accounts given by learned men of the time when the Arabian figures were first introduced into these parts of the world, and the various forms they have since received, as exhibited in fig. 4, pl. 2. Mr. Ward was at last satisfied, that none of these 3 dates prove they were ever used among us, in less than 100 years after the reading given to the latest of them.

Most writers, who have treated of the use of these figures, have thought they came first from the Persians or Indians, to the Arabians, and from them to the Moors, and so to the Spaniards, from whom the other Europeans received them. This was the opinion of John Gerard Vossius, (De Natura Art. lib. iii, cap. 8, § 6,) Mr. John Greaves, (De Siglis Arabum et Persarum Astronomicis, p. 2, where the form of them may be seen,) Bishop Beveridge, (Arithmet. Chronolog. lib. i, cap. 5,) Dr. Wallis, (De Algebra, cap. 3, p. 10,) and many others. And the Arabians themselves acknowledge that they had them from the Indians, as both Dr. Wallis (Ibid. p. 9), and Mr. Greaves (De Siglis Arabum, &c.) have shown from their writers.

But Isaac Vossius thought the ancient Greeks and Romans were acquainted with these figures, and that the Arabians took them from the Greeks, and the Indians from the Arabians, (Observat. ad Pomp. Mel. p. 64). For the proof of this he refers to Tyro and Seneca's Notes, (Vid. Grut. Inscript. vol. ii, ad fin.) and the treatise of Boethius De Geometria, (Lib. i, sub. fin.) But as to the notes of Tyro and Seneca, they seem to have no affinity with these figures, either in the number or nature of them; for they are not limited to 9, but are many times that number, and all different in form. Nor are they simple signs of numbers, but complex characters of several letters of those numeral words which they stand for in the Roman language, like our short-hands; and therefore vary in their shape, as they are designed to express cardinals, ordinals, or adverbs of number. This will appear by the table of characters annexed to these papers, in which are given the first 10 of each. But as to what Vossius says concerning Boethius, Mr. Ward observed in a curious manuscript of that writer, now in the library of Dr. Mead, nine characters, which he says were invented and used by some of the Pythagoreans in their calculations; while others of them made use of the letters of the alphabet for the same purpose. Boethius calls them apices vel characteres, (Lib. i, sub. fin.) These also are inserted in the table, to show the great affinity between them and the Arabian figures, as these latter were written two or three centuries since.

The opinion of Daniel Huetius differed from either of the former; for he imagined, the Arabian figures were only the letters of the Greek alphabet, corrupted and altered by ignorant librarians, (Demonstrat. Evangel. prop. iv. c. 13, p. 172.)

From this summary account of the rise and antiquity of these figures, it seems probable that they might owe their original to the Greeks, those common masters of all science, and passing from them first to the eastern nations, come round to these western parts, in the manner before described. We have no other author, who speaks of this matter, near so ancient as Boethius, whose words are very express, and much strengthened by the similitude of his characters with the Arabian figures. And therefore we may rather suppose they took their rise from these, than from the small Greek letters, with which Huetius compared them; since these latter are neither so like them, nor so old as the time of Boethius. And though what the Arabians say may be true, that they had them from the Indians, and not the Indians from them, as Isaac Vossius conjectured; yet it may be equally true, that the Indians had them first from the Greeks, and those Arabian writers, who are not very ancient, not have known it; nor are there any Indian monuments of sufficient antiquity to render this opinion questionable.

But whichever of these suppositions may be esteemed the most credible, with respect to the origin of these figures; Joseph Scaliger thought they were not received by the Europeans, as they came of later ages from the Arabians, long before the year 1300, (Lib. iii, Ep. 223.)

But John Gerard Vossius, was of the opinion they began to use them about the middle of the 13th century, or the year 1250, (De Natur. Art. lib. iii, cap. 8, § 7.)

Father Mabillon, in his treatise De Re Diplomatica, was necessarily led to attend to the use of these figures, particularly in dates. And he informs us, that they were rarely used before the 14th century, except in some few books of geometry and arithmetic. And presently after he says, it was not much to his purpose to treat of them, since he did not design to carry his work lower than the 13th century, (Lib. ii, c. 28, § 10.) By which he seems to intimate, that he had met with very few, if any, instances of Arabian figures, in such instruments at least, before the year 1300.

But no one appears to have examined this subject more carefully than Dr. Wallis; who has offered some arguments to prove that Gerbert, a monk, who was afterwards advanced to the papal see, and took the name of Sylvester II, had before the year 1000 learned the art of arithmetic, as now practised, with the use only of 9 characters (whatever their form then was) from the Saracens in Spain, which he afterwards carried into France, (De Algebra, c. 4, p. 17). But the Doctor thinks those characters or figures were known for a long time after only to such artists, and principally used by them in astronomical calculations; the Roman numerals being still retained in common use to express smaller numbers, (Ibid. p. 11, 15, 16.) Nor has he given the figures used by any of those writers before Johannes de Sacro Bosco, who died in the year 1256; and Maximus Planudes, a Greek, who flourished after him; which are here copied from him, in fig. 4.

Mr. David Casley, in his Catalogue of the Manuscripts of the King's Library, &c. has published a specimen of a manuscript from the Cottonian Library, called Calendarium Rogeri Bacon, (Plate xv.) and dated 1292. The figures in this book are Arabian, and, as Mr. Casley says, the oldest that he remembers to have met with in either of those libraries: for which reason they have a place also in the table.

It appeared exceedingly difficult, how to reconcile the opinions and observations of these several writers, concerning the first use of the Arabian figures in these western countries, with the time assigned even to the latest of the dates above-mentioned. And it could not but seem very strange, that no date of any writing should have been produced in those figures, or any other use of them discovered (except perhaps in some mathematical calculations, or books of arithmetic) long before the 14th century; and yet that a date should be found, so carved in a piece of wood, before the middle of the 12th century, for so common a purpose as the mantle-tree of a chimney.

But on a closer examination of the characters, Mr. Ward found reason to think, this was not really the case; and that instead of 1133, they ought to be read 1233, what has been taken for a 1, being designed for a 2. This reading seems to be confirmed by the shape of the two 33 that follow it, from which, if the bottom curve towards the right hand (as it was often made formerly) was taken off, the upper part would make the 2. Which agreement between those figures is not only usual at present, but often found in manuscripts of the 14th and 15th centuries. Though sometimes indeed it is otherwise; and the 2 has an angle at the top, when the 3 is round, which would not so well have suited this square hand. The reason which occasioned the carrying this date so high, has probably been the similitude between the small i over the preceding abreviated word domini and this 2. And he believes this date may claim the preference of being the oldest of the sort that has hitherto been discovered.

The antiquity ascribed to the Colchester date, namely 1090, has, it seems, been occasioned by a mistake in the copy; for the 0 in the place of hundreds, should have been made a 4, by drawing down an oblique stroke on each side from the bottom, which makes it 1490, before which time the 4 had long received that shape.

As to the date from Widgel-Hall, which gave occasion to this inquiry, it seems plainly intended to express the year 1000, and no more, by the Roman M in the escutcheon on the right side. For the characters in the other escutcheon cannot, Mr. Ward thinks, stand for figures, but must be the initial letters of two names I. G. as W. R. in the Helmdon date; and were very probably designed in both to denote the persons who erected those buildings. The omission of a character in the place of hundreds, is still an argument, that these last two were not made for figures. But what seems to put the matter past all doubt, is the want of evidence that the figure 6 had received that form till some ages afterward: and when it was introduced, the upper part was not at first made so erect, as it is here, but carried in a small arch just over the top of the circle. On the other hand, what looks here like the modern 6, was at that time the usual form of the capital G. This Mr. Ward found fully confirmed by a large collection of original grants, made by our ancient kings and others, and preserved in the Cottonian Library, (Augustus II.) On consulting these for half a century at least, both before and after the year 1016, the G is so written in a great number of them. For these reasons therefore he makes no question, but that character was designed for a G, and not a 6. And it is plain from other circumstances in Mr. Gulston's letter, that the building might very probably be as ancient as the year 1000; which renders this relic of it, considering how firm and sound it still is, a remarkable curiosity.

The use which may be made of these observations, is this: that so far as yet appears, any coin, inscription, or manuscript, with a supposed date before the 13th century, expressed in Arabian figures, may be justly suspected either not to be genuine, or not truly read; unless its antiquity be certain, from other clear and undoubted circumstances, and the date will bear no other reading: and, if it be a copy, that it has been taken with exactness.

Some Considerations on the Antiquity and Use of the Indian Characters or Figures. By Mr. John Cope. N° 439, p. 131.

The ingenious invention of figures by the sagacious Indians, is of such vast importance in numbering, that it can never be too much admired, though now their use is become so familar among us, that very few consider what a loss the want of them would be to people of every station in life: for, to consider only, that such a number as not long before the conquest would take up a good arithmetician whole days to count by the literal characters, is now by the help of figures commonly expressed by a child in a few minutes. This consideration of the vast use of figures, put Dr. Wallis, and others since him, on inquiring at what time they were first happily introduced into this island.

Dr. Wallis informs us, that we had the figures from Spain, into which nation they were brought by the Moors. The Moors had them from the Arabians; and the Arabians from the Indians. And it was the Doctor's opinion, that they were first brought into England about the year 1130; for that the first instance of their use which he had met with, was a date upon a chimney-piece, which date was 𝔐 133, the character 𝔐 which the Romans used to express 1000, being mixed with figures, as Dr. Wallis observes, was often done at their first coming in; and since that, in Philos. Trans. N° 266, is mentioned a date 1090, all in figures.

Lately too Mr. Cope produced a date on a chimney-piece at Widgel-Hall in Hertfordshire, which was 𝔐 16, the 𝔐 for the 1000, being here again mixed with figures. And he now produces a still earlier instance of the use of figures in England, being a draught of an inscription over a gateway at Worcester, built, it is believed, in the reign of King Edgar, and is this 9ʅ𝔙, 975, which is 158 years before the date of Dr. Wallis's, 41 years before that Mr. Ward produced last year, and is now 760 years standing. The account of this date Mr. W. had given him by Mr. Joseph Dougharty of Worcester, who is an ingenious and reputable person, and lives in the house over the gate-way on which this inscription is: he also said, that his house goes by the name of the oldest house in five counties; and it is the current opinion there, and reported by the ancient people in that place, that the house was built by King Edgar, where they say —— he sometimes kept his court: and all historians agree that Worcester was then a very considerable bishopric; and that Dunstan and Oswald, who were both successively bishops there in Edgar's time, were both his great favourites, especially Dunstan, for whom King Edgar had a very great regard: for it appears that the first thing Edgar did after he came to the crown, was to recall Dunstan from Flanders, where he had been 3 years in exile, and was immediately made prime minister, favourite, and confessor, as first bishop of Worcester, and afterwards Archbishop of Canterbury; on which last promotion his great friend Oswald succeeded him in the See of Worcester: and it is very likely that either Dunstan or Oswald, as having so much power, interest and riches, might erect a building there, of which this gate-way might have been a part; for as Edgar died in the same year 975, if we suppose the date to be fixed on the building the year it was finished, as is now commonly done, Edgar could not live or keep his court there, unless it was in some part of that year in which we suppose it to be finished.

Remarks on the foregoing Ancient Date, over a Gate-way, near the Cathedral, at Worcester. By John Ward, F. R. S. N° 439, p. 136.

Mr. Ward having lately communicated to the Royal Society some remarks on an ancient date, carved in wood, that was found at Widgel-Hall near Buntingford in Hertfordshire, with the characters 𝔐 16; which had been read 1016, supposed to be mixed numbers, the 𝔐 Roman, and the two others Arabian or Indian, as they are indifferently called. This led him to consider two other dates of the like kind, formerly published in the Philos. Trans.; one found at Helmdon in Northamptonshire, in mixed characters expressing, as was thought, 𝔐 133; and the other at Colchester, said to denote the year 1090, wholly in Arabian figures. But on searching into the origin of those figures, and the time when they were first brought into these parts of the world, he could meet with no examples of them in any manuscripts, before some copies of Johannes de Sacro Bosco, mentioned by Dr. Wallis, who died in the year 1256, which was 123 years after the latest of the three dates above-mentioned. As it could not therefore but seem very strange, that workmen should have made use of those figures for such common purposes, so long before they appear in the writings of the learned; so on a closer examination, and further inquiry, he found there was no reason, from any of these dates, to suppose it was really true in fact. For the Helmdon date, instead of 𝔐 133, should be read 𝔐 233; the Colchester date 1490, instead of 1090; and that at Widgel-Hall has no Arabian figures in it, the characters 1 and 6 not being numbers, but the initial letters of two proper names I G, in the usual form of those letters in that age.

But there had been soon after read before the Society, an account of a date at Worcester, more ancient than any of the 3 former; namely 97𝔳, or 97v, in which the unit is a Roman numeral, and the other two are taken for Indian figures. Now Mr. Ward observed in his former paper, that such mixtures were sometimes found in ancient numbers; though in what manner they were so used, he did not then explain, but for brevity contented himself with referring to Dr. Wallis's Algebra. The Doctor thought it necessary to take notice of this, in order to account for his way of reading the Helmdon date, in which the 𝔐 only is a Roman numeral. And Mr. Ward had met with a few instances of it in Dr. Mead's manuscript of Boethius, as ccc29 and DCC68, where the hundreds are numeral letters, and both the decimals and units Arabian figures, (De Arith. lib. ii.) But it is observable, that this is not done promiscuously; for the larger numbers are always letters, and the less figures; as in the Helmdon date. And Mabillon has observed, that in a curious manuscript copy of Thomas à Kempis, written in the 15th century, some of the pages are so numbered, (De Re Diplom. tab. xv.) Which method, so far as appears, was always attended to, and never in any one instance inverted. So that this Worcester date, which has a Roman numeral in the place of units, and the two preceding characters are supposed to be Indian figures, is not only without example, but directly contrary to all other instances of such mixed numbers. Which consideration alone might be of sufficient ground to think, there must be some mistake in the reading.

But the middle figure, taken for a seven, is as remarkable; which turning towards the left hand, forms two obtuse angles, one above, and the other below. This shape of the seven was never seen before, and seems by no means to suit that age. In the specimen of the figures taken from Johannes de Sacro Bosco, by Dr. Wallis, the figure seven is made in this form Λ, like the two legs of an isosceles triangle. And in Roger Bacon's Calendar, dated 1292, there is only this variation, that the leg to the left hand is somewhat shortened. And this form continued till printing was introduced among us; as is evident from Caxon's Polychronicon, and other books printed about that time. Nor is it found till later times in any other shape; unless that in Bishop Beveridge's table of Indian figures, the two legs of our ancient seven are drawn parallel, and arched at the top, instead of meeting in an angle; (Arith. Chron. lib. i, cap. 4.) and Planudes, a Greek writer, has kept the true Arabian form V, like the Roman five, which the Europeans inverted. The last alteration this figure received among us, was by raising the shorter leg horizontally. But no instance of it parallel to this in the Worcester date, or any thing like it, has before appeared. As there seems therefore no reason to suppose it a seven; so a probable conjecture may be offered, what it was designed for, and that is, the Roman numeral ten, which was made in this form, like an X; to which character, in our old square hand, this supposed seven would very well agree, by supplying only the two extreme parts to the right hand, in this manner X, which may easily be thought to have been decayed, and worn away by length of time.

As there is no reason to take the middle character for seven, so neither is there any to suppose the first was intended for a nine, being thus placed before two Roman numerals, as Mr. Ward takes them both to be. It has indeed some similitude with that figure; but that is nothing more than what was anciently, and still is, common to the letter ꝏ in that hand, which resembles a double O, with an oblique stroke turned inwards from the bottom of that to the right hand; so that if the other to the left be taken away, that which remains will appear in this form O, like what is here called a nine. And every one knows, who has any acquaintance with ancient inscriptions, that letters frequently perish in this manner, one part before another.

Upon these suppositions the true reading would be MXV. But since the old date is now destroyed, and modern figures put in its place, this must remain uncertain. But though the precise year of this date cannot now be determined with certainty, it is sufficient to have shown, that neither the order of the characters, their shape, nor the oldest examples of Arabian or Indian figures, any where found, do in the least countenance the reading given to it; but, on the contrary, all of them afford the highest probability, that it cannot be genuine.

On the Description of Curve Lines. By Mr. Colin Maclaurin, Math. Prof. Edinb. F.R.S. N° 439, p. 143.

Mr. Maclaurin was informed that some papers had lately been presented to the Royal Society, concerning the description of curves, in a manner that has a near affinity to that which he communicated to them formerly, and had carried farther since; and that it would not be unseasonable, nor unacceptable, if he should send an account of what he had done further on that subject since the year 1719. The author* of those papers taught mathematics at Edinburgh privately for some years, and some time ago, viz. in 1727, mentioned to Mr. M. some theorems he had on that subject; which, at the same time, Mr. M. showed him in his papers. Some time before that, he showed him a theorem which coincided with one of those in Mr. M.'s book, though he seemed not to have observed that coincidence; and indeed methods of that kind, are often found coincident that do not appear such at first sight. Mr. M. is unwilling to be the occasion of discouraging any thing that is truly ingenious, and renounces any pretensions of appropriating subjects to himself; but on the contrary, wishes justice may be done to every person, or to any performance in proportion to its merit; yet finding it fit he should take precautions, lest any one should take it in his head afterwards to say, he takes things from him which he may have had long before him; and therefore Mr. M. sends the following abstract of what he had done in this matter since the year 1719.

Mr. M. has so much on this subject by him, that he declares himself at a loss what to send; but at present he only gives an abstract of those propositions, which he takes to be more nearly related to those which this author has offered to the society from the conversations he had with him. In 1721, Mr. M. printed several sheets of a supplement to his book on the description of curve

* Mr. Braikenridge. See p. 5, of this volume.

lines, which he had not published, having been engaged for the most part in business of a different nature, and in pursuits on other subjects since that time. He first gives an abstract of that supplement, as far as it was then printed, and subjoins an account of some theorems he added to it the following year, viz. in 1722. He was led into those new theorems by Mr. Robert Simson's giving him at that time a hint of the ingenious paper, which has been since published in the Philosophical Transactions. Mr. M. had tried, in the year 1719, what could be done by the rotation of angles on more than two poles; and had observed, that if the intersections of the legs of the angles were carried over right lines, as in Sir Isaac Newton's description, the dimensions of the curve were not raised by this increase of the number of poles, angles, and right lines; and therefore he neglected this at that time, as of no use to him, confining himself to two poles only, and varying the motions of the angles as in his book. He found this by inquiring in how many points the locus could cut a right line drawn in its plane, and found, by a method often used in his book, that it could meet it in two points only.

Having found then, that three or more poles, were of no more service than two, while the intersections were carried over fixed right lines; he thought it needless to prosecute that matter then, since by increasing the number of poles, his descriptions would become more complex, without any advantage. But in June or July, 1722, on the hint he got from Mr. Simson of Pappus's porisms, he saw that what he has there ingeniously demonstrated, might be considered as a case of the abovementioned description of a conic section, by the rotation of any number of angles about as many poles; the intersections of their legs in the mean time being carried over fixed right lines, excepting that of two of them which describes the locus. For by substituting right lines instead of the angles, in certain situations of the poles and of the fixed right lines, the locus becomes a right line; as for example, in the case of three poles, when these three are in one right line, in which case the locus is a right line, which is a case of the porism.

It was this that led him to consider this subject anew; and first he demonstrated the locus to be a conic section algebraically; and found theorems for drawing tangents to it, and determining its asymptotes. He also drew from it at that time a method of describing a conic section through five given points.* This encouraged him to substitute curves for the right lines, to see if by this method he could be enabled to carry on his theorems, about the descriptions of

* The paper on this subject I have, says Mr. M. is dated July 31, 1722, at sea, being then in my way to London, going for Cambray. Orig.

lines through given points, to the higher orders of lines. Some of the theorems he found at that time accompany this. In November 1722, looking into Sir Isaac's Principia, he saw that the description of the conic sections by three right lines, moving as above, about three poles, could be immediately drawn from his 20th lemma, which itself is a case of this description. This gradually led him to seek geometrical demonstrations for the whole, as far as it related to the conic sections. He sent some leaves of this paper, dated at Nancy, November 1722. Since that time, he had not added much to this subject, but what relates to drawing tangents, determining the asymptotes, and the puncta duplica, or multiplicia of these curves. He considered it the less, as he did not find it more advantageous in any respect, than the method he had considered in his book, nor more general.

In 1727 he added to a chapter in his algebra, an algebraic demonstration of the locus, when three poles are employed; and the method of describing a conic section through five given points, subjoining at the same time, that if more poles are employed, and angles or right lines, the locus was still a conic section; which he thought was a remarkable property of the conic sections, not observed before.

These things he intended to put in order, and publish in the supplement to his book, a part of which had been printed since the year 1721. He intended also to give several other things in that supplement; two of which he only just mentions at present, as they are foreign to the present affair. He subjoins a problem determining the figure of a fluid, whose parts are supposed to be attracted to two or more centres; and a solution of a general problem about the collision of bodies.

The author of the papers given in to the Royal Society will not deny, that Mr. M. showed him the theorems, now sent, in 1727. He owned it last summer at least; Mr. M. intended to publish these very soon. Whether he has carried the subject farther, he leaves to the judgment of the gentlemen to whom they were referred. As to the demonstrations, it would take some time to put them in a proper form to be published. He could send those that are algebraic easily; but did not care to send those that are geometrical, till more leisure.

An Abstract of what has been printed since the Year 1721, *as a Supplement to a Treatise concerning the Description of Curve Lines, published in* 1719, *and of what the Auuhor proposes to add to that Supplement. By Mr. Maclaurin.* N° 439, p. 148.

I. In the first part of the supplement, a general demonstration is given of the theorem, that if two lines of the orders or dimensions, expressed by the numbers m and n, be described in the same plane, the greatest number of points in which these lines can intersect each other, will be mn, or the product of the numbers which express the dimensions of the lines, or the orders to which they belong.

II. In the next part, theorems are given for drawing tangents to all the curves that were described in that treatise by the motions of angles on given lines. Their asymptotes are also determined by more simple constructions than those which are subjoined to their descriptions in that treatise. Of these we shall give one instance here.

Suppose the invariable angles, fig. 1 and 2, pl. 3, FCG, KSH, to revolve about the fixed points or poles, C and S. Suppose the intersection of the two sides CF, SK, to be carried over the curve BQM, whose tangent at the point Q is supposed to be the right line AE; and let it be required to draw a tangent at P to the curve line described by P the intersection of the other two sides CG and SH.

Construction.—Draw QT constituting the angle SQT, equal to CQA, on the opposite side of SQ, that QA is from CQ; and let QT meet CS, produced if necessary in T. Join PT, and constitute the angle CPN equal to SPT, on the opposite side of CP, that PT is from SP; then the right line PN shall be a tangent at P, to the curve described by the motion of P, which is always supposed to be the intersection of CG and SH.

The asymptotes of the curve, described by P, are determined thus. Find, as in the abovementioned treatise, when these sides become parallel, whose intersection is supposed to trace the curve; which always happens when the angle CQS becomes equal to the supplement of the sum of the invariable angles FCG, KSH, to four right ones; because the angle CPS then vanishes. Suppose, in fig. 3 and 4, that when this happens, the intersection of the sides CF, SK is found in Q.

Constitute the angle SQT equal to CQA, as before, and let QT meet CS in T. Take CN equal to ST, the opposite way from C that ST lies from S. Through N draw DN parallel to CG or SH, which are now parallel to each other; then DN shall be an asymptote of the curve described by the motion of P.

If instead of a curve line BQM, a fixed right line AE be substituted, then the point P will describe a conic section, whose tangents and asymptotes are determined by these constructions. In this supplement, it is afterwards shown how to draw the tangents and asymptotes of all the curves which are described in the abovementioned treatise by more angles and lines.

III. The same method is afterwards applied to draw tangents to lines described by other motions than those which are considered in that treatise; of which the following is an instance. Suppose that the lines CP and SP, fig. 5, revolve about the poles C and S, so that the angle ACP bears always the same invariable proportion to ASP, suppose that of m to n. In the line CS, take the point T, so that ST may be to CT in that same proportion of m to n; then this point T will be an invariable point; since CS is to CT, as $m - n$ to n. Draw TP, and constitute the angle SPN, equal to CPT, so that PN and PT may lie contrary ways from SP and CP, and PN shall be a tangent of the curve described by the motion of the point P. Several other theorems of this kind are subjoined here.

IV. After these, lines or angles are supposed to revolve about three or more poles, and the dimensions of the curves with their tangents and asymptotes are determined. Suppose in the first place, that the three poles are C, S, and D, fig. 6, and that lines or rulers CR, SQ, QDR, revolve about these poles. The line which revolves about D, serves only to guide the motion of the other two, so that its intersection with each of them being carried over a fixed right line, their intersection with each other describes the locus, which is shown to be a conic section. The intersection of QDR with SQ, is supposed to be carried over the fixed right line AF; the intersection of the same QDR with CR, is supposed to be carried over the fixed right line AE; and in the mean time, the intersection of the right lines SQ, CR, that revolve about the poles S and C, describes a conic section.

This conic section passes through the poles C and S; and if you produce DC and DS, till they meet with AQ and AR in F and E, it will also pass through F and E: it also passes always through A the intersection of the fixed lines QF and ER; from which this easy method follows, for drawing a conic section through five given points. Suppose that these five given points are A, F, C, S, and E: join four of them by the lines AF, FC, AE, ES, and produce two of these FC, ES, till they meet, and by their intersection give the point D. Suppose infinite right lines to revolve about this point D, and the points C and S, two of those that were given, and let the intersections of the line revolving about D, with those that revolve about C and S, be carried over the given right lines AE, AF; then the intersection of those that revolve about C and S with each other, will, in the mean time, describe a conic section, that shall pass through the five given points A, F, C, S, and E.

It is then shown, that when C, S, and D are taken in the same right line, the point P describes a right line, fig. 7, as also when C, S, and A are in the same right line; which also follows from what is demonstrated in that very ingenious paper concerning Pappus's porisms, communicated by Mr. Simson, professor of mathematics at Glasgow, published in the Phil. Trans. N° 377.

In the next place it is shown, that if four right lines revolve about four poles

C, S, D, and E, and those that revolve about D and E, serve only to guide those that revolve about C and S; so that Q and R, the intersections of that which revolves about D, with those that revolve about E and S, be carried over the fixed lines AB and AF; and M the intersection of that which revolves about E with that which revolves about C, be carried over a third fixed line BF; then the intersection P of those that revolve about C and S, will, in the mean time, describe a conic section, and not a curve of a higher order. The conic section degenerates into right lines, when CP and SP coincide at the same time with the line CS, that joins the poles C and S, as in the preceding description; which coincides again with what is demonstrated in the abovementioned ingenious paper.

After this it is shown generally, that though the poles and lines revolving about them be increased to any number, and the fixed lines over which such intersections, as we described in the last two cases, are supposed to be carried, be equally increased, the locus of the point P will never be higher than a conic section; that is, let a polygon of any number of sides have all its angles, one only excepted, carried over fixed right lines, and let each of its sides produced, pass through a given point or pole, and that one angle, which we excepted, will either describe a straight line, or conic section.

Thus, if a hexagonal figure LQRPMN, fig. 8, have all its angles, excepting P, carried respectively over the fixed right lines Aa, Bb, Gg, Hh, Kk; then the point P in the mean time will describe a conic section, or a right line. The locus of P is a right line when CP and SP coincide together with the line CS. All these things are demonstrated geometrically.

V. After this, angles are substituted instead of right lines revolving about these poles; and it is still demonstrated geometrically, that the locus of P is a conic section or right line.

Suppose that there are four poles C, S, D, and E, fig. 9, about which the invariable angles PCQ, PSK, RDM, MEQ revolve; and that Q, M, and R, the intersections of the legs CQ and EQ, of EM and DM, and of DR and SR, are carried over the fixed right lines Aa, Bb, Gg, respectively; then the locus of P is a conic section, when CP and SP do not coincide at once with the line CS; but is a right line when CP and SP coincide at the same time with CS; and never a curve of a higher order.

VI. Having demonstrated this, which seems a remarkable property of the conic sections, or lines of the second order; it proceeds to substitute curve lines instead of right lines in these descriptions, as is always done in the treatise concerning the description of lines, and to determine the dimensions of the locus of P, and to show how to draw tangents to it to determine its asymptotes, and other properties of it. Mr. Maclaurin had observed in 1719, that by increasing the number of poles and angles beyond two, the dimensions of the locus of P, did not rise above those of the lines of the second order, while the intersections moved on right lines; and therefore he did not think it of use then to take more poles than two, since by taking more, the descriptions became more complex, without any advantage. When the intersections are carried over curve lines, the dimensions of the locus of P rise higher, but the curves described have double, or multiple points, as well as when two poles only are assumed; and therefore this speculation is more curious than useful. However, he subjoins some of the theorems that he found on this subject, concerning the dimensions of the locus of P, and the drawing tangents to it.

1. If, in fig. 6, you suppose Q and R to be carried over curve lines, of the dimensions m and n respectively; then the point P may describe a locus of $2mn$ dimensions.

2. If, in fig. 8, you suppose L, Q, R, M, N, to be carried over curve lines of the dimensions m, n, r, s, t, respectively; then the locus of P may arise to $2mnrst$ dimensions, but no higher; and if instead of lines revolving about the poles, you use invariable angles, the dimensions of the locus of P will rise no higher.

3. He then assumed 3 poles, C, D and S, (fig. 10) and supposed one of the angles SNL, to have its angular point N carried over the curve AN, while the leg NQ passes always through S, as in the description in the treatise of the general description of curve lines, while the angles QDR, RCP, revolve about the poles D and C: he supposes also the intersections Q and R to be carried over the curve lines BQ, GR, and that the dimensions of the curve lines AN, BQ, GR, are m, n, r, respectively; and finds that the locus of P may be of $3mnr$ dimensions; but that the point C is such, that the curve passes through it as often as there are units in $2mnr$.

4. If any number of poles are assumed, so as to have angles revolving about them, as about C and D in the last article, and the intersections are carried over other curves, the dimensions of the locus of P will be equal to the triple product of the number of dimensions of all the curves employed in the description.

5. If the invariable angles PNR, PMQ, (fig. 11) move so, that while the sides PN, PM, pass always through the poles C and S, the angular points N and M describe the curves AN and BM; and at the same time, the invariable angle RDQ, revolve about the third pole D, so that the intersections R and Q describe the curves ER and GQ; then the dimensions of the locus of P, when highest, shall be equal to the quadruple product of the numbers that express the dimen-

sions of the given curves AN, ER, GQ and BM, multiplied continually into each other. If more poles are assumed, about which angles be supposed to move, as RDQ moves about D in this description, and the intersections of the sides be still carried over curves, as in this example; the dimensions, of the locus of P, when highest, shall still be found equal to the quadruple product of all the numbers that express the dimensions of the curves employed in this description.

6. Suppose that the three invariable angles PQK, KLR, RNP, (fig. 12) move over the curves GQ, EL, AN, so that the sides PQ, KL, PN produced, pass always through the poles C, D, S, and that the intersections of their sides K and R, at the same time move over the curves FK and BR; then the dimensions of the locus of P, when highest, shall be equal to the product of the numbers that express the dimensions of the given curves multiplied by 6. If more poles, with the necessary angles and curves, are assumed between C and D, as here D is assumed between C and S, and the motions be in other respects like to what they are in this example; then in order to find the dimensions of the locus P, when highest, raise the number 2 to a power whose index is less than the number of poles by a unit; add 2 to this power, and multiply the sum by the product of the numbers that express the dimensions of the curves employed in the description; then this last product shall show the dimensions of the locus of P when highest.

The author is able to continue these theorems much further: but it is not worth while, especially since there is not any considerable advantage obtained by increasing the number of poles, above the method delivered in the above-mentioned treatise, of the description of curve lines. On the contrary, the descriptions there given, by means of 2 poles, will produce a locus of higher dimensions by the same number of curves and angles, than these that require 3 or more poles; and are therefore preferable, unless perhaps in some particular cases.

7. However, he has also found how to draw tangents to the curves that arise in all these descriptions: of which he gives one instance, where 3 right lines are supposed to revolve about 3 poles, and 2 of their intersections are supposed to be carried over given curve lines, and the third describes the locus required.

Let the right lines CQ, SN, DN, (fig. 13) revolve about the poles C, S, D; where that which revolves about D, serves to guide the motion of the other two; its intersection with CQ moving over the curve GQ, while its intersection with SN moves over the curve FN. Suppose that the right line Bb touches the curve GQ in Q, and that the right line Aa touches the curve FN in N. In order to draw a tangent to the locus of P; join DC, DS and CS, and constitute the angle DQR, equal to CQB, so that QR lie the contrary way from QD that QB lies from QC, and let QR meet DC in R. Constitute also the angle DNT, equal SNA, with the like precaution, and let NT meet DS in T. Join RT, and produce it till it meet CS in H; then join PH, and make the angle CPL equal to SPH, so that PL and PH may lie contrary ways from CP and SP; then PL shall be a tangent at P, to the locus described by P, the intersection of CQ and SN.

Mr. Maclaurin has also applied this doctrine to the description of lines through given points. But he supposes he has said enough at present on this subject; and concludes, after observing that in the abovementioned treatise, he has given an easy theorem, for calculating the resistance of the medium, when a given curve is described with a given centripetal force in a resisting medium, which he here repeats, because it has been misrepresented in a foreign Journal.

Let v express the centripetal force with which the body that is supposed to describe the curve, is acted on the medium; let v express the centripetal force with which the same curve could be described in a void; suppose $z = \frac{V}{v}$, then the resistance shall be proportional to the fluxion of z multiplied by the fluxion of the curve; supposing the area, described by a ray, drawn from the body to the centre of the forces, to flow uniformly. Let this theorem be compared with what the celebrated mathematician mentioned by that Journalist has given on the same subject, and it will easily appear what judgment is to be made of his assertion; and since several persons, and particularly the gentleman mentioned above in this paper, testify that Mr. Maclaurin communicated to them this theorem, before any thing was published on this subject by the learned mathematician he names, his observation on this occasion must appear the more groundless.

From this theorem, the author draws this very general corollary; that if the curve is such as could be described in a void by a centripetal force, varying according to any power of the distance, then the density of the medium in any place, is reciprocally proportional to the tangent of the curve at that place, bounded at one extremity by the point of contact, and, at the other, by its intersection with a perpendicular raised at the centre of the forces to the ray drawn from that centre to the point of contact. Let AL be the curve described by a force directed to the point S (fig. 14); let LT touch the curve at L, and raise ST perpendicular to SL, meeting LT in T; then the density in L shall be inversely as LT, if the resistance be supposed to observe the compound proportion of the density, and of the square of the velocity.

Besides what is here observed, he proposes to illustrate and improve several other parts of the treatise concerning the description of curve lines in this supplement.

That treatise requires these additions and illustrations the more, that though the whole almost was new, it was published in a hurry, when the author was very young, before he had time to consider sufficiently which were the best ways of demonstrating the theorems, or resolving the problems, for which this supplement he hopes will make some apology.

The following paper, dated at Nancy, Nov. 27, 1722, is that which the author mentions in his letter.

SECTION I.—PROP. 1. *Which respects the Description of Lines.*—About the poles C, B, D, (fig. 15) let the lines cd, BM, DR be moved; and let the concourse of the legs BM, DR be drawn along the given line PG, and the concourse of the legs cd, DR along the given line PQ; then the concourse of the legs cd, Bd will describe a conic section.

Draw rt parallel to the line BD given in position, meeting Bd in t; joint Pt, producing it to meet BD in F; and it will give the point F. For as the ratio of ru to rt is given, being the same as that of DG to DB, because of the similar triangles DMBG and rmta; and since ru is to rt, as QG to QF, the ratio of QF to QG will be also given; so that, because of the given line QG, there will be given QF, and hence the point F and the line PF. Since therefore BT and CR cut off the parts Pt, Pr, from the lines PF, PQ, given in position, their intersection d will always be in a given ratio in a conic section, by Lem. 20, lib. 1, Newton's Principia.

If the point D be taken anywhere in the right line BF; and if DG be always to QG, as BD to QF; the conic section will be the same as d describes.

The conic section passes through C, P, B and a, by completing the parallelogram Psav. It also passes through L, where the line BG produced meets PV, as also through K, where CD cuts the given line PG. Hence the pentagon PKCLB is inscribed in the section. And if the 5 points CKPBL be given, through which the conic section is to be drawn, or if the conic section is to be circumscribed about the given pentagon CLBPK, let any 2 sides, CK, LB, be produced to their intersection D; then join the rest PL, PK, and let the intersections of cd, DR, and Bd, DR, be always drawn along those lines PL, PR; then the intersection d will describe the section.

PROP. 2. About the given points F, C, G, S, (fig. 16) as poles, let the lines FQ, CN, GQ, SL be moved; and let the intersections of the lines FQ and CN, FQ and GQ, GQ and SL, viz. the points M, Q, L, always touch the lines AE, BE, HL, given in position; then the intersection of the lines CN, SL will describe a conic section.

Let the lines AM, HR meet BQ in E and H. Join CF and GS, meeting in D; join DQ, meeting CM and SL in N and R: then if EN and HR be joined, these will be lines given in position, by Lemma 1. For since the points F, C, D, are in the same right line; and the intersections of the lines FM, CM, and FQ, DQ, run over given lines, the intersection of the legs CM, DQ will also touch the given one. And, for the like reason, since S, D, G are in the same right line, the intersection of DQ and SL will also touch the given one.

Therefore, omitting the poles F, G, there is to be found the curve which the intersection P, of CN, SL, will describe, while by the lines CN, DN, SR, revolving about the poles C, D, S, the concourse of the two CN, DN touches the given line EN, and the concourse of the two SR, DN touches the given one HR; and that this is a conic section is plain from the foregoing proposition.

Some Experiments relating to Electricity. By Mr. Stephen Gray, F. R. S. N° 439, p. 166.

Feb. 18, Mr. Gray tried what effect would be produced on several sorts of wood with respect to the luminous part of electricity: the wood was made into rods of the same form with those iron ones mentioned in a former letter on this subject; the woods were fir, ash and holly; these being successively disposed on electric bodies, after the same manner as the iron rods had been, the tube being applied to one end, there appeared a light on it, but not with so great a force, nor did the light extend to so great a length; neither was the form of it conical, but rather cylindrical; but its extremity seemed to consist of a short fringe of light; when the light, that was given to the rod by the application of the tube, ceased, on a motion of the hand towards the point of the rod, the light came out again, as has been mentioned of the iron rods; but when the hand or finger was held near the point of these wooden rods, there was no pricking or pushing of the finger felt, as when the iron rods were used. He had some of these rods made much larger at one end than the other, and now applying his finger to the larger end, there not only appeared a light, but the finger was pushed, especially when the holly rod was used, and the cheek was a little pricked, but the smart was not near so great as when the iron rods were used; the large end of the rod was pointed with a much greater angle than the smaller one, yet there was very little, if any difference, in the form or size of the light from either end.

Having procured two pair of lines made of worsted yarn, one of them of a mazareen blue, the other of a scarlet colour; on the 3d of April, he suspended the boy first on the blue lines, and found that all the effects were the same, as when he was suspended on lines of blue silk. He then suspended him on the scarlet lines; but now, though the tube was as well excited, and the experiment often repeated, yet there was no effect produced on him, either of attraction of a pendulous thread, nor of pricking nor burning, by applying a hand near him: one of the iron rods being then first on the blue lines, all the same effects were exhibited, as when the same rod had been laid on silk lines of that colour; but on laying the same rod upon the scarlet lines, no manner of attraction, &c. was perceived.

In the Philos. Trans. N° 422, Mr. Gray gave an account of the experiments made on the communicative electricity of water, and found that water is attracted by the tube; with several remarkable circumstances with which this attraction is attended; but he has now found, that when the stand with those little ivory cups there mentioned, are set on any electric body, the same phænomena are produced, not only by holding the tube near the water, but when that is removed, and the tip of the finger placed over the water, viz. there is a little hill, or protuberance of water of a conical form, from the vertex of which proceeded a light and a small snapping.

May the 6th, was made the following experiment. The boy being suspended on the silk lines; and the tube being applied near his feet, as usual; on holding the end of his finger near a gentleman's hand, that stood on a cake made of shell lack and black rosin; at the same time another gentleman standing at the other side of the boy with the pendulous thread; the boy was then bid to hold his finger near the first gentleman's hand, on which it was pricked, and the snapping noise was heard; and at the same time the thread, which was by its attraction going towards the boy, fell back, the boy having lost a great part of his attraction; on a second moving his finger to the gentleman's hand, the attraction ceased: then the thread being held near that gentleman, he was found to attract very strongly; but having since repeated this experiment, though the attraction of the boy is much diminished, yet he does not quite lose it, till 2, 3, and sometimes 4 applications of his finger to the hand of him that stands on the electric body, but without touching him. At another time three persons stood, one of them on a cake of shell lack, &c. the other on one of sulphur, the third on a cake of bees-wax and rosin; the persons all holding hands, the boy applying his finger near the first man's hand, they all three became electrical, as appeared by the attraction of the thread, when held near to any of them.

June the 10th in the morning, were repeated the experiments with the wooden rods, the most material of which were made with the holly rod: this being laid on the glass cylinder, and a fir board, about a foot square and $\frac{3}{10}$ of an inch thick, being placed erect on a stand, set on another glass cylinder, so that the centre of the board was placed near the point of the rod, but not to touch it by near $\frac{1}{2}$ inch; then the tube being held near the thick end of the rod, there issued out a light from the small end, which was that next the board; and it came along with a hissing noise, and struck against the board: when the boy touched the board, there was a light; and at the same time another on the end of the rod, but he heard no snapping nor pricking of his finger, as when the brass plate and iron rod were used.

Experiments with the Scarlet and Blue Worsted Yarn repeated.—When the boy was suspended on the scarlet lines, he attracted the white thread at a very small distance; but the attraction ceased in about 6 or 7 seconds of time. Then the boy being taken off, an iron rod was laid on the lines, but there was no attraction of the thread by the body of the rod; but when the thread was held near either of its pointed ends, it showed a small repulsion, and in the dark a very small light was seen at each end of the rod.

When the boy was suspended on the blue lines, he attracted the thread to him when it was held at least a foot from him, and he continued his attraction to near 75 seconds, the iron rod continued its attraction not more than 36 seconds.

When he was suspended on the blue lines, he continued his attraction 50 minutes, on the scarlet lines 25 minutes, on the orange coloured lines 21 minutes.

By these experiments we see the efficacy of electricity on bodies suspended on lines of the same substance, but of different colours, and also that the attraction continues much longer on silk than on yarn, and consequently silk is the properest body we can make use of, to suspend those bodies on, to which we would communicate an electricity.

An Account of the Births and Burials, with the Number of the Inhabitants at Stoke-Damerel, Devonshire. Communicated by the Rev. Mr. Wm. Barlow. N° 439, p. 171.

On taking a survey, about Michaelmas 1733, of the inhabitants of Stoke-Damerel, in the county of Devon, the number of persons, men, women, and children, residing in the parish, amounted to 3361. By the register, it appears that in the same year, 28 couples were married, 61 males and 61 females baptized, and 62 people buried.

Whence it appears that the number of persons who died, is only one more than half the number of children born; and that about 1 in 54 died.

An extraordinary Case of the Foramen Ovale of the Heart, being found open in an Adult; communicated by Claudius Amyand, F.R.S. N° 439, p. 172.

A person dying at the age of 22, of an illness that had perplexed his physicians, was opened, to discover an imposthume, which was apprehended in the belly. As nothing was observed there worth notice, excepting a great relaxation of the viscera, the cause of his death was looked for in the thorax; there the lungs were strongly attached to the pleura on each side, and a large collection of water in each cavity, especially on the left, where the posterior lobe was inflamed, and tending to suppuration; the quantity of water in the pericardium was greater than usual, and the heart much larger than could be expected in so great an atrophy as the patient was reduced to; in it the foramen ovale was found open, so as to give passage to a large finger, when a fungous substance, which grew from the circumference of the foramen, and stopped up the same, was removed. The valve was hardly perceptible, it being callous and furled up. The ductus arteriosus was found close as usual. This patient had enjoyed great health till lately, and had given no sign of this opening of the foramen ovale, which is preternatural in adults.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1734, *pursuant to the Direction of Sir Hans Sloane, Bart. P.R.S. By Isaac Rand, F.R.S.* N° 440, p. 173.

This is the 13th catalogue of this collection, completing 650 plants.

The Apparent Times of the Immersions and Emersions of Jupiter's Satellites, which will happen in the Year 1737. *Computed to the Meridian of the Royal Observatory at Greenwich. By James Hodgson, F.R.S.* N° 440, p. 177.

Another repetition of the catalogue of these eclipses, pre-computed, and published for the accommodation of gentlemen intending to make observations on them.

A Proposition relating to the Combination of Transparent Lenses with Reflecting Planes. By J. Hadley, Esq. V. Pr. R.S. Communicated Jan. 9, 1734. N° 440, p. 185.

Having proposed the use of a telescope with the instrument for taking angles which Mr. Hadley formerly laid before this Society, (See N° 420) it gave occasion to consider the effects of combining several kinds of telescopes with reflecting planes, and, among others, led to the following proposition :

That if two lenses, of equal focal length, be put together in the form of a telescope, and a plane speculum be placed before one of them, so that the axis of the telescope make any angle with its surface, and a ray of light, the line of whose direction lies in a plane perpendicular to that surface, and passing through the axis of the telescope, fall on it, and be reflected from it, so as to pass through the telescope; then the line of its last direction, after passing the telescope, will make an angle with that of its first direction, before its incidence on the speculum, very nearly equal to double the angle made between the axis of the telescope, and the surface of the speculum.

Lemma.—Let the line FG be the common axis of the two lenses ID and KE, of equal focal lengths, fig. 6, pl. 2 ; to which let the lines AD, DB and BE, be each equal ; and let a ray of light, issuing from a point in the axis F, fall on the lens ID at I, and be there refracted into the line IG, cutting the axis in G, and meeting the lens KE in K ; where let the ray be again refracted into the line KH, cutting the aforesaid axis in H : the angles IFD and KHE are very nearly equal.

Demonstr.—It is known from dioptrics, that the lines FI, IG, KH, and FG, are all in the same plane ; and by the construction the lines AD, DB, and BE are equal ; and by prop. 20 of Huygens's dioptrics, the lines FA, FD, and FG are continually proportional ; consequently FA is to AD as FD to DG ; and dividing, FA is to AD as FD — FA (= AD) is to DG — AD (= BG). Therefore AD is to BG as FD to DG. By the same prop. the lines BG, EG, HG are also continually proportional, and BE (= AD) is to BG, as EH is to EG. Hence it follows, that the lines FD, DG, and EH, EG, are proportionals. But FD is to DG, as the tangent of the angle IGD or KGE, to the tangent of the angle IFD ; and EH is to EG, as the tangent of the angle KGE to the tangent of the angle KHE. The tangent of the angle KGE therefore has the same ratio to the tangents of each of the angles IFD and KHE, and consequently those angles are equal. Q. E. D.

In the demonstration of the above-cited proposition of Huygens, the thickness of the lenses are neglected, and the distance of the points I and K, from the line FG, supposed very small ; so that if either of those are too great, there may arise a sensible difference between the angles IFD and KHE.

Let DF and CG, fig. 7, represent the two lenses, put together as before, having their common axis in the line EL ; and BN a plane speculum, to which that line is inclined in the angle GHN ; and let AB be a ray of light falling on the speculum at B, as is before expressed, and let it be there reflected towards the point C of the lens CG, where it is refracted towards the point D of the lens DF, and there again refracted into the line DE, cutting the axis in E. The angle AOP contained between this last line DE, continued backward, and the first line of incidence of the ray AB, will be very nearly equal to double the angle of inclination of the axis of the lenses EL, to the plane of the speculum BN; i. e. double the angle GHN.

Demonstr.—Produce the lines of incidence and reflection of the ray AB and BC, till they meet the axis of the two lenses in I and L; and through the point B draw BK perpendicular to the plane of the speculum, and cutting the same axis in K; then the angles KBL and KBI are equal. The angle KLB is the difference of the angles IKB and KBL; and the angle HIB is the sum of the angles IKB and KBI = to KBL: therefore the angle IKB is equal to half the sum of the angles HIB and KLB. But by the foregoing lemma, the angles KLB and FED are very nearly equal. Therefore the angle IKB is nearly equal to half the sum of the angles HIB and FED; that is, to half the angle POB; and its complement BHI or GHN is nearly equal to half the angle AOP, the complement of POB to a semicircle. Q. E. D.

If the first incidence of the ray be supposed to be in the line ED, it will proceed in the same track as before, but with the contrary directions; so that the angle EOB made between the first incident ray, and the last reflected, will still be equal to the double of GHN, as before.

It is evident that on this principle an instrument might be constructed, the effects of which would in a great measure resemble those of that before-mentioned (N° 420): but it would be liable to the errors arising both from the spherical figure of the lenses, and also the different refrangibility of the rays of light, when the object is seen at a distance from the axis of the telescope; though those errors, by a proper disposition of the parts of the instrument, may be reduced to a very small quantity. However, for this reason, and also because the instrument seemed to be attended with greater inconveniencies, both in its construction and use, than the other, Mr. Hadley did not think it necessary to give any more particular description of it.

Of a large Bony Substance found in the Womb. By Edward Hody, M. D., F. R. S. N° 440, p. 189.

In examining the pelvis of a woman, 57 years of age, Dr. H. found a large bony substance, contained in the womb, and so strictly united to it, that they seemed to be one and the same body. On cutting the substance asunder, he observed, that the ossification went no farther than the thickness of a shilling;

the part immediately under the ossification was like firm flesh, and this flesh was softer and softer the nearer to its centre.

He thinks it unnecessary to remark, that the woman never had but 1 child, of which she was delivered about 27 years before she died.

Her chief complaints, for some years, were a short cough, great difficulty in breathing, frequent uneasiness in making water, or in going to stool, and a constant weight, or bearing down, on the parts of generation.

But the immediate cause of her death was an asthma; for she had only one lobe of the lungs left that was perfectly sound; the rest adhered firmly to the pleura, were very much contracted, and in some places scirrhous.

Experiments concerning the Impregnation of the Seeds of Plants. By James Logan, Esq. N° 440, p. 192.

As the notion of a male seed, or the farina fœcundans, in vegetables, is now very common, Mr. Logan only mentions such observations concerning it, as may have some tendency to that subject. And first it appears, from Miller's dictionary, that M. Geoffroy, from the experiments he made on maize, was of opinion, that seeds may grow up to their full size, and appear perfect to the eye, without being impregnated by the farina, which possibly may in some cases be true; for there is no end of varieties in nature. But in the subject he has mentioned Mr. L. believes it is otherwise, and that Mr. G. applied not all the care that was requisite in the management.

In the spring Mr. Logan resolved to make some experiments on the maize, or Indian corn. In each corner of his garden, he planted a hill of that corn, and watched the plants when they grew up to a proper height, and were pushing out both the tassels above, and ears below; from one of those hills, he cut off the whole tassels, on others he carefully opened the ends of the ears, and from some of them cut or pinched off all the silken filaments; from others he took about half, from others $\frac{1}{4}$ and $\frac{3}{4}$, &c. with some variety, noting the heads, and the quantity taken from each: other heads again he tied up at their ends, just before the silk was putting out, with fine muslin, but the fuzziest or most nappy he could find, to prevent the passage of the farina; but which would obstruct neither sun, air nor rain. He fastened it also so very loosely, as not to give the least check to vegetation.

The consequence of all which was this; that of the 5 or 6 ears on the first hill, from which he had taken all the tassels, from whence proceeds the farina, there was only one that had so much as a single grain in it, and that in about 480 cells, had but about 20 or 21 grains; the heads, or ears, as they stood on the plant, looked as well to the eye as any other; they were of their proper length, the cores of their full size, but to the touch, for want of the grain, they felt light and yielding. On the core, when divested of the leaves that cover it, the beds of seed were in their ranges, with only a dry skin on each.

In the ears of the other hills, from which he had taken all the silk, and in those that he had covered with muslin, there was not so much as one mature grown grain, nor other than as mentioned in the first: but in all the others, in which he had left part, and taken part of the silk, there was in each the exact proportion of full grains, according to the quantity or number of the filaments left on them. And for the few grains found on one head in the first hill, he immediately accounted thus: that head, or ear, was very large, and stood prominent from the plant, pointing with its silk westward directly towards the next hill of Indian corn; and the farina, he knew, when very ripe, on shaking the stalk, will fly off in the finest dust, somewhat like smoke. He therefore judged that a westerly wind had wafted some few of these particles from the other hill, which had fallen on the styles of this ear, in a situation well fitted to receive them, which none of the other ears, on the same hill, had.

Mr. Logan is positive, by his experiment on those heads, that the silk was taken quite away, and of those that were covered with muslin, none of the grains will grow up to their size, when prevented receiving the farina to impregnate them; but appear, when the ears of corn are disclosed, with all the beds of the seeds, or grains, in their ranges, with only a dry skin on each, about the same size as when the little tender ears appear filled with milky juice before it puts out its silk. But the few grains that were grown on the single ear, were as full and as fair as any ever seen; the places of all the rest had only dry empty pellicles, as described; and probably the same holds generally in the whole course of vegetation; though it may not be safe to pronounce absolutely upon it, without a great variety of experiments on different subjects. But there are few plants that will afford so fine an opportunity of observing on them, as the maize, or Indian corn; because its styles may be taken off or left on the ear, in any proportion, and the grains be afterwards numbered in the manner abovementioned.

Some Observations of the Eclipses of Jupiter's Satellites, made by Geo. Lynn, Esq. at Southwick, Northamptonshire. N° 440, p. 196.

The account of these eclipses is of no use now.

Concerning the High Tide in the River Thames, on Feb. 16, 1735-6. *By Mr. Tho. Jones* N° 440, p. 198.

Mr. Jones having in a former Number given an account of the tide's flowing on the 8th of March, 1725-6, which then flowed 20 feet 5½ inches, as he took it by a level from that high water mark to low water the next morning, and was 4 inches higher than had been known for 40 years before. He now adds, that having marked that high tide on a post, on Monday the 16th instant, (Feb. 16, 1736) the tide rose at the same place 6¾ inches above that mark, and flowed near 2 feet the last half hour but one before high water.

If the tide had flowed its full time, it would have flowed half an hour longer, and would have drowned the whole level.

A Singular Cutaneous Affection. By Abr. Vater, M.D. Professor of Anatomy and Botany in the University of Wittemberg and F.R.S. An Abstract from the Latin. N° 440, p. 199.

In the Phil. Trans. N° 424,* a history is given of a very uncommon case of diseased skin, occurring in a young labourer. A case parallel to this occurred in Germany in a young girl, who had been previously affected not only with swellings of the limbs and body (to which various domestic and empirical remedies had been applied) but also with a large, hard tumor between the shoulders, which was removed by discutient applications. But after the discussion of these tumours, a dry and hard crust or scab began to form, on the feet and hands, especially in the soles of the feet and palms of the hands, and this incrustation projected so much from the ends of the fingers and toes, that the patient could neither lay hold of any thing nor walk. The incrustation scaled off at intervals, especially after the use of various ointments; but at such times the girl was always ill, being swelled in her body, and troubled with a sense of oppression, and griping pains of the bowels, which symptoms continued until the incrustation was removed. She was at length cured by laxative mercurial medicines, and decoctions that purify the blood. Examined by the microscope, the incrustation appeared to be nothing more than the cuticle expanded and indurated. Dr. V. was informed of another similar case occurring in a young woman, likewise a German, whose hands and feet, with the fore-arm, were covered with a crust or scab, which scaled off twice a year, and resisted all the remedies which had then been tried. This patient laboured under an ob-

* Vol. vii, p. 543 of these Abridgments.

struction of the menses, which was supposed to give rise to the diseased state of the skin.

Experiments on the Vibrations of Pendulums. By the late W. Derham, D.D. F.R.S. N° 440, p. 201.

The account Mr. Bradley gave in Philos. Trans. N° 432, of observations made at Jamaica by Mr. Campbel, with a nice pendulum-clock of Mr. Graham's making, brought to mind some experiments Dr. Derham made some years before, which may be of use in observations of this nature.

The first he notices are some experiments made in the year 1704, with excellent instruments, on the vibrations of pendulums in vacuo; which were published in the Philos. Trans. N° 294. The sum of which is, that the vibrations in vacuo were larger than in the open air, or receiver unexhausted: also that the enlargement or diminution of the vibrations, was constantly in proportion to the quantity of air, or rarity, or its density, left in the receiver of the air-pump. And as the vibrations were larger or shorter, so the times were augmented or diminished accordingly, viz. 2 seconds in an hour slower, when the vibrations were largest, and less and less, as the air was re-admitted, and the vibrations shortened.

But notwithstanding the times were slower, as the vibrations were larger, yet he had reason to conclude, that the pendulum really moved quicker in vacuo, than in the air, because the same difference, or enlargement of the vibrations, as $\frac{2}{10}$ of an inch on a side, would cause the movement, instead of 2 seconds in an hour, to go 6 or 7 seconds slower in the same time; as he found by nice experiments.

The next experiments he made at several times, in 1705, 1706, and 1712, by the help of a good month-piece, that swings seconds. The weight that then drove it, was about 12 or 13 pounds, and it kept time exactly by the sun's mean motion: but by hanging on 6 pounds more, the vibrations were enlarged; but yet the clock gained 13 or 14 seconds in a day.

And as the increase or diminution of the power that drives the clock, accelerates or retards its motion, so doubtless does cleanness or foulness affect it, as also heat and cold; for all have the same effect on the pallets and pendulum.

The last experiments he made in 1716 and 1718, to try what effects heat and cold had on iron rods of the same length, or as near as he could to those that swing seconds. He made many experiments with round rods of about a quarter of an inch diameter, and with square rods, of about three quarters of an inch square. The effects on both which were the same.

At first he took the exact length of the rods, in their natural temper. Then he heated them well in a smith's fire, from end to end, nearly to a flaming heat; by which means they were lengthened $\frac{8}{10}$ of an inch. Then he quenched them in cold water; which made them $\frac{7}{100}$ of an inch shorter than in their natural state.

Then he warmed them to the temper of his body; by which means they were about $\frac{1}{100}$ of an inch longer than in their natural temper.

Afterwards he cooled them in a strong frigorific mixture of common salt and snow, which shortened them $\frac{3}{100}$ parts of an inch.

Afterwards he measured these rods, when heated in a hot sun, which lengthened them $\frac{8}{100}$ parts of an inch more than their natural temper.

All these experiments seem to concur in resolving the phenomenon of pendulum clocks going slower under the equator than in the latitudes from it; but yet he has too good an opinion of Sir Isaac Newton's notion of the spheroidal figure of the earth, easily to part with it; and therefore he leaves it to the consideration of others, how far the figure of the earth, and how far heat and cold, and the rarity and density of the air, are concerned in that phenomenon.

The Construction and Use of Spherical Maps, or such as are delineated on Portions of a Spherical Surface. By Mr. John Colson, M. A. F. R. S. N° 440, p. 204.

Geographical maps, and hydrographical charts, though representations of a convex spherical surface, were first delineated on planes, as being the most easy and obvious, though not the most natural and accurate representations; and they are sufficiently near the truth, when the part of the earth or seas described, is not of a very large extent. Such as these have been usually called chorographical and topographical maps; but when the map is any thing general, or is to contain any large tract of the earth or seas, suppose, for instance, one of the four quarters of the world, then, when they are projected, or represented on a plane, the parts must necessarily be distorted, one way contracted beyond the truth, another way dilated, so as to give no just idea of the whole. Nor can this distortion be possibly avoided, when any considerable part of a spherical surface, by any projection whatever, is to be represented on a plane. This distortion is indeed always regular, and according to certain laws; so that knowing the nature of the projection, it may tolerably well be allowed for. But to do this scientifically, requires much skill and accuracy in the maker, as well as proficiency and experience in the peruser; and therefore not so proper for an introduction to learners, in the rudiments of geography. Young minds are apt to receive wrong notions and prejudices from them; at least they cannot be rightly and easily instructed by them.

To obviate this inconvenience, geographers have contrived the terrestrial globe, on which they endeavour to delineate all the parts of the earth's surface in their natural state, as to longitude, latitude, distance, bearing, magnitude, &c. which being a true and genuine representation of the whole superficies of the earth, as far as it is yet known, is the best adapted for conveying just notions to young minds, and for preventing all false conceptions and prepossessions. After the first rudiments of geography have been imbibed from hence, they will be then prepared for the use of plain maps; and they will afterwards find, that large projections of particular countries, kingdoms, and provinces, in plano, will be of excellent service for their further improvement in this useful and necessary science. Nor will they now be in any danger of being misled by such maps, though they are not so just and natural representations of the terrestrial globe.

Now the same conveniencies that may be derived from the whole globe, may, in proportion, be had from any notable portions of it; as a hemisphere, a quadrant, a sextant, an octant, or other part. But with this advantage besides, that these partial spherical maps will not only be much less cumbersome, and more manageable than a whole globe, but may be made much more accurate and particular, as they are capable of being formed to a much larger diameter, than a globe can conveniently be made to. The maps may first be printed on a plane, as is usual in the common globes, and then pasted on thin convex shells of pasteboard, formed to the intended radius. The forming of these spherical coats of pasteboard will be a matter of no great difficulty, even to as large a diameter as shall be desired; but the chief art will be required in projecting the maps in plano, after the simplest and exactest manner, so as they may adapt themselves, with as little error as possible, to a spherical surface. For a plane surface cannot be converted into a spherical surface without some error. The best method of doing this, with the least possible error, may be as follows.

Instead of the usual slips or gussets, as is the manner of globe-makers, which are comprehended between two meridians at some distance, and are formed only tentatively and mechanically, without the help of any just theory, we may divide the whole spherical surface into parallel portions, or zones; that is, into parts terminated by two parallels to the equator, at the distance suppose of 10 degrees. As if the first of these portions, or zones, were at the equator itself, and extended to 5 degrees of latitude on each side of that circle; the second zone would be at the parallel of 10 degrees of latitude, and would extend to 5 degrees of latitude on one side, and to 15 degrees of latitude on the other side of that parallel; and so of the succeeding zones.

Now we may conceive the first of these portions, or zones, to e converted from a spherical surface to a plane in this manner, without sensible error; let the middle line of this zone, that is the equator, continue in its situation, and let the segments of the meridians on each side be conceived to unbend themselves gradually, till they are extended into right lines perpendicular to the equator; then will that which was before a zone, or portion of a spherical surface, with a small alteration become a portion of a cylindrical surface, circumscribed about the sphere; whose breadth is every where equal to 10 degrees of the sphere, and whose circumference is equal to the equator. And thus every parallel to the equator, as far as that of 5 degrees of latitude on each side, will be stretched and extended into a circle as large as the equator; but they will all keep the same distance from one another, and from the equator, that they had before. This extension, or alteration, will be every where regular and uniform, and will be but very little, even where it is most; for the least of these circles, which is the parallel of 5 degrees of latitude, has the same proportion to the circle it is stretched to, or the equator, as the sine of 85 degrees has to the radius, or as 9961947 to 10000000; which approaches very near to a ratio of equality. And now it will be easily conceived, that without undergoing any other alteration, or distortion, this portion of a cylindrical surface may be rectified, or extended into a plane parallelogram, whose length will be equal to that of the equator, and whose breadth will be equal to an arch of 10 degrees of the same equator.

Consequently, by an operation that will be just the reverse of this, if on a plane we delineate such a parallelogram as this, we may then lay down all the places that are contained in it very exactly, in their proper situation of longitude and latitude, and then apply its middle line, or equator, to that of a globe of a due magnitude, which will then become a portion of a cylindrical surface, circumscribed about the globe. Then by pressing it close to the body of the globe, we shall cause it to contract itself a very little, but regularly, which contraction will be only according to longitude, and not at all according to latitude; and then the cylindrical surface will be changed into that of a sphere, and will become the first spherical zone before described, with all its delineations in their due position, without sensible error.

In like manner in the second spherical portion, or zone, comprehended between the parallels of 5 and 15 degrees, whose middle line is the parallel of 10 degrees, we may conceive the segments of the meridians to unbend gradually on each side, and to extend themselves into tangent right lines, which therefore will form a segment of a conical surface, still touching the globe in the parallel of 10 degrees of latitude. The axis of this cone will coincide with the prolonged axis of the globe, and the side of the cone, which is to be estimated from the vertex to the circle of contact, will be the co-tangent of the latitude, or the tangent of 80 degrees. Now this portion of a conical surface may easily be conceived to be unrolled, or to be expanded into a plane surface, without undergoing any other alteration, and then it will become a portion of a sector of a circle; which portion will have for its length, or middle line, an arch of a circle described with the said tangent, as a radius, whose length will be the same as the parallel of contact, and its breadth will be equal to an arch of the equator of 10 degrees, as before. This segment of a sector of a circle, so produced, may therefore be easily described in plano, and within it may be inserted all the places belonging to it, according to their longitude and latitude. Then it must be applied to the globle, so as that its middle line shall coincide with the parallel of 10 degrees; then by pressing it may be bent to the surface of the globe, every meridian to its respective representative, by which it will uniformly contract a little according to longitude, but not at all according to latitude. And thus the globe will be covered as far as 15 degrees of latitude.

The next zone, or that belonging to the parallel of 20 degrees, may be thus constructed à priori. On a plain paper, with radius equal to the tangent of 70 degrees, describe an arch, whose length is equal to that of the parallel of 20 degrees; as also two other concentric arches on each side, at a distance from the middle arch equal to an arch of 5 degrees. This will be the required segment of the circular sector, in which are to be inserted all the places belonging to it, according to their longitude and latitude. Then the middle line or arch is to be applied to the parallel of 20 degrees on the globe, and the segment of the conical surface thence arising, is to be duly contracted as before, or pressed close to the globe; by which means this zone will also be completed. And in the same manner we are to proceed to the succeeding zones, till the whole globe is covered. And the method will not differ in any material circumstance, if instead of a whole globe, we are to construct any part of it only, or what is here called a spherical map.

A Copy of an ancient Chirograph, or Conveyance of Part of a Sepulchre, cut in Marble, lately brought from Rome, with some Observations on it by Roger Gale, Esq. V. P. S. N° 441, p. 211.

This marble lately arrived from Rome, and deposited in the noble museum of Sir Hans Sloane, is a most valuable piece of antiquity, exhibiting a complete formula of a chirograph, or conveyance of one part of a burying-place from one family to another, but neither of them of any note, seeming by their agnomina to have been only liberti, or descended from such. Agricola indeed is a Roman name, but those of his wife Lacena, and his son Protus, are both Greek.

By this chirograph Herennius Agricola obtains from Titus Flavius Artemidorus, a right to 4 ollaria, which were niches or repositories, where they placed cineraria, urns, or vessels of stone or earth, containing the ashes of the dead, and were here 53 in number.

This monument was situated on the left side of the Via Salaria, which ran to the north-west of Rome from the Porta Collina. It stood in the ground of Volusius Basilides, and the consideration for the conveyance of it is one Sesterce. It is very usual in sepulchral inscriptions to find the monument of one family in the field of another, the proprietor of the monument reserving the right of that to himself when he sold the ground; or purchasing so much ground from the owner as was sufficient for erecting the monument. All sepulchres, when once a body was interred therein, were esteemed as religious and sacred, and were not to follow the possession of the field.

The stone is turned with an arch at top; the whole length of it is $27\frac{1}{4}$ inches; the breadth at the bottom is $10\frac{1}{4}$ inches, and at the base of the arch $12\frac{1}{4}$, as it widens gradually upwards. The letters are cut in a small indifferent character; that of the E and the F are remarkable, being always formed in this manner E, *f*. It was probably placed over or between the four niches, or ollaria, granted to M. Herennius Agricola, in this monument, by T. Flavius Artemidorus, to declare and assert the right and possession of them to the former, and his family, till they were all filled.

Of the Revolutions which small Pendulous Bodies, by Electricity, make round larger ones from West to East, as the Planets do round the Sun. By Mr. Stephen Gray. N° 441, p. 220.

Mr. Gray made several new experiments on the projectile and pendulous motion of small bodies by electricity, by which they are made to move about larger ones, either in circles or ellipses, and that either concentric or excentric to the centre of the larger bodies about which they move, so as to make many revolutions about them; and this motion constantly the same way that the planets move about the sun, viz. from the right to the left, or from west to east; but these little planets, if they may be so called, move much faster in their apogeon than in the perigeon parts of their orbits: which is directly contrary to the motion of the planets about the sun.

On the Construction of a Quicksilver Thermometer; also Observations on the Eclipses of Jupiter's Satellites, Anno 1731 *and* 1732. *By M. Jos. Nic. De l'Isle, F.R.S.* N° 441, p. 221.

In order to have surer grounds for experiments of natural philosophy in Russia, and that they might be compared with those of other countries, M. De l'Isle applied this winter to the construction of thermometers of mercury, regulated by the expansion of that fluid proportionably to its bulk. This expansion is indeed not very perceptible, considering that Dr. Halley, in the experiments he made on it above 40 years since, N° 197 of the Phil. Trans.; found that the said expansion, by the heat of boiling water, was no more than the 74th part of the bulk of the mercury.

M. Amontons also relates, in the Memoirs of the Royal Academy of the year 1704, that this expansion of the mercury is only the 115th part of its bulk from the greatest heat to the greatest cold that is felt at Paris. But M. De l'Isle found in the great cold at Petersburg, on the $\frac{16}{27}$ Jan. 1732-3, in the morning, that the bulk of the mercury was condensed almost a 50th part of the extent it had in boiling water. The cold on that day, the wind being at east, was one of the severest ever felt there. His new thermometers of mercury he had made of a good large size, and in such manner that, having divided in each the whole quantity of contained mercury into 100,000 parts, and having marked the extent of the bulk of that mercury in boiling water, he can at any time see on the divisions of these thermometers, by how many parts the bulk of the mercury is condensed through the present temperature of the air. And though he has made four of these thermometers, which differ very much as to their size, and the quantity of mercury they contain, yet they agree within a very few of these parts.

As pure mercury is of the same nature every where, and not liable to any alteration from being inclosed in a tube; and as it is probable, that taking it equally purified, it will in different countries be subject to the same expansion, if exposed to the same degree of heat; for this reason it is probable, these thermometers may very well serve to compare the temperature of different countries; especially as he found by experience, that these thermometers may be rendered fit enough to mark sensibly the increase or diminution of the bulk of the mercury, within one or two parts out of the 100,000 contained in the whole bulk. This kind of thermometers has also this advantage, that as they mark the proper expansion of the mercury in each temperature of air, they may serve to show every moment the correction to be made in the height of the mercury in simple barometers; which will serve for reducing them to the height they would have in an equal temperature of air; and one might, for this end, chuse and agree on the heat of boiling water, as a fixed term, which, in all appearance, will be the same all over the world. If the Royal Society should approve this new construction of thermometers, and should order some of their members to make the like, we might hereafter be able exactly to compare the temperature of England with that of this country, and other places where the like thermometers should be made. In order to reap this advantage from his experiments, M. De l'Isle proposes to communicate to the Royal Society all the observations he has made for 4 or 5 years past, on the barometer and thermometer. M. De l'Isle was informed, that 4 or 5 years since, the Royal Society sent to M. Abraham Vater, at Wittenberg, large thermometers of spirit of wine, made and regulated by an instrument maker of the Royal Society, to compare the observations to be made in Germany, by means of those thermometers, with the observations made in England by the like thermometers, the one being regulated by the others. M. Weidler, professor of mathematics at Wittenberg, mentions in the account which he gave of his meteorological observations for the year 1729, that he has furnished himself with one, which he intends to make use of hereafter for his meteorological observations. He also says, that the observers of the Royal Society of Berlin make use of a like thermometer; and M. De l'Isle had received from thence, observations on the heights of the thermometer of spirit of wine, made probably with that instrument, for the whole year 1729, and for the first 3 months of 1731. Those observations are engraved on copper plates, where the heights of the spirit of wine are expressed in parts of the French, English, and Rhinland foot. If the Royal Society approve of this kind of thermometers, and are desirous he should compare them with his; if they also desire that meteorological observations with those thermometers of spirit of wine should be made in Russia, he begs you would send him several of them; but then he begs that those sent him may be well regulated, and exactly compared with those the observers of the Royal Society make use of; supposing that some person of their body is appointed to keep journals of these observations. M. De l'Isle will send in exchange to the Royal Society, if they desire it, some thermometers of mercury regulated by and compared with the four large ones he made at Petersburg.

After this, M. De l'Isle inserted the last observations on the satellites of Jupiter, which were made at Petersburg, since those inserted in the 3d volume of the Memoirs of the Academy of Petersburg, to the present time, but are omitted now, as of no use to repeat on the present occasion.

Experiments on the Perforation of the Thorax, and its Effects on Respiration. By W. Houston, M. D. F. R. S. Communicated by Philip Miller, F. R. S. An Abstract from the Latin.* N° 44, p. 230.

These experiments, 6 in number, were made upon dogs and puppies. The admission of the external air into the cavity of the thorax (even when both sides were perforated) did not impede either the respiration or the barking of the animals. When the dogs howled, the lungs protruded through the wounds made in the thorax, and when the dogs ceased making a noise, the lungs again went in. But what may seem paradoxical, the dilatation of the lungs, in some of these experiments, was observed to be synchronous with the contraction of the thorax, and e contrà. This phenomenon the author supposes to have been owing to the violent convulsive action of the abdominal muscles, whereby the lower part of the lungs becoming suddenly pushed up, the air would be accumulated in their upper part, which would consequently be distended. It is not to be supposed that this is the case in the natural, undisturbed respiratory action. From these experiments it was evident, that in the natural state the lungs occupy the whole cavity of the chest, their surface being in close contact with the membrane which lines the thorax.

Observations, Astronomical, Physical, and Meteorological, for the Year 1733, *made at Wittemberg. By John Fred. Weidler, Professor of Mathematics, and F. R. S.* N° 441, p. 238.

These meteorological and other observations, being temporary, are now of no use.

Concerning the crooked and angular Appearance of the Streaks or Darts of Lightning in Thunder-Storms. By James Logan, Esq. N° 441, p. 240.

Mr. Stephen Hales, in his Statical Essays, vol. ii, p. 291, mentions this phenomenon of the streaks or darts of lightning in thunder-storms appearing crooked and angular, as a thing not yet accounted for, and therefore he guesses at a solution of it.

The clouds are generally distinct collections of vapours, like fleeces; and therefore the rays of light through them must pass through very different densities, and accordingly suffer very great refractions: from thence, therefore, that appearance must undoubtedly arise. For it is highly absurd to imagine,

* These experiments were made while Dr. H. was at Leyden in the years 1728 and 1729.

that fire darted with such a rapidity, can from any assignable cause deviate in fact from a right line, in the manner it appears to us. And this, if duly considered, may probably be found a plenary solution.

Observations on the Aurora Borealis made in England. By Andr. Celsius, F. R. S. and Sec. R. S. of Upsal in Sweden. N° 421, p. 241.

Here are registered several instances of the appearances, more or less, of the aurora borealis; from which M. Celsius infers, that

In several of these observations he is certain as to the time of the clock: so that if it has happened that others have observed the same phænomena, the longitudes of places may be determined by them with greater exactness than by the satellites of Jupiter; which he takes to be the principal use that may be made of these observations, especially in making maps of the northern countries, where these lights more frequently occur.

Some Experiments made on Mad Dogs with Mercury. Dated June 3, 1735. *By Dr. Robert James,* of Litchfield.* N° 441, p. 244.

Dr. James here gives an account of some experiments made on mad dogs

* Dr. Robert James was descended from a family of great respectability in Staffordshire. After studying at Oxford, he practised physic first at Sheffield, next at Litchfield, then at Birmingham, and lastly in London. In 1755 he was, by the king's mandamus, admitted to the degree of M. D. at Cambridge.

His first publication appears to have been the above paper, afterwards reprinted with considerable additions, in a separate form. After this appeared his Medicinal Dictionary in 3 vols. folio, 1743; a compilation which proves that the author was well acquainted with the writings of the physicians of antiquity, as well as with the most esteemed medical publications of his own days. For whatever relates to the history of the medical art and the histories of diseases, this dictionary may be consulted with advantage; but much of the physiology and pathology which it exhibits is now exploded; nor are the curative directions, in many instances, the best that could have been given. In the materia medica too much notice is taken of the virtues attributed to different medicinal substances by the ancients; and with regard to the pharmaceutical part of this work, that is now become almost useless; for, during the 60 years and upwards which have elapsed since the publication of this Dictionary, pharmacy, in consequence of the improvements and discoveries made in chemical science, has undergone a complete reform. Add to this, that most of the articles in this compilation are discussed with too much prolixity. The work should have been less bulky, and more select.

Dr. James's other publications are as follow. A Translation of Ramazzini de Morbis Artificum; a Practice of Physic, in 2 vols; a Treatise on Canine Madness; for the cure of which he proposed mercury; a remedy which had been before recommended in another form by M. Desault, of Bordeaux, in a Treatise published in 1733. A Dispensary; and a Dissertation on Fevers. The last mentioned publication was written chiefly in recommendation of his celebrated Fever Powder; a preparation of antimony, to which the pulvis antimonialis of the new London Pharmacopœia is supposed to be analogous.

with mercury, which he had reason to believe to be the most effectual preservative against, and perhaps even a cure, for the hydrophobia.

About Michaelmas, 1734, Mr. Floyer complained that he was afraid of a madness among his hounds; for that morning one had run mad in the kennel, and he was apprehensive that most of the rest were bitten by him: Dr. J. took this opportunity of telling him that he had long believed that mercury would, if tried, prove the best remedy against this infection; and that if the idea he had formed of this poison was just, he was pretty sure the medicine would answer, notwithstanding the difficulty there is of determining the effects of a medicine à priori; and that it was at least worth while to try. Mr. Floyer neglected this advice till the Feb. following. Mean time he tried the medicine in Bates, commonly known by the name of the pewter-medicine; as also every thing else which was recommended to him by other sportsmen, but to no purpose; for some of his hounds ran mad almost every day after hunting. On this he took his hounds to the sea, and had every one of them dipt in the salt-water; and at his return, he brought his pack to another gentleman's kennel, 6 miles distant from his own. But, notwithstanding this precaution, he lost 6 or 7 couple of his dogs in a fortnight's time. At length on further persuasion, Mr. Floyer tried the experiment on 2 of his hounds that were both very far gone. They refused food of all sorts, particularly fluids, slavered much, and had all the symptoms of a hydrophobia to a great degree. That night he gave 12 grains of turpeth mineral to each of the 2 dogs, which vomited and purged them gently. Twenty-four hours after this, he gave to each 24 grs. and after the same interval he gave 48 more to each. The dogs salivated considerably, and soon after lapped warm milk. At the end of 24 hours more, he repeated to one dog 24 grs. more, and omitted it to the other. The dog that took this last dose, lay on the ground, salivated extremely, was in great agonies, and had all the symptoms of a salivation raised too quick; but he got through it: the other relapsed, and died.

To all the rest of the pack he gave 7 grs. of the turpeth for the first dose, the second 12, at 24 hours distance, which was repeated every other day for some little time. The method was repeated at the 2 or 3 succeeding fulls and changes of the moon. From this time he lost not another

Dr. James died in 1776, aged 73. A spirited vindication of Dr. J.'s character from the charge of empiricism, in consequence of the patent he obtained for his Fever Powder, has been written by Dr. Heathcote. This is a subject respecting which there will always be a difference of opinion. With regard to Dr. J.'s merits as an author, if he is not distinguished for much originality of thought or conciseness of expression; yet he has shown himself to be a faithful and industrious collector of medical information down to his own time; and it must be confessed that few have surpassed him in point of erudition.

hound; and though several were afterwards bitten by strange dogs, the turpeth has always prevented any ill consequences.

Dr. J. and his friends tried the same thing on many other dogs, and it had never failed in any one instance; though dogs bitten at the same time, and by the same dogs, have run mad after most other methods had been tried.

As to the experiments made on mankind, Dr J. had opportunities of making only three. The first was on a girl about 14 years of age. The calf of her leg was so torn by a mad dog, that the surgeon was obliged to use means to prevent a mortification from the bite. She was vomited by the turpeth. Three days before the next change of the moon, the vomit was repeated, and again the very day of its changing. The same method was pursued the next full moon. The girl is very well.

The 2d was on a boy of about 10 years of age. He had 4 holes in one of his legs, made by a mad dog. The turpeth was given as above, and the wounds dressed with digestives, and he continues well.

The 3d case was that of a young man of about 18 years of age. The bite was on the hand. A great number of dogs were bitten at the same time, in the town where he lived. About 6 days after the mischief was done, several dogs that had been wounded ran mad; on which he applied to Mr. Wilson, apothecary in Tamworth, to whom the Dr. had communicated the success of the turpeth in this case. The young man was at this time very melancholy and dejected, had tremors, and slept very little for some nights before, though he was not apprehensive that the dog which had bitten him was mad. He had a dry scab on his hand: on applying to Mr. Wilson, he was vomited with Vin. Benedict. ℥ij.

The next thing he took was made according to the following prescription; viz. ℞ Turpeth. Min. gr. xij. Lap. Contrayerv. ʒi. Ther. Androm. q. s. M. F. Bol. N° 3, sumat unum singulis noctibus hora decubitûs superbibendo Julap. seq. Cochl. iv. ℞ Aq. Rut. ℥vj. Theriac. ℥ij. Syr. Pæon. c. ℥iſs Tinct. Castor ʒij. M. F. Julap.

On taking these, the patient sweat very much, and had 2 loose stools every day after them: his tremors went off, and he slept better. After this he went into the cold-bath, and continued perfectly well.

But what is remarkable in this case is, that the wound ran a thick digested matter after this method, and threw off the scab like an eschar; after which it healed of itself.

Instead of endeavouring to explain the action of mercury in these cases, Dr. J. makes an observation or two on the antiquity of this disease; which he the rather chooses to do, because Cælius Aurelianus, in his account of it, does not seem to build so much on the authority of Homer, as, in his opinion, he might have done. Indeed he quotes a passage out of the 8th Iliad, where Teucer calls Hector κυνὰ λυσσητῆρα, but he does not seem to think this sufficient to prove that Homer was acquainted with this madness. But he omits two more passages in the same author, which, joined with this, amount to a demonstration that Homer was by no means ignorant of it. The first is in the 9th Iliad, l. 237, where Ulysses is on his embassy to Achilles. He describes to the last mentioned hero, the distress the Grecian army was in through his absence; and when he has painted Hector as terrible as he can, he compares his fury to the rage of a mad dog.

————Ἕκτωρ δὲ μέγα σθένεϊ βλεμεαίνων
Μαίνεται εκπάγλως, πίσυνος Διῒ, οὐδέ τι τίει
Ἀνέρας οὐδέ Θεούς· κρατερὴ δὲ ἑ λύσσα δέδυκεν.
————Hector verò valde trucibus oculis adspiciens
Furit terribiliter, fretus Jove: nec quicquam honorat
Viros neque Deos; ingens autem ipsum rabies invasit.

If Homer had designed as a physician to describe a mad dog, he could not have expressed his looks by a more proper turn than Βλεμεαίνων. It must also be considered, that this discourse is directed to Achilles, who, having studied physic under Chiron, was consequently more capable of receiving an idea of the mischief Hector did to his countrymen by this metaphor.

In the 13th Iliad, Hector is again called Λυσσώδης, by Neptune. It must be observed that λύσσα, λυσσήτηρ, and λυσσώδης, can properly, and in their natural signification, be applied to no other madness, than that which is peculiar to a dog, though metaphorically it may, as in the instances Dr. J. has given, as also in Sophocles and Euripides. The word λύσσα or λύττα is used to signify the madness of dogs by Aristotle, Galen, and Dioscorides. And Λυσσόδεκτος is used by the last mentioned author to signify a man bitten by a mad dog. Λυσσάω is used by Aretæus in this sense, and Λυττώσαις by Plutarch, to express the same thing.

What the Dr. would infer from this is, that Homer was certainly acquainted with the madness of dogs; and if dogs in his days ran mad, it is probable they would bite men, and if so, to be sure, an hydrophobia would be the consequence; and yet Plutarch will have it that it was first noticed in the days of Asclepiades, famous for his practice in Rome before the death of Mithridates.

Another strong evidence of its antiquity, is that instinct which directs every dog to avoid some that is mad, on smelling, seeing, or even hearing him. If this is not instinct, it is reason; and that in a higher degree, than we ourselves can pretend to. Now instinct must be coeval with the creation, or at least the fall; and therefore [canine] madness cannot be much younger.

A Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands. By Mark Catesby, F. R. S. with some Extracts out of the 8th Set. By Dr. Mortimer, Secr. R. S. N° 441, p. 251.

This part contains some account of the different kinds of snakes and vipers found in those parts.

A Catoptric Microscope. By Robert Barker, M. D., F. R. S. N° 442, p. 259.

Though microscopes, composed of refracting glasses only, have been vastly improved, as to their effects of magnifying; yet they have been attended with such great inconveniences, that their application to many arts, in which they might be very convenient, is not so common as might be expected, and mankind have reaped but a small part of the advantage obtainable from so surprising and useful an instrument.

Among the inconveniences mentioned, the following are the most considerable:

1. That in order to magnify greatly, it is necessary that the object-glass be a portion of a very minute sphere, whose focus being very short, the object must be brought exceedingly near; it will therefore be shaded by the microscope, and not visible by any other light than what passes through itself; in this case therefore, opaque objects will not be seen at all.

2. Objects, illuminated this way, may be rather said to eclipse the light, than to be truly seen, little more being exactly represented to the eye, than the outline; the depressions and elevations within the out-line appearing like so many lights and shades, according to their different degrees of thickness or transparency; though the contrary happens in ordinary vision, in which the lights and shades are produced by the different exposure of the surface of the body to the incident light.

3. Small parts of large objects cannot easily be applied to the microscope, without being divided from their wholes, which in the case of vivi section defeats the experiment, the part dying, and no more motion being observed in it.

4. The focus in the dioptric microscope being so very short, is exceedingly nice, the least deviation from it rendering vision turbid; therefore only a very small part of an irregular object can be seen distinctly this way.

To remedy these defects, Dr. Barker has contrived a microscope on the model of the Newtonian telescope, in which he has been greatly assisted by that excellent workman, Mr. Scarlet, jun. As to the effects of this instrument, it magnifies from the distance of 9 to 24 inches.

Fig. 1, pl. 4, represents the entire microscope, mounted on its pedestal, on a proper joint, contrived so as to direct the instrument towards any object.

Fig. 2, the section of the instrument; in which AB is the larger concave metalline speculum; CD the lesser concave metalline speculum; EF a hollow brass screw, to fasten in the 1st dioptrical glass, or plano-convex lens; GH another screw fastening on the hollow cylinder EFIK (in which the dioptric glasses are contained) to the body of the microscope; IK a cap with a small perforation, serving as an aperture to the eye-glass, or 2d lens, convex on both sides; ML is a long screw passing through the nuts P and V, serving to bring the small speculum to a proper distance from the larger; NQ a sliding piece moved by the screw, carrying the stem QR, and the little speculum CD; YX a screw for the cap at fig. 3; that at fig. 4, is to be screwed on the aperture IK.

Fig. 5, shows the construction of the microscope; in which i is an object supposed erect; from which rays falling on the speculum ab, will be reflected to the focus k, where they will form an inverted image; and being reflected by the small speculum cd, they will pass through the perforation of the great speculum, and falling on the plano-convex glass ef, converge again, and form an erect image at l; which being brought very near to the eye, and so considerably magnified, will be distinctly seen through the eye-glass gh.

An Account of the Standard Measures preserved in the Capitol at Rome. By Martin Folkes, Esq. V. P. R. S. N° 442, p. 262.

In the wall of the capitol is a fair stone of white marble, of the length of 8 feet 5 inches English, and of the breadth of 1 foot 9½ inches; on which are inscribed the standards of several measures with these respective inscriptions:

Piede Ro: Pal. IIII. Onc. XII. Deti XVI.
Piede Greco.
Canna di Architet. Palmi X.
Staiolo Pal. V. Quar. III.
Canna di Merca. Palmi otto d'altra misura.
Braccio di Merc. Pal. III. d'altra misura
Braccio di Tessito di Tela.
Curante Lu. Pœto.

The lines, that represent these measures, are cut in the marble, pretty deep; but as they have, consequently, a considerable thickness, it is somewhat difficult to be very exact in taking off their dimensions. Mr. Folkes however did it as nearly as he could, by setting the point of his compasses in the middle of the cross lines, that are drawn to determine the beginnings and ends of the measures. The palm of the architects is easier to give than the others, because the whole canna is inscribed on the stone: this he therefore took off, and then divided it into 10 equal parts. Afterwards his chief attention was given to the Roman foot, as of greater consequence than the other measures. They all however follow, as they occurred to him, in such parts as the London foot contains 1000.

The Roman foot 966 +. This is divided on the stone, first into 4 palms, and then on the upper part into 12 unciæ, and on the lower into 16 deti, according to the inscription.

The Greek foot 1006 +. This is also divided like the Roman.

The canna of the architects 7325. It is divided into 10 palms, each of which is therefore $732\frac{1}{2}$ of the English foot.

The staiolo, being 5 palms and $\frac{3}{4}$, is 4212 —.

The canna de mercanti, divided into 8 palms of another measure, 6 feet 6 inches $\frac{3}{4}\frac{1}{8}$.

The braccio de mercanti, divided into 4 palms of another measure, 2 feet 9 inches $\frac{1}{2}\frac{1}{4}$.

The braccio di tessitor di tela, divided into 3 parts, 2 feet 1 inch $\frac{1}{2}\frac{1}{4}$.

The palm of the architects is assigned by Mr. Greaves 732 of the English foot; and the same is given by M. Picart to the Paris foot, as $494\frac{1}{4}$ to 720; which reduced, becomes 732 + of the English foot, as before, and as it came out from this trial.

The Roman foot is given by Picart, from this very stone, $653\frac{1}{10}$ of such parts, as the Paris foot contains 720; that is, by reduction, 967 + of the English; and the same by Fabretti, who also measured it on this stone, is assigned to the palm of the architects, as 2040 to 1545; which reduced on the former measure of the palm, is $966\frac{1}{2}$ of the English foot. These measures come out as near as the nature of the standard can possibly allow; and as it was somewhat fresher in Picart's time, than it is now, Mr. F. would make no difference in the proportion he has assigned; but supposes the Roman foot on this marble was intended to be such a one, as should contain 967 parts of the English very nearly.

Mr. Greaves had long before assigned the measure of the Roman foot from Cossutius's monument, to be 967 of the English, and had preferred that measure to the others he had taken from the tomb of Statilius, and the Congius of

Vespasian. And there seems no doubt but Cossutius's foot was the foot intended to be inscribed on this marble; though that monument is itself now lost.

For the Greek foot, there seems to be no further mystery, than that it was intended to be made to the Roman, in the proportion collected from Pliny, which is, that 625 Roman feet made 600 Greek; by which account the Greek foot should contain 1007 of such parts as the Roman contains 967; and the actual quantity Mr. F. took off, was 1006.

Some Observations, made at Wittemberg in 1734. By John Fred. Weidler, F. R. S. N° 442, p. 266.

These observations, like those of former years, by M. Weidler, are chiefly on the aurora borealis, the weather, &c.; and no longer useful.

Observations made of the Latitude, Variation of the Magnetic Needle, and Weather, by Capt. Christopher Middleton, in a Voyage from London to Hudson's Bay, Anno 1735. N° 442, p. 270.

These observations are nearly alike with those before printed, Vol. vii, p. 465 and 617, of these Abridgments.

An Experiment, to show that some Damps in Mines may be occasioned only by the burning of Candles under Ground, without the Addition of any noxious Vapour, even when the Bottom of the Pit has Communication with the outward Air, unless the outward Air be forcibly driven in at the said Communication or Pipe. By the Rev. J. T. Desaguliers, LLD., F. R. S. N° 442, p. 281.

Exper. 1.—In a cylindric glass-receiver, open at both ends, whose lower end is plunged in water, and upper end covered with a plate, having a hole of near an inch bore, a candle of 6 in the pound will not burn quite the time of one minute, before it goes out.

Exper. 2.—A candle will burn almost as long when the receiver is quite covered.

Exper. 3.—The receiver having the hole of the plate open, and a pipe at bottom communicating with the external air, will burn but a little longer than in the first experiment; and by blowing in at the pipe with your mouth, it will go out rather sooner.

Exper. 4.—Blow in at the pipe with bellows, and the candle will burn as long as you will.

A Chemical Experiment, serving to illustrate the Phenomenon of the Inflammable Air shown by Sir James Lowther, Bart. as described in Phil. Trans. N° 429. *By Mr. John Maud.* N° 442, p. 282.

Sir James Lowther made an experiment on some air which he collected out of a coal-mine, and brought in bladders, close tied, by sea to town; the effects of which was, that the air being pressed out of the bladder through the small orifice of a tobacco-pipe, would catch fire from a lighted candle, and burn like an inflammable spirit, till it was all consumed.

On considering that the cause was only a great quantity of sulphureous vapours fluctuating in that air, Mr. M. was naturally induced to make an essay, by an artificial mixture, to produce the like effect. It is very well known (he observes) to every one versed in chemical affairs, that most metals emit great quantities of sulphureous vapours, during the effervescence they undergo, in their solutions in their respective menstrua, or solvents. Of these fumes, iron emits a great quantity while it is dissolving in oil of vitriol, which are very inflammable, and not easily to be condensed. These fumes Mr. Maud collected into a bladder with the desired success, and having produced before the Royal Society two bladders of this fictitious air, at the same time that Sir James Lowther made trial of his, they both exhibited the same phænomena. A particular account of the preparation made use of is as follows.

Mr. Maud took ʒij of oil of vitriol, and mixed it with ʒviij of common water, which he put into a glass with a flat bottom, about 10 inches wide, and 3 deep, with a long neck; to this he added ʒij of iron filings: there instantly arose a great heat, with a violent ebullition, and the iron was wrought upon very fast, with a copious exhalation of fumes. To the end of the neck of the glass he luted a bladder, void of air, the neck of the bladder being fastened to a tobacco-pipe; the fumes arising from the dissolving metal soon puffed up the bladder to its full extent, when that being taken away, the neck of it being first tied close with a string, he applied another in the same manner: thus may be got as many bladders full as you please, while the effervescence lasts. Two of these bladders were tried before the Society, and exhibited a flame like those of Sir James Lowther's, very like in the smell, though somewhat different in the colour of the flame. After Mr. M. had pressed part of the air out of the bladder, by drawing back the hand, the flame was sucked into the bladder, which set on fire what inflammable air remained all at once, and went off like a gun, with a great explosion.

It is worthy of notice in this experiment, that all the air which filled the bladders was, as it were, generated de novo out of the mixture, or else recovered from being locked up in the body of the metal in an unelastic state.

This experiment will easily explain a very probable cause of earthquakes, volcanoes, and all fiery eruptions out of the earth; for nothing more is requisite than iron, a vitriolic acid, and water. Now iron is generally found accompanied with sulphur; and common sulphur may be analized into an inflammable oil, and an acid liquor like oil of vitriol. This acid therefore in the bowels of the earth, by being diluted with a little water, surrounds the iron and works upon it in the same manner as described above; an effervescence and intestine heat arise; the air which comes from the mixture is rarefied, and becomes very elastic; its impetus, as it is the more compressed by the incumbent weight of earth, is increased even to an unlimited degree; and at length, like gunpowder, will remove all obstacles, and exhibit to the spectators above ground the terrible phænomena of earthquakes and eruptions. Sometimes these inflammable fumes, if very much heated, as soon as they come to the open air, will catch fire, and so produce those fiery eruptions, of which there are so many instances in the world.

An Account of the Storm, Jan. 8, 1734-5. *By Mr. Henry Forth.* N° 442, p. 285.

At Darlington, 14 miles south from Durham, lat. 54° 46′, the evening before the 8th of Jan. 1734-5, Mr. Forth's barometer stood at 29 inches, but had been gradually falling for two days. The wind was then S. W. and of the second degree of strength; which increased towards midnight a degree more. Most of the day there was snow or sleet.

The 8th in the morning he found his glass fallen to 28.38 inches, and at 4 o'clock P. M. down to 28.05 inches, but by 10 in the evening risen again to 28.45 inches. All this while the wind was in the north east, with only a moderate gale, though attended all day with snow, which at night was 2 inches and a quarter deep; and about 8 it began to freeze. As the wind in the southern parts was all that time in the opposite quarter, Mr. Forth would have expected an accumulation of the air, and as a consequence the rising of the barometer, at the time of its falling the lowest. Had the storm been the night before, when in the northern parts the wind was in the same direction, and had afterwards fallen, he would then have imputed the fall to the quick return of the current of air to restore the equilibrium.

Of the Bones of Animals changed to a Red Colour by Aliment only. By John Belchier, Surgeon, F. R. S. N° 442, p. 287.

That the circulation of the blood is carried on through the bones, is evident (Mr. B. observes) from many phænomena in surgery; but that it is universally and intimately distributed through the most solid and compact substance of the bones, though hitherto by some made a matter of doubt, will appear undeniably from the instances here produced; which are the bones of several hogs, of a different breed, changed to a deep red colour, merely by aliment. And what makes this still more surprising is, that neither the fleshy nor cartilaginous parts suffer the least alteration in colour or in taste.

The diet of these hogs was bran, after it had been boiled in a copper with printed callicoes, in order to clean them from a dirty red colour occasioned by an infusion of madder root, which is used to fix the colours printed on the cloth. Some of these colours are made with preparations from iron, others with a mixture of alum and sugar of lead. The parts printed with the preparation of iron, produce black and purple; those printed with the mixture of alum, red of different degrees, according to the strength of the mixture. The bran having absorbed the red colour discharged from the cloth, was mixed with the common food of the hogs, and produced this effect on their bones.

On examining these bones, Mr. B. observes in general the solid parts to be most tinctured, and the teeth particularly, except the enamelled part, which is of a different substance; and on sawing them through, he found the internal parts equally tinged, except at the ends of the bones, where the substance is more spongy. And in order to discharge the colour, he macerated them in water for many weeks together; boiled them often, and steeped them in spirits, but all proved ineffectual; nor was the least tincture given to any of the liquids, on which he made experiments.

An Observation of a white Liquor resembling Milk, which appeared instead of Serum separated from the Blood after it had stood some time. By Alexander Stuart, M. D., F. R. S. N° 442, p. 289.

One John Wicks, a carver, in Bromley-street, about 40 years of age, had been ill about 3 weeks by a loss of appetite and indigestion, and at last a pain and distention of his stomach, with a low degree of an inflammatory fever; his tongue dry, rough, and of a rusty brown colour in the middle, with a white soft list on each side; his urine very high coloured, with a large quantity of a slimy pink-coloured sediment; his stools very yellow and loose.

Eight ounces of blood being taken away; instead of serum, nothing appeared above the coagulum but this white liquor, resembling milk, which Dr. S. poured off to the quantity of about 4 oz. at first there was no smell perceptible; but in 6 days it began to have the smell of rotten eggs: it stood in a room where there was a fire, for some hours of the day, for 3 weeks more, in which time it did not alter its consistence nor smell.

The patient had eaten very little for a week before Dr. S. first saw him; and only a little of a calf's foot stewed, the night before, for supper, and no breakfast that day. He was addicted to drinking strong pale malt liquor every day he was in health.

If this be chyle, it is a substance very different from milk, which is apt to turn sour and thick by keeping, and never contracts the putrid smell of rotten eggs, as this did. Whether it be not chyle turned putrid, and near to purulency, by a long circulation in the blood-vessels, but not converted into blood, through some defect in the sanguification, is a question which probably cannot be decided, without more observations and experience.*

The coagulum of the blood was covered with a sizy pellicle, about the thickness of a shilling. The red part was of a grumous, tender, incoherent consistence.

Though he was much better in a week's time, the Doctor ordered 5 oz. of blood to be taken away, to see what change had been made; and he found the coagulum covered with a sizy pellicle to the thickness of half a crown, the red part of a due consistence, the serum clear, without any chyle.

The urine became clear, and he recovered in about 2 weeks after the Doctor had first seen him.

An Account of what was observed on opening the Corpse of a Person who had taken several Ounces of crude Mercury internally; and of a Plum-Stone lodged in the Coats of the Rectum. By Dr. Madden, of Dublin. N° 442, p. 291.

Some time ago Dr. M. was present, with Dr. Robinson, and Mr. Nichols, surgeon-general at Dublin, at the opening the body of a gentleman in that town, who for several years had found great difficulty in going to stool. This disorder increased on him towards the latter end of his life, and he was seized with a violent distemper, of which Dr. M. says he could give no description, having never attended him. In order to procure a passage downward, which

* It was probably the albuminous fluid existing in an excessive proportion in the blood; and if so, it would have yielded a coagulum had it been exposed to a sufficient degree of heat; but to this simple test it was not subjected.

Dr. M. supposed was a principal complaint, he took, by the advice of a physician, several ounces of crude mercury, at different times, without any relief, and at length he died.

On opening the abdomen, which was very much distended, there burst forth a great quantity of wind, though the guts and stomach were not wounded. The stomach was empty; and the inner coat was very much inflamed from one end to the other. In several places of the small guts, there were scattered grains of crude mercury, and along with them generally a black gritty powder, very like Æthiops mineral, which was doubtless the mercury changed into that consistence.

The colon was distended, at its origin, to twice the thickness of an ordinary man's arm about the shoulder. This extraordinary thickness extended to about the length of 10 or 12 inches; from hence it gradually decreased, and where it was attached to the stomach, it was not above a third part of that size. It was much inflamed at its origin, and contained at least 6 quarts of liquid excrement, in which was observed crude mercury, as also some of the black powder abovementioned.

The colon, where it parted from the stomach, and diverged towards the left kidney, adhered about the space of 3 inches to the omentum; and on separating the adhesion, there was found an abscess and inflammation, which had communicated to those parts of the ileon, contiguous to the colon. The colon had in this place a perforation, about $\frac{3}{4}$ of an inch in diameter, and 4 smaller perforations, about the size of a goose quill, through which some excrement had passed into the abdomen.

The coats of the colon, as it approached the intestinum rectum, became scirrhous, about the space of 6 inches, and the capacity was gradually smaller. The valves of the colon about this place were of a reddish colour, and were more scirrhous than the other parts of the intestine. The coats of the colon, where it was continued to the rectum, were at least half an inch thick, and its capacity was not above the fourth part of the natural size.

On cutting the gut horizontally hereabouts, there was perceived a body which stopped the passage, and seemed to the touch almost of a cartilaginous consistence. Having opened the gut lengthwise, it was found that it was no more than 2 of the valvulæ conniventes coli become scirrhous, and which protruded downward into the rectum.

There was also found a small plum-stone in this place, which was quite buried in the tunica villosa, and had made itself a bed between the coats of the rectum. It had formed a small abscess, which discharged into the cavity of the pelvis; but it had no communication with the cavity of the rectum.

A Solar Eclipse observed at Rome, May 3, 1734, N. S. By the Abbé de Revilus, F. R. S. and Andr. Celcius, F. R. S. N° 442, p. 294.

True time.

At 10^h 22^m 35^s, A. M. The eclipse had begun a little.
11 5 0, The greatest obscuration 2 digits.
11 51 0, The end of the eclipse.

The Description and Manner of using an Instrument for measuring the Degrees of the Expansion of Metals by Heat. By Mr. John Ellicott. N° 443, p. 297.

AA, (fig. 6, pl. 4) represents a flat plate of brass, which, for more strength, is screwed down to a thick piece of mahogany: on this plate are screwed three pieces of brass, two of which, marked BB, serve as supports for the flat iron bar C; and which, on account of its use, is called the standard bar. The upper part of the third piece of brass is a circle, about 3 inches diameter, divided into 360 equal parts or degrees: within this circle is a moveable plate, divided likewise into 360 parts, and a small steel index. The brass circle in the figure is marked D, and the moveable plate d. On the standard bar is laid the bar of metal E, on which the experiment is to be made, as E.

F is a lever, 2½ inches in length, fastened to an axis, which turns in two pieces of brass, screwed to one of the supports, marked B: to the end of this lever is fastened a chain, or silk line, which, after being wound round a small cylinder, to which the index in the brass circle D is fastened, passes over a pulley, and has a weight hung to the end of it: on the axis, to which the lever is fixed, is a pulley, ¼ of an inch diameter, to which a piece of watch-chain is fastened; the other end of this chain is hooked to a strong spring, marked G, and bearing against one end of the metal E.

H is a lever, exactly of the same form and dimensions with the former; but the chain fastened to the pulley on its axis, is hooked to the standard bar.* The line fastened to the end of this lever, after being wound round a cylinder, to which the moveable plate is fixed, passes over a small pulley, and has a weight hung to the end of it; or rather the same line passing under a pulley, to which the weight is hung, has its other end fastened to the lever F: thus one weight serves for both levers, as in the figure.

From this description it is plain, that whenever the bar E is lengthened, it gives liberty to the weight to draw the lever F upwards by its action on the

* N. B. The chain to the former pulley being fastened to a spring, and not directly to the metal E, is only for the more easy shifting the metals.

spring G; and the index will, at the same time, by means of the silk line, be carried forward in the circle; and as the bar shortens, it will return back again; the same motion will be communicated to the standard bar. The lengthening the bar the 20th of an inch, will carry the index once round the brass circle, which is divided into 360 degrees; therefore, if the metal lengthens the 7200th part of an inch, the index will move one degree.

To make an experiment with this instrument, lay a bar of any kind of metal, as E, on the standard bar; then heat the standard bar to any degree of heat with a lamp, and mark the degree of its expansion, as marked by the moveable plate: observe also the degree of expansion of the metal E, by the heat communicated to it from the standard bar, as marked on the brass circle by the index: let the instrument stand, till the whole is thoroughly cold; then removing the bar E, lay a bar of any other metal in its place, and heat the standard bar to the same degree of heat as before, which is seen by the moveable plates marking the same degree of expansion. Then the index will show the degree of expansion of the second metal, as it did of the first; and by this means the degrees of expansion of different metals, by the same degree of heat, may be exactly estimated.

A further Account of the Bones of Animals being made Red by Aliment only. By John Belchier, F. R. S. N° 443, p. 299.

In the former account, p. 79, concerning the red bones of the hogs, Mr. B. mentioned, that the colour was occasioned by bran being mixed with their common food, after it had been used to clean printed callicoes; the colours of which were made, some from preparations of iron, which were the blacks and purples; others from preparations of alum, and sacc. saturni, which produces the red colours; and that madder root was used to fix these colours on the cloth. To which of these preparations the colour was owing, could not then be determined. Some were of opinion, that it was entirely occasioned by the preparations of iron; others, that it was the whole blended together. But, to clear up this point, Mr. B. made the following experiments. The first was made on a cock, by mixing some of the madder root with fig-dust, on which they feed. The cock dying within 16 days after his first feeding on the madder, he was dissected, and the bones examined, not in the least expectation of finding them tinged in so small a time; but they were found universally of a red colour. So that, from this experiment it appears, that the madder alone causes this alteration. But why the bones only are affected, must be determined by future experiments.

The Immersions and Emersions of the four Satellites of Jupiter, for the Year 1738, *computed to the Meridian of the Royal Observatory at Greenwich. By James Hodgson, F. R. S.* N° 443, p. 301.

Mr. Hodgson's usual annual calculation of the eclipses of Jupiter's satellites: but omitted, for the same reason as before.

On the Viper-catchers, and their Remedy for the Bite of a Viper. By William Burton, M.D. at Windsor. N° 443, p. 312.

William Oliver and his wife called on Dr. B. with their vipers, and either of them offered to be bitten by any viper, and to suffer their arm to swell for some time; and then, by the external application of a common cheap remedy, in a few hours to remove all the symptoms. Accordingly the experiment was made in the town-hall at Windsor, before Dr. Derham, Dr. Waterland, the physicians, apothecaries, and surgeons of this town, and many other gentlemen of the neighbourhood. The man was bitten in the upper joint of the thumb, and higher upon the same arm, by two different fresh vipers. His thumb, hand and arm soon after swelled much, and all the usual symptoms of a viper-bite followed. He applied the remedy [sallad oil] before us, with the promised success.

An Account of the Experiments made June 1, 1734, *before several Members of the Royal Society, and others, on a Man, who suffered himself to be bitten by a Viper, or common Adder; and on other Animals likewise bitten by the same, and other Vipers. By Cromwell Mortimer, M.D. Secr. R. S. With some Remarks on the Cure of the Bite of a Mad Dog.* N° 443, p. 313.

William Oliver and his wife, from Bath, who follow the business of catching and selling vipers, offered themselves to be bitten by any viper that should be procured, trusting to the virtue of a remedy they had discovered in trying a variety of things, when the woman was once accidentally bitten, and the usual known medicines, even the oil of vipers, had no effect in assuaging her pains, especially of her breast on the same side as the hand in which she had received the wound. This remedy, which is only common oil of olives, and, from its use with sallad, is commonly known by the name of sallad oil, recommends itself not only for its efficacy, but also on account of its being readily to be come at when accidents happen.

On the 1st of June 1734, in the presence of a great number of persons, the said William Oliver was bitten by an old black viper, or adder, brought by one of the company, on the wrist and joint of the thumb of the right hand, so that drops of blood came from the wounds. He said that he immediately felt a violent pain and shooting from the wounds, both to the top of his thumb and up his arm, even before the viper was loosened from his hand; soon after he felt a pain, resembling that of burning, trickle up his arm; in a few minutes his eyes began to look red and fiery, and to water much: in less than half an hour, he perceived the venom seize his heart, with a pricking pain, which was attended with faintness and shortness of breath; on which he fell into violent cold sweats: in a few minutes after this, his belly began to swell, with great gripings and pains in his back, which were attended with violent vomitings and purgings. He said, that during the violence of these symptoms, his sight was gone twice, for several minutes at a time, but that he could hear all the while. He said, that in his former experiments he had never deferred making use of his remedy longer than when he perceived the effects of the venom reaching his heart; but this time, being willing to satisfy the company thoroughly, and trusting to the speedy effects of the oil, which had never failed him, when used in time, he forbore to apply any thing till he found himself exceedingly ill, and quite giddy.

About an hour and quarter after he had been first bitten, a chafing-dish of glowing charcoal was brought in, and his arm, the cloths being stripped off, was held over it, as near as he could bear it, while his wife rubbed in with her hand the sallad oil, (which Dr. M. had procured and kept himself in his pocket, lest they should privately add any thing to it, having bought it by the name of Lucca oil). Turning his arm continually round, as if she would have roasted it over the coals; he said that the pain soon abated, but the swelling did not diminish much, most violent vomitings and purgings soon ensued, and his pulse became so low, and so often interrupted, that it was thought proper by the physicians present, to give him the following cordial draughts, at about a quarter of an hour's distance between each.

1. ℞. Aq. lact. pæon. comp. āā. ℥iij. Sp. lavendulæ ʒi. m. pro duobus haustibus.

2. ℞. Confect. Raleigh. ʒss. aq. theriacal. ℥iss. sp. c. c. gtt. x. m. f. haustus.

3. ℞. Confect. Raleigh. theriac. Andromach. āā. ʒss. sal. c. c. gr. v. aq. theriacal. ℥ij. pro duobus haustibus.

He said he was not sensible of any great relief from these cordials; but that a glass or two of olive oil drank down, seemed to give him some ease.

Continuing in this dangerous condition, he was put to bed, as soon as one could be got ready for him, where his arm was again bathed with his remedy over a pan of charcoal set by the bed-side; but continuing to complain much of his back and belly, Dr. M. advised his wife to rub them likewise with sallad oil, heated in a ladle over the charcoal: which she did accordingly; on which he declared he found immediate ease, as if by some charm; and he had not above two or three retchings to vomit and stools afterwards, but made water plentifully, which was not discoloured. He then soon fell into a sound sleep; only was often interrupted by persons coming to see and inquire after him, till near 12 o'olock, from which time he slept continually to 5 or 6 next morning, when he awaked, and found himself very well; but in the afternoon, on drinking some rum and strong beer, so as to be almost fuddled, the swelling returned with much pain, and cold sweats; which abated soon, on bathing the arm as before, and wrapping it up in brown paper soaked with oil.

Immediately after the man, two pigeons were bitten by the same viper. They soon sickened, and seemed giddy. Nothing being applied, the one died in about an hour's time, the other half an hour after. The flesh of both was turned quite black, as if mortified; the blood was coagulated, and looked black.

June 3, the man's arm remained swelled, looked red, marbled with spots of yellow, but felt soft; and he had the perfect use of it, and even of his fingers, no pain or stiffness being left. He then caused a small spaniel dog to be bitten on the nose by a fresh viper; some oil was immediately applied hot, and rubbed well in, till all the hair of his nose was thoroughly wet; the dog did not seem very uneasy; his nose only swelled a little; he eat soon after; his nose was bathed once more that evening; he was found very well next morning; but his nose was bathed again, to make sure of his cure; he remained perfectly well without any symptoms ensuing, and was alive and well a year after.

Another pigeon was likewise bitten under the wing at the same time as the dog, but by a fresh viper. The oil was immediately applied hot, and rubbed well in, and the feathers of the wing were thoroughly wetted with it. This bird did not seem at all disordered with the venom, but ate soon after, and was found well the next morning, without any remarkable inflammation or swelling about the part. The hot oil was rubbed in again for 2 or 3 days, twice a day, and the bird continued well, so that the viper-catchers carried it with them out of town in triumph, having never before experienced the efficacy of their remedy on so small an animal; which, as it receives the same quantity of venom by a bite as a larger one, is more liable to die under it; and they kept it alive above 3 months, when they killed it and ate it.

They said that they had experienced their remedy to take effect on cows, horses, and dogs, 10 hours after being bitten; but that for themselves, who were frequently bitten in the fields, as they caught the vipers, they always carried a phial of sallad oil along with them, that as soon as they perceived themselves wounded, they without any loss of time bathed the part with it; and if it was the heel, they wet the stocking thoroughly with it; if the finger, which happened oftenest, they poured some of it into that finger of their glove, which they immediately put on again, and thus never felt any further inconvenience from the accident, not even so much as from the sting of a common bee.

Perhaps the oil may be found of use in the bite of rattle-snakes, and other venomous animals; especially if we consider, that in the fields a man seldom or never receives more than one bite at a time, which does not infect him with so much venom as was instilled into the man's blood, when in these voluntary experiments he suffered himself to be bitten twice together; and had likewise been bitten 3 times but about a week or 10 days before; some remains of which venom, it is highly reasonable to imagine, might still infect his blood at the time he repeated the experiments, so as to make a fresh quantity of the venom operate with greater violence on his body, than if he had been quite a fresh man, never infected with the like poison before, or at least at so great an interval of time, that his blood might have been entirely free from all remains of such an acrid infection. From these experiments is it not reasonable to imagine, that the oil by itself may be as efficacious against the sting of a scorpion, as if scorpions were infused in it?

Thus then a remedy against the bite of the viper is as publicly known as the famous Dampier's powder against the bite of a mad dog, first published by Sir Hans Sloane, Bart. when secr. R. S. in N° 237 of these Transactions, Anno 1698;* which afterwards, when he was president of the Royal College of Phy-

* The lichen cinereus terrestris is mentioned as being said to be exceedingly efficacious in curing dogs bitten by mad dogs; in a letter of Mr. Oldenburg's, secr. R. S. Lond. July 6, 1672. See Derham's Collection of Philosophical Letters between Mr. Ray and his Correspondents, p. 110, printed at London, 1718, 8vo.

Dr. M. thought it proper to add the following passage taken out of the Journal-book of the Royal Society, supposing it to be what Mr. Oldenburgh hints at in his letter.

"Nov. 16, 1671. (Sir Robert Moray) exhibited a certain plant, which was by Mr. Wray called lichen terrestris cinereus, said by Sir Robert Moray to be very good to cure dogs bitten by mad dogs: His Royal Highness having caused it to be given to a whole kennel of dogs, bitten by a mad one, which were all cured, except one of them, to whom none of it was given."

The specimen was kept in the repository.

The same virtue is likewise ascribed to this plant, in the third part of Morison's Plantar. Hist. Oxon. published at Oxford, Anno 1699, in folio, p. 632, where the author, speaking of the lichen terrestris cinereus, Raii Hist. et Synops. says, Adversus morsum canis rabidi egregium est medicamentum.

Dampier, and the College of Physicians, in their Pulv. Antilyssus, prescribe equal quantities of the sicians, by his proposal, was introduced into the London Pharmacopœia, under the name of Pulvis Antilyssus, Anno 1720. The composition of which is, ash-coloured ground liver-wort and black pepper: the manner of giving it, not only to men, but to dogs and cattle, being accurately set down in the above-mentioned Transaction. Thus it is hoped, that certain cures are discovered for the only two sorts of venomous bites of animals, to which the happy soil of Great Britain exposes its inhabitants.

A brief Account, by Mr. John Eames, F.R.S. of a Work entitled, The Method of Fluxions and Infinite Series, with its Application to the Geometry of Curve Lines, by the Inventor Sir Isaac Newton, Kt. &c. Translated from the Original, not yet public. To which is subjoined a perpetual Comment upon the whole, &c. by John Colson, M. A. and F. R. S. N° 443, p. 320.

This posthumous work of our late excellent president, a translation of which we have now received from the hand of the learned and ingenious Mr. Colson, has been long and impatiently expected by the curious in these matters; and now it appears, it will fully answer, if not exceed, those expectations, as well as confirm the reputation the author has so justly acquired by his other writings. For it is written with the same genius and acumen, it explains the principles of his method of fluxions with great clearness and accuracy, and applies those principles to very general and scientific speculations in the higher geometry. And further to explain this work, and to supply such things, for the use of common readers, as the author, according to his usual brevity, has often omitted; the translator has given a comment on a good part of the work, and has promised the rest at a proper season.

lichen and pepper: but Dr. Mead, in a single quarto leaf, published by him Anno 1735, altered the proportions of the composition, prescribing double the quantity of lichen to that of the pepper. This difference in the proportions must be left to the judgment of practitioners; but on the authority of another minute in the Society's Journal-books, it may not be improper to make an addition to the abovementioned Dampier's powder.

"March 7, 1671-2. Sir Robert Moray mentioned, that a whole kennel of dogs, belonging to his Royal Highness, were bitten by a mad dog, and had been lately cured by a certain herb called stellaria, or star of the earth."

This plant is the lychnis viscosa, flore muscoso Casp. Bauhin. in English, Spanish catch-fly. See these Transactions, N° 187, where is a receipt to cure mad dogs, &c. in which this plant is a principal ingredient; which receipt, communicated by Sir Robert Gourdon, was there published by his Majesty's (special) command, Anno 1687.

Therefore, suppose the composition were to be thus:

Take ash-coloured ground liver-wort, black pepper, and the herb Spanish catch-fly, all finely powdered, of each 2 drachms, for 4 doses, to be taken as Dampier prescribes in his letter in these Transactions, N° 237. Orig.

This text may very well be divided into three parts: an introduction, containing the method of infinite series; the method of fluxions and fluents; and lastly, the application of both to the most considerable problems of the higher geometry. The comment consists of very valuable and curious annotations, illustrations, and supplements, in order to make the whole a complete institution for the use of learners.

Of an Inguinal Rupture, with a Pin in the Appendix Cœci, incrusted with Stone; and some Observations on Wounds in the Guts. By Claudius Amyand, Esq. F. R. S. N° 443, p. 329.

Oct. 8, 1735, Hanvil Anderson, a boy 11 years of age, was admitted into St. George's Hospital, for the cure of a hernia scrotalis, which he had from his infancy, and a fistula between the scrotum and thigh terminating into it, which for a month before had discharged a great quantity of an unkindly matter. The rupture was small, and not troublesome, and part of it could be replaced; but as it appeared that the sinuous ulcer sprung from that part that could not, it was evident that the cure of the fistula depended on the cure of the hernia, which latter could be obtained by no other operation than that for the bubonocele, which was agreed to, and performed the 6th of December following.

This operation proved the most complicated and perplexing Mr. A. ever met with, many unsuspected oddities and events concurring to make it as intricate as it proved laborious and difficult.

This tumour, principally composed of the omentum, was about the size of a small pippin; in it was found the appendix cœci perforated by a pin incrusted with stone towards the head, the point of which having perforated that gut, gave way to a discharge of fæces through the fistulous opening in it, as the portion of the pin obturating the aperture in it shifted its situation. The abscess formed in the hernial bag occasionally, and the suppuration for 2 months last past from this place outwardly, had knit and confounded, and, as it were, embodied together the gut and omentum with the hernial bag, and these with the spermatic vessels and the testicle; so that it was as difficult to distinguish them from each other, as it was to separate them without wounding them; this pin, whose point was fixed in the omentum, continually shifting its situation, and occasioning a discharge of fæces. The pin frequently lying in the way of the knife, and starting out of the wounded gut, as a shot out of a gun, the inundation of fæces on this occasion, from a gut which could not well be distinguished, were so many difficulties in the way; but the greatest yet was, what to do with the gut, which all this while was unknown, and could not be known till the operation was over; for this appendix cœci, which was the only gut found in the rupture, was so contracted, carnous, duplicated, and changed in its figure and substance, that it was impossible to determine what kind of gut it was, or to find out that it was only this appendix elongated, and in disguise.

None of these difficulties were apprehended when the operation was undertaken, in which Mr. A. proceeded as usual; the omentum, lying uppermost in the hernial bag, was dissected from the parts it was knit to, and particularly the gut it was embodied with, and afterwards cut off close to the abdominal muscles, without any previous ligature, the vessels in it being small, and its substance more like a sweet-bread than a caul.

Much time was spent in this dissection; the operator being straitened for room, and greatly disturbed by the discharge of the fæces coming out of the gut, on every motion the pin lodged in it, and the omentum suffered, on the separation of these from each other. The gut forming a double tube, like a double-jointed syphon, continuing in the curve as it passed over the testicle and spermatics, was separated one part from the other, and from the adjacent parts, as far as the aperture in the abdominal muscles, where its unperforated end was separated from them, and thence stretched out and unfolded, which brought in view the aperture made in it by the pin, hitherto concealed, through which that part of it, which was incrusted with chalk, had just made its way out on an occasional pressure, as a cork out of a bottle. It was the opinion of the physicians and surgeons present, to amputate this gut: to which end a circular ligature was made about the sound part of it, 2 inches above the aperture; and this, being cut off an inch below the ligature, was replaced in the abdomen, in such a manner that an artificial anus might be made there, if the patient's case should require it. Afterwards so much of the hernial bag as had been detached from the skin, the spermatics, &c. was cut off, which, as they appeared in a sound state, were preserved in situ. The fistulous opening adjoining to the thigh, and answering to the aperture in the gut, was opened; some angles of skin in the way removed; the aperture in the muscles, which had been enlarged by incision, was stopped up with a tent; and the rest of the dressings and the situation of the patient ordered so, as to remove from the wound all such pressure from within, as might disturb the cure.

It is easy to conceive that this operation was as painful to the patient as laborious to Mr. A; it was a continued dissection, attended with danger on parts not well distinguished; it lasted near half an hour, during which the patient vomited largely, and had several stools, but was soon composed by $\frac{1}{4}$ oz. of diacodium, and emollient embrocations and fomentations, frequently applied warm on the belly; he was blooded, and an emollient carminative oily clyster was ordered to be applied in the evening; but as he was easy, and the belly not tense, that was omitted. He was confined to a very spare diet, and his body kept open by clysters, injected every second day, when stools were wanted, to prevent straining. When dressed on the 4th day after the operation, every thing appeared well, and there was good reason to hope for a cure, especially as the discharge by the anus was natural. The tent put into the abdominal aperture was not removed till the 8th. On the 10th, the ligature round the appendix cœci, where it had been amputated, dropped off, and no fæces followed it; and as it was then plain they had taken the natural course, from that time the wound was treated like an ordinary one, only care was taken to keep a strong and constant pressure over the abdominal aperture, as well to fence against the intrusion of the viscera into the wound, as by a strong incarnation and cicatrix, effectually to secure the patient against a rupture. During the time of the cure he was confined to his bed, always kept to spare diet, and ordered never to go to stool but in a bed-pan; by these means the wound was completely healed up in less than a month, and the patient soon after discharged with a truss, which he was ordered to wear some time, to confirm the cure.

That the appendix cœci should be the only gut found in this rupture, is a case singular in practice: this was full of excrements, and occasionally could be distended with an additional quantity, which on pressure was returned into the colon, with that kind of noise which guts replaced generally give. This had occasioned a diminution of the tumour when compressed, before the operation was performed, as the patient was lying backwards with his head downwards, and an increase of it as he stood erect, when the fæces from the colon could get into it again.

The patient does not remember when he swallowed the pin, which had perforated the gut within the rupture. But as this rupture was from his infancy, fixed and unreducible, it is likely the pin had then made its way into the appendix cœci prolapsed; and that an inflammation ensuing, had occasioned an adhesion, by which the increase of the tumour had been checked, and the reduction of the prolapsed parts rendered impracticable.

The surgeons who constantly dressed the patient before the operation, observed then, as they have since, that the humour discharged formerly at the fistula, had frequently the appearance and the smell of excrements, so that there is no doubt that the cause of it was the wound made in the gut, by the pin giving way occasionally to such a discharge. The patient also perfectly remembers, that the imposthumation or gathering, preceding the fistulous discharge, was attended with very little pain, or much less than generally attends suppuration. Which shows that the extravasation of the excrements from the gut into the hernial bag, and the bursting of this bag, were the cause of the fistulous discharge, and of the continuance of it outwardly.

As to the pin found in the rupture at the time of the operation, it is observable, that two-thirds of it, incrusted with a chalky matter, were confined and concealed within the gut; the other third, next the point, had made its way through it, the point of which was so lodged in the omentum, where it was fixed, as to leave a free passage for the excrement from the perforated gut outwardly, whenever the perforation in the gut, on shifting the position of the inclosed pin, could open, and afford a passage for the discharge of the fæces this way, which was as often as this conical or pyramidal pin altered its place, or did not exactly obturate the aperture in the appendix cœci, it exactly fitted. The aperture made in the gut by the pin lay concealed, the point being lodged in the omentum, lying parallel with the gut, which was here duplicated, where it was so secured. that it seemed almost impossible it could ever make its way out of this place, and its other confinement in the gut, as the aperture was callous, and so resisting that it was with some violence it was forced out of its confinement, through an aperture fitted for the point only, and so straight, that the report on its coming out, was like that of a cork out of a bottle; for though it appeared that the opening had occasionally been enlarged, as the incrusted part of the pin was pressed forward into it, yet it is plain that nature's attempts to get rid of it had been fruitless, and might possibly have been so during all the patient's life.

Sir Hans Sloane has furnished the curious with instances of bodies incrusted in the guts with stone, and of some making their way out, when there was little probability of it. Daily experience shows how far nature will struggle to free herself; so that it is always most eligible to trust them to her care: this may appear from the difficulties that have attended the cure of this case, which at last did not prove so successful as it was first hoped for; for the patient having been remiss in the wearing of his truss, on some effort the guts found a way into the inguen again, 6 months after the healing of the wound. This case also shows, that the best operation, or the utmost care, is no security against the relapse of a rupture. This is the third or fourth instance Mr. A. had met with, of the insufficiency of this operation to effectuate a cure of ruptures; and yet it is plain that this is far more likely to prove effectual than the caustic, or any other method cried up for the cure of this evil. In a growing age, a good spring truss is an effectual remedy; and in an adult, this should be the ultimate one, though it is no more than a palliative cure.

After mentioning 4 other cases of hernia and wounded guts, Mr. Amyand subjoins the following practical inferences.

Hence it appears, that the parts inflamed and in contact, have been coalesced and knit together, so as to prevent any extravasation from the wounded or bursted gut into the cavity of the abdomen.

That the cure in some cases has been owing to a free discharge of the fæces through the wound; and consequently that when, in a gut rupture, the part prolapsed cannot be reduced; a cure may be hoped for, by making such an opening in the guts, before they are entirely sphacelated, as may procure a free discharge to the fæces pent in, and thus secure the patient's life.

That if this happens to the colon or cæcum, its tube will so far be preserved, as to open a free discharge for the fæces the natural way; and if that cannot be obtained in a wound of the small gut, yet the discharge may be secured by making the wounds an artificial anus.

That the readiest way to obtain a cure of a wounded or bursted gut, is to keep it in contact with the outward wound, and the patient in a very low diet.

That the deligation of the vessels of the omentum, previous to its amputation, being liable to many exceptions, it is more eligible to forbear it, excepting when the vessels are large; for when reduced loose and floating, it is less liable to the inflammations and suppurations that attend the separation of the ligature.

*Experiments on Quicksilver. By Herman Boerhaave, M. D. &c. Part II.** N° 443, p. 343.

In this 2d part of his communications on this subject, as well as in the 3d part, which is inserted in another of the Transactions, but of which, for the reader's convenience, the substance will be introdueed here; Dr. B. prosecutes his experiments on quicksilver, for the purpose of ascertaining the truth or fallacy of its fixibility by the fire, and other properties ascribed to it by the alchemists.

Dr. B. subjected pure quicksilver for 15 years and a half (viz. from Nov. 1718, to May 1734) to the continued action of a degree of heat equal to 100 and upwards of Fahrenheit's thermometer, in a phial, which admitted the air without admitting any particles of dust. At the expiration of this length of time, he found the quicksilver in the phial still fluid, with the exception of a very small portion of a black powder on its surface, which powder was afterwards revived by trituration in a glass mortar. Being afterwards subjected to

* For Part I. see Philos. Trans. N° 430; of these Abridgments, Vol. vii, p. 619.

distillation, the whole of the mercury passed over into the receiver in the form of fluid quicksilver, without leaving any residuum in the retort. Hence (says Dr. B.) it is evident, that quicksilver is not fixable by long continued exposure to the degree of heat beforementioned.

But lest it should be objected that the access of the air prevented its fixation, Dr. B. subjected a quantity of purified quicksilver to 100 degrees of heat for ½ year (viz. from Dec. 1732 to July 1733), in close vessels. He afterwards subjected a quantity of this quicksilver to the heat of a sand-bath, nearly equal to the heat of boiling water, in vessels well closed. The quicksilver was found to be unaltered, excepting a very small quantity of black powder on its surface, which, as in the former instance, was revived by trituration. Being subjected to distillation, the whole of the quicksilver passed over into the receiver, leaving no residuum in the retort. The futility therefore of attempting to fix quicksilver is clearly proved.

But it had been asserted by some chemical authors and particularly by Van Helmont, that some metals were resolvable into quicksilver. "When lead (says the last mentioned chemist) is dissolved by alkalies and salts, or oil, which take in the sulphur and separate it from the body, the lead by this means becomes changed into a volatile running mercury, which can no more endure the fire, as before, but is cold and running like water, and without a metalline form." (Vide Paradoxical Discourses of V. Helmont, Lond. 1685. Part II. § 22.) The same thing is affirmed by Joachin Becher. (Vide Collectanea Quingentor. Experimentorum à p. 310, ad p. 333.) But after subjecting lead to processes similar to those described by the last mentioned authors, as well as to a treatment similar to that described by Isaac Hollandus; Dr. B. was unable to extract a single particle of quicksilver from that metal.

Dr. B. afterwards made a numerous and laborious set of experiments on amalgams of lead and quicksilver, of tin and quicksilver, and lastly of gold and quicksilver. After long-continued* digestion in a heat of 84, the amalgam of lead being distilled, the quicksilver passed over into the receiver, minus 43 grs. in 3 oz. (the total quantity of quicksilver operated upon in this experiment) in part accounted for by the red powder (precipitate per se), which remained behind in the retort, which was not volatile in the aforesaid degree of heat. The weight of the lead was found to be the same as before the operation, viz. 1 oz. Hence it appears that no mercury is obtainable from lead† by amalgamation, digestion and distillation with quicksilver, in the manner abovementioned.

* From the 11th of Feb. 1732, to the 10th of Jan. 1735.

† In other words, none of the lead is convertible into mercury.

An amalgam of tin and quicksilver in the same proportions (viz. 1 oz. of tin and 3 oz. of quicksilver) being digested in the same degree of heat, and afterwards subjected to distillation; there passed over into the receiver 2 oz. 4 drs. of quicksilver. There remained behind in the retort a powder, the finer part of which consisted of fixed mercury,* and the coarser black portion of particles resembling tin. There adhered to the neck of the retort a small quantity of quicksilver, weighing 2 drs. 5 grs. The residuary mass of tin weighed 1 oz. 1 dr. 9 grs. There was a loss of 46 grs. This experiment shows that quicksilver cannot be extracted from tin; but more than a seventh part of the quicksilver remained combined with and fixed in the tin, even when subjected to a red heat for the space of 4 hours.—Dr. B. mentions that the quicksilver distilled from the lead and tin amalgams, on being shaken in a clean white glazed earthen vessel soon deposited a black stain on the sides of the vessel: that on being spread out on writing paper, the said quicksilver left a black trace where it passed over the paper; and lastly, that when at rest, the surface of such quicksilver was always covered with a greasy pellicle or film. Hence he supposes, that some of the particles of these metals may be volatilized by the quicksilver; but this he observes is very different from the actual conversion (as some have supposed) of a portion of these metals into quicksilver.

The results of Dr. B.'s experiments on amalgams of gold and quicksilver, are related in the 3d part of his communications on this subject. After subjecting an amalgam of gold and quicksilver to digestion and repeated distillations,† the gold was found to be of the same weight, and to possess the same properties as it did before it underwent this treatment. The quicksilver was in part converted into a very fine brown powder, of a sharp metallic taste, which however was again brought to the state of fluid quicksilver, by the action of a stronger degree of heat. Thus there was no purification of the quicksilver by the gold in this experiment, the latter metal being found unaltered after the operation.

When an amalgam of gold and quicksilver is exposed to the fire, the silvery appearance of the amalgam is changed first into a brown, and afterwards into a black colour; but by the action of a higher degree of heat, the yellow colour is restored to the gold, and the silvery lustre to the quicksilver. From these experiments it is inferred, that all hopes of fixing quicksilver by subjecting it to the action of the fire with gold, must be relinquished for ever.

* i. e. not volatile in the beforementioned degree of heat.

† In the course of these experiments, the distillations were repeated 877 times!

A Partial Eclipse of the Moon, observed at Wittemberg, Oct. 2, 1735, *N. S. By J. F. Weidler, F. R. S.* N° 443, p. 359.

At 0^h 59^m A. M. The eclipse began.
3 36 The eclipse ended.

Account of a Shock of an Earthquake felt in Sussex, Oct. 25, 1734. *Communicated by Charles Duke of Richmond and Lenox, &c. F. R. S. And of another in Northamptonshire, in Oct. Anno* 1731, *by the Rev. Mr. Wasse.* N° 444, p. 361.

On Oct. 25, 1734, between 3 and 4 in the morning, there happened an extraordinary earthquake in Sussex. And what confirms the Duke in the opinion that there really was an earthquake, is, that almost every one agrees in the same description, as to the sensation, the hour of its happening, and the perfect calm that was at that time. His Grace observes that the shock was vastly more felt towards the sea-side, as at Shoreham, Tarring, Goreing, Arundel, and Havant. At his house of Goodwood, which is near 3 miles north of Chichester, and about 7 from the sea, it was not so perceivable as at Chichester, and where it was still less so than by the sea-side. It is not heard as yet that there was the least touch of it in any parts of the vale on the north-side of the Downs, which for the most part run east and west. What Dr. Bayley of Havant says of the different motions of the beds, according to the different situations they were in, seems very well worth observing, being a very curious man. Above 50 more accounts might be collected from the several places abovementioned; but as they all tend to the same purport, the following may be sufficient.

An Account of the same Earthquake at Havant in Sussex. By Edw. Bayley, M. D. N° 444, p. 362.

Oct. 25, 1734, between 3 and 4 o'clock in the morning, an earthquake was felt at Havant, in Sussex: the shock was so considerable, as to be observed by one or other in most houses of the town. Happening to be awake at that time, the Dr. perceived the bed shake under him, with a quick tremulous motion, which continued about 2 or 3 seconds, then ceased; and after a very short intermission was repeated in the same manner, and lasted about the same space of time. He was at first much surprised at such an unusual phenomenon; but on a little recollection, concluded it must be occasioned by an earthquake, and was soon confirmed in his conjecture by the concurrent observatons of the neighbours, and afterwards by accounts of the same from many other places; in some of which it seems to have been more violent than at Havant. Several persons in this place say, that they not only perceived the shaking of their beds, but also the rocking of their houses, with a rumbling noise of drawers, and the like moveable goods, in their chambers, and other rooms. A learned and ingenious gentleman in that town affirmed, that the motion of his bed appeared to him like the tossing of a vessel when it crosses over a wave, the head and feet rising and falling alternately several times; whereas the Dr.'s seemed rather to rock from side to side: but these contrary motions of the two beds are easily accounted for, by considering their different positions, the former standing directly east and west, and the Dr.'s north and south: for supposing the undulatory motion, which the earth might have at that time, was propagated from east to west, the same kind of motion which caused the former bed to rise up and down lengthwise, must make the latter rock from side to side; as may be observed in two vessels sailing in contrary directions on the same waves of the sea, that which crosses the waves at right angles being tossed up and down endwise, while the other moving in a line parallel with the waves, is rocked from side to side. What makes the Dr. more inclined to think the progressive motion of this earthquake was from east to west, is, because it appears from the best accounts, that it was observed sooner east than westward, and likewise extended farther from east to west, than north and south.

The Dr. thinks it may not be amiss to take notice of some remarkable phænomena, which happened before and after, as well as some other circumstances which immediately attended this earthquake, most of them agreeing with those signs which have been observed by the learned to precede or accompany former earthquakes, in these and other parts of the world. It was observable, that there had been of late more rain and wind for several months successively, than for many years past; especially from the beginning to the middle of this month; about which time it cleared up, and the weather became suddenly very cold, with frosty mornings, the wind blowing generally pretty hard from N. W. On the 23d, the cold abated considerably; it was cloudy, but no rain. The 24th was very calm all day; it rained most part of the afternoon, though the mercury stood at $30\frac{8}{10}$. It continued very calm all night, and rained hard for some time before and after the earthquake happened; but it soon cleared up, and a strong gale of wind rose within half an hour, or, as some say, within a quarter afterwards: it continued blowing hard all the forenoon.

Philip Boisdaune, Esq. of the parish of Funtington in the county of Sussex, and many other persons, all agree, that there was a manifest shock of an earthquake felt on Oct. 25, about ¼ before 4 in the morning, which lasted by fits some few seconds, about ¼ of a minute, or while one might deliberately count to 20: for most of the accounts concur in this particular, that the chairs, wainscot, doors, chests of drawers, and other moveables, were heard rattling; and one, that a bell rung of itself just before they felt the heaving of their beds; and that there was no wind stirring at that time, but that it rained, and the wind rose soon after.

Of a Shock of an Earthquake felt in Northamptonshire, in October 1731. *By the Rev. Jos. Wasse.* N° 444, p. 367.

About 4 in the morning, Oct. 10, 1731, the Rev. Mr. Jos. Wasse, rector of Aynho in Northamptonshire, says, that his windows rattled, as if somebody had been dancing over-head. The concussion lasted about 1 minute; others thought it lasted about 2 minutes. It alarmed the neighbouring villages, Bloxham, 4 miles south-west from Anyho; Barford, 5; Banbury, 4 west; Adderbury, 1 mile west; Crowton, 1 mile to the east; and Charlton, as much to the north. There was no notice of its progress south or south-east. About 1 minute after, some of the town of Aynho saw a great flash of lightning. In the morning the sky looked of a land-colour. It was said that there was a former shock felt upon Oct. 8, about 3 in the morning; and that the latter was preceded by a noise like distant thunder.

It is remarkable, that this shock was perceived to extend more from east to west, than from north to south; which particular was likewise observed in the last shock felt in Sussex 1734.

Experiments on Quicksilver. By Dr. Boerhaave, Part III. N° 444, p. 368. *From the Latin.*

The substance of this paper has been incorporated with Part II. inserted at p. 93, of this vol. of the Abridgments.

Concerning an Improvement of the Diving Bell. By Mr. Martin Triewald, F. R. S. Captain of Mechanics, and Military Architect to his Swedish Majesty. N° 444, p. 377.

Mr. Triewald having made trials with the diving bell and air barrels in several depths, on the coast of the Baltic, according to the ingenious improvement of Dr. Halley, made in the year 1716, but with some small additions; he found by experience, that no invention founded on any other principles than those of the campana urinatoria, can be of use in any considerable depths; or that the diver, in any other invention whatever, can be a single moment safe. As to the many inconveniences that attend other inventions, he only mentions that of a water armour, in which the man is drowned in an instant, when such a machine receives the least leak; whereas experience has shown, that when such an accident has happened to the diving bell, as to his knowledge it did once, when the diver was 12 fathom under water, and a pretty large hole happened to be struck in the bell, by a bolt of the wreck he went upon, when the air rushed out of the same with such violence as astonished the beholders by the excessive boiling on the surface of the water, fearing, not without reason, that the man in the bell was drowned; but he clapped his hand to the hole or leak, and gave a sign to be hauled up, which was done with all the ease and safety as if no accident had happened to him, the water having only risen about half a foot into the bell by this leak.

The very same diver that was then in the bell is 63 years of age, and has used the business of diving ever since he was 20, in a common diving bell. He declares that never a worse accident happened to him in his business, except once, when the bell he was in rushed down at once about a fathom or more, by the carelessness of those that worked the bell; at which time the blood came out of his nose and ears, feeling besides an intolerable pressure on his whole body; which shows, that when a man in a diving bell is slowly and gradually let down, he at such a time and by degrees respiring compressed air, which by the lungs is forced into the blood, cannot feel the external pressure, though of highly compressed air, surrounding him, and that of the water reaching some parts of his body; which convenience no other invention can yield or afford, where the diver is to draw his breath from air in its natural state.

Mr. T. has often with a great deal of pleasure observed, that when he has caused the bell to stop, being lowered down 5 fathom, and the diver taking in the air contained in an air barrel, lowered down a fathom deeper than the bell, without opening the cock for discharging the hot air; the water would, by the access of the air out of the barrel, be almost all expelled out of the bell; and when the same was again lowered down 5 fathom more, the same operation with another air barrel repeated, and the bell afterwards hauled up, it was no small matter of delight to see, that every fathom the bell came up, it would discharge itself of the superfluous and large quantity of air; which came up from the bottom of the bell in very large bubbles, as large as ostrich eggs; which discharge of air and phenomenon continued till the equilibrium of the air in the bell, and pressure of the water, were restored, and till the bell came above the surface of the water.

At other times he has observed, when no air was by the way taken into the bell, but the same lowered down the common way, and hauled up again after some time, that the very instant when the bell should part with the surface of the water, the strength of two men more was required at the capstan at that time, than before and after the bell hung freely in the air; from whence he thinks it plainly appears, that the air which passes through the lungs of an animal loses its elasticity, and that the lungs of a man make a kind of a vacuum in the bell; for which reason the diver feels at the instant when the bell parts with the water, a very smart pressure in his ears.

Though experience thus has taught that no invention is more safe and useful than the campana urinatoria, with the ingenious improvements of Dr. Halley; yet M. Triewald has found, that this invention is not to be used without considerable charge; requiring a large vessel, and number of hands, to the working and managing of such a large diving bell, and the air barrels with their respective weights for sinking; which charges however, according to the depth of water, and the value of what is to be brought up from the bottom of the sea, may not be regarded; but since it more frequently happens in these parts, that cargoes of a far less value than the loadings of Spanish galleons, &c. are to be dived for; then next to the goodness of the invention, he has found it necessary to think how the expences might be lessened, and the diving bell still answer all the purposes of Dr. Halley's; which improvement is as follows:

The diving bell, AB, fig. 8, pl. 2, is made of copper, and reduced to a very little compass, in respect to Dr. Halley's, by which means it is easily managed by two hands: yet Mr. T. thinks that a diver may not only live in the same as long, and with as much ease, at a very considerable depth of water, as in a bell of twice its capacity, for this reason, though a man in a large bell has undoubtedly more air than in a less, and consequently should be able to subsist much longer on a large quantity of air than on a small parcel: yet as his head is kept chiefly in the upper part of the bell, occupied by the hot air, he receives very little or no benefit from the air under his chin or breast, though never so fit for respiration; which air in the lower parts of the bell will yet remain cool a long time after he has been in the bell, and with difficulty drawn his breath; which cannot be denied, and is very obvious to any who have been in a German bagnio, and such as are made use of in this country, where in a single room all the degrees of heat are to be felt, by means of a contrivance like stairs to the very top of the ceiling: a man on the uppermost step feels an excessive heat, so that one not much used to it cannot endure the same, nor draw his breath, but will faint away; whereas on the first, second, and third steps from the floor, the heat is very moderate; nay, sometimes the air near the floor pretty cool, when at the same time near the ceiling the heat is intolerable.

To obviate this inconvenience, he caused a spiral tube of copper, bc, to be placed close to the inside of the bell, so fixed that it may be easily taken out and cleansed at pleasure; and at the same time not to incumber the diver in the bell. At the upper end of this tube, a flexible leather tube is joined, 2 feet long; at the end of which is a turned ivory mouth-piece, which the diver, as soon as he perceives the air to grow hot in the top of the bell, keeps constantly in his mouth, which he is able to do in any position, by means of the flexible tube, standing, sitting, bowing his head, &c. And all the while he draws his breath through it, and the air from c; by which contrivance he not only draws continually cool and fresh air as long as any is in the bell, but occasions at the same time a circulation, which is so necessary to the very being of air, especially in a compressed state, and its preservation for the use of animals; and so much the more necessary, as any body who has been in a diving bell for a long time without any new supplies of air, and has been reduced to the last extremity of breathing in it, will agree, that when at such a time the bell begins to be drawn up, and by that means the compressed air allowed to expand, and be put into motion ever so little, the man receives as it were a new life, and incredible comfort and ease.

Again, when, in coal-pits, levels are driven in the coal, or through dykes, the air of the level or adits growing hot by the breath and sweat of the hewers and workmen, for want of a circulation of the air; he has found it to be an excellent remedy, to place along the side of the drift or adit, a square wooden box, open at both ends, laid from the place where the air is cool and good, reaching as far, by joining one box close to another, as where the work is carried on. Thus, by this simple contrivance, a circulation of air is obtained, and sometimes to that degree, that when a candle is held at the end of the box where the cool air enters, the flame is driven out by the current of cold air entering and circulating through the box.

By which experiment he thinks, that though the diver should not keep the end of the flexible tube in his mouth, which he may easily do, yet that the air would circulate through the copper tube, and he will receive no small benefit by it. DD are the weights for sinking the bell, so contrived as easily to be hooked on the same hanging on the cable. The iron plate E, fixed to the chains FFF, serves the diver to stand upon, when he is at work.

The bell is very well tinned on the inside; and as in all rivers, and the coasts of the Baltic, the water is exceedingly clear and limpid, because there is no ebb and flood, M. Triewald has placed strong convex lenses GGG; by which means the diver can not only see what is under him, but also on all sides at a good distance.

These glasses have strong copper lids, HHHH, like snuff boxes, which are shut, when there is no occasion to discover any objects in the bottom of the sea, and serve to preserve the glasses from being broken.

A Description of the Moose Deer of New England, and a sort of Stag in Virginia*; *with some Remarks on Mr. Ray's Description of the flying Squirrel of America. By Mr. Samuel Dale.* N° 444, p. 384.

The moose-deer has been mentioned by several authors; but their accounts have generally been so very imperfect, as to afford little satisfaction to the curious enquirers into natural history. The first mention that Mr. Dale finds made of this animal, is by Mr. Josselyn, in a small tract, called New England's Rarities; where he says, "that it is a goodly creature, some of which being 12 feet high, their horns exceeding fair, with broad palms; some being 2 fathoms from the top of one horn to the other." Much to the same purpose is the account he gives of this animal in another book, called Two Voyages to New England, where he says, "that the moose or elk is a creature, or rather a monster of superfluity, when full grown, being many times bigger than an English ox." What Neal, in his History of New England, Vol. ii, p. 573, has of this animal, called by him the mose, is copied from Josselyn. The best and fullest account of this animal was sent by Mr. Dudley, and published in Phil. Trans. N° 368, where he makes them to be of 2 sorts. viz. the common light grey moose, called by the Indians, wampoose; and the large or black moose. As to the grey moose, Mr. Dale takes it to be no other than that which Mr. John Clayton, in his account of the Virginia quadrupeds, published in Phil. Trans. N° 210, calls the elk; which in the Memoirs for a Natural History of Animals, published at Paris, and rendered into English by Mr. Pitfield, p. 167, is called by the name of the stag of Canada, of which Mr. Dale has seen a single horn, sent by Mr. Mark Catesby from Virginia, by the name of an elk's horn, which was in all respects like those of our red deer or stags, only larger, weighing about 12 pounds avoirdupois; and from the burr to the tip, measured by a string, about 6 feet high. Mr. Dudley writes that his grey moose is most like the ordinary deer; that they spring like them, and herd together sometimes to the number of 30 in a company. But whether he means the red, the Virginian, or the fallow deer, is uncertain, having said nothing of their horns, which was necessary to distinguish them. The black moose is accounted by all writers a very large creature. Mr. Josselyn makes it many times larger than an ox; and Mr. Dudley writes, that the hunters have found a buck or stag-moose 14 spans high from the withers; which at 9 inches to the span,

* The moose is the American variety of the *cervus alces* of Linnæus.

is 10 feet and a $\frac{1}{4}$; and that a doe or hind of the fourth year, killed by a gentleman near Boston, wanted but 1 inch of 7 feet in height. The stag, buck, or male of this kind, has a palmed horn, not like that of our common or fallow deer, but the palm is much longer, and more like that of the German elk; from which it differs, in that the moose has a branched brow-antler, between the burr and the palm, which the German elk has not.

Fig. 7, plate 4, represents the head, or rather the attire, as it is called in heraldry, of a black moose-deer, which was sent to Mr. Dale from New England; the dimensions of which are, as follow:

AB 56 inches, CA 34, CE 31, CD 34, DH 30, FG $9\frac{1}{2}$, FI 14, KL 7.

The horn of this New England black moose agrees not in figure with either of those mentioned in Phil. Trans. N° 227, and N° 394, found fossil in Ireland; the last of which Mr. Kelly writes, that for want of another name, they called them elks-horns. Mr. Dale suspects that those horns Mr. Ray mentions, in his Synopsis Methodica Animalium quadruped to have seen with Mr. Holney, an apothecary at Lewis in Sussex, as also in divers museums, were not the horns of this black or American moose, but of the German elk; because that inquisitive gentleman takes no notice of any brow-antlers they had, which Mr. Dale thinks was too remarkable to have escaped his observation, had there been any such.

As to the number of young ones, or calves, which the moose brings forth at a time, authors vary: for, Mr. Dudley says, they bring forth only 2: but Josselyn in his 2 Voyages, and from him Neal, that they bring forth 3; and that they do not go so long pregnant, as our hinds, by 2 months. What these two last-mentioned authors write, as to their casting their calves a mile distant from each other, does not seem probable; nor does Mr. Dale find that Neal, in his description of this animal, makes any mention of their having a long tail, though charged so by Mr. Dudley, who also omits the brow-antlers in his description of their horns.

There is another beast of the deer kind, which though very common in Virginia, and doubtless in others of the northern provinces of America, yet so far as Mr. Dale knows is not described by any author. Mr. Beverley, in his present state of Virginia, mentions both elk and deer in that country, but does not describe either.

But by what Mr. Dale received from Mr. Catesby, the first should be the Canada stag, and the other the deer here mentioned. Mr. Clayton also mentions the elk, which he says are beyond the inhabited parts, and are the same with Mr. Beverley's; as also the deer of which he says there are abundance; yet he does not describe them, but calls them red deer, though they are not the same with what we here call by that name, but of those that follow.

That which Mr. Dale takes for the undescribed deer, is of the stag-kind having round horns like them, not spreading out as in the stag or red deer, but meeting nearer together at their tips, and bending forwards over the face of the animal; the brow-antlers are not crooked, standing forwards, but straight and upright, as represented fig. 8, the dimensions of which are as follow:

ab 11 inches, acb 20, ad $12\frac{1}{4}$, df $12\frac{1}{4}$, de 11, gh $2\frac{3}{4}$.

The skin of this deer is of a sand colour, with some black hairs intermixed, and while young spotted all over with white spots, like some sorts of fallow deer; being likewise about their size when full grown. The Dama Virginiana Raii Synop. Animal. Quadruped. p. 86, which was formerly in St. James's-Park, seems to be different from this; if Mr. Willoughby was not led into a mistake in taking it to be of the palmate kind, by only seeing it when the horns were shed: perhaps this last of Mr. Ray may be the maurouse of Josselyn's Voyages, p. 91, which he says is like the moose, only his horns are but small, and the creature about the size of a stag; but his description is too short to be satisfactory.

There are other kinds of deer mentioned by Mr. Josselyn in his book, p. 87, as natives of that country; as the buck, stag, and rein-deer: but whether they are the same with those called by the same names in Europe, Mr. Dale cannot determine; their descriptions being omitted. Mr. Josselyn also mentions, as another kind of American deer, an animal called a maccari, caribbo, or pohana: but by the account he gives, it seems to be a fiction; no such animal being, Mr. Dale thinks, in rerum natura.

Mr. Ray, in his Synop. Quad. p. 215, rather refers the sciurus Americanus volans to the mouse, than to the squirrel kind,* because their tails are broad and plain, and not turned over their backs when they sit; which mistake may probably arise from only seeing the skin of a dead one, when the hair of their tails had been eaten off by mites: for, in one Mr. Dale saw alive, which was brought from Virginia, the tail was hairy, as in others of the squirrel kind, though rather thinner; and it turned over the back as in other squirrels.

Dr. Mortimer observes, that the same species of flying squirrel has been found in Poland; a description of which, with an accurate figure, is given by M. Klein, Phil. Trans. N° 427.

And that as to the large horns found fossil in Ireland; he has taken particular notice, in several he saw, besides the main horns being palmated, that the brow-antlers are so likewise; a circumstance peculiar to the rein-deer species,

* The American flying squirrel is of a pale rufous-brown colour, white beneath, and is the *mus volans*, Linn. The Polish or European flying squirrel is a different species, larger, of a grey colour, white beneath, and is the *sciurus volans*, Linn.

being of great service to them in removing the snow, in order to get at the grass or moss underneath, which is their chief subsistence in Lapland.

An Attempt to explain the Phenomenon of the Horizontal Moon appearing larger than when elevated several Degrees above the Horizon, supported by an Experiment. By Dr. Desaguliers. N° 444, p. 390.

This apparent increase of the moon's diameter, which a telescope with a micrometer shows to be only apparent, is owing to the following early prejudice we have imbibed from children. When we look at the sky towards the zenith, we imagine it to be much nearer to us, than when we look at it towards the horizon: so that it does not appear spherical, according to the vertical section EFGHI, fig. 9, pl. 4, but elliptical, according to the section eFghi. The sky thus seen strikes the eye in the same manner as the long arched roof of the isle of a cathedral church, or the ceiling of a long room.

This being premised: let us consider the eye at C, on the surface of the earth; and imagine C at the surface to coincide with K at the centre; to avoid taking into consideration that the moon is really farther from the eye when in the horizon, than when it is some degrees high. Now when the moon is at G, we consider it as at g, not much farther than G; but when it is at H, we imagine it to be at h, almost as far again. Therefore, while it subtends the nearly same angle as it did before, we imagine it to be so much larger, as the distance seems to us to be increased.

Dr. D. contrived the following experiment to illustrate this: he took two candles of equal height and size, AB, CD, fig. 10; and having placed AB at the distance of 6 or 8 feet from the eye, he placed CD at double that distance; then causing any unprejudiced person to look at the candles, he asked which was largest? and the spectator said they were both of a size; and that they appeared so, because he allowed for the greater distance of CD; and this also appeared to him, when he looked through a small hole. Then desiring him to shut his eyes for a time, Dr. D. took away the candle CD, and placed the candle EF close by the candle AB, and though it was as short again as the others, and as little again in diameter, the spectator, when he opened his eyes, thought he saw the same candles as before. Whence it is to be concluded, that when an object is thought to be twice as far from the eye as it was before, we think it to be twice as large, though it subtends but the same angle.—And this is the case of the moon, which appears to us as large again, when we suppose it as far again, though it subtends only the same angle.

The difference of distance of the moon in Perigeo and Apogeo, will account for the different size of the horizontal moon at different times, adding also the consideration of the faintness which vapours sometimes throw on the appearance.

An Explication of the foregoing Experiment, to account for the Appearance the horizontal Moon seeming larger than when higher. By the same. N° 444, p. 392.

Dr. Desaguliers having made an experiment with 3 ivory balls, for confirmation of what he had advanced, namely, that the deception arises from our judging the horizontal moon to be much farther than it is; which is as follows.

Two equal ivory balls, fig. 11, pl. 4, were set one beyond another in respect of the eye at E, namely, AB at 20 feet distance from the eye, and CD at 40. Now it is certain, by the rules of optics, that the eye at E or F, will see the ball CD, under an angle but half as large as it sees the ball AB; that is, that the ball CD must appear no larger than the ball op placed by the side of AB. But when looking at the two balls with the naked eye in an open room, we consider that CD is as far again from the eye as AB, and we judge it to be as large as AB, as it really is, notwithstanding it subtends an angle but of half the size. Now if, unknown to the spectator, or while he turns his back, the ball CD be taken away, and another ball op of half the diameter be placed in the same line, but as near again, at the side of AB, the spectator thinking this last ball to be at the place of CD, must judge it to be as large as CD, because it subtends the very same angle as CD did before.

It follows therefore—that if a ball be imagined to be as far again as it really is, we make such an allowance for that imagined distance, that we judge it to be as large again as it is, notwithstanding the angle under which we see it, is no greater, than when we look at it, knowing its real distance. For this reason, the moon looks larger in the horizon, and near it, than at a considerable height, or at the zenith: because it being a common prejudice to imagine that part of the sky much nearer to us which is at the zenith, than that part towards the horizon; when we see the moon at the horizon, we suppose it much farther; therefore as it subtends the same angle nearly, as when at the zenith, we imagine it so much larger as we suppose its distance greater.

The reason why this experiment is difficult to make, is because the light from the ball op is too strongly reflected on account of its nearness; but if we could give it so little light, as to look no brighter than the ball CD, it would deceive every person. Dr. D. has made the experiment so as to deceive such as were not very long-sighted; but he found it very difficult to deceive those who see at a great distance; though they would all be deceived, if the distances were of

300 or 600 feet. Now in the case of the moon, the deceit is helped, because the vapours, through which we see it when low, diminish its brightness, and therefore have the same effect as would (or does) happen in the experiment, when the light of the ball OP strikes the eye no stronger than the light of the ball CD.

Some Observations on a Man and Woman bitten by Vipers. By Joseph Atwell, D. D., F. R. S. and Principal of Exeter College, Oxford. N° 444, p. 394.

The man who had been bitten by a viper in the presence of several members of the R. S., was again bitten in the presence of several besides Dr. A. in the public hall of Exeter college at Oxford. He received two punctures in the wrist, a little above the thumb: the blood issued, and more venom lay on the orifices, than could be immediately imbibed. He complained in about half an hour's time, that the poison was got up to his shoulder, and was entering his body; but notwithstanding this, he was not suffered to apply his medicine [sallad oil] till an hour and 10 minutes after he was bitten: by which time he began to be flushed and in a sweat, his hand swoln and discoloured.

On an application of his medicine, he found some abatement of his pain; but the swelling appeared more visible, and spread itself farther into his arm. In about a quarter of an hour the man sunk under the table, and complained of violent pains in his back and bowels, and he could not bear to be moved. At last, his pulse failing, his jaw being fallen, his countenance changed, and eyes fixed, he was stretched upon the table, and the medicine was applied to his belly and stomach. Soon after which, recovering a little, he began to vomit, and brought up more than a quart of phlegm and bile. In this condition he lay for more than an hour; and then was removed into Dr. A.'s lodgings; where he was seized again with a fit of vomiting, and also purging, and so continued till midnight. Dr. A. kept him in his own house above an hour, in hopes of his growing better; but his disorder still continuing, and the man being too weak and feeble even to stand, he sent him in a chair home to his own lodgings; where he was put into bed, and after midnight fell asleep, and awaked the next morning perfectly well; excepting that his arm was still swoln, and the flesh pitted, as if it had been dropsical. His arm was bound up in papers, dipped in his own medicine; and this was all, as far as the Doctor could observe or learn, that was applied to it.

The same day they caused 2 young chickens to be bitten; one died in 2 hours, and the other in 4 hours time. A third was bitten 3 times, and then had the medicine applied; but it died at the end of 10 hours. The flesh of this last was grown very black, and there was much extravasated lymph between it and the skin, which stunk intolerably; but Dr. A. could not perceive, that the viscera were at all discoloured.

July 4.—Another fowl, half grown, was bitten in 2 places, and the medicine was applied: half an hour after which, the fowl eat meat, and seemed much recovered, but was dead in 14 hours time.

July 6.—Two half-grown cocks were bitten; the first was bitten but once, yet violently, and turned black immediately; it had the medicine applied, eat meat afterwards, and seemed pretty well; yet died in 20 hours. The other was bitten 2 or 3 times, but hardly wounded, and not half so much discoloured as the former: they bathed the wound with viper-oil, but the fowl died in little more than 2 hours.

July 8.—Two young pigeons were bitten; the one had viper-oil applied immediately, but sickened, and died in 4 hours: the other had olive-oil applied, and recovered perfectly; the flesh beginning to return to its natural colour in about an hour's time.

July 17.—The woman was bitten in the public hall of Brazen-Nose College, in presence of Dr. Frampton, Dr. Frewin, and several other physicians, Dr. A. and many others. It had been suspected, that they played some tricks with their vipers, and made them spend their rage and venom beforehand: to obviate which a physician of the company had provided some fresh vipers, which he himself had caught a day or two before, and kept in his own custody till that time. The woman was bitten twice by one of these, and received 3 wounds, one in the thumb and 2 in the fore-finger. Her hand was soon swoln and spotted, and her finger turned black. After 23 minutes, she applied the medicine to her hand, but not farther than the swelling went; in which she was perhaps to be blamed, and probably the following illness was in some measure occasioned by it. She walked home very well in appearance: but about 3 hours after the bite was received, she grew very sick, and in great pain; was seized with vomiting, purging, and fainting-fits, which continued all night, insomuch that her life was despaired of: nor had she any sleep till noon the day following. Dr. A. saw her about 6 that evening, when she awaked, and he found her very well in spirits, but complaining of most acute pains in her finger. Her arm, shoulder, back, and breast, on that side, were much swoln and inflamed: all those parts thus affected were bound up in papers soaked in the medicine. After this there appeared on her finger 2 large bladders, full of a black corrupt matter; and this not only on the wound, but one of them on a distant part of the finger from it. She could not be persuaded to open them, which the Doctor believes would have eased her considerably.

July 20.—The swelling was considerably abated, and almost reduced entirely into her hand, which began to pit: but she complained still of her finger, and could hardly endure to have it dressed with fresh papers. She continued in bed till the 22d, for the sake of keeping her hand in an easier posture; and then she came abroad.

The same day that the woman was bitten, they caused a fowl to be bitten; but the wound was not deep, and little more than a scratch. Nothing was applied to it, and it died in 20 hours. A large puppy was also bitten the same day, 3 times, in the head; it had the medicine applied, but died in about an hour.

It was known that the man and woman kept themselves fasting those days when the experiment was to be tried on them: this occasioned a suspicion that they might take some antidote to prepare their bodies: for which reason, Dr. A. ordered the man to bring him some vipers after dinner, under pretence of making some further experiments on dogs. He had provided at the same time some fresh vipers without his knowledge, and then proposed to him to be bitten by one of them, and apply his medicine immediately. His hand was besmeared with the medicine in applying it to a young dog, on which an experiment had just been made. Two vipers were tried on the man, but neither would bite him: one of them attempted it several times, and spilt his venom, but always caught back his head again, as if there had been something in the hand offensive. Suspecting that the smell of the medicine might occasion it, they made him wash his hand, after which another viper bit him immediately: but whether this conjecture was right or not, must depend on further trial. The man received the bite on the joint of the thumb, and the blood issued at the 2 orifices. He applied the medicine instantly: the thumb appeared black soon, the hand was swoln, and the flesh pitted immediately. He drank a mug of ale after it, and then went home to bed. Next morning his whole arm was swoln; but he was so well, that he went 6 miles out of town, and came home again in the evening. Dr. A. saw him again in the morning, when the swelling was almost gone above the elbow, but the flesh pitted below: the wound had blistered, but the bladders were filled with a water, and not any thing of that black matter which appeared on the woman's finger. They caused the young dog, beforementioned, to be bitten the same day, and applied the medicine: another dog was bitten 3 times in the nose, and nothing applied: both were much swoln, but very likely to live. They also thrust the teeth of a viper's head, cut off 24 hours before, into the flesh of a fowl, which turned black immediately; but the fowl perfectly recovered, without any application.

Some Electrical Experiments intended to be communicated to the Royal Society, by Mr. Stephen Gray, F. R. S. and taken from his Mouth by Cromwell Mortimer, M. D. R. S. Secr. Feb. 14, 1735-6, *being the Day before he died.* N° 444, p. 400.

Exper. 1.—Take a small iron globe, of an inch or inch and half diameter, which set on the middle of a cake of rosin, of about 7 or 8 inches diameter, having first excited the cake by gently rubbing it, clapping it three or four times with the hands, or warming it a little before the fire. Then fasten a light body, as a small piece of cork, or pith of elder, to an exceedingly fine thread, 5 or 6 inches long, which hold between the finger and thumb, exactly over the globe, at such a height, that the cork, or other light body, may hang down about the middle of the globe; this light body will of itself begin to move round the iron globe, and that constantly from west to east, being the same direction which the planets have in their orbits round the sun. If the cake of rosin be circular, and the iron globe placed exactly in its centre, then the light body will describe a circular orbit round the iron globe; but if the iron globe be placed at any distance from the centre of the circular cake, then the light body will describe an elliptical orbit, which will have the same excentricity as the distance of the globe from the centre of the cake.

If the cake of rosin be of an elliptic form, and the iron globe be placed in its centre, the light body will describe an elliptical orbit, of the same excentricity as the form of the cake.

If the iron globe be placed in or near one focus of the elliptical cake, the light body will move much swifter in the apogeè part of the orbit than in the perigeè part, contrary to what is observed of the planets.

Exper. 2.—Take the same, or such another iron globe, and having fastened it on an iron pedestal about one inch high, set it on a table: then set round it a glass hoop or portion of a hollow glass cylinder, of 7 or 8 inches diameter, and 2 or 3 inches high: this hoop must be first excited by warming and gently rubbing it; then hold the light body suspended as in the first experiment, and it will of itself move round the iron globe from west to east, in a circular orbit, if the hoop be circular and the globe stand over its centre, but in an elliptic orbit, with the same excentricity, if the globe does not stand in the centre of the hoop, as in the first experiment, when the globe does not stand on the centre of the cake.

Exper. 3.—This same iron globe being set on the bare table, without either the cake of rosin or the glass hoop, the small light body, being suspended as in experiments 1 and 2, will make revolutions round it, but slower and nearer to it, than when it is placed on a cake of rosin, or within a glass hoop.

Remarks.—Mr. Gray had not yet found that these experiments would succeed, if the thread, by which the light body was suspended, was supported by any other thing than a human hand; but he imagined it might happen the same, if the thread should be supported or fastened to any animal substance whatever; and he intended to have tried the foot of a chicken, a piece of raw flesh, or the like.

He thought to explain the foregoing particular, by the following odd phenomenon, of which he asserted he was very certain, having often observed it, viz. if a man, resting his elbows on his knees, places his hands at some small distance from each other, they will gradually accede to each other, without any will or intention of the man to bring them together; and they will again recede of themselves. In like manner, the hand will be attracted by the body; or the face of a man, if he stand near a wall, will be attracted to the wall, and be again repelled by it.

He told the Doctor, he had thought of these experiments only a very short time before his falling sick; that he had not yet tried them with variety of bodies, but that from what he had already seen of them, which struck him with new surprise every time he repeated them, he hoped, if God would spare his life but a little longer, he should, from what these phænomena point out, bring his electrical experiments to the greatest perfection; and he did not doubt but in a short time to be able to astonish the world with a new sort of planetarium never before thought of, and that from these experiments might be established a certain theory for accounting for the motions of the grand planetarium of the universe.

In trying these experiments since Mr. Gray's death, the Doctor found that the small light body will make revolutions round a body of various shapes and substances, as well as round the iron globe, if set on the cake of rosin. Thus he tried with a globe of black marble, a silver sand-dish, a small chip box, and a large cork. He observed that the cake, if nothing stood upon it, would in any part strongly attract the light body, as held suspended by the thread; but when the globe, or other body, was set upon it, the edges of the cake attracted the strongest, and so gradually the attraction seemed as it approached the centre to grow less, till at a certain distance it was changed into a repulsion, which proceeded from the globe, or other body placed on the cake, which very strongly repels the light body, unless it be held very near it, and then it attracts it strongly. While the light body is suspended, as in the foregoing experiments, if the finger of the other hand be brought near it, it will fly from the finger, or be repelled by it with great vigour.

Some Thoughts on the Sun and Moon, when near the Horizon, appearing larger than when near the Zenith. By James Logan, Esq. N° 444, p. 404.

It may perhaps be needless now to add any thing in confirmation of Dr. Wallis's solution, in the Transactions, N° 187, of the sun and moon's appearing so much larger at rising or setting, than when in a greater altitude; though some have very absurdly still gone on to account for it from vapours. It is true indeed, that it is these vapours, or the atmosphere alone, that make those bodies, when very near the horizon, appear in a spheroidal form, by refracting, and thereby raising the lower limb more than the upper; yet these can be no cause of the other. The sun and moon, each subtending about half a degree, appear in the meridian of the breadth of 8 or 10 inches, to some eyes more, and to others less; and in the horizon to be 2 or 3 feet, more or less, according to the extent of ground they are seen over; but if one can have an opportunity of seeing the sun rise or set over a small eminence, at the distance of a mile or two, with tall trees on it standing pretty close, as is usual in woods without underwood, his body will then appear to be 10 or 12 feet in breadth, according to the distance and circumstances of the trees he is seen through; and where there has been some thin underwood, or a few saplings, Mr. Logan has observed that the sun setting red, has appeared through them like a large extensive flame, as if some house was on fire beyond them. Now the reason of this is obvious, viz. that being well acquainted with trees, the ideas of the space they take up are in a manner fixed; and as one of those trees subtends an angle at the eye, perhaps not exceeding 2 or 3 seconds, and would scarcely be distinguishable, were it not for the strong light behind them, the sun's diameter of above 30 minutes takes in several of them, and therefore will naturally be judged vastly larger. Hence it is evident, that those bodies appear greater or less, according to the objects interposed, or taken in by the eye on viewing them. And to this only is that phenomenon to be imputed.

Mr. Logan acknowledges that, this method of arguing is not new; yet the observations here given may probably tend, he thinks, to illustrate the case beyond what had been advanced on the subject.

A Catalogue of the Fifty Plants, from Chelsea-Gardens, presented to the Royal Society by the Company of Apothecaries, for the Year 1735, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. Præs. By Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præs. ac Prælec. Botan. N° 445, p. 1.

This is the 14th annual present of 50 plants, completing the number of 700 plants presented.

The Case of a Lad bitten by a mad Dog. By Mr. Edw. Nourse, F. R. S. and Surgeon to St. Bartholomew's Hospital. N° 445, p. 5.

Stephen Bellass, about 16 years of age, was, some time in June 1735, bitten by a mad dog through the nail of his right thumb: Mr. N. being immediately called, proposed to make a ligature above, and to cauterize the wounded part: but that not being complied with, he desired Mr. Gernum the apothecary, who was present, to make up the remedy mentioned by Dampier in the Phil. Trans. N° 237, and N° 443, viz. ℞ Lichen. ciner. terrestris, Piper. nig. aa ℥i. f. Pulvis. Of this powder he took ʒi. within an hour after he was bitten; repeating it the next morning before he set out for Gravesend, where he was 10 days, and was dipped in the salt water every day; during which time he repeated the medicine night and morning, and continued so to do for 40 days.

The boy was without the least sign of being affected by the poison, till Tuesday the 11th of Jan. 1736-7, when in the evening he complained of a numbness in 3 of the fingers of the hand that was not bitten. Next morning he was sick, had great pain across his stomach, and in all his bones; in the evening Mr. N. was sent for to bleed him, the people about him supposing he had got cold. When he came, he found him feverish, with a hard full pulse: he asked what complaints he had? he told him those abovementioned. Mr. N. inquired what nourishment he had taken that day? the answer was, none, for he could not swallow: Mr. Nourse looked into his mouth, but there was no inflammation; neither did any thing occur that could produce the difficulty of swallowing. Mr. N. offered him some sack-whey in a basin, but he started at the sight of it, not suffering it to come near him: he was then offered a spoonful, which he was prevailed on to swallow: the moment it was down, he was convulsed, and a remarkable horror appeared in his countenance, which was succeeded by a profuse sweat all over his face and head. He afterwards took another spoonful; the consequence was as before, but in a higher degree. Mr. N. was now convinced that this was the ὑδροφοβία, and that it arose from his having been bitten 19 months before; for after the most strict inquiry, it did not appear that he had been bitten by any animal since; and if he had, it is very probable Mr. N. would have known it, his master living next door to him, and the boy knowing how much danger he was thought to be in, when he was bitten. Mr. N. acquainted his friends with his apprehensions, and desired further advice; on which Dr. Monro was sent for, who ordered him to be let blood, a repetition of the abovementioned medicine in a bolus every 4 hours, and a clyster: he was blooded, and the clyster was injected; but he could not be prevailed on to take more than 1 bolus. That night was spent with great inquietude, and without any sleep: Thursday morning he was generally convulsed, and had frequent retchings and yawnings alternately. About noon his mind, which till then continued sound, left him, and he raved and foamed at the mouth till 5 o'clock in the afternoon; at which time nature seemed quite spent, and he lay very quiet till 7, when he died.

Thus the poison was latent near 19 months; which Mr. N. finds mentioned by others, but it never fell within his own observation before.

An Explanation of the Runic Characters of Helsingland. By Mr. Andrew Celsius, R. S. Suec. Secr. F. R. S. and Professor of Astronomy at Upsal. N° 445, p. 7.

It is well known, that there are stones found in several parts of Sweden, which were formerly set up as obelisks in memory of the dead. These monuments are marked with the ancient northern letters, called Runor or Runic characters. But there is one province of North Sweden, named Helsingland, where 5 of those stones occur, which have characters cut into them, that seem to differ from the common Runic. On the introduction of our modern letters, these Runic characters became so little regarded, that their interpretation was lost even to the Swedish antiquarians, till the year 1674; when Magnus Celsius, the author's grandfather, then professor of astronomy in the university of Upsal, revived their reading, and drew up the following alphabet of them, ranged after the manner of the ancients, fig. 1, pl. 5.

There are but 16 letters, and the words are frequently distinguished either by three points set perpendicularly over one another, or by two at some distance asunder.

Among the several alphabets hitherto known, it would be difficult to find one like the foregoing; if we may not perhaps except the characters of the Persepolis inscriptions, which have not yet been deciphered. For the letters generally used signify different sounds, according to their various shapes: whereas in this alphabet, the same character often denotes a different sound, according to the diversity of its place and attitude between the two parallels. Thus a straight stroke, standing perpendicular to the parallel lines, signifies I, F, D and S. For when it joins these parallels, it signifies I; when it rests on the lower parallel, it signifies F; on the upper, S; and D when it touches neither of them. The small wedge leaning to the right, and placed near the upper parallel, denotes L; in the middle, N; and O, near the lower. A line descending from the upper parallel, and making a curve downward to the left, stands for K; the same placed contrarywise, from the lower parallel upward, expresses R: and so of the rest.

The intention of the first inventor of these letters, seems to have been, to form all the characters of small wedges, straight and crooked lines, and two points, variously placed between the two parallels. For the wedges may be placed 15 different ways, as represented in fig. 2.

The straight line may also have 15 different situations, as in fig. 3.

The crooked lines can likewise be varied 14 different ways, as in fig. 4.

Lastly, the two points admit of 12 variations, as in fig. 5.

But as the ancient Sueo-Gothi had but 16 letters in their alphabet, they did not want all these variations of the wedges, lines, and points: therefore they employed 6 variations of the wedges; of the straight lines, 5; of the crooked, 3; and but 2 of the points.

If we now suppose these Helsingic characters to be older than the common Runics, the greatest part of the common Runics can easily be derived from the Helsingics, by adding a perpendicular line to the small wedges and curves; as appears by fig. 6.

But if we suppose the common Runics to be older, and to be derived, as it is very probable, from the ancient Greek and Roman letters; we must, in the contrary way, deduce the Helsingic characters from the common Runics, by subtracting the perpendicular line.

As a specimen, fig. 7 represents a stone found at Malstad; of which M. Celsius took an exact copy in the year 1725, in company with his uncle, the Rev. Dr. Olave Celsius, of whom he expected a complete account of all these Helsingic inscriptions.—On the outward limb or border, is what is represented in fig. 8.—In the first curvature, as in fig. 9.—In the second snake or dragon, as in fig. 10.—In the inner limb, as in fig. 11.—In the second curvature, as in fig. 12.—In the first snake, as in fig. 13.—In the heads of the snakes, as in fig. 14.

The inscription of the figures is thus rendered into English:—

Frument erected this stone to Fisiulfi the son of Brisi: but Brisi was the son *of Lini. But Lini was the son of Un. But Un was son of Fah. But Fah the son of Duri. But he (the son) of Barlaf. But he the son of Drun: but he (the son) of Lanas: but he (the son) of Fidrasiv. Frumunt the son of Fisiulfi made these Runic* [*letters.*] *We have placed this stone to the north of Bala stone. Arva was the mother of Fisiulfi. Siulfir (or Fisiulfir) was the Governor of this Province. His place of abode was in Rimbium.*

That this monument was erected since christianity began to flourish in Sweden, sufficiently appears by the figure of the cross. It is probable that Fisiulfi, as the governor of the Province, was descended of a very noble family; seeing his genealogy is traced 10 generations backward. Now if we suppose Frumunt to have been 30 years of age when he erected this monument for his father, and, with Sir Isaac Newton, allow 30 years for each generation; we shall find 330 years from the death of Fisiulfi to the birth of Fidrasiv, who is the stock of these generations.

A figure of this stone is in M. de la Motraye's Travels; but with considerable errors in the windings of the snakes, and in the letters, as well as in the explanation of them.

A Collection of the Observations made on the Eclipse of the Moon, on March 15, 1735-6, *which were communicated to the Royal Society.*

A Lunar Eclipse observed in Fleet-street, London, March 15, 1735-6. *By Mr. Geo. Graham.* N° 445, p. 14.

10^h	13^m	the beginning.
11	11	the total immersion.
12	49	the emersion.
13	47	the end.

The same, by Dr. Halley at Greenwich, p. 14.

The beginning	10^h	13^m	37^s
The immersion	11	9	42

The same, observed in Fleet-street. By M. Celsius, F. R. S. p. 15.

10^h	22^m	5^s	the shade on the middle of Kepler.
11	9	17	the total immersion is about to begin.
13	45	50	the eclipse is nearly ended.
13	46	12	the eclipse is certainly ended.

*The same, observed in Covent Garden, London. By Dr. Bevis,** p. 16.

True Time.

10^h 9^m 40^s A thin penumbra commences near Hevelius.

10 10 20 the penumbra is now very sensible.

* John Bevis, M. D. and a valuable member of the R. Soc. was born Oct. 31, 1695, o. s. near Old Sarum in Wiltshire. At a proper age he was entered at Christ's College, Oxford, where he applied diligently as well to the study of physic, for the practice of which he was intended, as to other sciences, particularly astronomy and optics, in which he became a considerable proficient, both in theory and in practice. Having taken his degree of M. D. he left the university, and travelled through France and Italy, where he made respectable connections, and on his return commenced the occupation of a physician at and near London, where he had considerable practice.

But the study of physic afforded him little pleasure in comparison with that of contemplating the celestial bodies and their motions. As early as 1738 he had made an excellent collection of astronomical instruments, for furnishing a new observatory, which he had built at Stoke Newington near London. Here he became an indefatigable observer, having filled 3 folio volumes with observations made in the course of one year. From these he selected the most important parts, making one volume of 196 pages, on large paper, where it frequently appears that the transits of 160 stars, &c. have been observed by him in one night.

Dr. Bevis continued to observe the heavens with the same assiduity till the year 1745; when, from his vast collection of materials, he undertook the laborious task of arranging, and publishing by subscription, a work entitled Uranographia Britannica, or an exact view of the heavens, on 52 plates, similar to that of Bayer, representing the constellations, and all the fixed stars observed by former astronomers, with the addition of those observed by himself.

Those plates, so honourable to his country and to himself, though they have been engraved for so many years, have unfortunately been prevented from coming before the public; having entrusted the care of engraving the plates, and receiving the subscriptions, to a person who, after receiving several hundred pounds of the money subscribed, became a bankrupt; by which the work passed into the hands of the creditors, and thus has been lost to the world.

Dr. Bevis was the real author of a great many works, which have been well received by the public, but which his modesty prevented him from taking the merit of. It is to him we are indebted for the publication of Dr. Halley's Astronomical Tables, after they had been printed more than 20 years; having supplied some auxiliary tables, and the precepts for using them, he brought the whole to light in the year 1749.—At a meeting of the Board of Longitude, Sept. 18, 1764, Dr. Bevis was nominated to compute the observations made at Greenwich, and to compare them with those made at Portsmouth and elsewhere, for the purpose of ascertaining the accuracy of Mr. Harrison's Time-keepers.—In Mr. Simpson's Essays, p. 10, are delivered practical rules for finding the aberration, which were drawn up and given him by Dr. Bevis, with examples of the correction applied to several stars, which he had carefully observed with proper instruments; by which he has proved, the first of any one, that the phenomena are as conformable in right ascension, as Dr. Bradley, who made this great discovery, found them to be in declination.—Several pieces of the Doctor's were inserted in the few numbers that were published of a work, called The Mathematical Magazine, by Mr. Moss and Mr. Witchell, particularly a curious paper on the Satellite of Venus, and several sheets of a new Mathematical Dictionary.—Dr. Bevis enriched the Philos. Trans. with 27 valuable papers, mostly containing Astronomical Observations, viz. from vol. 40 to vol. 59 inclusive. He announced in the Journal des Sçavans, for August 1771, an English translation of La Lande's Astronomy,

True Time.			
10^h	11^m	40^s	beginning of the eclipse.
11	10	0	the total immersion.
12	46	56	beginning of the emersion.
13	46	25	the true shadow ends.
13	48	30	the penumbra no longer sensible.

The same observed at Yeovil in Somersetshire. Latitude 50 *Degrees* 52 *Minutes. By Mr. John Milner,* p. 18.

The beginning of the eclipse	10^h	6^m	0^s
Beginning of total obscuration	11	4	30
Middle of the eclipse	11	54	0
End of total obscuration	12	43	30
The end of the eclipse	1	39	15

Some Investigations, by which it is proved that the Figure of the Earth must approach very near to an Ellipsis, according to the Laws of Attraction Inversely as the Squares of the Distances. By M. Alexis Clairaut, F. R. S. and of*

made principally by himself; but this was never published, though left ready for the press at his death.—The only things which appeared separately with his name, besides the papers in the Philos. Trans. just mentioned, were two pamphlets, the one entitled "The Satellite Sliding Rule," for determining the immersions and emersions of Jupiter's four Satellites. The other was, "An Experimental Enquiry concerning the Contents, Qualities, and Medicinal Virtues of the two Mineral Waters lately discovered at Bagnigge Wells near London, &c. in 8vo. 1760."

Dr. Bevis made some curious experiments on the refractive power of glass, in the composition of which he had used a quantity of borax, and found the refrangibility was as great as that of English crystal. He corresponded with most of the principal astronomers in all parts of the continent; several of whom make honorable mention, in their works, of the civilities and attention they received from him, either during their stay in England, or by communications to them abroad.

On the death of Mr. Bliss, in 1765, his friends made great exertions to procure for Dr. Bevis the situation of Astronomer Royal, but the superior interest of Dr. Maskelyne secured the office for the latter.

A few years before his death, Dr. Bevis removed from his house and observatory at Stoke Newington, to reside in the Temple, London, for the better convenience of his occupation as a physician, and at the Royal Society; which occasioned an interruption in his astronomical observations. In this situation he died Nov. 6, 1771, at 76 years of age; his death having been occasioned by a fall he received a short time before, in going rather too hastily from his instrument to the clock, in observing the sun's meridian altitude.

In his disposition, Dr. Bevis was lively, amiable and liberal; extending his services to all deserving objects, under any kind of embarrasments.

* Alexis Claud Clairaut, F.R.S. and member of the French Academy of Sciences, &c. was a most respectable mathematician. He was born May 13, 1713, at Paris, where his father was a teacher of mathematics. He was it seems a kind of premature genius, which seconded by his

the Royal Academy of Sciences at Paris. N° 445, p. 19. *Translated from the Latin.*

According to Newton's Princip. (cor. 3, prob. 91, lib. 1, and prop. 19, lib. 3) if an elliptic spheroid, consisting of fluid and homogeneous particles, mutually

father's great attentions, produced very early and extraordinary effects. Having learned the letters of the alphabet from the diagrams in Euclid's Elements, he could read at 4 years of age, and even write tolerably well. In a similar degree of advance he passed through the mathematical sciences of arithmetic, algebra, geometry, &c. so as to master Guisnee's application of algebra to geometry, at 9 years of age. At 10 he studied l'Hopital's Conic Sections, and soon after the Analyse des Infiniments Petits, of the same author. At 12 he astonished the Academy of Sciences, by reading to them his discovery of four curves of the third order, by means of which may be found any number of mean proportionals between two given lines. And at 13 he laid the foundation of his excellent work on Curves of a Double Curvature, printed 3 years after.

The same year, 1726, our young author formed a juvenile society, by associating together a number of ingenious youths like himself, at once for improving themselves and the mathematical sciences; among whom were several who afterwards became some of the most respectable members of the Royal Academy of Sciences; of which academy young Clairaut himself was admitted a member at 18 years of age, being 3 years below the limit prescribed by a regulation of the academy, a regulation which they dispensed with in this instance on account of his surprising merit. The same year he presented to the academy two ingenious memoirs of his own inventions. Soon after this, he accompanied M. Maupertuis to Basle on his visit to John Bernoulli; and on his return, he found the academy much occupied about the question concerning the figure of the earth; in consequence not long after he and Maupertuis, retiring to Mount Valerien, formed the project of the measurements at the polar circle, in which both of them bore so conspicuous a part. In this retreat it was, that the Marchioness of Chatelet, having resolved to learn the science of geometry from Clairaut, attended him there to receive her lessons; which gave occasion to his composing his pleasant little treatise on geometry. On the question too of the figure of the earth, about this time, he wrote several interesting memoirs. The delicate observations of Mairan on the lengths of pendulums, gave occasion to Clairaut to present a memoir on their oscillations. And the discovery of Bradley on the aberration of the fixed stars, gave also occasion to Clairaut's presenting a valuable calculation on that subject, in which he made improvements, by extending his views to that of the planets also, dependent on the same cause.

Several other memoirs, on various subjects, as, the annual parallax of the stars, the nature of the refraction of light, conduct us to his still more important labours, in the application of the geometrical calculus to the profoundest considerations in physics and astronomy. This produced his work on the theory of the figure of the earth on hydrostatical principles; in which he considered all the circumstances and states of the earth, as to fluidity and rigidity. And next his theory of the moon, in which he at length detected a subtile error, which had been committed by all the best calculators on that delicate subject. After a long continued labour on this object, in 1751 he carried the prize proposed on the subject by the Academy of Petersburg. Also, in 1754 came out the first edition of his Lunar Tables; and in 1765 was given a second edition of the same corrected; to which was added the piece containing the theory which had gained the Petersburg prize.

During those labours, Clairaut composed his elements of algebra, which appeared in 1746; these elements are in the same easy and familiar stile as those of his geometry, beforementioned. In 1754 attracting each other in the inverse ratio of the squares of the distances, be revolved about the axis Aa, fig. 12, pl. 4, by which the columns CE, CN, CA, in that spheroid, may be in equilibrio, and so the spheroid may always have the same figure, the gravity at any point of the surface N must necessarily be inversely as the radius CN.

To know therefore whether the spheroid has this property, let us inquire what attraction any corpuscle N of the whole spheroid suffers, according to the direction CN; and from that attraction let us deduct that part of the centrifugal force which proceeds from the rotation of the spheroid acting in the direction CN; and then see whether the remaining force is proportional to $\frac{1}{\text{CN}}$. We shall first then investigate the following problem: and as we intend to apply our discoveries to the spheroid of the earth, which all agree to be very little different from a sphere, our computations must be adapted to those spheroids which have the smallest difference between the two axes.

PROB. I. To find the attraction, which the spheroid AEae, differing very little from a sphere, exercises on a corpuscle at the pole A.

For the solution of this problem we may repeat cor. 2, prop. 91, of the Principia, by which we learn the manner of finding the attraction of any spheroid, viz. by substituting in the general value of CE, a quantity differing infinitely little from AC; and as in that case the problem becomes much easier, we may solve it in the following manner.

Let AMDad be a sphere, to the radius AC; we must find the attraction of the space arising from the rotation of ADaE, which attraction added to the attraction of the sphere, will give the attraction sought.

To find the attraction of the space arising from the rotation of ANEaDM, call AC, r; DE, ar; AP, u; then, from the nature of the ellipse, $\text{NM} = a\sqrt{2ru - uu}$, and from the nature of the circle $\text{AM} = \sqrt{2ru}$. But the space arising from

he produced his work on the Determination of the Terrestrial Orbit, in which are considered the perturbations caused by the action of the other planets. And some time after he successfully applied the same principles to the theory of the celebrated comet of 1759; the result of which calculations are given in a work which appeared in 1760. And when, in 1758, the academy lost M. Bouguer, a pension of 3000 livres, which he enjoyed from the Marine Board, was divided between Monnier and Clairaut, which new engagement produced from his pen an excellent memoir on the manœuvering of ships.

A considerable share of his attention was also employed on the subject of achromatic telescopes, with the different kinds and combinations of glass, to render their effects colourless. These labours are contained in three memoirs printed by the academy in the years 1756, 1757, and 1762. But it were endless to particularise all his valuable labours.

This last year, however, terminated the useful existence of this great man; his death being occasioned by a severe cold he had taken, in returning home one night after supper, at 53 years of age.

the rotation of $NnmM$ will be $\frac{ac}{r}(2ru - uu)du$, where c is the circumference to the radius r.

Now because of the smallness of NM, we may account all the particles of matter contained in that space as equally attracting the corpuscle at A; therefore we shall have the attraction of that small space, if we multiply that solidity by the attraction at M, and that attraction at M is $\frac{1}{AM^2} \times \frac{AP}{AM}$. Thus will be had analytically $\frac{u}{2ru\sqrt{2ru}} \cdot \frac{ac}{r} \cdot 2ru - uu \cdot du = \frac{ac}{2rr\sqrt{2r}}(2rdu\sqrt{u} - udu\sqrt{u})$ the integral of which, $\frac{ac}{2rr\sqrt{2r}}(\frac{4}{3}ru\sqrt{u} - \frac{2}{5}uu\sqrt{u})$, is the attraction of the space arising from the rotation of ANM. In which value if we take $u = 2r$, we have by reduction $\frac{8}{15}ac$. Hence the attraction of the whole space AEac is expressed; then adding $\frac{4}{3}c$ for the attraction of the whole sphere, we have $\frac{4}{3}c + \frac{8}{15}ac$ for the attraction of the ellipsoid.

Corol.—For an oblong spheroid a will be negative, and the united attraction will be $\frac{4}{3}c - \frac{8}{15}ac$.

Note.—If the foregoing spheroid, instead of circular elements arising in PN, consist of other elements, for instance elliptical, which should differ from a circle no more than the ellipsis AE, and by which there would be the same superficies as by the circles PN, it appears that the attraction will be still the same, because in those elements PN, whatever the remaining force might be, the circles PM being taken away, it will be as it were composed of parts which would have the same attraction as on that of the ellipsoid, having regard to the smallness of NM, and to the equable quantity of matter.

LEMMA. Let KL be a circle, fig. 13, H its centre, VH perpendicular to the area of the circle, and NH = VH, but making with it an angle infinitely small, or very small: then the attraction of the circle KL on N, may be taken, without sensible error, as the attraction of the circle on V; or, which is the same thing, that the one attraction differs from the other only by a quantity infinitely less, with respect to both, than as VN is less in respect to HV.

To demonstrate which proposition, it must be shown, that two corpuscles being placed at the extremity of any diameter KL, there is one attractive force at N, and another force at V, the sum of which may be accounted the same. But, neglecting the computation for having the attraction of the body at K on the corpuscle at N, it will be easily seen that it will be the same with the attraction on V, to which should be added a small quantity involving NV. In like manner it may be seen, that the attraction of a body at L on the corpuscle N, will be the same as the attraction on V, deducting the same small quantity. Therefore the sum of both these attractions is one and the same.

Corol.—Instead of the circle KL, if there was a certain ellipsis, or any other curve line, which should differ very little from a circle, by the same arguments as in the note, it is easily gathered that the foregoing proposition would always hold good.

THEOREM I. Let AEae, fig. 14, be an elliptic spheroid, the axis of revolution being Aa; then the attraction this spheroid exerts on a corpuscle at N, is the same as that attraction, which any spheroid exerts whose pole is N, its axis of revolution Nn, and its second axis the radius of a circle, having the same superficies as FG, the elliptic section of the ellipsoid AEae, by a plane erected perpendicularly on FG, its conjugate diameter.

To demonstrate this, conceive innumerable elements KL, parallel to the ellipsis FG, that is, all erected on ordinates to the diameter, it is evident that the spheroid AEae will differ from the aforesaid spheroid only in this, that in the first all the elements make with CN an angle differing infinitely little from a right angle, but in the second all the elements make a right angle, without any difference, while in both spheroids the elements have the same superficies. But, by the preceding proposition, the attraction of every element KL on N, is considered as the same in both cases; but as to the thickness of the elements KklL, we may take Hh for the perpendicular hi, because of the smallness of the angle ihH; therefore the total attraction of both spheroids may be taken the one instead of the other.

PROB. II. To find the attraction of the spheroid AEae on a corpuscle at any point N.

Let AC $= a$, CE $= b$, CN $= r$, CG the conjugate diameter to CN will be $\frac{ab}{r}$, since a and b have very little difference; from the preceding proposition, find the attraction of the spheroid, whose greater axis is r, and the less $\sqrt{\frac{abb}{r}}$ or $b\sqrt{\frac{a}{r}}$.

For this, we must apply the formula which we found in prob. 1, viz. $\frac{2}{3}c - \frac{8}{15}ac$, or $\frac{2}{3}pr - \frac{8}{15}apr$, putting pr for c, but in this formula instead of a substituting $\frac{r - b\sqrt{\frac{a}{r}}}{r} = 1 - \frac{b}{r}\sqrt{\frac{a}{r}}$, or $\frac{3}{2}n - m$, putting $a + ma$ for b, and $a + na$ for r, and in the computation neglecting the second powers of n and m.

If therefore $\frac{3}{2}n - m$ be put instead of a, the aforesaid formula will become $\frac{2}{3}pr - \frac{4}{5}prn + \frac{8}{15}prm$, or $\frac{2}{3}pa - \frac{2}{15}pan + \frac{8}{15}pam$; which expression is the required attraction of the spheroid on N.

If $n = o$, then we have $\frac{2}{3}pa + \frac{8}{15}pam$ for the attraction on the pole a.

But if $n = m$, then we have $\frac{2}{3}pa + \frac{6}{15}pam$ for the attraction at the equator.

THEOREM II. Let AEae, fig. 12, as before, be a spheroid, whose axes differ by a very small quantity, which for greater perspicuity I shall call infinitely small. If this spheroid be conceived to be of a fluid and homogeneous matter, and revolved about the axis Aa in correspondent time, that the gravity of the column CE may be equal to the gravity of the column AC, that is, by the Newtonian principles, the attraction in E, neglecting the centrifugal force, may be to the attraction at A, as CA to CE: I say, that all the columns CN, wanting an infinitely small quantity of the second degree, will preserve an equilibrium with those two columns; that is, the attraction on N, neglecting the centrifugal force, simply in the direction CN, is to the attraction on A, as CA to CN.

For the demonstration, the same notation will serve as in the preceding proposition: first find the centrifugal force at E, which may agree with the equilibrium of the columns CE, CA. Then say, as $\frac{2}{3}pa + \frac{6}{15}pam - f : \frac{2}{3}pa + \frac{8}{15}pam :: 1 : 1 + m$, hence is found $f = \frac{8}{15}pam$.

Then for exhibiting the gravity at N composed of the attraction, omitting the centrifugal force, find the centrifugal force at N, or, which is the same, on M upon the sphere, which must differ from each other only by an infinitely small quantity of the second order, if DE be supposed to express the centrifugal force f at E, then MN will express the centrifugal force at N, for the centrifugal forces are as the radii, when the times of revolution are the same, and by the property of the ellipses it is DE : NM :: CE : MP.

But if the centrifugal force act in the direction NP, it must be reduced to NC, and NO will be the remaining part. Therefore the centrifugal force at N, or at M, is to the centrifugal force at E, or at D, as NO is to DE. Therefore the expression for the centrifugal force at N will be $\frac{8}{15}pan$, and consequently the expression for the gravity there will be $\frac{2}{3}pa - \frac{2}{15}pan + \frac{8}{15}pam - \frac{8}{15}pan$, or $\frac{2}{3}pa - \frac{2}{3}pan + \frac{8}{15}pam$.

Now to find the centrifugal force at N, which results from the equilibrium of the columns, the gravity at A must be to the gravity at N, as NC to AC; but the gravity at A is $\frac{2}{3}pa + \frac{8}{15}pam$, which expression being drawn into $\frac{1}{1+n}$ or $1 - n$, after reduction it becomes $\frac{2}{3}pa - \frac{2}{3}pn + \frac{8}{15}pam$, and is the same expression as that above.

Hence we see that there can be only an infinitely small difference between the figure which the earth ought to have by the Newtonian hypothesis, and the ellipsoid. For as the quantity DE is about the 230th part of AC, in the preceding computation we neglect only a quantity of the same order with $\frac{1}{230 \cdot 230}$.

On the Efficacy of Oil of Olives in curing the Bite of Vipers. By Stephen Williams, M. B. F. R. S. N° 445, p. 26.

In presence of several gentlemen of the faculty of physic, Wm. Oliver, the viper-catcher (mentioned N° 443,*) suffered himself, on June 26, 1735, to be bitten by a female viper; which being enraged, fixed her fangs in the middle part of his fore-finger. Blood soon issued out at the wounds: but that the poison might more strongly appear, the same viper immediately bit a pigeon in the breast, which expired in less than half an hour. Another pigeon was also bitten by the same viper, which expired also, though not so soon as the first. The man immediately complained of an acute pain in the wounded part; and it soon looked red, then became of a livid colour: his finger swelled to a great size, and he could not bend it. Soon after this his hand also began to swell: he complained of faintness, and pains flying to his arm, shoulder, and arm-pits. In half an hour's time from the bite, his specific being applied, and strongly rubbed into the part affected, procured him immediate ease. His pain lessened, his finger became flexible, his spirits seemed more chearful: the specific being thus several times repeated and applied, his pains gradually diminished. The next day his finger and hand remained swoln, but without pain: the skin began to appear yellow, and pustules like bladders appeared on his finger; which being pricked, emitted a sanious liquor. In 2 days time all his symptoms vanished, and he became perfectly well.

June 30, the gentlemen of the faculty met again, and tried several experiments on puppies, cats, and pigeons; when they proved the efficacy of this man's specific, to the great satisfaction of the company.

A Proposal for the Measurement of the Earth in Russia. By Mr. Jos. Nic. de L'Isle, first Professor of Astronomy, at Petersburg, and F.R.S. N° 445, p. 27.

Necessity, or the exigencies of geography and navigation, put mankind very early on measuring the earth. On the first determination of the magnitude of the earth in geographical measures, as in Stadia and Arabian miles, the ancients did not employ any great degree of exactness. They were content to set down the circumference of the earth, and of its parts, in round numbers; probably because they did not expect to be able to attain much preciseness in a research of this nature. But as their desires of improving geography increased, they

* Page 84, of this vol. of these Abridgments.

found it necessary to have a more exact knowledge of the magnitude of each degree, not only in great measures, as in miles and leagues, but also in perches, toises and feet.

As to Russia, the geographical measures of which are wersts, divided each into 500 sagenes, and each sagene supposed to be exactly 7 feet English; this proportion once known, and the exact ratio of the English to the French foot, or to the toise of 6 feet, which the French astronomers employed in their measurements, and of which they found a degree of a great circle contained 57060; if it be asked what more is requisite for concluding that a degree of a great circle contains 104½ wersts? and what remains towards the perfection of the geography of Russia, in the most minute detail, but to employ this measure of wersts, sagenes, and English feet, in actual measurements; and to construct the charts by the most exact methods of geometry; it may be answered, we should be very happy, if in the geography of Russia we were arrived at this pitch; not only in the general map, but in that of any particular district, the nearest and of most concern to us. But besides that we are as yet far from pretending to this; it may be made to appear, that it is not possible to attain it, without undertaking an equal, and even a greater work than all that has been hitherto done in France and elsewhere, towards the measurement of the earth.

For if the earth be not truly spherical, all the degrees of the great circles will not be equal to each other; and those of the small circles, taken at a certain distance from their parallel great circles, will not have the same relation that the degrees of the small circles, taken at the same distance, would have on a sphere. In all this there might possibly arise an infinite variety, according to the figure the earth might have; and as it is not yet decided what is the earth's true figure, and that there is no better method of ascertaining it than by observations made in so great an extent as that of Russia; therefore the perfection of the geography of Russia stands in need of this great undertaking; which, besides its usefulness, will yield much honour, by contributing towards the deciding the celebrated question of the earth's figure.

There have been some who have long since suspected, and even thought they were furnished with proofs, that the earth is not exactly spherical. But supposing the earth to be bounded by a curve surface, such as it would be by the level of the sea carried quite over all the earth; it is in this manner, the earth being considered as covered with a fluid, that Sir Isaac Newton, in the first edition of his Principia, has demonstrated, that supposing this fluid homogeneous, and the earth to have been at rest at the time of its creation, it must have assumed the figure of a perfect sphere: but afterwards, supposing it to have a motion on its axis, as is well known it has in 24 hours; this spherical figure must have been changed into that of a spheroid, flatted at its poles, in which the degrees on the meridian must be greater towards the poles, than near the equator.

Sir Isaac confirms this hypothesis of the earth's figure, by observations of the diminution of the simple pendulum on approaching the equator: to which Dr. Pound adds the analogy the earth has with some of the other planets, as Jupiter, which sometimes appears oval, its least axis being that about which it makes its revolution.

This opinion of Sir Isaac has likewise been maintained by Mr. Huygens, though with some small difference. But in 1691, Mr. Eisenschmid having compared the measurements of the earth made in different latitudes, as that of Father Riccioli in Italy, of Mr. Picart in France, and of Snell in Holland; and having found that the degree, which resulted from those different measurements, continued to decrease in approaching the poles, which is quite the contrary of what follows from the earth's figure supposed by Sir Isaac and Huygens, Mr. Eisenschmid was therefore of opinion, that the earth was longer at the poles.

This opinion of Mr. Eisenschmid was afterwards confirmed by the late Mons. Cassini, in the observations of the meridian of Paris. For in 1701, having carried on these operations to the Pyrenæan mountains, which is a space of above $7\frac{1}{4}$ degrees, he found that as he advanced to the south, these degrees increased $\frac{1}{800}$ part, or 72 toises each degree.

Since the meridian of Paris was, in 1718, carried on northward to the sea, M. Cassini, the son, found on comparing more than 8 degrees, which this meridian contains from sea to sea, that the increase, going northward, was but from 60 to 61 toises each degree; as may be seen in the large treatise published in a separate volume, as a sequel to the memoirs of the Royal Academy of Sciences of Paris for the year 1718. These reasons did not hinder Sir Isaac from persisting in his first opinion of the figure of the earth flatted at the poles, as appears in the 2d and 3d editions of his Principia, published in 1713 and 1726: and it is very surprising, that by this very figure of the earth he demonstrates a certain motion it has, to explain in the Copernican system the precession of the equinoxes, or the apparent motion of the fixed stars in longitude. Sir Isaac finds the inequality of the degrees on the meridian, in so little an extent as that of France, not sensible enough to be possibly determined by immediate observations; and he is of opinion, that we ought more to rely on the observations of the simple pendulum, and on the other principles which he has built upon, to conclude the earth flatted at the poles.

In 1720, M. Mairan attempted to reconcile the two different hypotheses of Sir Isaac and M. Cassini, by imagining that the earth, at its creation, being without motion, was of a much more oblong figure than that which Cassini thinks it has at present; so that it might have been reduced to that which it now has, by the diurnal motion on its axis, &c. But Dr. Desaguliers, who is of Sir Isaac's opinion, has made appear, in the Philos. Trans. N° 388, that M. Mairan's supposition is contrary to the laws of motion; and has moreover proposed several considerable doubts on the observations and suppositions employed by M. Cassini, in his determination of the earth's figure in 1718.

As soon as the meridian of Paris had been extended from one sea to the other, and M. Cassini had thence deduced a confirmation of the system of the earth's being longer at the poles; M. De Lisle imagined a new method of deciding the question, viz. by the observation of the degrees of the parallel, compared with those of the meridian. For that purpose he considered, that as the degrees of the meridian and those of the parallel, at the same elevation of the pole, had different relations, according to the different figures ascribed to the earth; nothing more was requisite for concluding which hypothesis was the true one, than to determine this relation by immediate observation.

Having supposed, that there had been observed on the parallel of Paris, a space nearly of the same magnitude with that on the meridian, that is, of about 13 degrees, since that on the meridian is about $8\frac{1}{2}$ degrees; he found by an exact calculation, that according to the figure which M. Cassini has given to the earth, this space ought to contain $13\frac{1}{2}$ of the parallel, more than in the hypothesis of the earth's being spherical; which appeared considerable enough to decide between these two hypotheses, and by a stronger reason between the hypotheses of Newton and Cassini, seeing the difference ought to be still more considerable than that now specified.

He concluded, at least, that, independent of the figure of the whole earth, which could not be determined by the sole observations made in France, without making suppositions, and admitting principles, which are still liable to be contested; it would be of great consequence towards constructing exact charts of the kingdom, to ascertain this relation by observations, which consisted only in forming triangles along the parallel of Paris, and observing at the two ends the difference of the meridians, by the most exact methods. This difference seemed to be so considerable, that he was in hopes of being able to determine it by means only of two places within sight of each other, and situated to the east and west; provided their difference of longitude were accurately observed, independently of astronomical observations, by means of lighted fires; after the manner that M. Picart put in practice in Denmark, for determining the difference of longitude of the astronomical tower at Copenhagen and of Uraniburg in the Isle of Huen. With this intent, in the month of April 1720, M. De Lisle went some distance from Paris southward, to the places which he judged most proper for the purpose; but his design was not then executed, for want of assistance, and for other reasons, which he passes in silence. Since that time, he found that the Marquis Poleni had hit upon the same thought; as may be seen in his letter to the Abbot Grandi, dated in November 1724.

The decision of this famous question, of the figure of the earth, had stopped there, when in the year 1733, the minister of France having thought it necessary to construct an exact map of the whole kingdom; and being informed, that the work could not be better carried on, than by the astronomers of the Royal Academy of Sciences, applied to M. Cassini on that head; who was of opinion, that, to execute it with the utmost exactitude, the same method ought to be employed as for the meridian, by taking through the whole extent of the kingdom, triangles connected by means of objects seen successively, one from another, &c. This project of making a map of France by such triangles, had been already offered to M. Colbert, by Mons. Picart, in 1681, but was not then executed. However, M. Cassini proposed, that these triangles should be begun in a direction perpendicular to the meridian; in order to render these operations of service towards the decision of the earth's figure, pursuant to the method spoken of above: and M. Cassini, having in person undertaken these operations, and having carried them that same year, 1733, from Paris to St. Malo, whose longitude from Paris M. Picart had observed in 1681; the relations of the degrees on the meridian and parallel, were found to be such as were required in the hypothesis of the earth lengthened at the poles, and even more lengthened than Cassini had determined in 1718. For instead of the diminution of a 60th part for each degree of the parallel, which M. De Lisle had found according to the earth's figure, as determined by Cassini in 1718, he deduced from his operations in 1733, a diminution of the 36th part of each degree.

It is true, that M. Cassini, in the account he gave of this determination at the public meeting of November 14, 1733, does not give it as entirely certain; because the longitude of St. Malo, with regard to Paris, was collected from one observation only of Jupiter's first satellite, in which there may possibly be some error: but at least M. Cassini seems certain, that there is a very considerable diminution in the degrees of the parallel of Paris, which confirms his opinion of the earth's being longest at the poles. This we are likely to have a better assurance of hereafter, as we are informed that this measurement of the parallel of Paris, is carrying on in France by M. Cassini's sons, M. Maraldi's nephew, and several other young mathematicians, instructed by M. Cassini in this sort of work.

M. De Lisle has already said, that all these operations performed in France, for the figure and magnitude of the earth, could not serve to determine the earth's figure out of France, without the assistance of certain hypotheses; unless the same thing were undertaken and carried on in the other regions of the earth, more northern and southern than France. It was on this consideration, that the Royal Academy of Sciences took up the resolution of sending some astronomers to make the like observations as near the equator and the poles as possible, which are the places where the difference of the degrees on the meridian ought to be the greatest, according to the different hypotheses.

In the month of April 1735, three mathematicians and astronomers of the academy, viz. Messieurs Godin, Bougher, and De la Condamine, set out from France for the province of Quito, the most northern part of Peru in America; to observe, just under the equinoctial line, the magnitude of some degrees of the meridian and equator.

As to the other mathematicians and astronomers of the same academy, viz. Messieurs de Maupertuis, Camus, Clairaut, and Monnier, who were sent to the north, they departed from France in April 1736, with Mr. Celsius, professor of astronomy at Upsal, who accompanied them to Sweden, as far as the bottom of the gulph of Bothnia, where they might measure about a degree on the meridian at its crossing the polar circle. But as they had not finished their operations, it is not yet known whether the magnitude of the degree measured by them, favours the opinion of M. Cassini, or that of Sir Isaac Newton. All we know is, that they have found the length of the simple pendulum favourable to the latter, that is, longer under the polar circle than farther south. De la Croyere had already found the same thing: for being at Archangel in 1728, he there observed, in the most exact manner he possibly could, the length of the simple pendulum, which he found to be $\frac{3}{20}$ parts of a line longer than at Paris.

We are likewise informed by the other astronomers gone to Peru, that in their way towards the equator, being at St. Domingo, in the latitude of 18° 37′, they there found the pendulum swinging seconds, to be about 2 lines shorter than at Paris. Thus, all we as yet know from those gentlemen, on the expeditions to the north and the line, confirms the opinion of Sir Isaac Newton and his adherents: and yet M. Mairan pretends, that this shortening of the pendulum towards the equator, is in one sense entirely independent of the earth's figure.

Thus it appears from the foregoing account, that the question concerning the earth's figure is not yet at an end. Nay, it is not impossible, that after finishing all the observations which are actually making, new difficulties may arise, and new objections be started, that may prevent its being entirely decided. However, all this work cannot fail giving great light to this important question, and procuring considerable advantages to geography, astronomy, and natural philosophy.

It is with this view, and particularly to render such important service to the geography of Russia, that M. De Lisle thinks it necessary to undertake a work of that nature in Russia; towards executing which they have great advantages, which other nations have not. One of the principal of these advantages, is the great extent of Russia every way. For were the meridian of the imperial observatory of Petersburg to be determined, it might be carried to between 22 and 23 degrees; which is a fourth part of the distance from the pole to the equator. The meridians of Mosco and Astracan are not of less extent; and consequently we might, by the measurement of some one of these meridians, determine more exactly, than could have hitherto been done, the inequality that subsists between the degrees of the meridian.

This is what the great Cassini wished, when, after having, in the year 1701, determined this inequality by the extent of 7 degrees observed in France, as has been mentioned above, he says, that this fact might be verified by measurations of greater extent, if the other princes of the earth would contribute as much as the King of France, towards the perfecting of sciences.

In the great extent which might be given to the meridian of Petersburg, there would be the advantage of knowing, by operations connected together, or uninterrupted, the magnitude of some degrees equal to those which have been measured in France, and to that which the French astronomers have measured in Sweden; and not only all the degrees between the two, which the French astronomers have not had in their power to observe, but also some degrees farther northward than that measured by them in Sweden.

As the exigencies of geography require the triangles, taken for the determination of the meridian, to be continued on every side, and principally in directions perpendicular to the meridian, or according to the parallels; with how great exactness may we not then determine the proportion of the degrees on the parallels to those on the meridian, by means of the vast extent of the Russian empire, which on its western side extending as far as all the dominions of Europe from the most northern to the most southern, has no other bounds to the east than the east itself, so to speak; seeing its extent that way contains near half the earth?

Another great advantage to be obtained by the work now proposed to be made in Russia, is, that, coming after others, we shall reap the benefit of all their knowledge and experience in the like kind of measurements: whence we may expect to succeed and execute it better than could have been done elsewhere, by applying timely remedies against the difficulties that occurred in other places.

These operations are to be founded on a basis of the greatest length possible; which must be actually measured, and with the greatest exactness; as it is to serve for a foundation to the measurement of all the triangles. And in this point too there is a very great convenience near Petersburg, seeing on the ice, we may measure out a basis, greater than has been hitherto taken, namely, from the coast of Ingria about Peterhoff, to the coast of Finland toward Systerbeck. There is not less than 20 wersts distance between these two extremities, and this great distance may be measured very exactly, the ice being very even. And as this basis is situated between the Isle of Cronstad and Petersburg, in a direction nearly perpendicular to the distance from Petersburg to Cronstad; there can be no better method for inferring thence, by exact observation of the angles taken at the extremities of this basis, the distance from the centre of the imperial observatory, to the steeple of the new church at Cronstad; which two objects are seen reciprocally from each other, and are not less than 30 wersts asunder: and this distance, once known exactly, will serve as a foundation for all the triangles to be taken; of which each of the sides may have not less than from 30 to 40 wersts, according as objects are found advantageously situated for that purpose. It should begin with the mountain of Douderhoff, which, with the imperial observatory, and the steeple of Cronstad church, forms one of the most convenient triangles imaginable for the subject proposed.

In taking observations at these three places, it must then be tried to discover others of the same advantageous situation; but when no remarkable objects are found of the desired situation and distance, they must be erected on purpose, in the same manner as in other countries. The most necessary instruments for executing this undertaking, are, besides the ordinary astronomical instruments, a common quadrant of between 2 and 3 feet radius, for observing the angles of the triangles; and a portion of a circle of the greatest radius that can be conveniently had, for observing the arches of the heavens corresponding with the distances measured on the earth. As to the instrument for observing the arches of the heavens, its radius ought not to be less than from 12 to 15 feet: but it is not necessary that it should contain a large portion of a circle. It is only requisite to have this portion somewhat larger than the arch of the heavens intended to be measured. Thus, as the meridians, which may be traced in Russia, can be extended only between 22 and 23 degrees, it will suffice that the instrument employed, be a portion of a circle of 30 degrees

M. Picart, for his first operation, got an arch of a circle made of 18 degrees and of 10 feet radius, with which he thought himself sure within 2 or 3 seconds: and no other instrument was used in the chief observations for the meridian of Paris. The astronomers who are gone to America, carried with them an instrument of 12 feet radius, and of a portion of a circle of 30 degrees. But those come to Sweden, contented themselves with a portion of a circle of $5\frac{1}{2}$ degrees, and 9 feet radius: but this instrument, made by Mr. George Graham, a very able English mechanician, is by its construction so exact, that the astronomers who have used it, think themselves certain to 2 seconds. The one wanted for the observations in Russia, ought to be made by the same artist, and of the same construction.

It is with such an instrument that Mr. Bradley, a celebrated English astronomer, has discovered, in the meridian altitudes of some fixed stars, certain constant and annual variations, which do not proceed either from the variation of the refractions, or from the parallax of these stars, or from any nutation or wavering of the earth's axis; but which he accounts for by the successive motion of light. Whatever be the cause of these variations, as they may possibly happen in the space of time requisite to be spent in making the observations for the meridian, or in passing from one end of the meridian to the other; it is necessary, with the same instrument, or such another, that is of pretty near the same exactness, to examine the variations of the stars made use of: it would therefore be of considerable advantage, not only for the observations of the measurement of the earth, but also for all the other principal researches in astronomy, to have orders given for procuring two mural quadrants of Mr. Graham's make, and of the same construction, as already specified; for which there are walls already raised at the imperial observatory, in the plane of the meridian. With these two quadrants, which might be of 7 feet radius, and the moveable telescope 9 or 10 feet long, we should be in a condition to make observations of the utmost accuracy, such as the present state of astronomy requires.

Besides these instruments now mentioned, which are of absolute necessity to a solid establishment of astronomy and geography in this country, there are still some other smaller instruments, that may be of great use in the operations, or may serve to make other curious and useful observations at the same time, that those for the measurement of the earth are making.

When the sides of the triangles, taken for measuring the earth, terminate at very elevated places, as on the tops of the highest mountains, it is necessary to reduce these triangles to what they would be, had they been observed in horizontal planes on a level with the sea. For this purpose, we must know the height of the mountains above the sea's level, which cannot always be determined geometrically, or would at least be too tedious to perform: therefore, in the meridian of Paris, which crossed very high mountains, M. Cassini was of opinion, that he ought to fix their height by a shorter method, which is that of the height of the simple barometer, observed on the top of each mountain, and compared with that observed at the same time in another place, whose elevation above the sea's level was known. But as that method supposes the knowledge of the proportion which the different fallings of the mercury keep with the different heights to which the barometer is carried; and as natural philosophers are not as yet entirely agreed on this head, for want of observations of sufficient accuracy; thence it happened, that Dr. Desaguliers, making it appear that M. Cassini has not employed the most exact proportion, found reasons for correcting, or at least for doubting, of some of M. Cassini's calculations. Thus it must be by the assistance of new experiments, better circumstanced than those hitherto made, and pursuant to a theory entirely agreeing with these experiments, that this method may be employed with certainty, for determining the height of mountains by the barometer, and reducing the angles observed from the tops of these high places, to what they would be, if they had been observed on a plane at the level of the sea. Now these new observations can be made on our way in tracing the meridian; and for that purpose M. De Lisle began to construct compound barometers, which being very nice, will serve to observe with accuracy, the quantity of the mercury's fall, at the different elevations to which they shall be carried, to fix with greater certainty the proportion of that fall.

There is still another method of determining the elevation above the level of the sea of all the points, in which the triangles terminate, that are made for the measurement of the earth. This may be done by beginning these operations near the sea, and actually measuring how many toises and feet the places of the first stations are elevated above the level of the sea. For if the angles of the apparent elevations of the second stations, seen from the first, be afterwards observed, it will be an easy matter, from the known distances, to deduce the true elevations of the latter above the former, and consequently above the sea's level, making proper allowances in the calculations for the difference of the apparent level from the true one. In this method, nothing is to be apprehended but the variation of refractions; but for this a remedy may be found, for the most part, by returning again, that is, by reciprocally observing the first stations seen from the second: for if it be found, that as much as the second station appears elevated above the first, so much the first is depressed below the second, except the small difference which must arise according to the given distance, it will be a proof, that the refraction has been of no prejudice.

The other considerable observations and experiments, to be made in the journies undertaken for such inquiries, are, the observations of the magnetic needle, both as to its dip and variation: but chiefly the observations of the length of the simple pendulum, which at present is become requisite to be observed with as much exactness, and in as many places, as is possible; but also for which there are new methods invented, which probably surpass those hitherto made use of; in as much as, since those methods have been found by the the Royal Academy of Sciences of Paris, it was thought proper to notify them to the astronomers sent to Peru, to put them in practice in their observations.

The actual Mensuration of the Basis proposed in the preceding Article. By M. De Lisle. N° 445, p. 50.

M. De Lisle undertook to measure the basis mentioned above, and had the good fortune to measure it very exactly on the ice, by taking the precise distance between the castle of Peterhoff, and the castle of Doubki, opposite to it, on the coast of Finland. He found the distance between the opposite walls of these castles to be 74,250 feet English. This basis, being much greater than any of those employed hitherto for this purpose, gives room to expect great exactness in the whole work, when it shall be carried on in the same manner. It will at the same time serve to make a very exact map of the bottom of the gulph of Finland. It is for the same design, and for better ordering the charts of the coasts of the Baltic, that he intended (as soon as my project shall be approved here in its full extent) to begin to measure my triangles along the coasts of Ingria and Livonia, to the islands of Dagho, Oesel, &c. And that the charts of the places taken in by these triangles may be finished at the same time, he designed to take with him all the charts of these parts, which could be had to verify and correct them in his way.

He likewise intended to publish, as soon as possible, all the operations and observations made in the expedition; that thus early benefit may be reaped from them, and that the public, at the same time the charts come out, may be acquainted with the foundation on which they are constructed.

Observations of two Parhelia, or Mock-Suns, seen Dec. 30, 1735; *and of an Aurora Borealis, Dec.* 11, 1735. *By the Rev. Timothy Neve.* N° 445, p. 52.

On Tuesday, Dec. 30, 1735, between Cherry Orton and Alwalton, in the county of Huntingdon, Mr. Neve observed two parhelia, the first of which shone so bright, that at first sight he took it for the real sun, till looking a little farther on his left hand, he was convinced of the mistake, by seeing the true sun much the brightest in the middle, and a mock-sun on each side, in a line exactly parallel to the horizon. He guessed their distance to be about 40 diameters of the sun, or, as they usually appear, 23 degrees. That on the left hand of the sun, when he saw it first, was small and faint, but in about 2 minutes time it became as large and bright as the other, and appeared at once as two white lucid spots on each side the sun, east and west, seemingly as large, but not so well defined: in about 3 minutes they lost both their colour and form, and put on those of the rainbow; the red and yellow in both very beautiful and strong nearest to the sun, the other colours fainter. They became as two parts of an arch, or segment of a circle, with the concave towards the sun, only round at top, the light and colours streaming downwards, and tending towards a point below. This continued for about 4 or 5 minutes, when the colours gradually disappearing, they became as before, two lucid spots, without any distinction of colours. They lasted a full hour, sometimes one brighter, and sometimes the other, according to the variation of the clouds and air, as he supposed. When he first saw it, it was exactly a quarter after eleven. There had been a frost in the morning, which went away pretty soon, with a thick mist, and between 10 and 11 o'clock cleared up, leaving only a haziness in the air behind it: the weather quite calm, wind about N. W.

These parhelia are commonly seen with a circle or halo round the sun, concentrical to it, and passing through the disks of the spurious or mock-suns. But there was not the least appearance of such a circle here, it having only a tendency towards one, when it was seen with the rainbow colours.

The other phenomenon, was that pretty common one of the aurora borealis, of an uncommon appearance. A little after 5 o'clock, the northern hemisphere was obscured by a dusky red vapour, in which, by degrees, appeared several very small black clouds near the horizon. The first eruption of the lights was within a quarter of an hour, full east, from behind one of the small dark clouds, and soon after several others full north. These streams of light were of the same dusky red colour as the vapour, just appeared, and vanished instantly. He saw 8 or 10 of these at once, about the breadth of the rainbow, of different heights, several degrees above the horizon, which looked like so many red pillars in the air; and no sooner did they disappear, but others showed themselves in different places. In about half an hour, this colour of the vapour gradually changed towards the usual white, and spread much wider and higher; and after that, appeared as common.

Observations of two Parhelia, or Mock-Suns, seen at Wittemberg, in Saxony, on Dec. 31, 1735. *O. S. By John Frid. Weidler, F. R. S. &c.* N° 445, p. 54.

Dec. 31, 1735, O. S. a little after 10 in the morning, being informed that several suns were seen in the heavens, Mr. W. went into his garden, and immediately saw near the sun S, on its left or western side, the parhelion B, as large as the true sun, fig. 15, pl. 5. This mock-sun was amidst small, round, white clouds, set thick, and close to one another. The middle of this mock-sun shone with so great a light, that the naked eye could not bear it; so that he viewed it attentively through a glass darkened with the smoke of a wax-candle. The light of the parhelion B appeared much weaker than that of the true sun. Its circumference, facing the sun, was red: but that part of the stream FG, which was towards the sun, was purple. Within the red border appeared the other colours of the rainbow, as yellow, green and azure; the stream BH was likewise embellished with red and yellow. Both edges of this were reddish, and its middle yellowish. The sun S was 15°½ above the horizon; and its image B was near the same altitude: the distance from S to B was 20°: the arch FG was near 6° in length. Most of the southern part of the hemisphere was overspread with white clouds, interspersed with some darker ones. There were some thin clouds before the true sun, through which its rays easily passed. When thicker clouds surrounded the sun, the brightness of the parhelion was lessened: the parhelion was now and then hidden by dark clouds. Soon after first observing the parhelion B, was seen the beautiful rainbow CDE, parallel to the horizon, with its horns turned to the north. It had the usual colours of the rainbow, all very distinct. The purple was on the side facing the sun; next to it was the yellow, then the green, and last the azure. The point D was 61° distant from the horizon; therefore the diameter of the rainbow was 58°: however, only a part of the rainbow CDE was seen, the ends of which were sometimes but 38° from one another: for more or less of it appeared at different times, but scarcely above a 4th part of its circumference at any time. It lasted till the sun and most part of the sky was overcast by thick clouds. The thickness of the rainbow CK, as well as I could estimate by the bare eye, was 1° of a great circle.

From another place, whence he had a full view of the hemisphere, a little before 11, he saw another parhelion A to the east, 20° from the sun, as the former was, and raised 15° above the horizon. This mock-sun was not inferior to the other B, in brightness, for the naked eye could no more bear it than that: its light was white; its figure round, and its size equal to that of the sun S. This parhelion A shot out the stream IL, which was rectlinear, white and resplendent, 8° long, and void of colours; and it lasted somewhat longer than the former, without changing its figure. On the sun's being hid by thick clouds, about $\frac{1}{4}$ hour after eleven, both these mock-suns disappeared; but they became visible again, on the sun's shining bright.

The whole of the phænomena observed in these parhelia comes to this: that the true sun, S, was accompanied by two parhelia, both 20° distant from the sun, one on each side, and having nearly the same altitude with the sun from the horizon. Above the parhelia, part of a rainbow surrounded the zenith; and each of the parhelia sent forth a bright luminous stream or tail, one rectilinear and white, the other somewhat curved and coloured. Moreover, from the western parhelion, a stream parallel to the horizon, and somewhat pointed, extended itself on the side opposite to the sun; and this scene lasted the 2 hours of 10 and 11 before noon, till thick clouds put an end to it. There was no appearance of an entire crown, such as usually accompanies parhelia, and encircles the sun.

An Observation of three Mock-Suns seen in London, Sept. 17, 1736. *By Martin Folkes, Esq. V. Pr. R. S.* N° 445, p. 59.

Sept. 17, 1736, as Mr. Folkes was reading a little after 7 in the morning, in a room looking towards the north-east, he accidentally noticed an odd stream of coloured light, shooting upwards from the sun, shining through a thin waterish cloud; but recollecting the appearance was several degrees more northerly than the sun's true place at that time, he went to the window, and found what he had taken for the sun was a parhelion, shooting out a short horizontal stream or tail towards the north; the sun itself shining pretty bright and clear at the same time. He also observed, that the stream he had at first seen, was part of an arch concentric to the sun, and passing through the parhelion: this arch was for a good way tolerably defined, and tinged with red on the inside, and a bluish white on the other. Casting his eye to the other side the sun, he perceived a second parhelion, at the same distance from him, towards the south, though not yet so bright as the first. He then went up to the leads of his house, where he soon found the phenomenon considerably to improve, the arch round the sun forming itself into more than a semicircle, reaching almost to the horizon northward, and with very little discontinuance beyond the second parhelion towards the south. He then began to perceive a third parhelion, where the circle surrounding the sun would have been cut by the vertical passing through him; and in the same place his circle was touched by the arch of another, in some sort confounding itself with it in the place where the third parhelion appeared: this was a good deal fainter than the other two, and the last arch extended but a little way, so as to be difficult to determine where its centre lay; this arch was coloured also, but with red on its convex part. He had some time before this begun to see also another circle, surrounding the sun at the distance of about 45°, which appeared to be about twice the distance of the first; and this also increasing while he was considering it, became little less than a semicircle, being also tinged with red like the other on the inner side. When the circle had thus pretty well formed itself, he also discovered the arch of a 4th, touching this, or rather confounding itself with it, in its highest part, and surrounding, as it seemed, the zenith. Of this last circle he saw, when it was most complete, better than half, and it was much stronger coloured than any of the others, being of a bright red on its convex part, and a good blue on the concave. In the part where this circle confounded itself with the larger of those that were concentric to the sun, their common part was nearly white, and brighter than the rest, though hardly enough to call it a 4th parhelion. The principal mock-suns continued tolerably bright till near 8 o'clock, the southern part of the phenomenon improving as the northern decayed; and the southern parhelion was once so bright, that, taking the advantage of a place where a chimney shaded the true sun, it cast a very visible shadow: the white and luminous horizontal tail also, that went from this parhelion, was much longer than that of the other, reaching at one time beyond the outer of the two concentric circles. The parhelia themselves, though very luminous, were, however, never defined with any exactness as to their discs, but looked as we sometimes see the sun through a thin whitish cloud, and they were themselves of a reddish colour on that side next the true sun. About 8 the phenomenon was sensibly decreased, and had entirely disappeared by 20^m after. All these appearances are exhibited in fig. 16, pl. 5.

Of a Rupture of the Ileum from an external Contusion, in a Letter from Christian Wolf, Professor of Mathematics at Marpurg, &c. to Wm. Rutty, M. D. formerly Secretary to the R. S. Dated March 3, 1731. *An Abstract from the Latin.* N° 445, p. 61.

Professor Wolf here states that a labourer had received a fatal accident from a large stone falling upon the abdomen, in such manner as to occasion a contusion, but no laceration. The man died very unexpectedly the day after the accident. On opening the body, a large rent was discovered in the ileum, and its contents were found effused into the cavity of the abdomen.

Some new Statical Experiments. By J. T. Desaguliers, LL. D. F. R. S. N° 445, p. 62.

When a long and heavy body, lying on the ground, is to be raised up at one end, like a lever of the second kind, while the other end keeps its place, and becomes the centre of its motion; the prop used to support it at any point in its whole length, sustains a certain pressure from the beam. Now these experiments are to show, by a force drawing always in the direction of the prop, what is the quantity of the pressure on the prop, according to its length, and to the angle which it makes with the beam, or with the horizon, and to the distance from the centre of motion of the beam at which the prop is applied. For when the prop is taken away, the force drawing in the direction of the prop will keep the beam in equilibrio; and a force ever so little superior to the friction added to the power, will make it overpoise the beam, and raise it higher; but overcome the power and bring down the beam, if it be added or applied to the beam.

Though in every case and experiment we have this analogy, taken from mechanical principles, viz. that,—The intensity of the power: Is to that of the weight:: As the distance of the line of direction of the weight: Is to the distance of the line of direction of the power;—yet to find those distances nicely in the several applications of the prop, we must have recourse to geometrical constructions and reasonings. With these and the algebraical expressions of the same, the experiments exactly agree.

In the machine here used, the iron bar, or parallelipiped representing the heavy body, weighs 12 drams, 12 dwt. 12 grains, or 6060 grains; and its centre of gravity is at the distance of 20$\frac{1}{4}$ inches from its centre of motion. The props used are, the one of 5, and the other of 10 inches. To overcome the friction, allowed for by certain rules in all cases, the Dr. used a nice brass pulley, of 3 inches diameter, its pivots only $\frac{75}{1000}$ of an inch in diameter; so that the 60th part of the power added to it, will in all cases overcome the friction.

Case I. *In which the Prop is perpendicular to the Horizon, exemplified by two Experiments.*

Exper. 1.—The prop is 5 inches, and placed under a point in the bar, 10 inches from the centre of motion. Here the power acting in the direction of the prop, able to keep the bar in that situation, or the pressure sustained by the prop, will be found 250 oz. 17 dwt. 15 grains; and the friction 8 dwt. 15 grains. The foot of the prop is to be at 8 inches and $\frac{66}{100}$ from the centre of motion.

Exper. 2.—If the same prop, of 5 inches, be placed under a point in the bar at 30 inches from the centre of motion, the power or pressure will be 8 oz. 12 dwt. 13 gr. and the friction equal to 2 dwt. 21 gr. The foot of the prop is to be distant from the centre of motion 29 inches $\frac{58}{100}$.

CASE II. *In which the Prop is perpendicular to the Bar, exemplified by three experiments.*

Exper. 1.—Now let the prop, still 5 inches long, be placed so as to be perpendicular to the bar at a point 12 inches from the centre of motion. Here the power expressive of the pressure should be 19 oz. 18 dwt. 4 gr. and the friction 6 dwt. 15 gr.; but on account of a correction necessary to be made to this, because the bar is thick as well as heavy, and the centre of gravity above the surface to which the prop is applied, the power or pressure sustained will be only 19 oz. 15 dwt. 5 gr. and the friction 6 dwt. 14 gr. The distance of the foot of the prop in this case is 13 inches from the centre.

Exper. 2.—The prop here is 10 inches long, still perpendicular to the bar, under a point in the bar, 24 inches from the centre. The power equal to the pressure sustained, should be, if the bar was only heavy, and not thick, 9 oz. 19 dwt. 4 gr.; the friction 3 dwt. $11\frac{1}{2}$ gr.; but with the proper correction, explained hereafter, it must be only 9 oz. 17 dwt. 15 gr.; the friction 7 dwt. 7 gr. Here the foot of the prop is to be 26 inches from the centre.

Exper. 3.—If the end of the prop be placed under a point in the bar, so that the horizontal distance of the foot of the prop be exactly equal to the distance of the centre of gravity from the said centre of motion, viz. 20.5 inches, the power or pressure sustained by the prop will be precisely equal to the weight of the bar, viz. 12 oz. 12 dwt. 12 gr. In this case, the prop is distant from the centre of motion on the bar 17.9 inches, and the friction 4 dwt. 5 gr.

CASE III. *In which the angle made by the prop with the horizontal line is given, either acute or obtuse.*

As this case is very intricate, on account of the several powers of the sine and cosine of the given angle, which are multiplied into the prop, and into the weight of the beam, we will exemplify it only in one experiment; which is, when the angle made by the prop, with the horizontal line contained between the foot of the prop and the centre, is acute: then there is a maximum of pressure, which it appears by experiment is the very same as the calculation gives. Suppose the angle made by the prop and the horizontal line to be 60 degrees; the calculation of this maximum shows, that if the prop be 10 inches long, the distance measured on the bar, to which the upper end of the prop must be applied, will be 10 inches $\frac{96}{100}$, the bar itself making then an angle of about 52° 12′; and the horizontal distance between the centre of motion and the foot of the prop, is then 11 inches $\frac{72}{100}$.

N. B. Three things are to be remarked in this case:

First, That when the angle made by the prop and the horizontal line, contained between the centre of motion and the foot of the prop, is acute, as in the last experiment, there is always a maximum: whereas when the same angle is obtuse, there is no positive maximum; for then the pressure continually increases, the nearer the prop is to the centre of motion.

Secondly, That when the angle of the prop with the horizon is acute, as in the last experiment, the bar, or long and heavy body, can be raised by applying the power or prop always with the same angle to the horizon, quite up to a vertical situation.

Thirdly, That the first case, which is when the prop is perpendicular to the horizon, is only a particular case of this more general one.

CASE IV. *Is when the angle made by the prop with that part of the beam contained between the point to which it is applied, and the centre of motion, is given, either acute or obtuse.*

As the expression of the power in this case, is fully as intricate as in the last, the Doctor gives only one example or experiment; and, for the greater satisfaction of those that see it, he chose that in which the pressure is in its maximum. He supposes, as before, the angle made by the prop, still 10 inches long, with that part of the beam contained between the point to which it is applied, and the centre of motion, to be acute, and of 60°; then the maximum of pressure will be, when the part of the beam intercepted between the centre of motion and the upper end of the prop is 12 inches $\frac{81}{100}$; the bar is then elevated about 50° 13′, and the horizontal distance between the centre of motion and the foot of the prop, is then 11 inches $\frac{27}{100}$.

N. B. Observe also in this case, as in the last,

First, If the angle made by the prop, and the part of the beam intercepted between the point of application and the centre of motion, is acute, there will always be a maximum. The contrary will happen, if that angle is obtuse.

Secondly, If the angle is acute, the bar cannot be raised up to a vertical situation by applying the power or prop constantly with the same acute angle; but it may be raised quite up, if the angle of the prop with the beam is obtuse.

Thirdly, The second case is but a particular case of this general one.

The Apparent Times of the Immersions and Emersions of Jupiter's Satellites, for the Year 1739, *computed to the Meridian of the Royal Observatory at Greenwich. By James Hodgson, F. R. S.* N° 445, p. 69.

These calculations are omitted for the same reasons as before.

An Account of the Peruvian or Jesuit's Bark. By Mr. John Gray, F. R. S. at Carthagena, from some Papers given him by Mr. William Arrot, a Scotch Surgeon, who had gathered it at the Place where it grows in Peru.* N° 446, p. 81.

The tree from which the Jesuit's bark is cut, grows in the kingdom of Peru, in the Spanish West Indies, and is found most commonly in the provinces of Loxa, Ayavaca, and Quenca, situated between 2 and 5 degrees of south latitude. This tree is tall, and has a trunk rather thicker than a man's thigh, tapering from the root upwards; it has no boughs or branches, till near its top; where they grow as regular as if lopped artificially, and with the leaves form exactly the figure of a hemisphere: its bark is of a blackish colour on the outside, and sometimes mixed with white spots; whence commonly grows a kind of moss, called by the Spaniards, barbas; its leaves resemble much the leaves of our plum-tree, are of a darkish green colour on their upper or concave side, and on their lower or convex side, reddish; its wood is as hard as common English ash, and rather tough than brittle.

There are 4 sorts of the bark of this tree, to which the Spaniards give the following names, viz. cascarilla colorada, or reddish bark; amarylla, yellowish; crespilla, curling; and blanca, whitish; but Mr. Arrot could only find 2 different sorts of the tree, and he believes that the other 2 sorts of the bark are owing to the different climates where it grows, and not to a different species of the tree. The bark called colorada and amarylla, is the best, and it differs from the blanca in this, that the trunk of the former is not near so thick as that of the latter; the leaves as described above; whereas those of the blanca are larger, and of a lighter green colour, and its bark has a very thick spongy substance, whitish on the outside, and is so tough, that it requires the force of an axe to slice it from the tree. It is as bitter when cut down as the best sort, and has then the same effect in intermitting fevers; but when dry and long kept it turns quite insipid, and is good for nothing. Both sorts have a much surer and quicker effect in cures when green than when dry. As the bad sort is in great plenty, and the best very scarce, and hard to be come at, large quantities of it are cut yearly, and sent with a little of the fine bark to Panama for Europe.

The tree of the crespilla is the same with that of the amarylla and colorada, but it grows in a cold frosty climate; by which means the bark is not only altered in its quality, but is also whitish on the outside, though cinnamon-coloured

* Cinchona officinalis. Linn.

within, and ought in medicines to be rejected. This sort and the blanca grow plentifully in the province of Ayavaca, 50 leagues from Piura, and 62 from Payta, a port in the South Sea; as also in Cariamango, Gonsonama, and Ximburo, whence they commonly send it to Payta, and there sell it as the best. The blanca likewise grows in the province of Quenca, and in the mountains of Caxamarca: but the true and genuine fine Jesuit's bark, which is of a reddish or yellowish colour, is only found from 5 to about 14 leagues round the city of Loxa, in the province of Loxa, called generally by the Spaniards, Provincia de las Calvas. This city is situated between 2 rivers, that run into the great river Marannon, or of the Amazons, and lies about 100 leagues from Payta, and in a direct line about 110 leagues south-east from Guayaquil, though by the common road near 200. The places about Loxa, where this fine sort is found, are La Sierra de Caxanuma, Malacatos, Yrutasinga, Yangana, Mansanamace, La Sierra de Boqueron, and a place called Las Monsas.

The bark trees do not grow all together in one spot, but intermixed here and there with many others, in the woods; it happens indeed sometimes, that clusters of them are found together, though at present they are much scarcer than in former times, a great many of the fine large bark trees having been entirely cut down, that their bark might the more easily be sliced off.

The soil where the best sort thrives, is generally in red clayey or rocky ground, and very frequently on the banks of small rivers descending from high mountains.

That this tree flourishes and bears fruit at the same time all the year round, is certainly owing to the almost uninterrupted rains, that fall in those high mountains where it grows, which continue with little or no intermission: though about 3 or 4 leagues down in the low country, where it is excessively hot, there are wet and dry seasons, as in other hot countries, the rains beginning in December and ending in May; this season the Spaniards who live there call temporal, and it is general all thereabouts; whereas what they call paroma is a cold rainy season, that lasts in all the mountainous places of these countries from June to November, but especially in the city of Loxa and places adjoining, where Mr. Arrot has passed 25 or 30 days without once seeing the sun, and felt the air so extremely cold, that he was obliged always to be wrapped up in his cloak, and to be in continual motion to keep himself warm. Such excessive cold so near the line, appears to Europeans incredible; but many places in these latitudes are so, by their situation and vicinity to high mountains.

The properest season for cutting the bark is from September to November, the only time in the whole year of some intermission from rain in the mountains. Having discovered a spot where the trees most abound, they first build huts for the workmen, and then a large hut to receive the bark, to preserve it from the wet: but they let it lie there as short a time as possible, having beforehand cut a road from the place where the trees grow, through the woods, sometimes 3 or 4 leagues, to the nearest plantation or farm-house in the low country, whither, if the rain permits, they presently carry the bark to dry. These preparations made, they provide each Indian, these being the cutters, with a large knife, and a bag that can hold about 50 lb. of green bark; every two Indians take one tree, whence they cut or slice down the bark, as far as they can reach from the ground; they then take sticks about half a yard long each, which they tie to the tree with tough withs, at proper distances, like the steps of a ladder; always slicing off the bark, as far as they can reach, before they fix a new step; and thus they mount to the top, the Indian below gathering what the other cuts; this they do by turns, and go from tree to tree, till their bag is full; which, when they have plenty of trees, is generally a day's work for one Indian. As much care as possible must be taken that the bark is not cut wet; should it so happen, it is to be carried directly down to the low country to dry, otherwise it loses its colour, turns black, and rots; and if it lie any time in the hut without being spread, it runs the same risk: so that while the Indians are cutting, the mules, if the weather permits, ought to be carrying it down to the place appointed for drying it, which is done by spreading it in the open air, and frequently turning it.

Mr. Arrot had the curiosity to send above 50 seroons from the woods to the city of Loxa, where he put it into a large open house, and dried it under cover, never exposing it either to the sun or night air, imagining that the sun exhaled a great many of its fine parts, and that the night air, or serene, was very noxious to it; but he found the colour of the bark thus cured, not near so bright and lively as that dried in the open air. He is of opinion, that a very short time will put an end to this best sort, or at least it will be extremely hard to be got, by reason of its distance from any inhabited place, the impenetrability of the woods where it grows, and the scarcity of the Indians to cut it, who, by the Spaniards' hard usage and cruelty, are daily diminishing so fast, that in a very few years their race in that country will be quite extinct.

Mr. Arrot says, that the small bark which curls up like sticks of cinnamon, and which in England is much esteemed, as being cut off the branches, and therefore reckoned better and more effectual in curing fevers, is only the bark of the younger trees, which, as it is very thin, curls in that manner; and that the bark of the branches would not compensate the trouble and expences of cutting. He also says, that after the bark is cut off any tree, it requires at least 18 or 20 years to grow again; which is directly contrary to what Dr. Oliver says in N° 290 of the Phil. Trans. He added besides, that its fruit is no ways like a chestnut, as the Doctor informs us in the same paper; but rather like a pod, which incloses a seed somewhat like a hop-seed, and that he had sent some of them to England.

He could not tell by what artifice or stratagem the Jesuits have got this bark to be called after them, if not that they carried it first into Europe, and gave themselves out as the first discoverers of its virtues: but he asserted, that the current opinion at Loxa is, that its qualities and use were known by the Indians before any Spaniards came among them; and that it was by them applied in the cure of intermitting fevers, which are frequent over all that wet unhealthy country.

An Account, by Mr. John Eames, F. R. S. of a Book entitled, A Mathematical Treatise, containing a System of Conic-Sections, with the Doctrine of Fluxions and Fluents, applied to various Subjects. By John Muller. N° 446, p. 87.

The ingenious author of this work,* observing how much time is necessarily spent, and pains taken, in learning these valuable parts of mathematics, thought it would be very well worth his while to lessen both, which he hopes he has done considerably, in the following treatise. He has divided it into 3 parts, contained in so many books.

In the first of these, he considers the properties of the 3 sections of a cone, as well in, as out of the cone. And to make this part of the work of more service to the reader, Mr. Muller has not only selected the most considerable properties of these curves, that are to be met with in other writers, both ancient and modern; but has added several new ones, which, as he informs us, are inserted in their proper places. And that such gentlemen as are desirous to read Sir Isaac Newton's Principia, but are at a loss for want of a sufficient acquaintance with conic-sections, may be the more obliged, he has taken particular care to demonstrate such properties as Sir Isaac presupposes his reader to be acquainted withal. Accordingly, he has prefixed a table of such propositions, informing him as well where they are to be met with in this book, as in Sir Isaac Newton's Principia Mathematica.

The proofs made use of in his demonstrations, are sometimes algebraical, at

* He was afterwards, for many years, professor of Fortification and Artillery, in the Royal Military Academy, Woolwich, for the use of which, he published several books on those subjects, which are still in great repute, being the best works extant of the kind.

other times geometrical, according as he finds the one to be plainer and shorter than the other.

The second book treats of the direct method of fluxions. And here he hopes the first principles of this method are laid down, not only in a new, but very plain and concise manner. He proceeds to show the use of fluxions in the solution of the common problems of finding the maxima and minima of quantities, the radii of the evolution of curves, and the radii of refraction and reflection. Under the first of these heads he says, particular care has been taken to distinguish the maximums from the minimums, a thing which has not been noticed so much as it ought to have been. And whereas some mathematicians, having made use of what they call infinitely small quantities, are forced to reject something out of the equation, for finding the fluxion of a rectangle, whose sides are varying quantities, Mr. Muller uses only finite quantities; and finds the fluxion of such a rectangle after a new manner, without rejecting any quantity for its smallness. He does the same in finding the fluxion of a power. And to avoid the use of infinitely small quantities, introduces a new principle, viz. that a curve line may be considered as generated by the motion of a point carried along by two forces or motions, one in a direction always parallel to the absciss, and the other in a direction always parallel to the ordinate. Hence he infers, that the fluxion of the ordinates is to the fluxion of the absciss, as the ordinate is to the subtangent of the curve.

Having likewise proved from the first supposition, that if the describing point, when arrived at any place given, should continue to move onwards, with the velocity it has there, it would proceed in a right line, which would touch the curve in that point; he concludes that the direction of the force in that place, is in the tangent to the curve: consequently, the 3 directions being known in each place, the proportion between the velocities of the urging forces will be likewise known. So that the nature of the curve being given, the law observed by these velocities may be found; and if the law of the velocities be given, the nature of the curve may likewise be given.

In the third and last book, we have the inverse method of fluxions, with its application to the several problems solvable by it; such as the superficial and solid contents of curvilineal figures, the rectification of curve lines, centres of gravity, oscillation and percussion. Here also Mr. Cotes's tables of fluents are explained and illustrated by examples.

He finishes this book with a great variety of problems, of a physico-mathematical nature, several of which are new, and were proposed to him by Mr. Belidor. Some indeed are not so, having been solved by Messieurs Varignon and Parent; but then he has solved them after a different, and, as he hopes, a more agreeable manner, the construction being more simple, and the process much shorter.

Observations of the Moon's Transit by Aldebaran, April 3, 1736, *at London. By John Bevis, M. D.* N° 446, p. 90.

At 7^h 40^m, apparent time, the moon's body and Aldebaran were seen together in the distinct base of the telescope.

A Collection of the Observations of the Lunar Eclipse, Sept. 8, 1736, *which were sent to the Royal Society.* N° 446, p. 92.

1. In Fleet-street, London, by Mr. Geo. Graham, F. R. S. and by Mr. James Short of Edinburgh, F. R. S. P. 92.

Apparent Time.

At 12^h 58^m 0^s Beginning of the eclipse.
14 3 45 Beginning of total darkness.

The observation made with a $5\frac{1}{2}$-inch reflecting telescope, magnifying about 38 times.

2. In Covent-Garden, London, with a 5-foot telescope. By J. Bevis, M. D. p. 93.

Apparent Time.

At 12^h 56^m 50^s Beginning of the eclipse.
14 2 25 Total immersion of the moon.

3. At Wittemberg in Saxony. By J. F. Weidler, R. S. S. &c. P. 94.

1^h 50^m 0^s Beginning of the eclipse.
2 53 00 The total obscuration.
4 44 00 Beginning of the emersion.

4. At Hudson's-Bay, by Capt. Christopher Middleton, F. R. S. P. 96.

Being in Hudson's-Bay, in the latitude 55° 34′ north, and on the meridian of the North-Bear-Island, which lies 30 miles to the westward of Charlton, Capt. M. observed this total eclipse of the moon. The weather was very clear, but the motion of the sea rendered his telescope useless, and he missed the beginning.

Apparent Time.

8^h 43^m Total immersion of the moon into the shadow.
10 29 The emersion.
11 37 The end.

This same eclipse being observed by Dr. Bevis at London, when he made the true time of the total immersion of the moon's body into the shadow, $14^h\ 2^m\ 25^s$; consequently the difference of longitude between London and North-Bear-Island in Hudson's Bay, is $5^h\ 19^m\ 25^s$, or $79^\circ\ 51'$.

A Solar Eclipse observed at London, Sept. 23, 1736. By J. Bevis, M. D. N° 446, p. 98.

$4^h\ 45^m\ 31^s$ Beginning of the eclipse.

Observations of the Occultation of Mars by the Moon, Oct. 7, 1736. N° 446, p. 100.

1. By Mr. Geo. Graham, F. R. S. in Fleet-street, London, with a refracting telescope of 12 feet.

The first contact could not be seen for clouds.

Apparent Time.	
At $14^h\ 24^m\ 44^s$	Mars appeared about half covered.
14 25 21	Mars totally covered.
15 11 22	The moon appeared, but Mars was not seen, no part being yet emerged.
15 15 11	Judged it was quite emerged, but clouds prevented the moon's limb from being distinctly seen.

2. In Covent-Garden, by J. Bevis, M. D. p. 101.

Before the eclipse, he took several differences of right ascension and declination between ♂ and μ Piscium, for ascertaining the true place of Mars: as also several differences of right ascension and declination between the moon and Mars, before and after the eclipse.

Apparent Time.	
$14^h\ 24^m\ 10^s$	He was surprised to see Mars continue quite round, though hardly, to appearance, disjoined from the scabrous edge of the moon; but that instant he thought it began to lose its figure.
15 14 46	The moon being just clear of a cloud, saw Mars partly emerged.
15 14 49	He seemed just half out; then clouds came on again, so that the final contact was not seen.

Observations of the Transit of Mercury over the Sun, Oct. 31, 1736. N° 446, p. 102.

1. By Mr. George Graham, F.R.S. in Fleet-street, London, p. 102.

Apparent Time.	
At $9^h\ 22^m\ 00^s$	Mercury not yet seen, then clouds.

Apparent Time.	
At 9^h 25^m 37^s	He first saw Mercury for a few seconds, and judged he was got entirely within the sun's disk, or perhaps a little more; then clouds again, with some intervals of a few moments between, which allowed a sight of Mercury about 3 or 4 several times; then quite cloudy till near 12, when we had a sight of the sun for a few minutes, and took his transit upon the meridian; at which time we judged Mercury to be about two of his diameters, or a little more, within the sun's disk, and a little past the vertical line.
12 10 27	We had again a sight of the sun, but Mercury was gone off.

2. At the observatory of Bononia, by Sig. Manfredi, F. R. S. P. 103.

Beginning of Mercury's ingress	22^h	7^m	56^s
Ingress of the centre	22	9	34
Total ingress	22	11	12
Beginning of the egress	0	50	50
Egress of the centre	0	52	28
Total egress	0	54	6
The mora of Mercury's centre on the sun's disk	2	42	54
Semimora	1	21	27
Middle time of the transit	23	31	1

3. Extracts of a Letter from Mr. Professor Weidler, F. R. S. &c. to Dr. Mortimer, Secr. R. S. dated at Wittemberg, Jan. 1, 1737, N. S. P. 110.

Mercury appeared within the sun's eastern limb, as represented in fig. 17, pl. 5.

10^h	49^m	20^s	...... at	1	12^h	4^m	30^s	...... at	5
11	36	00	...... ☿ abt.......	2	12	44	20	....	6
11	52	20	...... at	3	12	52	45	....	7
12	2	30	....	4					

A Collection of Observations, relating to the Comet which appeared in January, February, and March 1736-7. N° 446, p. 111.

1. Observations on that Comet made at Oxford, by J. Bradley, F.R.S. P. 111.

Mr. Bradley made several observations on the late comet, during the last 5 weeks of its appearance, which enabled him to find out the elements of a parabolic trajectory, on which a calculus might be founded, that would correspond with each of his observations within about a minute of a degree: but the first of them being taken many days after the time of the perihelion, and the whole series comprehending but a very small portion of the trajectory; he was sensible, that a little error, either in the observations themselves, or in the places of the fixed stars, with which the comet was compared, might occasion a considerable difference in the situation and magnitude, &c. of the orbit, deduced from them alone; and therefore he was desirous of having some earlier and accurate observations, to determine those elements with more certainty: but not having yet been able to procure such, he no longer defers laying before the Society the particulars of his own, with the comparison between the observed places of the comet, and those computed from such elements as he had already collected from his own observations.

Mr. B. first saw the comet on the 15th of February 1737, between 6 and 7 in the evening, when its nucleus appeared small and indistinct, and its tail, extending above a degree from the body, pointed towards the star in Lino Austral. Piscium, marked ξ by Bayer. Applying the micrometer to a good 7-foot tube, he observed, that at $7^h\ 32^m$, equal time, the comet preceded the said star 1° 1′ 40″ in right ascension, and was 20′ 20″ more southerly than the star. Note, That the equal time is likewise made use of in all the following observations.

Assuming the place of this star, as it is settled in the British Catalogue, as he likewise does others hereafter mentioned, it follows, that the comet's right ascension was 23° 58′ 0″, and its declination 1° 31′ 55″ north.

February 17, 7 hours 33 min. the comet followed α in Nodo Lin. Piscium 31 min. 25 sec. in right ascension, and was 52 min. 30 sec. more northerly. Hence the comet's right ascension was 27 deg. 38 min. 20 sec. and its declination 2 deg. 21 min. 10 sec. north.

February 18, $7^h\ 14^m$, a small star (whose right ascension was afterwards found to be 29° 0′ 5″, and declination 2° 58′ 30″ north) preceded the comet 24′ 0″ in right ascension, and was 15′ 30″ more northerly. Hence the comet's right ascension was 29° 24′ 5″, and its declination 2° 34′ 0″ north.

February 21, $7^h\ 25^m$, the comet preceded ν Ceti 1° 6′ 0″ in right ascension, and was 38′ 20″ more southerly. Hence its right ascension was 34° 25′ 10″, and its declination 3° 47′ 20″ north.

February 22, $7^h\ 45^m$, the comet followed ν Ceti 30′ 5″ in right ascension, and was 18′ 45″ more southerly. Hence the comet's right ascension was 36° 1′ 15″, and its declination 4° 6′ 55″ north.

February 25, $7^h\ 45^m$, a small star (whose right ascension was afterwards found to be 40° 34′ 0″, and declination 5° 5′ 30″, north) followed the comet 2′ 30″ in right ascension, and was 2′ 30″ more northerly than the comet. Hence the comet's right ascension was 40° 31′ 30″, and its declination 5° 3′ 0″ north.

The difference of right ascension and declination, between this star and the comet, was taken with a 15-foot telescope; but the place of the star was determined by one observation made with the 7-foot tube.

February 27, 8^h 45^m, the comet preceded a small star, 1° 16′ 0″ in right ascension, and was 2′ 15″ more southerly. The right ascension of this star was afterwards, by a single observation, found to be 44° 37′ 40″, and its declination 5° 38′ 30″ north. Hence the comet's right ascension was 43° 21′ 40″, and its declination 5° 36′ 15″ north.

March 4, 8^h 0^m, a small star, whose right ascension was found to be 49° 30′ 30″, and its declination 6° 38′ 30″ north, preceded the Comet 7′ 30″ in right ascension, and was 10′ 0″ more southerly. Hence the right ascension of the Comet was 49° 38′ 0″, and its declension 6° 48′ 30″.

March 12, 8^h 25^m, the Comet preceded μ Tauri 2° 5′ 50″ in right ascension, and was 4′ 25″ more northerly than the star. Hence the Comet's right ascension was 58° 12′ 40″, and its declination 8° 16′ 50″ north.

March 14, 9^h 0^m, the Comet followed the 47th star of Taurus in the British catalogue 12′ 50″ in right ascension, and was 0′ 15″ more northerly than the star. Hence the Comet's right ascension was 60° 8′ 5″, and its declination 8° 34′ 5″ north. This, and all the following observations, were made with a good 15-foot telescope, the Comet now appearing too faint to be well observed with the 7-foot tube.

March 17, 8^h 40^m, the Comet followed γ Tauri 25′ 5″ in right ascension, and was 9′ 40″ more northerly. Hence its right ascension was 62° 47′ 55″, and its declination 8° 58′ 45″ north.

March 19, 7^h 50^m, the Comet followed the same star 2° 4′ 50″ in right ascension, being 23′ 55″ more northerly. Hence its right ascension was 64° 27′ 40″, and declination 9° 13′ 0″ north.

The same night, at 9^h 0^m, the Comet preceded *d* Tauri 47′ 40″ in right ascension, and was 22′ 50″ more southerly. Hence its right ascension was 64° 30′ 20″, and declination 9° 12′ 35″ north.

March 20, 8^h 5^m, the Comet preceded *d* Tauri 0′ 30″ in right ascension, and was 16′ 35″ more southerly than the star. Hence its right ascension was 65° 17′ 30″, and declination 9° 18′ 50″ north.

March 22, 8^h 15^m, the Comet followed the same star 1° 36′ 10″ in right ascension, and was 3′ 50″ more southerly. Hence its right ascension was 66° 54′ 10″, and declination 9° 31′ 35″ north.

This was the last night that he saw the Comet; for the moon being then in her increase, entirely obstructed its further appearance. The light of the Comet was indeed, even in the moon's absence, so very weak, that he found it difficult, in some of the latter observations, to take its place with any tolerable certainty; which is, in part, the cause of some little disagreement observable in the Comet's places taken from the same stars on different nights; though there are likewise other irregularities that occur in this series of observations, which seem to arise from small errors in the assumed places of the stars.

Supposing the trajectory described by this Comet to be nearly parabolical, conformable to what Sir Isaac Newton has delivered in the 3d book of his Princip. Math. he collects from the foregoing observations, that the motion of this Comet, in its own orbit, was direct, and that it was in its perihelion, Jan. 19, 8^h 20^m equal time at London. That the inclination of the plane of the trajectory to the ecliptic was 18° 20′ 45″. The place of the descending node ♉ 16° 22′. The place of the perihelion ♒ 25° 55′. The distance of the perihelion from the descending node 80° 27′. The logarithm of the perihelion distance from the sun 9.347960. The logarithm of the diurnal motion 0.938188.

From these elements, by the help of Dr. Halley's general table for Comets, to which they are adapted, he computed the places in the following table; which also contains the longitudes and latitudes of the Comet, calculated from the observed right ascensions and declinations abovementioned, with the differences between the observed and computed places.

Oxon. 1737, Equal Time.	Com. Longit. Observat.	Lat. Aust. Observat.	Com. Long. Computat.	Lat. Aust. Computat.	Diff. Long.	Diff. Lat.
Feb. 15d 7h 32m	♈ 22° 45′ 7″	7° 53′ 27″	♈ 22° 45′ 0″	7° 53′ 1″	+ 7″	+26″
17 7 33	26 30 30	8 27 21	26 30 44	8 28 6	− 14	−45
18 7 14	28 18 14	8 44 20	28 17 46	8 43 57	+ 28	+23
21 7 25	♉ 3 26 34	9 26 50	♉ 3 26 53	9 26 46	− 19	+ 4
22 7 45	5 4 53	9 40 0	5 5 28	9 39 27	− 35	+33
25 7 45	9 42 18	10 12 21	9 41 19	10 12 22	+ 59	− 1
27 8 45	12 36 43	10 31 42	12 36 16	10 31 13	+ 27	+29
Mar. 4 8 0	19 3 0	11 6 46	19 3 5	11 7 8	− 5	−22
12 8 25	27 49 58	11 43 3	27 49 53	11 43 19	+ 5	−16
14 9 0	29 47 42	11 49 59	29 47 19	11 49 26	+ 23	+33
17 8 40	♊ 2 30 57	11 56 31	♊ 2 30 50	11 56 49	+ 7	−18
19 7 50	4 12 36	12 0 19	4 12 45	12 0 47	− 9	−28
9 0	4 15 11	12 1 12	4 15 13	12 0 52	− 2	+20
20 8 5	5 3 10	12 3 5	5 3 32	12 2 33	− 22	+32
22 8 15	6 41 30	12 6 15	6 41 19	12 5 42	+ 11	+33

From the small differences between the Comet's observed and computed places, exhibited in the last two columns of this table, we may reasonably conclude, that the orbit, as above determined, cannot differ much from the truth, and must therefore be near enough to enable future astronomers to distinguish this Comet on another return, and thus to settle its period; which he cannot at present pretend to do, not having met with an account of any former Comet that seems likely to have been the same with this, a description of which has been given particular enough to determine this point.

2. The same observed at the Aventine Hill at Rome. By Didaco de Revillas Abb. Hieronym. R. S. S. p. 118. From the Latin.

Feb. 16, 1737, about 7 o'clock, p. m. the Comet appeared for the first time in the western part of the heavens, 8 or 9 degrees lower than Venus, and declining a little towards the south from her vertical circle. With the naked eye nothing was perceived but a small whitish line, of a faint light: yet with a very good telescope of Campani's, of 6 feet, besides the tail, which extended to the opposite part from the sun, and appeared like a small line without the telescope, was likewise espied the nucleus, though encompassed all round with a thin atmosphere. As there was then no quadrant at hand, and not only a fog intercepted, but the twilight deprived the view of the neighbouring fixed stars, the apparent place of the Comet could not be determined for that night.

From the 16th till the 19th, as also after the 25th, there happened other impediments, which prevented observations. And in the nights between 19 and 26, the Abbé could not otherwise determine the apparent place of the Comet, than by comparing the phenomenon with Venus; as he only employed a small quadrant, whose tube was scarcely an English foot in length. From the vertical altitudes therefore, both of the Comet and Venus observed at the same time, were collected the vertical differences of both, as below.

Times.			Vert. Differ.	
20^d	7^h	59^m	$5°$	$22'$
22	7	0	3	56
23	7	20	3	13
24	6	15	2	30
25	7	30	1	47

3. Observations on the Comet, and of an Eclipse of the Sun, Feb. 18, 1736-7, made at Philadelphia in Pennsylvania. By Dr. Kearsly, p. 119.

Jan. 27, about 6 in the evening, Dr. K. saw a dull star, about 3 or 4 degrees above Mercury, and a little to the southward of a vertical passing through him, but took little notice of it then, not thinking of a Comet; but by comparing ☿'s place with the fixed stars, he afterwards thought it might be a Comet.—On the 31st, about 6^h 30^m P. M. he took its distance from Venus, by a reflecting instrument of Mr. Hadley's make, 14° 40′; but by a forestaff, 14° 50′;

and a right line passed over the Comet, Venus, and the Pleiades. The night following, about $6^h\ 20^m$, its distance from Venus was, by Hadley's instrument, $13^\circ\ 25'$. The rest of his observations, by such instruments as he had, being none of the best, and the Comet growing very dull, are as follow:

Feb.	7^d	6^h	47^m	Comet from Venus $7^\circ\ 40'$.
		7	3	——— from Aldebaran $59^\circ\ 40'$.
				——— from Algenib $17^\circ\ 45'$, by a fore-staff.
				A right line from the Comet over Venus passed over the bright star in the side of Perseus.
	11	7	14	Comet from Venus $7^\circ\ 12'$.
		7	20	A right line over the Comet, Venus, and head of Cassiopeia.
	17	7	20	The Comet was in a right line, and to the northward of two stars; distance of the stars supposed about $40'$, and the Comet from the least $30'$. These stars were the south node of Pisces, the brightest from Venus $10^\circ\ 20'$, from Aldebaran $50^\circ\ 30'$, as he found it set down, but must be very false.
	20	30	7	Comet from Aldebaran 34°, from Lucida Cap. γ $19\frac{1}{2}$.
	21	30	8	Wanted about a degree of oculus ceti.—Which was the last sight he had of it.

P. S. The eclipse Feb. 18, could not be well observed here, by reason of clouds. Dr. K. rectified the clock by one of Heath's large ring dials. At $7^h\ 18^m$ there was a small dent in the sun's edge, whence the beginning 1 or 2 minutes sooner: just before the end, viz, 10^h 11 or 12^m, he had a sight of the sun again, and there was then a dent in the sun's edge, so that the end must be 10^h 13 or 14^m in the morning: about the middle of the eclipse, there was a large spot near the middle of the enlightened part, which was the north side of the sun.

4. The same observed in Jamaica. By Rose Fuller, M. D. F. R. S. p. 122.

At Spanish Town, Jamaica, there was the appearance of a Comet, which was first perceived about the 26th of January, but must, by its plainness then, have been visible for some time before. It was in the west at first, some degrees below and directly under Venus. Every night it appeared nearer to that star, but inclined northerly. In about a fortnight, it was parallel to it, and in a week after, it was no more to be seen.

5. The same observed at Madras. By M. Sartorius, a Missionary there, p. 122.

For 7 days before Feb. 9, about 7 in the evening, there appeared a dim

Comet: it is seen in the west, under Venus, towards the s. w. It looks through a tube of 10 or 11 feet long, like a dim or pale planet; its tail tends upwards.

6. The same observed at Lisbon, by G. R. Vanbrugh, Esq. on board the Burford Man of War, p. 123.

At $6^h\ 49^m$ P. M. we saw a Comet with a long brush tail; at which time its altitude was found 5° 15′, its distance from Venus 18° 5′; and Venus's altitude was observed 20° 40′. It bore due west.

A Description of some Mammoth's Bones, dug up in Siberia, proving them to have belonged to Elephants. By John Phil. Breyne, M. D. F. R. S. N° 446, p. 124.

In the Philos. Trans. N° 403 and 404, Sir Hans Sloane gave accounts of elephants' teeth found under ground. In the same year, viz. 1728, Dr. Breyne was busied about the very same matter, especially to prove, that the extraordinary large teeth and bones found under ground, and dug up in several places of Siberia, by the name of mammoth's, or mammut's, teeth and bones, were,

1. True bones and teeth of some large animals once living; and,

2. That those animals were elephants, by the analogy of the teeth and bones with the known ones of elephants.

3. That they were brought and left there by the universal deluge.

After that, viz. in the year 1730, Dr. Messerschmidt returned to Dantzic from his travels through Siberia, and communicated some curious draughts of a part of a skeleton, viz. of a very large skull, dens exertus et molaris, with the os femoris, belonging to the animal commonly called mammoth, found in Siberia; by which our assertion, that the teeth and bones, called in Russia mammoth's bones, are the true teeth and bones of elephants, is not only put in a clearer light, but seems demonstrated beyond all doubt.

In 1722, Dr. Messerschmidt found two very large teeth, which he sent to Dr. Breyne. After he had made an accurate and nice examination of them, he found that one is a dens molaris, or grinder, a foot broad, half a foot long, and 3 inches thick, weighing 8 lb. and ℥iij, pretty entire, except that it is broken in two pieces, and the extremities of the roots spoiled. The substance is between that of a bone and stone, except that on the upper part of the outside, some parallel undulated lines appear, which have still preserved the enamel of the tooth.

The other is a piece of a dens exertus, or tusk, 8 inches long and 3 inches thick, or 1 lb. and 6 oz. weight; in some places not different from ivory, but in others calcined like the common unicornu fossile.

What Ysbrand Ides mentions of the mammoth's teeth and bones, is deserving of notice: as also the Journal of Laurens Lange's Journey to China, and the remarks of Capt. John Bernard Muller, in his Present State of Russia. These are the chief authors* who have treated of the mammoth's teeth and bones, as a very remarkable and particular curiosity of Siberia. And what we may select out of them, as matters of fact, are the following particulars.

1. That those teeth and bones are found in Siberia, chiefly in the northern parts, near the rivers Jenizea, Trugau, Mongam-sea, Lena, &c. towards the icy sea; at the time when the ice has broken the banks of those rivers, so that part of the adjacent mountains fall down; and that they are found in such quantity as is sufficient for trade, and to make a monopoly for the Czar.

2. That sometimes skeletons of this kind are found nearly complete.

3. That those teeth and bones are not found always of the same size, but sometimes very large; as dentes molares, or grinders, of 20 or 24 lb. weight, and dentes exerti, two of which weighed 400 lb.; sometimes of a middle size, as those abovementioned, and at other times still smaller.

4. That of those teeth, viz. dentes exerti, some are used as ivory, to make combs, boxes, &c. Capt. Muller says, that in every respect it resembles the common ivory, being but a little more brittle, and easily turning yellow by weather or heat.

Out of these quoted remarks, joined to ocular inspection, Dr. Breyne thinks he may advance three things.

1. That those mammoth's teeth and bones are truly natural teeth and bones, belonging to very large living animals; because they have not only the external figures and proportions, but also the internal structure analogous to natural teeth and bones of animals.

2. That those large animals have been elephants, as appears by the figure, structure, and size of the teeth, which accurately agree with the grinders and tusks of elephants, as represented by several writers.

3. That those teeth and bones of elephants were brought thither by a deluge, by waves and winds, and left behind after the waters returned into their reservoirs, and were buried in the earth, even near the tops of high mountains. And because we know nothing of any particular extraordinary deluge in those countries, but of the universal deluge of Noah, it is more than probable, that we ought to refer this strange phenomenon to the said deluge.

It may be noticed, that such teeth and bones are also to be found in several

* Add Gmelin and Pallas.

other countries, besides Siberia, as Poland, Germany, Italy, England, Ireland, and many others; but less common than in Siberia, and not so well preserved, but more wasted and calcined, doubtless by the greater warmth of those climates.

Hither also are to be referred the large bones found under ground, or rather tusks of elephants, known by the names of ebur, seu unicornu fossile, which are of the same origin with the mammoth's teeth, but different, as they are better preserved, and have still the natural bony substance, and may serve the workmen as natural ivory, and in some measure the physicians and apothecaries as ebur, seu unicornu fossile.

Of the above bones, the head weighed 130¼ lb. avoirdupois weight, or 152 Russian pounds; its length or greatest height is 48 inches; its greatest breadth near the ears 29 inches, 5 lines; its thickness, from the forehead to the nape of the neck, 22 inches, 5 lines.

One grinder weighs 8 lb. 9 oz. or 10 lb. Russian; its greatest length 12 inches; its perpendicular height 5 inches; its thickness or breadth, 3 inches; it is made up of above 20 transverse lamellæ, a finger thick, perpendicularly erect, lying close to each other, and its root composed of two apophyses.

The tusk, by some improperly called the horn, of the right side, having a two-fold direction by being bent outward and backward, which is peculiar to the male elephant, it being straighter in the female. It is the ebur fossile of the shops, and weighs 137 lb. or 160 lb. Russian; its length, or the exterior circumference of its back part, was 136 inches, 5 lines; the circumference of the root, where it got clear of the socket, was the greatest, being 18 inches 5 lines; the subtended arch, from one extremity to the other, 55 inches.

The tusk answering to the foregoing on the left side, was quite like that on the right, except the contrary direction of its curvature, and its less weight, having lost its point; for it weighed only 128½ lb. or 150 lb. Russian.

The right thigh-bone weighed 21 lb. 6 oz. or 25 lb. Russian; its perpendicular length is 38 inches 5 lines; the greatest breadth of its upper head, or apophysis, 11 inches; its circumference at the middle of the bone about 13 inches.

The bones of this skeleton, with the ribs, vertebræ, and others belonging to it, were found in the side of a sandy steep hill, on the eastern bank of the river Indigirska, which falls into the northern ocean, not far from the mouth of the rivulet Wolockowoi ruszei. And some of these bones are found not only in these parts, but likewise in the sand hills on the rivers Chatanga, Thomas, Tobol, Irtisch, &c. which are all at a good distance from the sea; though neither elephants, nor chimerical behemoths, have been ever seen in those countries, nor could they live there by reason of the inclemency of the air. Wherefore the best judges follow the opinion of the learned Dr. Woodward, the Scheuchzers, and others, in taking them for the bones of antediluvian animals, or of such as were conveyed thither in the universal deluge.

Of a large Glandular Tumour in the Pelvis, and of the pernicious Effects of crude Mercury given inwardly to the Patient. By Dr. Andrew Cantwell, of Montpellier. N° 446, p. 139.

Dr. C. was called to visit an English gentleman who was ill. In the house where he lodged, was one P——r M——n, born in France, but settled in Cadiz. This poor gentleman, having been very ill for 2 or 3 years, had lost the use of his left leg and thigh, was subject to frequent head-achs, and pains in his bones, but more especially in his legs; for which, because he had been given to women, his physicians in Cadiz salivated him twice, sent him to several hot waters, and gave him all the remedies they could imagine, but to no purpose; for his illness increasing, he had from time to time great difficulty of making water, and going to stool. In this condition he came from Spain to Marseilles, and from thence was sent to the waters of Baleruc, of which he drank a great quantity. But as they did not pass, his physician there ordered him strong purges, with clysters of a decoction of tobacco, and the like. He then began to vomit his excrements; on which the physician to the Marquis of C———'s regiment in Spain, who happened to be there, ordered him $\frac{1}{2}$ lb. of crude mercury by the mouth, which made him suffer the most exquisite pains; and his belly swelled, and became as stiff as a drum. Here Dr. Montagne was sent for, who soon discovered the error in the preceding practice, by feeling a solid body near the rectum, which obstructing the passage, hindered the clyster-pipe from entering far enough into the gut. After his departure, the patient was again ordered clysters, which were injected with a crooked pipe, and several purges; till at the end of 8 days he died, having his belly larger, stiffer and harder than ever. Though Dr. C. arrived the day before his death, he saw him not till after he expired. His physician having invited him to open the body, he willingly consented, curious to find the solid resistance or tumour, which he could give no account of. He sent for the surgeon of the village, who opened the abdomen, which was filled with a whitish liquor of some consistence. The epiploon was all dissolved, and swam in this liquor like so much pus.

This water poured out, Dr. C. examined the intestines. The colon was burst under the stomach, and in 3 other places at its lower part; and so was the cæcum; the ileum all inflamed, and in one part gangrened. The lips of the ruptures were plastered with excrements, all beset with a prodigious number of globules of quicksilver; and when the intestines were disengaged and taken out, the quicksilver fell from them in large drops. The other viscera were in the natural state, except the liver, which was gangrened.

As Dr. C. was very solicitous about the tumour, he looked into the pelvis, where he found an excrescence of a prodigious size, which filled all its left side. He took the knife, and cleared all round the tumour; when he found the urinary bladder close pent up between the anterior part of the tumour and the ossa pubis, which occasioned the strangury the patient had been tormented with; the rectum, which lay on the middle of the os sacrum, was also vastly pressed on by the tumour, which seemed to take its rise from the holes that are in the left side of that bone. The surgeon was so unluckily impatient, that while Dr. C. laid down the knife, in order to separate the ossa pubis with a hatchet, he cut out the tumour. Dr. C. then examined the os sacrum, which was so very soft, that his fingers entered it every where on the left side.

The tumour was oviform, and was covered over with several membranes: its weight was $2\frac{1}{2}$lb.; its longest axis 5 inches and somewhat more than $\frac{3}{4}$, French measure; its shortest $4\frac{3}{4}$ inches. At first sight Dr. C. took it for a parenchyma, but on dissection he found it analogous to the liver in substance, colour and consistence. Its artery, vein and nerve were very large, and were distributed through its whole substance: wherefore he really took it to be one of the conglobate glands of the pelvis, whose vessels yielding to the blood impelled thither with greater force and in larger quantity than usual, on account of the violent exercises of dancing, jumping, &c. which the patient very much practised, gave room to its increase to that enormous size. On opening, he remarked 3 very apparent divisions in it: and where the psoas lay over it, and one of the pyramidales beat on it, it was ossified. He preserved it in brandy, and found that the small vessels, that were most filled with blood, pressed it out into the interstices of the neighbouring ones.

The weight the patient constantly complained of at his left hip; the difficulty he had in going to stool, and that of thrusting a syringe far enough into the rectum to give him a clyster with any success; the tumour itself, which was easily felt on putting the finger into the anus; with the palsy of the left leg and thigh, might have given other indications to the physicians, than those they took. And doubtless the frictions and other heating medicines, the patient was plied with, contributed to augment his illness. In fine, the crude mercury he swallowed, the vast quantity of Baleruc water he drank before it, with the strong cathartics taken by the mouth and anus, seem to have cut him short of some months, which he might have lived, had he used no other remedies than a slender relaxing diet.

A Catalogue of the Fifty Plants from Chelsea-Gardens, presented to the Royal Society by the Company of Apothecaries, for the Year 1736, *pursuant to the Direction of Sir Hans Sloane, Bart. P. R. S. By Isaac Rand, F. R. S.* N° 447, p. 143.

This is the 15th annual present, amounting to 750 plants.

Of a Narhwal or Unicorn Fish, taken in the River Ost, in the Duchy of Bremen. By Dr. Steigertahl, F.R.S.* N° 447, p. 147.

Towards the end of Jan. 1736, N. S. was taken a sort of whale, called the narhwal or sea-unicorn. It was taken in the river Ost, near the village Bellum, where it falls into the Elbe, in the duchy of Bremen, 4 German miles from the sea. A great quantity of fat was taken out of it, to make whale-oil; but this train-oil was of almost intolerable stench, because this narhwal feeds on carcasses: for nar signifies a carcass or dead body, according to Valentini in his Museum Museorum.

Such care was taken of the skin, before the dissection, that it was cured with salt and alum, and stuffed so as to give the just figure of the fish: having left with it the bones of the skull, and some vertebræ near the tail.

The skin was spotted with dark brown spots on a white ground. The epidermis was transparent, and under it was another skin very thin and spotted; but the true skin was brown, and near an inch in thickness. On the top of the head was a semilunar hole, as in the porpoise, according to the description given by John Daniel Major, and published in the Miscell. Academ. Nat. Curios. Dec. 1, An. 3, p. 22. This hole opens into the two canals which run through the skull to the palate, and are called by Major, ductus hydragog. They did not remark in the skin any opening or outlet for the excrements; and it is said, that this narhwal voided them through the hole on the top of the head.

Concerning the horn, Dr. S. agrees in opinion with Wormius and others, who take it for a tooth; but he cannot believe that its sole use is to break the ice: it rather serves the fish for seeking its food. A captain of a Greenland vessel assured him, that being on the coast a whale-fishing, and having taken one, as he was turning the whale to get at the fat, he found on the opposite

* Monodon monoceros. Linn.

side to him a narhwal, that had stuck this tooth into the whale's belly, up to its mouth, and had sucked the blood and humours.

Fig. 1, pl. 6, represents the unicorn fish. 1, Shows a semilunar hole, through which the fish cast out water and blood, on dying; 2, a small rising on the middle of the back, and fleshy as the fins; 3, the mouth, very little, without teeth in the upper jaw, except this dens prominens, or tusk; which has by some been taken for a horn; and no lower jaw was found; 4, the eye, very small; 5, the fin on the right side, which, as well as the opposite, is fleshy; 6, the tail, fleshy, like the fins, which, taken according to its width, is not vertical, but horizontal; 7, the prominent tooth or tusk, generally taken for a horn.

The length of this narhwal, from N° 3 to 6, was 17 feet 9 inches; the tooth 6 feet 3 inches; the greatest thickness, measured round, was 14 feet; the skin was smooth, without scales, like that of an eel, and was white, marked with blackish spots.

A Description of the same Narhwal, communicated by John Henry Hampe, M.D. F.R.S. N° 447, p. 149.

In a creek, called the Beluhmer Wadt, belonging to the Bailiwick of Newhaus, in the duchy of Bremen, has been caught alive, an unknown fish of a large size, 18 to 20 feet in length, and 4 in diameter. He has on the fore part of the head, just above the mouth, which is very small, a horn 6 feet long, white like ivory, and curiously twisted. The body is white, sprinkled with black spots, and smooth like an eel. The head is, in comparison of the body very small, about 16 inches in length, and the same in diameter. The eyes are also small, about the size of a sixpence. On the upper part of the head, is a hole about 3 inches in diameter, out of which probably he spouts water, like the whales. On each side of the neck are placed two black fins, one above another, and at a small distance from each other. They are half an inch in thickness, of one hand's breadth, and 2 feet in length, round on the fore part, all fleshy, and of a liver-colour.

Of a Water Insect, not hitherto described. By M. Klein, F.R.S.* N° 447, p. 150. *From the Latin.*

A friend of M. Klein's presented him with a water insect, found at Uderwanga in East Prussia, among fresh water crabs, and utterly unknown to the crab fishers. From the great number of its feet, and surprising facility of

* The insect here described is the *monoculus apus* of Linnæus

moving them, it may with equal, if not better reason, be called scolopendra aquatica scutata, than Aldrovandus, p. 721, de cetis, calls a certain fish of the whale kind, scolopendra cetacea.

A, fig. 2, pl. 6, represents the insect on its upper part, covered with a shield; which nearly resembles tortoise-shells, except that along the middle of the back it is a little gibbous, and towards the extremity of the body it has a triangular hiatus, slightly indented: it is entire, and almost of the same substance, though of a more dilute colour, with the sheaths of the wings of the scarabæus Gœdarti, produced from weevils, or what is called scarabæus rosarum. The eyes pass out through the shield, and are a little prominent.

B, fig. 3, represents the insect delineated on the under part; where at the same time appears a vast number of legs: each of these has a certain bag as at D, fig. 5, terminating in 3 feet, or rather claws; the two anterior ones have this in peculiar, that their three feet or claws are longer than the others, though they differ from each other in length. All the claws of the greater and smaller feet consist of similar articulations; and such as the hairs of the forked tail of this scolopendra, or the antennæ of other insects have.

M. Klein supposes this insect makes use of the longer claws of both the anterior feet, and of the hairs of its forked tail behind, for antennæ, by means of which it timely discovers either its pursuing enemies, or such as it meets with in lurking places; unless we are to suppose the two short horns, that appear in the fig. at that place, where we are reasonably to look for the head, to be antennæ.

C, fig. 4, represents the body, bared of its shield, viewed on the back; on account of which the shield is carefully divided lengthwise, which as to the part explained, is not continued with the back. In the thin cuticle of the lower part of the shield, and that on both sides, may be observed, as in the fig. punctures like needle work.

M. Klein could not certainly determine, whether it sucks in the water through these apertures into the cavity between the gibbous shield and the cuticle, and again emits it; or whether it fills the cuticle with air, or empties it, according as it wants either to go down to the bottom, or rise up to the top of the water. The insections or segments are about 30, but the legs cannot easily be numbered. In the extreme part of the body which separates the shield, the rings of the insections or segments are beset with small spines, and that in the same order, as they appear delineated fig. 2 and 4.

D, fig. 5, represents one of the legs next the anterior one, with the little bag.

B, fig. 3, represents another leg in a different view.

As long as this insect lived, it continually, and with singular facility, moved its feet, drawing at the same time into its sheath and putting out again, the extreme part of its body.

The same sort of Insect found in Kent. By the Rev. Mr. Littleton Brown, F. R. S. With a Remark, by the Secretary, Dr. Mortimer. N° 447, p. 153.

Mr. Brown presented a creature, whose name he could not learn from any books or persons. He brought it from a pond on Bexby Common, where great numbers had been observed for 5 weeks before. The pond was quite dry the 24th of June, but on its being filled with the great thunder-shower on the 25th, within two days the pond was observed to swarm with them. And it was thought observable, that there is no duct or channel that could convey them from any adjacent place.

Here E, fig. 6, pl. 6, represents this insect. Its legs are very extraordinary. Dr. Mortimer counted 42 on a side, in one of those found in Kent; the 20 next the head are nearly of a size, but then they grow gradually smaller and smaller towards the tail. He took out one of the larger ones of the left side of the chest; the foot consists of 5 flat membranous claws, with a stiff rib along their middle, and beset with hairs on the edges, like those of crabs; on the lower side of the leg hangs an oval bag, and beyond that grows a large thin membrane, which can be extended by a bony rib that runs across it; this membrane and the whole foot, is convex on the side next the head, and concave on that next the tail; the thigh, or first joint of the leg, is webbed on each side; so that the whole structure of the legs seems to show that they are rather designed for swimming with, than walking. The leg represented at E, was drawn, when the insect lay on its back, as at B. Many parts of this insect, though no larger than the figures, have some resemblance to those of the Molucca crab.

An Account and Abrtract of the Meteorological Diaries for the Years 1729 *and* 1730. *By Geo. Hadley, Esq. F. R. S.* N° 447, p. 154.

The diary kept by Mr. Hauksbee, by order of the Society, at their house in Crane-Court, consists of observations of the barometrical heights twice a day, i. e. morning and evening, in inches, decimals and centesimals; the thermometer likewise, in its proper graduations, and the weather, with the hour of each observation. The winds are omitted. The depth of rain is set down several times for the most part in each month, the sum of which is to be divided by 10, the funnel which catches the rain being so much larger in surface, than that of the vessel which receives the rain from it.

That from Southwick, near Oundle, in Northamptonshire, by George Lynne, Esq. contains the height of the barometer once a day, and the winds, the steadiness and strength of which are likewise marked with proper marks and figures. Observation is made of the upper and under currents of the air, when it so happened. The thermometer is marked twice a day; the weather often, both by day and night; the rain from time to time, and the quantity of each particular shower often set down by itself, with some other miscellaneous observations, as haloes, thunder-storms, and sudden changes of wind, &c. He remarks that his thermometer is placed in an out-house exposed to the air, but screened from the sun, which is a proper precaution in using that instrument. The remarkable rises and falls of the Mercury are also marked with proper marks; which method would be useful in the other columns also, for comparison of diaries, if some certain rule were agreed on.

That from Kent, 16 miles south east from London, gives an account of the barometer once a day, sometimes twice or thrice, with the hour of each observation, and the winds, weather, and rain, the proportion of which for every day, is given at the end of each month. There is also a separate column for the height of the clouds, which is divided into three orders; and where there are two orders at a time, they are both noted; as also when any of them move with different velocities or directions, which he supposes to be commonly a sign of change of the wind: but he does not inform us by what method he determined their heights or velocities. The reigning wind, and general strength of it, is noted at the end of each month; the eclipses also, and the times of the new moons; which he observes make it appear, that the notion of the change of weather depending on the age of the moon, is without any ground: with other miscellaneous observations; as the aurora borealis, fruitfulness or sterility of the season. He had no thermometer.

That from Hudicksvall in Sweden, by M. Olave Broman, shows the height of the barometer, sometimes once, sometimes twice or thrice a day, o. s. in English measure, with the winds, and the strength of them, and the weather. There is also to the diary 1729, annexed an account of the height of the sea-water for every day, which varies in the whole about 2 inches, and is sometimes interrupted by floods from rain. This probably relates to the tides in the gulph of Bothnia. There is no thermometer, nor the quantity of rain set down.

That from Risinge, in Ostrogothia, in Sweden, by Sueno Laurelius, pastor and provost, gives the height of the barometer for the most part 3 times, sometimes 5 times a day, with the hour of the observations, o. s. in English measure. He refers for the descriptions of his barometer and thermometer to the diary 1727. The winds, with the degree of their strength, weather, and depth of rain, are also set down.

In that from Upsal in Sweden, by Mr. Andrew Celsius, Astr. P. R. and F.R.S. observations are made 3 times a day, of the barometer and thermometer, both which instruments were made by Mr. Hauksbee; the winds, with their strength, and the weather, and depth of rain, from time to time.

That from Svenaker in Sweden, near Trollhetta, by Torstanus Wassenius, V. D. M. &c. contains the height of the barometer twice a day, sometimes 3 times, o. s. in Swedish feet and inches and decimals, which being supposed to be in proportion to English as 974.375 to 1000, the mean heights are reduced in the tables into that measure. The winds also, with their strength, are noted, and the weather. There is no thermometer. Notice is taken on thunder storms, and other meteors.

That from Lunden in Sweden, by Mr. Conrad Quensel, Math. Prof. in Acad. Carolina, contains observations of the barometer twice a day, o. s. in English inches and decimals, and 4ths of them; the winds, with their strength, and the weather. The thermometer is Florentine, and therefore the observations not inserted in the table.

That from Bygdea in Sweden, by Mr. John Telinus, pasor there, has observations of the barometer twice a day, morning and evening, o. s. in English inches and decimals; the winds, with their strength, and weather. The two last months are wanting. There is no thermometer.

That from Betna in Sudermanland, by Mr. And. Geringius, pastor and pro vost, has observations of the barometer thrice a day, except in the first part of January, o. s. in English inches and decimals; the winds, with their strength, and the weather, with other meteorological observations, and on the seasons, as to fruitfulness and sterility, &c. The aurora borealis is frequently mentioned. The thermometer is peculiarly graduated, and so could not be inserted. There is a column for rain.

From Wittemberg in Saxony, there are two diaries communicated, one from Mr. Mat. Hasius, Math. Prof. the other from Mr. J. Fred. Weidler, LL. B. and Math. Prof. Primar. That by Mr. Hasius has the height of the barometer several times a day, sometimes 4 or 5 times, o. s. in English inches and decimals, and the parts of these in vulgar fractions, but are reduced to decimals in the tables. He used two barometers and thermometers. Those marked I, are Mr. Hauksbee's, those marked II, Florentine. The coldest day he ever observed, was February the 5th 1726. It contains also the winds, with their strength, and weather. Mr. Weidler gives the height of the barometer 3 times a day, N. S. in Paris inches and lines, and the parts of these in vulgar fractions;

the winds also, with their strength, and the weather, and quantity of rain, in cubes and lines, but at the end of each quarter the depth is given in Paris inches and lines. The thermometer is Mr. Hauksbee's. There are some astronomical observations of eclipses, &c. He takes notice, that an occultation of Venus by the moon, observed with a telescope of 18 feet, may serve to prove the moon to have an atmosphere; for being then in the quadrature with the sun, the planet appeared to lose its cusps, and become oval, when it came near the moon.

That from Padua, by the Marquis Poleni, shows the height of the barometer once a day, o. s. in English inches and decimals; the winds, and sometimes their strength, and weather. The depth of rain is given both for the old and new style.

That from Bengal, by Mr. Bellamy, preacher to the factory, has the height of the thermometer twice a day, morning and evening; the winds, with their strength, and the weather, for the year 1730. The medium of the thermometer is taken from both the evening and morning heights, the difference there being very great in proportion between morning and evening.

That from Boston in New-England, by Paul Dudley, Esq. F. R. S. shows the weather 3 times a day, and wind once or twice. No barometer or thermometer.

The Abo observations for the year 1730, by Mr. D. Sporing, show the height of the barometer twice a day, in Swedish inches and decimals, but the mean heights are reduced to English in the tables. They show also the winds and weather, and in the last column the aurora boreales, which are frequent in most months of the year.

That from Naples, by Cyrillus, shows the height of the thermometer, which is Mr. Hauksbee's, once a day. The winds, with their strength, and weather, and depth of rain in Neapolitan measures, 23 of which make a London inch. The barometrical heights he has not set down, because he found them not to agree with those of former years, which made him suspect his instrument to be out of order; but as it appears he had removed his habitation, it might be owing to its being situated higher or lower than the former. An eruption of Vesuvius happening, an account is given of it, and of damage done by lightning, and also of the seasons, as to fruitfulness and healthiness.

First, he observed on the barometrical tables of these two years, that they confirm former remarks made by Dr. Derham and others, of the consent of the barometers in places at a good distance from each other. Not only the monthly mean heights agree in the three diaries of these two years here in England, but also the greatest ascent and descent of the mercury happen commonly on the same day, and the barometers have been found to agree in their motions to an hour, so far asunder as Townly in Lancashire, and Greenwich near London, which is near 160 miles, though that might be partly accidental. The barometer at Crane-Court and Southwick, distant about 55 miles, being compared, seem very seldom to vary from their mean difference above $\frac{1}{10}$ and $\frac{1}{5}$ each way; at Southwick and Kent something more. From whence it might be expected, that the weather should be much the same in all these places; which yet seems not to agree with accounts in some years from different parts in this island, not very far distant: and Mr. Hadley has observed sometimes clouds to lie in one part of the horizon for a great part of a day, which have discharged a large quantity of rain in places not far off, while the place, where he has been, has all the while enjoyed fair weather, and vice versa. Whence it appears, that the barometrical alterations of the air extend farther than their effects, as to the production of rain, at those times. Comparing the diaries of Crane-Court and Upsal, he finds the barometers vary from their mean difference an inch and half each way; Crane-Court and Padua as much, or more, and often go a pace quite contrary ways at the same time, and their monthly differences are also very variable, so that their agreement at any time seems to be but accidental.

Secondly, he observes, that the descents of the mercury below the mean heights of each place, taken in this way of Dr. Jurin's, are generally much greater than the ascents of it above; and there are also other extraordinary descents of the mercury in every year, of the same kind. The reason seems to be, because the expansion of the air, by which it becomes lighter in some one place, being the original of the alterations in the atmosphere, its effects by condensation or accumulation of the air, in the places round about, will be more dispersed, and therefore less sensible.

Thirdly, the variation or range is greater the farther north, as has been heretofore observed, and appears in these tables; and likewise it is greater generally in the winter than summer months. The sum of the motion of the mercury upwards and downwards, taken from the Berlin wandering line, with a pair of compasses, in the year 1726, amounts to about 76 inches, which gives $5\frac{1}{3}$ for a month, and about 0.21 for each day. But the barometer is by much most steady in the summer.

4thly, The mean height of the barometer has already been applied to determine the respective heights of places, and also the absolute height above the surface of the sea. Dr. Scheuchzer, in his Tables published in the Transactions of this Society, N° 405, 406, supposes, from Mr. Marriot, the mean height at the surface of the sea to be 28″ 1‴ Paris measure, which reduced to English, gives 29 inches, .993. This agrees very well with a Diary communicated to the Society, containing 10 months of the year 1723, and Jan. 1724; the author of which found by experiment, that in the place where his barometer was kept, the Mercury stood $\frac{1}{10}$ and $\frac{1}{2}$ higher than at the surface of the sea, which was not far from his habitation. The mean height of the barometer for those 10 months (leaving out the January following, which seems to be a very irregular month) is 29.825, to which adding $\frac{1}{10}$ $\frac{1}{2}$, it will give the mean height at the surface of the sea 29.975; so the difference between these is only, 018, and therefore probably may be near the truth, but may hereafter be more exactly determined by experiments. Then allowing about 90 feet, or rather less, for each 10th of an inch in height of the Mercury in smaller altitudes, or in greater according to the tables calculated for that purpose, by Dr. Scheuchzer and Dr. Nettleton, and published in the Trans. N°. 388, you will have the height of each place pretty nearly, provided the observations be carefully made, and continued for a sufficient time; for the yearly mean heights, in one of the places in these tables, appear to differ near $\frac{1}{10}$ of an inch in these two years; and in most of them, the last of these two years exceeds the first, two or three hundredths: the barometer also ought not to be removed to a lower or higher place.

The thermometers agree, especially as to the hottest days in the year, more than might be expected from places at such a distance.

The winds are of so uncertain and variable a nature, that they require a more than ordinary care and diligence in making the observations, and a great length of time, and comparison of a vast number of them, before any thing can be deduced more than is commonly known; and therefore he only gives this hint, that if the observers would take particular notice, in great storms, of the time when the Mercury first begins to rise, whether before, or after, or in the very height of it, it might be a direction to judge when an abatement or increase of it might be expected, if any regular order should be found therein, which might be serviceable on some occasions. But if any attempt should be made to lay down any thing certain concerning the rise and progress of the variable winds, it will appear, by considering the cause of the trade-winds, that for the same cause the motion of the air will not be naturally in a great circle, for any great space, on the surface of the earth any-where, unless in the equator itself, but in some other line; and, in general, all winds, as they come nearer the equator, will become more and more easterly, and as they recede from it, more and more westerly, unless some other causes intervene.

A Collection of the Observations of the Solar Eclipse, Feb. 18, 1736-7, *sent to the Royal Society.* N° 447; p. 175.

1. Observed in Fleet-street, London, by Mr. Geo. Graham, F.R.S. p. 175.

Apparent Time.

At $2^h\ 25^m\ 9^s$ P. M. a small impression appeared on the sun's limb; he judged the beginning to have been about 5 or 6 seconds sooner.

A cloud covered the upper limb, and prevented a sight of the ending.

Between 12 and 1 o'clock, he measured the diameter of the sun with a micrometer. At the time of the greatest obscuration, the lucid part of the sun's diameter was equal to 392 such parts, as his whole diameter contained 2188.

By a transit of the sun at noon, and of Sirius at night, which, compared with preceding ones, he found his clock went too fast for mean solar time, about one second in a day.

2. The same Eclipse observed at the Royal Observatory at Greenwich, in Company with Dr. Edm. Halley. By Dr. J. Bevis, p. 176.

Apparent Time.

At $2^h\ 25^m\ 39^s$ P. M. the beginning.
5 3 29 the end.

At the end, the sun's limb appeared somewhat tremulous, and a small thin cloud came over it. Dr. Bevis judged the time might be relied on to 2 or 3 seconds.

3. At Edinburgh, by Colin Maclaurin, F. R. S. p. 177.

In the history of eclipses collected by Ricciolus, there are very few said to be annular; and of these, some have been controverted, as that seen by Clavius at Rome, April 9, 1567; and that seen by Jessenius at Torgaw in Misnia, Feb. 25, 1598; which are both disputed by Kepler. Some astronomers, ancient and modern, have been of opinion, that no eclipse can be annular; and since such seem to have been rarely observed, and Mr. Maclaurin has not met with a particular description of any of them, he gives as full an account of this eclipse as he can collect from the observations that were made at Edinburgh, and those communicated to him from the country.

During the eclipse the sky was generally favourable in the southern parts of Scotland; and though there were great showers of snow in the north, they had sometimes a view of it. There was something very entertaining in the annular appearance, a phenomenon that was equally new to all who saw it.

A little before the annulus was complete, a remarkable point or speck of pale light appeared near the middle of the part of the moon's circumference, that was not yet come upon the sun's disk; and a gleam of light, more faint than this point, seemed to be extended from it to each horn: Mr. M. did not mark the precise time when he first perceived this light, but is satisfied that it could hardly be less than $\frac{1}{4}$ of a minute before the annular appearance began. Mr. Short, who was in another chamber at some distance, and made use of a larger telescope, said that he saw it 20 seconds before the annulus was completed. He was surprised with this light at first, and did not immediately recollect that it proceeded probably from the same crown that was seen about the moon in a total eclipse of the sun at Naples in 1605; and was observed by many in different parts of Europe in the three late total eclipses, of 1706, 1715, and 1724.

Most of those who observed the eclipse with telescopes, mention in their letters, that as the annulus was forming, they perceived the light to break in several irregular spots near the point of contact, and that the moon's limb seemed to be indented there. Some express themselves as if those irregular parts had appeared to them in a kind of motion. Such appearances of a tremulous motion, in certain periods of solar eclipses, are mentioned by Hevelius and others.

The annulus appeared to the eye to be central for some time, but in the telescope it was always broader towards the south-east, than towards the north-west part of the sun's disk. The breadth appeared much greater to the naked eye, than could have been expected from the difference of the semidiameters of the sun and moon. This was so remarkable, that such a phenomenon must have confirmed those astronomers in their opinion, who imagined that the diameter of the moon is contracted in her conjunctions with the sun. This appearance probably proceeded chiefly from the light's encroaching on the shade, as is usual; but whatever was the cause, every body seemed surprized that the moon appeared so small on the disk of the sun.

It was observed, that the motion of the moon appeared more quick in the formation and dissolution of the annulus, than during its continuance. This is particularly described by Mr. Fullarton, of Fullarton, in a very exact account of the eclipse, as it appeared at his seat at Crosby, near Air, on the west coast of Scotland. He writes, that " the annulus appeared to be nearly of an uniform breadth during the greater part of the time of its continuance, but seemed to go off very suddenly; so that when the disk of the moon approached to the concave line of the sun's disk, they seemed to run together like two contiguous drops of water on a table, when they touch one another;" and he adds, that it came on in the same way. This appearance seems to be accountable from the same optical deception as the former.

During the appearance of the annulus, the direct light of the sun was still very considerable; but the places that were shaded from his light appeared gloomy. There was a dusk in the atmosphere, especially towards the north and east. In those chambers that had not their lights westwards, the obscurity was considerable. Venus appeared plainly, and continued visible long after the annulus was dissolved, and other stars were seen by some: one gentleman is positive, that, being shaded from the sun, he discerned some stars northwards, which he thinks by their position were in ursa major.

It was very cold at this time; a little thin snow fell; and some small pools of water in the college area, where there was no ice at 2 o'clock, were frozen at 4. A reflecting telescope of a large size, and of a much greater aperture than ordinary, that took in the whole sun, and burned cloth very suddenly through the tinged glass at the beginning of the eclipse, and on that account could not then be used with safety, was that by which Mr. Short observed the annular appearance. Some curious gentlemen found, that a common burning-glass, which kindled tinder at $3^h\ 59^m$, and burned cloth at $4^h\ 8^m$, had no effect during the annular appearance, and for some time before and after it.

The first internal contact of the disks, at the formation of the annulus, was considerably below the west point of the sun's disk; and the second contact, at the dissolution of the annulus, seemed to be about 10° eastwards from the north point or zenith of the disk. The breadth of the annulus towards the south-east part of the sun's disk, was at least double of its breadth towards the opposite part, about the middle of this appearance. Mr. M. proposed to have made some estimation of the ratio of the continuance of the annular appearance, where it was central, to its continuance at Edinburgh, from that of the arithmetical mean between the numbers that should express the proportion of the greatest and least breath of the annulus, to the geometrical mean between the same numbers; or from the ratio of the radius to the sine of half the arch intercepted between the two points of internal contact; but he did not obtain these ratios with sufficient exactness.

At $3^h\ 31^m\ 43^s$ the annulus was dissolved, after having continued $5^m\ 48^s$; the middle of the eclipse was therefore at $3^h\ 28^m\ 49^s$. In this the time by observation did not agree so well with the time by computation, as in the beginning of the eclipse, the difference being here about 4 minutes. The irregularities of the moon's surface occasioned the same appearances, in some measure, as at the formation of the annulus.

The beginning of the eclipse at............	2^h	5^m	36^s
The beginning of the annular appearance	3	25	55
The end of the annular appearance	3	31	43
The end of the eclipse	4	44	51

At Hopeton-house, 9 miles west, and a little northwards from Edinburgh, Lord Hope observed the annular appearance begin at 3^h 25^m, the end of this appearance at 3^h 31^m, and the end of the eclipse at 4^h $44\frac{1}{2}^m$. His lordship was obliged to observe the eclipse at a distance from the clock, and to determine the times by a pocket watch, that had been adjusted by a very good dial that day at 12 o'clock; but he says, that the duration of the annular appearance was 6^m, as near as could be judged by a watch that did not show the seconds.

At Crosby, on the west coast of Scotland, about 4 miles north from Air, Mr. Fullarton observed the eclipse to begin at 2 o'clock. A distinct annulus was formed about 20^m after 3, which continued exactly 7^m, measured by a pendulum vibrating seconds. It appeared rather broader on the lower verge of the sun; but the difference must have been very small, for it was but barely discernible in a species of the eclipse 6 inches over, cast on a piece of paper behind the eye-piece of a telescope 6 feet long. He adds, that the day light was not greatly obscured, appearing only so much dimmer than usual, as that of the sun is, when seen through a very gentle mist in a fine morning in April or May. Sir Thomas Wallace found that the annular appearance continued at his house, near Lochryan in Galloway, 5^m.

From the observation at Crosby, the centre of the annular penumbra seems to have entered Scotland not far from Irwine. It next proceeded towards the east, with a considerable inclination northwards; and probably left Scotland not far from Montrose on the east coast; for the Rev. Mr. Auchterlony found, that the annular appearance continued there 7^m, as near as he could judge by an ordinary watch. The annulus also appeared to him of a uniform breadth, through a common telescope. This observation, though not so exact as that at Crosby, is however confirmed by that at St. Andrew's, mentioned below. These two observations at Crosby and Montrose, were made nearer the path of the centre, than any others that have been communicated.

As for the southern limit of this appearance, the eclipse was not annular at Newcastle, and there wanted about 40° of the sun's limb to appear in order to form an annulus, according to the observation of Mr. Isaac Thomson, communicated by Mr. Blake, a gentleman of the county of Durham, who was present with us at Edinburgh during our observation. The whole duration of the eclipse was 50^s less by his, than by our observation. Nor was the eclipse annular at Morpeth, whence Mr. John Willson writes, that the body of the moon appeared almost entirely on that of the sun; and that, to the naked eye, the disk of the sun seemed to be almost round.

But of all the observations that have been communicated, that of Mr. Long at Longframlington, 7 computed miles north of Morpeth, determines the southern limit with the greatest exactness. The annulus, he says, was very small there on the upper part, and the duration 40 or 41 half seconds, measured by a pendulum 9.81 inches long; from which we may conclude, that the limit was very near this place. This curious observation, with several others, was communicated by Mr. Mark at Dunbar. At Alnwick, in Northumberland, the eclipse was annular, but the time of its continuance was not measured.

At Berwick, the annular appearance continued between 4 and 5 minutes: the end of the eclipse at Dunbar, by Mr. Mark's observation, was at 4^h 48^m 16^s; but some mistake was committed in reckoning the vibrations of the pendulum, in measuring the continuance of the annulus.

At St. Andrew's, this appearance was observed to continue precisely 6^m, by a pendulum clock, by Mr. Charles Gregory and Mr. David Young, professors in the university. By a figure of the annulus taken from its image, projected through a telescope on a paper screen, the breadth towards the south-east part of the sun's disk, was rather more than double of its breadth towards the opposite part.

The observation at Montrose has been already mentioned. At Aberdeen the annulus was observed by Mr. John Stewart, math. prof. for 3^m 2^s. It was almost central, when the clouds deprived him of any further view of it; he thinks it probable, that it continued there about 6^m. Several gentlemen, residing on the coast northwards from Aberdeen, were desired to observe the continuance of the annulus; but I do not find that any of them saw this phenomenon from the beginning to its end.

At Elgin, the eclipse was observed annular at 3^h 29^m, the larger part of the ring being uppermost, by the Rev. Mr. Irvin, who had a view of it for about 30^s; but by reason of intervening clouds could not determine the beginning or end of this appearance. At Castle Gordon, Mr. Gregory had one view of the eclipse while it was annular, but could make no further observation for the same reason.

At Inverness, the eclipse was annular for some minutes, as observed by several gentlemen; but they did not measure the precise time how long it continued. By the accounts from Fort Augustus and Fort William, it is doubtful whether the eclipse was annular in those places or not. Fort Augustus is at the west end of Lochness, and probably was not far from the northern limit of this phenomenon.

Several gentlemen of very good credit, who are not in the least short-sighted, assure Mr. M. that about the middle of the annular appearance, they were not able to discern the moon on the sun, when they looked without a smoked glass, or something equivalent.

Mr. Maclaurin remarks this, because it may contribute to account for what at first sight appears surprising, that there are so few annular eclipses in the lists collected by authors. Kepler, in his Astron. Optic. does not seem to acknowledge, that any eclipse, truly annular, had ever been observed. There are none mentioned by Ricciolus, from the year 334 till 1567, though there are 13 or 14 total eclipses recorded within that period; yet it is allowed, that the extent and duration of the annular appearance may be considerably greater in the former, than of the darkness in the latter. It may have contributed to this, that annular eclipses must have been rather incident in the winter season in the northern hemisphere, and that eclipses have been more readily total in the summer, when their chance of being visible was greater, and the season more favourable for observing them. But perhaps the chief reason why few annular eclipses appear upon record, is, that they have not been distinguished in most cases from ordinary partial ones. The darkness distinguished total eclipses, or such as were very nearly total; and it is these chiefly, that historians mention. There are two central eclipses of the sun still famous among the populace in this country: that of March 29, 1652, was total here, and that day is known among them by the appellation of Mirk Monday. The memory of the eclipse of Feb. 25, 1598, is also preserved among them, and that day they term, in their way, Black Saturday. There is a tradition, that some persons in the north lost their way in the time of this eclipse, and perished in the snow.

There was a remarkable total eclipse of the sun in this country, June 17, 1433, the memory of which is now lost among the populace; but it appears from a passage in a manuscript in our library, that it was formerly called by them the Black Hour, after their usual manner. It is described thus: "Hoc anno fuit mirabilis Eclipsis Solis, 17mo die mensis Junii, hora quasi tertia post meridiem; et per dimidium horæ tenebræ tanquam in nocte supergressæ sunt superficiem terræ, ita ut nihil obtutibus humanis pervium fuit; unde abhinc vulgariter dicta fuit hora nigra." This eclipse is not in Ricciolus's Catalogue, but is mentioned by him in another place, Schol. cap. 2, l. 5. By a computation of this eclipse, the sun was within 2° of his apogeum, and the moon within 13° of her perigeum; so that this must have been a remarkable eclipse.

The progress of the shadow was towards the south-east; and Sethus Calvisius cites the Turkish annals for its being total in some part of their dominions.

You will perceive by this account, that we have no observatory in this place; but we are in hopes that some time or other we shall obtain one from the patrons of the university. I doubt this last eclipse will not be distinguished by any particular appellation among the populace, as the former that were central in this country. The remembrance of it however will be preserved by the curious, who observed it with great pleasure, and agree that it was the most entertaining spectacle of this kind they ever saw.

4. The same observed at Edinburgh. By Sir John Clerk, Bart. F.R.S. p. 195.

The eclipse began at $5^m\ 36^s$ after 2. The annular appearance began at 25^m 55^s after 3; its continuation was $5^m\ 48^s$. The end of the eclipse was at $44^m\ 50^s$ after 4; all reckoned by apparent time. We had half a score good reflecting telescopes to make these observations, and our calculations perfectly agreed, so that you may depend upon them as most exact.

This was not done by us as a matter of mere curiosity, but to assist in ascertaining the motions of the moon, on Sir Isaac Newton's theory, on which a good deal of the doctrine of the longitude will depend. Sir Isaac's calculation, as to the beginning of this eclipse, was pretty right; but not so well as to its central appearance Two spots in the sun made a very distinct appearance to us, as they entered under the moon's body; one was a little above the central or horizontal line of the sun; the other was near the edge, on the east quarter. The first, by comparison with the sun's diameter, was larger than the disk of our earth; it was dark in the middle, and certainly emitted no fire or light. The edge of the moon appeared a little ragged or rough, but not mountainous, because of the sun's light. There was no considerable darkness, but the ground was covered with a kind of a dark greenish colour. Two stars appeared, the planet Venus, and another farther eastward. This account is what you may depend on.

5. The same Eclipse observed at Cambridge, and Kettering; p. 197.

The beginning by the clock	at	2^h	36^m	40^s	
The end	at	5	14	12	Exact.

Times observed at Kettering, as follow:

Beginning	2^h	21^m
2 Digits	2	36
Centre .... {	3	7
	4	22
End	4	59

N. B. The observatory clock was 1 minute 50 seconds too slow, which being added all the way, will give true time.

6. The same observed at the Institute of Bononia. p. 199.

The eclipse began at $3^h\ 33^m\ 35^s$, being more than 7 minutes sooner than the calculation made it.

7. The same observed at the Aventine Hill at Rome. By the Abbè de Revillas, p. 200.

The beginning there was at $3^h\ 43^m$.

8. The same Eclipse observed at Wittemberg. By J. F. Weidler. p. 201.

Neither the beginning nor the end was seen; only some digits were observed on the decrease; particularly 8 digits were eclipsed at $4^h\ 50^m\ 31^s$.

A Proposal to make the Poles of a Globe of the Heavens move in a Circle round the Poles of the Ecliptic. By the Rev. Ebenezer Latham, M. D. and V. D. M. N° 447, p. 201.

As we now have the globes of the heavens, they are only formed for the present age, and do not serve the purposes of chronology and history, as they might, if the poles on which they turn were contrived to move in a circle round those of the ecliptic, according to the present obliquity of this. By this means we might have a view of the heavens suited to every period, and that would answer the ancient descriptions, those of Eudoxus, for instance, who is supposed to borrow his from the most early observations; and of Hipparchus, &c. Nor could any contrivance better enable the lowest reader to judge of the merits of the controversy about the Argonautic expedition, as far as it depends on this: for it will verify to the sight the path of the colours, &c. at any time.

N. B. That globes, to answer the end here proposed, though differently constructed, had long before been made and published by Mr. Senex, who at the next meeting of the R. S. gave the following account of his contrivance.

A Contrivance to make the Poles of the Diurnal Motion in a Celestial Globe pass round the Poles of the Ecliptic. Invented by John Senex, F. R. S.* N° 447, p. 203.

The poles of the diurnal motion do not enter into the globe, but are affixed

* Mr. Senex, F. R. S. was a bookseller, and a celebrated maker of globes and planispheres, &c. He died Dec. 30, 1741.

at one end, to two shoulders or arms of brass, at the distance of 23° and $\frac{1}{2}$ from the poles of the ecliptic. These shoulders at the other end are strongly fastened on to an iron axis, which passes through the poles of the ecliptic, and is made to move round, but with a very stiff motion; so that when it is adjusted to any point of the ecliptic, which you desire the equator may intersect, the diurnal motion of the globe on its axis will not be able to disturb it.

When it is to be adjusted for any time, past or to come, bring one of the brazen shoulders under the meridian, and holding it fast to the meridian with one hand, turn the globe so about with the other, that the point of the ecliptic, which you would have the equator to intersect, may pass under no degrees of the brazen meridian: then holding a pencil perpendicular to that point, and turning the globe about, it will describe the equator as it was posited at that time; and transferring the pencil to $23\frac{1}{2}°$, and $66\frac{1}{2}°$ on the brazen meridian, the tropics and polar circles will be described for the same time.

By this contrivance, the celestial globe may be so adjusted, as to exhibit not only the risings and settings of the stars, in all ages, and in all latitudes, but the other phænomena likewise, that depend on the motion of the diurnal axis round the annual axis.

The Solution of Kepler's Problem, by J. Machin, Sec. R. S. N° 447, p. 205.

Many attempts have been made at different times, but Mr. Machin thinks never any yet with tolerable success, towards the solution of the problem proposed by Kepler: to divide the area of a semicircle into given parts, by a line from a given point of the diameter, in order to find a universal rule for the motion of a body in an elliptic orbit. For among the several methods offered, some are only true in speculation, but are really of no service. Others are not different from his own, which he judged improper: and as to the rest, they are all some way or other so limited and confined to particular conditions and circumstances, as still to leave the problem in general untouched. To be more particular; it is evident, that all constructions by mechanical curves are seeming solutions only, but in reality unapplicable; that the roots of infinite serieses are, on account of their known limitations in all respects, so far from affording an appearance of being sufficient rules, that they cannot well be supposed as offered for any thing more than exercises in a method of calculation. And then, as to the universal method, which proceeds by a continued correction of the errors of a false position, it is, when duly considered, no method of solution at all in itself; because, unless there be some antecedent rule or hypothesis to begin the operation, (as suppose that of a uniform motion about the upper focus, for the orbit of a planet; or that of a motion in a parabola for the perihelion part of the orbit of a comet, or some other such) it would be impossible to proceed one step in it. But as no general rule has ever yet been laid down, to assist this method, so as to make it always operate, it is the same in effect as if there were no method at all. And accordingly in experience it is found, that there is no rule now subsisting but what is absolutely useless in the elliptic orbits of comets; for in such cases there is no other way to proceed, but that which was used by Kepler: to compute a table for some part of the orbit, and therein examine if the time to which the place is required, will fall out anywhere in that part. So that, on the whole, it appears evident, that this problem, contrary to the received opinion, has never yet been advanced one step towards its true solution: a consideration which will furnish a sufficient plea for meddling with a subject so frequently handled; especially if what is offered shall at the same time appear, as he trusts it will, to contribute towards supplying the main defect.

LEMMA I.—*The Tangent of an Arch being given, to find the Tangent of its Multiple.*—Let r be the radius of the circle, t the tangent of a given arch A, and n a given number. And let T be the tangent of the multiple arch $n \times$ A, to be found.

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

The tangent T will be $\dfrac{\overline{r+\tau}|^n - \overline{r-\tau}|^n}{\overline{r+\tau}|^n + \overline{r-\tau}|^n}\varrho$:

Which binomials being raised according to Sir Isaac Newton's rule, the fictitious quantities τ and ϱ will disappear, and the tangent T will become equal to

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

This theorem, which he formerly found for the quadrature of the circle, at a time when it was not known here to have been invented before, has now been common for many years; for which reason it is premised at present, without any proof; only for the sake of some uses that have not yet been made of it.

Corol. 1. From this theorem for the tangent, the sine, suppose Y, and cosine Z of the multiple arch $n \times$ A, may be readily found.

For if y be the sine, and z the cosine of the given arch A, then putting vv for $-\,yy$, and substituting $\frac{ry}{z}$ for t, and $\frac{rv}{z}$ for τ, and $\frac{r\mathrm{T}}{\sqrt{rr+\mathrm{TT}}}$ for Y:

The sine Y will be $\dfrac{\overline{z+v}|^n - \overline{z-v}|^n}{2r^n}\varrho$.

The cosine Z will be $\dfrac{\overline{z+v}|^n + \overline{z-v}|^n}{2r^{n-1}}$.

Each of these may be expressed differently in a series, either by the sine and cosine conjointly, or by either of them separately.

Thus Y the sine of the multiple arch $n \times A$, may be in either of these two forms, viz.

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

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

Where the letters A, B, C, &c. stand, as usual, for the co-efficients of the preceding terms.

The first of these theorems terminates when n is any integer number; the other, which is Sir Isaac Newton's rule, and is derived from the former by substituting $\sqrt{rr-yy}$ for z, terminates when n is any odd number.

The cosine z may, in like manner, be in either of these two forms, viz.

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

$$\text{or} = r - \frac{nn}{2rr} \text{A} y^2 - \frac{nn-4}{3.4rr} \text{B} y^4 - \frac{nn-16}{5.6rr} \text{C} y^6 - \text{\&c.}$$

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

Corol. 2. Hence the sine, cosine, and tangent of any submultiple part of an arch, suppose $\frac{1}{n}$ A, may be determined thus:

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

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

For these equations will arise from the transposition and reduction of the former, for the tangent and sine of the multiple arch, on the substitution of t, y, z and A; for T, Y, Z and $n \times$ A.

Corol. 3. Hence regular polygons of any given number of sides may be inscribed within, or circumscribed without, a given arch of a circle. For if the number n express the double of the number of sides to be inscribed within, or circumscribed about, the given arch A; then one of the sides inscribed will be the double of the sine, and one of the sides circumscribed the double of the tangent of the sub-multiple part of the arch, viz. $\frac{1}{n}$ A.

LEMMA II.—*To find the Length of the Arch of a Circle within certain Limits, by means of the Tangent and Sine of the Arch.*—Let t be the tangent, y the sine, and z the cosine of the arch A, whose length is to be determined; and let ϱ, τ, v be expounded as before; then, if any number n be taken, the arch of the circle will be

always less than $\dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} \times n\varrho$,

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

For if, by the preceding Corollaries, a regular rectilinear polygon be inscribed within, and another without, the arch A, each having half as many sides as is expressed by the number n; then will the former of these quantities be the length of the bow of the circumscribed polygon, or the sum of all its sides, which is always greater, and the latter will be the length of the bow of the inscribed polygon, which is always less, than the arch of the circle: how great soever the number n be taken.

Corol. 1. Hence the serieses for the rectification of the arch of a circle may be derived.

For by converting the binomials into the form of a series, that the fictitious quantities, ϱ, τ, v may be destroyed; it will appear, that no number n can be taken so large as to make the inscribed polygon so great, or the circumscribed so little, as the series

$\dfrac{ry}{z} - \dfrac{ry^3}{3z^3} + \dfrac{ry^5}{5z^5} - \dfrac{ry^7}{7z^7} +$ &c. in one case, or its equal

$t - \dfrac{t^3}{3r^2} + \dfrac{t^5}{5r^4} - \dfrac{t^7}{7r^6} +$ &c. in the other case.

Therefore, since the quantity denoted by the sum of the terms, in either of these serieses, is always greater than any inscribed polygon, and always less than any circumscribed, it must therefore be equal to the arch of the circle.

Corol. 2. If, in the first of the above serieses, the root $\sqrt{rr - yy}$, be extracted, and substituted for z, there will arise the other series of Sir Isaac Newton, for giving the arch from the sine; namely,

$y + \dfrac{y^3}{6r^2} + \dfrac{3y^5}{40r} + \dfrac{5y^7}{112r^6} +$ &c. or otherwise,

$= y + \dfrac{1}{1.2.3} \times \dfrac{y^3}{r^2} + \dfrac{3.3}{1.2.3.4.5.} \times \dfrac{y^5}{r^4} + \dfrac{3.3.5.5.}{1.2.3.4.5.6.7.} \times \dfrac{y^7}{r^6} +$ &c.

Schol. In like manner, as the arches of the polygons serve to determine the arch of the circle, so by comparing the areas of the circumscribed and inscribed polygons, $\frac{1}{2}nr\text{T}$ and $\frac{1}{2}n\text{YZ}$, the area of the sector of a circle may be found. For if T, Y and Z be the tangent, sine and cosine of the arch A; then, by the second Lemma, the area of the circumscribed polygon

will be found to be $\frac{1}{2}nr\varrho \times \dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} = \frac{1}{2}nr\text{T}$.

and the area of the inscribed will appear to be

$$\tfrac{1}{2}n\varrho \times \frac{\overline{z+v}|^{\frac{2}{n}} - \overline{z-v}|^{\frac{2}{n}}}{4r^{\frac{2}{n}-1}} = \tfrac{1}{2}n\text{YZ}.$$

But on the expansion of these binomials it will appear, that no number n can be taken so large as to make the one so large, or the other so small, as the area denoted by the series.

$$\tfrac{1}{2}r \text{ in } t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \text{\&c.}$$

So that this area being larger than any inscribed, and smaller than any circumscribed polygon, must be equal to the area of the sector.

It may further be observed, that as the arch or area is found from the sine, cosine, or tangent of the arch, by means of the limiting polygons, so may the sine, cosine or tangent be found from the length of the arch, by the same method.

Thus, if A be the arch, whose tangent T, sine Y, and cosine Z, are to be determined, then will the

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

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

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

For it may be made to appear, from the first Lemma, and its Corollaries, that if in any of these theorems, as suppose in the first, the quantity A stand for the bow of the circumscribed polygon, then will the quantity T, exhibited by the theorem, be always larger; but if for the bow of the inscribed, always less than the tangent of the arch, how great soever the number n be taken; and consequently, if A stand for the length of the arch itself, the quantity T must be equal to the tangent; and the like may be shown for the sine, and, mutatis mutandis, for the cosine.

These principles, from whence he has here derived the quadrature of the circle, which is wanted in the solution of the problem in hand, happen to be, on another account, absolutely requisite for the reduction of it to a manageable equation. But he has enlarged, more than was necessary to the problem itself, on the uses of this sort of quadrature by the limiting polygons, because it is one of that kind which requires no other knowledge but what depends on the common properties of number and magnitude; and so may serve as an instance to show, that no other is requisite for the establishment of principles for arithmetic and geometry. A truth which, though certain in itself, may perhaps seem doubtful from the nature and tendency of the present inquiries in mathematics. For, among the moderns, some have thought it necessary, for investigating the relations of quantities, to have recourse to very hard hypotheses; such as that of number infinite and indeterminate; and that of magnitudes in statu fieri, existing in a potential manner, which are actually of no magnitude. And others, whose names are truly to be reverenced on account of their great and singular inventions, have thought it requisite to have recourse even to principles foreign to mathematics, and have introduced the consideration of efficient causes, and physical powers, for the production of mathematical quantities; and have spoken of them, and used them, as if they were a species of quantities by themselves.

N. B. In the following proposition Mr. Machin has, for the sake of brevity, made use of a peculiar notation for composite numbers, or such quantities as are analogous to them, whose factors are in arithmetical progression.

The quantity expressed by this notation has a double index: that at the head of the root at the right-hand, but separated by a hook to distinguish it from the common index, denotes the number of factors; and that above, within the hook on the left hand, denotes the common difference of the factors proceeding, in a decreasing or increasing arithmetical progession.

Thus the quantity $\frac{\overset{\alpha}{\smile}}{n+a}(^{m}$ denotes, by its index m on the right hand, that it is a composite quantity, consisting of so many factors as there are units in the number m; and the index α above, on the left, denotes the common difference of the factors, decreasing in an arithmetical progression, if it be positive; or increasing, if it be negative; and so signifies, in the common notation, the composite number or quantity, $n+a.n+a-\alpha.n+a-2\alpha.n+a-3\alpha.$ and so on.

For example: $\frac{\overset{2}{\smile}}{n+5}(^{6}$ is $= n+5.n+3.n+1.n-1.n-3.n-5.$ consisting of six factors whose common difference is 2. After the same manner $\frac{\overset{2}{\smile}}{n+4}(^{5}$ is $= n+4.n+2.n.n-2.n-4$, consisting of five factors. According to which method it will easily appear, that if a be any integer, then $\frac{\overset{2}{\smile}}{n+2a+1}(^{2a+2}$ will be $= nn-1.nn-9.nn-25$, continued to such a number of double factors, as are expressed by $a+1$, or half the index, which in this case is an even number. So $\frac{\overset{2}{\smile}}{n+2a}(^{2a+1}$ will be equal to $n.nn-4.nn-16.nn-36$, and so on, where there are to be as many double factors as with one single one (n) will make up the index $2a + 1$, which is an odd number.

If the common difference α be an unit, it is omitted:
Thus, $\breve{n}(^6$ is $= n.\, n - 1.\, n - 2.\, n - 3.\, n - 4.\, n - 5$, containing six factors.
So $\breve{6}(^6$ is $= 6.\, 5.\, 4.\, 3.\, 2.\, 1$, and the like for others.

If the common difference α be nothing, then the hook is omitted, and it becomes the same with the geometrical power: so $\overset{0}{\underset{n+a}{\smile}}(^m$ is $= \overline{n+}|^m$ according to the common notation.

Proposition 1.—*An arch less than a semicircle being given, with a point in the diameter passing through one of its extremities; to find by means of the sine of a given part of the arch less than one half, the area of the sector subtended by the given arch, and comprehended in the angle made at the given point.*—Let PNA, fig. 7, pl. 6, be a semicircle described on the centre C, and diameter AP, and let PN be the given arch less than a semicircle, and S the given point in the diameter AP, passing through one extremity of the arch NP in P. Then taking any number n greater than 2, let PK be an arch in proportion to the given arch PN, as unity to the number n; and let it be required to find, by means of the sine of the arch PK, the area of the sector NSP, subtended by the given arch NP, and comprehended in the angle NSP made at the given point S.

From N and K let fall, on the diameter AP, the perpendiculars NM and KL, and join CN and CK. Then let t stand for CP, the semidiameter of the circle; f for CS, the distance of the given point S from the centre; p for SP, the distance of it from the extremity of the arch, through which the diameter AP passes; and y for KL, the sine of the arch KP in the given circle.

These substitutions being presupposed, the problem is to be divided into two cases; one when SP is less, and the other when it is greater than the semidiameter CP.

Case 1.—If SP be less than CP, then take an area H equal to the sum of the rectangles expressed by the several terms of the following series continued ad libitum:

$$\frac{py}{1} + \frac{t + \overset{2}{\overline{\smile n + 1}}|^2 \times f}{\breve{3}|^3} \times \frac{y^3}{t^2} + \frac{9t - \overset{2}{\overline{\smile n + 3}}|^4 \times f}{\breve{5}|^5} \times \frac{y^5}{t^4} + \frac{9 \times 25t + \overset{2}{\overline{\smile n + 5}}|^6 \times f}{\breve{7}|^7} \times \frac{y^7}{t^6} + \&c.$$

And the area $\frac{1}{2}n \times$ H will determine the area of the sector NSP ad libitum.

For the sector PSN, being the excess of the sector NCP above the triangle NCS, will be the difference of two rectangles: $\frac{1}{2}$CP $\times$ PN $- \frac{1}{2}$CS $\times$ NM; but PN is the multiple of the arch PK, namely $n \times$ PK; and NM is the sine of that multiple arch; therefore if for CP be put t, for CS, f, according to the supposition; and if for PK be substituted $\frac{y}{1} + \frac{1}{3|^{3}} \times \frac{y^3}{t^2} + \frac{9}{5|^{5}} \times \frac{y^5}{t^4} + \frac{9 \times 25}{7|^{7}} \times \frac{y^7}{t^6} +$ &c. by cor. 2, lem. 2; and for NM

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

according to cor. 1, lem. 1, the area of the sector will appear in a series, as is above determined.

But since the number n is greater than 2, and the given arch PN is less than a semicircle, and consequently KL or y, the sine of the submultiple arch PK, is less than the semidiamer CP or t; it may thence be easily proved, that the series will approximate to the just quantity of the area, ad libitum.

Corol. 1.—Hence, if the number n be taken equal to $\sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$, the sector NSP will be $= \frac{1}{2}npy + \frac{n^3t - n.nn - 1.p}{12tt} y^3 + **** + \frac{n^3}{1120t^5} y^7 +$ &c.

For the numerator of the coefficient of the third term in the series, that determines the area H, namely, $9t - \overset{2}{\overline{n+3}}|^{4} \times f$, is equal to $9t - nn - 1.nn - 9.f$, which, according to the above determination of the number n, will become nothing; therefore, if for $t - p$ be put f in the second term, and the value of n be substituted for n in the third and fourth, the series for the area will appear on reduction to be as is here laid down.

Corol. 2.—Hence the area of the sector NSP may be always defined nearly by the terms of a cubic equation.

For the number n, as constructed in the former corollary, is always greater than the square root of 10, and consequently $\frac{y}{t}$ is always less than the sine of one-third part of the given arch; so that the fourth term $\frac{n^3}{1120t^5} y^7$, with the sum of all the following terms of the series, can never be more than a small part of the whole sector.

Corol. 3.—If R stand for 57,2957795 &c. degrees, or the number of degrees contained in an angle subtended by an arch of the same length with the radius of the circle, and M be the number of degrees in an angle which is to four right angles, as the area NSP to the area of the whole circle; then will M be $= \frac{np}{t} \times \frac{Ry}{t} + \frac{n^3t - n.nn - 1.p}{6t} \times \frac{Ry^3}{t^3}$, nearly.

For $\frac{M}{R} \times \frac{tt}{2}$ will appear, by the construction, to be equal to the sector NSP.

Case 2.—If SP be greater than CP, then take an area H equal to the sum of the terms in the following series:

$$\frac{py}{1} + \frac{t - \overset{2}{\overline{n+1}}|^{2} \times f}{\breve{3}|^{3}} \times \frac{y^3}{t^2} + \frac{9t + \overset{2}{\overline{n+3}}|^{4} \times f}{\breve{5}|^{5}} \times \frac{y^5}{t^4} + \frac{9 \times 25t - \overset{2}{\overline{n+5}}|^{6} \times f}{\breve{7}|^{7}} \times \frac{y^7}{t^6} + \&c.$$

and the area $\frac{1}{2}n \times$ H, will be the sector, as before.

For the point S being on the contrary side of the centre, to what it was before, it will easily appear, that the change of $+f$ into $-f$, must reduce one case to the other, without any other proof.

Corol.—Hence, if the number n be taken equal to $\sqrt{\frac{t+f}{f}}$, or in this case $\sqrt{\frac{p}{f}}$, then the series for the sector will want the second term, as in the former it wanted the third.

Definition.—The angle called by Kepler the anomalia eccentri, is a fictitious angle in the elliptic orbit of a planet, being analogous to the area described by a line from the centre of the orbit, and revolving with the planet from the line of apsides; in like manner as the mean anomaly is a fictitious angle, analogous to the area described by a line from the focus.

Otherwise, if C be the centre, S the focus of an elliptic orbit described on the transverse axis AP, and the area NSP in the circle be taken in proportion to the whole, as the area described in the ellipsis about the focus, to the whole: then is the arch of the circle PN, or the angle NCP, that which Kepler calls the anomalia eccentri.

This angle may be measured either from the aphelion, or from the perihelion: in the following proposition it is supposed to be taken from the perihelion.

PROPOSITION 2.—*The mean anomaly of a comet or planet, revolving in a given elliptic orbit, being given; to find the anomalia eccentri.*—The solution of this problem requires two different rules; the first and principal one serves to make a beginning for a further approximation, and the other is for the progression in approximating nearer and nearer ad libitum.

1. The rule for the first assumption: let t, f, and p, stand, as before, for the semi-transverse axis of the ellipsis, the semi-distance of the foci, and the perihelion distance; then taking the number n equal to $\sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$, let T stand for $\frac{2t}{nnt - nn - 1 . p}$; and P for $\frac{2p}{nnt - nn - 1 . p}$, or $\frac{p}{t}$T; which constant numbers, being once computed for the given orbit, will serve to find the angle required nearly by the following rule.

Let M be the number of degrees in the angle of mean anomaly to the given time, reckoned from or to the perihelion; and supposing R, as before, to stand for 57,2957 &c. degrees; take the number N $= \sqrt[3]{\frac{3\text{T}}{n\text{R}}}$ M, and let A be the angle whose sine is $\text{N}\sqrt[3]{\frac{1}{2}+\sqrt{\frac{1}{4}+\frac{\text{P}^3}{\text{N}^6}}}+\text{N}\sqrt[3]{\frac{1}{2}-\sqrt{\frac{1}{4}+\frac{\text{P}^3}{\text{N}^6}}}$; then the multiple angle $n \times \text{A}$, will be nearly equal to the anomalia eccentri.

The truth of which will appear from the resolution of the cubic equation, in the last corollary to the preceding proposition.

Corol. 1.—If the quadruple of the quantity $\frac{\text{P}^3}{\text{N}^6}$ be many times greater or many times less than unity; or, which amounts to the same, if the mean anomaly N, be many times less, or many times greater, than the angle denoted by the given quantity $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, one or the other of which two cases most frequently happens in orbits of very large eccentricity; then the theorem will be reduced to a simpler form, near enough for use.

Case 1.—If M be many times less than $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, then the angle A may be taken for that whose sine is $\frac{t \times \text{M}}{np \times \text{R}}$.

Case 2.—If M be many times greater than $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, then let A be the angle whose sine is $\text{N} - \frac{\text{P}}{\text{N}}$; and the multiple angle $n \times \text{A}$, according to its case, will be nearly equal to the angle required.

Corol. 2.—In orbits of very large eccentricity, the perihelion distance p is many times less than the semi-distance of the foci f, and the number $n = \sqrt{5+\sqrt{25+\frac{9p}{f}}}$; is always nearly equal to $\sqrt{10}$, or to the integer 3, either of which may be used for it, without any material error in the orbits of comets.

2. The rule for a further correction ad libitum.

Let M be the given mean anomaly, t the semi-transverse axis, as before; and let B be equal to, or nearly equal to, the multiple angle $n \times \text{A}$, before found; then if μ be the mean anomaly, and x the planet's distance from the sun, computed to the anomalia eccentri B; the angle B taken equal to $\text{B} + \frac{t}{x} \times \overline{\text{M} - \mu}$, will approach nearer to the true value of the angle sought; and by repetitions of the same operation, the approximation may be carried on nearer and nearer, ad libitum.

This last rule being obvious, the explication of it may be omitted at present.

Scholium.—In this solution, where the motion is reckoned from the perihelion, the rule is universal, and under no limitation. But had the motion been taken from the aphelion, the problem must have been divided into two cases: one is, when the eccentricity is less than $\frac{9}{16}$; the other is, when it is not less, but is either equal to, or more than in that proportion.

If the eccentricity be not less than $\frac{9}{16}$, then the same rule will hold, as before, only putting the aphelion distance, suppose a instead of the perihelion distance p, and substituting $-f$ for $+f$ in the rule for the number n.

If the eccentricity be less than $\frac{9}{10}$, then take the number n equal to $\sqrt{\frac{a}{f}}$, and $\frac{t}{na} \times \frac{\text{M}}{\text{R}}$ will be nearly equal to the sine of the submultiple part of the anomalia eccentri, denominated by the number n, as before.

It is needless to observe, that the like rules would obtain in hyperbolic orbits, mutatis mutandis. But that which perhaps may not appear unworthy of being remarked, concerning this sort of solution from the cubic root, is, that though the rule be altogether impossible, on a total change of the figure of the orbit, either into a circle, or into a parabola; yet it will operate so much better, and stand in need of less correction, according as the figure advances nearer, in its change, towards either of those two forms.

That the use of the method may better appear, it may not be amiss to add a few examples.

The following are two for the orbits of planets, one the most, and the other the least eccentric; but which are more to show the extent of the rule, than to recommend the use of it in such cases; for there are many other much better, and more expeditious methods, in orbits of small eccentricity. The other two examples are adapted to the orbits of two comets, whose periods have been already discovered by Dr. Halley; the one is to show the use of one of the rules in the first corollary, and the other is to explain the use of the other rule.

Example 1.—*For the Orbit of Mercury.*—If an unit be put for the semi-transverse axis t, the eccentricity 0,20589 will become f, and the perihelion distance p will be 0,79411; therefore by means of the number R, given as before, the constant numbers for this orbit will appear to be, $n = 3,56755$, $\text{T} = 0,5857271$, $\text{P} = \frac{p}{t}\,\text{T} = 0,4651319$, and hence $\frac{3\text{T}}{n \times \text{R}} = 0,0085965$.

Example.—Suppose M, the mean anomaly from the perihelion, to be 120° 00′ 00″, to which it is required to find the anomalia eccentri.

Here, since the mean anomaly M is not many times more than the limiting angle $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, which in this orbit is about 74°, recourse must be had to the general rule in the proposition.

The number N then, which is $\sqrt[3]{\frac{3\text{T}}{n\text{R}}\text{M}}$, will be $= 1,0104195$; which, found, gives $\text{N}\sqrt[3]{\frac{1}{2} + \sqrt{\frac{1}{4} + \frac{\text{P}^3}{\text{N}^6}}} = 1,0389090$; and also $\text{N}\sqrt[3]{\frac{1}{2} - \sqrt{\frac{1}{4} + \frac{\text{P}^3}{\text{N}^6}}} = -0,4477126$. Therefore the sum of both, under their proper signs, viz. 0,5911964, will be the sine whose arch 36°,24195 is the angle A; the multiple of which $n \times \text{A} = 129°,295503$, will be the angle to be first assumed for the anomalia eccentri.

For a further correction; this angle, now called B, whose sine is suppose y, and its cosine z, gives, by a known rule, $t + \frac{f}{t}z = 1,1304$, for x the planet's distance from the sun; and by another known rule $\text{B} - \frac{f\text{R}}{tt}y = 120°,16568$, for μ the mean anomaly to the anomalia eccentri B. Therefore the correct angle $\text{B}_2 = \text{B} + \frac{t}{x} \times \overline{\text{M} - \mu}$, will be $129°,14846 = 129°8'54'',5$, erring, as will appear from a further correction, about $\frac{1}{10}$ of a second.

This angle, being thus determined, will give by the common methods, $137° 48' 33\frac{1}{2}''$, for the true anomaly, or angle at the sun: the sine of the true anomaly being in proportion to the sine of the anomalia eccentri, as the semi-conjugate axis, to the planet's distance from the sun. So that the equation of the centre in this example is $17° 48' 33\frac{1}{2}''$.

Example 2.—*For the Orbit of Venus.*—Supposing, as before, the mean distance t to be unity, and the eccentricity f to be 0,0069855; the constant numbers for this orbit will be, $p = 0,9930115$; $n = 6,4116$; $\text{T} = 1,562134$; $\text{P} = 0,1551217$; $\frac{3\text{T}}{n\text{R}} = 0,0127571$; and the limiting angle, $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, will appear to be about 303 degrees.

Example.—Let M be 120° 00′ 00″, as in the former example. Then, since the mean anomaly is, in this case, not many times less than the limiting angle, the general rule must be used as before; according to which the number N will appear to be 1,152585; the sine of A will be 0,3217917; the angle A, 18°,77132; and the multiple $n \times \text{A}$, or angle B, for the first assumption of the anomalia eccentri, will be 120°,35416.

This angle B will give, by the method before explained, the angle B = 120°,34555, or 120° 21′ 44″ ferè, for the anomalia eccentri correct; the error of which will appear, on examination, to be but a small part of a second.

In this example, the true anomaly is 120° 41′ 25″,1; and consequently the equation of the centre no more than 41′ 25″,1.

Example 3.—*For the Orbit of the Comet of* 1682.—To know the mean anomaly of this comet, to any given time, it is to be premised, that it was at the perihelion in the year 1682, on Sept. 4, at $21^{\text{h}}\ 22^{\text{m}}$, equated time to the meridian of Greenwich, and makes its revolution about the sun, as Dr. Halley has discovered, in $75\frac{1}{2}$ years.

The perihelion distance p is, according to his determination, 0,0326085 parts of the mean distance t. So that the constant numbers for the orbit will be, $n = 3,1676061$; $\text{T} = 0,2054272$; $\text{P} = 0,00669867$; and the limiting angle, $\frac{2np}{3t}\text{R}\sqrt{\text{P}}$, will be about 19′, or $\frac{1}{3}$ of a degree.

In the orbits of comets, the rule for the first assumption of the anomalia eccentri, is generally sufficient without correction.

Thus, suppose the mean anomaly M to be 0,072706, (as it was at the time of an observation made at Greenwich on August 30, 1682, at $7^h\ 42^m$ eq. T,) then the general rule (which must be here used, since the angle of mean anomaly is not above 4 or 5 times less than the limiting angle) will give $n \times$ A or B $= 2^\circ\ 12'\ 48'',7$, erring about $\frac{7}{10}$ of a second from the true anomalia eccentri.

But in these orbits, the rules in the first corollary to the second proposition most frequently take place, especially the last; and the calculation may also be further abbreviated, by putting the square root of 10, or the integer 3, for the number n.

Example.—Suppose the mean anomaly to be $0^\circ,006522$, or $23'',4792$: here, since M is 50 times less than the limiting angle, the rule in the first case of the first corollary may be used; that is, to take the sine of the angle A $= \frac{t \times M}{np \times R}$.

Therefore, if the number 3 be put for n, the sine of A, which is $\frac{tM}{3pR}$, will be $= 0,00116367$; and consequently the angle A will be $4'\ 0'',011$; and the multiple angle $n \times$ A, to be assumed for the anomalia eccentri, will be $12'\ 0'',033$, the error of which will be found to be about $\frac{1}{30}$ of a second.

EXAMPLE 4.—*For the Orbit of the great Comet of the Year* 1680.—This comet, according to Dr. Halley, performs its period in 575 years; and was in its perihelion on Dec. 7, 1680, at $23^h\ 09^m$ eq. T. at London; the perihelion distance p is 0,000089301, in parts of the mean distance t: therefore supposing the number n to be $\sqrt{10}$, the constant numbers for the orbit will be T $= 0,2000161$; P $= 0,000017862$, and the limiting angle $\frac{2np}{3t} R\sqrt{P}$, will be about $\frac{1}{6}$ of a second.

Example.—Suppose the mean anomaly to be $3'\ 31'',4478$, or $0^\circ,05873541$, (as it was at the time of the first observation made on it in Saxony, on Nov. 3, at $16^h\ 47^m$ eq. T, at London,) here, since the mean anomaly is many times greater than $\frac{1}{6}$ of a second, the rule in the second case of the first corollary may be used; that is, by taking the sine of A $= N - \frac{P}{N}$.

But the number N or $\sqrt[3]{\frac{3T}{nR}M}$ is $= 0,05794134$; and $\frac{P}{N}$ will be $= 0,0030827$; therefore ($N - \frac{P}{N} =$) 0,05763307, will be the sine whose arch $3^\circ,30397$ is the angle A; and the multiple angle $n \times$ A $= 10^\circ\ 26'\ 53'',05$, will be the angle to be first assumed for the anomalia eccentri; the error of which will be found to be less than a second.

The true anomaly, computed from this angle according to the rule in the example for Mercury, will appear to be $171^\circ\ 38'\ 24''$, from the perihelion.

By these examples it appears, that the solution is universal in all respects; for the first two, compared with the last two, serve to show that it is not confined to any particular parts of the orbit, but extends to all degrees of mean anomaly: and by comparing the second with the last, it sufficiently appears to be universal with respect to the several degrees of eccentricity; since in one the equation of the centre, for the reduction of the mean to the true motion, is not so much as the 170th part of the whole; whereas in the other it amounts to almost 3000 times as much as the mean motion itself.

Postscript.—On reviewing the reflections on the quadrature of the circle, Mr. M. believes it may be necessary, to prevent any mistake that may arise from the different opinions that obtain about the nature of mathematical quantity, to explain himself a little on that head; as also to add a few words to show how the method of quadrature, by limiting polygons, takes place in other figures, as well as the circle.

He takes then a mathematical quantity, and that for which any symbol is put, to be nothing else but number with regard to some measure which is considered as one. For we cannot know precisely and determinately, that is, mathematically, how much any thing is, but by means of number. The notion of continued quantity, without regard to any measure, is indistinct and confused; and though some species of such quantity, considered physically, may be described by motion, as lines by points, and surfaces by lines, and so on; yet the magnitudes or mathematical quantities are not made by that motion, but by numbering according to a measure.

Accordingly, all the several notations that are found necessary to express the formations of quantities, refer to some office or property of number or measure; but none can be interpreted to signify continued quantity, as such.

Thus some notations are found requisite to express number in its ordinal capacity, or the numerus numerans, as when one follows or precedes another, in the first, second or third place, from that on which it depends; as the quantities $\grave{\grave{x}}$, $\grave{x}$, x, $\acute{x}$, $\acute{\acute{x}}$, referring to the principal one x.

So, in many cases, a notation is found necessary to be given to a measure, as a measure; as for instance, Sir Isaac Newton's symbol for fluxion $\dot{x}$; for this stands for a measure of some kind, and accordingly he usually puts an unit for it, if it be the principal one on which the rest depend.

So some notations are expressly to show a number in the form of its composition, as the index to the geometrical power x^n, denoting the number of equal factors which go to its composition, or what is analogous to such.

But that there is no symbol or notation, but what refers to discrete quantity, is manifest from the operations, which are all arithmetical.

And hence it is, there are so many species of mathematical quantity, as there are forms of composite numbers, or ways in their composition; among which there are two, more eminent for their simplicity and universality, than the rest: one is the geometrical power formed from a constant root; and the other, though well known, yet wanting a name as well as a notation, may be called the arithmetical power; or the power of a root uniformly increasing or diminishing; the one is only for the form of the quantity itself, the other is for the constitution of it from its elements.

Now from the properties of either of these, it would be easy to show how the quadratures of simple figures are deducible from the areas of their limiting polygons. Mr. M. just points out the method from the arithmetical power, as being the shortest and readiest at hand.

Let z, $\acute{z}$, $\acute{\acute{z}}$, &c. or z, $\grave{z}$, $\grave{\grave{z}}$, &c. be quantities in arithmetical progression, diminishing or increasing by the common difference $\dot{z}$; and let, as before explained, $\overset{\dot{z}}{\smile}\!z^{(m}$ signify the arithmetical power of z, denominated by the potential index m, namely, $z \times \acute{z} \times \acute{\acute{z}}$, &c. whose first root is z, and last $z - \overline{m - 1} \times \dot{z}$; which being supposed, the element of the arithmetical power will be $m\dot{z} \times \overset{\dot{z}}{\smile}\!\acute{z}^{(m-1}$; that is, the product made from the multiplication of the two indices, and the next inferior power of the next root in order. For the first arithmetical power $\overset{\dot{z}}{\smile}\!z^{(m}$ is $= z.\overset{\ddot{z}}{\smile}\!\acute{z}^{(m-1}$, and the next $\overset{\dot{z}}{\smile}\!\acute{z}^{(m}$ is $= \overset{\dot{z}}{\acute{z}}^{(m-1} \times \overline{z - m\dot{z}}$, therefore the difference will be as is explained.

And consequently, since the sum of these elements or differences, taken in order from the first to the last, make up the quantity according to its termini; hence, if z be the absciss of a curvilinear figure, whose ordinate y is equal to mz^{m-1}, a demonstration might easily be made, that the [form of the quantity for] the area will be z^m; that is, the same multiple of the next superior power of z divided by the index of that power.

For since the arithmetical powers do both unite and become the same with the geometrical power, when the differential index $\dot{z}$ is supposed to be nothing; the magnitude of the geometrical figure will be implied from the magnitudes of the two polygons made up of rectangles, one from the increasing arithmetical power, the other from the diminishing, though it be true, that the elements of the polygons cannot be summed up, when $\dot{z}$, the measure of the absciss z, is supposed to be nothing.

In like manner, in any other case where z and $\acute{z}$ are two abscisses, whose difference, as a measure, is $\dot{z}$; and y, $\acute{y}$ the two ordinates; the magnitude of the figure will be implied by the magnitudes of the two polygons, which are made from the sum of the inscribing and circumscribing elements $\dot{z}y$ and $\dot{z}\acute{y}$, though the figure itself is not to be resolved into any such primogenial rectangular elements.

And thus the symbol $\dot{z}$, considered as a component part of the rectangle $\dot{z}y$, may bear a plain interpretation; viz. that it is the measure according to which the quantity z is measured; nor can he see that any other interpretation need to be put on a symbol, which, like a measure, is used only to make other things known, but is of itself for nothing but a mark.

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

A Description of a new Invention of Bellows, called Water-Bellows. By Martin Triewald, F.R.S. Military Architect to his Swedish Majesty. N° 448, p. 231.

The water-bellows now proposed by M. Triewald, as to their effect, are noways inferior to the wooden bellows, used in Sweden at all their iron forges, and furnaces, &c. but far more advantageous, not only for iron furnaces, but also for many other smelting-works requiring large bellows.

It may seem at first a little strange, that water should be able to blow the fire; but whoever has read the Philos. Trans. and seen the invention there described, as used at Tivoli in Italy, and several other places, called Soffi d'Acqua, and attentively considers the following description, will be convinced, that this new invention of water-bellows, is built on the very self-same foundation, to which leathern and wooden bellows owe their use and origin, and will in several cases prove of more signal service.

These water-bellows A, A, represented in fig. 8, pl. 6, are made of wood, not unlike the shape of diving-bells, in the form of a truncated cone, and consequently wider below than at top, where they are furnished with close heads B, B, but at the lower ends E, E, quite open. At the heads B, B, are two valves V, V, which open inwardly, and are made like the claps of other bellows, with their hinges, and the valves themselves covered with hatters felt, and are shut by an easy steel spring, till the air from above opens the same, which happens only when these bellows receive their motion upwards; but are shut by means of the pressure of the air within, when they sink down into the water. On the same heads are two pliable leathern tubes R, R, fixed one at the top of each water-bellows, which tubes are made and prepared in the same manner as those used in water-engines for extinguishing fire. These leathern tubes, or pipes, reach from the bellows, to wooden tubes T, T, which carry the wind into the iron furnace M, or any other place desired.

These bellows are also provided with iron chains k, K, fastened to two sweeps s, s, by which means they hang perpendicular from the beam of the balance, and at the same distance from the centre of its motion c.

On the balance are two sloping gutters F, F, into which the water alternately runs from the gutter G, and so give motion to the whole work; so that these last-mentioned gutters F, F, do the same service as an over-shot, or any other water-wheel, and cost a great deal less, but give as even and regular a motion, as any pendulum, for measuring time; for as soon as so much water runs into either of the aforementioned inclined planes of the gutters, so that the momentum of the water exceeds the friction near the centre of motion c, the gutter immediately moves down with a velocity increasing, till the balance meets with the resistance of the wooden springs H, H, and at the same time raises the opposite water-bellows, or that bellows which is fixed under the opposite gutter. In the same moment again as the said gutter begins its motion, being come down on the spring, and delivers all the water it has received; at the very same time the water begins to run into the opposite gutter, which receives its load of water almost as soon as the former is emptied; so that one of the gutters does its effect, as soon as the other has done his, and this alternately one after the other.

These sloping gutters, on the balance, do therefore all the service and effect which a water-wheel does in working the ordinary bellows, and that by means of the power which the water applies to the wheel of giving the ordinary bellows their motion, after the same manner does the water here empower the sloping gutters to do the same work.

But as to the manner and the means by which these water-bellows are fit to blow the fire, and to perform the same as leathern or wooden bellows, there is no other reason, but the very same in which the effect of the ordinary bellows consists. For an ordinary pair of bellows blow for no other reason, but that the air, which enters the bellows, and which they contain when raised, is again compressed or forced into a narrower space, when the bellows close: now since the air, like all other fluids, moves to that place where it meets with the least resistance, the air must consequently go through the opening left for it, with a velocity proportioned to the force by which the air is compressed, and must thus blow stronger or weaker, in regard to the velocity by which the top and bottom of the bellows meet; the blast also will last in proportion to the quantity of air, drawn into the bellows through the valve or wind-clap.

This happens after the same manner in our water-bellows; for the contained air cannot force itself down through the water, more than through a well-secured deal-board with pitch. When the bellows are lowered down into the water; the contained air must necessarily be compressed by the water, which rises alternately into the bellows A, A; so that the air must recede, and pass through the leathern tubes R, R, where it meets with the least resistance. From all which it undoubtedly follows, that the larger, that is to say, the more air these water-bellows are made to contain, and the greater the velocity is by which they are made to descend into the water, so much greater is their effect; and that the effect which they are able to perform, must be equal to that of leathern or wooden bellows of the same capacity, in containing an equal quantity of air.

As to the advantages which this new invention has over others, it is well known that the power which works the common bellows, used at iron furnaces, must be sufficient not only to compress the bellows, but at the same time to force down the lever with its weight or counterpoise; which lever serves again to raise the bellows, when the cog or button on the axle-tree of the water-wheel slides off from the bellows-tree; so that the power must be sufficient at once to produce two different effects; whereas these new water-bellows require scarcely any greater power than what is necessary to overcome the friction near the centre of motion, or the axis C; for in this invention an advantage is obtained, which very rarely happens in mechanics, viz. that the weight to be moved is, on the balance in equilibrio; since the bellows A, A cannot be otherwise conceived, than as two equal, though heavy weights, in a pair of scales, which balance each other, though their weight be ever so great; so that, if each of these bellows should weigh a ton, they must still equiponderate;—which is so much easier attained to, as it requires very little art to make them both of a weight, and order them at equal distances from the centre of motion. It is consequently known how small a power is required to set the scales of a balance, with equal weights, in motion, notwithstanding the weight may be as great as possible; all which may with good reason be applied to these water-bellows.

And though it cannot be denied, but that the bellows which sinks down into the water-hole or sump N, becomes so much lighter, as it loses of its weight in water, by which means the water-bellows to be raised seems so much heavier, as the former loses of its weight, by being let down into the water; yet this is compensated, if we consider, that the water which falls down along the sloping gutter, acquires the power of a falling body; which power increasing in the same proportion as the bellows to be raised becomes heavier, this power suits admirably well the weight to be raised; for the bellows that sinks down into the sump N, does not at once lose its weight in the water, but gradually as it descends deeper; and after the same manner the ascending bellows does not become at once heavier than the other, but gradually, being heaviest just when the lowermost edge gets even with the surface of the water; and that happens at the same instant of time when the power of the water in the sloping gutter is at the highest pitch, or has received its greatest momentum.

This shows that the power required to work these water-bellows is far less, and consequently less water will be consumed in working these bellows, than those commonly used; and again, that an iron furnace, which for want of water to work the common bellows, cannot be kept at work longer than 6 weeks, though it be provided with all other necessaries, may, by means of such water-bellows as here described, be kept at work at least as long again.

It is also known to miners, what great loss and inconvenience it is, when the hearth or mouth of an iron furnace is placed low, in a wet and damp place, which they are often obliged to be, in regard to the axle-tree of the water-wheel which works the bellows; for which reason such furnaces as stand in the like moist places, give daily considerably less iron, than others which are better situated. There is likewise no small difficulty in finding a fit situation for such iron furnaces where iron guns are cast, and require deep pits under the mouth of the furnace: but by means of this new invention of bellows, one may be at liberty to place the mouth of the furnace as high as one pleases, as it is very easy to guide the blast by means of wooden or leaden tubes, as far as necessary, and in a proper direction into the furnace; which advantage cannot so easily be obtained by the bellows in common use.

It may also be accounted as no small advantage which these bellows afford, in being of so very easy a structure, that any carpenter at first sight is able, not only to construct the whole engine, but easily to repair every part of it, requiring at the same time the least repairs of any that can be used; and if the bellows should be cast iron, they would last for ages; and when cast strong, they would not require any weight to sink readily in the water. They might be covered with lead, or be made of thin copper, with a thick leaden hoop at top, to make them sink. As for their shape, it is not absolutely necessary they should be of the same as the figure annexed denotes; for if we would not bestow iron hoops on the bellows, they might be made square, or triangular, or any other shape, provided they be as wide again at bottom as at top; and when made of wood, it will be necessary to provide an edge round the tops, for containing stones or leaden weights, as much as will be found necessary to sink them readily, when they are lowered down into the water.

Lastly, If we consider the charge of those bellows used at iron furnaces, as to the bellows themselves, the water-wheel, and its axle-tree, &c. and compare the same with the cost of these, we shall easily find a vast difference, not to mention the great charges of keeping the common bellows in repair. Before concluding it may be mentioned, that the blast of these bellows is governed and moderated in the same manner as the common ones, viz. by letting more or less water into the sloping gutters, and by taking out and letting in plugs for that purpose, placed in holes near the top of the water-bellows.

An Abstract of Meteorological Observations for 6 Years, made at Padua. By Sig. Poleni. N° 448, p. 239. *Translated from the Latin.*

The following is an abstract of 6 years meteorological observations, made according to the rules recommended by Dr. Jurin; and consequently corresponding with those published in Phil. Trans. N° 421. The instruments used were the same, and posited in the same places, and applied in the same manner, as there mentioned. The observations are as follow:

Table A, *showing the Rain and Snow-Water for every Month.*

	1731	1732	1733	1734	1735	1736
	In. Dec.	In. Dec.	In. Dec.	In. Dec.	In. Dec.	In. Dec.
January	2.546	2.129	1.855	1.034	4.052	6.541
February	3.093	1.959	0.405	1.735	2.420	2.981
March	0.976	2.765	5.642	1.558	5.162	2.721
April	3.434	5.432	3.816	1.706	1.452	1.227
May	0.602	1.864	5.330	4.372	2.681	4.444
June	4.253	2.872	2.712	4.555	3.865	2.777
July	3.402	1.585	3.874	7.015	4.992	3.064
August	7.372	3.112	3.679	3.082	0.720	1.844
September	2.216	0.089	0.589	2.899	1.287	2.479
October	4.354	9.164	2.788	4.391	1.878	0.529
November	1.653	0.957	0.382	1.307	0.542	1.454
December	0.306	3.528	1.065	4.909	0.634	0.572
Sum of the whole year	34.207	35.456	32.137	38.563	29.685	30.633

By which all the varieties, as to different years and seasons, easily appears. And the medium annual quantity, among all these 6 years, is 33.447.

Table B, *showing the Quantities for the 4 Seasons in all the Years.*

	Winter. In. Dec.	Spring. In. Dec.	Summer. In. Dec.	Autumn. In. Dec.
1731	5.759	6.647	13.598	8.017
1732	4.522	10.300	7.226	10.186
1733	6.321	15.758	8.762	3.759
1734	4.074	8.014	14.034	10.125
1735	10.450	10.848	7.805	2.337
1736	11.945	8.054	6.361	4.588
Sum.	43.071	59.621	57.796	40.012

Table C.

	The sum of the heights of the barometer. Inch. Dec.	The sum of the heights of the thermometer. Inch. Dec.	The mean height of the barometer for each day. Inch. Dec.	The mean height of the thermom. for each day. Inch. Dec.
1731	10850.65	18286.25	29.72	50.09
1732	10870.19	18361.30	29.70	50.17
1733	10867.18	18301.95	29.77	50.14
1734	10850.24	18305.78	29.73	50.15
1735	10861.21	18274.87	29.76	50.06
1736	10870.07	18338.42	29.70	50.10

In the table C may be seen the sums of the heights of the barometer and thermometer for each year; as also the mean heights corresponding to each day. For the whole last 6 years, the mean height of the barometer, referable to each day of the said 6 years, is 29 inches 73 dec. differing only, 03 parts from that of the former 6 years, which was 29 inches 70 dec. And likewise the mean height of the thermometer for each day of these last 6 years, is found to be 50 inches 12 dec. differing only ,04 parts from that of the former 6 years, which was 50 inches 16 dec. showing a remarkable uniformity.

S. Poleni adds some observations on the declination of the magnetic needle. In April 1733, he found by repeated observations, that the declination was 13½ degrees westwards. On the last days of the year 1736, he found it 13¾ degrees. If therefore what has been already said of the declination of the needle, in Phil. Trans. N° 421, be compared with this, it will appear, that the declination for the 3 first of these 6 years increased more than it did for the 3 last.

The Imperfections of the Common Barometers, and the Improvement made in them by Mr. Cha. Orme of Ashby-de-la-Zouche. With some Observations, Remarks, and Rules for their Use. By Mr. Henry Beighton, F. R. S. N° 448, p. 248.

Nothing is more wanted than a theory of the weather on mechanic principles; and nothing in all philosophy seems of more immediate concern, than the state of it. In order to this, a complete history of the weather is necessary, from thence to deduce such rules and observations as may in some measure form such a theory: and could we in any tolerable degree foretel, but by some small space of time, the change of the weather, it would be of admirable use, in those affairs on which the chief part of our welfare and subsistence depends.

It was from such considerations, that more than 20 years ago Mr. B. began, and has continued, to keep a diary of the weather, the last 6 years of which are here subjoined; but cannot think himself so well qualified, as to form a just theory on them, though they may have their uses, when they fall into more able hands. Yet he thinks he can generally foretel for a day, or perhaps two, the change, or what continuance the weather will have.

And though so many ingenious persons, since the invention of Torricelli's barometer, have been endeavouring to bring that machine to perfection; yet notwithstanding all their care and pains, the air interspersed and mixed with all fluids, has in some measure frustrated their labours, and it has remained imperfect: for while there are any small quantities or particles of air remaining in the quicksilver, it will be constantly rising in hot weather, and falling in cold: which really perverts the very end and design of a barometer, which should show the pressure of the air, and foretel when either fair weather or rain is coming; instead of which it is in a great measure a thermometer, foretelling heat instead of fair, and cold instead of rain and stormy weather: and these imperfections, more or less, attend all the various sorts of barometers, that have hitherto been invented.

The barometer here described, is not different in form from some usually made, it being of the diagonal kind, from whence the more minute alterations are more readily discovered: of this form many have been made, by the late curious operator Mr. Patrick, who has, in his way, well deserved of the curious; who, though he had done so much towards the proving the weight of the atmosphere by which the mercury in the tube was sustained, he himself did not believe it, but ran into the absurdity of the funicular hypothesis.

There is an inconveniency or imperfection in most, if not all, of those diagonal barometers; for after some time, by the various rising and falling, and changes of the weather, of heat and cold, the small particles of air interspersed in the mercury, have got together in a larger mass, as they will incline by attraction, which will separate the mercury; and that quantity of air will be dilated by heat, and contracted by cold, so as to spoil the design of the instrument.

Besides, there is such a cohesion or attraction of the mercury to the tube, especially in the small ones, that after some time, the mercury that is not truly cleansed from its dross, and purged of all its air, in remarkable changes of the weather, will neither rise nor fall. All which embarrasment is taken off, and the difficulties surmounted, in

Mr. Cha. Orme's Improvements of the Barometer, by the Method following:

First, The quicksilver is all purified from its dross and earthy particles by distillation; and when the tube is filled by a pound and half, or 2, or 3lb. of mercury, and all the air got out by the methods used in filling tubes, then the remaining air is got out by such an intense heat of fire as makes the mercury boil; by which ebullition an innumerable quantity of small particles are emitted, and blow with a great velocity at the open end of the tube, till all the air is quite cleared out; which operation is continued for the space of 4 hours; and when no more bubbles would rise in the tube, it remained whole, with its mercury of a most lively sparkling brightness, with this difference only, that the mercury, so purged of its air, did not fill the tube so high as when first put in, by about 2 inches; which is a plain demonstration, that in that tube, which was 49 inches long, there was interspersed in the mercury at first filling it, so much air as would fill 2 inches of the said tube, which was a 24th part of the said space. In this way every part of the mercury boiled for a long time, and the tube became gradually so red-hot, that with a warm knife impressions could be made in any part of it.

And this I the rather mention, by reason I have heard several persons, and those not incurious, affirm it was impossible.

The Perfection of these Barometers, by which they exceed all others ever observed, in the following Particulars.—1. They are sensible of the most minute changes of the air.

2. They foretel the weather by a much longer space of time than others, as mostly 20 hours, sometimes 36 or 48 hours: nay, before great tempests, and such rains as cause great floods, for a much longer time before they happen.

3. Though they are so sensible of such minute changes of the air, yet the most intense heat will not raise them a hair's-breadth, nor the greatest cold make them fall. This shows that they are perfect barometers, and not in any degree thermometers.

4. If they show for rain, you may by them distinguish whether it will be little or much.

5. As by other barometers you cannot tell the weather, but by a past and a present observation; these tell, the moment you come to them, what the weather is going to be: for by tapping the case with your finger, if it is going to be fair, or very fair weather, the mercury will rise that moment a 10th of an inch, or more: but if for foul, it will scarcely make any sensible rise.

Remarks.—1. Though you can foretel it will rain on the morrow, it is impossible to tell where that rain will fall: for as every shower has space, i. e. length and breadth, if it rains in that particular field, yet it may be fair in the next adjoining: and if in harvest, or on a journey, you proclaim it will rain on the morrow, some will, if it does not fall on their land, or on their coat, be so silly as to say the prediction was false.

2. The barometer only shows the pressure or weight of the atmosphere, and inclination of the air, in and about the country where it stands, and not always in a particular spot: so that in foretelling of great rains, people are apt to say the indication is false, because they have not seen or heard of it; when perhaps in a day or two you will hear, that it did then fall 3, 4, or perhaps 10 miles off. For though the rain should be over us when the glass fell, yet the wind, which bloweth where it listeth, carries the clouds and rain with it.

3. It is very hard to distinguish on the mercury's falling, whether it will be rain or high winds, these equally causing the mercury to subside.

4. Of all those who guess at the weather from the whims of their own brains, it is observable, it is not true one time in ten, nor do any two of them agree about it. But from observations on this barometer, it will seldom fail you once in 20; so that it is above 100 to 1 preferable.

5. If from the state of the mercury yesterday and this morning, it be pronounced the next day will be no rain, and we look at the glass no more to-day; perhaps winds may arise, and so alter the atmosphere's weight, and the glass falls much, it will rain on the morrow, contrary to what was at first expected. Here it is plain, had the glass been seen again in the afternoon, the rain might have also been foreseen.

Hence it is evident from these remarks, that judgments are taken on the weather from barometers, which do not prove so; and this produces opinions in the vulgar and ignorant, that there is no judgment at all to be had from them.

If the barometer could only foretel very great and remarkable changes of the weather; for instance, in harvest-time, that a very great rain, or perhaps floods, were coming; the husbandman would stop cutting down his grain, and save some of it being spoiled by the wet: or on a journey, if I know that if I do not get home by such a time, or pass such rivers, the floods will be so great, as not only to prevent me, but endanger my life: and perhaps here is a man's fortune saved, nay his life, merely from the indications of the barometer; and he who reckons this nothing, deserves neither.

The greatest storm that has been in our days, was Jan. 8, 1734-5. On the 5th the mercury began to fall, and on the 8th was a 10th below 28 inches; which has not been seen in this age, or perhaps since Torricelli's time; thence I could plainly indicate, that it would be the greatest flood we ever heard of, or the greatest storm we ever felt; the latter of which it proved.

Some Rules and Observations for fore-knowing the Weather, by the rising and falling of the Mercury.—Though rising always presages fair, and falling foul weather, yet there are several difficulties and niceties in making a true judgment from them, and herein consists the chief part of the art.

We need not recount the several observations made by Dr. Halley, Dr. Beal, Dr. Derham, Mr. Patrick, and others, though they are most of them applicable to this improved diagonal barometer, as they are in so many hands, and in most authors on the subject, and because Mr. B. has collected them, in order to be made public, at the request of the improver of the barometer, Mr. Orme, and for his use; which some time since were put into the hands of Dr. Desaguliers, who is acquainted with Mr. Orme and his glasses. Mr. B. only inserts here some few observations, which may be called rules, as he has deduced them from time to time, in using Mr. Orme's glasses, and keeping a register of the weather.

Rules and observations for the improved diagonal barometer.—1. This barometer very rarely foretels thunder, seldom falling at all before it, which Mr. Patrick observes others do.

2. In serene and hot weather, when the mercury is high and rising, and you have all the possible certainty of fair weather the next day, and if there happen to fall great showers, you may conclude they have been driven on you by thunder, though you have heard nothing of it.

3. When the mercury is pretty high, and has fallen to foretel rain, and it rises again before the rain comes; it indicates there will be but little of it.

4. If the mercury continues falling while it rains, it shows it will rain the next day.

5. In fair weather, when the mercury has continued high or rising, if it falls a little to-day about noon, and towards the evening rises again, you must expect a single shower the latter part of the next day, or perhaps by noon, and then fair weather again forward.

6. When the mercury rises gradually, about half a 10th perpendicular, and

continues so to do for many days together; you may reasonably expect a fair season for as long a time as it was rising, unless some gales of wind intervene, and especially the s. w. by s. or thereabouts.

7. When the mercury rises very fast, or falls very fast, neither the fair nor foul weather it forebodes will continue long.

8. Without knowing how the mercury has stood some little time before, a true judgment cannot be given at all times; for suppose we find it in a rising condition, it will probably be fair; but if it had been higher some hours ago, and fell, there must happen a shower.

Why the mercury in the diagonal barometer, if it be for fair weather, on tapping the case several times, which jars and makes the tube tremble, will rise at every stroke for several strokes together, and in all sometimes a 10th of an inch, or more, in the perpendicular; may be thus accounted for:

1. There is a cohesion of the mercury to the tube, which hinders its rising, and such tapping releases that.

2. But it is observable, that it will rise a little at all times, even when it is in a standing, or even in a falling condition. This may be accounted for thus:

The mercury and atmosphere are in an equilibrio, and tapping starts and raises the mercury a little in a boiling manner, especially its upper surface, which is seen to leap, or be in a swimming posture; then the pressure of the atmosphere over-balances the remainder of the mercury, and it must rise a little.

Or such violent jarring puts the mercury in a lateral and upward motion, for downward it cannot go, which takes off its gravity, as the winds lessen the pressure of the air; therefore it must rise a little.

But then it is observable also, that if the mercury was in a standing condition, or falling, such rising as above, will in a minute return to the same place again; and even when the mercury is in a rising condition, it will, in that space of time, fall a little part of that it rose by such tapping.

This barometer has the coruscations, as they were observed in Mr. Patrick's pendant one; for by tapping the case with the finger in a dark place, it will emit several bright flashes, along the empty part of the tube. This is an argument that the vacuum is very pure, and the mercury truly purged.

Collections from the diary of the weather and barometer, in order to settle rules for foretelling the weather by the barometer.

Great Storms.

Before them the mercury falls 3 or 4 days, and is exceedingly low.

1734-5, Jan. 4, at night the merc. at 29.92 inc.
5.... night........... 29.66
6.... night.......... 29.2
7.... night.......... 28.1
8.... noon.......... 27.9

lower than has been known by $\frac{1}{10}$, and the greatest storm of wind ever heard of in this age, in the south of England, as also in France and Holland.

1736, Jan. 31 .... 29.47
Feb. 1 .... 29.15
2 .... 28.39 rain and stormy.
1734, Aug. 11 .... stormy.

Great Floods.

Before which the mercury falls very much.

1735, Sept. 4 .... 29.7
5 .... 29.6
6 night............. 29.6
7 .... 29.25 The greatest flood that has been at Coventry, being about the middle of England, these 40 years, and yet the mercury fell but little.

1735, Oct. 23 .... 29.55
24 night............. 28.8
25 night............. 28.78
26 .... 28.85
27 .... 28.26 a great flood.
1735, Aug. 19 .... 29.3
20 .... 29.28
21 .... 29.3
22 .... 29.2
23 .... 29.2 stormy, great rain.
24 .... 29.38 floods.
1735, Dec. 2 .... 29.32 rain.
3 .... 29.5 fair.
4 .... 28.8 rain.
5 .... 28.9 rain.
6 .... 29.5 fair.
7 .... 29.52 great rains and floods.

Thunder.

The mercury seldom falls for rains that come by thunder. See diary, June 2, 1735.

Thunder.

When the mercury did rise.

1733, June 21 ...... 29.16 ...... 29.56
22 ...... 29.56 ...... 29.56
23 ...... 29.62 ...... 29.65 hot.
24 ...... 29.65 ...... 29.57 sultry.
25 ...... 29.54 ...... 29.52 sultry.
26 ...... 29.51 ...... 29.59 great thunder.
27 ...... 29.57 ...... 29.56 a very violent thunder, from 10 in the morning to 1 in the afternoon, doing great damages.

1735, June 1 ...... 29.3 ...... 29.8
2 ...... 29.4 ..... 29.55 thunder and great rains.

Thunder.

The mercury fell before it.

1733, July 27 ...... 29.44 hot, fair.
28 ...... 29.37 wind, rain.
29 ...... 29.09 violent thunder.
1734, Aug. 7 ...... 29.59 sultry.
8 ...... 29.46 fair.
9 ...... 29.25 thunder.
10 ...... 28.87 rain, thunder.

Frost.

A frost, when the mercury is high, brings rain.

1731, March. The mercury was high all the month, and no rain, but what followed the frost the 17th and 29th.

Dry Season.

In June 1729, and the mercury scarcely ever above changeable.

In Aug. 1730, the mercury never lower than 29.37.

1731, from the 1st to the 10th, and rain came the 16th, though the mercury was rising.

Frost.

A great frost, though the mercury fell; but it was attended with a great snow, which might occasion it to subside.

1731, Jan. 1 .... 29.56.... rain.
2 .... 29.46.... 29.12 rain.
3 .... 28.78.... 28.72 wind.
4 .... 28.72.... 28.81 frost, great snow.
5 .... 28.93.... 29.12 snow, frost.

Great Rains.

Though the mercury was rising.

1732, May 1 .... 29.28.... 29.25 wind.
2 .... 29.21.... 29.25 rain all day, snow hard from 8 to 11.
3 .... 29.34.... 29.0 rain.
4 .... 29.09.... 29.09 rain.
5 .... 29.12.... 29.34 wind.
6 .... 29.44.... 29.46 fair.
7 .... 29.52.... 29.39 rain, and great floods.

Great rain, though the mercury fell but little.

1733, 24 .... 29.6 .... 29.54 wind.
25 .... 29.51.... 29.54 fair.
26 .... 29.52.... 29.54 fair.
27 .... 29.5 .... 29.39 violent rain for more than 11 hours.

Great Rains.

The mercury falling very much.

1734, July 10 .... 29.65.... 29.67 fair, hot.
11 .... 29.63.... 29.62 fair, hot.
12 .... 29.59.... 29.4 rain.
13 .... 29.29.... 29.13 great rains.

——The mercury falling a great while before the rain came, and the rain continued as long.

1736, May 19 .... 29.75 fair wind...... 29.8
20 .... 29.8 cold wind, fair 29.7
21 .... 29.65 cold wind 29.52
22 .... 29.39 wind, clouds, rain 29.31
⊙ 23 .... 29.28 cloudy, fair.... 29.27
24 .... 29.32 fair.......... 29.35
25 .... 29.32 clo. wind, rain 29.24

1736, May 26 .... 29.15 rain.......... 29.15
27 .... 29.12 rain.......... 29.2
28 .... 29.28 rain.......... 29.23
29 .... 29.37 wind, cloudy, rain.
1735, Feb. 22 .... 29.43
23 .... 28.82
24 .... 28.9
25 .... 28.76 great rain.

Just after hot or sultry weather, the mercury generally falls.

See 16 Sept. 1731.
8 Aug. 1734.

After the aurora borealis, there generally follow high winds.

27 Oct. 1733, a large aurora borealis, and the 28th, 29th, and 30th, high winds.

See 23 Jan. 1734.

The mercury falling pretty much, and neither wind nor rain succeeded.

1733, from the 18th to the 21st it fell 41, and no wind or rain at all till the 25th.

Sultry weather generally makes the mercury fall soon after.

1734, Aug. 8.

After a great storm the mercury rises very fast.

1734, Aug. 11.
1736, Feb. 6.

Before great winds the mercury falls very soon.

1734, Aug. 26.
1736, Feb. 8.

The mercury below 28 inches.

1734, Dec. 15, at 27.9.
1735, Jan. 8. at 27.9.

In winter, before frosts, the mercury generally rises pretty fast.

1735, Dec. 12.

Before a thaw the mercury falls.

1735, Dec. 13.
17.
1736, Feb. 9.

The mercury falls suddenly before a great snow.

1731, Jan. 4.
1736, Feb. 8.
21.

When the mercury falls for high winds, and it continues to fall when that wind is come, it is likely to be tempestuous, or continue some time, unless rain succeeds.

1736, 22 Nov.. 29.62 fair, warm.. 29.62
23...... 29.49 windy, warm 29,32 wind.
24...... 29.1 high wind .. 28.88 28.73 stormy.

Some of these collections are quite contradictory to any settled rules, and such will happen, and others confirm them; but he has collected so very few of a sort, though the diary furnishes a great many, that till more are in this manner collected, it will be very doubtful to form any rules from them.

An Account of a Sulphureous Vaporiferous Cavern in a Quarry at Pyrmont, similar to the Grotta del Cane at Naples. By Mr. Misson and others. Communicated to the Royal Society by John Philip Seip, M. D. Aulic Councillor and Archiater to the Prince of Waldeck, and F. R. S. N° 448, p. 266.

An account of a sulphureous vaporiferous cavern in a quarry at Pyrmont, near to the famous chalybeate springs. Birds, including poultry, and the smaller quadrupeds, such as dogs, cats, &c. were suffocated on being exposed for a sufficient length of time to the vapour emitted from this cavern, which in this respect resembles the Grotta del Cane at Naples. When exposed to this exhalation but for a short time, and afterwards brought out into the open air, the animals commonly revived. A candle would not burn in this cavern. [It would seem that this supposed sulphureous vapour consisted of the so called fixed air, or carbonic acid gas.]

On the Effects of Dampier's Powder, in curing the Bite of a Mad Dog. By John Fuller, Esq. Jun. F. R. S. N° 448, p. 272.

Mr. Fuller imagined the use of the lichen cinereus terrestris with black pepper, had been so infallible a remedy for the bite of a mad dog, that there needed no proofs of its virtue; he himself has used it on dogs, and always with success; and some years since, a mad dog or cat had bitten some children and their mother, at Battle: and mixing the lichen according to Dampier's direction, they all took it, as well as a dog or two that were bitten, and none of them had any bad effects from the bite.

Christmas 1737, a neighbour's servant going to search whether a dog suspected to be mad had been wormed, which dog died mad in 3 or 4 days after, was bitten very much in both his hands; he went to a person, who had great success in using the lichen cinereus terrestris with pepper for the bite of a mad dog. The man took his medicine every day; about 10 or 11 o'clock he complained of a violent heat, and pain in his head, which Mr. F. suspected was the effect of the bite, and not the medicine; but after he had taken it for such a stated number of days, he grew better, and continued well ever after.

The man had tied his fingers with shoe-maker's ends, which are often used for a cut; and they were all very much inflamed, and very sore. Mr. F. made him take them off, and all his plasters, and wash his hands with salt and water, and in a fortnight's time they were quite well.

Another Case of a Person bitten by a Mad Dog. By David Hartley, M. A. and Mr. Fr. Sandys.* N° 448, p. 274.

About the latter end of Nov. 1732, Mr. Soame's groom was bitten in the hand by a mad dog, so as to fetch blood. It was not known in the family for 3 days. On the 4th day, when Fr. Sandys first saw it, the wound was healed; but it was opened again by him, and kept so for some time, but at last healed sooner than was intended, by the servant's neglect.

* Dr. David Hartley, better known by his metaphysical than by his medical writings, was born in 1705. His father was a clergyman. After completing his education at Cambridge, where he prepared himself for the profession of physic, he went to settle at Newark upon Trent, whence he removed to St. Edmund's Bury; he afterwards came to London, and at last fixed himself in Bath. From these frequent changes of abode, it may be inferred that he never got into very extensive practice.

In 1739 he wrote a pamphlet in favour of Mrs. Stevens's medicine for the stone. His evidence had great weight with parliament, who voted a large sum of money to Mrs. Stevens, the discoverer of this supposed lithontriptic; which, however, did not long uphold the character that had been given of it, and is now gone into disuse.

As a writer Dr. H.'s principal work is his Observations on Man, published in 1749; a work replete with ingenious, but not always tenable, theories and opinions. The above, and another paper on a case of calculus, are the only communications of his which are in the Phil. Trans. He died in 1757.

He was bled, took a purge, after that ¼ oz. of pulvis antilyssus, every morning for 3 days, and was ordered to go into cold water every day for some time; but he neglected it after the 3d day. Besides, Fr. Sandys ordered him to forbear all meats, and drink nothing but water. He continued in this regimen for about 5 weeks; then finding himself well, would confine himself no longer to it.

On Jan. 7 following, he was seized with a sickness, vertigo, and faultering in his speech and memory; and at last his vertigo increased to such a degree, that he fell down twice in the space of half an hour; and the last time he did not recover his senses, till he was put to bed, and blooded to the quantity of 18 or 20 oz. He continued all night restless and sullen, and in the morning was blooded again, to the quantity of 15 oz. Dr. Hartley was sent for, and came about 8 at night, and found him very sullen, thirsty, but averse to drinking, and his pulse quick and hard. He ordered him to be put into the cold bath; but he refused to comply with it, till he saw that force would be used. About midnight his pulse rising, the Doctor ordered him to be blooded to the quantity of 16 or 18 oz. he continued restless all night. About 8 in the morning he went into the cold bath again: about 10 Dr. Hartley went away, leaving it as his opinion, that the cold bath and bleeding should be freely repeated, as the circumstances should require.

About noon Mr. Sandys came, and bled him immediately, to the quantity of 18 or 20 oz. he continued restless all this night. On Mr. Sandys's asking him whether his aversion to drinking proceeded from any pain in swallowing, or some other cause? he said it was from a pain in swallowing. The next morning his strength not being at all diminished, and his pulse continuing full as vigorous as ever, Mr. Sandys bled him again to the quantity of 15 or 16 oz. yet he still remained the same, and took the same care of his horses as usual. Mr. Sandys went away, leaving orders that as long as these symptoms, viz. restlessness, strength, and aversion to drinking continued, he should be blooded freely, and put into the cold bath.

He was blooded twice more within the week, so that the whole quantity which he lost in that time was about 120 oz. After the last bleeding his symptoms disappeared, and he grew weak, low spirited and sleepy; he then went 8 times into the cold bath. He did not take any medicines during his whole illness.

N. B. This person continued well anno 1738.

An Inquiry concerning the Figure of such Planets as revolve about an Axis, supposing the Density continually to vary, from the Centre towards the Surface. By Mr. Alexis Clairaut, F. R. S. Translated by the Rev. John Colson, F. R. S. N° 449, p. 277.

Notwithstanding that part of Sir Isaac Newton's mathematical principles of natural philosophy, where he treats of the figure of the earth, is delivered with the usual skill and accuracy of that great author; yet I thought something further might be done in this matter, and that new inquiries may be proposed, which are of no small importance, and which possibly he overlooked, through the abundance of those fine discoveries he was in pursuit of.

What at first seemed worth examining, when on applying to this subject, was to know why Sir Isaac assumed the conical ellipsis for the figure of the earth, when he was to determine its axis? for he does not acquaint us why he did it, neither can we perceive how he had satisfied himself in this particular: and unless we know this, we cannot entirely acquiesce in his determinations of the axes of the planets. It seems as if he might have taken any other oval curve, as well as the conical ellipsis, and then he would have come to other conclusions about those axes.

I began then with convincing myself by calculation, that the meridian of the earth, and of the other planets, is a curve very nearly approaching to an ellipsis; so that no sensible error could ensue by supposing it really such. I had the honour of communicating my demonstration of this to the R. S. at the beginning of the last year; and I have since been informed, that Mr. Stirling, one of the greatest geometricians I know in Europe, had inserted a discourse in the Philos. Trans. N° 438, where he had found the same thing before me, but without giving his demonstration. When I sent that paper to London, I was in Lapland, within the frigid zone, where I could have no recourse to Mr. Stirling's discourse, so that I could not take any notice of it.

The elliptical form of the meridian being once proved, I no longer found any thing in Sir Isaac Newton, about the figure of the earth, which could create any new difficulty; and should have thought this question sufficiently discussed, if the observations made under the arctic circle* had not prevailed on us to believe, that the shape of the earth was still flatter than that of Sir

* On a late examination and re-measurement of that part of the meridian, considerable errors have been detected in the old measures; by which means those measures are brought to harmonize and accord with all other measurements in different places, and yielding in the result a like figure of the earth as those do.

Isaac's spheroid; and if he himself had not pointed at the causes, which might make Jupiter not quite so flat as by his theory, and the earth something more.

As to Jupiter, he says, (Page 416 of the 3d edition of Phil. Nat. Prin. Math.) that its equator consists of denser parts than the rest of its body, because its moisture is more dried up by the heat of the sun. But as to the earth, he suspects its flatness to be a small matter greater than what arises by his calculation. He insinuates, that it may possibly be more dense towards the centre than at the superficies. I am something surprised that Sir Isaac should imagine, that the sun's heat can be so great at Jupiter's equator, when it has no such effect at that of the earth; and that he does not ascribe each to a like cause, by supposing that Jupiter also may be of a different density at the centre from that at the superficies.

But whatever reason he might have for introducing two different causes, I give the preference to the hypothesis which supposes unequal densities at the centre and at the circumference. I have inquired, by the assistance of this theory, what would be the figure of the earth, and of the other planets which revolve about an axe, on supposition that they are composed of similar strata, or layers, at the surface; but that their variable density, from the centre towards the circumference, may be expounded by any algebraical equation whatsoever.

And though my hypothesis should not be conformable to the laws of nature, or even though it should be of no real use; which would be the case, if the observations made by the mathematicians now in Peru, compared with ours in the north, should require that proportion of the axes, which is derived from Sir Isaac's spheroid; I thought however that geometricians would be pleased with the speculations contained in this paper, as being, if not useful, yet curious problems at least.

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

PROBLEM I.—*To find the Attraction which a homogeneous Spheroid* BNEbe, fig. 9, pl. 6, *differing but very little from a Sphere, exerts on a Corpuscle placed at* A *in the Axis of Revolution.*——1. We may conceive the space BNE*b*DMB, included between the spheroid and the sphere, to be divided into an infinite number of sections perpendicular to the axe AC*b*. Supposing then that every one of the particles, which are contained in one of these elements or moments N*n*mM, exerts the same quantity of attraction on the body at A, which may be supposed because of the smallness of NM; we shall have $c\alpha \times \text{PM}^2 \times \text{P}p \times \frac{\text{AP}}{\text{AM}^3}$ for the attraction of any one of these elements; putting c for the ratio of the circumference to the radius, and α for the given ratio of MN to PM, that is, of DE to CD.

Now if we make $\text{CA} = e$, $\text{CB} = r$, $\text{AM} = z$; and for PM, AP, Pp, if we substitute their values expressed by z, and then seek the fluent of the foregoing quantity; we shall have $\frac{4c\alpha r^3}{3ee} - \frac{4c\alpha r^5}{5e^4}$ for the value of the whole attraction of the solid generated by the revolution of BDbEB: to which if we add $\frac{2r^3c}{3ee}$, the attraction of the sphere, we shall have $\frac{2r^3c}{3ee} + \frac{4cr^3\alpha}{3ee} - \frac{4c\alpha r^5}{5e^4}$ for the required attraction of the spheroid on the corpuscle A.

PROBLEM II. *Supposing now the Spheroid* BE*be*, fig. 10, *to be no longer of a homogeneous Matter, but to be composed of an infinite Number of Elliptical Strata, all similar to* BE*b*, *the Densities of which are represented by the Ordinates* KT *of any Curve whatever* VT, *of which we have the Equation between* CK *and* KT; *the Attraction is required which this Spheroid exerts on a Corpuscle placed at the Pole* B.—2. Making $\text{BC} = e$, $\text{CK} = r$, by the foregoing proposition, we should have $\frac{2r^3c}{3ee} + \frac{4cr^3\alpha}{3ee} - \frac{4c\alpha r^5}{5e^4}$ for the attraction of the spheroid KLK, if it consisted of homogeneous matter; and the fluxion of this quantity $\frac{2rrc\dot{r}}{ee} + \frac{4c\alpha r^2\dot{r}}{ee} - \frac{4c\alpha r^4\dot{r}}{e^4}$ would be the element or moment of the orb KLKklk. But because the density is variable, we must multiply this value of the attraction of the orb by KT, and the fluent of this quantity will be the value of the attraction of the spheroid KLK.

As to the value of KT, which expresses the density of the stratum KLKklk, we shall take only $fr^p + gr^q$, because we shall see afterwards, that a value more compounded, as $fr^p + gr^q + hr^s + ir^t$, &c. which by the property of series may express all curves, would not produce any variety in the calculation.

Therefore multiplying the foregoing equation by $fr + gr^q$, we shall have $\frac{2cf \times \overline{1+2\alpha} \times r^{3+p}}{ee \times \overline{3+p}} - \frac{4c\alpha fr^{5+p}}{e^4 \times \overline{5+p}} + \frac{2cg \times \overline{1+2\alpha} \times r^{3+q}}{ee \times \overline{3+q}} - \frac{4c\alpha gr^{5+q}}{e^4 \times \overline{5+q}}$ for the quantity of attraction of the spheroid KLK, exerted on a corpuscle placed at B.

3. In this value making $r = e$, we shall have $\frac{2cfe^{1+p}}{3+p} + \frac{8cfe^{1+p}\alpha}{\overline{3+p} \times \overline{5+p}} + \frac{2cge^{1+q}}{3+q} + \frac{8cge^{1+q}\alpha}{\overline{3+q} \times \overline{5+q}}$, which will express the force of attraction of the spheroid BEb, exerted on a corpuscle placed at the pole B.

THEOREM. *A Corpuscle being placed in any Point* N *of the Surface of the* *foregoing Spheroid* BEbe, *I say it will undergo the same Attraction from this Spheroid, as if it were placed at the Pole* N *of a second Spheroid revolving about the Axe* NO, *the second Axe being the Radius of a Circle equal in Superficies to the Ellipsis* FG; *supposing this second Spheroid* NGOF (fig. 11) *to be composed of the Strata* MInqQ, *whose Densities are the same as those of the Strata* KkLlKk, *of the first Spheroid.*—4. In the discourse I had the honour of communicating to the R. S. being then at Torneo, printed in the Philos. Trans. N° 445, I have demonstrated this proposition as to a homogeneous spheroid; and the same reasoning will obtain in this case also.

PROBLEM. III. *To find the Attraction which the Spheroid* BEbe (fig. 10) *exerts on a Corpuscle placed at any Point* N *of the Superficies.*—5. We will make, as above, BC $= e$, CE $= e + e\alpha$, and also CN $= e + e\lambda$, and half the conjugate diameter of CN will be CG $= e + e\alpha - e\lambda$; whence the radius of a circle, equal in superficies to the ellipsis FG, will be a mean proportional between CE and CG, that is, $e + e\alpha - \frac{1}{2}e\lambda$. Therefore the spheroid BE*be* exerts the same attraction at N, as would be exerted at the pole of a spheroid NGOF, (fig. 11) of which the principal axis would be NO $= 2e + 2e\lambda$, and the second would be to the principal, as $1 + \alpha - \frac{3}{2}\lambda$ to 1.

Therefore in the expression of the attraction at the pole, (Art. 3) we must substitute $e + e\lambda$ instead of e, and $\alpha - \frac{3}{2}\lambda$ instead of α. But if f and g must no longer be the same; for we may easily perceive by the foregoing Theorem, that the density must be the same in this spheroid NGOF, at the distance $r+r\lambda$ from the centre, as it is in the spheroid BE*be* at the distance r. Therefore $f\left(\frac{e}{1+\lambda}\right)^p + g\left(\frac{e}{1+\lambda}\right)^q$ must be put instead of $fe + ge$. Thus we shall have

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

for the attraction of the spheroid BE*be* at N.

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

7. If we would have the attraction at any point M within the spheroid, in the expression of the attraction at N, we must put r instead of e. The proof of this is plain from the same reasons that Sir Isaac Newton makes use of, (Corol. 3, Prop. 91, l. 1, Princip. Math.) to show that the attraction of an elliptic orb, at a point within it, is none at all.

PROBLEM IV. *Let* RΠrπ (fig. 12) *be a Circle whose Centre is* Y; *it is required to find the Attraction which this Circle exerts on a Corpuscle at* N, *ac-*

cording to the Direction HY; *supposing the point* H, *which answers perpendicularly below the Point* N, *to be at a very small Distance from the Point* Y.—8. Let there be drawn $\Pi H\pi$ perpendicular to the diameter RYr, and let the space $R\Pi\pi$ be transferred to $\pi\Pi Z$. Then the space $\pi Z\Pi r$ will be the only part of $R\Pi r\pi$, which will attract the body N according to HY.

To find the attraction of this little space, we will suppose it to be divided into the elements Ttss, the attractions of which, according to HY, will be $\frac{Ttss \times QT}{NT^3}$, or $\frac{2HY \times Qq \times QT}{NT^3}$, the fluent of which $\frac{2HY \times HQTZ}{NT^3}$ is the attraction of $TZrs$, according to HY. In which if we put $\Pi\pi$ for HQ, we shall have $\frac{\Pi H\pi R \times 2HY}{NT^3}$, or $\frac{\frac{4}{3}HY \times \Pi H^2 \times c}{NT^3}$, for the attraction required.

9. It is easy to perceive, that if, instead of a circle, the curve $R\Pi r$ were an ellipsis, or any other curve whose axes were but very little different from one another, the foregoing solution would be still the same.

PROBLEM V. *To find the Attraction which an Elliptical Spheroid* KLk (fig. 13) *exerts on a Corpuscle placed without its Surface at* N, *according to the Direction* CX *perpendicular to* CN.—10. To perform this, we will begin by drawing the diameter $C\mu\nu$, which bisects the lines Rr perpendicular to CN; and the ratio of CH to HY shall be called n. Then accounting the ellipsis Rr as a circle, see the foregoing article, we shall have, by the foregoing problem, $\frac{\frac{4}{3}nc \times RH^2 \times CH}{NR^3}$ for its attraction, according to HY; which being multiplied by the fluxion of MH, the fluent of this will be the attraction of the segment of the spheroid RMr.

This calculation being made, and Nm being substituted for NR, we shall have $\frac{2ncr^5}{5e^4}$ for the attraction of the spheroid in N, according to the direction CX.

PROBLEM VI. *To find the Attraction of a Corpuscle* N, *according to* CX, *towards an Ellipsoid* BNEbe, *composed of Strata, the Densities of which are defined by the Equation* $D = fr^p + gr^q$.—11. Take the fluxion of the quantity $\frac{2cnr^5}{5e^4}$, which expresses the attraction of the homogeneous ellipsoid KLk, and you will have $\frac{2cnr^4\dot{r}}{e^4}$ for the attraction of an infinitely little elliptic orb; which being multiplied by the density D, gives $\frac{2cnfr^{4+p}\dot{r}}{e^4} + \frac{2cgnr^{4+q}\dot{r}}{e^4}$, the fluent of which $\frac{2cfnr^{5+p}}{5+p \times e^4} + \frac{2cgnr^{5+q}}{5+q \times e^4}$, is the attraction of the spheroid KLk, according to CX. Therefore the total attraction of the spheroid BNEbe on the corpuscle N, according to the direction CX, will be $\frac{2cfne^{1+p}}{5+p} + \frac{2cgne^{1+q}}{5+q}$.

Now if we have regard to the smallness of the line Nν, and observe how little angle νNC will differ from a right one, we may perceive that the diameter CN contains the same angle with the perpendicular NX in N, as the diameter CN with the perpendicular at ν; that is, that the angle NCν is the same as the angle CNX; so that instead of n we may take $\frac{\text{CX}}{\text{CN}}$. Therefore the foregoing expression of the attraction of the ellipsoid BEbe, acting according to the direction CX, on a corpuscle placed in N, will be $\frac{2cfe^{1+p}}{5+p} \times \frac{\text{CX}}{\text{CN}} + \frac{2cge^{1+q}}{5+q} \times \frac{\text{CX}}{\text{CN}}$.

PROBLEM VII. *To find the Direction of the Attraction of a Corpuscle* N *towards the Ellipsoid.*—12. By the second Problem we shall find the attraction of the spheroid according to CN to be $\frac{2cfe^{1+p}}{3+p} + \frac{2cge^{1+q}}{3+q}$, by expunging what may be here expunged. Then by taking a 4th proportional to these 3 quantities, the first of which is the attraction according to CN, the 2d is that according to CX, and the third is the right line CN; there will arise

$$\frac{\frac{fe^{1+p}}{5+p} + \frac{ge^{1+q}}{5+q}}{\frac{fe^{1+p}}{3+p} + \frac{ge^{1+q}}{3+q}} \times \text{CX} = \text{CL}.$$

Whence we shall have NI for the direction required, of the attraction of the corpuscle N.

13. If we suppose $p = q = o$, that is, if the spheroid be homogeneous, we shall have CI $= \frac{3}{5}$CX; which agrees with what Mr. Stirling has found, in that curious dissertation he has published in the Philos. Trans. N° 438.

PART II. *The Use of the foregoing Problems, in finding the Figure of Spheroids, which revolve about an Axis.*—14. Let us now suppose, that the foregoing spheroid BNEbe, (fig. 13) which is still composed of strata of different densities, revolves about its axis Bb, and that it is now arrived at its permanent state. It is plain that the particles of the fluid, which are on its surface, must gravitate according to a direction perpendicular to the curvature BNE; for without this condition there could be no equilibrium.

We shall now inquire, whether the elliptic figure we have ascribed to our spheroids can have this property, and to produce this effect, what must be the relation between the time of revolution of the spheroid and the difference of its axes.

Let us then put ϕ for the centrifugal force at the equator, and the centrifugal force at N will be $\frac{\phi \times \text{PN}}{\text{CE}}$, or $\frac{\phi \times cx}{2\text{CE} \times \alpha}$, because 2PN $\times\ \alpha = cx$.

By resolving this centrifugal force according to the perpendicular to CN, we shall have $\frac{\phi \times CX}{2\alpha \times CE}$; to which adding $\frac{2cfe^{1+p}}{5+p} \times \frac{CX}{CN} + \frac{2cge^{1+q}}{5+q} \times \frac{CX}{CN}$, found by by prob. 5, will give the whole force of the body N, according to the direction CX, when the spheroid is turned about its axis. But because this body, by virtue of the attraction according to CN, and the force according to CX, ought to have a perpendicular tendency to the superficies; we shall have this analogy, $CN : CX :: \frac{2cfe^{1+p}}{3+p} + \frac{2cge^{1+q}}{3+q} : \frac{\phi}{2\alpha} \times \frac{CX}{CE} + \frac{2cfe^{1+p}}{5+p} \times \frac{CX}{CN} + \frac{2cge^{1+q}}{5+q} \times \frac{CX}{CN}$. And hence, because CN and CE may be assumed as the same on this occasion, it will be $\phi = \frac{8cfe^{1+p}\alpha}{\overline{3+p} \times \overline{5+p}} + \frac{8cge^{1+q}\alpha}{\overline{3+q} \times \overline{5+q}}$.

The Spheroid being supposed ellipical, Bodies will gravitate perpendicularly to its Surface.

And as in this value of the centrifugal force, no quantity enters but what will agree to any point N; we may therefore conclude, that when our supposed elliptical spheroid performs its rotation in a proper time, so that the centrifugal force at the equator may be as before; then the centrifugal force in any other place N will be such as it ought to be, to cause bodies to gravitate in a direction perpendicular to the surface.

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

15. If we now consider, that ED (fig. 14) being taken for the centrifugal force in E, then will MN express the centrifugal force in N, and consequently MI will be such a part of this force as acts according to NC; we shall have $\frac{8cfe^{1+p}\lambda}{\overline{3+p} \times \overline{5+p}} + \frac{8cge^{1+q}\lambda}{\overline{3+q} \times \overline{5+q}}$ to be subtracted from the attraction at N. Hence $\frac{2cfe^{1+p}}{3+p} + \frac{\overline{2p-10}cf\lambda e^{1+p}}{\overline{3+p} \times \overline{5+p}} + \frac{8cf\alpha e^{1+p}}{\overline{3+p} \times \overline{5+p}} + \frac{2cge^{1+q}}{3+q} + \frac{\overline{2q-10}cg\lambda e^{1+q}}{\overline{3+q} \times \overline{5+q}} + \frac{scg\alpha e^{1+q}}{\overline{8+q} \times \overline{5+q}}$ will be the gravity at N.

The Gravity at the Equator.

16. In this value making $\lambda = \alpha$, we shall have $\frac{2cfe^{1+p}}{3+p} + \frac{\overline{2p-2}cf\alpha e^{1+p}}{\overline{3+p} \times \overline{5+p}} + \frac{2cge^{1+q}}{3+q} + \frac{\overline{2q-2}cg\alpha e^{1+q}}{\overline{3+q} \times \overline{5+q}}$ for the gravity at the equator.

17. If we subtract the value of the gravity in N from the value of the at traction or gravity at the pole, in art. 3, we shall have

$\frac{\overline{10 - 2pcf\lambda e}^{1+p}}{3+p \times 5+p} + \frac{\overline{10 - 2qcg\lambda e}^{1+q}}{3+q \times 5+q}$. But it is easy to perceive, that λ is proportional to the square of the sine of the arc PM, or of the complement of the latitude. Whence we may therefore conclude, that the diminution of the gravity from the pole to the equator, is proportional to the square of the cosine of the latitude; or, which is the same thing, that the augmentation of gravity from the equator to the pole, is as the square of the sine of the latitude, as Sir Isaac Newton has demonstrated in his hypothesis of a homogeneous spheroid.

18. From the following calculation it is easy to conclude, that Sir Isaac's theorem, (Prin. Math. lib. 3, prop. 20) which is this, that the gravity in any place within, is reciprocally as the distance from the centre, cannot obtain here. For we may see by the foregoing expression, that the gravity in N cannot be to the gravity in P, as 1 to $1 + \lambda$, except when $p = q = 0$, which happens only in Sir Isaac's homogeneous spheroid.

It was for want of considering, that this theorem was demonstrated by Sir Isaac only in the case of his homogeneous spheroid, that several geometricians have too hastily concluded, that this theorem might be applied to determine the ratio of the earth's axes, and the lengths of the pendulum observed in two places of different latitudes. Dr. Gregory is one of those who have fallen into this mistake, in his Elements of Astronomy, lib. 3, sect. 8, prop. 52. And in the Philos. Trans. N° 432, it is concluded, from the proportion of gravity at Jamaica to that at London, that the diameter of the equator must exceed the earth's axis by the 190th part, which computation was founded on this 20th proposition, lib. 3, of Sir Isaac's Principia, which is true only of his spheroid.

The Manner of finding the Axes of the Spheroid, the Variation of the Densities of the Strata being taken at pleasure.

19. Let us now suppose, that the centrifugal force at the equator is known by observation, as also within the earth, &c. and that it is a certain part, as the mth part of the gravity; by articles 14 and 16, we shall have this equation:

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

From hence it will be easy to derive the value of α, because f, g, p, q, will be given, from the hypothesis that will be chosen, for the variation of the density in the internal parts of the spheroid.

20. And if on the contrary α be given, that is, if we know by observation the ratio of the axes of the planet concerned; then by the foregoing equation we may perceive, whether we have assumed an agreeable hypothesis for the variation of the densities: but we cannot precisely determine what this hypo-

thesis must be, because there is but one equation, in which 4 indeterminate quantities f, g, p, q, are involved. And indeed there might be many more than 4 indeterminate quantities, if we should assume more than two terms in the general equation of the densities $D = fr^p + gr^q + hr^r$, &c.

21. In order to apply the foregoing theory to the earth, it might seem at first sight, that by the assistance of observations made for measuring the length of the pendulum, we might have other equations, which with the foregoing equation A, would determine the coefficients and exponents now mentioned; but we shall soon see the impossibility of this, on two accounts: first, there need be only two observations, as to what concerns the length of the pendulum. For because, by art. 17. the augmentation of the gravity from the equator to the pole, is proportional to the square of the sine of the latitude, two observations as much determine the problem as an infinite number can do: so that we could have but one other equation besides the foregoing. This equation will be (B) $\frac{\acute{p}-p}{p} = \dfrac{\dfrac{\overline{5-p}f\alpha}{\overline{3+p}\times\overline{5+p}} + \dfrac{\overline{5-q}g\alpha}{\overline{3+q}\times\overline{5+q}}}{\dfrac{\overline{p-1}f\alpha}{\overline{3+p}\times\overline{5+p}} + \dfrac{f}{3+p} + \dfrac{g}{3+q} + \dfrac{\overline{q-1}g\alpha}{\overline{3+q}\times\overline{5+q}}}$.

The first member of this equation expresses the gravity at the equator subtracted from the gravity at the pole, and divided by the gravity at the equator; a quantity which may be known in numbers, by determining the length of the pendulum at two different latitudes. The other member of the equation is an expression of the same quantity, as it is deduced by the preceding calculus.

Secondly, This new equation B cannot be of any service in determining the coefficients and exponents f, g, p, q, &c. For we shall now show, that the foregoing ratio $\frac{\acute{p}-p}{p}$ has such an immediate connection with α, that one of them being determined, the other will necessarily be so too, independently of the values of f, g, p, q, &c. This may deserve our attention, and the proof is thus:

The Figure of the Spheroid being known, the Augmentation of Gravity from the Equator to the Pole will be known also; *and so vice versa.*

22. Because the ratio of the gravity to the centrifugal force is very great, and is expressed by m, in the equation A we may reject the third and fourth terms; by which means the equation will be reduced to this, $\frac{f}{3+p} + \frac{g}{3+q} = \frac{4mf\alpha}{\overline{3+p}\times\overline{5+p}} + \frac{4mg\alpha}{\overline{3+q}\times\overline{5+q}}$. And if from this equation we deduce the value either of f or g, and substitute it in the equation B; having first rejected the first and fourth terms of the denominator, as in this case may be done; we shall have, after the calculation is made, whatever is the number

of terms in the equation of the densities, $\frac{\acute{p}-p}{p} = \frac{10}{4m} - \alpha$, or $\frac{\acute{p}-p}{p} = \frac{1}{115} - \alpha$, by putting 288 for m, as has been long known. It is easily seen from this equation, that when α is determined, $\frac{\acute{p}-p}{p}$ will be so too, which was the thing proposed to be proved.

23. But from this equation there follows a very singular proposition, and which, in some sort, is contrary to the sentiments of Sir Isaac Newton, page 430 of the 3d edition of his Principles. And this is, that if by observation it shall be discovered, that the earth is flatter than according to the spheroid of Sir Isaac, that is, if the diameter of the equator exceeds the axis by more than the 230th part, the gravity will increase less from the equator towards the pole, than according to the table which he has given for his spheroid, prop. 20 of the 3d book. And on the contrary, if the spheroid is not so flat, the gravity will increase more from the equator towards the pole.

24. It is thus that Sir Isaac Newton expresses himself about it, when he relates the experiments made towards the south, concerning the diminution of gravity, which experiments make it greater than his theory requires. He affirms, that the earth is denser towards the centre than at the superficies, and more depressed than his spheroid requires. But by the foregoing theory we may easily perceive, that if the density of the earth diminishes from the centre towards the superficies, the diminution of gravity from the pole towards the equator will be greater than according to Sir Isaac's table; but at the same time the earth will be not so much depressed as his spheroid requires, instead of being more so, as he affirms. Yet I would not by any means be understood to decide against Sir Isaac's determination, because I cannot be assured of his meaning, when he tells us, that the density of the earth diminishes from the centre towards the circumference. He does not explain this, and perhaps instead of the earth's being composed of parallel strata, its parts may be conceived to be otherwise arranged and disposed, so as that Sir Isaac's proposition shall be agreeable to the truth.

25. As to Dr. Gregory, who has attempted to comment on this passage of Sir Isaac, I think I have demonstrated, that he has committed a paralogism. He says (Element. Astronom. lib. 3, § 8, prop. 52 schol.) that if the earth is denser towards the centre, or if, for example, it has a nucleus of greater weight than the other parts, the diminution of gravity from the pole towards the equator shall be greater than if the whole were of the same density; and in this he is right. But he is in the wrong, I think, immediately to conclude from thence, that the earth has a greater flatness. Whence can he conclude this?

it can be only from that proposition of Sir Isaac which informs us, that gravity is in a reciprocal ratio of the distances: because he gave us the proposition but the page before, as a method for determining the figure of the earth. But we are not allowed to make use of this proposition in this case, because it has been shown, art. 18, that it can take place only on the supposition of a homogeneous spheroid. Therefore, &c.

26. It will not be very difficult, without any regard had to the foregoing theory, to find the ratio of the axes of a spheroid, which we may suppose to have a nucleus at the centre, of greater density than the rest of the planet; and hence we shall be easily assured of Dr. Gregory's mistake.

27. Setting aside all attraction of the parts of matter, if the action of gravity is directed towards a centre, and is in the reciprocal ratio of the squares of the distances, the ratio of the axes of the spheroid will then be that of 576 to 577: and the gravity at the pole is greater than at the equator by the 144th part, or thereabouts. Which may be a confirmation of what is here advanced, especially to such as will not be at the pains of going through the foregoing calculations. For we may consider the spheroid now mentioned, in which gravity acts in a reciprocal ratio of the squares of the distances, as composed of matter of such rarity, in respect of that at the centre, that the gravity is produced only by the attraction of the centre or nucleus.

28. In the foregoing calculations, in order to find the axes of our spheroids, and to know whether their figure makes a sensible approach to that of the conical ellipsis, we have had recourse to this principle, that gravity ought always to act in a direction perpendicular to the surface. Two reasons have prevailed with us to make use of this principle, rather than the other, which consists in the equilibrium of the columns. The first is, because the calculations founded on it are more simple. The second is, that considering the state of the actual solidity of the earth, it should seem as if this principle were the more indispensably necessary. However, because Sir Isaac Newton, and all the other philosophers, who have treated about the figure of the earth, have taken it, as it were, at its first formation, at which time they suppose it to have been fluid: we shall here make the same supposition, and we shall assume no other ratio for that of the two axes, than that of the spheroid which results from a coincidence of these two principles.

We shall begin by inquiring what is the whole weight of any column CN, fig. 15. To do this, we must resume the expression of the attraction in any point M of the column CN; then multiply it by $\dot{r} + \lambda\dot{r}$, and by the density $fr^p + gr^q$, and afterwards we must find the fluent. Thus we shall have

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

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

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

$$+ \frac{\overline{8+4p}cgf\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}} + \frac{\overline{8+4q}cfg\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}}$$ for the total gravity of any column CN, having regard only to the attraction.

29. If in this expression we make $\lambda = 0$, we shall have the gravity of the column at the pole.

30. And if we make $\lambda = \alpha$, we shall have the aggregate of the attractions of the column at the equator.

31. Now because the column CN is in equilibrio with the column CB; it follows from thence, that if we subtract the weight of the column CB, from the aggregate of the attractions of the column CN, the residue must be equal to the sum of the centrifugal forces of the colnmn CN. Now to endue our spheroids with this property, we will resume the expression of the centrifugal force in E, which we found art. 14, which will give

$\left(\frac{8cfe^{1+p}\lambda}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{1+p}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\frac{r}{e}$, for that part of the centrifugal force which acts according to CM, in any place M, by expunging the terms in which $\alpha\alpha$ would be found. This value being multiplied by $\dot{r}$, and by the density, will give, when we have taken the fluent, $\frac{8cf^2e^{2+2p}\lambda}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}} + \frac{8cfge^{2+p+q}\lambda}{\overline{2+p}\times\overline{3+q}\times\overline{5+q}}$

$+ \frac{8cfge^{2+p+q}\lambda}{\overline{2+q}\times\overline{3+p}\times\overline{5+p}} + \frac{8cg^2e^{2+2q}\lambda}{\overline{2+q}\times\overline{3+q}\times\overline{5+q}}$ for the sum of the centrifugal forces of the column CN, still expunging those terms in which either $\alpha\alpha$ or $\lambda\lambda$ are found.

Then making this expression equal to $\frac{\overline{4+2p}cf^2e^{2+2p}\lambda}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}} + \frac{\overline{8+4p}cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}}$

$+ \frac{\overline{8+4q}cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}} + \frac{\overline{4+2q}cg^2e^{2+2q}\lambda}{\overline{1+q}\times\overline{3+q}\times\overline{5+q}}$, which is the difference of the weight of the column at the pole CB, from the sum of the attractions of the column CN, we shall have the equation

$$\frac{ppff}{\overline{1+p}\times\overline{2+p}\times\overline{3+p}\times\overline{5+p}} + \frac{2pqfg}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}\times\overline{2+q}}$$

$$+\frac{2pqfg}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}\times\overline{2+p}}+\frac{qqgg}{\overline{1+q}\times\overline{2+q}\times\overline{3+q}\times\overline{5+q}}=0,$$

where we have put $e=1$, for the greater simplicity of calculation.

Determination of such spheroids, as make the principle of the equilibrium of the columns, and that of gravity perpendicular to the surface, to coincide with each other.

32. This equation informs us, that when out of all the infinite varieties, which will be supplied by the equation of the densities $D = fr^p + gr^q + hr^t$, &c. we shall have taken at pleasure all the coefficients, and all the exponents, one only excepted; if this last be such in respect of the others, that it may fulfil the conditions of the foregoing equation, the spheroid, being supposed in a state of fluidity, will be in equilibrio, because it will unite as well the principle of a perpendicular tendency to the surface, as that of an equipoise of the several columns.

33. Before concluding this paper, I shall make a few reflections on the principles we have now made use of, for determining the figure of a spheroid revolving about its axe.

The first principle which, after Mr. Huygens, we have had recourse to, and which consists in making bodies gravitate perpendicularly to the surface, seems to me of absolute necessity. For if there were never so little water on the surface of the earth, it could not be at rest, if it had a tendency any how inclined to the surface.

The second principle, made use of by Sir Isaac Newton, and which consists in an equilibrium of the columns CE, CN, CP, could be thought necessary I think, only for these two reasons; the first is that which is usually assigned, that at the first formation of the earth, it was probably in a state of perfect fluidity; in which case it must acquire such a figure, as will result from the equilibrium of the columns, and from the gravitation acting perpendicularly to the surface. Indeed though this reason has a degree of plausibility, yet there are many who think it to be of small force. Perhaps, say they, the earth has never been in this fluid condition.

The second reason, which I believe will have a greater weight with every body, is this: considering the earth as it is at present, and without carrying our thoughts so far back as to its formation, if the ocean, which is now on its surface, has any considerable depth, and if its parts preserve a communication with each other, from region to region, by subterraneous canals; it can only keep an equilibrium by this means, because its superficies is the same as it would have, were the whole a fluid.

34. This second reason has suggested a reflection to my mind, concerning

the equipoise of the columns now calculated, art. 31 and 32. Let us first suppose, that the earth is our fluid spheroid, composed of layers of different densities; and that afterwards this fluid hardens into a solid, so that the different strata, of which it is made up, are of no other use but to cause a gravity by their attractions. Then let us suppose, that the seas and great waters about the earth have a communication with each other, by means of some subterraneous canals. As the waters of the sea, which unite with each other, are probably homogeneous, the foregoing calculation, in which we have considered the spheroid as a fluid, can no longer take place, because we have there supposed, that the fluid contained in the canal BCN is of a density, that varies from the centre to the circumference. From hence it seems to me, we must undertake the computation of the equilibrium of the columns after another manner, thus: We must examine whether two canals, as CN and BC, which are filled with a homogeneous fluid, will be in equilibrio, all the other parts of the spheroid continuing as above.

35. To do this, we will begin with finding the gravity of any column CN, fig. 15, arising from attraction alone. First then, we must resume the expression of the attraction in any point M, art. 7. Then we must multiply it by $\dot{r} + \lambda\dot{r}$, which will give

$$\frac{2cfr^{1+p}\dot{r}}{3+p} + \frac{\overline{8+4p}\,cf\lambda r^{1+p}\dot{r}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha r^{1+p}\dot{r}}{\overline{3+p}\times\overline{5+p}} + \frac{2cgr^{1+q}\dot{r}}{3+q}, \text{ \&c.}$$

And taking the fluent of this quantity, we shall have

$$\frac{2cfe^{2+p}}{\overline{3+p}\times\overline{2+p}} + \frac{4cf\lambda e^{2+p}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha e^{2+p}}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}} + \frac{2cge^{2+q}}{\overline{3+q}\times\overline{2+q}},$$ &c. for the gravity of the whole column CN.

36. If in this value we make $\lambda = 0$, we shall have the gravity of the column at the pole.

37. And if we subtract the gravity of the column at the pole, from the whole sum of the attractions of the column CN, we shall have

$$\frac{4cf\lambda e^{2+p}}{\overline{3+p}\times\overline{5+p}} + \frac{4cge^{2+q}\lambda}{\overline{3+q}\times\overline{5+q}},$$ which must be equal to the sum of the centrifugal forces of the column CN, in order that the columns CB and CN may be in equilibrio.

But we shall find this really to obtain. if we resume the quantity

$$\left(\frac{8cfe^{1+p}}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{1+q}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\frac{r}{e},$$ which, by art. 31, expresses that part of the centrifugal force in M, which acts according to CM. Then multiplying this expression by $\dot{r}$, and seeking the fluent, we shall have

$\frac{4cfe^{2+p}\lambda}{\overline{3+p} \times \overline{5+p}} + \frac{4cge^{2+q}\lambda}{\overline{3+b} \times \overline{5+b}}$ for the aggregate of the centrifugal forces of the column CN. And this being the same as the foregoing, shows that the columns CB and CN are in equilibrio, supposing them to be homogeneous; nor are we here obliged, as in art. 32, where we consider them as heterogeneous, to suppose the coefficients f, p, &c. to have any certain relation among one another.

38. Perhaps it may be urged, that the foregoing calculus agrees only to a canal, as BCN, which passes through the centre; and that we ought to prove in the same manner, that the water included in any other canal pqr, would observe an equilibrium. But it appears to me, that this property may be derived from the former; for it follows, from the foregoing calculation, that if we might be allowed to make this hypothesis, viz. that independently of the attraction of any matter, the gravity at any distance CN from the centre, see fig. 15, would be proportional to $\frac{2cfc^{1+p}}{3+p} + \frac{\overline{2p-2}cf\lambda e^{1+p}}{\overline{3+p} \times \overline{5+p}} + \frac{8cf\alpha e^{1+p}}{\overline{3+p} \times \overline{5+p}}$, &c. it is plain from thence, that a mass of the homogeneous fluid, which should turn about the axis CB, would assume the same form as that of our heterogeneous fluids. But if this spheroid should then put on a fixed state, except only some canal pqr, the water in this canal would be in equilibrio; for without this, the spheroid could not be esteemed as having arrived to its fixed state. But this supposition comes to the same as that of our heterogeneous spheroid, composed of elliptical layers, in which should be found a canal pqr of a homogeneous fluid; provided that the space, which this canal possesses in the globe, be not of so large an extent, as to change the law of attraction.

The only three planets, in which we can be assured of gravitation, and the centrifugal force, are the sun, Jupiter, and the earth. As to the sun, the centrifugal force is there so small, in respect of its gravity, that his poles must be very little depressed, so that we cannot be sensible of it by observation. Then as to Jupiter, observations make him something less flat than according to Sir Isaac Newton; that is, than if he were composed of matter of a uniform density. Therefore by the foregoing theory, he must be a little more dense towards the centre, than at the parts near the superficies. We might make a thousand hypotheses about the manner of distributing the inequality of density, proceeding from the centre towards the circumference, which would all agree with the figure observed, and which are very easy to calculate by the principles here laid down.

As to what concerns the earth, I shall wait till we receive the observations which must have been lately made in Peru; that by comparing those with what observations we have made under the arctic circle, and with those of Mr. Picart in France, we may have the true difference of the earth's diameters at the equator and at the poles. Then our theory may be applied, to determine whether the earth is more or less dense at the central parts than at the surface,* or whether it be every where of a uniform density, as it ought to be, if, without admitting very gross errors in the observations, it may be concluded, that the earth is really the spheroid of Sir Isaac Newton; and this case would be the simplest and the most natural of all.

I am here obliged to acknowledge, that if the observations we have made in the north may be relied on,† and if we must admit as incontestable as well the measure of a degree as the length of the pendulum, the foregoing theory could not be reconciled to the phenomena. For it follows from our observations, that the diameter of the equator must exceed the earth's axis by more than the 230th part; and that the gravity at the pole must be greater than that at the equator by more than the 230th part also; which will by no means agree with what we have deduced in art. 23.

As to what concerns the measure of gravity in Lapland, as being not so liable to error as the measuring a degree; the earth may be not quite so flat as Sir Isaac's spheroid requires. By the table of the length of the pendulum, exhibited in the treatise concerning the figure of the earth, published this year by Mr. de Maupertuis, and by art. 22 of the present discourse, the earth may be more elevated at the equator than at the pole by the 266th part, or thereabouts. After the true quantity of the earth's flatness shall be fully settled, if it should be found to have this figure, I should be apt to think it is a little more dense at the centre than towards the superficies. But if on the contrary we should be well ascertained, that the earth is raised higher at the equator than at the pole, by above the 230th part: and if, for any sufficient reason, we may something shorten the length of the pendulum that beats seconds in the north; there would be some grounds to allow, that the earth is not so dense at the central regions as at those near the surface. But if it shall happen, that we can neither diminish the length of the pendulum, nor the excess of the equatorial diameter above the axe, I must then give up my hypothesis. Yet I shall think it may be of some use to have thus discussed it, because possibly no one would have imagined what might have been the result of it. It appears that

* It has since been determined, by Dr. Hutton's calculations on the observations made at mount Schehallien, in Scotland, that the mean density of the earth, is about 2 times more than at the surface; and that it is therefore probable the central parts of the earth are still much more dense. See Philos. Trans. vol. 68, part 33.

† It has since been found very erroneous. See p. 287, of this volume of Abridgments.

even Sir Isaac Newton was of opinion, that it was necessary the earth should be more dense towards the centre, in order to be so much the flatter at the poles; and that it followed from this greater flatness, that gravity increased so much the more from the equator towards the pole.

New Experiments on Ice. By the Abbè Nollet, F. R. S. at Paris.* N° 449, p. 307.

1. Ice that begins to melt, and water that begins to freeze, have always the same degree of cold.

2. That cold may be increased by a mixture of salts.

3. It has been thought for a long time, that saltpetre was most fit to increase the cold of ice; but experiments have shown, that few salts increase cold so little as that salt. Mix one part of fine saltpetre with two parts of beaten ice, and Mons. Reaumur's thermometer will descend in it but $3\frac{1}{2}$ degrees below the freezing point.

What had caused this mistake, is, that people generally made use of saltpetre of the first or second melting, as being the cheapest; but that saltpetre, not being purified, contains a great deal of sea-salt; and it was in proportion to the quantity of the sea-salt that the effect was the greater.

From this last observation, one may deduce an advantageous method for trying gunpowder; for as of the three ingredients of which it is made up, salt-petre is the only one that can increase the cold of ice; if one part of gunpowder, or a little more, be mixed with two parts of ice; and it increases its cold more than $3\frac{1}{2}$ degrees, it is a sign that the salt-petre contained in it is not well purified; and the best powder will be that which least increases the cold of ice.

4. Sea-salt, that is the Bay-salt, which is commonly used at table in France, and that which is immediately taken from the mines, called sal gemmæ, usually gives the greatest degree of cold; for pot-ash gives sometimes a little more, but generally less. Sea-salt mixed with ice in the abovesaid proportion, gives 15 degrees of cold on Mons. Reaumur's thermometer, and sal-gem. 17.

5. Ashes of green wood .... 3 deg.
6. .... Sea-coal ....
7. .... Vitriol 2
8. .... Tartar 10
9. .... Common pot-ash (in French called soude ordinaire) 3
10. .... Pot-ash made of sea weed 11

This last pot-ash may be substituted instead of sea-salt, for making ice-

* The Abbè Nollet acquired a great degree of celebrity by his lectures and writings on experimental philosophy, and particularly by his experiments and observations on electricity. He was a member of the French Academy of Sciences, of the Royal Society of London, and of various other scientific institutions. He died at Paris in 1770, at the advanced age of 70.

creams, in places where salt is dear, as in France, where it is sold for 10 sols a pound. 1st, Because in France this pot-ash is sold only for $2\frac{1}{4}$ sols a pound. 2dly, Because, not freezing so fast, it does not spoil the creams by reducing them to icicles. 3dly, Because ice-creams made this way, will keep longer in a condition fit to serve at table.

11. Sugar	4 deg.
12. Alum	$1\frac{1}{2}$
13. Salt of glass	10
14. Sal ammoniac	$12\frac{2}{3}$
15. Quick-lime	$1\frac{1}{4}$
16. Glaubers salt	2

17. The cold of ice may still be considerably increased by a mixture of spirit of wine; about a drinking-glass full of spirit of wine to a pound of beaten ice.

18. The cold of ice will not increase, unless the ice melts.

Experiments.—Put into one vessel 4 ounces of ice, beaten very small, and into another vessel 2 ounces of sea-salt; set the two vessels in a mixture of ice and salt, which is to be renewed still, till by means of the thermometer you find, that the salt and the ice of the first two vessels have acquired each of them 10 or 12 degrees of cold; then mix the salt with ice, and this mixture will not increase the degree of cold that the ingredients had acquired, because the mixture does not melt. But if, instead of salt, you mix with the ice spirit of nitre, cooled to the same degree as the ice, as this last is liquid, it will melt the ice, and considerably increase its cold.

19. Salt mixed with water, increases its cold.

20. Of all salts, sal ammoniac gives the greatest degree of cold;* so that if that salt has been cooled in ice, and then one part of it be thrown into 2 parts of water, cooled to the same degree in ice, that water will become colder than ice, and will freeze other water thrown into it in a small quantity.

This last observation may be applied to the cooling of liquors where no ice is to be had; for there is hardly any place, but what has wells: now the water of a well moderately deep, wants about 8 or 10 degrees of the cold of ice; and sal ammoniac being cooled beforehand in the well, will, by mixing with some of the water of that well, come very near to the cold of ice.

An Observation of the Magnetic Needle being so affected by Cold, that it would not traverse; by Capt. Christopher Middleton, F. R. S. N° 449, p. 310.

In the Philos. Trans. N° 418, Capt. M. mentioned a strange phenomenon relating to the sea-compass, which he had frequently observed, when among

* Mr. Walker of Oxford has shown that there are other salts which are preferable to sal ammoniac for producing artificial cold, and particularly muriate of lime. See Phil. Trans. for 1795 and 1801.

the ice in Hudson's Bay; viz. that the magnetic virtue of the needle was so far lost or destroyed, that it would not traverse as usual, even when the ship was in a considerable motion: and in his voyage thither last year, he observed the compass would not move at all, any longer than the quarter-master kept touching it. There was then much snow on the land, and many isles of ice around them, and the sea not very smooth: he ordered one of the compasses to be brought into the cabin, but did not find it any better, till it had stood near the fire about $\frac{1}{4}$ of an hour, and then it began to traverse very well; he then ordered it to be placed in the binnacle, and another to be brought into the cabin, changing them alternately thus every half hour, and found by this means he could make them traverse as well as in any other part of the world. He was obliged to continue this practice, till near 100 leagues from the coast; but afterwards he had no occasion for that trouble. What should be the cause of this wonderful phenomenon he could not conjecture, being certain the compasses, as to their mechanical structure, were very perfect, and answered very well both before and after, during the whole voyage.

Concerning an uncommon Palsy of the Eye-lids. By Dr. Andrew Cantwell of Montpelier. N° 449, p. 311.

A lady, about 30 years of age, was troubled with a very singular disease. It was an intermitting periodical palsy of the eye-lids, which began every evening about 6 o'clock, with a defluxion from the great canthus, of a whitish matter of some consistence; so that she remained blind till next morning, and then recovered the use of her eye-lids as before. This disorder held her for 4 months; from which time all remedies ordered by her physician proving ineffectual, she was sent to Baleruc for the benefit of the waters.

As Dr. C. lodged in the same house, he had a fair opportunity of observing the effects the waters had on her. She was pumped on the back part of her head and neck 7 times, without receiving any sensible benefit: the 9th time her disorder seized her an hour later than usual, and the defluxion was less and thinner. The next evening it retarded 2 hours, and the following night she had as much command of her eye-lids as ever. She took the douche, for so they call that way of pumping, the next morning and evening, and was entirely cured. Dr. C. sat with her an hour that evening, carefully observed her eye-lids by candle-light, and asked her several questions on her disorder. She opened her eyes as well as he did his, and set out the next day for Montpelier.

An Account of the Man whose Arm with the Shoulder-blade was torn off by a Mill, Aug. 15, 1737. *By Mr. John Belchier, F.R.S.* N° 449, p. 313.

Samuel Wood, about 26 years of age, being at work in one of the mills near the Isle of Dogs, over-against Deptford, and going to fetch a sack of corn from the farther part of the mill, in order to convey it up into the hopper, carelessly took with him a rope, at the end of which was a slip-knot, which he had put round his wrist; and passing by one of the large wheels, the cogs of it caught hold of the rope, and he not being able to disengage his hand instantly, was drawn towards the wheel, and raised off the ground, till his body being checked by the beam which supports the axis of the wheel, his arm with the shoulder-blade was separated from it.

At the time the accident happened, he says he was not sensible of any pain, but only felt a tingling about the wound, and being a good deal surprized, did not know that his arm was torn off, till he saw it in the wheel: when he was a little recovered, he came down a narrow ladder to the first floor of the mill, where his brother was, who seeing his condition, ran down stairs immediately out of the mill to a house adjacent to the next mill, which is about 100 yards distant from the place where the accident happened, and alarmed the inhabitants with what had happened to his brother; but before they could get out of the house to his assistance, the poor man had walked by himself to within about 10 yards of the house, where, being quite spent by the great effusion of blood, he fainted away, and lay on the ground; they immediately took him up, and carried him into the house, and strewed a large quantity of loaf-sugar powdered into the wound, in order to stop the blood, till they could have the assistance of a surgeon, whom they sent instantly for to Limehouse; but the messenger being very much frighted, could not give the surgeon a clear idea of the accident, so that when he came to see the condition the man was in, he had no dressings with him for an accident of that kind; but had brought with him an apparatus for a broken arm, which he understood by what he could learn from the messenger to be the case. However, he sent home for proper dressings, and when he came to examine particularly into the wound, in order to secure the large blood-vessels, there was not the least appearance of any, nor any effusion of blood; so having first brought the fleshy parts of the wound as near together as he could by means of a needle and ligature, he dressed him up with a warm digestive, and applied a proper bandage: the next morning he opened the wound again, in company with 2 surgeons more; and not perceiving an effusion of blood at that time, he dressed him as before, and sent him in the afterneon to St. Thomas's Hospital, where he was admitted a patient under the care of Mr. Ferne; from which time he was constantly attended, in expectation of a hemorrhage of blood from the subclavian artery; but there being no appearance of fresh bleeding, it was not thought proper to remove the dressings during the space of 4 days, when Mr. Ferne opened the wound, at which time likewise there was not the least appearance of any blood-vessels; so he dressed him up again, and in about 2 months time the cure was entirely completed.

On examining the arm, within a day or two after it was separated from the body, they found the scapula fractured transversely, as were likewise the radius and ulna in 2 places: but whether these bones were fractured before the arm was torn off, the man cannot possibly judge. The muscles inserted into the scapula were broken off near their insertions, but the muscles arising from the scapula came away with it entire. The latissimus dorsi and pectoralis, were likewise broken off near their insertions into the os humeri. The integuments of the scapula, and upper part of the arm, were left on the body, as also the clavicle.

But it is very surprising, that the subclavian artery, which could never be got at to be secured by art, should not bleed at all after the first dressing; the artery being separated so happily, that when the coats of it were contracted, the fleshy parts pressed against the mouth of it, and prevented any effusion of blood.

An Account of a Bullet which lodged near the Gullet for almost a Year. By George Lord Carpenter, F. R. S. &c. N° 449, p. 316.

The late Lord Carpenter was wounded in the mouth, at the defence of the breach of Brihuega in Spain, by a small Spanish musket-ball, which having taken away part of his upper lip, beat out all his teeth, except 2, on one side, broke and splintered part of his upper jaw-bone, went through his tongue, and lodged itself near his gullet, where it remained 51 weeks and 3 days, before it was extracted, the surgeons thinking it had been spit out with some of his teeth soon after his being wounded.

The ledge which was made on the bullet by the 2 fore teeth, lying almost by the gullet, and continually grating on it, occasioned an intolerable pain, and preventing him from swallowing any thing but liquids, it brought him so low, that his life being despaired of, to make a final trial, his tongue was drawn out as far as it could be, and one of the surgeons feeling the ball with his probe, which he then took to be a piece of a tooth, several pieces of teeth having been beaten into his tongue by the bullet, and endeavouring to extract it, he took hold of the ledge with his forceps, and drew the ball out; after which he recovered in a few weeks.

The marks of the fore teeth are to be seen on the bullet, and where it flatted upon the jaw-bone.*

An Obstruction of the Biliary Ducts, and an Imposthumation of the Gall-Bladder, discharging upwards of 18 *Quarts of bilious Matter in* 25 *Days, without any apparent Defect in the Animal Functions. By Claud. Amyand, Esq. F.R.S.* N° 449, p. 317.

Mr. La Grange, about 50 years of age, of a sallow bilious complexion, died of an abscess in the vesica fellis.

Dr. Vatas, his physician in ordinary, reports, that about 14 years before, this gentleman was afflicted with a tertian ague, which was cured by the bark, and from that time had complained of a sense of weight, and some uneasiness and hardness in the region of the liver and borborygmi, which were relieved by frequent purgations; notwithstanding which, he had enjoyed all the appearances of health, till about 4 months before his death, when some symptoms of the jaundice first began to appear on him, which had greatly increased 5 or 6 weeks before he died, when he began to complain of shooting pains on the right hypochondrium; which was soon followed with a hard inflammatory tumour there, tending to suppuration.

May 4, Mr. A. met Dr. Vatas, and Mr. Fiquel, his surgeon, in order to open a large abscess pointing below the cartilages of the 2d and 3d spurious ribs on the right side. It was determined to open it immediately with a lancet, upon which a pint of a purulent fetid matter was discharged. The aperture being large, and the dressings easy, by the next day a very large quantity of sanies, and some pus left in the bag, had found a vent; and this was so great, that it was thought proper to renew the dressings twice a day.

This had the desired effect so far, that from this time the matter daily decreased, till the 12th of May, when during the night the wound had discharged near 2 quarts of matter of a saffron colour, intermixed with large flakes and thick lumps of a coagulated lymph or jelly, tinged of a deep yellow; and it was surprising that on dressing they made way for the discharge of about a quart more of the same, as they enlarged the orifice of the bursted bag, to

* A very extraordinary narrative of a gun-shot wound, is to be seen in N° 320, of the Philos. Trans. It is the case of one Dr. Feilding, who was shot in near the eye, and after 29 years the bullet was cut out near the pomum adami.—Orig.

favour the coming out of the large flakes and lumps of jelly, obturating at times this orifice. During this day the discharge was very great, and at night about a pint more of the same matter was emptied.

From this time a short and thick canula was left in the opening of the bursted bag, this causing a more easy and constant discharge, and a vulnerary injection, strongly saturated with spirit of wine, had the good effect to diminish it very considerably; but yet it continued so very great, that they apprehended the patient would soon sink under so great a flux of this bilious matter, and the rather that his stomach and his rest failed him; but the discharge daily lessening, and his appetite and rest returning in proportion, he recovered strength enough to be able to walk.

All this while the appearances of the jaundice were wearing off, the urine was returned to its natural colour, and the patient had regularly a natural stool every day, till about 8 days before his death, when his body becoming costive, the physician found it necessary to discharge the fæces, by clysters and lenient purges. Whilst Mr. A. attended him, his belly was always free from fulness or tension, being soft and lank, and he was less troubled with wind, than he had been for many years before. Two days before he died, he went to air himself in another room, and caught cold: this is presumed to have occasioned a fever, followed with a lethargy, in which he continued till the 29th of May, when he died.

On dissection, it was observed, that the patient was not near so extenuated as might have been expected, after so great a discharge of bile and lymph during 25 days; for much fat was yet observed under the skin and elsewhere, and his flesh not much sunk from the natural state, but the blood-vessels were found extremely empty. In the abdomen, the caul or omentum was shrivelled up, and adhered to a large bag or cystis, affixed to the inside of the great lobe of the liver, and stretching from thence along the right flank, over one half of the kidney on that side. The left lobe of the liver was removed from the left side to the right, not reaching farther than the right edge of the cartilago ensiformis, and the pylorus: the ligamentum latum suspensorium hepatis, was drawn backwards into the right hypochondrium. The liver was of a natural colour, but very small, and more decayed and wasted in proportion than the other viscera, but as free as they from any preternatural adhesion, obstruction, or induration, and the bag or cystis, arising from it, strongly adhering by its outside only, to the peritoneum, down to the right kidney.

On passing a finger through the wound in the integuments, it entered first into a cavity made between the peritoneum and the outside of the cystis, in which the matter of the abscess had been lodged, and then through a hole in the cystis, or grand bag, through which the great collection of bile in this saccus had afterwards made its way; and it was observed, that the strong coalition of this bag to the peritoneum, round that part where the pus had been collected, had shut up all communication with the cavity of the belly, and so prevented any extravasation into the abdomen.

Now the bag or cystis being separated from the peritoneum, and this and the liver spread on a board, it was observed that the matter had been collected in the gall-bladder, without affecting the liver itself. The vesica fellis was become a very large bladder, and extended so as then to appear capable of containing 3 pints, or more; it was nearly as broad as long: it arose very broad from the surface of the right lobe of the liver, which it occupied about 10 inches in circumference, or more: its bulk had removed the stomach and pylorus from their natural situations, and pressed them far under the left hypochondrium, and that part of the colon placed naturally on the right kidney, forwards on the spine: its surfaces were rugged and unequal, as that of a potatoe, and its coats thick and horny, forming several tumours, elongations or expansions, of different sizes and figures; one of which, as large as a hen's egg, was full of a cretaceous matter, intermixed with hard white stones. This cretaceous bag was made in the duplicature of the vesica fellis, but had no communication with nor opening into it, which several other tumours, appearing of the same kind had; whence it was presumed that some very small pieces of creta, found in the great bag, might have dropped from them into it; but it is more likely they had dropped them there, because nothing like them had been discharged through the wound. The outer opening in this bag answered in the cavity of the abscess, where incision had been made, as this latter was formed between it and the peritoneum. In the bag were found about 2 oz. of the same bilious matter which had all along been discharged; which being computed, must be equal to, if not exceed, the quantity of 18 or 20 quarts, during the 25 days the patient lived, from and after the opening of the tumour.

It has been observed, that the liver was in a natural state, and that the matter collected in the vesica fellis, had not in the least wounded or affected the liver itself; so that the great quantity of bile and lymph daily discharged through the incision, must have proceeded from the internal surface of the distended gall-bladder. This put them on inquiring for the radices cysticæ and hepatico-cystic ducts; i. e. for those very ducts which Giovanni Caldesi so carefully traced in several animals, and delineated in his Observ. Anatomiche al illustr. Sig. Francisco Redi 1687, and which Verheyen discovered in the bullock kind, but could not trace in man; these ducts, by which so great a quantity of gall had been deposited in the vesica fellis, for as much that the cystic duct was obturated, by which some anatomists have supposed the gall to flow back from the hepatic ducts; and on dissection, Mr. A. traced a trunk, like to that which Bidloo and Winslow observed in man; and resembling that formed by several branches in the liver, and discharging itself into the vesica. He would gladly have traced this further, but the time allowed for dissection did not permit him to pursue this inquiry.

The ductus communis choledochus was found empty, and opening, as usual, into the cavity of the duodenum; but the cystic duct was so compressed by the bag, that nothing could pass through it. The spleen, pancreas, and all the other viscera, were in a natural state, except that some of them had changed and altered their natural situations.

On the whole it appears, 1st, That the animal functions have been in nowise vitiated by some of the viscera having been displaced; and notwithstanding that for 25 days, the discharge of the bile through the wound had been so great, that little was left to pass into the duodenum, yet he digested his food well. The stools continued regular, till within a few days before death, and even to the last the fæces all along retained their natural colour. 2dly, It may be observed, that the jaundice was not occasioned by the obstruction of the cystic duct, though that is apprehended as a common cause of this malady; for this obstruction must have been of many years standing, and this patient's jaundice was of a very late date. Nor was his jaundice owing to any retention of the bile in the porus biliarius, from the tumour continually pressing that duct, and so obstructing the free discharge of the bile, from the glands of the liver into the duodenum and gall-bladder; nor even to the strong compression and total obstruction of some, and almost all the biliary ducts, viz. the porii biliarii, the ductus hepaticus, the hepatico-cystic, and the ductus-cysticus, and communis choledochus, the principal of which are seated in the concavity of the great lobe of the liver, under the pressure of this great and hard tumour, and under its increase for near 14 years together, obstructions and compressions generally accounted as primary and idiopathic causes of the jaundice, because no distemper like the jaundice had appeared in this patient, till within a few months before his death, and no true jaundice till within a few weeks, and only then as the abscess formed in the neighbourhood of the liver had brought an inflammation there; but as all the symptoms of his jaundice began to wear off, soon after the pus had got a vent, viz. as the inflammation of the liver brought on occasionally by a suppuration in the neighbourhood wore off, and some days before the bursting of the vesica fellis, it does not appear unlikely, that this inflammation of the liver was the pathognomonic cause of the jaundice here; which inflammation of the liver, as it was accidental, so the jaundice occasioned by it, was actually removed soon after a vent was made for the purulent matter, which had occasioned this inflammation.

Some Observations on the foregoing Case of Mr. Le Grange. By Alexander Stuart, M. D. F. R. S. &c. N° 449, p. 325.

Dr. Stuart points out what appears to be the mechanical and necessary connexion between these apparent causes and their effects, in this uncommon case.

1. As to the original or prime cause of all the symptoms, viz. the distention of the gall-bladder, now become a morbid cystis of an enormous extent.

If we consider the size and figure of the liver, and the situation of the gall-bladder, with the course or direction of the biliary vessels, from various places of the liver, towards that narrow space where the pori biliarii open into the cystis, it will appear, that in almost every position of the body, at least in an erect, supine and lateral position, some of these biliary ducts terminating in the gall-bladder, are perpendicular, or nearly perpendicular, to the horizon and to the cystis: therefore, as far as gravity takes place in the animal economy, the bile descending by these ducts, will press on the contents of the full cystis and its sides, as a cylinder of that fluid, of the length of the secretory ducts or pipes, and of the diameter of the cystis.

Besides this, the extremity of every one of these small ducts, conveys its fluid into the full cystis, as a wedge acted on by the repeated strokes, impulses, or pressures of the circulating blood of the vena porta, where it supplies the gland at the origin of each secretory duct. Therefore, by the known laws of hydrostatics and mechanics, it is apparent, that the force of this secretion of the bile, into the gall-bladder, is very great, and the quantity copious; sufficient at least to distend the cystis to an enormous pitch, where the discharge by the ductus cysticus, is not equal to the secretion by the pori biliarii and the ductus hepatico-cysticus.

These powers sufficiently account for the distention of the abdomen in an ascites, of the womb in gestation, of the bladder in a morbid or voluntary retention of urine; also of morbid imposthumes or tumours, and of the gall-bladder in the case before us.

But this distention could never have happened, without a total or partial obstruction of the excretory duct, the ductus cysticus. Had this obstruction been at once total, as when a calculus is thrown suddenly out of the cystis into the duct, and stops it totally, he must have had the jaundice immediately, or very soon after: for, notwithstanding the strong powers abovementioned, it would have been impossible for the sides of the cystis to have yielded to such a sudden dilatation, no more than the womb in the first week of gestation, can be dilated to the pitch it is brought to in the 9th month, without a rupture: so that the dilatation here must have been very slow and gradual, and therefore the obstruction must have been at first, and probably for many years, only partial; and the gall-bladder thus slowly distended, gradually yielded and gave way, only for the reception of the excess of the secretion beyond the excretion, and so prevented the jaundice, or regurgitation of the bile into the blood.

This partial obstruction of the cystic duct may probably have been occasioned by one of those small soft incysted tumours, lodged between the membranes of the cystis fellea, near the origin of its excretory duct, containing a soft white pultaceous matter, with calculi, or chalky concretions, in its centre. If this was the case, it is conceivable that while the contents of this small incysted tumour were fluid or soft, they might not be capable to obstruct totally the current of the bile through the excretory duct: but as the matter of it grew thicker, and its bulk increased, by pressing gradually more and more on the duct, the obstruction must increase; and the formation of calculi, by their pressure, must at last make the obstruction total. But as the cystic duct was, at opening of the body, entirely coalesced and obliterated, its vicinity and situation, with respect to these small incysted pultaceous and cretaceous tumours, cannot be precisely determined; and therefore this is offered only as a probable conjecture.

The bulk, contents, and adherences of the gall-bladder to the right side, were doubtless to him a very sensible, and to us a visible cause of his first symptom, the increasing weight he had felt in the region of the liver, for 14 years before his death.

The current of moving humours in the animal body, is always determined most strongly to the place of least resistance: therefore, by the partial obstruction of the cystic duct, a greater quantity of bile than usual will be forced on the biliary ducts, leading directly from the liver into the great hepatic duct, to discharge itself by the choledochus communis into the duodenum, sufficient for the moderate uses of the animal economy; though not so perfectly sufficient, but that the peristaltic motion, in this case, felt the want of the cystic bile, or at least the defect of it so far, as to become weak and imperfect, too weak to propel the excrements, or keep the elastic air within due bounds; and therefore the patient must be subject to flatulent distentions, and some degree of costiveness, only to be relieved by supplying the want of a sufficient natural stimulus of the gall, by the artificial stimulus of purgatives and clysters, to assist from time to time the expulsion both of the excrements and also of the flatuses, for the ease of the patient, as was practised in this case.

As to the jaundice, which began to show itself 4 months before his death, and continued increasing, till the external purulent tumour in his side was opened, when it began to decline, and quite disappeared soon after the gall-bladder burst: it is easy to conceive, that so long as the gall, descending from the pori biliarii, could make its way into the cystis fellea, and dilate it, there could be no regurgitation of the bile into the blood, and therefore no jaundice: but as soon as the purulent imposthume began to form itself in the neighbourhood and contact of the distended gall-bladder, it encroached or pressed on the cystis fellea, by the force of a multitude of vessels, pouring pus into the cavity of the imposthume, urged on by the circulation of the blood, which is more forcible in these vessels than in those of the liver: and therefore this purulent tumour increasing, will very forcibly encroach on the cystis fellea in contact with it, and not only hinder its further distention, but even force the gall it contains to regurgitate, or return again by the pori biliarii upwards, and from thence, by the capillaries of the vena cava, into the blood, and so produce the jaundice; without raising an inflammation or obstruction in the liver itself, whose vessels and passages remain open, though the bile take a retrograde course in its biliary secretory ducts.

But as soon as this accessory pressure is taken off from the cystis fellea, by opening and emptying the purulent tumour or imposthume in its neighbourhood, adjoining and adhering to it, the bile begins again to flow freely into the cystis fellea, and to dilate it as before; therefore the regurgitation of the bile into the blood ceases, and the jaundice begins to decline.

Then, as soon as the rupture or bursting of the gall-bladder happened, and it began to be emptied, all degrees of resistance being now totally taken off from the pori biliarii, they spew out their contents so copiously, that the hepatic ducts are gradually frustrated by such a strong revulsion; the bile begins to flow all to the wounded and almost emptied cystis biliaria, and either very little or none to be carried by the ductus hepaticus to the choledochus communis, whose diameter and passage into the duodenum was found larger than usual, but empty. In this state, which was the last stage of his distemper, the peristaltic motion begins to fail, the expulsion of the excrements to be very tardy, or not at all to succeed without the assistance of purging medicines or clysters, which also had but a very slender effect; the patient ceases to be nourished, though he takes a competent quantity of food, and dies in a week after this costiveness began.

The degree of perfection of the natural, vital and animal functions in this person, during 14 years indisposition, was certainly owing to the soundness of all the viscera, and an almost sufficient secretion and excretion of bile by the ductus hepaticus, into the choledochus communis, whose cavity and passage into the duodenum was large and open, which could not have been and have continued, without a continual and proportional flux of bile through it: for it is well known, that as soon as the fluids cease to flow through their natural ducts, their sides soon collapse, coalesce, and at last totally shut up. Thus the urachus, and canalis arteriosus Botalli in the fœtus, shut up totally soon after the birth; and Mr. Amyand and Dr. S. have seen one of the ureters totally coalesced and shut up, for want of a fluid from the kidney, which had secreted no urine for some time, having become a cystis, filled with a thick white pultaceous matter, nearly of a cretaceous consistence.

Therefore, as the cystic duct was found obliterated, and the choledochus communis large and open, it is plain that no bile had for some time flowed through the former, and that there was a constant supply from the hepatic duct to the latter, for the uses of the animal economy; until the wound or rupture of the gall-bladder, gradually abating its current by that channel, at last stopped it quite, and put an end to life in a few days after.

The apparent Times of the Immersions and Emersions of the four Satellites of Jupiter, for the Year 1740, *computed to the Meridian of the Royal Observatory at Greenwich. By James Hodgson, F. R. S. In all* 374. N° 449, p. 332.

This is another repetition of Mr. Hodgson's usual calculation of these eclipses.

A Continuation of an Account of an Essay towards a Natural History of Carolina, and the Bahama Islands; by Mark Catesby, F. R. S. with some Extracts out of the 9th Set, by Dr. Mortimer, Secr. R. S. N° 449, p. 343.

The extracts and account of the 8th set are inserted in N° 441 of the Phil. Trans. This 9th set begins with pl. 61, of the 2d vol. and as the foregoing treated chiefly of serpents, this contains the figures and descriptions of several quadrupeds, intermixed with plants.

Matthiæ Belii, Pannonii, R. S. Lond. S. &c. Observatio Historico-Physica de Aquis Neosoliensium Æratis, vulgo Cement-Wasser dictis, Ferrum Ære permutantibus, in Epistolâ ad illustr. Dm. Hans Sloane Bart. &c. scriptâ, communicata. N° 450, p. 351.

This paper gives an account of the copper waters (cement-wasser) in the copper-mines near the town of Neusol in Hungary. The rain, or spring water, as it percolates through the sides of the excavations worked in these mines, becomes impregnated (from its action on the copper-pyrites) with vitriol of copper, and is conveyed by numerous channels into large wooden cisterns, or reservoirs, placed in various parts of the mines. When pieces of iron are thrown into the water collected in these reservoirs, and are suffered to remain therein for a sufficient length of time, they are, according to the belief of the vulgar, turned into copper; i. e. the copper is precipitated upon the surface of the iron in its metallic state.* It is afterwards scraped off from the iron, and melted and wrought into a variety of utensils. The copper thus obtained is found to be exceedingly pure.†—Respecting the proportion of vitriol of copper in this so called cement-water, Mr. B. states that lbj (medicinal weight) of the said water, when evaporated to dryness, gave ℈iiss of residuum, which, being dissolved in pure water, yielded a green solution. When this solution was afterwards filtered and evaporated, Mr. B. obtained ℈ij of crystallized vitriol, [vitriol of copper] besides 6 grs. of a yellow precipitate.

Of a Bubonocele or Rupture in the Groin, and the Operation upon it; by C. Amyand, Esq. Serjeant-Surgeon to His Majesty, and F. R. S. N° 450, p. 361.

October 8, 1737, Mrs. Bennet, 70 years of age, of a thin habit of body,

*This apparent transmutation of iron into copper is easily explained by the laws of chemical attraction. The vitriolic acid, with which the copper is combined in the so called cement-water, having a greater affinity for iron than for copper, it deserts the latter to unite with the former. Accordingly, when a piece of iron is thrown into such water, it is immediately acted upon by the vitriolic acid, that acid dissolving and combining with a portion of the iron, and letting fall the copper in its metallic state, which, during its precipitation attaches itself to the surface of the undissolved portion of iron. There is, therefore, in this instance no transmutation of one metal into another; but merely a decomposition and a new chemical union produced by the force of elective attraction, the iron being combined with the acid in place of the copper, and the latter being precipitated in its metallic state.

† This mode of obtaining copper is practised in many other countries besides Hungary, the water collected in, or issuing from most copper mines holding more or less vitriol of copper [sulphate of copper] in solution. Considerable quantities of copper are annually separated from the water of the copper-mines in Anglesey by means of iron, as at Neusol.

had a return of a tumour in the groin, with unusual pains, which was soon followed with an excruciating pain in the belly, and such colics, retchings, and excrementitious vomitings, as usually attend the strangulation of the gut in the miserere mei. This came upon her unawares, and the distress she was in, made her forget that for 25 years before she had had a swelling in the groin, as large as a hasel nut, which seldom had given her any uneasiness, and which she never suspected to be a rupture. Latterly the patient had been more subject to colics than usual, but this was imputed to bad digestion; and that day she had used no motion capable of producing a rupture; so that it was by chance that Mr. Despaignol, who was sent for the next day, discovered the cause of the complaint. She was blooded, clystered, fomented, &c.; but the complaints subsisting with a continual singultus, Mr. A. was called in the 11th.

The tumour was then oblong, about the size of a hen's egg, somewhat inflamed, yet not tense, nor so painful as to make him take much notice of it.

On the repeated use of the abovementioned means, and of lenient purges and opiates, the vomitings and hiccough were at times stopped, and the patient made so much easier, as to give hopes of success; but as during 6 days the patient had no passage, and the tumour could not be reduced, it was thought unsafe to delay the operation any longer.

The tumour felt unequal, though it appeared even, and pappy, as the tumours of the omentum generally are; and therefore of that kind which is always most difficult to reduce, the omentum not having that elastic springiness which favours the replacing of the guts.

On dissection, it was found embodied in the hernial bag, and that upon the external surface of the slits in the abdominal muscles, the folds of it had formed a round protuberance, not unlike the os tincæ in the vagina, or like a bourlet, which, by compressing the gut, prevented its return into the belly, and by obstructing the opening, as the gut pressed upon it, had strangulated about an inch of the gut encompassed by it in the hernia.

This being the 6th day from the beginning of this disorder, the gut in that place was found of a very swarthy colour, but yet springy; so that it was not quite mortified. It lay inclosed in a net, formed by the omentum (like a fish in a fishing-net) strangulating the gut under its pressure without the abdominal muscles.

It was with some difficulty the omentum was torn off and separated from the bag it was attached to; and as this lay in the way of the reduction of the gut, and almost sphacelated, so it was cut off without any previous ligature, though its vessels were turgid and large, as it was impossible to pull it out so as to make the ligature upon the sound part of it; after which the reduction of the gut might easily have been made, without enlarging the annular slit; for this made no stricture to prevent it.

But the quantity of the omentum within it being large and bulky, and the gut in a very diseased state, it was thought more expedient to enlarge it, to make the reduction of the whole easy: afterwards the omentum was detached from its adhesion round this place, and pulled farther out, and a ligature being made upon the sound part of it, that was also replaced in the belly, and the entrance stopped with a conical tent, dipped in the yolk of an egg, &c. the belly was embrocated, and the dressings well secured; for as the patient was very much oppressed with an asthma, so she was obliged to be sitting in bed.

From this time the hiccough and excrementitious vomitings disappeared, but the retchings and vomitings continued near 5 days longer, before the fæces detained above the strangulated gut, could make their way downwards, though they were frequently visited by clysters and lenient purges.

The patient was blooded immediately after the operation, and soon after took an emollient and carminative clyster, which was repeated night and morning, and an oily laxative every 4 hours.

At first the evacuations were exceedingly fetid, black, griping, and frequent; but they became more moderate, as she took absorbents and diluents; but yet so frequent, that it was thought proper to restrain them by gentle astringents.

In 5 or 6 days the stools had removed the tension which appeared on the belly after the operations; the retchings and vomitings, and the remaining symptoms went off, the wound digested well, and the patient continued in a mending way.

It has been already observed, that this old woman was very much afflicted with an asthma: she had at times violent fits of it, and the 14th day from the operation she had one, with a total stoppage of the discharge from her lungs, which choaked her on the 17th day.

This case was a proof of what Mr. A. had frequently observed, on similar occasions, viz. that as the omentum is the principal obstacle to the reduction of the guts in ruptures; so it is the occasion of the greatest accidents that attend that evil. It wraps up and incloses the prolapsed gut like a net, whose fastened end within the belly strangulates the part detained in the rupture without the abdominal apertures, where it is confined, and is productive of such folds in it, and pressures of the gut wrapped up therein, as is oftener the cause of a strangulation and miserere mei, than the tendinous slits of the external oblique muscles in the inguinal rupture, or tendinous opening in the navel, which upon these is seldom found inflamed, and can never contract so suddenly, as to obstruct the return of the gut into the abdomen, when the omentum is wanting: agreeably to which, it is rare to find any strangulated rupture that is not attended by it.

The pain attending the prolapsus soon swells the vessels of the omentum, and that will fill up the apertures in the abdominal muscles, through which the viscera are fallen out, so as to prevent their return, and bring on an inflammation: if by plentiful bleeding the vessels emptied do not facilitate the return of the parts prolapsed, then all the consequences follow that are generally observed upon the like occasion; and if blood-letting with the remedies before mentioned do not produce the desired effect soon, it is very seldom that any thing is got by the application and use of all the other means prescribed.

It is however certain, that it is very dangerous to depend too long upon those remedies, and that a suspension of the symptoms is no security, whilst the due course of the fæces is interrupted.

The case here mentioned may be a warning to others not to delay too long an operation, whereby the parts are to be released from confinement, and which oftener would be successful, if it was not delayed so long.

An Account of a Pin taken out of the Bladder of a Child. By Mr. William Gregory, Surgeon. N° 450, p. 367.

Mr. G. was called to the assistance of a woman in travail. The fœtus presented in a transverse position; he soon recovered the feet, and in a few minutes delivered the woman. The funiculus umbilicalis was so short, that it was with difficulty he could make a ligature on it, in order to make a separation: he immediately extracted the secundine, and measured the funiculus, which was little more than 4 inches long.

As soon as the woman was taken care of, he examined the child, which he found to be imperfect in several parts, there being no anus, neither privities to distinguish of what sex it was: where the vulva should be, there was a small perforation, though no appearance of labia, through which the urine always passed away; there was likewise a large hernia umbilicalis, and a little lower in the linea alba, was a perforation, into which the intestinum rectum opened, and there the excrements passed during the time the child lived, which was almost 10 weeks.

Several days before the child died, a gangrene appeared on the hernia, which soon passed into the intestines, and occasioned its death: the hernia, in his opinion, was occasioned by the shortness of the funiculus, which did not grow in length proportionable to the fœtus; the child in all other parts was perfect. When the child died, he had liberty from the parents to inspect into it: he did not go through a regular dissection; but only inspected into the intestinum rectum, which he found as above described, and the urinary bladder, which he found very small, and no urine in it; the child was never observed to make water in a stream whilst it lived, which makes him of opinion, the sphincter vesicæ was imperfect. In handling the bladder, he found something sharp pointing to his finger; he could not discover what it was, till he snipped off the neck of the bladder: he then took out of the bladder a tough kind of substance, about as large as a small fig, in which was a pin with the head on, and very black.

Of a very extraordinary Calculus taken out of the Bladder of a Man after Death. By the Marquis de Caumont. N° 450, p. 369.

The Marquis de C. states that he was induced to send to the President of the R. S. the drawing of an uncommon stone, found lately in the bladder of a dead body, which he had engraved in his own presence. It is exactly conformable to the original. The most able physicians, and the best anatomists, assured him that they never saw any thing like it. He can vouch, that the engraving, though very exact, does not come up to this singular work of nature; the 10 branches of which, that spread from the centre, have some resemblance to those of certain plants. It was a matter of difficulty to think, that the system of juxt-apposition, which is employed to explain the successive growth of common stones or calculi, can hold good on this occasion. He dares not however advance, that vegetation has any share herein: though the shape of the branches of the stone, of the canals, or papillæ, which seemed destined to convey the nutritious juices, in some measure favoured this hypothesis. He thought proper to join to the figure of the stone, the account of the patient's distemper, in whose bladder it was found; as Mr. Salien, sugeon of Lisle in the county of Venaissin, sent it to him. The fact, of itself, cannot fail of ap pearing curious. And skilful lithotomists may reap some advantage by it, for perfecting their operations. For allowing the possibility of calculi of a conformation somewhat like this, which they may judge of by knowing the bulk of the stone, they will understand, that in such a case, no other method but that of the high operation can facilitate the extraction of an extraneous body, whose branches cannot fail causing considerable lacerations; unless they found some favourable circumstances, and that the contexture of it were brittle enough to break it before being extracted.

An Account of the abovementioned Case. By Mr. Salien, F. R. S. N° 450, p. 371.

Joseph Vasse, of Le Thor, a small town at a short league's distance from Lisle, in the county of Venaissin, 66 years of age, of a robust constitution, who used to travel about to fairs and markets in that county, dealing in corn and cattle, without having ever complained of any indisposition, began, on the 14th of February 1731, to feel in the night-time some difficulty of making water, attended with a smarting about the glans; which however did not hinder him from attending his business as before.

On the 28th of March 1732, he was seized in the night with a true iscuria, which cruelly tormented him. M. Salien was sent for on the 29th in the evening, to search him, and to draw off the urine. He drew accordingly 6 cups, each containing 1 pint and a quarter. The patient found immediate ease, and continued without pains or fever, so that he thought himself quite cured. But the night following the pains returned, which made him resolve to come to Lisle, to be nearer at hand to be sounded. He came on the 30th of March; and had his water drawn off regularly every day, morning and evening, till the 15th of April next, during all which time the patient suffered no pains, did not fall away, nor had any symptoms of sickness upon him.

On the 15th of April, he supped with his usual appetite; but half an hour after supper, he was seized with a violent shaking fit, which lasted a full hour, on which a burning fever ensued, attended with an unquenchable thirst, with great head-ache, and an extraordinary restlessness.

In this condition M. S. found the patient about 8 in the evening, being the hour he usually went to sound him. He immediately prepared himself to draw off his water, according to custom, thinking thus to procure him some ease. Till then the catheter had entered without any obstacle; but this time, on pushing it into the bladder, he felt a stone which obstructed its passage. He turned the catheter to the left, and hit upon one of the branches of the stone, as represented fig. 1, pl. 7.

In order to know whether there was not another stone, he drew the catheter a little back, turning it to the right, which was done without any difficulty; and having pushed it in again, he met with another branch of the same stone, which he took for a stone different from the former, and concluded then, that he had found several stones in the patient's bladder; and that if the bad symptoms which appeared, should continue any longer, there was no probability of his recovering. Accordingly, the hiccough coming on him on the 20th, and the other symptoms not discontinuing, he died on the 28th. The stone was taken out 4 hours after his death.

Concerning the abovementioned Stone. By Sir Hans Sloane, Bart. Pr. R. S. N° 450, p. 374.

The abovementioned stone is so singular, that among some hundreds of those in the possession of Sir Hans Sloane, he had none that came near it.

Once indeed he had under his care a gentleman between 60 and 70 years of age, who had extraordinary difficulties in making water, and an inconveniency even beyond that; which was, that he could not sit in an ordinary chair without suffering extremely in the region of the peritonæum. With the help of lenient soft medicines and waters, he voided by the urethra a stone, which was flat in the middle, and smooth, but had five points, resembling the rowel of a spur. The points of the rays were sharp, but there were no asperities or crystallizations on their surfaces. It was small, so as after many days to pass along the urethra: but if it had not passed through the neck of the bladder, but remained in the bladder, it would, in all probability, have attracted matter to all the points or rays, and increased in all dimensions.

It is very common, that when any extraneous solid substance gets into the bladder, there is either attracted to it, or adheres to and surrounds it, a tartareous calculous concretion, which assumes the figure of the said body then in its centre, as a nucleus.

There was a soldier cut in St. Thomas's Hospital, London, for the stone, which, when taken out, was found to cover a musquet-bullet, that had been shot into his bladder, where it was covered by a calculous concretion.

Sir Hans S. had a silver bodkin, which a gentlewoman used for her hair; and thinking with it to thrust back a stone that was engaged in the neck of her bladder, it slipped into it, and the calculous matter gathered on the larger end into a stone of an oblong figure, and equal thickness, of half an inch all round the bodkin.

He had also a common pin, which by some means had got into the bladder of a young woman, and was there coated all over by a calculous matter; but having occasioned a fistulous ulcer in her groin, it was discharged thence with the matter of the fistula.

It is in this manner that bezoars are formed: for he had the common East-India bezoars, which are roundish, and had in their centres the seeds of a sort of acacia, which had attracted, or was coated over by that substance, esteemed a great cordial or alexipharmac; while others are long, and were gathered in layers or coats on the stalks of vegetables. And he had one formed round the stone of that great plum, which comes pickled from thence, and is called mango.

As to the asperities or prickles on the rays, they were noticed, so long since as the time of Cornelius Celsus, who, lib. 7, c. 26, calls them calculi spinosi.

It may seem very strange and paradoxical, what he can assert is true, that the fewer the knobs, asperities, or prickles, are on the surface of calculi, the more troublesome they are to the persons in whose bladders they lie. Dr. Hickes was the most tormented with the stone in his bladder, of any he ever knew, especially on any motion. He would not submit to be cut for the distemper, on account of his age, and many other reasons; but ordered his executors, that he should be opened after death, and the stone taken out of his bladder, put into a silver box, and given to Sir Hans, who had been his physician for many years, to place it in his collection of such kind of curiosities. It is very particular in this stone, that the protuberances and prickles on it were few, and at a distance from each other. Every one of them had made a hole in his bladder, like a sheath or socket; and when, upon motion, they were removed out of their corresponding sheaths, they hurt the bladder in the sound parts, and put him on the rack of pain.

When they are thick-set, one hinders the other from entering or wounding so deep; and perhaps gets not much farther than the mucus which lines the inside of the bladder.

An Account of some Oil of Sassafras crystallized. By Mr. John Maud, F.R.S. N° 450, p. 378.

Mr. M. observed that some essential oil of sassafras, which had stood exposed to a frosty night, in an open vessel, was changed, 3 parts out of 4, into very beautiful transparent crystals, 3 or 4 inches in length, half an inch in thickness, and of an hexagonal form.

These crystals subsided in water, were indissoluble in it, inflammable in the fire, and when exposed to it, melted into their pristine state. Hence it was evident, that they still retained the natural qualities of an oil, though they appeared under a different modification of their constituent parts.

What was most remarkable, was the metamorphosis from a fluid to a solid body, of such a particular figure, and from a yellowish liquor, not unlike Madeira wine, to a very pellucid body, like ice congealed from the most transparent water. This seemed to afford a new instance of crystallization, which being generally accounted for by the particles of a fluid, or those of any other body, suspended by the fluid, brought nearer by cold, and at length coming within the sphere of each other's attraction, united together into an immediate contact. This being one of the heaviest oils, and even heavier than water, is the more likely thus to unite, as its parts are nearer together. This may be a hint to the curious, to discover in what the difference between solidity and fluidity consists; and also shows how much the colour of bodies depends on the mechanical situation of their parts.*

Of an extraordinary Damp in a Well in the Isle of Wight. By Mr. Benj. Cooke, F.R.S. N° 450, p. 379.

In June 1733, a farmer, in hopes of finding a perpetual spring of good water, sunk a well of 7 feet diameter, to the depth of 45 feet, through a soil which at the surface was a kind of brick earth mixed with sand, but in descending became almost wholly hard coarse yellow sand. The work employed the labourers about 20 days, without finding the least appearance of water.

At the distance of about 18 feet from the top, a stratum of a mineral mixture, about 9 inches thick, was dug through, without any inconvenience; nor were the workmen in the least incommoded in carrying on the work, till about the 12th day after, when towards the evening they were much annoyed with a faint suffocating heat, which they compared to that coming from the mouth of an oven, and which, as they were drawn up, was most remarkably perceived, when they came opposite to the mineral stratum abovementioned, to come out in the form of a warm sulphureous halitus.

The next morning, a lusty young man attempted to go down, hand over hand, as the workmen call it, by means of a single rope, which was used to draw up the earth dug out; but as soon as he came opposite to the abovementioned stratum, he became incapable of sustaining his own weight, but fell down to the bottom, and died immediately.

Another young man, not suspecting the cause, had the rope nimbly drawn up; and having seated himself astride a cross-stick fixed to the rope for that purpose, was hastily let down to his friend's assistance; but when he came to the same distance from the top, he was observed to give the rope a very great shock, and when he came to the bottom, fell down, as the other had done before him, was seized with violent convulsions, which held him more than a quarter of an hour, and then he expired.

* See a like crystallization from Thyme, by Dr. Neumann, which he calls Camphora Thymi, N° 389 and 431, of the Phil. Trans.—Orig.

A third person, in hopes of fetching up this second before he was quite dead, was tied fast into a large basket, and let down with more caution; but when he came to the same stratum, finding his breath going, as he expressed it, he cried out, and was drawn up again; but remained in the open air for the space of near half an hour, pale as dead, panting and speechless.

The dead bodies were, within three hours space, drawn up by the help of a sort of tongs, used to fetch things up from the bottom of the sea; but brought such a disagreeable stench in their cloaths with them, as made several hardy men, who assisted in doing it, vomit.

The next day a cat was let down, and at the same place seized with convulsions; but being drawn quickly up again, soon came to herself; which experiment was repeated several times for some weeks following, by which it was found, that this destructive vapour was sometimes of a greater and sometimes less force, and sometimes quite gone, so that the cat felt no uneasiness; and a lighted candle, which would sometimes be immediately extinguished as soon as it sunk below this deadly stratum, would burn clearly at the very bottom.

It was very remarkable, that there was a whitish fog in the well, so thick that one could but just see the dead bodies through it.

Water being scarce in that place, the well was left open for about 8 months, in hopes the damp might at last wholly leave it; but instead of that, it became worse; and not confining itself within its first bounds, it overflowed at the top, where, when the air was moist, it appeared like a thin white fog; and when the air was dry, could be perceived like a warm breath, at all times diffusing a sulphureous stench, something like that which arises from filings of iron, while corroding with vinegar, affecting those who came into it with a giddiness, shortness of breath, and propensity to vomit; so that at last the well was filled up, being troublesome to the family which lived near it.

The stratum abovementioned, which is continued to the neighbouring clift, where, when heated with the summer's sun, it gives a noisome sulphureous smell, and is, after moderate rains, covered with a yellowish efflorescent salt, very astringent and acid.—On the shore below there are gathered pyrites.

P. S. The vein which was cut through in the middle of the well, from whence were emitted the fatal effluvia, is a crude ore made up of iron, sulphur and acid salts, mixed with pyrites.

These effluvia were not perceived till after the vein had imbibed the air for several days. While the air continued dry, these effluvia subsided, and lay in the lower part of the well, which seemed filled near to an exact level with the stratum from whence they came.

But when the weather became rainy, the quantity as well as the impetus of the effluvia increased to such a degree, as to appear in mornings over the top of the well, in the form of a mist, and gave great annoyance to those who came within its sphere of action. From hence it is worth observing, that the same damp, according to the variation of the weather, is specifically heavier or lighter than the air.

Concerning Magnets having more Poles than two. By Mr. John Eames, F. R. S. N° 450, p. 383.

The sagacious Dr. Halley, in his account of the changes of the variation of the magnetic needle, on the hypothesis of the earth's being one great magnet having four magnetic poles, tells us, that he had found two difficulties not easy to surmount; the one was, that no magnet he had ever seen or heard of, had more than 2 opposite poles, whereas the earth had visibly 4, if not more, &c. On looking over the copy of the Journal-book of this honourable society, Vol. 2, an article is in the following words:

July 20, 1664, "Mr. Ball produced several loadstones, and among them two terrellas, whereof one seemed to have 4 poles, with a circle passing between them, of no virtue at all. Some of the company suggested, that it was probable this stone consisted of two stones, by nature cemented together by a piece that had no magnetical quality in it; and if single, whether the respective poles were opposite."

An Account of some Magnetical Experiments. By J. T. Desaguliers. N° 450, p. 384.

In the year 1715, trying some experiments on a very large weak loadstone, the Dr. found that it had several poles. He then tried several other loadstones, and often found 4 poles in such as had been armed when he took off their armour. In large coarse stones he found sometimes 8, 9, or 10 poles. This made him believe that all loadstones had several poles; but when he tried Lord Paisley's loadstones, and other very good ones, he then found that homogeneous loadstones had but 2 poles; those that have more being only an aggregate of magnetic and other matter, which makes a heterogeneous substance. Such is the society's great loadstone; for it has several poles.

An Account of some Magnetical Experiments made before the Royal Society. By the Rev. J. T. Desaguliers, LL. D. F. R. S. N° 450, p. 385.

Dr. D. took a bar of iron, of 1-4th of an inch diameter, which having been

15 years in an erect position, had acquired a fixed pole at top, so that the end which had stood uppermost attracted the north end of a compass-needle, and the other end the south end of the needle; and having suspended it by a string for the space of half a year, it acquired a fixed south pole at that end, as well as it had done at the other in the time of 15 years, without diminishing the virtue of the other end: so that both ends of the rod in any situation attracted the north end of the compass-needle.

That rods of iron untouched, or which have not acquired a magnetic virtue by their situation, will with their upper end, whatever end of the bar be held upwards, attract the north end of the needle, and the lower end of the bar the south end of it, is a truth known many years ago, and mentioned in Dr. Brown's book of vulgar errors.

Some further Magnetical Experiments made before the Royal Society. By Dr. Desaguliers. N° 450, p. 386.

It is well known, and has been often found by experience, that an iron bar untouched by a loadstone, will, with its upper end, attract the north end of the needle of a compass, when the said bar is held upright, and the south end of the needle with its lower end, when applied to it, still in a perpendicular position, whatever end of the bar be held up; unless the bar has acquired a fixed pole by having been long in a vertical position. But if the bar, from a vertical, be brought to a horizontal position, the needle will return into the situation it had before, which was in the magnetical meridian, the bar being then at right angles to it. On raising or sinking the end of the bar which is farthest from the needle, the one or the other end of the needle will begin to move towards the bar. Such a bar has in itself no fixed magnetic virtue; but if it had, it must be heated red-hot, and then cooled in a horizontal position. A bar thus prepared, is fit to make the following experiments, communicated by M. du Fay.

Hold the bar upright, and give it a blow or two against the ground with its lower end; and that end will attract the south end of the needle, when the bar is held horizontal, and at right angles to the magnetic meridian: the other end held horizontal in the same manner, will attract the north end of the needle. Invert the bar, and its virtue will be lost by striking as many blows with it against the ground with the other end: then strike another blow or two, and the end which attracted the north end of the needle, will now attract the south end; and so vice versâ, the position being still horizontal.

If the blow be given against the ceiling, or any horizontal body, with the upper end of the bar, the same virtue will be communicated as before.

This will likewise happen, if the upper or lower end of the bar be struck with a hammer or mallet; whether the blow be given end-wise or at right angles to the bar: nay, though it should be given in the middle of the bar; the position of the bar at receiving the blow being all that is requisite; for if you give the bar only a jerk or shake in that vertical position, it will receive the virtue, as if there were in the iron several threads or beards fixed at one end, as M. Du Fay supposes, which the blow or shake laid all one way, and which were placed the other way by inverting the bar, and then giving it a shake or blow.

When the bar is placed horizontally, a blow in the middle destroys its virtue.

Of an Antique Metal Stamp, in the Collection of his Grace Charles Duke of Richmond, Lenox and Aubigny, F.R.S. &c. being one of the Instances, how near the Romans had arrived to the Art of Printing; with some Remarks by C. Mortimer, M.D. Sec. R.S. Lond. N° 450, p. 388.

Since arts and sciences, especially statuary and sculpture, were arrived at so great perfection, when the Roman empire was in its glory, as the many beautiful statues, the exquisite intaglias, and the fine medals, which time has delivered down to us, sufficiently evince; it is much to be wondered at, that they never hit upon the method of printing books.

The dies they made for their coins, and their stamping them on the metal, was in reality printing on metal; their seals cut in cornelians and agates, and their pressing them on dough and soft wax, was another sort of printing; and a third sort was the marking their earthen vessels, while the clay was soft, with the name of the potter, or the owner the vessel was made for. These being of a larger size, were properly called signa; the seals cut in stone were called sigilla; sigillum being a diminutive of signum, as tigillum is of tignum: but the later and more barbarous Latinists have formed the diminutive of signum into signetum; and if a very small pocket-seal, they have called it signaculum. See Joh. Mich. Heinecius de Sigillis. Francof. 1709, fol. p. 16, et seq.

The learned Montfaucon, among his immense treasures of antiquities, in his Antiquité expliquée, Tom. 3, Part. 2d. Chap. 12, gives the figures and descriptions of several of these larger sigilla or signa, on which he says, the names were all cut in hollow in capital letters; and he imagines their use to have been to mark earthen vessels, particularly those great earthen jars, in which the Romans used to keep their wines. If any of them had occurred to him with the letters excisæ, exsculptæ, protuberant or standing out, as the types in our modern way of printing are made, so accurate a describer of antiquities could not have passed it over without having mentioned it, and that the rather because of its being a greater rarity: though several lamps of terra cocta are stamped with letters impressed or hollow, from such protuberant letters as in fig. 2, pl. 7, but the greater number have the letters raised, or standing out.

This stamp is made of the true antient brass, and is covered over with a green scale or coat, such as is usually seen on antient medals. It was found in or near Rome. On the back is fastened a ring, the hole of which is $\frac{27}{40}$ of an English inch one way, and $\frac{23}{40}$ the other way; the plate itself is two inches long, wanting $\frac{1}{40}$, and its breadth exactly $\frac{23}{40}$ of an inch: the sides are parallel to one another, and the ends are likewise parallel to each other, but they are not on an exact square with the sides, varying about 1 degree and a half from an exact rectangle. On the under side stand two lines or rows of letters, $\frac{7}{40}$ of an inch in height, and well-formed Roman capitals: their faces stand up, all upon an exact level with one another, and with the edge or border of the stamp; their protuberance or height above the ground is different, the ground being cut uneven; for close to most of the letters the ground is cut away only $\frac{2}{40}$, close to some near $\frac{3}{40}$, and close to the edges full $\frac{3}{40}$. The first line contains these letters, CICAECILI /, with a stop or leaf to fill up the line; in the second line, HERMIAE. SN. Which is judged to be read *Caii Julii* Cæcili, Hermiæ *Signum*. Who was probably a man in a private station, so that his name has not been handed down to us in any monuments, but only accidentally in this stamp. In Gruter occur two of the name of Hermias, and several of the Cæcilii, but none with these two names joined together.

The use of this stamp seems to have been for the signature of the above-mentioned private man, to save him the trouble of writing his name, as some people have at present. It was certainly used on paper or membranes, being first dipped into ink, or some sort of paint, because of the protuberance of the letters, the hollow letters being fitter for soft substances, on which they leave the impression standing up, and consequently more legible. Another argument, that this stamp was not to be used on any soft substance into which it might be pressed quite down to the ground, is the unevenness and roughness with which the ground is finished, which, was it to have made part of the impression, the workman would have finished with more accuracy; but he, knowing that the surface of the letters was to perform the whole work required, was only attentive to finish them with that accurate evenness that these have.

Mattaire, in his Annales Typographici, Hagæ 1719, in 4to. p. 4, concludes from the best authors, that our modern art of printing was first thought of about the year 1440. A copy of the book he mentions, ib. p. 13, called Speculum nostræ Salutis, being pictures of stories out of the bible, with verses underneath, in Dutch, is in the Stadhouse at Haarlem. Each page was printed from a block of wood, like a sorry wooden cut; and this was the first essay of printing, which hint was taken from engraving, and is what he means p. 4, by typi fixi; after which they soon improved to use separate types, as we do now, which he terms, ibid. typi mobiles.

This stamp is, in reality, a small frame of fixed types, and prints with our modern printer's ink, which is only a sort of black paint, as readily as any set of letters, cut in the rude manner these are, can be expected to perform.

We see by this stamp of two lines, that the very essence of printing was known to the Romans, and they had nothing to do but to have made a stamp with lines three or four times as long, and containing twenty instead of two lines, to have formed a frame of types that would have printed a whole page, as well as Coster's wooden blocks, which he used in printing the Speculum Salutis.

In the first volume of a collection of several pieces of Mr. John Toland, printed Lond. 1726, in 8vo. p. 297, is a small tract of his, entitled, Conjectura verosimilis de prima Typographiæ Inventione, which is founded on the following passage in Cicero, in cap. 20, lib. 2, de Natura Deorum; where Balbus the stoic uses the following words in an argument against Velleius an Epicurean.

Hic ego non mirer esse aliquem, qui sibi persuadeat, corpora quædam solida atque individua vi et gravitate ferri; mundumque effici ornatissimum et pulcherrimum, ex eorum concursione fortuita? Hoc qui existimet fieri potuisse, non intelligo cur non idem putet, si innumerabiles unius et viginti formæ literarum (vel aureæ vel quales libet) aliquò conjiciantur; posse ex his in terram excussis annales Ennii, ut deinceps legi possint, effici; quod nescio an ne in uno quidem versu possit tantum valere fortuna.

He conjectures that this very passage gave the first hint to the inventors of printing about the year 1445, because they retained even Cicero's name for their types, calling them formæ literarum, and made them of metal, as he says, aureæ vel quales libet. Moreover, in cap. 10, lib. 3, de Divinatione, Cicero has the very phrase imprimere literás.

Brands for marking cattle were in use in Virgil's time, Georg. lib. 3, ver. 158, where he says,

Continuoque notas, et nomina gentis inurunt.

Procopius, in his Historia Arcana, says, the emperor Justinus, not being able to write his name, had a thin smooth piece of board, through which were cut holes in form of the four letters IVST which, laid on the paper, served to direct the point of his pen; which being dipped in red ink, and put in his hand, his hand was guided by another. Possibly this may likewise have given the hint to the first of our card-makers, who paint their cards in the same manner, by plates of pewter or copper, or only pasteboards, with slits in them in form of the figures that are to be painted on the cards.

An Occultation of Mercury by Venus, May 17, 1737, *observed at the Royal Observatory at Greenwich. By J. Bevis, M. D.* N° 450, p. 394.

At 9^h 43^m 4^s p. m. app. time, Mercury was only the 10th or 12th part of Venus's diameter distant from her; afterwards the view was intercepted by clouds.

At 9^h 51^m 10^s Venus shone out again very bright, and Mercury was quite covered by her. Afterwards clouds again prevented any further view of the phenomenon.

Of a new Azimuth Compass, for finding the Variation of the Compass or Magnetic Needle at Sea, with greater Ease and Exactness than by any ever yet contrived for that Purpose. By Capt. Christopher Middleton, F. R. S. N° 450, p. 395.

To discover at sea the declination of the magnetic needle, or variation of the compass, with some tolerable degree of certainty and exactness, is a thing of great use and importance in the art of navigation. The instruments and methods hitherto used for this purpose, are subject to several inconveniencies, errors, and defects; to remedy which, this new azimuth compass was contrived, which has by experience been found effectual. It would be needless to give a description of the instrument; the cap therefore only shows the manner of using it, which is as follows:

1. The instrument must be rectified, or fitted for observation, by turning it about till the 4 cardinal points, that are hung on the centre pin, agree with the 4 cardinal points on the chart, at the bottom of the box; then will the needle, that shows the magnetic meridian, stand at no degrees, and the east and west points at 90 degrees, on the graduated circle within the box; and in this situation it must be kept, as near as may be, during the whole time of the observation.

2. Let the index of the quadrant be placed to that degree of the arch, on

the rim of the box, which the observer judges to be nearly equal to the height of the sun or star whose azimuth is sought; for by this means the object will be more readily found.

3. Turn the quadrant round towards the sun or star, till it appear on the vertical hair within the telescope, to an eye looking through the small hole or sight; and then slide the index a little upward or downward on the arch, till the object by this means be brought to coincide or touch the visible horizon.

Lastly, The degrees and minutes then marked by the index on the arch of the quadrant, will show the altitude of the object, which will always be the same, whether the instrument be in motion or at rest; at the same time the degree cut by the index on the horizontal rim, or circumference of the compass box, will give the magnetical azimuth of the sun or star.

How the variation of the needle is found by means of magnetical azimuth and altitude thus obtained, is taught in every treatise of navigation. But as the resolution of this problem is somewhat troublesome, and requires such a knowledge of the doctrine of the sphere, as every seaman has not attained, an easy method is here exhibited of discovering the variation of the compass without any manner of calculation.

1. Let the magnetic azimuth of the sun, or any star, when it is near the prime vertical, and considerably elevated above the horizon, be found according to the directions already given, before it arrive at the meridian, and note well the altitude, or let the index remain fixed at the same point on the arch.

2. Find the magnetic azimuth of the sun or star in like manner as before, when it is exactly at the same degree of altitude, after it has passed the meridian: and,

3. If these two magnetical azimuths be equal, the needle has no variation; if unequal, add them together, and half their sum will be the true azimuth; or subtract the less from the greater, and half the difference will be the variation required. The circumstances of the observation will the more readily discover whether the declination is easterly or westerly.

N. B. Though it would be very commendable in gentlemen who use the sea, to learn the names of most of the principal fixed stars, yet even that knowledge is not necessary in the use of this instrument: neither is it needful in this case to know exactly the latitude of the place of observation, provided the difference of latitude between the observations be not very great; it is sufficient, that care be taken to observe the self-same star, before it comes to the meridian, and after it has passed it; and for the sake of greater exactness, the caution before given should be regarded, viz. that the star be at some considerable height above the horizon, and also near the prime vertical.

An Account of a Book presented to the Royal Society, entitled, Notitia Hungariæ novæ Historico-Geographica, &c. Auctore Matth. Belio. By the Rev. Zachary Pearce, D. D. F. R. S. &c. N° 450, p. 398.

The author of this work is the Rev. Matthias Bell, a pastor among the Lutherans at Presburg in Hungary.

This first volume is to be followed by several others; for the kingdom of Hungary includes 48 districts or counties, and this volume gives an account of only one of them, and indeed is chiefly taken up with the history of the city of Presburg, or Pisonium, as he calls it, which, though inferior in other respects to the city Buda, is the place where the emperors, as kings of Hungary, are crowned, where the states of the kingdom assemble, and the courts of justice are held.

This volume consists of two parts. The first is general, and gives an account of the physical and political state of the whole district or county of Pisonium, describing its soil, produce, rivers, the temperature of its air, the nature of its inhabitants, its ancient inhabitants and present ones, its nobility, magistrates, and whatever belongs to the natural and political history of the district.

The second part is occupied with the description of the city Presburg; where the author is very copious and elaborate in setting forth every thing that relates to it, particularly its ancient state under the several nations who possessed it, and its present state under the Austrian family. Leaving to the next volume the description of the 4 other cities or principal towns, situated in the same district.

A short Account of Mr. Kersseboom's Essay, on the number of People in Holland and West Friezland, as also in Haarlem, Gouda, and the Hague; drawn from the Bills of Births, Burials, or Marriages, in those Places. By John Eames, F. R. S. N° 450, p. 401.

It is well known to what useful purposes the bills of births and burials, at the city of Breslau, the capital of Silesia, have been applied, by Dr. Halley; as also what curious observations have been made, both moral, physical, and political, by Sir William Petty, on the same argument, several years before, and by Dr. Arbuthnot and others since. Our industrious author has not only consulted them, but acquainted himself more particularly with Mr. King's observations in Davenant's Essays, &c. in order to render himself more capable of making a just estimate in this matter.

He begins with the number of inhabitants in the two provinces of Holland

and West Friezland; these he makes at this time, viz. 1738, to amount in all to 980,000, and gives the following table of the particulars. It exhibits the number of people of all ages, living at the same time, from the birth to extreme old age; which, because it shows the chances of mortality within the ages mentioned, he calls the table of contingency of life and death.

The Table of Contingency.

Of above 90 years old there are	500
of 90 to 86 inclusive	2,500
85....81..........	6,500
80....76..........	13,000
75....71..........	20,300
70....66..........	27,300
65....61..........	34,300
60....56..........	40,800
55....51..........	47,000
50....46..........	53,000
45....41..........	57,800
40....36..........	62,500
35....31..........	67,600
30....27..........	58,400
sum above 27 years..........	491,500

Of 26 to 21......	94,300
20......16......	83,400
15......11......	87,200
10...... 6.....	91,800
5......birth....	131,800
sum under 27 years...	488,500
ditto above 27 years...	491,500
sum of all the inhabitants	980,000

This table is founded on three principles, viz. correct observations on the tables of assignable annuities in Holland, which have been kept there for above 125 years; in which the ages of the persons dying are truly entered; on a supposition that there are yearly born in the two provinces 28,000 living children; and lastly, that the whole number of inhabitants in any country, is to the number of the births, as 35 to 1.

From this table it appears, 1. That about half the number of people in the two provinces are above 27 years old, and consequently that near the other half are under that age. 2. Then, by following what has been observed for more than 100 years in England, and particularly in London, out of 35 children born, 18 of them are boys, and 17 girls, the people in these two provinces will consist of 504,000 males, 476,000 females.

He further remarks, that it appears from the assignable annuities for lives beforementioned, the females have in all accidents of age lived about 3 or 4 years longer than the same number of males; which he thinks appointed as a compensation for the continual excess there is in the birth of the males above the females.

He then notices the quality of these 980,000 inhabitants, and says he sees no reason to differ from the proportion of Mr. King in Davenant's Essays, who with a great deal of pains and judgment has divided the people of England in this manner;

The proportion for every 100,000 inhabitants is,

Married men and women	34,500
Widowers	1,500
Unmarried young men and children	45,000
Servants	10,500
Travellers, strangers, &c.	4,000
	100,000

If this proportion be admitted, then the number of each sort in Holland and West Friezland will be as below. He adds, that the said provinces can raise at this time 220,000 able-bodied men, deducting $\frac{1}{10}$ for diseases and other infirmities. But then he admits at 16 years of age, whereas Dr. Halley admits none till 18, persons under that age being generally too weak to bear the fatigues of war, and the weight of arms. He then proceeds to rectify the mistakes of the learned Isaac Vossius, who makes but 550,000 in Holland, West Friezland, &c. Disallows Sir William Petty's account of the number of people in London, because he makes them alone equal to the inhabitants of Holland and West Friezland together.

He closes the whole with a table of the present value of annuities on lives, in proportiou to the ordinary or common bonds charged on those provinces, and subject to the extraordinary taxes raised at this time, viz. 1738. Annexed are the degrees of mortality or fatality, said to be in the Hague and Haagambagt, as also the numbers and conditions of the inhabitants of Amsterdam, Haarlem, Gouda, and the Hague, as also London at this present time.

The two provinces of Holland and West Friesland.		Amsterdam	Haarlem	Gouda	Hague	London
Married men and women	338000	86156	17420	6900	14850	241800
Widowers	14700	4218	760	300	720	13100
Widows	44100	13858	2280	900	2380	45700
Unmarried youth and children	441000	93990	22700	9000	16190	215700
Servants	102900	28318	5300	2100	4870	85000
Travellers, strangers, &c.	39300	14460	2040	800	2490	52300
Total	980000	241000	50500	20000	41500	653600

The fatality of the quarters.	dead.
Spring to summer	307
Summer to the autumnal equinox	286
Autumn to winter	287
Winter to spring	286

The fatality of the months 31 years, one with another.	dead.
January	102
February	88
March	95
April	77
May	112
June	100
July	92
August	95
September	99
October	93
November	95
December	99

Hence it appears, that March is less fatal at the Hague and Haagambagt than April, and April than May and June; that May is the most fatal month of all; that the remaining months are nearly equal. It appears further, that 3 parts or seasons of the year, are very nearly equal; but that the other quarter or season, beginning at the vernal equinox, is more fatal than any of the rest by a 15th part.

The Table of Annuities for Life.

Let the annuity be 100 guilders a year, on a life under a year old.

			Guilders		Guild.	Sti.	
Its present value is			1667	that is	6	0	per Cent.
Upon a life of	5 years to	1 inclusive	1869		5	7	
	10	6	1835		5	9	
	15	11	1770		5	13	
	20	16	1667		6	0	
	25	21	1587		6	6	
	30	26	1515		6	12	
	35	31	1429		7	0	
	40	36	1334		7	10	
	45	41	1212		8	5	
	50	46	1093		9	3	
	55	51	971		10	6	
	60	56	840		11	8	
	65	61	709		14	2	
	70	66	570		17	11	

The use of the table.—Quest. Let it be desired to know the present value of any annuity for life, for instance, of 90 guilders a year, which was granted in the year 1703, on a life then of 3 years old.

Answer. The life now, in 1738, is between 37 and 38 years old; hence the number between 40 and 36 gives 1334, for the present value of an annuity of 100 guilders; hence $\frac{1334 \times 90}{100} = 1200$ guilders, is the present value of the annuity for that life.

An Answer to that Part of Mr. W. Kersseboom's Essay, which treats of the Number of the Inhabitants of London. By William Maitland, F. R. S. N° 450, p. 407.

M. Kersseboom asserts, that the city of Paris, in the year 1684, and at the close of the last century, contained more inhabitants than the city of London. And to prove that Paris contains a greater number of inhabitants than London, he has had recourse to the accounts of christenings annually published in both cities, without inquiring into the nature of his authorities; which if he had, he would soon have discovered, that the former is a perfect account, while the latter is perhaps the most defective of any extant; for the christenings there mentioned, are only those where the parish clerks are present: which he thinks cannot amount to near two thirds of the whole.

The burials in the annexed table, by some typographical errors in the political account of the author's History of London, from which it is taken, being increased 491 above the real number, in Graunt's account, the sum total of which amounting to 90350, must be reduced to 89859; and as in the annexed term of years, there appears to have died of the plague 1741, three and a half of which, he computes, would have died of common distempers, out of each hundred, which amounting to about 61, this being deducted from 89859, the real number of the burials, the sum will be reduced to 89798, which taken from 90883, the whole number of the christenings, the remaining sum will be 1085, which being divided by 10, the medium will be 108½ yearly in favour of the christenings.

A Decenary Account of the Christenings and Burials of London, in the following Years.

Years.	Christened.	Buried. Com. Dist.	Buried. Plague.	Totals Buried.
1626	6701	7400	134	7534
1627	8408	7713	4	7717
1628	8564	7740	3	7743
1629	9901	8771	0	8771
1630	9315	9228	1317	10545
1631	8524	8288	274	8562
1632	9584	9527	8	9535
1633	9997	8392	0	8392
1634	9855	10899	1	10900
1635	10034	10651	0	10651
Tot.Gen.	90883	88609	1741	90350

This difference, in favour of the christenings, is owing to the citizens of that time being almost of the same religion; but the civil war breaking out soon after, the people deviated into a variety of sects, subverted the church of England, and assuming the civil power, established a new hierarchy, or church-government. But the members of the abolished church continuing to baptize among themselves, without reporting their christenings to the new-appointed members of the company of parish-clerks, occasioned a very great defect in the account of christenings annually published by the said parish-clerks.

From this epocha is to be dated the majority of the burials in the bill of mortality, over the christenings of London: and though the church of England was soon after re-established, yet the numerous dissenters of all denominations, persevering in their separation, continued to baptize among themselves, without sending in accounts of their christenings to the restored members of the company of parish-clerks; and the schism still continuing, the accounts of the christenings and burials of this city, remain on the ancient foot of division and imperfection.

Add to this, that not only all the foreign churches in London christen within themselves, but likewise many churches and chapels of the church of England, that send not in their accounts to the company of parish-clerks, which, together with those of the dissenters and foreigners of all denominations, amount to no less a number than 181 congregations, whose accounts of christenings are not published: by which it is evident, that the vast disparity between the christenings and burials of this city, is not owing, as Mr. Kersseboom imagines, to the residence of the court, the convention of parliament, and the great resort of people from all parts, but in fact to the great defect abovementioned.

However, that gentleman, from the aforesaid very defective account of the christenings of this city, has calculated the number of its inhabitants by a medium of the christenings in the years 1684 and 1685; by which he makes the number at that time amount to 500344: but as this number is only taken from a medium of 2 years, he imagines it too great; therefore to reduce the same to the number of 469700, by a medium of 20 years, he has unwarrantably precluded the sum of 14702, the number of christenings in the year 1684, to make room for the sum of 11851, the number of christenings in the year 1674; by which the number of the inhabitants of London, is very much lessened.

And as a further instance of Mr. Kersseboom's partiality in favour of the city of Paris, he has calculated the number of its inhabitants, without mentioning the uncertainty of a calculation founded on a short space of time, as he has done in the case of London, at a medium of the christenings for the years 1670, 1671 and 1672, by which he makes them at that time, amount to 610300; adding that the number must have been greater at the end of the last century; as by his extravagant manner of calculation it should be at present.

But as it appears by the above specified 10 years account, that the christenings of London greatly exceed the burials of that time, it will not be denied, that they exceed the same at present; especially if we consider, that the number of christenings in Paris, at a medium of 9 years, preceding that of 1737, exceeded that of the burials 98 yearly; notwithstanding that city not only abounds with a vast number of religious of both sexes, who are sworn to celibacy, but likewise many thousands of students belonging to the university, who lead a single life; whereas in London, there are no such persons, to prevent the increase of its inhabitants.

And as in the author's political account of London, it appears that, at a medium of 9 years, there are annually buried in London 29542, and in Paris only 17804, which is 11738 in favour of the former; so must the births in London at present, according to the above-specified 10 years account, the reasons aforesaid, and the Paris account of christenings, yearly exceed those of Paris 12320; whence it appears, that the inhabitants of London exceed those of Paris, above three fifths in number.

Mr. Kersseboom seems dissatisfied with Sir Wm. Petty's assertion, that the city of London contained as many inhabitants as the province of Holland and West-Friesland: which our author thinks will be no difficult matter to make appear, by allowing that gentleman his supposed number of 28000 children to be annually born in the said province; whereas, according to the above-specified

10 years account, and the Paris proportion of births, there must be annually born in London 31008 children: therefore, as this number, according to Mr. M.'s calculation, is the produce of 725903, the present number of the inhabitants of London; so must 28000, the number of children supposed to be born yearly in the province of Holland and West-Friesland, be the produce of 655485, the present number of the inhabitants of the said province. Notwithstanding Mr. Kersseboom, by his excessive and unprecedented reckoning of the births at a thirty fifth part of the people, has calculated them at 980000; whereas by the ingenious and learned Dr. Halley's method of calculation, which is so highly approved of by Mr. Kersseboom, that he seemingly would be thought to make it the standard of his calculations, the inhabitants of the province of Holland and West-Friesland do not amount to 29 times the number of the births, which gives room to suspect, that Mr. Kersseboom has introduced this excess, to increase the number of people in the said province of Holland and West-Friesland.

A Water-Level to be fixed to Davis's Quadrant, by which an Observation may be taken at Sea, in thick and hazy Weather, without seeing the Horizon. By Charles Leigh, Gent. N° 451, p. 413.

The sea-quadrant now in use, invented by Capt. Davis, for taking the sun's altitude, is an instrument well known, universally approved, and sufficiently accurate; this, together with a long use of this instrument, has occasioned such a fondness for it, that it would be no easy matter to dissuade the navigator from the use of it, to any other.

It is true, that when the natural horizon is obscured by thick and hazy weather, this instrument, as it now stands, is of no use; which too often occasions melancholy consequences, such as the loss of ships and cargoes, and men's lives. If therefore, to this instrument an apparatus were added, such as an artificial or portable horizon, that could be as effectually relied on, as that of the true or natural; and at the same time plain, easy, and obvious; it would be needless to attempt proving its usefulness.

The principle on which Mr. Leigh's apparatus is founded, is, "That the surface of all liquids, when free from any external cause, that have a communication with each other, though divided and separated in their surfaces, will be truly in a horizontal plain."

The quadrant, and its construction, being well known, it is sufficient to notice the two sections of two concentric circles, as AB, CD, fig. 3, pl. 7, on which the degrees and minutes are graduated; E, the common centre, through which goes a brass pin fixed to the apparatus EF, which is an index or radius to the section CD, on which index is fixed a brass tube 15 inches long, in the extremities of which are fixed perpendicularly two glass tubes Eh and dh. 4 inches long, with brass ferrels on the tops.

On the central pin, which is fixed in the index, is also fixed the brass horizontal vane EZ obliquely, in which is a hole for the central glass tube Eh, to come through three fourths of its length; close to which, and from the common centre, comes a white fine thread, the end being fixed in the vane EZ; and in the same manner is a thread fixed close to the glass tube dh.

To prepare this instrument for observation, pour water into the tube Eh, till its little surface rises to the central thread; then to keep it fixed there, shut the slide or stop, fixed on the top of the central tube, and there it will continue; then you may at pleasure pour or drop water into the tube dh, till its surface also rises to the thread fixed there; and if too much water is dropped in, dip in a wire with a small bit of sponge or cotton fixed to the end, till you exactly trim the tubes; for the greatest nicety and exactness lies in trimming the surfaces true to the threads.

Being thus prepared for observation, place yourself conveniently, where there is the least motion, on a stool or the deck, and having the quadrant in its proper position on your lap, open the slide on the top of the tube Eh, that the water may have its natural tendency, which will be truly horizontal, conformable to the above principle; then keeping your eye on the central thread, bring that and the little surface into one, which will be effected with the same ease, as if you observed by the natural horizon; then keep moving the end of the index F, till you bring the speculum of the sun in the little hole on the horizon vane, close to the thread, so that you have, as it were, but one object to look at during the time of observation.

But if you use the shadow vane, you must bring the upper edge of the shadow on the central line, drawn on the horizon vane, as usual; remembering as often as you rest, waiting the sun's rising, to close the slide, which prevents the water's running out, it then remaining immoveable. And thus continuing to do, till the sun is on your meridian, cast up the two sums as is usual, that is, the degrees cut by the shadow vane, and those cut by the upper edge of the index on the greater arch, which sum will give what is required, viz. the sun's distance from the zenith. On the end of the index is fixed a sight vane N, by which you may observe by the natural horizon, the very same way as with the common quadrant; so that the one will be the proof of the other.

The Description and Use of an Apparatus added as an Improvement to Davis's Quadrant, consisting of a Mercurial Level, for taking the Co-altitude of the Sun or a Star at Sea, without the usual Assistance of the sensible Horizon, which frequently is obscured. By Charles Leigh, Gent. N° 451, p. 417.

Since the former communication, in the foregoing article, Mr. L. has made such alterations and improvements in it, as have rendered it complete and perfect for the use intended, which have been confirmed by repeated experiments, as well on board ships, as on shore.

To arrive at perfection in navigation, 3 things are absolutely requisite, viz. the variation, the latitude, and the longitude; which last is as yet concealed from us. The two former indeed, we have a tolerable certainty of, especially the first, which may be found by observation, almost at any time the sun is visible, in or above the horizon, either by an amplitude or azimuth; but it is not so in regard to the latitude, by any certain method, but what is considered as too abstruse for common practice; for it is but once in 24 hours that an observation can be made by the sun, and even that space of time is so very short, that if the horizon should then be obscured, or a cloud intercept the sun's rays, the dead reckoning is then the only guide, which in fact, is little better than groping in the dark.

The first instrument made conformable to the same principle, was with a water-level; but finding that water was subject to some inconveniencies, Mr. Leigh has altered the apparatus, and changed the fluid from water to mercury: this alteration and improvement will better appear by the instrument, represented fig. 3, pl. 7, where AB, CD, represent the segments of two different concentric circles; E the common centre, in which moves the pin or axis fitted to the index or label EF; on which label is also fixed the horizontal tube Gg, which has a communication with the two glass vertical tubes Eh, dh, in which the mercury moves. On each top of the vertical tubes are fixed a large hollow brass cylinder hh, having in their tops a pin, by closing of which, the included air is prevented from any communication with the external; by which means this advantage is obtained, that it prevents, in a great measure, that too quick and vibratory motion, natural to the fluidity, joined to the gravity of mercury when moved, and at the same time, by having a sufficient space and quantity of air in the cylinders at top, does not in the least impede the true level; but notwithstanding this precaution, the mercury still would be subject to a tremulous motion, were it not that the diameters of the vertical tubes, to that of the horizontal, are as 2 to 1, and consequently the area 4 to 1; by which means this inconveniency is also removed, without any way affecting the horizontal level.

The first trimming or preparing the tubes with mercury is sufficient, and when the two little convex surfaces of the mercury appear just visible above the level rings EE, then is the instrument correctly trimmed; if they appear much above or below the rings, move the tubes a little up or down, till the surfaces are adjusted to the rings; which is effected by means of the regulating screw l, fixed at the end of the base tube.

To observe by the sun, was described in the former article; but to observe by a star, another person must look through the slit on the horizon vane, and over the upper edge of the shade vane, and bring the star to coincide with it, proceeding in the same manner as before, with the sun.

An Account of the Extirpation of part of the Spleen of a Man. By Mr. John Ferguson, Surgeon. N° 451, p. 425.

On the 5th of January, Mr. F. was called to Thos. Conway, who had received a wound with a great knife, which went through the muscular part of his fore-arm, and into the left hypochondrium. It was 24 hours after he had received the wound before Mr. F. saw him. He found the spleen out at the wound, and that what by pressing and thrusting of it with the fingers, endeavouring to return it into its place, which they that were about him could not accomplish, and by being so long exposed to the air, it was quite cold, black and mortified. He considered that cutting away the mortified part, must be attended with the greatest danger, and was to him, an unprecedented case; yet that the patient must inevitably die, if it was not done: he therefore made a ligature with a strong waxed thread, above the unsound part, and cut off $3\frac{1}{2}$ oz. of the spleen: notwithstanding the ligature, there was a pretty large artery that sprung with great violence, which he immedately tied up; and, after bathing all the parts with warm wine, he returned the remaining part of the spleen into its place, leaving the ends of the threads out of the wound, to draw them away by, when they should digest off, which they did on the 10th day, and came away with the dressings.

He dressed the wound with digestives, and the abdomen was stuped twice a day with an emollient fomentation; and after stuping, it was always malaxated with an emollient liniment, which the patient said always gave him ease. What he most complained of, was that he could not make water, for which Mr. F. every day gave him a carminative clyster, which kept his belly from swelling; and always when the clyster came away, he got some water made along with it:

this symptom went off on the 7th or 8th day. He perfectly recovered, followed his business, and found no inconvenience from the want of the part of the spleen which he lost. The wound through his arm was also quickly cured.*

Concerning a Ball of Sulphur supposed to be generated in the Air. By Mr. Benjamin Cooke, F. R. S. of Newport in the Isle of Wight. N° 451, p. 427.

The great heats we have lately suffered, were ushered in by a very gloomy night of almost continual lightning, accompanied with very loud claps of thunder, which, as usual, were towards the morning followed by very heavy showers of rain. Early next day, in a meadow near the sea-shore, far from any house, and where it has not been known that any improvement has been carried on, a husbandman found a beautiful yellow ball lying on the turf. It proved to be of sulphur, of which it smelt uncommonly strong. It was frosted, as it were, all over with an efflorescence of fine, shining, yellowish crystals, which soon fell off with the lightest touch.

It has on one side, a deep hole, admitting the end of a middle-sized knitting-needle, and on the opposite side a deep depression; which would induce one almost to think its form had been at first nearly spheroidal, formed by a revolution round a supposed axis connecting those two parts. It has several other holes scattered irregularly up and down its whole surface, some fit to admit a hog's bristle, others a hair; as if it had been made of a fine powder, and some thin liquid, and after mixing had suffered some fermentation; but those parts of it which are solid, seem more compact than those of the common roll brimstone of the shops, and the powder of it burns with a whiter flame, and less acid fumes. Its longest diameter is between 8 and 9, and its shortest betwixt 6 and 7 tenths of an inch; its weight is 108 grains.

We find frequent mention, in the description of thunder storms in hot climates, that there falls often a flaming bituminous matter to the ground, which sometimes burns not to be soon extinguished, but more frequently spatters into an infinite number of fiery sparks, doing great damage where they strike, always attended with a sulphureous suffocating smell, commonly compared to that of gunpowder.

Whether this sulphureous ball was intended for one of these, but by some accident missed firing, it is now time to consider. Had it been formed in the earth, how should it get to the surface, without losing that most elegant frosty covering of fine shining crystals, and appear not in the least sullied, or its pores

* Mr. F. in the letter prefixed to this account remarks, that though the spleen had been often taken out of dogs, yet this he believes to be the first instance of the extirpation of a considerable portion of it in the human subject.

filled with earth, or other terrestrial matter; on the contrary, not the least adhesion of any thing of that kind can be observed: besides, brimstone made the ordinary way, seems to have a different texture of its internal parts from this ball. From these observations Mr. Cooke concludes it not formed in the earth.

An Account of a Book entitled, Observationes de Aere et Morbis epidemicis, ab Anno 1728, *ad finem Anni* 1737, *Plymuthi factæ. His accedit Opusculum de Morbo Colico Damnoniensi. Auctore Joanne Huxham, M. D., R. S. S. Londini,* 1739, 8*vo. Drawn up by Thomas Stack, M. D., F. R. S.* N° 451, p. 429.

This book and the continuation of it, are so well known to medical men of the present day, that it is deemed unnecessary to reprint Dr. Stack's analysis of it.

An Abstract by C. Mortimer, M. D. Secr. R. S. of an Inaugural Dissertation published at Wittemberg 1736, *by Dr. Abraham Vater, F. R. S. concerning the Cure of the Bite of a Viper, cured by Sallad-Oil.* N° 451, p. 440.

The author being informed of the use of oil of olives against the bite of vipers, in the case of William Oliver, before several members of the R. S. and others; he communicated the same to Dr. Vater at Dresden, who had an opportunity of trying the efficacy of this remedy, by an accident happening in that city; which case being remarkable, he has related it at large in the above-mentioned dissertation, as follows:

The head journeyman in the Royal Dispensary at Dresden, being the last year preparing some Italian vipers for a patient of distinction, was, through negligence, bitten by one of them in one of his fingers. The man, finding himself wounded, was greatly frightened, he tried various things; among others he applied theriaca outwardly to the wound, but felt no relief from it; and in the space of a few hours, his whole arm swelling to an enormous degree, he felt great pain in it, with remarkable tensions under his arm-pit towards his heart, attended with a faintness. Therefore, almost despairing of recovery, having tried all things in vain; he went to Dr. Vater, who having been informed of the virtue of olive-oil in this case, as before mentioned, ordered the man to anoint his whole arm with it hot, and several times, on which the desired effect soon followed: for the swelling, after one or two anointings, began to fall; the pains, with the other symptoms, were assuaged, and gradually ceased, and the patient recovered perfectly in a day or two. He took nothing inwardly besides a simple mixture, (Sp. Vitriol dulcis. Sp. Vitriol, p. i, Sp. V, p. iij,) with an anodyne mineral liquor, on which a copious sweat ensued, which sensibly relieved the patient. Though this medicine might contribute to the cure, yet the chief part in this affair is to be ascribed to the oil of olives, because on anointing with it, the symptoms abated instantly.

Dr. Vater, in speaking of the serpentine or viper-stone, relates a very extraordinary accident, if true, from Kæmpfer's Amænit. p. 579. The case was this: in the house of a Dutch governor on the coast of Coromandel, a servant maid happened to be bitten in the foot by a cobra cabelo. The serpentine-stone was immediately laid on; which falling off, and no other being to be had, nor any new milk being at hand to wash out the pores of the stone in, a wet nurse being in the house, who was anxious for the sudden effects of the poison, milked some milk upon the stone out of her own breasts; on which her nipple began immediately to be painful, and soon after the whole breast of that side swelled, and was inflamed, even to the hazard of her life for 3 days together, and the hardness did not leave her breast in less than 10 days. It must be remarked, that her nipple was before somewhat excoriated by the gum of her nursling, by which the small veins being laid bare, it was readier to receive the infection of the venom rendered more active by the warmth of the milk.

When he speaks of oil of olives in particular, and its effects against poison in general, he cites a remarkable passage from Matthiolus in his Comment, Lib. 2, Dioscorid. p. 232, where he says, he had found by experience, that oil prepared by himself, into which a great number of scorpions had been put, being anointed on the heart, and where the pulsations of the arteries of the hands and feet are felt, frees from all poisons; nay, it likewise cures those who have been bitten by vipers, or stung by any other venomous animals. The author, comparing this with the virtue of the oil alone, for the bite of a viper, concludes, that the scorpions infused in it add nothing to its real virtue.

He concludes this dissertation, by endeavouring to explain the manner of its operating, which he attributes to its fat inviscating nature, by which it sheathes the spiculæ of the poison. He remarks, that Celsus, Lib. 5, c. 27, advises, after dipping a person in a hydrophobia in cold water, to put him into warm oil. Lastly, he mentions the great secret of the viper-catchers, that is, the fat of vipers; which, he thinks, acts in the same manner as the olive-oil.

Abstracts of 2 Letters from M. Dufay, F. R. S. &c. to Dr. Mortimer, Sec. R. S. concerning the Efficacy of Oil of Olives in curing the Bite of Vipers. N° 451, p. 444.

In the 1st extract, dated Paris, Aug. 8, 1737, M. Dufay says, after he had given the Academy an account of Dr. M.'s observations on the remedy against the bite of vipers, a committee was appointed to make the same experiments. But whether it be, that the French vipers are more venomous than the English, or that the bites were more considerable, of the several pigeons and fowls that were bitten, not one recovered, though they were immediately rubbed with oil. They died in $\frac{1}{4}$ of an hour, or in 1 hour's time at farthest. The like experiments have been made on several other animals; but as the gentlemen are resolved to repeat them, M. D. did not send an account of them. All he could say at that time was, that the remedy seemed to be not so sure in France as in England, where he found by the public newspapers, that a rattle-snake had been brought, and that its bite has been cured by the same remedy.

In the 2d Abstract, dated at Paris, Dec. 11, 1737, he says, two members of the Academy had been employed to make the experiments relating to the cure of the bite of vipers, and they had accordingly made some upon dogs, cats, pigeons, chickens, ducks and turkeys; some of which had been cured, but some others died notwithstanding this remedy; and there were even some that did not die, though they were bitten very deep, and yet no application of oil was made. This is the report they had made of these experiments; and they are determined to make new ones. All that can be thought concerning the difference of the success of this remedy at London and at Paris, is, that all vipers are not equally venomous; that all bites are not perhaps equally easy to be cured; and that the vipers in France are more dangerous than those in England. Finally, the sequel of these experiments will probably teach us, in what cases this remedy may be applied in this country, France.

M. D. then remarks that they had made with success the phosphorus of Kunckel, as good and as fine as that of Mr. Godfrey: they made 9 drachms at the first operation.

Concerning the Poison of Henbane-Roots. By Dr. Patouillat, Physician at Toucy in France. N° 451, p. 446.

The 26th of Jan. 1737, Dr. P. was called to a cottage near Toucy; where he was surprised to find 9 persons together, all having the true symptoms of being poisoned; with this difference, that some were speechless, and showed no other signs of life than by convulsions, contortions of their limbs, and the risus sardonicus; all having their eyes starting out of their heads, and their mouths drawn backwards on both sides; others had all the symptoms alike. However, 5 of them now and then opened their mouths, but it was to utter howlings: and whenever they expressed articulated words, it seemed as if they would prophesy. One, for example, said, In a month my neighbour will lose a cow: another, In a little time you will see the crown pieces of 60 pence at 5 livres. Among these 9 persons, there was a woman 5 months gone with child, and a child of 2 years; 4 boys of 9, 12, 15 and 18; and 3 girls of 15, 17 and 19 years of age, who had all 3 the misfortune of the green-sickness upon them at that time. The madness of all these patients was so complete, and their agitations so violent, that in order to give one of them the antidote, he was forced to employ 6 strong men to hold him, while he was getting his teeth asunder, to pour down the remedy: and as they could not all be watched at once, one of the boys got away, and ran to a pond 100 paces from the house, into which he leaped; but as he was seen, he was soon taken out.

It was in vain to examine those wretches concerning the nature of the poison they had taken, as they were quite senseless. Happily the father of the family, by being absent, was free from this misfortune. Of him Dr. P. learned, that digging his garden the preceding day, he had found several roots resembling common parsnips; and having carried them home for parsnips, they were boiled in the soup; and the unlucky mistake was not apprehended, till the children were in this dreadful state. He described the plant, which he thought he had taken for parsnips; on which Dr. P. went into the garden, to find what it was; but as it had no leaves, he was obliged to derive the knowledge of it from the roots; and soon knew it to be the henbane,* which is a very strong poison; and so much the more dangerous, as the patients could give no account of their ailments, nor of the quantity of the poison they had taken.

To the boys Dr. P. gave the Tartar. Stibiat. in so large a dose, that the oldest took 45 grains, and the others in proportion.

For the woman, he had recourse to Theriaca in a triple dose; not thinking it safe to give her the emetic, on account of her pregnancy. He gave the same remedy to the child, by reason of its tenderness.

To the girls, besides the Theriaca, which they took in very large doses, having used 4 oz. of it, he gave warm milk, in which was dissolved salt of rue. The next day he visited the patients, and found them in a quite different condition; for they had all recovered the use of their reason, but remembered

* Hyoscyamus niger. Linn.

nothing of what had happened. All this day, every object appeared double to them, that is, on looking at a man, or a beast, or a tree, they saw two.

He returned to see them the day after, and found that the symptoms were removed; but were succeeded by another altogether as surprising, viz. all objects appeared to them as red as scarlet. This last symptom ceased gradually on the third day, and after that time they made no complaint.

Concerning the Virtues of the Star of the Earth, Coronopus, or Bucks-horn Plantain, in the Cure of the Bite of the Mad-Dog. By the Rev. Mr. Tho. Steward, V. D. M. N° 451, p. 449.

Mr. S. here states that, in the Phil. Trans. N° 443, two vegetables are mentioned as possessing great virtues against the hydrophobia; viz. the lichen-cinereus terrestris and the stellaria or star of the earth, by which king James's hounds, that had been bitten by a mad-dog, are said to have been cured. But the last mentioned plant (the star of the earth) he apprehends to be a mistake, Mr. Ray having taken the Spanish catchfly for the star of the earth; whereas according to Mr. S. the true star of the earth is the coronopus or bucks-horn plantain, as Mr. Ray himself acknowledged in a letter to Mr. S. adding that he (Mr. Ray) concluded that a false plant had been sent to the king for the star of the earth.

Relative to this subject Mr. S. introduces the following letter from Sir Hans Sloane, the then president of the R. S. to Mr. Ray, dated June 1, 1687, in these words: "I send you inclosed the specimen of a plant growing on New-market-Heath, and in Surrey, known by the name of the star of the earth in those parts. It is particularly taken notice of on the account of its extraordinary and admirable virtue, in curing the biting of mad-dogs, either in beasts or men. One of his majesty's huntsmen having proved it a great many times, gave the king his way of using it, which was an infusion in wine with treacle, and one or two more simples. His majesty was pleased to communicate it to Gresham College, to the R. S. and nobody knowing the plant by that name, some there present confirming its use in some parts of England in that disease, the herb being as little known here as if it came from the Indies, I told the society, I would let you have the best specimen of it, which I question not is known to you. If you please to give your sentiments, you will extremely oblige, &c." To this Mr. Ray returned the following answer: "I received your letter with the specimen inclosed, which seems to me to be the sesamoides salamanticum magnum of Clusius, or lychnis visc. &c. of Bauhin, which I have observed to grow plentifully upon Newmarket Heath, &c. I wonder it should have such a virtue as you mention, but it seems it is well attested. Dr. Hulse writes to me, he finds it in Grey's Farrier."* This (Mr. S. observes) seems pretty evidently to refer to the same plant mentioned by Aubry,† and this surely was the plant that not being well dried and preserved, the society could not tell what to make of, and which Mr. Ray found to be the sesamoides, which he then thought was the plant that Grey called the star of the earth; but upon further consideration, he was firmly persuaded, that the coronopus, and not the sesamoides, was the plant intended by de Grey (for so his name ought to be written) and indeed, to Mr. S. there seemed to be the greatest probability, if not absolute certainty, of this latter opinion; for the sesamoides was a plant so little known in Grey's time, that the botanists who were contemporary with him, took it for a plant that was wholly a stranger in England, as may be seen in Johnson upon Gerard, and in Parkinson, and the manner of giving it, as directed by Grey, viz. first 3, then 5, and then 7 plants, roots and all, speaks it to be a small herb, such as is the coronopus, and not such a large one, with a great, sticky or woody root, as the sesamoides. Of this Mr. S. was very sure, that in Norfolk, his native county, and which, if he mistook not, was Grey's also, the coronopus is called the star of the earth (and among other names given it by Dodonæus, this of Stellaria, and Stella Terræ, is one, p. 95 of the English translation; and he describes it as lying

* Entitled, The Expert Farrier, 2d edition, 4to. 1652, p. 160. His receipt is as follows: "Take the herb which grows in dry and barren hills, called the star of the earth; gathering it 3 days together. The first time you must gather 3 of these herbs, with all the whole roots; and wash them clean, and pound them well; which done, give them to your horse in milk, beer, ale, or white-wine; but be careful the horse takes all the herbs and roots: if you will, you may make up these herbs and roots in fresh or sweet butter, which will do as well. The 2d day give the horse 5 of these herbs and roots, as before; and the 3d day give him 7. Do this punctually, and you may be well assured the horse will be perfectly cured; for though I have never tried this medicine, yet I know that the party of whom I had it, has cured many cattle of all sorts with it. I myself can say thus much of this receipt, that I knew it cure a whole kennel of hounds, one beagle excepted, which they did not suspect to be bitten; so he fell mad and died, but all the rest escaped. Another time, a gentleman's son of my acquaintance was unfortunately bitten, who was cured by the party who taught me this receipt; and this young gentleman, who was then a boy of 10 years old, was so far spent with the disease, before this man took him in hand, that his senses were affected, and he talked very idly; yet he cured him, so as he lived and did well, &c."

† In a letter to Mr. Ray, published by Mr. Derham, and dated Aug. 5, 1691, wherein he says, that king James sent to the R. S. a plant called the star of the earth, with the receipt made of it to cure the bite of a mad dog, which is in Phil. Trans. N° 187.

spread upon the ground like a star; and Gerard gives the same description of it, and Parkinson, in his Theatrum, yet more fully, p. 501, viz. that the leaves lie round about the root in order one by another, thereby resembling the form of a star, and therefore called herba stella; by which name, among others, it is called by Cæsalpinus, Lobel, &c. But whoever met with the name stellaria, or stella terræ, among the synonyma of the sesamoides in any botanic writer before Mr. Ray, who afterwards retracted it, as has been fully proved? In that part of Norfolk where Mr. S. was born, not far from Norwich, towards the sea-coast, where the bucks-horn plant grows abundantly, there was great use made of it when he was but a lad, and always with good success, so far as ever he could hear. One story he could tell of his own knowledge, which might seem too trifling to mention, were it not to show the efficacy of the simple. About 40 years ago, when he lived at a place called Debenham in Suffolk, a person unknown to him, having heard that he knew an herb that was good against the bite of a mad-dog, sent to desire a sample of it, with directions how to use it; and sometime after he had half a dozen fine chickens brought him. He asked whence they came? It was answered from such a one, the name he had forgotten. He said he did not know him: to which the reply was, that it was the man to whom he had sent the plantain, which had saved the lives of half a dozen hogs of his, that had been bitten by a mad dog; and he thought the least he could do was to send me half a dozen chickens as a token of his gratitude. After all, Mr. S. would not be positive, that the lychnis, or catch-fly, was not good contra morsum canis rabidi; but was confident that it was not the true star of the earth.

In a P. S. Mr. S. states, that a friend of his had informed him, that there was a wonderful cure performed on a woman in Suffolk, several years ago, who had been bitten by a mad-dog, and in whom evident symptoms of the hydrophobia appeared, who yet was saved, by the use of a powder given by the direction of the Lady Brook in Suffolk. It seems the powder went by the name of The Lady Brook's Powder, and was generally supposed to be chiefly, if not only, the coronopus dried and pulverized: and he had such an opinion of the great virtue of this simple, that till he had some convincing evidence of its having failed, he could scarcely avoid considering it as a specific against the bite of a mad-dog.

Of the Reduction of Radicals to more Simple Terms. By Mr. Abr. Demoivre, F. R. S. N° 451, p. 463. *From the Latin.*

Mr. Demoivre having explained, in the appendix to Saunderson's Algebra, his method of extracting any root of the binomial $a + \sqrt{-b}$, this induced Wm. Jones, Esq. F. R. S.* to desire him to do the same by the possible binomial $a + \sqrt{+b}$; a request which Mr. D. here complies with, though he is sensible that this has been done already, by Sir I. Newton and others.

PROB. I.—*To reduce the Binomial* $\sqrt[n]{a + \sqrt{b}}$ *to Simpler Terms.* Suppose that this binomial, including its general radicality, can be reduced to the other binomial $x + \sqrt{y}$, freed from that radicality. Now to find such quantities x and y, try whether the sum of the binomials $\sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$ makes nearly an integer number, which may be readily done by a table of logarithms; if it do, then put $2x =$ to this whole number. Next try whether $\sqrt[n]{aa - b}$ be an integer; if it be, put $m =$ this new integer; then will $y = xx - m$; and therefore the given binomial will be reduced to the given form. But before proceeding to the demonstration, it may be illustrated by two or three examples.

Example 1.—Let the binomial $\sqrt[2]{54 + \sqrt{980}}$ be proposed.

Put $a = 54$, $b = 980$; then will $\sqrt{b} = \sqrt{980} = 31.3049$ nearly; which gives $a + \sqrt{b} = 85.3049$, and $a - \sqrt{b} = 22.6951$. Now the root of the first number is 9.236 nearly; and the root of the latter is 4.763; the sum of which roots is 13.999, which is very near the whole number 14. Therefore putting $2x = 14$, or $x = 7$; then since $y = xx - m$, and $m = \sqrt{aa - b} = \sqrt{1936} = 44$; therefore is $y = 49 - 44 = 5$; so that the binomial reduced will be $7 + \sqrt{5}$.

Example 2.—Let $\sqrt[3]{45 + \sqrt{1682}}$ be reduced simpler.

Put $a = 45$, $b = 1682$; then is $\sqrt{b} = 41.01219$ nearly; hence $a + \sqrt{b} = 86.01219$, and $a - \sqrt{b} = 3.89781$.

Now the cube root of the former number is 4.4142, and the cube root of the latter number is 1.5857; the sum of which roots being 5.9999, which is nearly 6; therefore put $2x = 6$, or $x = 3$; but it being $y = xx - m$, and $m = \sqrt[3]{aa - b} = \sqrt[3]{343} = 7$; therefore $y = 9 - 7 = 2$; and hence the binomial reduced is $3 + \sqrt{2}$.

Example 3.—Let $\sqrt[3]{170 + \sqrt{18252}}$ be reduced simpler.

Put $a = 170$, $b = 18252$, then will $\sqrt{b} = 135.1$ nearly; which gives $a + \sqrt{b} = 305.1$, and $a - \sqrt{b} = 34.9$.

Now the cube root of the former number is 6.73 nearly, and the cube root of the latter is 3.26, the sum of which roots is 9.99, nearly equal to the whole number 10. Put therefore $2x = 10$, or $x = 5$; then since $y = xx - m$, and

* See an account of Mr. Jones, who was the father of the late Sir William Jones, in the introduction to Dr. Hutton's Mathematical Tables, p. 119.

$m = \sqrt[3]{aa - b} = 22$; therefore $y = 25 - 22 = 3$; and hence the binomial reduced is $5 + \sqrt{3}$.

Demonstration—Take any binomial, as $\sqrt[3]{a + \sqrt{b}}$, which suppose reducible to the binomial $x + \sqrt{y}$; then, by cubing, $x^3 + 3xx\sqrt{y} + 3xy + y\sqrt{y} = a + \sqrt{b}$.

Put $x^3 + 3xy = a$, and $3xx\sqrt{y} + y\sqrt{y} = \sqrt{b}$.

Then whatever the index of radicality may be, from the square of the former part subtract the square of the latter, and there will remain $x^6 - 3x^4y + 3xxyy - y^3 = aa - b$; then extract the nth root of both sides, that is, in the present case the cube root, it will give $xx - y = \sqrt[3]{aa - b}$; or making $\sqrt{aa - b} = m$, it will be $xx - y = m$, and therefore $y = xx - m$. Now in the above equation writing a for $x^3 + 3xy$, and $xx - m$ for y, there results the equation $4x^3 - 3mx = a$.

Now resume the equation $2x = \sqrt[3]{a + \sqrt{b}} + \sqrt[3]{a - \sqrt{b}}$; and to take away the radicality $\sqrt{}$, make $a + \sqrt{b} = z^3$, and $a - \sqrt{b} = v^2$, there will then result these two new equations, $z^3 + v^3 = 2a$, and $z + v = 2x$; it follows therefore that $\frac{z^3 + v^3}{z + v} = \frac{a}{x}$. But $\frac{z^3 + v^3}{z + v} = z^2 - zv + v^2 = \frac{a}{x}$; and besides $zz + 2zv + vv = 4xx$.

Taking the difference of these equations, there results $3zv = 4xx - \frac{a}{x}$; but $z^3v^3 = aa - b$; therefore $zv = \sqrt[3]{aa - b}$, which being put $= m$, there will arise $3m = 4xx - \frac{a}{x}$, or $4x^3 - 3mx = a$, which is the very same equation that came out before; and thus it will revert to the same in every case of radicality whatever.

To try therefore whether the expression $\sqrt[n]{a + \sqrt{b}}$ can be reduced to a simpler state; put $2x = \sqrt[3]{a + \sqrt{b}} + \sqrt[3]{a - \sqrt{b}}$, and $\sqrt[n]{aa - b} = m$, also $y = xx - m$; then the expression reduced will be $x + \sqrt{y}$, if it can admit of integral, or at least rational quantities.

But in case these should not be integer or rational quantities, yet the rule above will be of use in solving equations of any kind, as will be seen hereafter.

In the mean time perhaps this doubt may arise, whether this rule will obtain universally in any powers whatever of the binomial, viz. whether in any binomial, whose index is n, if from the square of the sum of the terms in the uneven places, there be subtracted the square of the sum of those in the even places, the remainder will be another binomial having the index n. To which Mr. Demoivre answers, that this is a fact which has been observed by many writers, and therefore may be considered as confirmed by experience. But as he has never seen any demonstration of it, he adds the following.

Take the binomial $(x + y)$, and expand it; take also this other binomial $(x - y)^n$, which also expand; put $(x + y)^n = s$, and $(x - y)^n = p$; now it is evident that, if the expanded binomials be united by addition, their sum will give double the sum of the uneven terms of the first binomial; but if the latter be subtracted from the former, then the remainder will be double the sum of the even terms of the same binomial; hence it follows that $\frac{s+p}{2}$ is the sum of the uneven terms, and $\frac{s-p}{2}$ the sum of the even terms.

From $\frac{ss + 2ps + pp}{4}$, the square of the first sum, taken $\frac{ss - 2ps + pp}{4}$, the square of the last, the remainder will be $ps = (x + y)^n \times (x - y)^n = (xx - yy)^n$, the nth root of which is $xx - yy$.

Corol.—By putting $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, and making $\sqrt[n]{aa - b} = m$, then expounding n successively by 1, 2, 3, 4, 5, 6, &c. there will arise the following equations:

1st. $x = a$
2d. $2x^2 - m = a$
3d. $4x^3 - 3mx = a$
4th. $8x^4 - 8mx^2 + m^2 = a$
5th. $16x^5 - 20mx^3 + 5m^2x = a$
6th. $32x^6 - 48mx^4 + 18m^2x^2 - m^3 = a$
7th. $64x^7 - 112mx^5 + 56m^2x^3 - 7m^3x = a$
&c.

Now these equations are of the same form as the equations for cosines, though they are things of a quite different nature. Thus, let r be the radius of a circle, c the cosine of any given arc, and x the cosine of another arc, which is to the former, as 1 to n. Then it will be,

1st. $x = c$
2d. $2x^2 - r^2 = rc$
3d. $4x^3 - 3r^2x = r^2c$
4th. $8x^4 - 8r^2x^2 + r^4 = r^3c$
5th. $16x^5 - 20r^2x^3 + 5r^4x = r^4c$
6th. $32x^6 - 48r^2x^4 + 18r^4x^2 - r^6 = r^5c$
7th. $64x^7 - 112r^2x^5 + 56r^4x^3 - 7r^6x = r^6c$
&c.

Now putting $r = 1$, for brevity, then the general form of these is $2^{n-1} \times x^n - 2^{n-3} \times \frac{n}{1} x^{n-2} + 2^{n-5} \times \frac{n}{1} \cdot \frac{n-3}{2} x^{n-4} - 2^{n-7} \times \frac{n}{1} \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} x^{n-6} +$ &c. $= c$.

The difference in these equations consists chiefly in this, that the former are derived from the equation $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, but the latter from the equation $2x = \sqrt[n]{a + \sqrt{-b}} - \sqrt[n]{a - \sqrt{-b}}$; and if this latter equation be freed from its general radicality, there will be obtained equations for the cosines.

Let therefore the equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt[3]{a - \sqrt{-b}}$ be proposed to be freed from its radical sign $\sqrt[3]{}$.

Put $\sqrt[3]{a + \sqrt{-b}} = z$, and $\sqrt[3]{a - \sqrt{-b}} = v$; also put $z + v = 2x$. Hence it will be 1st, $z^3 = a + \sqrt{-b}$, and 2nd, $v^3 = a - \sqrt{-b}$; consequently $z^3 + v^3 = 2a$. But since $z + v = 2x$, therefore $\frac{z^3 + v^3}{z + v} = \frac{a}{x} = zz - zv + vv$. But $(z + v)^2 = zz + 2zv + vv = 4xx$; therefore $3zv = 4xx - \frac{a}{x}$. Now since $z^3v^3 = aa + b$; therefore $zv = \sqrt[3]{aa + b}$; which being put $= m$, it will then be $4xx - \frac{a}{x} = 3m$, or $4x^3 - 3mx = a$.

Hitherto we have had two kinds of equations; the first, in which m was put $= \sqrt{aa - b}$; the latter, in which it was $= \sqrt[3]{aa + b}$. The former may be called hyperbolical, the latter circular.

PROB. 2.—*To extract the Cubic Root of the Impossible Binomial* $a + \sqrt{-b}$.

Suppose that root to be $x + \sqrt{-y}$, the cube of which is $x^3 + 3xx\sqrt{-y} - 3xy - y\sqrt{-y}$.

Now put $x^3 - 3xy = a$, and $3xx\sqrt{-y} - y\sqrt{-y} = \sqrt{-b}$. Then the squares of these will give two new equations, viz.

$$x^6 - 6x^4y + 9x^2y^2 = aa,$$
$$-9x^4y + 6x^2y^2 - y^3 = -b.$$

Then the difference of these squares is $x^6 + 3x^4y + 3x^2y^2 + y^3 = aa + b$; the cubic root of which is $xx + y = \sqrt[3]{aa + b} = m$ suppose; hence $y = m - xx$; which value of y substituted in the equation $x^3 - 3xy = a$, gives $x^3 - 3mx + 3x^3 = a$, or $4x^3 - 3mx = a$; which is the very same equation, as had before been deduced from the equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt{a - \sqrt{-b}}$; but yet it does not follow, that in the equation $4x^3 - 3mx = a$, the value of x can be found by the former equation, since it consists of two parts each including the imaginary quantity $\sqrt{-b}$; but this will be best done by means of a table of sines.

Therefore let the cube root be extracted of the binomial $81 + \sqrt{-2700}$. Put $a = 81$, $b = 2700$: then $aa + b = 6561 + 2700 = 9261$, the cubic root of which is 21, which put $= m$, which makes $3mx = 63x$; therefore the equation to be resolved will be $4x^3 - 63x = 81$, which being compared with the equation for the cosines, viz. $4x^3 - 3rrx = rrc$, gives $rr = 21$, hence $r = \sqrt{21}$, therefore $c = \frac{a}{rr} = \frac{81}{21} = \frac{27}{7}$.

To find then the circular arc to the radius $\sqrt{21}$, and cosine $\frac{27}{7}$; put the whole circumference = C, and take the arcs $\frac{A}{3}$, $\frac{C-A}{3}$, $\frac{C+A}{3}$, which will easily be known by a trigonometrical calculation, especially by using logarithms; then the cosines of the arcs to the radius $\sqrt{21}$, will be three roots of the quantity x; and since $y = m - xx$, there will therefore be as many values of y, and thence a triple value of the cube root of the binomial $81 + \sqrt{-2700}$; which must now be accommodated to numbers.

Make then as $\sqrt{21} : \frac{27}{7}$ :: so is the tabular radius : to the cosine of an arc A, which will be nearly 32° 42′; hence the arc C − A will be 327° 18′, and C + A 392° 42′, of which the 3d parts will be 10° 54′, and 109° 6′, and 130° 54′. But now as the first of these is less than a quadrant, its cosine, that is, the sine of 79° 6′, ought to be considered as positive; and both the other two being greater than a quadrant, their cosines, that is, the sines of the arcs 19° 6′ and 40° 54′, must be considered as negative. Now by trigonometrical calculation it appears, that these sines, to radius $\sqrt{21}$, will be 4.04999, and − 1.4999, and 3.0000, or $\frac{9}{2}$, and $-\frac{3}{2}$, and -3. Hence there will be as many values of the quantity y, viz. all those represented by $m - xx$, viz. $21 - \frac{81}{4}$, and $21 - \frac{9}{4}$, and $21 - 9$, that is, $\frac{3}{4}$, $\frac{75}{4}$, 12, the square roots of which are $\frac{1}{2}\sqrt{3}$, $\frac{5}{2}\sqrt{3}$, $2\sqrt{3}$; therefore the three values of $\sqrt{-y}$, will be $\frac{1}{2}\sqrt{-3}$, $\frac{5}{2}\sqrt{-3}$, $2\sqrt{-3}$; hence the three values of $\sqrt[3]{81 + \sqrt{-2700}}$ are $\frac{9}{2} + \frac{1}{2}\sqrt{-3}$, and $-\frac{3}{2} + \frac{5}{2}\sqrt{-3}$, and $-3 + \frac{1}{2}\sqrt{-3}$. And by proceeding in the same manner, there will be found the three values of $\sqrt[3]{81 - \sqrt{-2700}}$, which are $\frac{9}{2} - \frac{1}{2}\sqrt{-3}$, and $\frac{3}{2} - \frac{5}{2}\sqrt{-3}$, and $-3 - \frac{1}{2}\sqrt{-3}$.

There have been several authors, and among them Dr. Wallis, who have thought that those cubic equations, which are referred to the circle, may be solved by the extraction of the cube root of an imaginary quantity, as of $81 + \sqrt{-2700}$, without any regard to the table of sines: but that is a mere fiction; and a begging of the question; for on attempting it, the result always recurs back again to the same equation as that first proposed. And the thing cannot be done directly, without the help of the table of sines, especially when the roots are irrational; as has been observed by many others.

PROB. 3.—*To extract the nth Root of the Impossible Binomial* $a + \sqrt{-b}$.

Let that root be $x + \sqrt{-y}$; then making $\sqrt[n]{aa + b} = m$, and $\frac{n-1}{n} = p$, describe, or conceive to be described a circle, the radius of which is $\sqrt{m}$, in which take any arc A, the cosine of which is $\frac{a}{m^p}$; and let C be the whole circumference. To the same radius take the cosines of the arcs $\frac{A}{n}$, $\frac{C-A}{n}$, $\frac{C+A}{n}$, $\frac{2C-A}{n}$, $\frac{2C+A}{n}$, $\frac{3C-A}{n}$, $\frac{3C+A}{n}$, &c. till the number of them be equal to n. Then all these cosines will be so many values of x; and the quantity y will always be $m - xx$.

PROB. 4.—*Having given any Equation, of the Kind of those above described; to know whether its Solution is to be referred to the Hyperbola or to the Circle.*

Let n denote the highest dimension of the equation: divide the coefficient of the second term by $2^{n-3} \times n$, calling the quotient m: then see whether the square aa be greater or less than m^n; if it be greater, the equation is to be referred to the hyperbola; but if less, to the circle.

Let there be given the equation $16x^5 - 40x^3 + 20x = 7$, where $n = 5$; therefore $2^{n-3} \times n = 20$: divide 40 by 20, the quotient is $2 = m$; hence $m^n = 32$, and $aa = 40$; and as this is greater than the power 32, the equation is to be referred to the hyperbola. But since in the hyperbolical case there was put $\sqrt[5]{aa - b} = m$, it follows that $aa - b = m^5 = 32$, and therefore $b = aa - 32 = 49 - 32 = 17$. Now the root of the equation in this case is $\frac{1}{2}\sqrt[5]{7 + \sqrt{17}}$ $\sqrt[5]{7 - \sqrt{17}}$: but $\sqrt{17} = 4.123105$ nearly; therefore $7 + \sqrt{17} = 11.123105$, and $7 - \sqrt{17} = 2.876895$; also the 5th root of the former number is 1.6221, and the 5th root of the latter 1.2353, the sum of which roots is 2.8574, and the half sum 1.4287 is the value of x in the given equation.

Again, let the equation $16x^5 - 40x^3 + 20x = 5$ be given; in which m is still $= 2$, but $a = 5$, and the square aa is less than 2^5 or 32; therefore the value of x cannot be obtained without the quinquisection of an angle; and that is performed by our general theorem, by taking, to the radius $\sqrt{2}$, the arc whose cosine is $\frac{a}{m^p} = \frac{a}{4} = \frac{5}{4}$, which is the arc of 27° 55′ nearly, the 5th part of which is 5° 35′. Now the log. cosine of that arc, to the radius 1, is 9.9979347; but since our radius is $\sqrt{2}$, to that log. add the log. of $\sqrt{2}$, that is 0.1515150, the sum will be 10.1484497; from which taking away the 10, the remainder 0.1484497 will be the log. of 1.4075 nearly, the number sought. And in like manner the other four roots may be found.

It may be further remarked, that if the equation be of the hyperbolic kind, and n be an odd number, there will be only one possible root; but if n be an even number, there will be only one value of the square xx, the rest being impossible.

If the equation be of the circular kind, all the roots will be possible.

To know how many of the roots will be affirmative, and how many negative, in equations to cosines, observe the following rule: if n be an even number, there will be as many affirmative roots as negative. But if n be an odd number, but such that $\frac{n+1}{2}$ be an even number, the number of affirmative roots will be $\frac{n-1}{2}$, and the number of negative $\frac{n+1}{2}$.

But if $\frac{n+1}{2}$ be an odd number, the number of affirmative roots will be $\frac{n+1}{2}$, and the number of negative $\frac{n-1}{2}$.

END OF THE FORTIETH VOLUME OF THE ORIGINAL.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1737, *pursuant to the Direction of Sir Hans Sloane, P. R. S. By Isaac Rand, F. R. S.* N° 452, p. 1. *Vol. XLI.*

This is the 16th annual presentation of this kind, making the number of 800 different plants.

Of the Measure and Motion of Effluent Water, By James Jurin, M. D. F. R. S. &c. N° 452, p. 5. *From the Latin.*

ESSAY I. *Of Water issuing from a Vessel kept always full, through a round Hole; and of its Resistance arising from a Defect of Lubricity.*—The ancients had no other measure of effluent water, than that uncertain and fallacious one, which, having no regard to the velocity, depended wholly on the perpendicular section of the stream. The first who opened a way to the truth, was Castelli, an Italian, and the friend of Galileo. He, having discovered that the quantity of water flowing through a given section of a stream, is not given, as the ancients thought, but that it is proportional to the celerity with which the water is carried through it; by this noble discovery he laid the foundation of a new and most useful hydraulic science. This discovery therefore engaged the philosophers to study this doctrine so carefully, that after Castelli's time there was hardly any eminent mathematician, who did not endeavour to add something to it, either by experiments, or by reasonings and arguments à priori.

But most of them, notwithstanding their great abilities, had no success in it, because of the exceeding difficulty of the work. For those who studied only the theory, laid down such theorems as were found to be false, when brought to the test by experiments; and those who laboured in making experiments, omitting to observe some minute circumstances, the importance of which they had not yet perceived, differed greatly from one another, and almost all of them erred from the real measure.

Of this there cannot be given a better example, than that simple and easy one, which has generally been a foundation for all the rest, and is what we have now undertaken to treat of diligently, when water issues through a circular hole made in the bottom of a vessel constantly full, with a constant velocity. Poleni alone has given the true measure of the water flowing out, or at least very near the true one; and Sir I. Newton alone has laid the foundation of discovering that measure; though most have rejected it, and some, concealing the author's name, have pretended that it was their own.

We shall therefore make our attempt under the conduct of these two leaders; and in the first place propose, under the name of phænomena, such things as either appear from experiments, or are confirmed by certain reasonings drawn from them; and in the last place, we shall attempt the solution of those phænomena.

Phænomena of Water flowing through a Hole in the Bottom of a Vessel constantly full.—1. The depth of the water, and the time of flowing out being given, the measure of the effluent water is nearly in proportion to the hole.

2. The depth of the water, and also the hole being given, the measure of the effluent water is in proportion to the time.

3. The time of flowing out, and the hole being given, the measure of the effluent water is nearly in a subduplicate proportion to the height of the water.

4. The measure of the effluent water is nearly in a ratio compounded of the proportion of the hole, the proportion of the time, and a subduplicate proportion of the depth of the water.

5. The measure of water flowing out in a given time, is much less than that which is commonly assigned by mathematical theorems. For the velocity of effluent water is commonly supposed to be that which a heavy body would acquire in vacuo by falling from the whole height of the water above the hole; and this being supposed, if we call the area of the hole F, the height of the water above the hole A, the velocity which a heavy body acquires by falling in vacuo from that height V, and the time of falling T, and if the water flows out with this constant velocity V, in the time T; then the length of the column of water, which flows out in that time, will be 2A; and the measure of it will be 2AF. But if we calculate from the most accurate experiments of Poleni, we shall find the quantity of water which flows out in that time, to be no more than about $\frac{571}{1000}$ of this measure 2AF.

Poleni's experiments seem to be preferable to all others, not only because of his extraordinary diligence and accuracy, but on other accounts also. He found, that the quantity of water flowing out of a vessel through a cylindrical tube, far exceeded that which flowed through a circular hole made in a thin plate, the tube and hole being of equal diameter, and the height of the water over both being also equal. And he found it to be so, when the tube was inserted, not only into the bottom, which others had observed before, but also into the side of the vessel.

Now a hole made in the thinnest plate must be considered as a short cylindrical tube. Whence it appears that a greater quantity of water runs through a hole made in a thin plate, than would have run out, if the thickness of the plate had been what is called infinitely small. But as such a plate can neither exist, nor even be conceived by the imagination, it remains that we increase the diameter of the hole, that the thickness of the plate may bear the least proportion possible to that diameter.

This Poleni performed with great judgment, when he made use of a diameter of 26 lines, and not quite a line thick; whereas before him hardly any one made use of a diameter of above 6 or 7 lines, or ever attended to the thickness of the plate or bottom of the vessel, except Sir I. Newton, who mentions his making use of a very thin plate. But Poleni exceeded all others, in considering not only the size of the hole, but of the vessel also, that the water might descend toward the hole with the greatest freedom, and the least impediment; so that there can be no doubt but that the measures taken by him, come much nearer the truth than any other.

6. Since then the measure of the water running out in the abovementioned time T, is $2AF \times \frac{571}{1000}$, the length of the column of water, which runs out in that time, is $2A \times \frac{571}{1000}$. Therefore if each of the particles of water, which are in the hole in the same space of time, passes with equal velocity, it is plain that the common velocity of them all, is that which the space $2A \times \frac{571}{1000}$ would be gone over in the time T, or the velocity $V \times \frac{571}{1000}$. But this is the velocity with which water could spring or jet in vacuo to near $\frac{1}{3}$ of the height of the water above the hole.

7. But when the motion of water is turned upwards, as in fountains, these are seen to rise almost to the whole height of the water in the cistern. Therefore the water, or at least some portion of it, spouts from the hole with almost the whole velocity V, and certainly with a much greater velocity than $V \times \frac{571}{1000}$.

8. Hence it is evident, that the particles of water which are in the hole at the same point of time, do not all burst out with the same velocity, or they

have no common velocity. Though mathematicians have hitherto taken the contrary to be certain.

9. At a small distance from the hole, the diameter of the vein of water is much less than that in the hole. For instance, if the diameter of the hole be 1, the diameter of the vein of water will be $\frac{21}{25}$ or 0.84, according to Sir I. Newton's measure, who first observed this wonderful phenomenon; but according to Poleni's measure $\frac{20}{26}$ or $\frac{20\frac{1}{4}}{26}$; that is, if you take the mean diameter, 0.78 nearly.

We should now proceed to the solution of these phænomena; but before doing this, it wiil be convenient to notice the following particulars.

1. We consider water no otherwise than as a fluid and continuous body, the parts of which yield to the least force, and are thereby moved among themselves.

2. By effluent water, is understood that quantity of it, which actually passes out of the hole; and though it may seem unnecessary, yet it may be proper to mention, that in my dissertation on the motion of running waters, inserted about 24 years ago, in the Philos. Trans. by defluent water I understood that whole quantity of water, which is put in motion within the vessel, and descends towards the hole.

3. We consider the amplitude of the vessel as infinite, or at least so great, that the decrease of the depth of water, during the whole space of time in which the water flows out of the hole, is imperceptible.

4. We consider water as running out with a constant velocity. At the beginning indeed of the motion it runs out, for a very small space of time, with a less velocity than afterwards. But we pass over the very beginning of the motion, and investigate the measure and motion of water, when it has acquired its utmost velocity. Now this must necessarily be constant, as long as the height of the superincumbent water remains the same.

5. We conceive the bottom of the vessel no otherwise than as a mathematical plane, or at least as so thin a plate, that its thickness is little or nothing, with regard to the diameter of the hole.

6. By the measure of effluent water in the following pages, we always understand that quantity of water which flows out of the hole in the same space of time that a heavy body, falling in vacuo, would take in passing through the height of the water above the hole.

7. By the motion of effluent water, we understand the sum of the motions of all the particles of water, which run out of the hole in the abovementioned space of time. But the motion of every particle, is as the factum of the particle itself, and of the velocity with which it bursts out of the hole.

8. That what we shall say hereafter may be the more easily conceived, we

shall first propose the more simple cases, and then proceed to those which are more compound, but nearer to the true state of things. Thus, in the first Problem, that the solution may be the more simple, we suppose the water to run out of the hole into a vacuum, and the particles of water, while they descend towards the hole, to be without any resistance arising from a defect of lubricity.

In the 2d and 3d Problems, the efflux of the water is still supposed to be in vacuo, but we conceive the particles of water, while they descend towards the hole, to meet with some resistance for want of lubricity, but so small, that the decrease of the motion of the water running out of the hole, thus occasioned, is to be accounted as nothing.

In the 4th and 5th we still retain the supposition of the vacuum; but the decrease of the motion of the effluent water, for want of lubricity, is supposed to be sensible.

Lastly, in the 6th and following Problems, we consider the thing as it really is, when it is transacted in the air, so that the particles of water suffer a sensible resistance, not only from each other for want of lubricity, within the vessel, but also after their going out of the vessel, from the attrition of the ambient air.

PROB. 1. *To determine the Motion, Measure, and Velocity of Water running into a Vacuum, through a Hole in the Bottom of a Vessel, where the Particles of Water meet with no Resistance for want of Lubricity.*—So long as the hole is stopped, the stopper sustains the weight of a column of water lying perpendicularly over it. On removing the stopper, the column of water, which lies perpendicularly over it, being no longer sustained, by its pressure causes the water to run out through the hole, and after having brought it to its due velocity, keeps the velocity of the effluent water constant, by its constant pressure.

It must be conceived indeed, that the motion of the water running out of the hole is derived, not only from the weight of the perpendicular column, but partly from the pressure of this column, and partly from the pressure of the surrounding water. But this makes the motion of the effluent water neither greater nor less, than if it arose from the pressure only of the perpendicular column: not less, because the pressure of the perpendicular column, if it is obstructed, will generate a motion proportionable to itself, and it can only be hindered so far as the surrounding fluid urges the effluent water: not greater, because the pressure of the surrounding fluid can add nothing to the motion of the effluent water, unless it takes away as much from the pressure of the perpendicular column.

Therefore the adequate motion of the water, flowing out of the hole, is the pressure or weight of the column of water over the hole. But a given force, howsoever applied, generates a given quantity of motion in a given time, towards those parts whither the force tends. Therefore the weight of the incumbent column, generates a like quantity of motion, in a given time, in the effluent water, as it could generate in the same time in the column itself, falling freely through a vacuum.

Now because, by the hypothesis, the particles of water find no resistance for want of lubricity, and all those particles, which are just going out in the very hole, are urged by an equal pressure of the superincumbent water, it is plain that the velocity of all these is equal.

Let v be that common velocity; a the height, in falling from which in vacuo that velocity would be acquired; A the height of the water above the hole; V the velocity acquired by falling in vacuo from the height A; T the time of falling from the same height; F the area of the hole; and let the water flow out of the hole in the time T. Now, because in the time T, with the velocity V, the space 2A will be run over, the space $\frac{2Av}{V}$ will be run over in the same time with the velocity v. Therefore this will be the length of the column of water, which flows out of the hole in the time T; and the magnitude of this column, or the measure of the water flowing out in the time T, will be $\frac{2AvF}{V}$, and the motion of the same will be $\frac{2AFv^2}{V}$. But the motion which can be generated in the column of water over the hole, in the same time, T, if carried by its own weight through a vacuum, is thus. Its velocity will be V, and as its magnitude is AF, its motion will be AFV. But that motion, from what has been said above, is equal to the motion of the column of water flowing out in the time T, or $AFV = \frac{2AFv^2}{V}$. Hence $v = V\sqrt{\frac{1}{2}}$.

Also, the measure above assigned, of the water running out in the time T, or $\frac{2AFc}{V} = AF\sqrt{2}$. Q. E. I.

Corol. 1.—Since $a : A :: v^2 : V^2$, therefore $A = \frac{Av^2}{V^2} = \frac{1}{2}A$. Therefore the height a, which the effluent water can reach, by turning the motion upwards, is half the height of the water in the vessel above the hole, which is the very height determined by Sir I. Newton, Princip. ed. 3, lib. 2, pr. 36.

Corol. 2.—If we ascribe to the effluent water, that velocity which is acquired by falling from the whole height of the water above the hole, that is, if we suppose $v = V$, then the above determined motion of the water $\frac{2AFv^2}{V}$, is $=$ 2AFV, or double that motion which can be generated by the column over the hole, and therefore not to be generated but by double this column, as in the Princip. ed. 2 and 3, lib. 2, pr. 36.

Scholium.—This measure here determined $AF\sqrt{2}$, or $2AF \times 0.707$, as it falls far short of that which is generally determined by mathematicians, viz. $2AF$, so it far exceeds that measure which is shown by Poleni's experiments, or $2AF \times 0.571$; and no wonder, for what is supposed in this problem, that the particles of water find no resistance in running down, the hypothesis is far from the true state of things.

PROB. II.—*To determine the Motion, Measure, and Velocity of water, running out into a Vacuum, through a Circular Hole in the middle part of the bottom of a cylindrical vessel, where the particles of water find Some Resistance for want of a Lubricity, but so small that the decrease of the motion of the effluent water occasioned, cannot be accounted any thing.*

Let ABCD, fig. 4, pl. 7, be an immense cylindrical vessel; EF a circular hole made in the middle part of the bottom; and, the water being perfectly at rest and unmoved in the vessel, let the stopper be removed from the hole, that a passage may be opened for the water through it.

Then because the water was at rest, and now begins to run out through the hole, and the water placed above follows that which runs out, and the natural motion of the water is not disturbed by pouring any over it, and the hole is in the very middle of the bottom, that portion of water which is in motion, and descends towards the hole, will necessarily assume some regular figure AHEFKB, of which the lower base is the hole itself, and the upper base, the upper surface of the water AB, and all the horizontal sections are circular. This is called a cataract; but we do not yet examine what is the figure of the cataract: it is sufficient for our present design, to observe that it is regular, and that the same quantity of water passes in a given time through each of its horizontal sections.

Now because all that water which tends downwards, is contained in the cataract, it follows that the rest of the water AHEC, BKFD, which is without the cataract, has no motion at all, and is perfectly at rest. Therefore in any horizontal section of the cataract HCK, whose centre is c, the points H, K, shall represent the bounds between the water descending towards the hole, and the surrounding quiescent water.

Also, as the point K is the bound of motion and rest, and the particles of water, while they are in motion, find a resistance for want of lubricity, the particle of water α within the cataract, fig. 5, next to the point K, must be carried downwards only with the least velocity. Otherwise it would necessarily carry with it the next particle a, placed without the cataract, contrary to the hypothesis. But the particle β, which is contiguous within to the particle α, will only descend with the least relative velocity, with regard to the particle α; because otherwise it would carry the particle α away with it, by accelerating it, and this particle α, being now in a quicker motion, would carry away with it the particle a. In like manner the particle γ being placed more within, and contiguous to the particle β, will descend with the least relative velocity with regard to the particle β; and the other particles δ, ϵ, &c. being placed one more within than another, will descend with the least relative velocity, with regard to each of the particles lying next to each of them without. And by this means the absolute velocity of the particles must necessarily increase gradually from the bound towards the centre c, that the velocity of the water may be greatest in the very centre, and least at each bound K and H.

But it is necessary that the resistance which each quicker particle finds from the friction of the adjacent slower one, placed without, should be perpetually equal through the whole section of the cataract. Otherwise that particle which finds the greater resistance, will accelerate the adjacent slower particle, till the resistance is by this means diminished, and becomes equal to that resistance which is found by the other particles. But if the resistance be every where equal through the whole section of the cataract, the relative velocity of the particles will be also equal every where, when one of them necessarily follows another.

Therefore the absolute velocity of every particle, which is the sum of all the relative velocities, from the circumference of the section to that very particle, taken all together, is in the ratio of the distance of the same particle from the circumference of the cataract.

Now let r be the radius of the hole, m to 1 in the proportion of the circumference to the diameter, mr^2 the area of the whole, v the velocity with which the water descends in the centre of the hole, a the height by falling from which in vacuo the velocity v is acquired, A the height of the water above the hole, V the velocity acquired by falling in vacuo from the height A, T the time of falling from the same, z the distance of any particle from the centre of the hole, and let the water run out in the time T.

Now the measure of the water, which goes out of the hole in the time T, will be found after this manner: z will be the radius of any circle within the hole, $2mz$ its circumference, $2mz\dot{z}$ the nascent annulus adjacent to that circumference, and $\frac{r-z}{r} \times v$ the velocity of the water in that annulus.

Since $\mathrm{V} : \frac{r-z}{r}v :: 2\mathrm{A} : 2\mathrm{A}v \times \frac{r-z}{\mathrm{V}r}$ the length of the stream flowing through the nascent annulus in the time T; the measure of that water will be $2mz\dot{z}$

$\times\ 2\text{A}v \times \frac{r-z}{\text{V}r} = 4m\text{A}v \times \frac{rz\dot{z} - z^2\dot{z}}{\text{V}r}$; the fluent of which, viz. $\frac{2m\text{A}v}{3\text{V}r} \times \overline{3rz^2 - 2z^3}$, when $z = r$, gives $\frac{2m\text{A}vr^2}{3\text{V}}$ for the measure of the water passing through all the hole in the time T.

But the motion of the same water will be also found thus. The measure of the water running through the nascent annulus, in the time T, being $\frac{4m\text{A}v}{\text{V}r} \times \overline{rz\dot{z} - z^2\dot{z}}$, and its velocity being $v \times \frac{r-z}{r}$, its motion will be $\frac{4m\text{A}v}{\text{V}r} \times \overline{rz\dot{z} - z^2\dot{z}} \times v \times \frac{r-z}{r} = \frac{4m\text{A}v^2}{\text{V}r^2} \times \overline{r^2z\dot{z} - 2rz^2\dot{z} + z^3\dot{z}}$, the fluent of which, when $z = r$, gives $\frac{m\text{A}v^2r^2}{3\text{V}}$ for the motion of the water, running out in the time T, through all the hole.

But this motion is equal to that which the column over the hole can acquire, in the same time T, by falling by its own weight through a vacuum, that is to the motion AFV, or $\text{AV} \times mr^2$; therefore $\frac{m\text{A}v^2r^2}{3\text{V}} = ma\text{V}r^2$. Hence $v = \text{V}\sqrt{3}$.

Also the abovementioned measure of the water issuing at the hole in the time T, viz. $\frac{2m\text{A}vr^2}{3\text{V}} = \frac{2m\text{A}r^2}{3\text{V}} \times \text{V}\sqrt{3} = \frac{2m\text{A}r^2}{\sqrt{3}}$. Q. E. I.

Corol. 1.—Since $\text{V}^2 : v^2 :: \text{A} : a$, therefore $a = \frac{\text{A}v^2}{\text{V}^2} = \frac{\text{A}}{\text{V}^2} \times 3\text{V}^2 = 3\text{A}$. Therefore the height to which the water can rise, with that velocity with which it runs out in the centre of the hole, is triple the height of the fluid above the hole.

Corol. 2.—The figure of the cataract will be determined in the following manner:

Let HK, fig. 6, be any section of the cataract, having the centre c; and let its radius $c\text{K} = y$, the height of the water above that section, or $c\text{I} = x$, t the time of falling in vacuo from the height x, and as before let $\text{LF} = r$, and $\text{LI} = \text{A}$.

Now the water passes through this section HK in the same quantity as it runs out of the hole EF. But if the vessel be shortened, so that its height be reduced from IL to Ic, and so that the section now becomes the very hole in the bottom of the vessel, the water will pass through this section in a given time, in the very same quantity as it passed through the same before the vessel was shortened.

Now the vessel being shortened, the measure of the water issuing by the hole HK, in the time t, by the preceding solution, is $\frac{2mxy^2}{\sqrt{3}}$, and the measure of the fluid issuing in the time T, is $\frac{2mxy^2}{\sqrt{3}} \times \frac{\text{T}}{t} = \frac{2mxy^2}{\sqrt{3}} \times \frac{\sqrt{\text{A}}}{\sqrt{x}}$: for $\text{T} : t :: \sqrt{\text{A}} : \sqrt{x}$.

But, from what has been said above, the measure of the water issuing by the hole HK, in the given time T, when the vessel is shortened, is equal to the measure of the fluid passing in the same time through the section HK, when the vessel is entire, or equal to the measure of it issuing by the hole EF in the same time. Therefore $\frac{2mxy^2}{\surd 3} \times \frac{\surd A}{\surd x} = \frac{2mAr^2}{\surd 3}$, or $y^2\surd x = r^2\surd A$, or $xy^4 = Ar^4$, which is the same equation of the hyperbolical curve, by the rotation of which he formerly showed that the figure of the cataract was generated.

Schol. 1.—The measure of the water now found, $2mAr^2\surd\frac{1}{3}$, or $2mAr^2 \times 0.57735$, rather exceeds the measure $2mAr^2 \times 0.571$, obtained from Poleni's experiments. But this difference, in some measure, arises from not considering the decrease in the motion of the water from resistance, in this problem.

Schol. 2.—The measure of the effluent water, as determined by this solution, is accurate, if the height of the vessel be considered as infinitely greater than the diameter of the hole. But as this height has a finite ratio to the diameter of the hole, the measure will be something less, so that, when the height is 5 times greater than the diameter, it will differ from the truth only the 32000th part, and when it is double, only about the 5120th part, which differences are smaller than can be discovered by any experiment. And this small difference proceeds from hence, that the abovementioned relative velocity, and therefore the absolute velocity of the particles of water, which have been considered as in a direction perpendicular to the horizon, are really in a direction somewhat oblique, when every particle comes near the axis of the cataract in descending.

But if a true and accurate solution be desired, when the altitude of the water has any ratio whatever to the diameter of the hole, it may be done as follows.

From the property of the cataract curve, in corol. 2 of this problem, viz. $xy^4 = Ar^4$, the subtangent of this curve at the place of the hole will be 4A, and at the place of any section the subtangent will be $4x$, that is, 4 times the height of the water above that section. But such a cataract curve is described not only by the exterior water, which flows beyond the hole, but also by that part of the water which flows through any annulus of the hole, that is, every particle of water describes such a curve.

Now let z be the distance of any particle, in the hole, from its centre, and let this particle descend through the smallest space in a tangent to the cataract curve. Hence its velocity in the direction of the tangent, or the velocity $\frac{r-z}{r}v$, explained in this problem, will be to the velocity in the perpendicular direction, as $\surd\overline{16A^2 + z^2}$ is to 4A; therefore the velocity in the perpendicular direction will be $\frac{r-z}{\surd 16A^2 + z^2} \times \frac{4Av}{r}$

And hence, after the manner of the above solution, the measure of the water passing through the nascent annulus will be $\frac{16m\text{A}^2v}{r\text{v}} \times \frac{rz - z^2}{\sqrt{16\text{A}^2 + z^2}}\dot{z}$. The fluent of which being taken, either by Cotes's forms or by infinite series, when properly corrected, will give the whole quantity run out by the hole in the time T; which in a series is the quantity $\frac{2\text{A}mr^2}{\sqrt{3}} \times 1 - \frac{r^2}{20.4^2\text{A}^2} + \frac{r^4}{56.4^4\text{A}^4} -$ &c. Hence, by supposing A to be infinitely greater than r, or the height than the hole, the measure comes out barely $2\text{A}mr^2\sqrt{\frac{1}{3}}$, the same as was determined before. Hence also,

When $\text{A} = 10r$, the measure is $2\text{A}mr^2\sqrt{\frac{1}{3}} \times (1 - \frac{1}{32000})$ nearly.

And when $\text{A} = 4r$, it is $2\text{A}mr^2\sqrt{\frac{1}{3}} \times (1 - \frac{1}{5120})$ nearly.

So that, instead of the true measure, we may always take $2\text{A}mr^2\sqrt{\frac{1}{3}}$, without any sensible error, even in so small an altitude, and much more in an altitude many times greater, as it usually is in experiments; which makes the computation very easy.

PROB. III.—*Supposing again the same thing as before, and neglecting the acceleration of the water without the hole; required to determine the Diameter of the Vein of water at the small distance without the hole, where the vein is most contracted, and the Velocity of the water in the Vein so contracted.*

In the solution of the former problem it was observed, that the particles of water passing through the hole, do not all issue with the same velocity, but every one with a greater velocity as it is nearer the centre; and that the relative velocity of the inner particles, with respect to the particles that touch each of them on the outside, is constantly equal through all the hole; and this relative velocity proceeds from the resistance given to the particles, by the surrounding water, as they descend towards the hole.

But after the water has passed the hole, and its outer surface is no longer resisted by the surrounding fluid, nor by the ambient air, because moving in a vacuum by the hypothesis, that relative velocity, or inequality of absolute velocity, can no longer obtain. For now the swifter particles must necessarily accelerate the slower contiguous ones, and must also themselves be retarded by the slower, till all the particles have acquired one common velocity, which will happen at a small distance without the hole.

But while all the particles are acquiring this common velocity, the diameter of the vein must necessarily be contracting. This happens in the same manner, as when a rapid river is joined with a slower, as the Rhone with the Saone: in the common channel, the velocity of the water from both rivers is equal, and the water passes through a section of this channel in like quantity as before, through the sections of both rivers; though a section of the Rhone below the junction is much less than the sum of the two sections of both rivers above the same.

Therefore let the radius of the contracted vein of water, where all the particles in its section have acquired an equal velocity, be ϱ, and let that common velocity be called υ: then as $\text{v} : \upsilon :: 2\text{A} : \frac{2\text{A}\upsilon}{\text{v}}$ the length of the vein, and therefore $\frac{2\text{A}\upsilon}{\text{v}} \times m\varrho^2$ is the measure of the water passing through the section in the time T; the motion of which in that time is therefore $\frac{2m\text{A}\varrho^2\upsilon^2}{\text{v}}$.

But the measure of the water passing through the section of the vein, must be equal to that passing through the hole in the same time, that is, $\frac{2\text{A}m\varrho^2\upsilon}{\text{v}} = \frac{2\text{A}mr^2}{\sqrt{3}}$, or $2\varrho^2\upsilon = \frac{2r^2\text{v}}{\sqrt{3}}$.

Also the motion of the water through the hole, as it is not altered by the action of the particles on each other, must be equal to the motion of the water through the section of the vein, that is, $\text{A}\text{v}mr^2 = \frac{2\text{A}m\varrho^2\upsilon^2}{\text{v}}$, or $2\varrho^2\upsilon^2 = r^2\text{v}^2$. Hence, dividing this equation by that immediately above, it gives $\upsilon = \frac{2\varrho^2\upsilon^2}{2\varrho^2\upsilon} = r^2\text{v}^2 \div \frac{2r^2\text{v}}{\sqrt{3}} = \frac{1}{2}\text{v}\sqrt{3}$, and $\upsilon^2 = \frac{3}{4}\text{v}^2$.

Hence $\varrho^2 = \frac{r^2\text{v}^2}{2\upsilon^2} = \frac{r^2\text{v}^2}{2} \times \frac{4}{3\text{v}^2} = \frac{2}{3}r^2$, and $\varrho = r\sqrt{\frac{2}{3}}$. Q. E. I.

Corol.—Since $\upsilon^2 = \frac{3}{4}\text{v}^2$, and the altitudes are in the duplicate ratio of the velocities generated by falling through them, therefore this is the velocity of the water in the contracted vein, by which it can jet upwards in vacuo to $\frac{3}{4}$ of the height of the fluid above the hole.

Scholium.—This extraordinary contraction of the vein of water was first discovered about 30 years before, by Sir I. Newton, when he was considering the motion of effluent water more attentively, on account of some difficulties proposed by Mr. Cotes, who was then taking care of the 2d edition of the Principia; and Poleni afterwards confirmed it by many experiments. From that time this phenomenon has greatly exercised the wits of philosophers, without however detecting the true cause of it.

The radius of the vein, $r\sqrt{\frac{2}{3}}$, or $0.8165r$, determined by this problem, is a little less than $0.84r$, as delivered by Sir Isaac; and a little greater than $0.78r$, according to Poleni's measure, being indeed nearly a mean between them both.

PROB. IV.—*Having given the measure of effluent water, through a circular hole in the bottom of a cylindrical vessel; to determine the motion of the same, and the velocity in the centre of the hole.*

Let the given measure of the water, issuing in the time T, be $2mr^2\text{A}q$; to which the measure assigned by the analysis in prob. 2 will be equal, viz.

$2mr^2\text{A}q = \frac{2mr^2\text{A}v}{3\text{v}}$, or $v = 3\text{v}q$. But the motion of the same water, assigned by the analysis in the same problem, is $\frac{mr^2\text{A}v^2}{3\text{v}}$; and by substituting here, instead of v^2, its value just now found, that motion becomes $3q^2mr^2\text{AV}$. Q. E. I.

Corol.—If from the motion which can be generated in the time T, by the column of water over the hole, viz. $mr^2\text{AV}$, be subtracted the motion of the water running out in the same time, viz. $3q^2mr^2\text{AV}$, there remains $mr^2\text{AV} \times \overline{1 - 3q^2}$ for the motion lost by the resistance in the time T.

PROB. V.—*With the same data and suppositions as before, and neglecting the acceleration of the water without the hole; it is proposed to determine the diameter of the vein of water at a small distance without the hole, where the vein is most contracted, and also the velocity of the water in the vein so contracted.*

By prob. 3, the measure of the water passing through a section of the vein, in the time T, is $\frac{2m\varrho^2\text{A}v}{\text{v}}$; which is also equal to the given measure $2mr^2\text{A}q$; hence $\varrho^2 v = r^2\text{v}q$.

Again, by prob. 3, the motion of the water passing through a section of the vein, in the time T, is $\frac{2m\varrho^2\text{A}v^2}{\text{v}}$, to which is equal the motion determined by the former problem, viz. $3q^2mr^2\text{AV}$; which gives $2\varrho^2 v^2 = 3q^2r^2\text{v}^2$. But

$v = \frac{2\varrho^2 v^2}{2\varrho^2 v} = \frac{3q^2r^2\text{v}^2}{2qr^2\text{v}} = \frac{3}{2}q\text{v}$; and $\varrho^2 = \frac{r^2\text{v}q}{v} = r^2\text{v}q \times \frac{2}{3q\text{v}} = \frac{2}{3}r^2$; therefore $\varrho = r\sqrt{\frac{2}{3}}$. Q. E. I.

Corol. 1.—The same ratio remains between the radius of the hole and that of the contracted vein, whether the motion of the effluent water be any how diminished by resistance, as in this problem, or not diminished as in prob. 3, being both ways the same, $\varrho = r\sqrt{\frac{2}{3}}$.

Corol. 2.—When the motion of the effluent water is diminished by resistance, the velocity is at the same time diminished in the contracted vein. For in prob. 3 it was $v = \frac{1}{2}\text{v}\sqrt{3}$, but now it becomes $v = \frac{3}{2}q\text{v}$; so that v is diminished from 0.866v to 0.856v, taking $q = 0.571$, according to Poleni's experiments.

PROB. VI.—*Supposing the water issuing through a circular hole in the middle of the bottom of a cylindrical vessel, when the particles of water, as they flow downwards within the vessel, suffer so great a resistance from a want of lubricity, that the motion of the fluid is much diminished by it, and also the measure of the effluent water being given; it is proposed to determine the motion of the same, and the velocity with which it passes through the middle of the hole.*

Let the given measure of the water, issuing in the time T, be $2mr^2\text{A}q$, as in prob. 4; and by that prob. we have for the motion of the same $3q^2mr^2\text{AV}$, also the velocity with which it passes through the centre of the hole, or $v = 3q\text{v}$. Q. E. I.

Corol.—When q is given, v is as v, that is, as $\sqrt{\text{A}}$.

Prob. VII.—*Supposing the water issuing into the air, and neglecting the acceleration of the water without the hole, proceeding from gravity; when any two of the following are given, it is proposed to determine the third, viz. the measure of the effluent water, its velocity in the axis of the contracted vein, and the diameter of the same vein.*

When the water issues through the hole into a vacuum, it is shown, in the solution of prob. 3, that the velocity of the particles of water is the same in the whole section of the contracted vein; but now, when the vein passes through the air, the velocity is no longer equal in all parts of the section: for the outer parts of the vein put the surrounding air into motion, and are retarded by it, so that they cannot acquire the same velocity as the rest. But the outer parts, when they are retarded by the air, retard the inner contiguous parts, and these the next; and so by this means every outer particle is carried slower than the contiguous inner one, so that the velocity is greatest in the axis of the vein, and least at the circumference. For which reason it is, Dr. Jurin thinks, that the middle parts of the water in fountains rise much higher in the open air than they would rise in vacuo.

Also, those parts of the air that are contiguous to the vein of water, when they are put into motion by this fluid, they put the adjacent ones into motion, that lie near them on the outside, and these the next outer ones, and these again the next, and so on successively to some distance without the circumference of the vein.

But the velocity of the particles of water must necessarily so decrease, from the axis of the vein to its circumference, that the relative velocity of every particle, wherever placed, must be every where the same, with respect to the particle lying on the outside. For if any particle had a greater relative velocity than the rest, it would find a greater resistance from the attrition of the adjacent outer particles, and thus would be reduced to an equal relative velocity with the rest. In like manner, every particle of the surrounding air, which is put into motion, will have all the same relative velocity with respect to the adjacent particles of the air outwards.

But the relative velocity of the particles of water among themselves, is very different from the relative velocity of the particles of air; as may be conceived in this manner. Any particle of water in the outer part of the vein, is solicited by the next particle inwards, to accelerate its motion; and is also retarded by the next particle of air; and when that outer particle has acquired the due velocity, these two contrary forces must needs be equal, one of which retards the particle, and the other accelerates it. But that cannot be done, unless the product of the relative velocity, and of the density of the accelerating particle of water, be equal to the product of the relative velocity, and of the density of the retarding particle of air. But the density of air is to the density of water as 1 to 900 nearly. Therefore the relative velocity between the outer particle of water and the next of air, is to the relative velocity of the two next particles of water, as 900 to 1 nearly,

Also, that inmost particle of air is solicited by the next contiguous particle of water to accelerate the motion, and retarded by the next particle of air outwards. And as here two contrary forces are equal to each other, the product of the relative velocity and density of the accelerating particle of water, will be equal to the product of the relative velocity and density of the retarding particle of air. Therefore the relative velocity between those two particles of air, will be to the relative velocity between the inmost particle of air and the next of water, as 900 to 1 nearly; and it will be to the relative velocity between the two next particles of water, as 900×900 to 1 nearly. And this great relative velocity will be always the same through the whole thickness of the ring of air, which is drawn into motion by the effluent water.

Now let r, m, v, a, V, A, T denote the same things as in prob. 2. Also let υ be the velocity of the water in the axis of the contracted vein, ρ the radius of the same vein, and R the radius of an imaginary vein, by which the velocity υ, by decreasing gradually, in like manner as it decreases in the true vein, is at length reduced to nothing. Also let the measure of the water passing through the hole in the time T, be $2qmr^2\text{A}$.

Now the measure of the water running in the contracted vein, in the same time, by proceeding as in Prob. 2, will be $\frac{2m\text{A}\upsilon\rho^2}{3\text{RV}} \times \overline{3\text{R} - 2\rho}$.

But these two measures are equal; therefore $3qr^2\,\text{RV} = \upsilon\rho^2\,(3\text{R} - 2\rho)$.

Further, as the measure of the water running through the hole in the time T, is $2qmr^2\text{A}$, the motion of the same, by Prob. 6, is $3q^2mr^2\text{AV}$. And the motion of the water running through the vein in the same time, by proceeding as in Prob. 2, is found $\frac{m\text{A}\upsilon^2}{3\text{VR}^2} \times (6\text{R}^2\rho^2 - 8\text{R}\rho^3 + 3\rho^4)$.

Now these two are equal; and hence $9q^2r^2\text{R}^2\text{V}^2 = \upsilon^2(6\text{R}^2\rho^2 - 8\text{R}\rho^3 + 3\rho^4)$.

Then these two equations being rightly reduced for exterminating R, we come to the following equation, $\rho^4\upsilon^2 + 2q\upsilon\text{V}r^2\rho^2 = 12q^2\text{V}^2r^2\rho - 9q\text{V}^2r^4$. From which may be found any of the three quantities ρ, υ, q; viz.

$$\rho = \frac{r}{\upsilon}\sqrt{q\text{V}} \times \sqrt{(\upsilon + 6q\text{V} - 2\sqrt{(3q\upsilon\text{V} + 9q^2\text{V}^2 - 2\upsilon^2)})},$$

$$\upsilon = \frac{q\text{V}r}{\rho^2} \times (r + 2\sqrt{3\rho^2 - 2r^2}),$$

$$q = \frac{\rho^2\upsilon}{r\text{V} \times (r + 2\sqrt{3\rho^2 - 2r^2})}.$$

Schol. 1.—It was supposed above, that the motion of the water running through the contracted vein, is equal to the motion of that which runs through the hole. But this is not true in mathematical strictness. For the motion of the water running through the hole, is equal both to the motion of the water through the contracted vein, and to the motion of the ring of air surrounding the vein, which air is drawn into motion by the water running through the vein. But the motion of the ring of air is considered as little or nothing, since its thickness is not greater than $\frac{R-\varrho}{900^2}$, and its density not greater than the 900th part of the density of the water. And thus the equations are rendered much simpler than otherwise they would be.

Schol. 2.—By corol. 1, prob. 5, when the water issues into a vacuum, the same ratio continues between the radius of the hole, and the radius of the contracted vein, whether the motion of the effluent water be in any degree diminished by resistance, or not. Hence, as to a physical quantity, it is accounted sufficiently true, that the ratio between those radii be considered as given, even when the water flows through air, however the motion of the effluent water may be diminished by resistance, or at least that the said ratio is varied the least possible. And the same is found to be true by the experiments hitherto made.

Also if the ratio is given between r and ϱ, the ratio between r and R is also given, or the ratio between the radius of the hole, and the imaginary radius, by which the velocity υ, by gradually decreasing, is reduced to nothing. For by eliminating υ from the two equations above, we come to an equation which gives $R = \frac{2}{3}\varrho + \frac{r\varrho}{3\sqrt{3\varrho^2 - 2r^2}}$. Besides, from one of the two equations we obtain $3r^2R : \varrho^2(3R - 2\varrho) :: \upsilon : qV$: and since the former ratio is given, the latter ratio is also given, that is, the quantity $\frac{\upsilon}{qV}$ is given.

Of two remarkable Caverns, the one Icy, and the other emitting noxious Effluvia. By Matthias Belius, F. R. S.* N° 452, p. 41. *Abridged from the Latin.*

The icy cavern opens from the frozen Carpathian mountain, near the village of Szelicze, the mouth, which faces the north, being 18 fathom high, and 9 wide. When the cold is severe in the country, M. B. says, the air within the cavern is warm; but that it freezes within the cavern, when on the outside the sun shines with the greatest heat. When spring begins, and the snow melts, the water trickles down into the inside of the cavern, and is there frozen into transparent ice, by the power of the internal cold, forming large clusters of icicles, as

* Author of a history of Hungary in 4 vols. folio, with plates, entitled Notitia Hungariæ Historico-geographica. Viennæ Austr. 1735—1742.

thick as casks branching out into many surprizing forms.* Thus not only the arches, formed by nature in the solid rock, but also the floor of the cavern are thickly covered with clear ice; which shines all about within the cavern, as if it were incrusted with crystal.

M. B. seems chiefly to ascribe the freezing quality of the cavern to the saline nature of its texture. The nature of the Carpathian mountains, is saline, nitrous, aluminous, and vitriolic; hence he concludes must ensue an almost constant congelation.

As to the cavern at Ribar, a village in the county of Zol, it emits very noxious vapours. It was formerly a rude copious fountain, and the water rising to a good height, overflowed on all sides. The water was petrifying; and generating a tophus, formed it gradually into such a mass, as became a kind of mound about the mouth of the spring, and dammed it up so as to prevent it from overflowing.

But afterwards, when subterraneous waters flowed from the interior of the fountain in the hidden passages, the ground began to give way near the old foundation, and at length formed a new opening; when it began to emit noxious vapours again, destructive to birds and other animals. In this cavern is heard the murmuring noise of running water; so that a river probably flows through the interior passages, and at last loses itself in some kind of swallows.

A very extraordinary Tumour in the Knee of a Person, whose Leg was taken off. By Mr. Jer. Peirce, Surgeon at Bath. N° 452, p. 56.

William Hedges of Stratton in Somersetshire, 25 years of age, of a muscular healthy habit, had never known any kind of disease; but about 8 years before, he first observed a small swelling on his right leg, near the superior epiphysis of the tibia, which he called a splint, about the size of a split horse-bean. He was not conscious of any bruise on the part, and was quite free from pain; yet from its constant increase, which during the first 2

* Dr. Townson, who, in his Travels into Hungary, gives a particular account of this remarkable cavern, states that he found abundance of icicles in it in the month of July, but in a state of thaw. The temperature of the cavern at that time (near Midsummer) was at 0 of Reaumur's thermometer, i. e. at the degree of melting snow. He therefore infers that the masses of ice found in this cavern must be formed in the winter, and consequently that contrary to this author's account and to vulgar report, the temperature of the air in the cavern, regulated to a certain extent by the temperature of the atmosphere without, is lower in winter than in summer. However, as the temperature of the cavern is but slowly affected by the temperature of the external air, Dr. T. admits that, when a very warm spring *suddenly* succeeds to a severe winter, a freezing cold may prevail within the cavern, for some time after it has begun to thaw without, and vice versâ.

years was very slow, but afterwards very fast, he was rendered quite incapable of labour from the time of hay-harvest 1735.

On taking off the limb in May 1737, it weighed, with the leg and foot, 69lb. which was 27 more than the leg some years before taken off at St. Bartholomew's hospital by Mr. Gay, for the like disorder.

On examining this tumour, the adjacent muscles were found destitute of their fibrous and fleshy appearance, probably from the pressure, and great extension, which they had suffered, and the little motion which for some years they had employed on the tarsus and toes; but the fasciæ and common membranes of the muscles, being greatly thickened and callous, adhered to the subjacent tumour; and on removing this callous integument, the tumour appeared covered with great quantities of blood-vessels, much distended, and of a colour more intensely red than natural.

The tumour itself was cartilaginous for the space of half an inch from its external surface; from whence it formed numberless bony substances of various forms, colours, and consistences, which, growing more and more numerous as they lay deeper, at last formed a continual substance completely ossified: in the centre of this bony substance was found about a quart of mucilaginous liquor, no ways fetid, though it was then 10 days from the operation, the colour and consistence of which nearly resembled that of linseed oil; in which were observed many little bony substances loose and floating, similar to many others adhering to the internal surface of the cavity, all which had nearly the appearance of those irregular incrustations, which in hollow rocks are sometimes made by the dropping of petrifying waters. After the operation, every circumstance of the cure proceeded well and the stump healed.

Mr. P. thinks it worthy of remark that the parts above the tumour were very little altered from their natural state. The cartilaginous extremity of the femur was perfectly smooth; nor had the rotula suffered any other injury, except the ossification of the ligament by which it is fixed to the tibia; but the superior extremity of the fibula was wholly lost in the tumour.

An Experiment concerning the Spirit of Coals. By the late Rev. John Clayton, D. D. N° 452, p. 59.

Mention is here made of a ditch, 2 miles from Wigan in Lancashire, the water in which would seemingly burn like brandy, the flame being so fierce, that several strangers boiled eggs over it; the neighbouring people indeed affirmed, that about 30 years before it would have boiled a piece of beef; and that whereas much rain formerly made it burn much fiercer, now after rain it would scarcely burn at all. It was after a long continued season of rain that Dr. C. went to see the place, and make some experiments, when he found that a lighted paper, though it were waved all over the ditch, would not set the water on fire. He then had a dam made in the ditch, and the water thrown out, to try whether the steam which arose from the ditch would then take fire, but he found it would not. He still however pursued his experiment, and caused it to be dug deeper; when at about the depth of half a yard, he found a shelly coal, and the candle being then put down into the hole, the air caught fire, and continued burning.

Dr. C. observed that there had formerly been coal pits in the same close of ground; and having got some coal from one of the nearest pits, he distilled it in a retort in an open fire. At first there came over only phlegm, afterwards a black oil, and then also a spirit arose, which he could noways condense, but it forced the luting, or broke the glasses. Once, when it had forced the lute, coming close to it, to try to repair it, he observed that the spirit which issued out caught fire at the flame of the candle, and continued burning with violence as it issued out in a stream, which he blew out, and lighted again, alternately, for several times. He then tried to save some of this spirit, taking a turbinated receiver, and putting a candle to the pipe of the receiver while the spirit rose, he observed that it caught flame, and continued burning at the end of the pipe, though you could not discern what fed the flame: he then blew it out, and lighted it again several times; after which he fixed a bladder, flatted and void of air, to the pipe of the receiver. The oil and phlegm descended into the receiver, but the spirit, still ascending, blew up the bladder. He then filled a good many bladders with it, and might have filled an inconceivable number more; for the spirit continued to rise for several hours, and filled the bladders almost as fast as a man could have blown them with his mouth; and yet the quantity of coals he distilled was inconsiderable.

He kept this spirit in the bladders a considerable time, and endeavoured several ways to condense it, but in vain. And when he wished to amuse his friends, he would take one of these bladders, and pricking a hole with a pin, and compressing gently the bladder near the flame of a candle till it once took fire, it would then continue flaming till all the spirit was compressed out of the bladder.

But then he found, that this spirit must be kept in good thick bladders, as in those of an ox, or the like; for if he filled calves bladders with it, it would lose its inflammability in 24 hours, though the bladder became not at all relaxed.*

An Experiment concerning the nitrous Particles in the Air; by the same.
N° 442, p. 26.

Dr. C. took a small gally-pot, and ground the top of it very smooth and true,

* This so called spirit of coals, was inflammable air.

and adapted to it a cover of blue slate, which he had likewise ground with much care. Into this gally-pot he put equal quantities of nitre and flowers of sulphur, about 1 dr. of each. He then fixed on the cover, putting it into a new digester, for 3 or 4 seconds. On opening it the next day, he perceived something had transpired between the top of the gally-pot and the cover; the top edges of the gally-pot, where the glazing was ground off, being discoloured, though the nitre and sulphur were very little diminished as to their weight; only they were melted into one lump, which he took out of the gally-pot.

Having set the empty gally-pot on a shelf, on looking at it the next day, he found long hoary hairs, very bright and brittle, all around the ground edges of the pot; which he gathered, and, tasting them, found them to be pure nitre. He then set the pot on the shelf again, and in 3 or 4 days, still finding there were fresh shoots made, as large as at the first, he gathered them a second and third time; so that he supposed the pot would have continued to have shot fresh nitre much longer, if he had not had urgent use for it, to make other experiments in. However, it is to be observed, that he had already gathered more nitre than he put into the pot at first; though he had taken all, or nearly all the nitre that he first put in together with the sulphur, out of the pot in a lump. Hence he infers we may have some conception of the nature of mineral earths, and how they increase, when once impregnated with the seeds of a mineral. This is also a proof of the quantity of nitrous particles with which the air abounds, since the large quantity of nitre which he collected out of the pot, when left empty on the shelf, could be supplied by the air only.

Concerning the Poison of Laurel-Water. By John Rutty, M.D. N° 452, p. 63.

Dr. R. expresses a wish that Dr. Mortimer's experiments with the milk, had more fully determined and ascertained it to be an antidote, than they have yet done. He was informed that some apothecaries in England, being used to sophisticate black-cherry-water with laurel leaves, will not be persuaded, that this is a poison on human bodies, notwithstanding our few instances; but Dr. R. confirms that it really is so by the following case:

At Lisminy in Westmeath, a girl of 18 years old, very well and healthy, took a quantity, less than 2 spoonfuls, of the first runnings of the simple water of laurel-leaves; within half a minute she fell down, was convulsed, foamed at the mouth, and died in a short time, nor was there any swelling on her body.

Essay on the Measure and Motion of Effluent Water. By Dr. James Jurin, F. R. S. N° 453, p. 65. *Translated from the Latin. Part II, being the Continuation from* p. 284 *of this volume.*

Of the Resistance of the Parts of Water among themselves arising from a Want of Lubricity.

We must now consider that resistance of fluids which arises from the motion of its parts among themselves, and is called, by Sir I. Newton, a resistance arising from a want of lubricity. He makes this of two sorts; one arising from the tenacity of the fluid, the other from the mutual attrition or friction of its parts.

The former he thinks is uniform in a given surface, or that it produces an effect proportional to the time; and this opinion is agreeable to experiments. The latter he considers as increasing in proportion to the velocity, or but little less. About this however he determines nothing, for want of experiments.

Hypothesis.—The resistances arising from the want of lubricity in water, Dr. Jurin considers as in a ratio compounded of the three following. 1. Of the ratio of the surface of the parts moved; 2. Of the ratio of the relative velocity with which the parts of water are moved among themselves; 3. Of the subduplicated ratio of the altitude of the fluid. All which are allowed by Sir I. Newton, and most other philosophers.

PROB. 8.—*To explain the Resistance of the Parts of the Cataract which arises from the Want of Lubricity.*

Let r denote the radius of the hole, A the altitude of the cataract, y the radius of any horizontal section, x the altitude of the cataract above that section, z the radius of any circle in that section, and v the velocity of the water in the centre of the hole.

Then $v\sqrt{\frac{x}{A}}$ will be the velocity of the water in the centre of the section having the radius y; and $\frac{y-z}{y}v\sqrt{\frac{x}{A}}$ the velocity in the circumference of the circle of the radius z; also $\frac{v}{y}\dot{z}\sqrt{\frac{x}{A}}$ the relative velocity, and $2mz\dot{x}$ the surface of the nascent cylinder, to the radius z and altitude x: then, by the above 3 positions, the resistance of cylindric surface, is as

$$2mz\dot{x} \times \frac{v}{y}\dot{z}\sqrt{\frac{x}{A}} \times \sqrt{x} = \frac{2mvxz}{y\sqrt{A}}\dot{x}\dot{z}.$$

Now let $\dot{x}$, x, and y be considered as constant quantities, while z flows till it becomes equal to y; then the fluent of the above expression will be $\frac{mvxz^2}{y\sqrt{A}}\dot{x}$, or $\frac{mvxy}{\sqrt{A}}\dot{x}$, by making $z = y$, which will be as the resistance of the nascent cylinder, of the radius y and altitude $\dot{x}$.

But, by the nature of the cataract curve, $xy^4 = Ar^4$, or $y\sqrt[4]{x} = r\sqrt[4]{A}$; hence the resistance of this nascent cylinder will be as $\frac{mvr\sqrt[4]{x^3}}{\sqrt{A}}\dot{x}$; and the resistance of the whole cataract, will be as the fluent of this fluxion, or as $\frac{4}{7}mvr\sqrt[4]{\frac{x^7}{A}}$, or as $\frac{4}{7}mvr\sqrt{A^3}$ by making $x = A$. And since, by prob. 4, it is $v = 3qV$, the resistance in the cataract will be as $\frac{12}{7}mvr\sqrt{A^3}$, or $qVr\sqrt{A^3}$. Q. E. I.

Corol.—Since V is as $\sqrt{A}$, the resistance in the cataract will be as qrA^2.

Schol.—In the above solution, $2mz\dot{x}$ has been used instead of $2mz\sqrt{\dot{x}^2 + \dot{z}^2}$, the true quantity; and if this be used, as also the subtangent and tangent of the curve as in prob. 4; then, by going through the same process as above, the resistance through the whole cataract will be as

$\frac{4}{7}mvr\sqrt{A^3} \times 1 - \frac{7r^2}{3 \cdot 4^3A^2} + \frac{7r^4}{6 \cdot 13 \cdot 4^5A^4} - \frac{7r^6}{23 \cdot 4^9A^6} +$ &c. But if the altitude A be considered as infinite, with respect to the diameter of the hole, all the terms of the series after the first will vanish, and the resistance will be barely as $\frac{4}{7}mvr\sqrt{A^3}$, the same as before determined.

If $A = 10r$, the resistance will be as $\frac{4}{7}mvr\sqrt{A^3} \times (1 - \frac{1}{2743})$ nearly.

If $A = 4r$, the resistance will be as $\frac{4}{7}mvr\sqrt{A^3} \times (1 - \frac{1}{439})$ nearly.

We may therefore use $\frac{4}{7}mvr\sqrt{A^3}$ for the measure of the resistance, without sensible error, even where the altitude of the water does not exceed 2 diameters of the hole, and much more in a far greater height.

PROB. 9.—*Having Given the Measure of the Water issuing through a Given Circular Hole, in the Middle of the Bottom of a Circular Vessel, of a Given Depth; to determine the Measure of the Water issuing from another Vessel of Any Given Depth, through Any Given Circular Hole.*

Let r denote the radius of the given hole, A the altitude or depth, $2mqr^2A$ the given measure of the water issuing in that time in which a body would fall in vacuo through the altitude A.

Then, by prob. 4, $3q^2mr^2AV$ will be the motion of the water issuing in the same time; and, by the cor. to prob. 4, the motion lost in the same time by the resistance will be $mr^2AV(1-3q^2)$. Hence therefore an equal force of resistance can generate this motion in the same time.

But the motions generated in the same time are proportional to the generating forces. Therefore the motion mr^2AV, which the weight of the column of water mr^2A can generate in this time by prob. 1, without any resistance, is to the motion $mr^2AV(1 - 3q^2)$, which the resistance can generate in the same time, as the weight of $mr^2\text{A}$, is to that resistance. Hence this resistance is $= mr^2\text{A}(1 - 3q^2)$.

In like manner, by putting s and E for the radius of the hole, and the altitude of another vessel, and $2pms^2\text{E}$ for the water issuing in the same time, in which a body falls in vacuo through the height E, the resistance in this new vessel will be $= ms^2\text{E}(1 - 3p^2)$.

But, by the cor. to prob. 8, these two resistances are to each other, as $qr\text{A}^2$ to $ps\text{E}^2$. Making this proportion then gives this equation, $pr\text{E}(1 - 3q^2) = qs\text{A}(1 - 3p^2)$; hence $p = \sqrt{\frac{1}{3} + (\frac{1 - 3q^2}{6qs\text{A}}r\text{E})^2} - \frac{1 - 3q^2}{6qs\text{A}}$, or $p = \sqrt{\frac{1}{3} + (\frac{1 - 3q^2}{6q}n)^2} - \frac{1 - 3q^2}{6q}n$ by putting $r\text{E} = ns\text{A}$. And hence $2pms^2\text{E}$ is the measure of the water issuing from the second vessel, in the time that a body falls freely through the altitude E. Q.E.I.

Corol. 1. If the diameters of the holes be in the ratio of the altitudes of the water, the ratio of the measures will be the same, as if the water issued without any resistance. For, if $r : s :: \text{A} : \text{E}$, or $r\text{E} = s\text{A}$, and $n = 1$, then is $p = q$; and hence $2qmr^2\text{A} : 2pms^2\text{E} :: 2mr^2\text{A} : ms^2\text{E}$, which is the ratio of the measures when void of all resistance.

Corol. 2. If E be considered as nothing in respect of the altitude A, then will n be as nothing also, and hence $p = \sqrt{\frac{1}{3}}$. Therefore the smaller the altitude E is taken, the nearer p approaches to $\sqrt{\frac{1}{3}}$.

Corol. 3. If s be infinitely great in respect of r, then is $p = \sqrt{\frac{1}{3}}$. Therefore the greater s is taken, the nearer p approaches to $\sqrt{\frac{1}{3}}$.

PROB. 10.—*The Water flowing into the Air; to determine the Ratio between the Diameter of the Hole and of the Contracted Vein.*

This ratio cannot be determined without experiments. By prob. 7, $\rho^2 = \frac{qvr^2}{v^2} \times (v + 6qv - 2\sqrt{3qvv + 9q^2v^2 - 2v^2})$; hence ρ is determined, when q and v are known.

But no experiments are known, by which q and v may be measured. Poleni's experiments show the measure of the effluent water, whence q is known: but they do not show the greatest distance to which the water is carried, when issuing horizontally from the hole; nor the distance to which the middle part of the vein reaches, that issues with the velocity v. And Mariotte's experiments measure the greatest perpendicular height, to which water jets, when its motion is turned upwards, whence v^2 is known; but they do not show the measure of the effluent water. Therefore, for want of proper experiments, the ratio can only be determined approximately, as follows.

By schol. 2, prob. 7, it was made probable, that the ratio is constant between these two radii, or at least that it is very little varied. From Mariotte's experiments it appears, that the difference between the altitude to which the water jets, and the altitude of the vessel, has nearly the duplicate ratio of the altitude of the vessel. Therefore let a be the height to which the water, in the axis of the vein, with the velocity v, can jet; then, by Mariotte's experiments, $A - a$ is as A^2, and $\frac{A^2}{A - a}$ is a given quantity.

But in one experiment, which Mariotte esteems a fundamental one, A was $= 60$ Paris inches, and he found $a = 59$ inches, the diameter of the hole being half an inch. So that in this case $\frac{A^2}{A - a} = 3600$; and since this is a given quantity, it will be always $3600a = 3600A - A^2$, or $a = \frac{3600A - A^2}{3600} = A - \frac{A^2}{3600}$.

Therefore, if $A = 1$ inch, or double the diameter of the hole, it will be $a = 1 - \frac{1}{3600}$. But $v^2 : V^2 :: a : A : 1 - \frac{1}{3600} : 1$. Therefore when the altitude of the vessel is double the diameter of the hole, there may be taken $v^2 = V^2$, or $v = V$.

Further, by cor. 4, prob. 9, as E decreases, p verges to $\sqrt{\frac{1}{3}}$. Therefore when the altitude of the vessel is very small, as about 2 diameters of the hole, then we may take p or $q = \sqrt{\frac{1}{3}}$.

But, by prob. 7, $\rho^2 = \frac{qVr^2}{v^2} \times (v + 6qV - 2\sqrt{3qvV + 9q^2V^2 - 2v^2})$, and here instead of v and q substituting their values just found, or V and $\sqrt{\frac{1}{3}}$, there results $\rho^2 = r^2\sqrt{\frac{1}{3}} \times (1 + 2\sqrt{3} - 2\sqrt{1 + \sqrt{3}})$, or $\rho^2 = r^2 \times (2 + \sqrt{\frac{1}{3}} - 2\sqrt{\frac{1}{3} + \sqrt{\frac{1}{3}}})$ $= r^2 \times 0.6687553907$; and hence $\rho = r \times 0.81777466$.

Here then is the value of ρ, when the altitude of the water is double the diameter of the hole: and since by schol. 2, prob. 7, ρ obtains a constant ratio to the radius of the hole, it will have the same value in any altitude of the water. Q. E. I.

Corol. 1. By prob. 7, $R = \frac{4}{3}\rho + \frac{r\rho}{3\sqrt{3\rho^2 - 2r^2}}$, hence by the value of ρ just found, there arises $R = r \times 3.98877150$, being the value of R when the altitude of the water is double the diameter of the hole: and since, by schol. 2 of the same problem, the ratio between r and R is constant, therefore R will have this same value whatever the altitude of the water be.

Corol. 2. Because v is nearly $=$ V, and q nearly $= \sqrt{\frac{1}{3}}$ when the altitude of the water is double the diameter of the hole; therefore, at this altitude of the water, $\frac{v}{qV} = \sqrt{3}$ very nearly. And since, by schol. 2, prob. 7, the ratio between v and qV is constant, therefore $\frac{v}{qV}$ will be $= \sqrt{3}$, whatever be the altitude of the water.

PROB. 11. *The Water issuing from a Vessel always full, through a Given Hole, into the Air; and having Given any one of the three following quantities,* *viz, the Measure of the Effluent Water, the Velocity in the Axis of the Contracted Vein, or the Altitude to which the Middle Part of the Vein can jet upwards; required to determine the rest.*

Let A be the height of the vessel, r the radius of the hole, $2qmr^2\text{A}$ the measure of the effluent water, v the velocity in the axis of the contracted vein, a the height to which the water can jet; and first let $2qmr^2\text{A}$ be given, whence q is given.

Now, by cor. 2 prob. 10, $\frac{v}{q\text{V}} = \sqrt{3}$; hence $v = q\text{V}\sqrt{3}$, and $v^2 = 3q^2\text{V}^2$

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

Secondly, if v be given;

then $q = \frac{v}{\text{V}}\sqrt{\frac{1}{3}}$, and $2qmr^2\text{A} = \frac{2mr^2\text{A}v}{\text{V}}\sqrt{\frac{1}{3}}$, also $a = \frac{v^2\text{A}}{\text{V}^2}$.

Lastly, if a be given; since $a = 3q^2\text{A}$,

hence $q^2 = \frac{a}{3\text{A}}$, and $q = \sqrt{\frac{a}{3\text{A}}}$. Also $v^2 = \frac{a\text{V}^2}{\text{A}}$, and $v = \text{V}\sqrt{\frac{a}{\text{A}}}$. Q. E. I.

PROB. 12. *Having given the Height to which Water jets through the Air, from a Vessel of a Given Height, through a Given Circular Hole; to determine the Height to which it will jet from any other Vessel, of any Given Height, and through any Given Circular Hole.*

Let the letters r, s, A, E, q, p denote the same things as in prob. 9; and let a and e be the heights to which water can jet, when spouting from vessels of the respective altitudes A and E.

Then, by prob. 11, $a = 3q^2\text{A}$, $e = 3p^2\text{E}$; hence

$3q^2 = \frac{a}{\text{A}}$, and $1 - 3q^2 = \frac{\text{A} - a}{\text{A}}$, and $q = \sqrt{\frac{a}{3\text{A}}}$, and $p = \sqrt{\frac{e}{3\text{E}}}$, and $p^2 = \frac{e}{3\text{E}}$.

And since, by prob. 9, $p = \sqrt{\frac{1}{3} + (\frac{1-3q^2}{6qs\text{A}}r\text{E})^2} - \frac{1-3q^2}{6qs\text{A}}r\text{E}$, or by putting $r\text{E} = ns\text{A}$, $p = \sqrt{\frac{1}{3} + (\frac{1-3q^2}{6q}n)^2} - \frac{1-3q^2}{6q}n$; hence by substituting $\frac{\text{A}-a}{\text{A}}$ for $1 - 3q^2$, and $\sqrt{\frac{a}{3\text{A}}}$ for q, and for $\text{A} - a$ writing α, it is $p = \frac{\sqrt{4\text{A}a + n^2\alpha^2} - n\alpha}{2\sqrt{3\text{A}a}}$, and $p^2 = \frac{2\text{A}a + n^2\alpha^2 - n\alpha\sqrt{4\text{A}a + n^2\alpha^2}}{6\text{A}a}$. But $p^2 = \frac{e}{3\text{E}}$; hence $e = \frac{2\text{A}a + n^2\alpha^2 - n\alpha\sqrt{4\text{A}a + n^2\alpha^2}}{2\text{A}a}\text{E}$. Hence, writing ε for $\text{E} - e$, it is $\varepsilon = \frac{n\text{E}\alpha}{2\text{A}a} \times (\sqrt{4\text{A}a + n^2\alpha^2} - n\alpha)$. Now, ε or $\text{E} - e$ bring given, e is also given, or the height to which the water can spout from the new vessel. Q. E. I.

Corol. 1. If the holes in both vessels be equal, or $s = r$; then $\text{E} = n\text{A}$, or $n = \frac{\text{E}}{\text{A}}$, hence $\varepsilon = \frac{n^2\alpha}{2a} \times (\sqrt{4\text{A}a + n^2\alpha^2} - n\alpha)$.

Corol. 2. If the altitudes of the vessels be equal, or $E = A$; then $r = ns$, or $n = \frac{r}{s}$, hence $\varepsilon = \frac{n\alpha}{2a} \times (\sqrt{4Aa + n^2\alpha^2} - n\alpha)$.

Corol. 3. If the diameters of the holes be in same ratio as the altutdes; then the water will spout to heights proportional to those of the vessels. For if $r : s :: A : E$, or $rE = sA$, and $n = 1$, then $\varepsilon = \frac{E\alpha}{A}$, or $\varepsilon : \alpha :: E : A$, or $E - e : A - a :: E : A$, or $e : a :: E : A$.

Corol. 4. Since $2p\sqrt{3Aa} = \sqrt{4Aa + n^2\alpha^2} - n\alpha$, it will be $\varepsilon = \frac{nE\alpha}{2Aa} \times 2p\sqrt{3Aa} = \frac{pnE\alpha\sqrt{3}}{\sqrt{Aa}}$; hence, for $\sqrt{a}$ substituting its value above mentioned $q\sqrt{3A}$, and properly reducing, $\varepsilon = \frac{pnE\alpha}{qA}$, or $\varepsilon = \frac{prE^2\alpha}{qsA^2}$.

Corol. 5. Hence, by making $p = q$, then $\varepsilon = \frac{rE^2\alpha}{sA^2}$, or $\varepsilon : \alpha :: rE^2 : sA^2$. That is, the defects of the jets, are as the diameters of the holes inversely, and as the squares of the heights of the vessels directly.

Corol. 6. When $s = r$, then $\varepsilon = \frac{E^2\alpha}{A^2}$ nearly in the duplicate ratio of the heights of the vessel; which agrees with Mariotte's rule.

Corol. 7. When $E = A$, then $\varepsilon = \frac{r\alpha}{s}$ nearly; that is, the defects of the jets are nearly as the diameters of the holes reciprocally.

First General Scholium.—In examining the truth of this theory by experiment, it will be proper, 1. To use a very large vessel, at least in the upper part, that during the time of making the experiment, the height of the water may not be sensibly changed. But if the vessel be not so large, but that during the efflux, the height of the water considerably decreases, then a mean between the greatest and least height is to be taken for the constant height: which is better than disturbing the natural motion of the fluid, by pouring fresh water into it.

2. Let the vessel be of such a depth, that when the water spouts from a hole in the side, the velocity of the fluid through the centre of the hole, may be safely taken for the velocity through all the hole, when there is no resistance.

3. Let the plate, in which the hole is made, be so thin, or at least have so thin an edge at the circumference of the hole, that the thickness there may be accounted as nothing with respect to the diameter of the hole; observing to shave away the thickness of the plate on the outer side, leaving the inner side plain next the water.

These things being prepared, the following experiments may be made, by which, as so many criteria, the certainty of the above doctrine may be judged of.

Exper. 1. When the water issues through a hole in the side of the vessel, measure carefully the diameter of the contracted vein, observing whether it remains always the same, however the altitude of the water may vary.

Exper. 2. Observe whether this diameter has always the same ratio to the diameter of the hole, in using different sizes of apertures.

Exper. 3. Observe the quantity of water issuing in a given time, through the same hole, either in the bottom or the side of the vessel, with different altitudes of the water.

Exper. 4. Observe the same, with holes of various sizes, but the same altitudes of the water.

Exper. 5. Observe how much water issues in a given time, in two different cases, in each of which there is the same ratio of the diameter of the hole to the height of the water. For if the quantities be found in a ratio compounded of the duplicate ratio of the diameters, and the simple ratio of the altitudes, as in cor. 3, prob. 9, it will be a great confirmation of our theory.

Exper. 6. In the same two cases, observe to what altitudes the water will jet upwards, through a large tube fitted to the side of the vessel, and perforated in its upper part. For if these altitudes be found proportional to those of the water in the vessel, as in cor. 3, prob. 12, it will be another sure confirmation of this theory.

Exper. 7. Observe also the height of the jet, with the same hole, but various heights of the water.

Exper. 8. Observe also the same, by varying the hole, but with the same height of the vessel.

Second General Scholium.—Till those experiments be carefully tried, we must avail ourselves of the experiments hitherto made. These are of three kinds: for they measure either, 1. The diameter of the contracted vein; or, 2. The measure of the effluent water; or, 3. The height to which the water jets. As to the

1st. The radius of the contracted vein, as measured by Sir I. Newton, is $0.84r$, when the diameter $2r$ is $\frac{5}{8}$ of an inch; and by Poleni, it is $0.78r$ nearly, when the diameter of the hole is $2\frac{1}{6}$ Paris inches. But by the calculation in this theory it is $0.818r$ nearly, for any diameter of the hole; being nearly a medium between those two.

2. As to the 2d, none of the quantities, measured by any one, are of any use, except those by Poleni, when the water issues through the above size of hole made in a thin plate, which he informs us is much less than when it passes through a pipe of the same diameter. And the medium among 10 such measured quantities, is $2mr^3\text{A} \times 0.571$, when the height of the vessel is 33 Paris inches

But the measure taken to this altitude, by our calculation, from Mariotte's fundamental experiments, is $2mr^2\text{A} \times 0.5768$, exceeding Poleni's measure about the 98th part only.

3. As Poleni rendered all the experiments of his predecessors useless, concerning the measure of the effluent water, because they took no account of the thickness of the plate through which the water issued; it might be suspected that the like fault enters into the experiments concerning the height of the jet of water. But Poleni has removed this doubt, by another excellent observation; viz. that water issuing through short tubes, or through holes in a thin plate, spout nearly to the same horizontal distance, or issue with nearly the same velocity.

Therefore, to try the certainty of our theory, let us make use of Mariotte's experiments concerning the altitudes of jets; assuming some one of them, as a foundation, for trying the heights in the rest of the experiments, by our 12th problem. Taking therefore that experiment, in which the diameter of the hole was 6 lines, and the depth of water in the vessel 34 feet $11\frac{1}{2}$ lines, or $419\frac{1}{2}$ inches, in which case Mariotte found the jet spouted to the height of 31 feet 8 or 9 inches, or $380\frac{1}{2}$ inches. Here then, $\text{A} = 419\frac{1}{2}$ inches, $a = 380\frac{1}{2}$, and $\alpha = 39$. But, in another experiment, where E, or the depth of water in the vessel, was 26 feet 1 inch, the water rose, through the same hole, to the height of 24 feet $2\frac{1}{2}$ inches; and by our cor. 1, prob. 12, the height of the jet, or the value of e, comes out 24 feet 3 inches, which is nearly the same.

Other cases are exhibited in the following tables, where it appears how nearly the calculated heights of the jets, computed by our theory, agree with those observed in Mariotte's experiments, when made with holes of various sizes, and with vessels of different heights.

I. With the Hole of 6 lines Diameter.

Alt. of the Vessel.		Height of the Jet, By Exper.		By Calcul.	
Ft.	Inc.	Ft.	Inc.	Ft.	Inc.
34	$11\frac{1}{2}$ ...	31	$8\frac{1}{2}$ ...	31	$8\frac{1}{2}$
26	1 ...	24	$2\frac{1}{2}$ ...	24	3
24	5 ...	22	10 ...	22	10
12	4 ...	12	0 ...	11	11
5	6 ...	5	$4\frac{3}{4}$ ...	5	5
5	0 ...	4	11 ...	4	$11\frac{1}{6}$
35	5 ...	32	0 ...	32	1

II. The Hole of 4 lines Diameter.

Alt. of the Vessel.		Height of the Jet, By Exper.		By Calcul.	
Ft.	Inc.	Ft.	Inc.	Ft.	Inch.
32	$11\frac{1}{2}$ ...	30	0 ...	30	0
24	5 ...	21	$8\frac{1}{2}$ ...	21	11
5	6 ...	5	$4\frac{7}{10}$ ...	5	$4\frac{4}{10}$

III. The Hole of 3 lines Diameter.

34	$11\frac{1}{2}$ ...	28	0 ...	28	0
26	1 ...	22	0 ...	22	1
24	5 ...	21	2 ...	20	11
5	6 ...	5	$4\frac{7}{10}$ ...	5	$3\frac{7}{10}$

Hence it appears, that the calculations from the above theory agrees so well with Mariotte's experiments, of the height to which the jets rise; as also with Poleni's measure of the effluent water; and with the diameter of the contracted vein, measured by Newton and Poleni, that it can hardly be doubted that the above theory is either true, or at least very near the truth.

A Collection of the Observations of the Eclipse of the Sun, August 4, 1738, *which were sent to the Royal Society.* N° 453, p. 91.

By Mr. George Graham and Mr. Short, FF.R.S. at Mr. Graham's house in Fleet-street, London, by a Refracting Telescope of 12 *feet focus, armed with a Micrometer, and by a Reflecting Telescope of* 9 *inches focal length.* p. 91.

Beginning of the eclipse at 9ʰ 59ᵐ 20ˢ A.M.
End at.................. 11 59 36
Duration 3 0 16
Quantity of obscuration by the micrometer 2 dig. 28. min.

2. *At Upsal, by M. Celsius, F.R.S. with a 7-foot tube, and a Graham's Micrometer,* p. 92.

$12^h\ 18^m\ 52^s$ .... True time, Beginning of the eclipse.
12 42 22 .... The end.
0 23 30 .... The duration.

3. *At Wittemberg, by J. F. Weidler, F.R.S. p.* 92.

The beginning could not be seen for clouds.

At $11^h\ 30^m$ increasing, 1 digit. eclipsed.
12 19 2 dig. 30' eclipsed.

4. *At Bononia, by S. Manfredi, F.R.S. &c.* p. 94.

At $23^h\ 0^m\ 10^s$ eclipsed, 1 digit.
0 3 0 the middle; 4½ digits.
1 18 1 the end of the eclipse.

Some Electrical Experiments, chiefly respecting the Repulsive Force of Electrical Bodies.—By Granvile Wheler, Esq. F.R.S. N° 453, p. 98.

The following experiments were made in the autumn of the year 1732, and repeated to Mr. Grey the following summer.

PROP. I. *Bodies made Electrical, by communicating with an Electrical Body excited by Friction, are in a state of Repulsion with regard to such excited Bodies.*

Exper. 1.—Mr. W. hung a fine white thread by a loop, to a horizontal blue silk line, about 4 feet long, tied at each end, and at about a foot distance from it, placed a glass tube $2\frac{1}{2}$ feet long nearly, and one inch and quarter diameter, fixed in the centre of a circular piece of wood supported on three brass screws, so that the tube and pendulous thread were parallel to each other. The tube being rubbed, the thread was attracted and repelled 7 or 8 times; in very good weather it moved to and from the tube 12 times, at above one foot distance. He then tied a piece of new smooth packthread to the top of the tube, and to the loop of the thread hanging down as before, and again excited the tube: the thread, without coming once towards the tube, went into and continued in a state of repulsion; but if he only touched the communicating packthread with a finger, the white thread immediately hastened to the tube: and on hanging another long piece of packthread, which reached the ground, to the communicating packthread, and again rubbing the tube, the pendulous white thread was so far from going into a state of repulsion, that it became attracted to the tube, and continued so, without showing the least tendency to a state of repulsion, as long as the virtue of the tube lasted.

Exper. 2. He tied a piece of small cane, about 16 inches long, and one fourth of an inch diameter at one end, and a little more at the other, at right angles to the top of the tube, fixed in the same pedestal as before, and making unequal arms with it; and at the end of the larger arm, a piece of stick transversely, about 6 inches long, so as it might slide backwards and forwards to and from the tube. This moveable short stick at one end supported a very fine white thread, at the other a very fine blue silk, by which means now a silk and a thread both at the same time hanging parallel to the tube. The thread, after the tube was rubbed, first was attracted, but then immediately repelled, and continued a considerable time in a state of repulsion; but on tying to the end of the shorter arm of cane, a piece of long packthread, which reached down on the table, and rubbing the tube again, the thread continued in a state of attraction, without being once repelled during the whole virtue of the tube, as in the preceding experiment. Yet the silk, whether the long packthread was added or not to the shorter arm of the cane, continued constantly attracted towards the tube; but on putting a short silk only 6 inches long, in the same circumstances, it would, after some time rubbing the tube, turn into a state of repulsion, the upper part first bending from the tube, and the lower part towards it, the upper bending still increasing till the whole was repelled; and it was remarkable, that the upper part or bending, on the approach of the finger, or any body not impregnated with electrical effluvia, flying towards it, and the under part or bending, rather seeming to fly from it, till the whole was saturated, and in a state of repulsion with regard to the tube, and then any part of it would come to the finger, or any other body, not made electrical. It is proper to add here one more difference remarkable between the thread and the silk. The thread in a state of repulsion touched with the finger, would immediately fly towards the tube; but the silk in the same state, after touching several times, still continued in a state of repulsion, and would not be attracted till squeezed from top to bottom between the finger and thumb, once, and sometimes two or three times. And further, the thread would immediately turn again into a state of repulsion, whereas the silk, after the violence committed by the thumb and finger, being attracted to the tube, would not without a good deal of rubbing the tube, be repelled again.

Corol. 1. From the different state of the pendulous silk and threads at the same time, under the same circumstances, the former being attracted while the latter is repelled, it follows, that a mere vibration of the parts of the tube is not sufficient to account for the electrical phænomena; which appears further from the two contrary states continuing some time, and from the same piece of silk being at once part in a state of repulsion, part in a state of attraction.

Corol. 2. That some bodies immediately receive and immediately part with the electric effluvia; but that others are some time before they receive it, or receive enough of it; and when they have received enough of it, part with it more unwillingly.

Corol. 3. That any light body, as a feather, after touching, or nearly approaching the tube, must fly from it: on contact or a near approach, it saturates itself with the electric effluvia, and by this means becomes itself electrical; and consequently from the foregoing experiments, is in a state of repulsion with regard to the tube. As soon as it touches any other body, it loses its acquired electricity, and therefore may be attracted as at first.

Prop. II. *Two or more Bodies made electrical, by communicating with an Electrical Body excited by Friction, are in a state of Repulsion with regard to one another; or Bodies made electrical by Communication, repel one another.*

Exper 1. Mr. W. suspended two pieces of white thread, each about one foot long, by loops, on a horizontal blue silk line, 4 feet long, about half an inch asunder from each other; and on holding the excited tube over them at a little distance, the two threads immediately receded from each other considerably at the bottom. He then removed one of the threads, and held the tube over the other, in the same manner as before. The single thread was not observed to move to either side; consequently the moving of the threads sideways was occasioned neither by the attraction of the cross line, nor that of the tube, nor by the frame of wood, to which the cross line was tied at each end, but only by their action on each other.

He then added a third string, at the same distance from the second, that the second was from the first, and on holding the excited tube over the middle one, at the same distance from the cross silk as before, when the strings continued in the same plane, the middle one stood still, and the string on each side of it receded considerably at the bottom part, which in this case must necessarily happen, on a supposition that they repel each other equally; for the two contrary forces of the outer threads destroy each other, and consequently the middle one must remain quiet; but there was nothing to hinder the middle one from repelling the two outer on each hand sideways. When, as it often happened, the three pendulous threads did not remain in the same plane, they then all receded from one another equally, and formed nearly a triangular prism; the three threads being the three edges, or rather a triangular pyramid with the top cut off.

On suspending 4 threads at the same distance as before from one another; if they continued in the same plane, they all parted, but the two outermost more from their neighbours, than the two in the middle from each other. If they moved out of the plane they were first in, they formed two prisms, each extreme with the two in the middle forming one, or rather a parallelopepid, less at top than at bottom.

When 5 strings were suspended, either the middlemost continued stationary when the plane was not altered, or if it was, they formed 3 prisms.

Exper. 2. Mr. W. afterwards placed two cross blue silks, of the same length as before: about half an inch asunder from one another horizontally, and tied at each end; and on each of these, at different times, hung 2, 3, 4, and 5 threads, at the same distances as before, when every thing succeeded, as it ought to have done, on a supposition of their mutually repelling one another.

Exper. 3. To each of the ends of two threads, suspended as at first, a feather being tied, the two feathers manifestly receded from each other; and when 3 threads had each a feather at their extremities, the middlemost became stationary, and the two outer went off on each hand.

Exper. 4. Mr. W. suspended afterwards 2, 3, 4, and 5 blue silk strings by loops, on one cross blue silk, and found the several experiments succeed in the same manner as in threads; except that they remained a longer time before they appeared in a state of repulsion, receded from one another more slowly, and continued much longer in the repulsive state, after the tube was removed.

Exper 5. This done, he made several experiments, by mixing silks of different colours, and silks and threads of different colours, and suspended them by turns on silks of different colours; whence arose several different phænomena. On suspending two black silks at the before-mentioned distances from each other, on a scarlet cross silk, they not only opened and receded from each other at the bottom considerably, but when the tube was held under, ran or jumped away from each other, to the very ends of the cross red silk that supported them, taking 2, 3, or more jumps from each other. The same was observed of two white silks suspended on red silk, but they did not move away so briskly as the black.

Exper. 6. Mr. W. tried whether threads hanging parallel as above, from a cross blue silk line, and joined with one or more transverse threads, so that the perpendicular threads remained nearly parallel, would mutually repel when the tube was held over them; and they seemed to repel each other full as strongly as before. When they were joined by only one cross thread towards the top, the lower parts separated considerably; when joined by two cross threads, one towards the top, and one towards the bottom, they separated both in the middle parts between the two cross threads, and at their lower ends under the second or lowest cross thread. When several were tied together at the top and bottom, and about a foot long, not by transverse threads, but in a knot at each end, they all bellied out from one another, describing a figure generated by an ellipsis, revolved about its greater axis; approaching nearer to a sphere, the stronger the repulsive force was. And though it was only a necessary consequence, he could not without some pleasure observe the knot at the bottom, as the strings swelled out, sensibly rising up. He could scarcely forbear imagining the bundle of silks, a bundle of muscular fibres.

Exper. 7. He suspended two brass, and afterwards two iron wires on a cross blue silk, in the same manner as the threads and silks before mentioned, and found the experiments succeed as in threads of the same number, except that they did not recede so far from one another, which must necessarily follow from their greater weight.

N. B. These experiments were made sometimes with the tube held over, sometimes held under the cross line; but they generally succeeded best when the tube was held under the extremities of the pendulous wires, which in this case separated much further, and kept their repulsive force much longer.

Exper. 7. Mr. W. hung up two fragments of barometer tubes, each about a foot long, by blue silk lines going through them, so that they hung parallel, horizontal, at equal heights, and about one quarter of an inch asunder; on holding the excited tube above and under them, they manifestly receded from each other.

He suspended the same fragments of tubes by blue silk lines of equal length, from a cross blue silk in a perpendicular position, each having a little red sealing wax at the upper end, to hinder the strings from slipping off. The excited tube being brought near them, they receded manifestly, especially at the lower ends; the distance from one another, when at rest, being about a quarter of an inch.

Corol. 1. From the repulsive state of the pendulous threads, tied transversely with two or more threads, and bending out from each other, where at liberty, it follows that all the threads of a table-cloth, or other large piece of linen, when made electrical, have a tendency to fly from each other: and consequently, were the repulsive force strong enough, the whole would be dissolved, or torn in pieces. A short thread of black silk, by repeated applications of the tube, has separated into its smallest fibres. Whence is suggested more plainly, than from any other known experiment, a reason for the dissolution of bodies in their respective menstruums, viz. that the particles of the solvend having imbibed the particles of the menstruum, so as to be saturated with them, the saturated particles become repulsive of each other, separate, and the mass flies to pieces.

And hence perhaps arises a reason, why particles of bodies specifically heavier than the menstruums in which they are dissolved, are, after the dissolution and dispersion, suspended all over the menstruum, viz. that they repel each other. Attraction is insufficient; for parts attracted equally in all directions, are, in effect, not attracted at all; and the imperfection of the fluid will not do; for if this occasioned the suspension, striking or shaking the vessel would make them subside.

Corol. 2. Hence we plainly see how heat may divide the particles of water with greater or less force, in proportion to the degree of saturation, and throw them into the air; where they may continue to ascend, if at the same time they are divided, they are expanded into little shells or bubbles, of a diameter large enough to be specifically lighter than the lower air, as Dr. Halley has sagaciously conjectured. Or if the upper parts of the air, as being less saturated than the lower parts, may be able to draw them upwards, till the excess of weight, which is constantly increasing, is equal to the excess of attraction.

PROP. III. *Bodies, made Electrical by rubbing, do themselves repel one another, or the electrical excited Bodies themselves repel one another.*

Exper. 1. The two fragments of tubes before mentioned, prop. 2, exper. 8, being suspended horizontally, and in a position parallel to each other, Mr. W. held in one hand, and with the other rubbed some time; then gently letting them go so as to be at rest, they receded from each other towards that end which had not been taken hold of.

Also, he suspended a single little tube, about a foot long, by a long blue silk line, perpendicularly, and on a table placed the great tube fixed in a stand as before, and excited each alternately, 2 or 3 times; then gently moved the tube with the stand it was fixed in, near the suspended little one: the little tube manifestly receded so much, that a cross blue silk line, stretched horizontally at about an inch distance on the opposite side, would sometimes, on the first approach of the great tube, be touched by it.

Exper. 2. Three scarlet silks, each pendulous by loops from a cross silk line, and close together, being rubbed downwards two or three times, between the finger and thumb, showed a considerable repulsive force with regard to each other, forming themselves immediately into a triangular pyramid, and continuing in this state of separation some time; and, which shows their attraction at the same time, with regard to other bodies not excited, coming to them when brought near them.

He observed the same repulsive force in 3 yellow and 3 green silks, under the same circumstances, and excited in the same manner, but not in so great a degree as in scarlet. In blue the repulsive force was scarcely discernible after several times rubbing.

Scholium. Dr. Hales, in the 12th article of his 13th experiment, in the 2d volume of his Statical Essays, observes, "That if a piece of one of the bronchiæ or gills of the muscle shell-fish, be cut off, and put into a small concave glass, with three or four drops of its liquor, and be then placed under a double microscope, the blood may be seen greatly agitated in the fine vessels; and at the cut edge of the piece of gill, may with great pleasure be seen many blood-globules, repelled from the cut orifices of the blood vessels, and attracted by other adjoining vessels: also other globules rolling round their centre, and repelling each other; whence (as he says) it is plain, that bodies, by brisk rubbing and twirling about, may acquire, in a watry fluid, both attractive and repulsive virtue or electricity."

From our last experiments, we are led to think, that the globules of the blood, if by friction they acquire an electrical attractive virtue, must of necessity repel one another; and that electricity is not so properly called an attractive and repulsive virtue, as a virtue attractive of those bodies that are not attractive themselves, and repulsive of those that are; and that this repulsive force of the electrical blood-globules, excited by friction, as they flow in their channels (and particularly in the small ones, and perhaps more so in those of the lungs, where the refrigerating power of the air may assist, as Dr. Hales has observed); this repulsive force of the blood-globules, may be the great cause that hinders the blood from coagulating as it circulates; may be the great cause of the constant perspiration in a healthy state, and of the increase of it, cæteris paribus, in proportion as the velocity and friction of the blood increases.

If these things are so, the necessity of exercise appears more plainly than ever, in order to keep the body in a healthy state, as we may observe here the very steps that nature makes use of to free herself from her suppressions.

An Account of some of the Electrical Experiments made by Granvile Wheler, Esq. at the Royal Society's house, on May 11, 1737. *By C. Mortimer, M.D. R.S. Secr.* N° 453, p. 112.

Exper. 1.—A large octavo book was placed horizontally on silk lines, and the upper surface strewed with several pieces of leaf brass, all or the greater part of which flew upwards, from one another, and off the book, on holding an excited tube at a little distance underneath the book.

Exper. 2.—Two lines were extended horizontally the whole length of the library, being between 30 and 40 feet, distant from one another about 2 feet at one end, and meeting together in a knot at their other ends, the whole lines being packthread, except 5 feet of silk line tied at each of the separated extremities, as well as at the knot where the other ends united, in order to stop the current of the effluvia. On the united extremities was placed horizontally a piece of card about 2 inches square, on which were strewed pieces of leaf brass. The excited tube being held at a little distance under the separated extremities of the packthread, the leaf brass on the card at the other end flew upwards, and off the card.

Exper. 3.—Five glass receivers, placed one within another, on an electrical cement of bees-wax and Venice turpentine, were all exhausted. In the innermost a fine white thread, about 5 inches long, was suspended from the crown of it, by means of a little cement made of bees-wax and oil. On moving the excited tube up and down near the side of, and horizontally to and from the outer receiver, the suspended thread manifestly made many vibrations corresponding to the motions of the tube.

Exper. 4.—An electrical circular cake of bees-wax and rosin, 10 inches in diameter, was placed horizontally on a tall glass receiver, near 3 feet high, such as is made use of for dropping the feather and guinea. This cake being the preceding evening about 8 o'clock, warmed with a hot iron held over it, and then struck perpendicularly all over its surface with the hands in parallel directions, and so left covered with a thin pasteboard, was about 12 o'clock next day at noon gently uncovered, and an ivory ball, about one inch and half diameter, placed in the centre, a fine white thread about ten inches long, with a small piece of cork, the size of a pin's head, at the end of it, being held between the finger and thumb, was gently let down on the vertex of the ball; it first flew off at some distance, and then made several pretty regular revolutions from west to east about it, in the form of a circle.

Exper. 5.—The ball was removed, and the cake again warmed and excited as before; after which the ball was replaced at a little distance from the centre, nearer to Mr. Wheler; the consequence of which was, that the pendulous little body moved with a direct motion as before, but in an orbit that resembled an ellipse, having the ball in one of its foci.

Exper. 6.—Two bullets fixed on little stands of cork, about one quarter of an inch high, were placed on the cake, each about an inch distant from its centre, and in a line with the centre and Mr. Wheler; the pendulous body described an orbit resembling an ellipse, having the two bullets for its foci, and the motion was direct from west to east.

Exper. 7.—Instead of the cork, another pendulous body of a cylindrical form was made use of, tied to a fine white thread, about 20 inches long; the cylinder consisted of two circular bases of paper, half an inch diameter, but all cut away except a ring and a small bar across the middle, through which basis 6 equal fine threads passed at equal distances from one another, knotted at the lower base separately, and joined together in one knot at about half an inch distance from the upper base, from which knot proceeded the long thread. This body moved from west to east about the central ball, and at the same time discovered a motion about its own axis in the same direction; but after 2 or 3 turns generally stopped, and turned the contrary way, which seemed to arise from the untwisting of the thread.

Exper. 8.—A thread about a foot long, was suspended from a horizontal line of packthread, and parallel to it an excited tube placed erect in a stand, the thread approached the tube, and continued in a state of attraction. A thread of the same length, suspended from a silk line, vibrated backward and forward 2 or 3 times, being first attracted, and then repelled, and continuing some time repelled; but on joining the top of the tube, by a packthread going round it, to the loop of the thread, the thread continued constantly in a state of repulsion, showing no tendency to attraction.

Exper. 9.—Two black silks, about the same length with the thread in the preceding experiment, were suspended by loops from a horizontal red silk line, at the distance of about half an inch from each other; on holding the excited tube under them, the silks swelled out from one another, and then jumped away on each hand to the distance of 2 feet.

Exper. 10.—A circular board of nearly the same diameter with the electric cake, was suspended horizontally by 6 silk lines, tied to one silk line which was brought over a pulley at the top of a frame of wood, so as to be moved up and down. From the board hung 6 fine white threads, about 18 inches long, fixed by a little cement at equal distances from each other. The board being let down till the ends of the threads were about an inch distant from the electric cake, which was directly under, and had the ivory ball on its centre; the threads all approached towards the centre of the cake, both when the ball was in the centre, and when taken away, keeping an equal distance from the centre, and from one another, as long as a packthread joined the circle of board and the frame to keep it steady; and on removing the ball out of the centre, towards the circumference, the figure lengthened, the threads next the ball advancing nearer the circumference; when the ball was placed at about an inch distance from the circumference, the thread that was before nearest the circumference, whipped between the ball and the centre, so as to be almost in the same plane with its two neighbouring threads, the figure formed by the extremities resembling an ellipse with one end cut off. But when, instead of the packthread that joined the board to the frame, a blue silk line was tied in the same manner in all respects, the threads, instead of coming towards the centre, all flew away at a great distance from the cake, and from one another.

It ought to be observed in the experiments of the circular motion of the pendulous body, that Mr. Wheler's hand seemed as steady as possible, except in the first experiment, when a little trembling appeared; Mr. George Graham taking a very good method to observe it, by keeping his eye fixed on a point at a considerable distance, in the same line with the end of Mr. Wheler's finger and his own eye.

Yet when Mr. Wheler had finished the experiments to the satisfaction of all present, Mr. Hawskbee, Mr. George Graham, and Dr. Mortimer, held the thread with the pendulous body over the cake with the ball on its centre, after the cake had been excited by Mr. Wheler; but they had no regular revolutions at all, though several very manifest motions were made with the hand, to try if a projectile motion might by that means be given to the pendulous body. Mr. Wheler had tried the same thing with his servant; from whence it is reasonable to conclude, that it is necessary, that the same person who excited the cake, should likewise hold the thread; as if there were some analogy between the effluvia excited by the clapping of the hand on the cake, and the effluvia which may be communicated along from the hand which holds the thread to the piece of cork at the end of it. And this seems to be the reason of what the late Mr. Grey said, viz. That there was something in the human hand essential to the experiment, which he had not yet found in any other supporter of the thread.

Some Remarks on the Electrical Circular Experiment of the late Stephen Gray, F. R. S. By Granvile Wheler, Esq.* N° 453, p. 118.

Some uncommon circumstances led Mr. W. to make Mr. Gray's circular experiment in the following manner. While he excited a cake of rosin and bees-wax, 10 inches diameter, by clapping with his hand, he let the ivory ball remain in a basin of water; then shaking off the drops, placed it in the centre, and with his right hand held a fine thread, about 8 or 9 inches long, having one end rolled up into a little ball, and the other, for about an inch, reduced to its greatest fineness, to only one fibre, himself and hand being supported on the back of a chair. The success was, he had a great many revolutions, to the number of 50, from west to east; but at first not so regular as towards the last, at first describing only about 1-third part of the circumference at a time, and after standing still a little, describing another third part. He might probably have had a great many more revolutions, but being tired, he was forced to rest himself, which he did for 10 minutes, then took up the thread again. The thread stood repelled at some distance, without making any revolutions, and at last only made half a one the contrary way to what it did before; but on wetting it, by drawing it 2 or 3 times over the surface of the water, it made again 20 more revolutions from west to east, only at a smaller distance from the ball, for the water must make it heavier, but full as regular as before, and rather quicker. The virtue of the cake must now have lasted about 3 quarters of an hour. After resting about 6 minutes, he tried again with the string fresh wetted, the ball and cake continuing as before; and had, to his great surprise, 100 revolutions in the space of about 12 minutes, the

* See Philos. Trans. N° 441, 444, of the Revolutions of pendulous bodies by electricity.—Orig.

revolutions being still quicker, and more regular, and nearer the ball; and at the 6th revolution of this last hundred, the thread was attracted to the surface of the ball, and, being wet, did not disengage itself, till pulled away; yet after this, it described the remaining 94 revolutions of the hundred, and seemed inclined to describe a great many more, but that he was forced to rest his arm again, which he did for about 8 minutes, then tried again, the thread being fresh wetted, and had 70 revolutions at nearly the same distance from the ball in less than 9 minutes, all very regular, and without any attraction of the thread to the ball. He rested again 16 minutes, wet the thread again, and held it as usual; it was repelled at about $\frac{1}{4}$ inch distance from the ball, but seemed to have no tendency to a circular motion; yet after continuing stationary about a minute, he perceived a motion about its axis, about which it took several turns; but still had little or no progressive motion, till about a minute longer, when it began to move forward, and continued doing so from west to east, for about 33 revolutions, very regular, but slower than in the last two cases, the string having been held about 10 minutes, and the revolutions performed in about 7 or 8 of them. In each of these last 3 times, it was rather longer before the progressive motion began than usual; and in all the trials of this experiment, he frequently perceived a motion about the axis, which was generally from west to east, though now and then the contrary way. The virtue of the cake must now have lasted near 2 hours; about 3 quarters of an hour after, he tried again, and had 60 revolutions from west to east, in about 10 minutes, the distance from the ball being still less than before, hardly $\frac{1}{4}$ of an inch, scarcely any revolution about the axis appeared, and at the beginning the thread was twice attracted to the ball. About an hour and a half after, the virtue of the ball was not quite gone, the wet thread being repelled, and making 3 or 4 revolutions from west to east, as well as moving a little about its axis the same way. But as it was reasonable to suppose the ball itself in the centre of the cake was now dry, with a feather dipped in water he wetted its surface; yet found no increase of virtue, rather a diminution of it, the pendulous body seeming scarcely at all repelled; but it is to be observed, that the ball, as it was wetting, twice tumbled over, and rolled on the surface of the cake; by which means the virtue of the cake might be much diminished.

It is not improper too to take notice here, that during the revolutions of the wet string, he frequently observed a kind of oscillatory motion, as if there was an alternate intention and remission of the repulsive force. As also that he often took notice of little plucks, and convulsive motions, in the pendulous body, and sometimes thought he has felt something like it in his arm that held it, though at no other time has he ever been sensible of any such thing.

He several times after repeated this experiment with the thread and ball both wet, and found it succeed much better than when they were both dry; and once he had 220 revolutions before he rested his arm. He tried too with the ball dry, and the string only made wet; but the virtue did not continue so long, as when both were wet.

He now flattered himself with hopes of success, if the thread was suspended from an undoubted fixed point, which therefore he proceeded again to try with the greatest care and caution, but in vain; the revolutions were uncertain.

This difference naturally led him to reflect on the cause of it. The tremor of the hand would not account for it; for this being both ways backward as well as forward, must as often hinder as promote a continual motion one way: and though in two opposite parts of a circle, the motion is really in contrary directions, and therefore the contrary impulses of a tremor may promote a revolution applied at opposite places of the orbit; yet as these tremors are irregular, and succeed much quicker than the revolutions are performed, they seem insufficient to account for the motions of the pendulous body, performed with any degree of regularity.

A stream of air in the room might impel along the tangent the pendulous body, kept at a distance from the ball by its repulsive force; and then gravity, taking place, might with the first motion compound a curve: but still the resistance of the air would soon destroy the original impulse, could a few revolutions be performed; and besides, one revolution could not be performed, because the same stream of air that began the motion, must be contrary to it in its return.

A finger held on the right hand near the pendulous body, when suspended from a fixed point, will make it revolve from west to east; but then it must be applied and removed alternately: the repulsive force therefore which the arm may acquire, by being held in the sphere of the effluvia, is insufficient; for, as it is in one place, it must impel only one way, and constantly the same way; and therefore, like a stream of air in the room, though it might create the beginning, it must hinder the completion of a revolution.

Sometimes he doubted, whether the pulse of the arm might not be assisting in giving a projectile motion. When one leg is laid over the knee of the other, a motion and heaving of the leg that lies over, answering to every stroke of the pulse, is very apparent at a distance: the arm therefore in some positions, in which its great artery meets with a proportionable pressure or resistance, may have a constant motion, though less perceivable.

What seemed the most probable solution, was this: when the arm is extended, the posture being uneasy, there must be a re-action of the muscles, or a continual pulling of the arm towards the body. When therefore the right arm is made use of, the pulling will be from right to left; and consequently the motion produced in the body held by it in the same direction, or from west to east. When the left arm is made use of, the re-action of the muscles will be from left to right, and therefore the motion of the pendulous body from east to west. And, agreeably to this, he has observed, when he used his left hand, all other circumstances continuing the same, the motion of the pendulous body was from left to right, or from east to west, contrary to what was observed when held by the right hand. Yet still neither of these solutions would account for the variety of odd particulars he has met with under various circumstances.

He proceeded therefore to try with rests for his arm of different heights, having an arm of wood, about 2 feet long, fixed to a rest for his telescopes, which could be raised to any height wanted; and then the experiment succeeds only well, when the rest was lower than the electric area, and the arm was supported on its elbow, which was the posture constantly made use of, when rested on a chair, the chair being lower than the electric area, that it might less affect the effluvia, as was then thought.

He began now to think, whether it was not possible, that an inclination to a motion one way in the person that holds the body, might not have such an influence on the arm, and consequently the string and pendulous body, as to determine them the same way by some pressure or bias put upon it, though no motion sensible, even to himself, was produced in the hand. If so, he might, by a contrary inclination, produce a motion the contrary way. Having therefore a fine day, and the circular cake being well excited, he tried if he could not produce a regular motion from east to west, about the ball in the centre, having his hand supported, as usual, on the back of a chair. He found he could produce a very regular one from east to west for many revolutions, and change from one motion to another, without being sensible he moved his hand at all.

He then wet the ball and string, as in the experiment beforementioned, and found he could tire himself with a motion either from east to west, or from west to east, as he pleased, without giving any motion, that he could perceive, to his hand or fingers. Hence many odd experiments that please, may, when repeated, succeed.

Since therefore the motion of the pendulous body, from a point undoubtedly fixed, is irregular, as he found by many different experiments, repeated with the greatest care and caution; and since he was convinced from these last mentioned trials, the motion from west to east, and from east to west, must generally have been determined by himself: he is inclined to think, that a desire of producing a motion from west to east, was the secret cause that determined the pendulous body to that direction, by some impression from Mr. Gray's hand, as well as his own, though he was persuaded at the same time, he was not sensible of giving any motion to his hand himself: and he the rather thinks this was the case, from the instance Mr. Gray gives, by way of explanation, of a man resting his elbows on his knees, this implying that he rested his arm on his elbow, as Mr. W. did himself.

But though upon the whole it does at last appear, that this motion from west to east in a pendulous body, applied to another in the centre of an electric area, is to be ascribed to the hand that holds it, and not solely to the nature of the electric effluvia, or the figure of the central body; yet still, perhaps, it may not be improper for astronomers to consider, whether or no a medium with this property, that all bodies immersed in it, are repulsive of one another, ought not to be joined with gravity to explain the heavenly phænomena; especially since the phænomena of fire, and our electric effluvia, have a great affinity to each other; and since many of the heavenly phænomena are to be accounted for, on this supposition, with great simplicity; and some of them, that have not yet perhaps been fully accounted for, seem necessarily to follow.

Of the Influence which two Pendulum Clocks were observed to have on each other. By Mr. John Ellicott, F.R.S. N° 453, p. 126.

The two clocks, on which the following observations were made, being designed for regulators, particular care was taken to have every part made with all possible exactness: the two pendulums were hung in a manner different from what is usual; and so disposed, that the wheels might act on them with more advantage. Upon trial they were found not only to move with greater freedom than common, but a heavier pendulum was kept in motion by a smaller weight. They were in every respect made as near alike as possible. The ball of each of the pendulums weighed above 23lb.; and required to be moved about 1° 5′ from the perpendicular, before the teeth of the swing wheel would scape free of the pallets; that is, before the clocks would be set a going. The weight to each was 3lb. which would cause either of the pendulums in their vibrations to describe an arch of 3°. The two clocks were in cases, which shut very close, and placed sideways to each other, so near that when the pendulums were at rest, they were little more than about two feet asunder.

The odd phænomena observed in them were these: in less than two hours after they were set a going, one of them, called N° 1, was found to stop; and when set a going again, as it was several times, it would never continue going two hours together. As it had always kept going with great freedom, before the other clock, N° 2, was placed near it, this led Mr. E. to conceive its stopping must be owing to some influence the motion one of the pendulums had upon the other; and on watching them more narrowly, the motion of N° 2, was found to increase as N° 1 diminished; and at the time that N° 1 stopped, N° 2 described an arch of 5°, that is nearly 2 degrees more than it would have done, if the other had not been near it, and more than it moved in a short time after the other pendulum came to be at rest: this made Mr. E. imagine that they had a mutual influence on each other.

On this he stopped the pendulum of N° 2, leaving it quite at rest, and set N° 1 a going, the pendulum describing as large an arch as the case would permit, viz. about 5°. In about 20 minutes after, he went to observe whether there was any motion communicated to the pendulum N° 2, when, to his surprise, he found the clock going, and the pendulum to describe an arch of 3°, whereas at the same time N° 1 did not move 4°. In about half an hour after, N° 1 stopped, and the motion of N° 2 was increased to very near 5°. He then stopped N° 2 a second time, and set N° 1 a going, as before; and standing to observe them, he presently found the pendulum of N° 2 begin to move, and the motion to increase gradually, till in $17^m\ 40^s$ it described an arch of 2° 10′, at which time the wheel discharging itself of the pallets, the clock went.

The arches of the vibrations continued to increase, till, as in the former experiment, the pendulum moved 5°; the motion of the pendulum N° 1 gradually decreasing all the while, as the other increased; and in three quarters of an hour after, it stopped.

He then left the pendulum of N° 1 at rest, and set N° 2 a going, making it describe an arch of 5°; it continued to vibrate less and less, till it described but about 3°; in which arch it continued to move all the time he observed it, which was several hours. The pendulum of N° 1 seemed but little affected by the motion of N° 2.

Mr. E. tried these experiments several times over, without finding any remarkable difference. The freer the room was from any motion, as people's walking about in it, &c. he found the experiments to succeed the better; and once he found N° 2 set a going in $16^m\ 20^s$, and N° 1 at that time stopped in $36^m\ 40^s$.

Further Observations and Experiments concerning the two Clocks abovementioned. By the Same. N° 453, p. 128.

The seemingly different effects, which the two clocks had on each other, Mr. Ellicott accounts for as follows.

The manner in which the motion is communicated to the pendulum at rest, he conceives to be thus: as the pendulums are very heavy, when either of them is set a going, it occasions by its vibrations a very small motion, not only in the case the clock is fixed in, but, in a greater or less degree, in every thing it touches; and this motion is communicated to the other clock, by means of the rail, against which both the cases bear. The motion thus communicated, which is too small to be discovered but by means of some such-like experiments as these, may be judged by many, insufficient to make so heavy a pendulum describe an arch of 2°, or large enough to set the work a going; and indeed it would be so, but for the very great freedom with which the pendulum is made to move, arising from the manner in which it is hung. This appears from the very small weight required to keep it going, which, when the clock was first put together, was little more than 1 lb. And if the weight was taken off, and the pendulum made to swing 2°, it would make 1200 vibrations before it decreased half a degree, so that it would not lose the 3000th part of an inch in each vibration. Indeed if the weight was hung on, the friction would be increased, and the pendulum would not move quite so freely; but even in that case it was found to lose but little more than the 2000th part of an inch, or about 3 seconds of a degree, in one vibration; and therefore if the motion communicated to it from the other, will make it describe an arch exceeding 3″, the vibrations must continually increase till the work is set a going. And that the motion is communicated in the manner above supposed, is confirmed by the following experiments:

A prop was set against the back of the case of N° 2, to prevent its bearing against the rail; and N° 1 was set a going; then observing them for several hours, Mr. E. could not perceive the least motion communicated to N° 2. He then set both the clocks a going, and they continued going several days; but he could not find they had any influence on each other. Instead of the prop against the back of the case, he put wedges under the bottoms of both the cases, to prevent their bearing against the rail; and stuck a piece of wood between them, just tight enough to support its own weight. Then setting N° 1 a going, the influence was so much increased, that N° 2 was set a going in less than 6 minutes, and N° 1 stopped in about 6 minutes after. In order to try what difference would arise, if the clocks were fixed on a more solid floor, he placed them, exactly in the same manner as in the last experiment, on the stone pavement under the piazzas of the Royal Exchange, and stuck the piece of wood between them, as before; and setting N° 1 a going, the only difference was, that it was 15 minutes before N° 2 was set a going, and N° 1 continued going near half an hour before it stopped.

From these experiments Mr. E. thinks it plainly appears, that the pendulum which is put in motion, as it moves towards either side of the case, makes the pressure on the feet of the case to be unequal, and, by its weight, occasions a small bearing or motion in the case on that side towards which the pendulum is moving; and which, by the interposition of any solid body, will be communicated to the other clock, whose pendulum was left at rest. The only objection to this, he conceives, is the different effects which the two pendulums seemed to have on each other. But this he hopes to explain to satisfaction.

For, notwithstanding these different effects, he soon found, by several experiments, that the two clocks mutually affected each other, and in the same manner, though not with equal force; and that the varieties observed in their actions on each other, arose from the unequal lengths of their pendulums only.

For, on moving one of the clocks to another part of the room, and setting them both a going, he found that N° 2 gained of N° 1, about $1^m\ 36^s$ in 24 hours. Then fixing both against the rail, as at first, he set them a going, and made the pendulums to vibrate about 4°; but he soon observed that of N° 1 to increase, and that of N° 2 to decrease; and in a short time it did not describe an arch large enough to keep the wheels in motion. In a little time after it began to increase again, and in a few minutes it described an arch of 2°, and the clock went. Its vibrations continued to increase for a considerable time, but it never vibrated 4°, as when first set a going. While the vibrations of N° 2 increased, those of N° 1 decreased, till the clock stopped, and the pendulum did not describe an arch of more than 1° 30′. It then began to increase again, and N° 2 decreased, and stopped a second time, but was set a going again, as before. After this N° 1 stopped a second time, and the vibrations continued to decrease till the pendulum was almost at rest. It afterwards increased a small matter, but not sufficiently to set the work a going. But N° 2 continued going, its pendulum describing an arch of about 3°.

Finding them to act thus mutually and alternately on each other, Mr. E. set them both a going a second time, and made the pendulums describe as large arches as the cases would permit. During this experiment, as in the former, he sometimes found the one, and at other times the contrary pendulum to make the largest vibrations. But as they had so large a quantity of motion given them at first, neither of them lost so much during the period it was acted on by the other, as to have its work stopped, but both continued going for several days, without varying one second from each other; though when at a distance, as was before observed, they varied $1^m\ 36^s$ in 24 hours. While they continued thus going together, he compared them with a third clock, and found that N° 1 went $1^m\ 17^s$ faster, and N° 2 went 19^s slower, than they did when placed at a distance, so as to have no influence on each other.

On altering the lengths of the pendulums, the period in which their motions increased and decreased, by their mutual action on each other, was changed; and would be prolonged as the pendulums came nearer to an equality, which, from the nature of the action, it was reasonable to expect it would. This discovers the reason why the pendulum of N° 2, when left at rest, would be set a going by the motion of N° 1; whereas if N° 1 was left at rest, it would not be set a going again by the motion of N° 2.

For he found, by several experiments, that the same pendulum, when kept in motion by a weight, would go faster, than when it only moved by its own gravity. On this principle, which may easily be accounted for, it follows, that during the time in which the shortest pendulum, N° 2, was only acted on by N° 1, it would move slower, and the times of its vibrations approach nearer to an equality with those of N° 1, than after it came to be kept in motion by the weight; and by this means the time which N° 1 would continue to act on it, would be prolonged, and be more than was required to make the pendulum describe an arch sufficient to set the work a going. But, on the contrary, while the pendulum of N° 1, which was the longest, was only acted on by N° 2, as it would move slower, the difference of the times of the vibrations would be increased; and consequently the time which N° 2 would continue to act on it, would for this cause be shortened, so that before the pendulum of N° 1 would describe an arch sufficient to set the work a going, the period of its being acted on would be ended, and it would begin to act on N° 2, at which time its vibrations would immediately decrease, and continue to do so till it came to be almost at rest. And thus it would continue, sometimes to move more, and at other times less, but never sufficiently to set the clock a going.

A Wound in the Cornea of the Eye successfully cured, By Mr. Thomas Baker, Surgeon to St. Thomas's Hospital. N° 453, p. 135.

A young woman, about 15 years of age, on the 6th day of Nov. 1733, received a wound just in the pupil of her right eye, by the point of a common fork. An inflammation followed, with great pain. The whole eye appeared dark and turbid; and the humours seemed confused, and blended together. Mr. B. opened a vein in the arm, and drew away 10 oz. of blood: he then washed the eye with a collyrium of trochisci albi rhasis, and common water, made blood-warm; and dressed it with a cataplasm of white bread and milk, with a little saffron in it. The next day there appeared on the wounded part of the cornea, a large thick slough: he dressed it in the same manner; and so continued till the 18th day of the same month, when the slough cast off. He purged her during this time with decoct. sennæ ℥ij, mann. solut. ℥ss, aq. pæon. comp. ʒij; m. f. potio, at the distance of about 3 days, just as he found her strength would permit. The inflammation and pain abated daily. During the whole time, the eye was quite blind, till the slough cast off, when she complained she saw double. In a very little time her sight returned, but not so perfect as before; her eye having somewhat of a cloud before it.

He made her 6 visits at the distance of 2 or 3 days, after the 18th: when he left her, she saw perfectly well, that cloud which she before complained of, being removed; her eye appeared fair and clear, and equally strong and useful to her as the other. A little speck, which was the cicatrix of the wound, remaining on the cornea, he made her a fontanel in the arm, and ordered her to keep it open, and not to touch the speck on her eye. More than 2 years afterwards, the speck had gradually decreased, and was so small, that it was scarcely visible; and her sight was as perfect and strong as before the accident.

An Account of a monstrous Boy. By Andrew Cantwell, M. D. dated Montpelier, Dec. 27, 1731, N. S. N° 453, p. 137.

There was at the above date at Montpelier, a boy 13 years of age, born at Cremona, who bore the lower parts of another boy, which seemed to issue from his epigastric region, between the cartilago ensiformis and the navel. The forepart of the one faced that of the other. The head and trunk seemed buried in the boy's abdomen, down to the hips, where the connection was plainly to be seen. This portion of the prominent body had a well-formed anus and penis. The scrotum had a fine down on it, but was void of testicles, and seemed to be filled with the intestines. Nothing passed through these 2 outlets. Dr. C. could perfectly well distinguish the 2 ossa iliûm in their natural state, but could not feel the os sacrum. The articulation of the femur was somewhat discernible on each side: and Dr. C. perceived the pulsation of the anterior crural arteries. The boy felt very sensibly when these additional feet, legs, or buttocks, were pinched, or over-much pressed. He had lately had the small-pox, and these had suffered by it equally with him. At his navel Dr. C. found a considerable rupture, which was covered by this portion of a body. This rupture would grow monstrously large in wet weather, and would diminish again in dry. It had a circular hole in it, which ran through the peritonæum. The boy was of a thin habit of body, but otherwise enjoyed good health. His father told the Doctor that this was the 7th child his wife bore him. She was 30 years of age at his birth, and bore him 2 more afterwards. All the rest were of the natural shape.

Three extraordinary Cases in Surgery. By Bezaleel Sherman, Surgeon, at Kelvedon in Essex. N° 453, p. 138.

Samuel Bush, being on the top of a very high timber tree, in order to shake down the acorns, he let go his hold; and by falling from one bough of the tree on another, he broke his thigh-bone; and one end of it, by the force of the fall, stuck fast in the ground, which fractured the bone in another place, about 2½ inches above the former. This entire piece of the os femoris was taken out; notwithstanding which, so large a callus united the two ends of the bone, that his thigh, when cured, was very little more than a quarter of an inch shorter than the other thigh. The surgeon who had the care of him, used his utmost endeavours, during the cure, to preserve the extension; but he imputed the largeness of the callus to a very great quantity of lap. osteocolla, which he made him take for 6 weeks or 2 months, in powder with milk, in an electuary, in his bread, and in his pudding; in short, in almost all the food he took.

One Fitch, of the parish of Kelvedon, had a foul ulcer in his mouth, with a caries in the lower jaw-bone, one part of which, from the suture at the chin to the end of it under the ear, in process of time entirely came out, with 3 teeth in it. This was also owing to a great quantity of osteocolla internally given, which was thought not only to expedite this large exfoliation, but at the same time to generate so large and firm a callus, that he can chew a hard crust, or any other food, on that side, as well as on the other.

John Spilman, had a sinuous ulcer in his rectum, about 2 inches from the anus. This had remained a twelvemonth, and was taken for the piles, and treated as such, both internally and externally. Mr. S. soon perceived a tumour in his buttock, 2 or 3 inches from the anus, which coming to suppuration, he opened it by incision; and after dressing it several weeks with little prospect of success, he discovered at the bottom of the ulcer something that looked like a bone, which when extracted, proved to be the lower jaw of a fish, as a whiting, or young cod, &c. And unquestionably this was swallowed at least a year before it came away, because the pricking pain he felt when the sharp end of the bone stuck in the rectum, was the symptom mistaken for the piles; and when this had made its way through the rectum, and got into the fleshy part, the aposthume followed in course; and the bone being extracted, the ulcer was soon cicatrized by the common methods of cure in such cases.

Account of a Woman, 68 Years of Age, who gave Suck to two of her Grand-children. By Tho. Stack, M. D. N° 453, p. 140.

A gentleman of credit having informed Dr. S. of a woman near 70 years old, who suckled one of her grand-children, his curiosity was excited to see so uncommon a sight; and the more, in order to try if he could not discover some fallacy in the affair. Wherefore he went in company with the gentleman, to a house in Tottenham-Court-Road, where the woman they inquired for appeared in an instant. Her breasts were full, fair, and void of wrinkles; though her face was very much withered, her cheeks and mouth vastly sunk in, her eyes red, and running with a clammy humour; and though she had in short, all the other external marks that one might reasonably expect to find in a woman, who had spent the last half of her past life in labour, troubles, and other concomitants of poverty, and through them had reached nearly to her 70th year. On pressing her right breast, she fairly squeezed out milk, which gathered in small drops at 3 of the lacteal ducts terminating in the nipple. This experiment Dr. S. made her repeat a 2d time, having himself carefully dried the end of the nipple with his handkerchief, as he had done before her first trial. Convinced of the truth of the fact, he asked her several questions about her case. The substance of her answers was as follows:

Her name by marriage was Eliz. Brian. She was in the 68th year of her age, and had not borne a child for 20 years and upwards. About 4 years before, her daughter being obliged to leave an infant she then gave suck to, in the care of this her own mother, and likely to be a considerable time absent; the old woman, finding the child froward for want of the breast, applied it to her own, barely in order to quiet the infant, without the least thoughts of milk. And this having reiterated several times, a son of hers, by that time grown a man, perceived that the child seemed to swallow somewhat from the nipple; on which he begged leave of his mother to try if she had not milk. The experiment succeeded: the youth drew milk from that same breast from which he had been weaned above 20 years, and which had been unaccustomed to any for 17 or 18 years before: the good woman then continued to suckle her grand-child in earnest: and after some time her daughter, viz. the infant's mother, seeing she was provided with such an extraordinary and tender nurse, was emboldened to bid fair for an increase of issue, which till then she knew not how to nourish or provide for. Accordingly, at the end of 2 years, she brought forth another child; on which the grandmother weaned the first, and suckled the latter; which she had done for the last 2 years, and continued to do. And this infant, in Dr. S.'s presence, took the nipple with as much eagerness, and seeming delight, as he ever perceived in a child of 2 years old; and at it plainly performed the actions of suction and deglution. The 2 children, both girls, are, as to constitution, such as he could wish to the dearest friend; plump and firm in flesh; in complexion cleanly, fair and healthy, and in temper brisk and sprightly; considering the lowness of their condition and education, and the mean diet of the nurse.

When this good woman came to town, which was near 2 years before, her milk abounded to that degree in both breasts, that, to convince the unbelieving, she would frequently spout it above a yard from her: a particular which, among others, the good man and woman of the house, and others of the neighbourhood, likewise assured him of. At the above date her left breast became dry, and she had no great quantity in the right: but what remained was as good milk as could be desired in a nurse. The poor woman seemed perfectly honest and artless, and even tended strongly to dotage.

A Letter from the Rev. Mr. John Clayton, afterwards Dean of Kildare in Ireland, to Dr. Grew, in Answer to several Queries relating to Virginia, sent to him by that Gentleman, A. D. 1687 *. N° 454, p. 143.

Having observed many mistakes in people's notions of Virginia, when speaking of the natives, which have arisen from the want of making a distinction in their expressions, when they speak of the English or whites, born there, thence called natives; and the aborigines of the country; Mr. C. therefore notices, that when he speaks of the natives in general, he means only the Indians.

As therefore to the 1st query:—Their wiochist, that is, their priest, is generally their physician; and is a person of the greatest honour and esteem among them, next to their king, or to their great war-captain.

2. Nature is their great apothecary, each physician furnishing himself, according to his skill, with herbs, or the leaves, fruit, roots, or barks of trees;

* This may serve as a sequel to the accounts of Virginia formerly given by Mr. Clayton. See these Transactions, N° 201, 205, 206, 210.—Orig.

of which he sometimes uses the juice, and sometimes reduces them to powder, or perhaps makes a decoction of them.

3. Though every one, according to his skill, is a sort of doctor, as many women are in England, yet their priest is peculiarly styled their physician, to be consulted on greater emergencies. The rules of the descent of whom, as to families, Mr. C. does not know; for they are a sullen close people, and will answer very few questions.

4. They reward their physician with no certain fees, but according as they bargain for Wampampeake skins, or the like. If the patient be an Englishman, they will agree for a match-coat, or a gallon or two of rum, or so forth, according to the nature of the cure. Sometimes the priest will sell his remedy.

5. Their king allows no salary; but every one that in any nature can serve his prince, is ready to do it, and to do it gratis.

6. They have no consultations, their practice being merely empirical. They know little of the nature or reason of things. Ask them any question about the operation of a remedy, and, if in good humour, perhaps they will reply, It cures; otherwise they will shrug their shoulders, and you may ask 40 questions, and not know whether they understand either the thing, or what it is you say to them.

7. They pay a certain deference of honour to their priest or wiochist, whose person they hold sacred; but they have no laws that bind them to it. In general, the will of their prince stands for reason and law.

8. The means by which they convey their art to posterity, Mr. C. takes to be this. They lodge in their wiochisan houses, i. e. their temples, certain kinds of reliques, such as men's skulls, some certain grains or pulse, and several herbs, which are dedicated to their gods; viz. the skulls in memory of their fights and conquests: the pulse by way of thanks-offering for their provisions; and the herbs, on the same account, for some special cure performed by them. For when any one is cured by any herb, he brings part of it, and offers it to his god; by which the remembrance of this herb and its virtue are not only preserved, but the priest also thus becomes best instructed, and skilled in the art of medicine. For otherwise, they are very reserved of their knowledge, even among themselves. Often when they are abroad hunting in the woods, and fall sick, or receive any hurt, they are then forced to make use of any herbs nearest at hand, which they are not timorous in venturing on, though they know not their virtue or qualities. And thus, by making many trials and experiments, they find out the virtues of herbs; and by using simple remedies, they certainly know what it is that effects the cure.

9. They are generally most famed for curing of wounds, and have indeed various very good wound-herbs, as an herb commonly called Indian-weed, which perhaps may be referred to the valerians, and be said to be platani foliis. They use also the gnafalium Americanum, commonly called there white plantain. As to our plantain, or the heptapleuron, they call it the Englishman's-foot, and have a tradition, that it will only grow where they have trodden, and was never known before the English came into this country. The most famous old physician among the Apomatic Indians, used mostly an herb, the leaf of which is much like self-heal in winter. It makes a good salve, only it fills a wound too fast with flesh. The great success they have in curing wounds and sores, seems mostly to proceed from their manner of dressing them; for they first cleanse them, by sucking, which, though a very nasty, is doubtless the most effectual and best way imaginable; they then take the biting persicary, and chew it in their mouths, and thence squirt the juice into the wound, which they will do as if it were out of a syringe. They then apply their salve-herbs, either bruised or beaten into a salve with grease, binding it on with bark and silk-grass.

10. The distempers among the English natives, are, scorbutical dropsies, cachexies, lethargies, seasonings, which are an intermitting fever, or rather a continued fever with quotidian paroxysms. These are now rarely sharp, but show themselves in a lingering sickness. The griping of the guts mostly dry, and when the tormenta ventris cease, they generally shoot into the limbs, and fix there, in a terrible sort of gout, taking away the use of the limbs. Thus they will pine away to skin and bone, so that their joints will seem dislocated, and their hands utterly crippled. Sore throats, which the last year were very frequent, and deemed infectious, running generally through whole families, and, unless early prevented, became a cancerous humour, and had effects like the French-pox. Likewise pains in the limbs, which seemed to proceed partly from the same humour floating up and down the body. These pains are very severe, mostly nocturnal; for while they walk, if they have the use of their limbs, they feel the least pain. The oil of a fish called a drum, was found very effectual to cure these pains, and restore the limbs.

There are three sorts of oils in that country, the virtues of which might not perhaps be found despicable; the oil of drums, the oil of rattle-snakes, and the oil of Turkey bustards. The oil of sassafras-leaves may be deservedly considered too, for they will almost entirely dissolve into an oil. But to return. There is another sort of distemper, which seems to be the lepra Græcorum. And it may perhaps be no bad conjecture, that this chiefly proceeds from their feeding so much on a delicate luscious sort of pork. Among the Indians they have a distemper called the yaws, and is nearly related to the French-pox; which, it is said, they cure with an herb that fluxes them.

11. The Indians mind neither the pulse nor urine, only judge by the common most remarkable symptoms: and some pretend to form a judgment from the countenance, and are fond of being thought physiognomists.

12. Mr. C. never could find, that they practised blood-letting. They purge much with several sorts of roots of their own country growth, and vomit frequently with various herbs. They sweat boldly and excessively, and after a very strange manner; for they have their sweating-stoves always on the bank of some river; whence they rush forth in the height of their sweat, and run into the water, where they wash and bathe themselves very plentifully. They use no blistering-plasters, but are exquisite at cupping. As the East Indians use moxa, so these burn with punk, which is the inner part of the excrescence or exuberance of an oak. When they design to give a purge, they make use of the following herbs: poake-root, i. e. solanum bacciferum, a strong purge, and by most deemed poison. The roots of tythymal. of which there are two sorts; the one flore minimo herbaceo, the other flore albo. The flower of this last is small, but large in comparison with the other: they are repentes, and grow in old manured grounds. They chiefly make use of the latter of these, and it is a most excellent purge, though it sometimes vomits. It is a quick, but moderate worker enough; and has this peculiarity, that it opens the body in the gripes, when other more violent purgatives will not move it. There is another herb, which they call the Indian purge. This plant has several woody stalks growing near 3 feet tall, and perfoliat: it bears yellow berries round about the joints. They only make use of the root of this plant. They use also the small fleur de lis, whose virtues seem not yet half known, for it has some extraordinary qualities: it does not grow above a hand high, it flowers in March, and is very fragrant. They use also some sort of the apocynums; particularly that which he thinks Gerard calls vincetoxicum Americanum, for there are several sorts of apocynums; he thinks 13 or 14, but they are not all purgative.

They have likewise several sorts of herbs for vomiting; one of which is a little sort of squills. They likewise take the leaves of a certain curious odoriferous shrub, that grows in the swamps, which Mr. C. takes to be the lesser sassafras; they bruise them in water, and then express the juice, which they drink warm. The Indian interpreter prized it much, as excellent physic, and said they found it a very sovereign remedy. The name the Indian gave it was wisochis, which is their general word for physic.

13. The rest of their Materia Medica consists of herbs, of which they have great plenty, and seldom prescribe any thing else. Mr. C. collected above

300 several sorts, that were no European plants; but he mentions at present only the most remarkable. And first, the sassafras tree, whose root is well enough known. It shoots forth its blossoms in March, which are yellow, and grow in little bunches like grape-flowers, and which, when gathered and picked from the husky bud, make a curious preserve. Most sassafras trees blossom, few bear berries, but those that do are generally very thick. They are shaped much like those of dulcamara, but are of a black colour, and very aromatic. The gum-tree, which he refers to the species of plane-trees, and distinguishes it by its fig-like leaf, only more sharply dented. Its leaf smells much like a lemon. The practice is to beat the tree, and then peel off the bark, and so scrape the gum, which has virtues like turpentine, but more astringent and drying. This they usually mix with their common turpentine, which is whiter and more butter-like, than the Venice or Chios turpentine. The further method of preparing this medicine is this: they expose it to the sun on paper, where at first it rather seems to melt, but it will afterwards grow hard; they then beat it to a powder, and administer it. They use much the young buds of the populus, *sive* tulippa arbor, a vast large tree, extraordinarily spacious, bearing flowers about April, much like tulips; its leaves are large, smooth, and well-shaped, which, together with the flowers, render the tree exceedingly beautiful. It bears its seed coniferous, and is an excellent opener of obstructions. The sorrel-tree bears a leaf something like a laurel, in taste much resembling lujula. They use it in fevers, and it seems with good success. This tree grows plentifully on the south-side of James river in Virginia. The swamp-plum-tree, the wood of which they calcine, and make into charcoal, which they beat to a powder, then mix it with grease, and make an ointment of it, with which they anoint the body, and foment it very much, thus curing the dropsy; for it opens the pores to that degree, that the water runs down their legs. Among their herbs, Mr. C. had nearly 40 several sorts shown him, as great secrets, for the rattle-snake-root, or that kind of snake-root which is good for curing the bite of the rattle-snake; but he has no reason to believe, that any of them are able to effect the cure.

He mentions a herb, though unknown, yet worthy to be brought from Virginia. It is the herb called there angelica, but which Mr. C. takes to be libanotis vera latifolia Dodonæi. It grows generally on a rich sandy ground, on a declining brow, that faces the rising sun; the root shoots deep into the earth, sometimes 3 feet, is very tender, and easily broken, of a white or rather cream-like colour; and being lactescent, yields a little milk, thick and yellow as cream; a very early plant. It seldom flowers or seeds under 5 years growth. The leaf is much like our wild angelica, only thinner, and more the colour of a willow-green. Those that seed, have a fistulous stalk about the thickness of dill, a white umbelliferous plant; the seeds are much like angelica-seed, but from the fragrancy of the root, and its being peculiarly bearded, he styles it a libanotis. It stops the flux, and cures it surprizingly. Again, it often loosens and purges the bodies of those that are bound, and have the gripes, especially if it proceeds from cold; and it prevents many unhappy distempers. He has known it give 14 or 15 stools, whereas it will not move a child in health. He thinks it the most sovereign remedy the world ever knew in the griping of the guts, and admirable against vapours. It is sudorific, and very aromatic, and will not be concealed; for wherever it is mixed, it will have the predominant scent. It is mostly called by those who know it in Virginia, by the name of angelica.

There is another root of the species of hyacinths; the leaves are grass-like, but smooth and stiff, of a willow-green colour, and spread like a star on the ground; from the middle shoots a tall long rush-like stem, without leaves, near 2 feet high; on one side grow little white bell-flowers, one above another. The root is black outwardly, but brown within. It is bitter, and probably it has much the same virtues as little centaury. Some call it ague-grass, others ague-root, others star-grass. There are several others whose virtues are by no means despicable; such as the chrysanthamum platani foliis, whose root is very useful in old pains, the sciatica and gout. It is a large herb, grows between 5 and 6 feet tall. There are likewise many others, which bear some analogy to the European plants, such as Solomon's seal, wood-sage, much better than the English; which the Indians use much for infusions, and which they take as we do diet-drink. Little-centaury, red, white, and yellow, &c. However, he could never find above 12 or 14 plants, natives of that country, that agreed perfectly with any of our European plants, but what had some notable differences, if they were not rather to be reckoned a distinct genus.

13. There go traditions of their having an art to poison their darts; but Mr. C. could never find any solid grounds for that report. He has observed, that in those countries, on an ill habit of body, the least scratch is dangerous; and that, for all the care that can be taken to prevent it, it often turns into a very desperate ulcerous sore. And as persons engaged in long marches are liable to many accidents, which may contribute to an ill state of health, when a slight wound in battle has then proved mortal; this he apprehends to have been the cause, why the physician has rather chosen to attribute the death of his patient to the poison of the dart, than the want of skill in himself.

14. As to their morals, they are simple and credulous, rather honest than

otherwise, and unpractised in the European art of lying and dissimulation; but as to the brutal passions, they are sottish and sensual as the beasts of the field.

15. They are almost always either eating or sleeping, unless when they go a hunting. At all hours of the night, whenever they awake, they go to the homing-pot, that is, maze dressed in a manner like our peeled wheat; or else a piece of venison barbecuted, that is, wrapped up in leaves, and roasted in the embers.

16. They drink little besides succahannah, that is, fair water, unless when they can get spirits, such as rum, from the English, which they will always drink to excess, if they can possibly get them; but do not much care for them unless they can have enough to make them drunk; and it seems they wonder much at the English for purchasing wine at so dear a rate, when rum is much cheaper, and will make them sooner drunk.

17. They use tobacco much, which they smoke in short pipes of their own making, having excellent clay, which Mr. C. tried in making crucibles, which he could not discern were inferior to the German. They make also neat pots of the same clay, which will endure the fire for any common uses.

18. They have no opium, though in some old fields on York river, there grow poppies perhaps of no despicable virtue. In fevers, and when their sick cannot sleep, they apply the flowers of stramonium to the temples, which has an effect like laudanum. It is asserted, that when the soldiers were sent over to quell the insurrection of Bacon, &c. being at James-town, several of them went to gather a sallad in the fields, and finding great quantities of a herb called James-town weed, they gathered it; and by eating it plentifully, were rendered foolish, as if they had been drunk, or were become idiots. Dr. Lee likewise assured Mr. C. that the same accident happened once in his own family; but that after a night or two's sleep, they recovered.

19. Their sports are dancing: their games are playing with straws, which as he was not perfectly acquainted with, he found it hard to describe; he can therefore only tell how it appears to a spectator: they take a certain number of straws, and spread them in their hands, holding them as if they were cards, then they close them, and spread them again, and turn them very suddenly, and dextrously. Their exercise is hunting, that is, shooting with a gun, or with bow and arrow, in which they excel. Their women work, plant the corn, and weave baskets or mats.

20. Several have been very old; seemingly without any remarkable difference between them and the English natives. If the English live past 33, they generally live to a good age; but many die between 30 and 33.

21. Mr. C. has been told, that one of their famous wiochists prophesied, that bearded men, for the American Indians have no beards, should come and take away their country, and that there should none of the original Indians be left within a certain number of years, he thinks it was 150. This is very certain, that the Indian inhabitants of Virginia are now very inconsiderable in number; and seem insensibly to decay, though they live under the English protection, and have no violence offered them. They are certainly no great breeders.

22. Though they are sluggish by nature, and slow of speech, yet their method of expression seems vehement and emphatical, and always attended with strong gesticulations. They are generally well proportioned, and for the most part are rather taller than the English. They have all either a very dark brown hair, that may well be called black, or a jet-black, all lank.

An Experiment, to prove that Water, when agitated by Fire, is vastly more elastic than Air in the same circumstances. By the late Rev. John Clayton, Dean of Kildare in Ireland. N° 454, p. 162.

Mr. Clayton having contrived a curious digester, in which bones could be easily dissolved in a very short time, he performed some trying experiments with it. In a small one having included about a pint of water, and, about ℥ij of a marrow-bone, he placed the vessel horizontally between the bars of the iron grate, about half way into the fire; and in 3 minutes time he found it raised to a great heat; on which he thought to have taken it out of the fire, lest it should have burst. For he remembered, that the screws of a digester, made after Mr. Papin's method, giving way, the head flew one way and the screws and irons another, with such violence, that the head having struck a brick, cut a piece quite out of it; which was one reason for his contriving a digester in this way, that the screws cannot possibly start, but that the vessel would sooner break in any other part. On a sudden however it burst, as if a musquet had been discharged. A maid that was gone a milking, heard it at a considerable distance; the servants said it shook the house. The bottom of the vessel, that was in the fire, gave way; the blast of the expanded water blew the coals quite out of the fire, all over the room; for the back of the fire-range was made just like an oven, so that circulating in it, it brought out all the coals at the mouth. All the vessel together flew in a direct line across the room, and striking the leaf of a table, made of an inch oak plank, broke it all in pieces, and rebounded half way of the room back again. He could not perceive any where in the room the least sign of water, though he looked carefully for it, and had put a pint into the digester, save only that the fire was quite extinguished, and every coal belonging to it was black in an instant.

But to confirm the elasticity of water, or to show, at least, that there is a much stronger elastic force in water and air, when jointly included in a vessel, than when air alone is inclosed, he made the following experiment: he took two ℥vj phials, into the one he put about ℥v of water, or better, and so corked it as well as possible; the other he corked in the same manner, without putting any thing into it. He inclosed them both in his new digester, four-fifths being filled with water; when the heat was raised to about five seconds, he heard a considerable explosion, and a jingling of glass within the vessel, and shortly after another explosion, but not so loud as the former; whence he concluded, that both the phials were broken. He then let the digester cool leisurely, and the next day he opened it; both the corks were swimming on the top of the water, but only one of the phials was broken, viz. that one into which he had not put the water.

Again, having had some very strong phials made, to make some peculiar experiments, he took one of them, and having filled it about a quarter full with water, and corked it very well, he set it in a square iron frame, with a screw to keep down the cork, and keep it from flying out. He then put it into a digester, four-fifths filled with water; which being heated to a due height; when opened, he found the cork forced into the phial, though the cork was so very large, that it amazed several who saw it, to conceive how it was possible for so large a cork to be forced into the bottle. Hence it manifestly appears, that the pressure in the digester, in which was proportionally more water, and less air, was stronger than the pressure within the phial, in which was proportionally more air and less water.

Then Mr. C. reasoned thus also of the two former phials: that the air in the phial, in which was included no water, making not a proportionate resistance to the ambient pressure in the digester, in which was a considerable quantity of water, the cork was forced inward with such violence, that it, together with the water, dashed the phial in pieces; but that in the other phial, in which there were five-sixths of water, the inward pressure in the phial being greater than the ambient pressure in the digester, in which were only four-fifths of water, the cork was forced outward; and that the small difference between the proportionate quantity of water and air in the phial and in the digester, being only as four-fifths to five-sixths, was the reason, not only why the bottle was not broken, but also of the faintness of the explosion.

Of a Girl, three Years old, who remained a quarter of an Hour under Water without drowning. By John Green, M.D. Secretary of the Gentleman's Society at Spalding in Lincolnshire. N° 454, p. 166.

On the following case Dr. G. observes, that the reason of the child's being able to abide so long under water was pretty evident: the child, most likely, was infirm, weak, and sickly, from the time of her birth, so that the foramen ovale was not grown up. He remembered about 3 years before to have seen a subject, a woman, 80 years old, who had the foramen ovale so large, that one might easily thrust the middle finger through it; but she was attended with the above-mentioned circumstance, that is, she never enjoyed a moment's health in her life.

May 16, 1737, Rebecca Yates, of Billson near Market-Bosworth in Leicestershire, had a daughter about 3 years of age, that fell into the milldam at the head, near the mill-wheel; and, by the force of the stream, was drawn under the water to the wheel, with her legs forwards; one of her legs went under the mill-wheel, and by reason of the nearness of the wheel to the floor of its water-way, the bulk of the child's leg stopped the wheel from moving at all. The sudden stopping of the mill so much surprised the miller, that he went immediately, and let down the shuttle; but finding it would not go quite down, he came up again into the mill, and looked both above and below, to see if he could find out the cause; then went and drew up the shuttle, and let it down again; but as the gate would not shut quite down, he could not as yet find out the cause of his mill standing still; for which reason he went backwards and forwards between the shuttle and mill-room, for 8 or 10 times, before he found out the cause; but at last he drew the shuttle quite up, by which means the force of the water drove the child from under the shuttle; then he put the shuttle quite down, and thereby discovered the child with her leg under the wheel, and lying on her face. The first word she spoke was, help me, which she repeated 3 times; the miller left her arm for some other person to hold her, while he endeavoured to remove the wheel, so as to get out her leg; and then she said again, for God's sake help me out, if you can: she spoke very briskly, after she was put to bed. But the mill-wheel had torn away all the shin, muscles, sinews, and tendons, of her leg, quite to the bone, and stripped them down to her heel; besides, the shuttle was drawn up and let down on the small of her back several times. The child lived from Monday till Friday, and then died of her wounds and bruises. The whole time of her being under water, which was at the depth of $4\frac{1}{2}$ feet, was near 15 minutes.

The Case of Mr. . . . . . Cox, Surgeon at Peterborough, who fell into a Pestilential Fever on tapping a Corpse lately dead of a Dropsy, drawn up by himself, and read before the Peterborough Society, Sept. 1, 1736. N° 454, p. 168.

An elderly gentlewoman, labouring under a dropsy about 12 months, underwent the operation of tapping 4 several times, by which 35 quarts of liquor were discharged; and dying at last of the distemper, Mr. C. was desired by her friends to let out the water that was then contained in the abdomen, as well to preserve the corpse the longer from putrefaction, as to prevent an annoyance to the company at the time of her funeral.—Yet notwithstanding this was done within a few hours after death, the included humours were become so putrefied as to discolour the external parts with a green and livid hue. The liquor itself was green, and somewhat thicker than new milk; in smell more fetid and offensive than what he ever met with, and so sharp and acrimonious in its nature, as deeply to corrode a silver canula, through which it passed. And what showed it to be highly malignant, may be judged of from the following circumstances.

The night after the operation, he was somewhat restless and uneasy, and the next day afflicted with small tremors, and an unusual lassitude; in about 3 days after, several angry pustules arose on his hands and fingers, and on every place where the least drop of water fell; some of which coming to matter, went off soon; those which did not, continued painful, and remained much longer. The thumb of his right hand, and middle finger of his other, were affected more severely than any other part, the pain more exquisite, the swelling more hard and large, and of a red dusky complexion. This was about the 6th day of his illness, and though the strongest suppuratives were made use of, yet they failed of the desired success, the pains being continual. Being persuaded from the great pulsation and heavy pains, that matter must lodge either under or on the periosteum, an incision was made to the bone, by which only two or three drops of matter were discharged. It was expected this small discharge might in some measure mitigate his pain, but it did not; the same evening, that pain he at first complained of was changed into universal convulsions, and the oppression on the vitals so great, as to threaten immediate death.

The intentions of cure were to fortify the heart with cordials, to enable it to resist and throw out the malignity, and to bring the sores to a plentiful digestion.

The first was treated with the highest alexipharmics; the latter, as at first, with strong suppuratives; this being about the 8th day of his illness, and the convulsions continuing, with an unequal and low pulse, and as there was little appearance of matter, blisters were applied, as near to the parts affected as possible, in order to make a revulsion from the heart, and throw off the morbid matter by the wounds. In about 3 days this point was gained, the convulsions began to abate, and the wounds to digest; in 4 more, he found a cessation of symptoms, except a faintness and lowness of spirits, which hung upon him for a great while after, which pestilential fevers are known always to leave behind them.

He supposes he might receive this infection, as much by inspiration as contact; for some of his assistants, who were in the room only, and never touched a drop of the liquor, found themselves much disordered, and afterwards broke out with red and livid eruptions; which sufficiently showed, that not only the liquor itself, but the effluvia also, were in the highest degree subtle and malignant.

The Variation of the Magnetic Needle, as observed in three voyages from London to Maryland. By Walter Hoxton. N° 454, p. 171.

N. B. The longitude is reckoned from the Lizard.

The first Voyage in 1732.

Latitude north		Longitude west		Variation west	
39°	53′	27°	16′	12°	0′
37	49	27	45	14	0
35	19	39	20	13	0
32	40	50	27	8	0
34	40	56	0	6	30
35	4	65	0	4	28
36	50	0	0	4	48
Return.					
36	11	56	20	9	22
34	52	53	0	6	17
34	33	52	0	6	15
34	45	51	0	6	5
34	36	50	0	6	23
36	0	49	30	7	37
37	20	48	0	9	23
38	4	48	20	10	0
39	27	47	40	10	23
40	8	45	40	10	38
40	30	45	0	13	4
42	32	42	20	11	43
42	40	42	0	12	39
43	27	40	20	13	24
43	32	39	50	13	42
49	48	9	0	16	30
The second Voyage 1733.					
48	12	3	18	18	0
46	7	4	30	16	35
44°	4′	7°	0′	16°	22′
42	17	9	0	16	36
40	0	12	0	15	38
38	5	14	20	14	51
37	36	14	45	13	24
36	32	15	52	13	17
36	16	16	12	13	0
34	2	21	51	11	34
34	4	23	18	9	51
35	6	30	33	10	28
35	12	31	38	9	48
34	23	31	22	10	23
33	34	32	25	8	18
30	19	31	26	7	12
29	17	31	11	6	45
32	24	37	55	6	39
32	50	38	35	10	36
32	11	40	23	11	0
31	19	41	9	6	42
32	25	43	0	5	0
34	5	47	20	8	49
33	45	49	24	10	45
35	1	54	10	8	33
34	0	54	4	5	53
33	41	54	0	5	12
33	51	55	0	6	35
34	59	60	0	7	2
36	32	59	30	7	49

Latitude north	Longitude west	Variation west
37° 1′	61° 10′	6° 45′
37 5	66 22	5 25
36 53	66 40	4 45
36 36	66 40	5 0
	Return.	
38 9	57 40	11 0
38 48	55 50	10 0
42 13	43 48	14 0
44 21	33 17	14 26
45 46	28 17	15 45
49 51	off Plymouth	13 27
50 20	off Portland	13 0
	The third Voyage, 1734.	
39 53	6 37	14 30
37 50	6 40	14 0
36 58	10 30	15 0
34 56	13 0	13 40
33 33	16 10	12 10
33 9	17 38	9 13
32 44	18 6	9 51
31° 39′	20° 13′	9° 49
30 55	22 53	9 6
30 17	25 26	8 39
30 1	27 14	7 56
30 1	27 54	6 48
29 55	30 20	7 41
29 57	33 12	8 0
29 51	37 37	5 41
28 55	39 28	5 23
29 8	40 0	7 12
31 10	44 46	8 6
31 7	46 45	4 46
30 42	49 0	4 40
30 29	49 48	4 0
30 31	52 10	4 49
30 18	53 0	4 45
30 23	55 0	4 22
30 58	57 30	4 52
37 9	68 0	4 50

Some Thoughts and Conjectures Concerning the Cause of Elasticity. By J. T. Desaguliers, LL.D. F.R.S. N° 454, p. 175.

Attraction and repulsion seem to be settled by the great Creator as first principles in nature; that is, as the first of second causes; so that we are not solicitous about their causes, and think it enough to deduce other things from them. If elasticity was admitted as a first cause; as it is by some, it is thought we should admit of too many principal causes in nature; which is contrary to the rules of good philosophy. Philosophers therefore have endeavoured to deduce elasticity from attraction, or from repulsion, or from both. It is observed, that the same particles that repel each other strongly, will attract other particles very strongly; as apppears by many chemical solutions, especially by the alternate solution and precipitation of metals in acid menstruums. Dr. Hales has proved this many ways, in his vegetable statics and hæmastatics. The elasticity of air seems to consist wholly in the repulsive power of its particles, which do not touch one another while the air is in its elastic state; and if those particles be brought nearer and nearer together, the effect of their repulsive force will increase, the air's elasticity being always proportionable to its density by compression, which property will be preserved, though compressed air be kept a year or two; notwithstanding, Mr. Hawksbee, in his Physico-mechanical Experiments, says, that air will lose part of its spring by being very much compressed. But the air with which he tried it, must have been filled with moist vapours; and it is well known, that the steam of liquors will lose its elasticity, especially where its heat decays. Dr. D. has kept several wind-guns, strongly charged, for half a year together, in which the air had lost none of its elasticity: others have found the air as strong after a year; and a person of credit has asserted, that a wind-gun having been laid by and forgotten for 7 years, when it was found, discharged its air as many times, and with as much force, as it used to do. Now, though air, compressed by any external force, does always increase in elasticity, as it diminishes in bulk; yet it may, by fermentation, diminish its bulk very much, without gaining any more elasticity: for if another fluid, whose parts repel one another, but attract the parts of air, be mixed with it, the repulsion of any two particles of air will be diminished, in proportion as a particle of the other fluid, insinuating itself between them, attracts them towards itself on either side. The same thing will happen to the other fluid, in respect of the particles of air, which mixing with its particles, do in the same manner destroy their repulsion. Thus, if we allow an attraction strong enough between the parts of two elastic fluids, it is possible, that by fermentation a solid may be made out of two elastic fluids, which would have still continued fluid without such a mixture. We are taught by chemistry, to mix fluids together, which immediately coalesce into a solid. When brimstone matches are burning, the effluvia of the sulphur repel each other to great distances, as may be known by the sulphureous smell. Now, though these particles repel each other, they attract the air very strongly, as appears by the following experiment.

Take a tall glass receiver, closed at top, holding about 4 quarts of air; and having put its open end over a bundle of brimstone matches on fire, standing up in the middle of a large dish with water in it, to keep the air from coming in at the bottom of the said receiver, you will observe, that not only as soon as the matches are burnt out, but a good while before, the air, instead of being expanded by the flame of the brimstone, will retire into less compass, the water beginning to rise from the dish up into the receiver, and continuing so to do till some time after the matches are burnt out; so that there will be in the receiver only 3 quarts of air, instead of 4, more or less, in proportion to the quantity of brimstone burnt. And this plainly happens by some of the effluvia, or little parts of the sulphur, attracting some of the particles of the air, so as to make an unelastic compound, that precipitates into the water. If the elasticity of the air is quite lost when the repulsion of its particles is taken off, or sufficiently counteracted, it must follow, that its elasticity depends on repulsion; and that this is often the case, appears by a great number of Dr. Hales's experiments.

The doctor took a cubic inch of mutton-bone, and having put it into his gun-barrel retort, he distilled out of it 200 or 300 cubic inches of air, into a large glass bottle, the weight of which air, together with the ashes of the bone left, weighed as much as the whole quantity of bone did at first. Now the air had been confined in that bone, together with many sulphureous particles, in such a manner, that the mutual attraction of the sulphur and the air had alternately destroyed each other's repulsive force, and brought those substances into a little compass; but the fire in the distillation separated them from each other, so as to restore them in a great measure, to their usual elasticity. This appeared by bringing a candle near the mouth of the bottle that held this revived air; for every time the candle was brought near, the air took fire, and flashed out of the bottle with a sulphureous smell.

The air may be consolidated in many hard bodies, so as to be there quite void of elasticity, and there do the office of a cement, till by the action of fire, or some particular fermentations, it is again restored to its perfectly elastic state. This is the meaning of Dr. Hales's words, when he says, that some bodies *absorb*, and others *generate* air; and the same bodies do sometimes absorb, and at other times generate air. He found more or less air in almost every solid substance that he tried; but, what was most remarkable, he found that the calculus humanus, or stone taken out of a man's bladder, was made up of above half its weight of air.

Some have endeavoured to solve elasticity by attraction only; as for example —If the string A B, fig. 7, pl. 7, be considered as made up of particles lying over one another in the manner represented at A D B; it is plain, that if the point D be forcibly brought to C, the parts will be drawn from each other; and when the force, that stretched the string, ceases to act, the attraction of cohesion, which was hindered before, will take place, and bring back the string to its former length and situation, after several vibrations. Now, though this seems to agree pretty well with the phænomena of a string in motion, it will by no means solve the elasticity of a spring fastened at one end, and bent either way at the other, like a knife or sword-blade, as in fig. 8. For if such a spring be bent from A to a, the particles on the side c, which now becomes convex, will be farther asunder at F, while the particles at D, carried to the concave part E, will come closer together: so that the attraction, instead of making the spring restore itself, will keep it in the situation in which it is, as it happens in bodies that have no elasticity, where perhaps only attraction obtains. Thus a plate of lead, a plate of copper, and a plate of soft iron, stands bent.

But the most probable way of accounting for the elasticity of springs, is to consider both a repulsive and an attractive property in the particles, after the manner of the black sand, which is attracted by the load-stone, and has been shown, by Muschenbroek, to be nothing else but a great number of small loadstones.

Let us suppose a row of round particles touching one another only in the points c in a line from A to B, fig. 9. It is plain, from what philosophers have shown concerning the attraction of cohesion, that on the least shake or alteration of the position of a straight line, these particles will run together, and form a sphere, in which the globules will have more points of contact. But if these particles have poles like magnets, in the opposite places marked n s, so that all the poles n, n, n, &c. repel one another; and all the poles s, s, s, &c. likewise repel one another, the line A B will continue straight; for if by any force the same line B A be put into another position, as into the curve b a, then the poles n, n, &c. being brought nearer together, while the poles s, s, &c. are further asunder, will repel one another more strongly, and so hinder the globules from running together towards the concave part; and the spring, left to itself, all this while supposing one end, as b, B, or β, fixed, will restore itself, throwing its end a back to A, and so on to α, by the first law; then, being in the position $\alpha\beta$, the poles s, s, &c. are brought nearer together, whose repulsion, thus increased, throws back α to A, and so on forward, the line of particles performing several vibrations round B.

May not a spring of steel, or other springs, consist of several series of such particles, whose polarity and attraction acting at the same time, will show why such bodies, when they have been bent, vibrate, and restore themselves?

If we take a plate of steel, and make it so hot till it looks white, and then immediately quench it, we thereby fix the metal in a state very near fluidity, so that the particles which the fire had almost brought to roundness, have but a very small contact; as appears by the fragility of the steel thus hardened, which breaks like glass, and has a short grain. Steel, thus hardened, is highly elastic; for what workmen call hard, is the most elastic; as appears by the congress of high-hardened steel balls, which return, in their rebound, the nearest to the place we let them fall from; and, next to glass, have the quickest elasticity of any thing we know.

That we may not be thought to have given an imperfect account of the elasticity of a steel spring, because such a one as we have described wants toughness, and will immediately fly, when bent to any degree; we must beg leave to consider further the properties of the round particles, or little spheres, of steel, in which we have supposed a polarity.

Let us suppose A B, fig. 10, to be two little spheres or component particles of steel, in which, at first, we will suppose no polarity, but only an attraction of cohesion. Then, whether the particles have their contact at c, d, e, n, or at δ, ϵ, s, their cohesion will be the same; and the least force imaginable will change their contact from one of those points to another; because in the rolling of these little spheres, they do not come into more or less contact, in one situation than another. But if we suppose the point n in each spherule to be a pole, with a force to repel all the other points n in any other spherule, and likewise s another pole, repelling the other points s; the spherules will cohere best, and be at rest in that position, where the points c, c, are in contact, and n and s at equal distances on either side. For if the spherules be turned a little, so as to bring the points d, d, into contact, as in fig. 11, the poles, n, n, being brought nearer, act against each other with more force than the points s, s, which are now farther off, and consequently drive back the spherules to the contact at c c, beyond which continuing their motion, they will go to δ δ, fig. 12, and so backwards and forwards, till at last they rest at c c, which we may call the point of equilibrium for rest in a spring. Now there are, besides this, two other points of equilibrium, beyond which the spring may break, which are the points e, e, towards n, and ε, ε, towards s; see fig. 13, that is, when the spherules have their poles n, n, brought very near together, the mutual repulsion increases so, that the attraction at the contact is not able to hold them, and then they must fly asunder, the spring breaking. We suppose the points e, e, to be the points of contact, beyond which this must happen; but that if the contact be ever so little short of it, as between e and d, the spherules will return to their contact at c, after some vibrations beyond it, as has been already said. This is the reason why he calls e, in one of the spherules, and its correspondent point ε, on the other side c, the points of equilibrium; for if the spring be bent towards a, fig. 9, so that the spherules, like A and B, fig. 13, touch beyond e, the spring will break. Likewise if the spring be bent the other way, till the spherules touch beyond e, then it will break the other way. Now when the spherules touch at e, e, or at ε, ε, the spring is as likely to return to its first position as to break; for which reason he has called the points e and ε, points of equilibrium, as also, having known by experience, that a spring left bent to a certain degree, has, after some time, broke of itself.

From all this it appears, that spherical particles will never make a tough spring; therefore the figure of the particles must be altered, in order to render it useful; and this is what is done in bringing down the temper of the hard steel, and letting down a spring, as it is called. What change ought to be made in the particles, we shall first show; and then consider how far that is done by those who make springs.

If the parts supposed globules, as in fig. 9, are now flattened at c, where the contact is, so as to put on the shape n e d c δ ε s, as in fig. 14, the contact will be much increased, and reach from d to δ, so that in bending the spring there will still remain a great contact in the particles, and the points of equilibrium for breaking, viz. e, e, above, and ε, ε, below, will be removed nearer to the poles n, or s, than when the particles are round; the consequence of which will be, that the spring must be bent much further, to be in danger of breaking, than in the former supposition; as may be seen in fig. 15, where two particles being opened about the point d as a centre, the attracting points c, c, and δ, δ, have still some force to help to bring back the particles to their whole contact; because, in this shape of the particle, the attracting points c, c, δ, δ, are removed only in proportion to their distance from the angular point d; whereas if the particles had been spherical, and the line d δ an arc of a circle, the attracting points c, c, and δ, δ. would have removed from one another further than in proportion to twice the square of the distance from d, as in fig. 11, and so have afforded very little help for bringing back the particles to their contact. A row of particles in the spring thus conditioned, is to be seen in the natural state at BA, fig. 16, and bent at ba in the same figure. Here it is to be observed, that if, in this figure of the particles, you would bend the spring to bring the particles to touch at their point of breaking equilibrium, you must open them so much on the contrary side, that the spring will be bent far beyond any uses intended to be made of it, as appears by fig. 17, where two particles are brought to touch at the equilibrating point e; and by fig. 18, where many particles being put into that condition, the spring is brought round quite into a circle.

Now the common practice in making springs, is the most likely to produce this effect required in the particles; for the hard spring, whose particles were round, or nearly so, is heated anew, and while cooling gently, the mutual attraction increases the contact, so that the particles grow flatter in those places where before they had but a small contact; and lest this contact should become too great, the spring's softening is stopped by quenching it in water, or oil, or grease. Another way of making springs, is to begin and shape them in cold unelastic steel, and then having heated them to a small degree, for example, to a blood red heat, immediately to cool them in some proper liquors. This also settles the particles in their oblong figure, through which they must pass before they become round, or nearly so, in a white heat. That particles of steel are fixed in the figures which they have at the instant of dipping, will not appear strange, when we consider, that dipping red-hot steel in cold liquors, in a particular position, makes it magnetical. If it be asked, how we account for making springs only with hammering, it is easily answered, that we can make iron and steel magnetical only with hammering; and if we can give and destroy poles in the whole piece, there is no improbability to think we can give poles to little parts; or rather bring into a particular situation the poles which they have; for if the poles that we have considered be placed quite irregularly, there will be no elasticity at all. Agreeably to this, springs may be made of other metals than iron or steel, though not so perfect, by hammering; for it will be sufficient for the little particles to have poles that attract and repel one another, driven by the hammering into a regular order.

This, applied to the vibration of a string, will better solve its several cases, than attraction alone; and the elasticity of glass is just the same as that of a very brittle steel-spring.

Some Thoughts and Experiments concerning Electricity. By J. T. Desaguliers, LL. D. F. R. S. N° 454, p. 186.

The phænomena of electricity are so odd, that though we have a great many experiments on that subject; we have not yet been able from their comparison to settle such a theory, as to lead us to the cause of that property of bodies, or even to judge of all its effects, or find out what useful influence electricity has in nature: though certainly, from what we have seen of it, we may conjecture, that it must be of great use, because it is so extensive.

Though some persons have been too hasty in their conjectures, and too apt to run into hypotheses not sufficiently supported by experiments; yet it would be of great use to settle some general propositions concerning electricity, from the light we have already, and what we may further discover by future experiments; provided we have a sufficient number of them to settle a general rule. For example; I now propose some general assertions to be considered, and to be rejected or allowed of, as a number of experiments shall determine; but to stand only as queries till they are settled.

I have hitherto avoided entertaining the Society on this subject, or pursuing it so far as I might have done, considering that I can excite as strong an electricity in glass, by rubbing it with my hand, as any body can, because I was unwilling to interfere with the late Mr. Stephen Gray, who had wholly turned his thoughts that way; but was disposed to decline it entirely, if he imagined that any thing was done in opposition to him. But now I intend not only to go on myself in making electrical experiments, but shall always be ready to make such as shall be proposed by any member of the Society. The Queries which I have already examined, are the following:

Query 1. Whether all bodies in general are not capable of receiving the electricity which has been given to a tube by friction, though there be a great many bodies, such as metals and vegetables, &c. in which we have not hitherto been able to excite any electricity by heat, or friction, or any other operation on the bodies themselves?

Query 2. Whether, when a string is stretched out at length, with a body hanging at one end of it, to which body we would communicate the electricity of the tube rubbed at the other end, the supporters of the string ought not to be of such bodies as are capable of having electricity excited in them, by friction, heating, beating, or patting, or some immediate operation on the bodies themselves?

Query 3. Whether these supporters of the string, mentioned in the last Query, which stops the electrical virtue from passing any further, are not of such a kind, as are incapable of having the electrical virtue excited in them immediately by any operation yet known; though they are all capable of receiving it from a rubbed tube, even at a great distance, by the communication of a string made of vegetable substances?

Query 4. Whether the reason, that some supporters transmit the electricity running from the rubbed tube along the string, to bodies beyond them, be not as follows, viz. that having received some of the electrical stream, they soon become saturated with it, and so receiving no more of it, let the rest pass on without disturbing it?

Query 5. Whether the reason, that supporters made of vegetable substances, metals, and such others, as stop the electricity abovementioned from running any farther along the string than the place where it rests upon them, be not this? viz. that they are never saturated with the electrical stream, but continually receive it, and transmit it to the next contiguous body, provided that contiguous body be of the same kind with themselves, and also contiguous to other bodies of the same sort: I mean such as would stop the electricity, if the string was supported by them. For even these supporters will transmit the electricity, if terminated at each end by bodies that transmit the electricity, when they support the string.

Query 6. Whether we may not distinguish all bodies in general, in respect of electricity, into such as may be excited to electricity, and such as cannot be excited to electricity? the two kinds of bodies receiving the electricity from other bodies into which it has been excited differently; the first also transmitting the electricity, while the others do not.

These queries are such as arise from a consideration of experiments made by others, and such as I have made myself.

Experiments relating to the first Query.—I stretched a cat-gut, about 5 feet in length, and fastened it to the top of two chairs in a horizontal situation; and such another cat-gut string to two other chairs parallel to the first, and at the distance of 15 or 20 feet from the former. I then suspended one end of a packthread to the middle of the first cat-gut, and carried it on so as to lay it over the middle of the other cat-gut, and leave the other end of the packthread hanging down about a foot below the cat-gut, with a loop to hang several bodies to it, successively to receive the electricity excited by the tube, and applied to the other end of the packthread.

All the bodies I tried received the electricity communicated from the rubbed tube along the string, which appeared by holding a thread fastened to a stick, the thread being attracted towards the suspended body.

1. A gold medal. 2. A silver medal. 3. A copper medal. 4. A brass ball. 5. A steel ball. 6. A tin ball. 7. A leaden ball. 8. Sulphur. 9. Sealing-wax. 10. Pumice-stone. 11. Bees-wax. 12. Resin. 13. Sal ammoniac. 14. Ivory. 15. Human bone. 16. Fish-skin. 17. Loadstone. 18. Flesh. 19. Cotton. 20. Wax-candle. 21. Tallow-candle. 22. A leek. 23. Celeri. 24. Tobacco-pipe. 25. A glass-ball. 26. A rush rolled up.

Experiments relating to Query 2,—Retaining the first supporting string of cat-gut, instead of the last cat-gut supporter, I made the packthread pass over the following substances successively, all which transmitted the electricity to the body suspended at the end of the packthread; viz. 1. A silk string. 2. Hair rope. 3. Parchment. 4. A thong of sheep-skin, but it stopped the electricity till it was dry and warm. 5. A list of woollen cloth. 6. A list of flannel. 7. Cadis, or a kind of worsted tape. 8. Quills. 9. Whalebone. 10. A man's thigh-bone. 11. A bladder. 12. A cat, held between two. 13. A tallow-candle. 14. A wax-candle (the string was also laid over the unburned cotton wick at the end of the candle). 15. A tallow-candle and its wick. 16. Tobacco-pipe, with a cat-gut or a packthread through it, or without, that is, a packthread string being fastened at each end of it. 17. A sword-belt. 18. A piece of a white hat. 19. A piece of a black hat. 20. A glass tube. 21. The same with water in it. 22. With spirit of wine. 23. The same with mercury in it. 24. Sealing-wax. 25. Crape.

All these substances, except the sheep-skin, the tobacco-pipe, the quills, the candles, and the bone, not only transmitted the electricity, but became so far electrical, as to attract the thread a little way on each side of the supported packthread. More experiments are required to be made, before this query can be turned into an assertion.

Experiments relating to Query 3.—Instead of the last supporter of cat-gut, near the suspended body, I made use of the following substances, stretched from chair to chair: and then the thread hanging on the stick was not at all attracted by the suspended ivory ball, which I made use of in all the experiments to try the supporters.

1. A hempen rope. 2. A small packthread. 3. A drawn sword. 4. A sword in the scabbard. 5. The scabbard without the sword. 6. A twisted cotton thread. 7. Tape made of thread. 8. Bars, tubes and wires of copper, brass, iron and lead. 9. White paper and brown. 10. A moist thong of sheep-skin. 11. Celeri. 12. Leeks. 13. Fir-wood. 14. A cane. 15. A piece of black thorn. 16. The same rushes that had before received the electricity when suspended. 17. A sponge dry. 18. White thread. 19. Hay. 20. A marble slab.

Such bodies as were too short to reach from chair to chair, were lengthened out by pieces of packthread at each end.

Experiments relating to Query 4.—The cat-gut supporters, and all the others mentioned in the experiments to Query 3, which transmitted the electricity, attracted the thread of the stick near the conducting packthread, but not so far as the chairs to which the said supporters were fastened.

Experiments relating to Query 5.—All the supporters which did not transmit the electricity, when they reached from chair to chair, were made to transmit, when they were lengthened out with cat-gut at each end, and then they became electrical themselves from one end to the other, as becoming part of the suspended body; and becoming so saturated, as not to be able to carry the electricity on either side any farther than the cat-gut to which they were fastened.

Experiments relating to Query 6.—The late Mr. Stephen Gray has, by rubbing, excited electricity in several of those bodies, which I have made supporters of, to transmit the electricity. See Philos. Trans. N° 366. I have done the same with several others, but not with all of them, though I shall try them all: but as it is more difficult to excite that virtue in some than others; and all the experiments in general succeed better in dry and cold weather, than in moist and warm, I must wait for proper opportunities to make the experiments, and then I shall communicate them.

Experiments concerning mixed Substances.—1. Cadis, or woollen tape, laid on thread-tape, when made a supporter, transmitted the electricity. 2. When the thread-tape was uppermost, the electricity was stopped. 3. When they were twisted together, the electricity was transmitted, but most weakly when the packthread going to the ball was laid over that part of the twist which had the thread-tape.

The two paper supporters, which did not transmit the electricity, ought to have done it according to Query 2; because, by Mr. Gray's experiments, electricity is to be excited in the paper by rubbing: therefore, perhaps the papers wanted to be drier or warmer, so that I shall try them again. These are the only two experiments that do not agree with the 2d Query; but I would not omit mentioning them, because it is the part of an impartial philosopher, to mention as well those things which favour, as those that disagree with his hypotheses and conjectures.

Experiments made before the Royal Society, Feb. 2, 1737-8. By J. T. Desaguliers, LL. D. F. R. S. N° 454, p. 193.

In the following account, which is the sequel of former experiments, I call conductors those strings, to one end of which the rubbed tube is applied; and supporters such horizontal bodies as the conductor rests on.

Exper. 1.—Old packthread supporters transmitted electricity but weakly, though more strongly when twisted with cat-gut; but new packthread did better.

N. B. Where it is not mentioned otherwise, an ivory ball hangs at the end of the conductor; and its electricity is tried by a thread applied near it.

Exper. 2. A conducting string of cat-gut received the electricity a little way; but did not carry it quite to the tube.

Exper. 3.—Two conducting strings, one of cat-gut, and one of packthread, compared, the first attracted less and less, as the distance from the tube increased; and the other more and more; till it was strongest at the suspended body: but both ceased immediately after the removal of the tube.

Exper. 4.—A sealing wax supporter transmitted the electricity, but received little or none when suspended. If it was but just rubbed with the hand, it attracted the thread when first suspended; and strongly, if much rubbed; but that virtue was soon lost, if the tube was applied to the conducting string, and then it would receive no more electricity from the tube. If the stick of wax was wet, then it would strongly receive the electricity.

A wax supporter wet, and silk string wet, did not transmit the electricity.

Exper. 5.—Dried ox-guts did not transmit electricity when held in hand; but when tied to cat-gut, transmitted it; and, when suspended, received it plentifully.

Exper. 6.—The same with a small cord.

Exper. 7.—The same with a rod of iron, and tube of brass.

Exper. 8.—A glass tube, made conductor, received the electricity but a little way.

Exper. 9.—Dry sheep-skin transmitted the electricity, but not when wet, though it received it then, when suspended.

Exper. 10.—A middle supporter of packthread was again supported on one side by a glass tube, and on the other by sealing-wax, and had at each end an ivory ball hanging. Those balls became electrical in the same manner, and at same time, as the ball at the end of the conducting spring.

Exper. 11.—When a bar of oak was made use of instead of the tube, or a small iron bar instead of the wax, the electricity was stopped: but when the bar was thrust a little way into a glass tube, the electricity was communicated as before.

Experiment made at the Royal Society, Feb. 9, 1737-8. *By the Same.* N° 454, p. 196.

I fixed 6 iron radii, of twisted iron wire, to a brass ring, of 2 feet diameter, and half an inch wide, which had a socket in the centre, by which to set it either on an upright glass tube, or on a wooden pillar: then were hung on the end of the 6 radii, next to the circumference, the following substances. 1. A piece of resin. 2. A stick of wax. 3. An apple. 4. An ivory ball. 5. A steel ball. 6. A glass ball.

Exper. 1 *and* 2.—I rubbed the tube, and applied it to the centre of this machine, as it stood on a glass tube; and the electricity was communicated to all the suspended bodies, and the ring also; but none of them received it, when the machine stood on a wooden pillar, with its foot on the floor.

Exper. 3.—I tied to the ends of the 6 radii as many cat-gut strings, but so long as to unite together about a foot higher than the centre of the ring, where they were suspended by another cat-gut string 3 feet in length, the top of which was fastened to a hempen rope. Then applying the rubbed tube very near the place where all the cat-gut strings joined over the ring, at which ring the same bodies were suspended as before, neither the bodies nor ring received any electricity.

Note. This was done in foul weather, when the electricity does not extend itself far from the tube: but in fair weather, the electrical virtue, at the same distance, reached the iron radii of the ring; and consequently the ring and bodies suspended, though the virtue was not propagated, along the cat-gut: for if the tube was applied a little higher to the single cat-gut, so as the effluvia, or virtue darted directly from the tube, did not reach the ring, or its iron radii, then no virtue was communicated to the ring nor to the suspended bodies, &c.

Exper. 4.—I suspended the ring by 6 packthreads, just in the same manner as the cat-gut strings before; but still all those strings were suspended by the perpendicular cat-gut of 3 feet in length. Then all the bodies received the electricity from the rubbed tube applied to the top of the pyramid of packthread.

Exper. 5.—Instead of the perpendicular cat-gut, between the pyramid of packthread and the upper hempen string, I substituted a packthread; and then no virtue was communicated to the ring, but all went up the hempen string, and was lost; except when the tube was held very near the ring, and then it gave a small degree of electrical attraction to the ring, and the bodies suspended at it.

Exper. 6.—Having again suspended the ring with the bodies and pyramid of packthreads to the perpendicular cat-gut, I tied a packthread to the ring, and carried it horizontally about 20 feet from the ring; and having fastened to it a cat-gut string, 3 feet long, I gave it an assistant to hold: then applying the rubbed tube to the end joining that cat-gut, the electricity was communicated to the ring, and all the suspended bodies, as appeared by applying the white thread near them, which was attracted by every part of the ring, and all the bodies.

Experiments made before the Royal Society, Feb. 16, 1737-8. *By the Same.* N° 454, p. 198.

Exper. 1.—I applied the rubbed tube to a burning candle, and it had no manner of effect on the flame; but as soon as the candle was blown out, it attracted the smoke at 4 or 5 inches distance.

Exper. 2.—A horizontal packthread, of about 18 feet in length, being terminated by the cat-gut strings, of 3 feet long each, and hung, towards one of the ends of the packthread, on it a candlestick with a lighted candle in it; then applying the rubbed tube to the other end of the packthread, the candlestick attracted the thread, and it was also attracted by the candle, but not within 2 or 3 inches of the flame; but as soon as the candle was blown out, the thread was attracted by every part of it; nay, even the wick, when it was quite extinguished.

Exper. 3.—I suspended a wax candle in the same manner, and the experiment succeeded the same; only the electricity came not so near the flame in the wax as in the tallow candle.

Exper. 4.—I hung an iron wire, 16 feet long, horizontally by two cat-gut strings at its ends, about 3 feet long each, and bent down the wire from the place joined to the cat-gut, so as to hang down a foot at one end; then applying the rubbed tube at the other end, this conductor carried the electricity along to the ball; but not so well as the packthread conductor; but it did something better when it was wet.

The same happened when the conductor was brass wire of the same length.

The packthread conductor also carried the effluvia stronger when wet.

An Account of some Electrical Experiments made before the Royal Society on Thursday the 16th February, 1737-8. By the Same. N° 454, p. 200.

Exper. 1.—I took the glass tube AB of two inches diameter, fig. 1, plate 8, which had at one end A, a brass ferril with a brim cemented to it, and at the other end B, a brass cap close at top, the brass-work being joined to it, in order to exhaust it of its air on occasion. When this tube was very dry it would become electrical by rubbing, so as to snap by passing the ends of the fingers near it; but that virtue could not be excited in the tube nearer the brass at the ends than from a to b, and not unless the tube was very dry within.

The tube being thus prepared, and having an ivory ball C of about 2 inches diameter, tied to it at the end B by a short string, I passed the tube through the horizontally suspended place DD, till it was stopped by the brim at A; and as it hung perpendicularly, the ball C was within a foot and a half of the floor. The plate DD was about 10 inches in diameter, and suspended by 3 small cat-gut strings, as E e, of about 2 feet in length, all which were tied together at E, to a hempen string hanging from the cieling at F.

By reason of the distance of the ends of the cat-gut strings close to the plate at e e e, I was able to thrust in between them one end of an open tube GG, after it had been rubbed so as to make it electricul, to see whether I could make the aforesaid suspended tube AB the conductor of electricity to the ball C; but the first trial was in vain.

Exper. 2. Then laying horizontally over the plate DD an iron bar, a quarter of an inch thick, and a yard long, I hung at the ends of it two ivory balls c, c, of the same size as C, by packthreads of the same length at the tube AB.

Having again made the tube GG electrical, I applied it over A, as before, and immediately the two balls *c*, *c*, received the electricity, so as to attract the thread of trial T, hanging at the end of the stick ST, when applied near them; though it received no motion when applied to C. But if the strings H c, instead of packthread, were cat-gut, then the balls c, c, received no electricity from the tube GG, rubbed and applied over A.

To be certain that the rubbed tube is made electrical, I pass my fingers near it after rubbing, to hear whether it snaps; but always rub again before I apply it; because by snapping it loses its electricity at the place where it snaps.

Exper. 3.—When I rubbed the tube AB, it would then attract the thread of trial T between a and b; but not at all above a or below b, unless when I applied the tube GG above A: then the thread of trial would be attracted by the plate DD, and the top of the great tube from A to a, but no lower. It would also be attracted by all the bar HH, and only 3 or 4 inches below H.

Exper. 4.—Having filled the tub AB with water, the electricity of the rubbed tube GG, applied at A, ran strongly down the tube AB, and impregnated the ball C, so as to make it strongly attract the thread of trial, while the balls c, c, received no virtue at all. But on wetting the cat-gut strings H, c, with a sponge, all the 3 balls c, c, c, strongly received the electrical virtue.

Exper. 5.—I took away the bar HH, and its balls and strings; and having well dried the tube, I rubbed it, and hung it up as before; so that it would snap, or attract the thread from a to b, but no where else.

Then putting the small bar HH into the middle of the tube AB, in its axis, represented by the pricked line, on application of the rubbed tube GG at A, the virtue was immediately communicated to the ball C. The same thing happened when instead of the bar, a brass wire, a walking cane, a small green stick, or small packthread, was placed in the axis of the tube.

Exper. 6.—I took a barometer tube, empty, and very dry, and placed it in the axis of the great tube AB; but it would conduct no electricity to the ball C; though it carried it down very readily when full of water, though quite dry on the outside.

Another small tube, open at both ends, which conducted no virtue to C when dry, being only moistened a little by the breath in blowing through it, carried down the virtue from A to C very strongly.

All this while the cat-gut strings E, e, received no electrical virtue.

Definition 1.—A body electrical per se, is such a body in which one may excite electricity by rubbing, patting, hammering, melting, warming, or any other action on the body itself, as amber, sealing-wax, glass, resin, sulphur, &c. besides many, if not all, animal substances.

Defin. 2.—A non-electrical, is such a body as cannot be made electrical by any action on the body itself immediately; though it is capable of receiving that virtue from an electrical per se.

Observations.—1. When the air is full of moist vapours, electrics per se are excited to electricity with very great difficulty, requiring to be often warmed, and much rubbed; as appears in exciting that virtue in glass, amber, wax, &c.

2. In dry weather, especially in frosty weather, the electricals per se will have their virtue excited with very little action on them; as appears by warming a glass receiver, which, without any rubbing, will cause the threads of a down feather, tied to an upright skewer, to extend themselves as soon as it is put over the feather. Sometimes resin and wax exert their electricity by only being exposed to the open air.

3. Electricals per se retain the virtue longest when kept near to, or inclosed by, other electricals per se. Thus the rubbed tube will retain its virtue pretty long in dry air, as appears by chasing a feather about the room very long without new rubbing; as also by lumps of resin and sulphur, &c. which have been melted and poured into dry drinking glasses, keeping their virtue long, if kept in those glasses, and wrapped in dry silk, or such sort of paper as will become electrical by rubbing; for as often as they are exposed to the air, they will attract.

4. Electrics per se communicate their virtue to any of the non-electrical, when brought near them; in which case the non-electric attract and repel like the electrics per se. Thus an iron bar suspended by a silken thread, a hair rope, or a dry cat-gut, when an excited electric per se is brought near it, will both attack and send out its effluvia to a non-electric held near it; as appears in the dark by the light coming out at the end of the bar.

5. An electric per se loses its excited virtue on communicating to the non-electric; and the sooner, the more of those bodies are near it. Thus in moist weather the rubbed tube holds its virtue but a little while, because it acts on the moist vapours that float in the air; and if the rubbed tube be applied to leaf-gold or brass, laid on a stand, it will act on it much longer, and more strongly, than if the same quantity of leaf-gold be laid on a table, which has more non-electric surface than the stand.

6. When a non-electric is suspended by, or only touches an electric per se, it receives the properties of an electric per se from a rubbed tube or wax, &c. This appears by the fire that flashes from the fingers of a man suspended by hair-ropes, or who stands on a cake of resin, when he has received virtue from the rubbed tube.

7. The virtue which a non-electric receives from a rubbed tube, runs on to the most distant part of the suspended body, from the place where the tube is applied, and seems to be collected there, from whence it flashes in the dark, snaps, and exerts its attraction on the thread of trial; though as the virtue runs along, it sometimes shows itself in other parts of the suspended non-electric.

8. If a non-electric, while receiving the virtue from the rubbed tube, be made to communicate with the floor of the room, or any other great non-electric body, by a non-electric string, how small soever, though but a thread, the virtue will not show itself, as it did before, at the extremities, where the flash of light was seen.

9. If a non-electric be ever so large, when suspended, it will receive electricity from the rubbed tube. And if 5 or 600 feet long, when the rubbed tube is applied at one end, the bodies hanging at the other end will become electrical. This has been tried by several people, as well as myself.

10. If a long non-electric string be fastened to an electric per se, and extended to a great distance, being supported by electric per se, to keep it from touching the ground, all bodies fastened at the end of it will become electrical, when the rubbed tube is applied at the other end, though the tube does not touch it, but is only brought within 2 or 3 inches of it.

Note, This string we have before called the conductor of electricity, and the cat-gut or silken strings, glass tubes, or whatever kept the long string from touching the ground, supporters.

11. If any of the supporters, mentioned in the last observation, be changed for a non-electric supporter, the virtue will there be stopped, and taken away by that supporter: but if that supporter be again supported by electrics per se, it will only receive so much electricity as will impregnate it, and then the virtue will go on to the end of the string, and impregnate the bodies fastened to it.

12. The non-electric receive the greatest virtue at the end of the string, and most of all, if they are wet. But the electrics per se, if long bodies, as long sticks of wax, and glass tubes, only become electric at the end next to the string.

13. Electrics per se will become non-electrics, if they be wet, or only moistened. Thus supporters that transmit the electricity immediately, stop it when wet with a sponge, or when blown through, if open tubes. And if the long electrics per se, hanging at the end of the conductor, be made wet, they will become non-electrics, and strongly receptive of the virtue given by the rubbed tube at the other end of the string. All the 6 experiments mentioned in the beginning of this paper, confirm this observation.

14. A non-electric having been impregnated with electricity, by the rubbed tube, is repelled by it, till it has lost its electricity by communicating it to another non-electric. Then being in its first state, it is again attracted by the tube, which holds it till it has fully impregnated it; then it repels it again. This is evident, by attracting a down feather by the tube in the air, and then repelling it: so as to make it dance backwards and forwards to and from a finger

held up at a foot or two from the tube. But the thing appears more plainly from the following

Exper. 7.—Having rubbed the tube Tt, fig. 2, pl. 8, and with it attracted a feather, the feather at t was repelled from the tube, whenever it was brought near it; but suddenly dipping the end T of the tube in water, the feather floating in the air came to it again, and stuck to the end of the tube at T, or near F.

In fair weather this experiment will not succeed, unless the tube be thrust pretty deep into water, a foot at least; but in moist weather an inch or two will do.

Though animal substances be generally thought to be electric per se, yet it is only when they are very dry: this is the reason why a living man suspended by a hair-rope, or standing on a cake of resin, to receive electricity from the tube, must be considered as a non-electric, by reason of the fluids of his body.

Of some Electrical Experiments, made at his Royal Highness the Prince of Wales's House at Cliefden, on Tuesday the 15*th of April,* 1738; *where the Electricity was conveyed* 420 *feet in a direct Line. By the Same.* N° 454, p. 209.

Having heard that electricity had been carried along a hempen string 5 or 600 feet, but having only seen it when the string was carried backwards and forwards in a room by silk supporters, Dr. D. wished to dry it with a packthread string stretched out at full length; for which purpose having joined a cat-gut string of 6 feet long, he fastened it to the inside of a door in the suite of rooms at Cliefden; and having also tied another cat-gut, like the first, to the other end of the string, he tied it up to the inside of the door at the other end of the house; but at the place where the packthread was joined to the cat-gut, he left a foot and a half of packthread hanging down, and fastened to it a lignum vitæ handle of a burning-glass. Then applying a rubbed tube at the other end of the string, he made the electricity run to the lignum vitæ, but with some difficulty, which he attributed to the size, being an animal substance, that still stuck to the packthread as it was new; therefore he caused the packthread to be wet with a sponge from one end to the other, to wash off the size: then was the electricity from the tube communicated very soon and very strongly; for the thread of trial was drawn by the lignum vitæ at the distance of a foot.

Afterwards having joined more packthread together, he made a string of 420 feet long, one end of which was fastened, by the interposition of cat-gut as before, to the iron gates in the garden, before the house, and the end which had the lignum vitæ handle, to the upper part of the door next to the back-side of the house in a large drawing-room, taking care that the string came through the middle of the opened doors through which it passed; and to prevent this string dragging on the ground, three pieces of cat-gut held across by two men, at equal distances from the ends, and from each other, supported it. The string was altogether dipped in a pail of water, before the experiment; but great care was taken, that the cat-gut should not be wet.

. Then he applied the rubbed tube at the end in the garden, while an assistant held the thread of trial near the handle abovementioned, which thread was strongly attracted, though the wind was very high, and blowed in the contrary direction to that in which the electricity ran along.

He first tried the experiment with the packthread dry, but then it would not do at that distance. The weather was moist when he made the experiment.

Botanical Observations, exhibiting accurate Descriptions of some Plants. By Paul Henry Gerard Moehring, M. D. N° 454, p. 211.

This paper contains a description of 6 plants; viz. 1. Salicornia ramis clavatis, squamis articulorum adpressis. — 2. Verbascum foliis cordatis crenatis acutis glabris: floralibus ternis. — 3. Senecio foliis pinnatifidis lacinulatis: laciniis omnibus laxis patentissimis linearibus acutis.—4. Illecebrum Linn. Corollar. gen. 947. Rupp. Jen. 79. Corrigiola Dillen. Giss. Supplem. adpend. 167.—5. Ruppia foliis linearibus obtusis.—6. Hippuris. Linn. gen. 1.—

Observations on an Anthelium, seen at Wirtemberg, Jan. 18, N. S. 1738. *By J. F. Weidler.* N° 454, p. 221.

An Occultation of Aldebaran by the Moon, Dec. 23, 1738, N. S. *Observed by Christfried Kirch, Astronomer Royal at Berlin.* N° 454, p. 223.

Immersion at	6^h	31^m	54^s	correct time.
Emersion	7	33	32	

The same Occultation, observed at Wittemberg. By J. F. Weidler, F. R. S. N° 454, p. 225.

Immersion	6^h	27^m	35^s
Emersion	7	29	20
Duration	1	1	45

A Solar Eclipse observed at Wirtemberg, July 24, o. s. 1739. By J. F. Weidler, F. R. S. N° 454, p. 226.

At 4^h	15^m	30^s	P. M.	Beiginning of the eclipse.
4	55	40	....	6 digits eclipsed.
5	24	40	....	9 digits, the greatest obscuration.
5	50	30	....	6 digits when decreasing.
6	27	20	....	End of the eclipse.

Of a terrible Whirlwind, which happened at Corne-Abbas in Dorsetshire, Oct. 30, 1731. *By Mr. J. Dorby.* N° 454, p. 229.

On Saturday Oct. 30, about a quarter before one in the night, there happened at Corne-Abbas, Dorsetshire, a very sudden and terrible wind whirl-puff, as Mr. D. calls it: some say it was a water-spout, and others a vapour or exhalation from the earth. It began on the south-west side of the town, passing directly to the north-east, crossing the middle of the town in breadth 200 yards. It stripped and uncovered tiled and thatched houses, rooted trees out of the ground, broke others in the midst, of at least a foot square, and carried the tops a considerable way. The sign of the new inn, a sign of 5 feet by 4, was broken off 6 feet in the pole, and carried cross a street of 40 feet breadth, and over an opposite house. It took off and threw down the pinacles and battlements of one side of the tower; by the fall of which, the leads and timber of great part of the north aile of the church were broken in. The houses of all the town were so shocked, as to raise the inhabitants. No hurt was done but only across the middle of the town in a line. Nor no life lost. No other parts of the neighbourhood or country so much as felt or heard it. It is supposed by the most judicious, that it began and ended within the space of two minutes. It was so remarkably calm a quarter after 12, that the exciseman walked through two streets, and turned a corner, with a naked lighted candle in his hand, unmolested and undisturbed by the air; and as soon as over, a perfect calm, but was soon followed by a surprising violent rain.

Of Letters found in the Middle of a Beech. By J. Theod. Klein, Secretary of Dantzick, F. R. S. N° 454, p. 231.

In the year 1727, a beech-tree was felled near Elbing, for the domestic use of John Maurice Mœller, then post-master of Elbing, now secretary of his native city. The trunk being sawed into pieces, one of these, 3 Dantzic feet

6 inches long, cleft in the house, discovered several letters in the wood, about 1 inch and a half from the bark, and near the same distance from the centre of the trunk. Two of these, DB, show their old bark smooth and sound. The wood lying between the letters and the bark of the trunk, as well as that between the letters and the heart of the tree, is likewise solid and sound, bearing not the least trace of letters. The characters BD, being somewhat hollow, receive the bark of the letters DB.

The same letters are seen in the bark of the tree, only that they are partly ill shaped, partly almost effaced; whereas those within bear a due proportion, as if done with a pencil.

It is an ancient custom to cut names, and various characters, on the rinds of trees, especially on such as are smooth. That this has happened to our beech, the mere inspection of the bark sufficiently shows. An incision made, the tubuli conveying the nutritious juice, and the utriculi in which it is prepared, are divided and lacerated, and more of them, as the incision was made deeper and wider: and consequently the sap is not carried on in the circulation, but extravasated and stopped at the wounds. Hence the origin of the characters in the bark and wood.

Now as a new circle of fibres grows yearly on the tree, between the wood and bark, a number of these may, in a process of years, more and more surround the engraved characters, and at length cover them. And this number was the greater in our beech, on account of better than half a century elapsed since the incision, which was made in the year 1672, as appears on the outside of the bark. But while new circles of fibres are successively added, the tunicle or skin of the bark is broken each time, and the utriculi extended and dilated.

M. Klein also mentions several other instances of the same kind, and accounted for in the same manner, as treated of by different authors; viz. Solomon Reisel, John Meyer, Luke Schrœck, John Chrit. Gottwald, John James Scheuchzer, and John Melch. Verdries.

On the Effects of Thunder on Trees, and on a large Deer's Horn found in the Heart of an Oak. By Sir John Clark, one of the Barons of his Majesty's Exchequer in Scotland, and F. R. S. N° 454, p. 235.

Being lately in Cumberland, Sir J. C. there observed three curiosities in Winfield-Park, belonging to the Earl of Thanet. The first was a huge oak, at least 60 feet high, and 4 in diameter, on which the last great thunder had made a very odd impression; for a piece was cut out of the tree, about 3 inches broad, and 2 inches thick, in a straight line from top to bottom. The second was, that in another tree of the same height, the thunder had cut out a piece of the same breadth and thickness, from top to bottom, in a spiral line, making 3 turns about the tree, and entering into the ground above 6 feet deep. The third was the horn of a large deer found in the heart of an oak, which was discovered on cutting down the tree. It was found fixed in the timber with large iron cramps; it seems therefore, that it had at first been fastened on the outside of the tree, which in growing afterwards had inclosed the horn. In the same park Sir John saw a tree 13 feet diameter.

Remarks on the foregoing. By the Editor, Dr. Mortimer. N° 454, p. 236.

This horn of a deer, found in the heart of an oak, and fastened with iron cramps, is one of the most remarkable instances of this kind, it being the largest extraneous body we have any where recorded, thus buried, as it were, in the wood of a tree. If J. Meyer, and J. Pet. Albrech had seen this, they could not have imagined the figures seen by them in Beech-trees to have been the sport of nature, but must have confessed them to have been the sport of an idle hand. To the same cause are to be ascribed those figures of crucifixes, Virgin Marys, &c. found in the heart of trees; as, for example, the figure of a crucifix, which I saw at Maestricht, in the church of the White Nuns of the order of St. Augustin, said to be found in the heart of a walnut-tree, on its being split with lightning. And it being usual in some countries to nail small images of our Saviour on the cross, of Virgin Marys, &c. to trees by the road-side, in forests, and on commons; it would be no greater a miracle to find any of these buried in the wood of the tree, than it was to find the deer's horn so lodged.

Sir Hans Sloane, in his noble museum, has a log of wood brought by Mr. Cunningham from an island in the East-Indies, which, on being split, exhibited these words in Portuguese, DA BOA ORA. i. e. Det [Deus] bonam horam.

On the Eruption of Vesuvius, in May 1737. *By N. M. d'Aragona, Prince of Cassano, and F. R. S.* N° 455, p. 237.

Mount Vesuvius is about 7 miles distant from Naples, and 4 miles from the sea. It rises in the middle of a large plain; and the foot of it begins from the sea-coast, which growing gradually higher, reaches the first plain, to which one can easily ride on horseback. The figure of the plain is nearly circular, being about 5 miles in diameter, and half a mile perpendicular height above the level of the sea. This is the basis of the mountain, out of which arises another, called Monte Vecchio, whose perpendicular height is about 400 paces, and its top little less than 2 miles in circumference, of an irregular figure. The top, before the year 1631, was of the form of a basin, but all surrounded with aged oaks, and vastly large chestnut-trees, the fruit of which afforded food sufficient for a number of cattle. In the bottom, a cavern was observed, into which people descended above 200 paces, by difficult and interrupted paths: this was the ancient mouth, which for a long time had constantly cast up great quantities of bituminous matter, and had at the same time burnt a considerable part of the neighbouring country, cultivated by the inhabitants round the hill.

Concerning the eruptions that have happened heretofore, they are very numerous, as well ancient as modern. Of the first, several are mentioned by Berosus Chaldæus, Pobybius, Strabo, in the time of Augustus, Diodorus and Vitruvius; and in Trajan's reign the name of the mountain became more famous by the death of Pliny. From that time it is thought the eruptions were less frequent, down to the year 1139; when, after a considerable eruption, it continued quiet somewhat less than 5 centuries; so that the horrid remembrance of the past ruins was pretty well obliterated out of the minds of the neighbouring inhabitants; who, vainly flattering themselves with hopes, that the inflammable matter was spent, planted the whole district round the mountain, which, by its fertility, became the delight of these parts. But they found themselves deceived and frustrated in their expectations: for in the year 1631, during 6 months space, continual rumblings were heard, and shocks of earthquakes felt: and afterwards, in the month of December, a dreadful fiery eruption happened, which first blew up part of the mountain into the air, in a terrible manner, and then vomited out water, ashes, stones and fire; inundating almost the whole country around to the sea, and for above 7 miles in breadth, with the dreadful loss of more than 4000 people. After which the mountain became silent, and remained considerably diminished in its height, from what it had been before.

It continued quiet for 29 years; but having rekindled in 1660, its fire filled the whole capacity of the immense hollow, which remained since the year 1631; whence, after several less eruptions, a new mountan appeared in 1685.

In 1707, not only the inhabitants of the neighbourhood, but also the whole city of Naples, were put into great terror, on account of the frequent noise and shocks, the fire seen on the top of the mountain, with a vast quantity of ashes, which issuing out with impetuosity, were dispersed all over our hemisphere, and darkened the light of the sun during one whole day. These were all manifest signs of the impending desolation: and yet this dreadful day, which had portended so much mischief, was beyond expectation, and to our great astonishment, followed by another as pleasant as could be desired: for the air was quite serene, and clear of ashes; and on the mountain there wap no other appearance besides that of a little smoke.

In the year 1724, the quantity of ashes and stones, thrown from the top of the mountain, was so heaped from the bottom up to the edge of the ol mountain, that the whole space from the old hill to the new, appeared but one continued mountain.

In 1730, there was another eruption of Vesuvius, though very inconsiderable in respect of the last.

This present year 1737, to the month of May, the mountain was never quiet: sometimes emitting great quantities of smoke, at other times red-hot stones; which, for want of a sufficient impelling force, fell on the same mountain. In the beginning of May, a smoke only was seen to issue from the open mouth at the top; and from the 16th to the 19th, subterraneous rumbling noises were heard.

On the 19th, fire was seen to burst out in thick black clouds; and the same day there were several loud reports, returning quicker towards the evening: And still more on Sunday night, when there constantly appeared a very great smoke mixed with ashes and stones: and the neighbourhood felt some shocks, like those of a weak earthquake.

On Monday the 20th, at the 13th hour, the mountain made so loud an explosion, that the shock was strongly felt even in the cities 12 miles round. Black smoke, intermixed with ashes, was seen suddenly to rise in vast curling globes; which spread wider, as it moved farther from the basin. The explosions continued very loud and frequent all this day, shooting up very large stones through the thick smoke and ashes, about a mile high, to the horror of the beholders, and the danger of all the neighbouring buildings.

At the 24th hour of the same Monday, May 20, amidst the noise and dreadful shocks, the mountain burst on the first plain, a mile distant obliquely from the summit, and there issued from the new opening a vastly large torrent of fire; whence, by the quantity of fire incessantly thrown up into the air, at a distance all the south side of the mountain seemed in a flame. The liquid torrent flowed out of the new vent, rolling along the plain underneath, which is above a mile long, and near 4 miles broad; and in its way it spread very speedily near a mile wide; and by the 4th hour of the night, it reached the end of the plain, and to the foot of the low hills situate to the south. But as these hills are rugged with rocks, the greater part of the torrent ran down the declivities between these rocks, and into two valleys; falling successively into the other plain, which forms the basis of the mountain; and after uniting there, it divided into 4 lesser torrents, one of which stopped in the middle of the road, a mile and half distant from the Torre del Greco. The second flowed into a large valley. The third ended under the Torre del Greco, near the sea; and the fourth at a small distance from the new mouth.

The torrent, which flowed into the valley, ran as far as between the church of the Carmelites and that of the Souls of Purgatory, by the 8th hour on Tuesday. The matter of the torrent ran like melted lead: in eight hours it advanced 4 miles. The trees, which the torrent found in its way, on the first touch took fire, and fell under the weight of the matter.

The torrent which ran behind the convent of the Carmelites, after setting the little door of the church on fire, entered not only by it, but also through the windows of the vestry, and into two other chambers. In the refectory, it burnt the windows; and even the glass vessels, that stood on the tables, were melted into a paste by the violent heat of the fire. Sixteen days afterwards, the matter continued hot, and was very hard, but it was broken by repeated blows.

A piece of glass fastened on the top of a pole, and thrust into this matter, was in 4 minutes reduced to a paste. Under the mass of the torrent were heard frequent reports, which made the church shake, as if by an earthquake. Along the whole surface of the torrent, there appeared small fissures, out of which issued smoke, that smelled of brimstone, mixed with sea-water; yet these exhalations are not poisonous, but rather a remedy for some diseases. The stones round about these fissures were observed to be covered with sublimed salts.

Iron, thrust into these fissures, was taken out moist; though on thrusting in paper, it was not moistened, but rather somewhat hardened.

At the same time when the new mouth opened, that on the summit of the mountain vomited a vast quantity of burning matter, which, dividing into torrents, and small streams, ran partly towards the Salvadore, and partly towards Ottajano; and at the same time that this matter issued out, red-hot stones were seen to be cast out of the mouth, in the midst of black smoke, frequent flashes of lightning and thunder, all produced by the same matter.

These impetuous expulsions of fire continued till Tuesday, when the eruption of the melted matter, the flashes, and thundering noise, ceased; but a strong south-west wind arising, the ashes were carried in great quantities to the utmost boundaries of the kingdom; in some places very fine, in others as coarse as Ischian sand: and in the neighbourhood they not only felt this plentiful shower of ashes, but likewise pieces of pumice-stones, and other large stones.

Tuesday night the fury of the mountain began to abate, so that on Sunday there was scarcely any flame seen to break out of the upper mouth; and on

Monday but little smoke and ashes. This day it began to rain plentifully, which continued to Tuesday, and afterwards for many days: a circumstance which has constantly happened after the eruptions of times past.

The damages done in the neighbourhood by this eruption of fire and ashes, are incredible. At Ottajano, between 4½ and 5 miles from Vesuvius, the ashes on the ground were 4 palms high. All the trees were burnt, or blasted, the people terribly affrighted, and many houses crushed by the weight of the ashes and stones that fell.

After the description of this fiery eruption, the Academy of Sciences at Naples made an accurate analysis of the matter, and of the salts, that were collected in great plenty near the fissures; and, towards the discovery of the truth, they made the following experiments:

Exper. 1.—Some of the stones of Vesuvius being pounded small, and the loadstone applied to the powder, some few particles were attracted by it; and the same powder, put into aquafortis, caused a sensible effervescence; whence it certainly contains no small quantity of iron: which was also found on trial in another eruption by Tomaso Cornelio. But for the greater elucidation of truth, one of these stones being applied to the magnetic needle, it turned to the stone; and then carrying it round to the opposite end of the needle, it immediately turned from it, in the same manner as if iron was applied near the compass.

Exper. 2.—The stones are not all of the same density or colour; but various, and of different ponderosity. Some are composed of real talc, others full of marcasites: some are almost all sulphureous, others nitrose; some of a grey colour, others red.

Exper. 3.—The matter of the current is spongy at top, but very dense towards the bottom; which is a proof of its fusibility; the heavier bodies subsiding, and the lighter remaining at top.

Exper. 4.—After growing hard, it retained part of the heat above a month, though unequally: for in the interior parts, where the air had no free access, and the matter was more compact, the heat was much stronger, than towards the surface.

Exper. 5.—Twenty days after the eruption, in divers parts of the mountain, from the bottom to the top, there were seen to arise many pernicious damps, [mofete] especially from the cavities, and the fissures of former torrents; as also on the plain: but none were observed in the matter of this last eruption. They issued out of the fissures under the appearance of a cold wind, and rose about 3 palms high; then they moved along the surface of the ground, and, after a progress of some paces, disappeared. Animals, which happened to graze where these passed, were all killed by it; and likewise a Teresian friar, who inadvertently breathed the vapour of one of these damps.

Exper. 6.—Having placed the barometer in the vapour, it underwent no change, but the thermometer fell somewhat more or less. A lighted torch, thrust into them at two palms from the ground, was soon extinguished by the action of the damp.

Exper. 7.—These damps grew gradually weaker in their pernicious effects, for above 3 months, even to the subsequent autumn; as has been generally found in other former eruptions, or when they happened to issue out of their vents.

Exper 8.—Concerning the salts which are generated in abundance in Vesuvius, I have, by order of the Academy, examined them by accurate experiments, My intention was to know, if besides sal ammoniac, there were also sea-salt, vitriol, nitre, or any other salt. I thought there was no better way of proceeding in this inquiry, than by crystallization; because it is universally allowed, that salts in crystallizing constantly retain one certain and determinate figure; sea-salt concreting into cubes, vitriolic salt into rhomboidal parallepipeds, alum into octædrons, and nitre into rectangular prisms on hexagonal bases. I imagined, that if the salt of Vesuvius happened to contain any particles of the salts abovementioned, it would discover them after crystallization. This way of reasoning was confirmed by experiment: for the Vesuvian salt, in crystallizing, left on the sides of the vessels small parcels of crystallized salts, which, observed through a microscope, resembled a tree with its branches, on the ends of which there appeared several pyramids of an irregular figure, but very sharp-pointed; and between the branches there were interspersed in some places a group of prisms, in others some small cubes: whence I inferred, that the salt was ammoniacal, and indeed a genuine and efficacious sal ammoniac, with insensible portions of nitre and sea-salt. Which coincides with the sentiments of the Royal Academy of Paris in 1705; with those of Thomas Cornelius in his Progymnasma de Sensibus; of Dominicus Gulielmini in his Treatise de Salibus; of Dr. Boerhaave in his chemistry, and many other writers.

Exper. 9.—In order to be convinced whether this salt was really ammoniacal, and of the nature of neutral salts, I mixed it with spirit of vitriol, and spirit of salt, without producing the least fermentation. I afterwards put some of it into oil of tartar per deliquium, but could not perceive any ebullition; so that it is to be ranked among the neutral salts.

Exper. 10.—Thrown upon red coals, it did not crepitate like sea-salt, but it boiled and swelled, and after evaporating it dried up.

Exper. 11.—It is of a very pungent taste, strongly pricking the tongue, and of a bituminous smell of brimstone, which occasions a violent head-ach by its volatile texture.

Exper. 12.—The salts taken from different stones are not all of the same weight or colour: for some are yellow and unctuous, as if rubbed all round with petroleum: others are very white, others blackish, and others of other colours, according to the stones they adhere to.

Exper. 13.—I have likewise found by experience, that the sal ammoniac of Vesuvius is much more efficacious, than any other salt known at this day, in cooling liquors. On dissolving some of it in water, it makes the water so cold, that the sides of the vessel which contains it, can hardly be touched without uneasiness, through the excessive cold.

Exper. 14.—Mons. Geoffroy, a celebrated member of the Academy of Sciences, thinks it a singular power of common sal ammoniac, that being mixed with a certain quantity of water, it rendered the water so cold, that it made the spirit of his thermometer, 18 inches high, fall 33 lines. But the Vesuvian salt makes the liquor of a thermometer, like his, fall $4\frac{1}{2}$ inches; which is equal to 54 lines. So that the efficacy of this salt, in causing the fall of the liquor, exceeds the efficacy of common sal ammoniac by 21 lines.

Exper. 15.—If round a vessel full of water cooled with snow, there be put some of the salt of Vesuvius, the water freezes and grows hard in a very little time.

Exper. 16.—If a good quantity of the salt of Vesuvius be put into snow, set round a glass vessel full of water, and then stir the vessel, the contained water becomes unfit to drink; having acquired a very disagreeable acrid sulphureous taste; a manifest sign, that the salt is divided into small particles, which passing through the insensible pores of the glass, enter into and mix with the water.

Exper. 17.—Of all kinds of salts, this dissolves in the greatest quantity in water; and perhaps the greater or less solubility of a salt in water, will be found proportional to its greater or less effect in cooling water.

Exper. 18.—Being put into brandy, or oil, besides that very little of it is dissolved, it occasions no descent of the liquor in the thermometer.

Exper. 19.—Being mixed with blood lately drawn from the vein of a man, but coagulated after settling, the blood was dissolved, and continued in that state for the space of 24 hours.

Exper. 20.—A solution of this salt being injected into the vein of a dog, first occasioned tremors, then universal convulsions, and lastly death. And 4 hours afterwards, having opened the dog, the blood, which should have been coagulated, was found fluid, both in the trunks of the veins, and at the ends of the arteries.

Exper. 21.—It has all the properties of sal ammoniac to that degree, that on substituting this Vesuvian salt, instead of common sal ammoniac, the strongest sort of aqua regia may be had for dissolving gold; which experiment was made with success by Mons. Lemery, in the academy of France.

Exper. 22.—If a lump of the mineral matter be reduced to a fine powder, and attentively viewed through a microscope, it appears very like the sand of Ischia, and is very proper for writing-sand. Hence probably that sand is nothing else but the same matter for a long time comminuted by the action of the sea.

Exper. 23.—In some of the stones there appear some few veins of gold, in others of silver, but insensible; and in others, which are very heavy, there is some antimony.

Exper. 24. A great dispute arose in the academy on the rise of the *(Mofete)* damps; for what reason these should be seen only in the old strata of the mineral substances, and not in the new, where by the action of the fire they ought to issue; which phenomenon, if I am not mistaken, may be accounted for in this manner. As the cooling of the burning matter began at the surface, we may think, that the more subtle heterogeneous particles, on the closing of the pores at the surface, remained in quantities buried in the lower parts of the matter; which, in process of time, becoming acutangular and of deleterious figures, yet cannot offend while imprisoned. But in new eruptions, when the shocks given to the matter produce many fissures, the damps, meeting with less resistance there, issue forth. As when the air is a long time pent up in some hollow, on giving it vent, it generally comes out in a pernicious vapour.

Exper. 25. It was observed, that the greatest shocks happened to such things as stood exposed to the volcano; but that those things which were not thus exposed to it, received but faint shocks: a manifest sign, that the vibration of the air had a great share in the shocks of the earth: which circumstance is taken notice of by Borelli with respect to Mount Etna.

An Abstract of a letter from an English Gentleman at Naples to his Friend in London, containing an Account of the Eruption of Mount Vesuvius, May 18, *and the following Days*, 1737, *N. S.* N° 455, p. 252.

I was lodged for some time at Chaja, and afterwards at Fontina Medina, in the face of this surprising mountain, and at 2 or 3 miles distance.

By all accounts, there has not been any eruption remembered near so violent, nor so furious; and authors mention none to this degree later than above 100 years since. On Friday, May 17, 1737, N.S. I observed, as far as I could see round, that the mountain was covered with white ashes a great way down, as it has been with snow in the winter. Pliny observes in these words: "Præcesserat per multos dies terræmotus minus formidolosus, qui Campaniæ non solum castella, verum etiam oppida vexare solitus." (Plin. lib. vi. ep. 20.) Other authors say the contrary; though it may very likely be so, round and near the foot of the mountain; but this time I have not found any body sensible of it here; but it is certainly true, that our windows and doors shook all the time of the violence of the eruption, which I take to be from the very great concussion of the air on the violent explosions.

On Saturday night, May 18, this great phenomenon began, and increased so much on Sunday, that it brought half the people out to gaze at it. There were certainly, among some, great apprehensions, by their being employed in processions, visiting their churches, and exposing their images of the Virgin Mary.

I very boldly set out on Monday, about 2 hours before sun-set. It was a melancholy sight, to see the road full of numbers of poor wretches, flying as from Sodom. I stopped on the way, to observe the vast clouds of smoke thrown up in a prodigious column, to an amazing height, which, by its gentle waving and undulation, was a most beautiful sight; and when it had mounted so high, that it had lost the force of the protrusion, it was carried by the wind a vast way; but not too far for one to observe how its rolls began to break, and, being dispersed and expanded, covered the country underneath with ashes and darkness. Many great flashes of lightning were darted through this pillar of smoke, and frequent discharges as of cannon or bombs, which were followed by falling stars, such as we see from well-made rockets. We turned off out of Portici, to gain the north-side of the mountain, as far as we could, in chaises, till we were forced to get upon asses or mules.

It was now growing dark, and the fire began to be visible, which it was not in the day-time, the sun bearing no rival.

In a little time, by the light of the mountain, though that was much obscured by the clouds and pillar of smoke, and the help of our torches, we scrambled over very rough roads, till we got within about a quarter of a mile of the great lava or current. But then we halted, as the scene on all sides became so stupendous and terrible.

We returned to Portici, where we supped, and got home, much fatigued, by 2 in the morning. The fury of this eruption was at its height this night, as to burning; but the next day, Tuesday, the columns and bouïllons of smoke were as great, and thrown out with as much violence, which, as the wind sat, carried its destruction, not of the large massy metallic bodies, but of infinite quantities of ashes and cinders, all that day, and part of the night. Through the columns of smoke was a continued lightning, the most beautiful sight imaginable.

The following day, Wednesday, we set out again, to view the west-side of the mountain at Torre del Greco, 8 miles from hence; where the great lava had stopped at the church of the Carmelites, but not without carrying part of it away. This lava had, from the declivity, taken the water-course, which was the preservation of the country from being drowned. This hollow, which was for some miles between 30 and 40 feet deep, and as many wide, was not only filled up, but the matter rose as many feet above the surface of the land about it. We walked to view it on one side, but the heat was so intense, and the sulphureous stench so suffocating, that we were obliged to keep at a good distance; and I was well informed by several, that it continued very hot 4 or 5 weeks after; so long in cooling is that great quantity of bituminous and metallic matter, with which this vomes is loaded.

As the fury of the expulsion and explosion was much abated on Tuesday morning, the stop here was about 4 o'clock that day in the afternoon; which might be the more easily conceived, when no more of this vast metallic matter was discharged, and the motion of all the rest was relented, for want of more protrusion, and the bitumen growing a little cooler. As this stop was made at the church, part of the lava took a turn into the great large road to Salerno, to a great height; which part is choaked up for ever, the expence being immense to remove it.

Some persons say, that the matter discharged this time in the different currents or lavas round about, would make a mountain as large as their sire. The Carmelites here soon fled, and were not come back 10 days afterwards, when we returned that way, to visit the south-east side, to view the great devastation which was made about Ottajano, 18 miles from hence; for though the great discharge of the metallic body ceased on Tuesday, a vast destruction of the country followed for a long time after; for as the force of the explosion was very great, it continued to throw out vast showers of cinders and ashes. The lands indeed, where the lavas fall, are annihilated to the owners; and the other materials destroy all the fruit and produce of the earth where they fall, which does not recover for a long time.

As we turned on the left from Torre del Greco towards Ottajano, we passed all the way through their masserias (farms); and the mountain, being to the windward of us for 3 or 4 miles, showered ashes plentifully upon us, and we lost our smell of every thing but brimstone. All the trees, vines, and hedges, bent under the weight of these ashes; several arms, and even bodies of trees, were broken with the weight; so that in some narrow roads we had difficulty to pass. Within a mile or two of the prince of Ottajano's palace, one can scarcely frame to one's self a sight of greater desolation; 10 successive northern winters could not have left it in a worse condition; not a leaf on a tree, vine, or hedge, to be seen all the way we went, and some miles further, as we were informed. Here, and at the town, they had a new earth, about 2 feet deep, some said more, by the account of the miserable inhabitants, who were a dismal spectacle. The storm fell so thick and heavy for that time, that they almost all fled, and many houses were beaten down. In one convent, two or three nuns were buried in the ruins. At Somma, on the north-east side, it has made great havock; a monastery of nuns was destroyed. After a long day's work, we returned at six o'clock.

Of the Lunar Atmosphere. By M. Jean Paul Grandjean de Fouchy, of the Royal Academy of Sciences at Paris. N° 455, p. 261. *Translated from the Latin.*

By an atmosphere is meant a certain assemblage of pellucid matter surrounding the planet, and capable of turning the rays of light, that pass through it, from a right lined course. M. F. does not here inquire what may be an atmosphere, in other respects, different from that of a refracting medium, but only undertakes to prove, that the moon is not enveloped by any thing capable of refracting the rays of light. He conceives an atmosphere, in this inquiry, to be a homogeneous fluid, with a spherical surface, of a uniform density, which is equal to the sum of the decreasing densities in the real atmosphere; purposely omitting the difference of density in the parts, as not disturbing the demonstrations.

Now if the moon be encompassed with an atmosphere, its diameter ought to be found greater than in the naked planet. And that the quantity of the increase may be known, let AIB, fig. 3, pl. 8, be the body of the moon, FEG its atmosphere; then the angle AHL will be that of the real diameter of the moon, and the angle EHL, made by the axis LH, and the ray AEH, will be the observed diameter: so that the angle EHA will be the increase of the moon's diameter by the atmosphere. But the angle EHA is opposite to the side EA, of the triangle EHA; and the angle AEH, the supplement to 180° of the horizontal refraction in the lunar atmosphere, is opposite to the side A H, the moon's distance from the earth. Also the side E A is the half of a chord of the lunar atmosphere touching the moon's body at A; therefore the sine of the increase E H A, will be to the sine of the horizontal refraction, as the semichord A E is to A H, the moon's distance.

Hence it follows, that the increase of the moon's diameter is insensible; for, if it amounted to $2\frac{1}{7}''$, supposing the horizontal refraction $5'$, i. e. at least 30 times greater than it can be supposed, as will be proved hereafter, then the semichord E A would be equal to 276 French leagues, and thus far exceed a like chord of the terrestrial atmosphere. Therefore, whether the moon is covered with an atmosphere or not, her diameter will always be observed the same: so that the observation of the lunar diameter can never be sufficient for resolving the problem.

But the solar eclipses afford better means for deciding the point; for the extreme rays bounding the cone of the lunar shadow, as they touch the moon's body, and pass through her atmosphere, will be necessarily inflected toward the axis of the cone; hence the cone will become shorter and more obtuse. But to know the quantity of that variation, it must be observed, that the ray F A, fig. 4, or its parallel E G, which in case there be no atmosphere, would be the limit of the lunar shade F A C, would be refracted towards the axis C A, at the ingress of the atmosphere G, and at the egress H: hence the semi-angle of the cone of the lunar shade will be increased by double the horizontal refraction in the lunar atmosphere.

Hence it follows, that in the supposition of a lunar atmosphere, a total eclipse of the sun will begin later, and end sooner, than without one; also, that in some certain cases, there would be no total eclipse; which, yet, the diameters of the sun and moon, observed in the same degree of anomaly, would require; for in these cases the cone of the lunar shade might be so constructed, as not to reach the earth's surface.

In the same manner, the duration and quantity of partial eclipses would be also diminished; and they would thus begin later, and end sooner, on the supposition of a lunar atmosphere, than without one; and in certain cases, there would be no eclipse at all, when there would otherwise be one.

Such then would be the phænomena in case there were an atmosphere about the moon. Let us now consider what is really observed. In the first place, as the axis of the lunar shade extends to 55 semidiameters of the earth, when greatest, and to $52\frac{1}{3}$ when least; and as the least distance of the moon from the earth is 54 of the earth's semidiameters; if the lunar atmosphere were capable of a horizontal refraction of $8''$, the semi-angle of the conical shadow would be increased by double the quantity, that is $16''$; therefore it would be equal to 16′41″ when most open, and to 16′5″ when narrowest. Also, the least semi-angle of the cone being supposed 16′ 5″, its axis will be less than the least distance of the moon from the earth, or 54 terrestrial diameters; and therefore the point of the lunar shade will not extend to the earth. So that, if there were an atmosphere about the moon, in which the horizontal refraction is 8″, there would be no total eclipse on the earth. There is, therefore, either no atmosphere about the moon, or else it produces a horizontal refraction less than 8″.

But certain total eclipses of the sun are observed with some duration of the total darkness. For instance, in the eclipse of 1724, the duration of total darkness amounted to $2^m\ 16^s$. The moon, at that time, ran over 1′15″ in her horary motion, and her shadow, with a parallel motion, a space 54 times greater on the earth's disk, that is, equal to 1° 7′ 30″; from which, if there be deducted the diurnal motion of a habitation, by which the duration of the eclipse can be prolonged, it gives the diameter of the shadow equal to 47′30″, or 45173 toises, or 22 Paris leagues. Hence, by calculation, it is found, that the axis of the cone of the lunar shade is greater by at least one diameter of the earth, than the moon's distance from it, which was then the least, the moon being then about the perigeum. Further, from the given diameters of the luminaries, observed in the same degree of anomaly, the axis of the cone of the lunar shade is found at least equal to 55 semidiameters: hence it follows, that the spot of the lunar shade on the earth's disk, and the axis of the cone, are found to be exactly the same, as the distances of the moon and the observed diameters of the luminaries seem to require. There is therefore no atmosphere about the moon, or, if there is any, it produces no sensible refraction.

But to leave no room for doubt, we may consider the reason for those phænomena, observed in solar eclipses, which have given cause to suspect a lunar atmosphere.

And first, that very faint light, observed in total eclipses, is no proof of any refraction in a fluid about the moon; for, by Maraldi's experiments, successfully repeated by M. Fouchy, it appears that the shadows of bodies that have no atmosphere, when they are exposed to the sun, are bright about the axis of the cone; and the more so as it is farther from the body itself. And the situation of an observer, in a total eclipse, is about the axis of the cone of the lunar shade, and near its vertex. It is no wonder therefore, that the middle of the shadow exhibits a kind of gloomy light, which may also be augmented by the rays being reflected by an illuminated air surrounding the shadow about the middle.

2dly. The lucid annulus about the moon, in total eclipses, by no means proves the existence of a lunar atmosphere, as will be plain to any one who hides the sun from him by balls of wood, or other opaque matter. Hence this is not to be ascribed to a lunar, but to a solar atmosphere; as has been proved by M. Mairan, in his treatise on the Aurora Borealis, p. 14.

3dly. The diminution of the lunar diameter, which in solar eclipses is observed to be about 30′ less than when the moon shines with a full orb, in the same degree of anomaly, by no means proves a lunar atmosphere; though some inequalities of mountains are observed in the circumference of the moon's disk, which quite disappear in the full moon; for lucid objects strike the fibres of the eye so strongly, that their motion is communicated to the neighbouring fibres, and so the image of the lucid body is increased beyond the due quantity, as is known by common experience: for if a stick be placed between the eye and the moon, the diameter of the interposed stick will seem to be diminished; but if at that time any cloud pass before the moon, the diameter of the stick will appear less diminished; and if the moon be quite obscured, there will be no diminution at all; and lastly, the diminution will vary according to the various intensity of the moon's light.

As for the inequalities of the mountains, they are, for the same reason, observed least in the full moon; for the lunar mountains, obscure of themselves, and seen in the bright orb of the sun, escape the eye much less, than when, shining in the full moon, they are extinguished in the splendour of that luminary; especially as the lunar light is so intense, that a star of the 3d magnitude can hardly be seen, when near it. But, to remove all doubt, if the limb of the moon, when in opposition to the sun, were the bound of an atmosphere, and not of her very body itself, the mountains in her circumference would never be observed by the longer telescopes with narrow objective apertures. Whereas M. Fouchy had often observed several inequalities of mountains in the disk of the full moon, with a telescope of 36 Paris feet, and an objective aperture of 1 inch: hence it follows that the disk of the full moon is bounded by the periphery of her body, and not of her atmosphere.

4thly. We must not omit to speak of that remarkable observation, in 1715, of the lunar coruscations, made by M. Delouville, in the presence of many astronomers of the Royal Society, and seen by Dr. Halley, N° 343, vol. vi, p. 158 of these Abridgments. We may suppose that the moon's visible limb is composed of the tops of mountains; which, in a total eclipse, hide the sun from an observer, in the same manner as the trees in great woods obstruct the sight. Hence if some rows of mountains, on the moon's surface, afford a free and direct passage for the solar rays, they must imitate a kind of coruscations, like as when, in a camera obscura, a ray of the sun is suddenly admitted by means of a speculum, and the picture of the external objects, drawn on the focus of the lens, is taken away, it will be enlightened with luminous traces, much resembling lightning.

From all these it is manifest, that there is nothing like a lunar atmosphere in solar eclipses. We may now speak of the occultations of the stars and planets by the moon.

If the moon be surrounded by an atmosphere. the planets and fixed stars must be seen, by an observer on the earth, to immerge behind the moon later, and to emerge from it sooner, than if she have no atmosphere; and even in some places where such occultations ought to appear, there would be none. To make this plain, let ABC, fig. 5, be the moon's body, and S a star at immense, or as at an infinite distance; then the parallel rays LV, MX, touching the moon's body on all sides, inclose a cylindric space, of which the base VZX includes all the parts of the earth's surface to which the star or planet is occulted by the moon. The observer therefore will see the beginning of the eclipse at V, and the end at X, and will measure the duration of time in which the moon may run through her diameter, or an equal space. But if we suppose an atmosphere of the moon, the ray IW will not remain parallel to the axis of the cylinder, but this will now become a cone, of which the section YTU will mark the earth's surface where the eclipse will be seen. Now the base YTU being contracted, the point Y will come on any part later than the point V, and the point U will quit it sooner than X; therefore the occultation will begin later, and end sooner, by supposing an atmosphere about the moon, than the contrary; and there will be no occultation in some places, where it ought to be observed without an atmosphere; for the place C, being included in the base VZX of the former cylinder, is without the conical section YTU. Besides, supposing the horizontal refraction in the lunar atmosphere equal to 8″, then VY will be 1384 toises, or a quarter of a Paris league; and hence it follows, that no eclipse must be observed in places according to calculation, whenever they are without a circle of a quarter of a league radius.

Another phenomenon also takes place on the supposition of a lunar atmosphere: in the part of the cylinder YR the star indeed will always be seen, but it will be through that medium: hence it will acquire a motion and colour different from the genuine, and that in all eclipses whatever, whether the star be one of the largest or smallest.

Besides, the duration of such occultations does not seem at all diminished, but is always found to be exactly agreeable to the moon's diameter and motion. As to those observations in which the star, after the contact, is seen to proceed a little in the moon's disk before the occultation, the whole cause of them may be referred to the increased diameters of the moon and star; for if the lunar atmosphere were the cause of this appearance, it would always be observed the same in all stars, and in any apertures of objectives. Besides, there has not as yet been observed the progression of any star in the moon's disk, unless it be of the first, or perhaps of the second magnitude, and that by only the half of it at most, and it is well known, that their whole diameter is insensible, and is only increased by spurious rays; whence the adventitious rays, both of the moon and star, are mixed in the bottom of the eye, before the true conjunction of their bodies; and if the visible limb of the moon were the limit of the atmosphere and not of the body, no mountains would be observed on its periphery with the larger tubes and narrower objective apertures; which, however, as beforesaid, are seen plainly enough.

From all these then it is manifest, that the moon is not surrounded with a refracting atmosphere, or one capable of being observed; though there might be one producing a horizontal refraction of 1″ or 2″; and this seems to be countenanced by the larger spots in the moon, which cannot by any means be taken for woods, as Hartsocker and some others have imagined; for the shadows of the edges are always observed nearer to the bright limb of the moon; whence it is rightly concluded that they are cavities, and not woods, as these would project a shadow from the other side. Further, some fluid may well be supposed to be in them; in which case it would be very agreeable to philosophy, that they should emit some vapours, the congeries of which would represent a kind of atmosphere; which would not be found very dense, since, by Sir I. Newton's demonstrations, it could hardly equal a third of that of the terrestrial vapours, nor be observed alike at different times, those vapours being destitute of any other addition.

An extraordinary sinking down and sliding away of some Ground at Pardines near Auvergne. By M. T.—— Communicated by Phil. Henry Zollman, Esq. F.R.S. N° 455, p. 272.

The village called Pardines, was composed of 46 buildings; the ground on which it was built, as well as that of the whole hill, is a good and light earth, mixed with a little white clay; there are also in it some stones and rocks of a middling size. This land was very well cultivated, and very fruitful, consisting of fields sowed with corn, of orchards, but for the most part of vineyards; the whole ground was overspread with fruit-trees, particularly walnut-trees. This earth used to dry soon, and chap from the heat; some clefts of a considerable depth, which growing wider and wider, often formed several gullies.

On the 23d of June 1733, about 9 in the evening, the inhabitants of the village saw the walls of their houses shake sensibly; on which they all withdrew out of them, and saw that the hill visibly melted away, as it were, the greater part of the land sliding along towards the vale; other parts subsided sensibly; in some places the earth, opening, formed new gulls, and those that were there before, grew much wider; sometimes the ground which slid along in large pieces, stopped, and tumbled one piece over another; and the rocks, which broke loose from that rolling earth, precipitated themselves into the valley, which became quite filled up with them, as well as with the earth which rolled down, by which the neighbouring road became impassable.

All this was done very gently, and even sometimes almost imperceptibly; a sensible motion was observed during the space of 3 or 4 days at different times; one house even did not fall till the 10th of July. During all that time no noise was heard, any otherwise than what proceeded from the rocks falling into the valley, and from some large masses of earth, which loosening themselves from the steeper parts, fell down with precipitation. By this rolling were carried away 26 buildings, some of which subsided with the ground, and, being shaken at their foundations, tumbled on a heap; the remains of some others appear yet, on those pieces of ground that rolled down into the valley. It is computed, that the ground which slid away, or was lost by being buried under the rubbish of the others, amount to the number of 150 acres of Paris measure. It is observable, that in this number were comprised several orchards, besides that the whole ground was covered with trees, either walnut-trees on the hill, or willows and poplars in the valley, about 4000 in all.

If one may conjecture what was the cause of so dismal an accident, it seems it proceeded from the situation of the ground, and the nature of the soil. The first surface of the hill, about 4 or 5 feet deep, was a pretty light earth, easily dried by the heat of the sun; under this first layer there was a stratum of fat clay, which at present lies open in several places, and which is very moist, so that the water is seen bubbling out of it in some places.

The great rains that fell in the beginning of the spring, soaked through and diluted this stratum of clay, which retained and gathered all the waters of the hill running between the two layers; the heat of the summer ensued, which dried up the upper surface, and formed it into a sort of solid crust, which resting on a fat and moist clay, and by its steep situation being inclined to slide towards the valley, its whole surface loosened itself by great pieces, and breaking in several places, slid along towards the place whither its declivity would naturally carry it. There are some parts which moved almost insensibly, and only sunk or subsided, either because the rolling of the neighbouring soils made room, that what was under this surface might slide off, or perhaps because the parts under this surface had been hollowed a long while before, by the waters which passed between this surface and the stratum of fat clay. Other parts, which were much more in number, rolled all together towards the valley, and whole pieces of vineyards are still seen, with the props remaining upright. There are again other parts, which in tumbling were overturned in different manners.

This accident is not without example in the province of Auvergne: there has not indeed been so considerable a one before, yet it has often happened, that pieces of earth, of a quarter or half an acre, have separated all in one piece, from the top of a hill, and slid down visibly on the lands below.

On the Worms which destroy the Piles on the Coasts of Holland and Zealand. By Job Baster,† M. D. F. R. S.* N° 455, p. 276.

In the year 1730, the persons appointed to take care of the dykes on our coasts, observed that the piles made of the hardest oak, defending the coasts of the Netherlands against the sea, were eaten through in a few months, so as to be broken by the least external force. Surprised at this uncommon and dangerous phenomenon, they inquired into its cause, and saw that a kind of worms, before that time very scarce, but now increased to an incredible number, had in so short a time eaten into those piles, between the highest and lowest water-marks, and threatened very great damage to the inhabitants of these countries.

If a pile of the hardest oak has stood 6 months on the shore, and be taken out in summer or autumn, there appears mud and filth sticking to its outer surface; which being scraped off with a knife, discovers a vast number of holes, hardly so large as pins heads.

Viewing this mud through a microscope, there are seen, 1. A number of whitish points, not larger than grains of sand.—2. Some very small worms.

The whitish points seem to be the eggs of this insect, and the worms to be such as are already hatched from them; and these worms gradually perforating

* The animal here described is the *teredo navalis* of Linnæus.

† Job Baster, an ingenious Dutch physician and naturalist, distinguished himself by a controversy with the celebrated Mr. Ellis, relative to the animal nature of the corallines, which Dr. Baster considered as rather the habitations of the inclosed polypes than as forming a constituent part of the animal. His chief work however is his *Opuscula Subseciva,* containing observations on various marine animals. These interesting observations were continued, in distinct parts, from the year 1759 to 1765, and are illustrated by very instructive plates. They relate to the propagation and ovaria of shell-fish in general; the ostrea, mytuli, pholades, and tellinæ; and the structure of several species of the testacea is explained in a satisfactory manner.

the outer surface of the wood, rendered soft by lying in the water, made the aforesaid holes, and through them worked their way into the substance of the wood.

A small style of whalebone or lead, thrust into these small holes, runs straight into them for 3 or 4 lines, so that its outer end always makes a right angle with the pile: but afterwards, if the style be gently pushed forward, it does not continue in the straight line, but runs either way, generally upward.

But if one of these piles be split lengthwise, with a hatchet or wedge, it is found full of passages, or hollow cylindrical ducts, each of which contains a worm, surrounded with a thin testaceous substance, exactly filling the duct, and forming its involucrum or sheath, in which it can move with freedom.

The ducts, beginning at the outer surface by a narrow hole, grow gradually wider, and run either straight, oblique, upward or downward. But what is most surprising, these ducts never run into each other, nor communicate; but each continues separate for every single worm. Over the worm's head there are 2 or 3 drops of a salt liquor, thicker than water, but not the least appearance of the dust of the corroded wood.

Whence it appears, that all the wood, which had before filled up the place of the duct, in which the worm with its covering is now found, was eaten and consumed by the worm: and as it seems quite incredible, that an animal, which appears soft, and almost as fluid as the white of an egg, should be able to eat through such hard wood; the description of this xylophagous worm is offered to the Royal Society, in order to give them some knowledge of this water-insect, which has done so many millions damage to these countries.

They are found of various sizes and thickness. There are some of the younger ones not above an inch or two in length; some of a middle size, such as represented in figures 6 and 7, pl. 8; and some 13 or 14 inches long.

But in order to a more accurate description, we will divide the animal into head, body, and tail.

The head is of a most wonderful structure, being covered with two hard shells or hemicrania, of a substance neither testaceous nor osseous, securing their softer contents: and being viewed through a microscope, they appear as in fig. 8, as well as they could be drawn. These hemicrania are two white bodies, much harder than the substance which forms the testaceous covering; the inner surface hollow and smooth; the outer, convex and rough, with 3 fibres running different ways; and both together perfectly represent a double bit, of that kind of borer, called an auger.

The upper part of the external convex surface, A, fig. 8, has a very sharp edge, in which the first series of fibres begins from a point; which fibres gradually dilating, and running lengthwise, end about the middle part of it, which makes a right angle with the upper part. In this part the fibres being elevated, run crosswise, B, fig. 8. The lower part is thicker than the upper, but softer and less compact. In this part the fibres are raised up and rough, first curved, then straight, and, like the others, run lengthwise to the lower edge of this part, which is strongly fastened to the head by various ligaments C, D.

The concave or inner part of these hemicrania, which contains the softer parts of the head, is very smooth; but almost in the middle has a very small and tender eminence or process, in shape much like Dr. Rau's process in the organ of hearing, fixed at one end, and loose at the other, running almost the whole width, and doubtless destined for supporting some of the inner parts of the head. See A, fig. 9.

These two hemicrania, connected together by strong ligaments, and as it were by a small hinge, by means of which they can dilate without separating, besides their defending the soft head from external injuries, are the instruments with which the animal gets its food. For whatever way it turns its head, the raised and rough fibres, running either lengthwise or crosswise, always rub off some of the wood.

These hemicrania being carefully removed, the contained parts, fig. 11, are laid open to view; but they are so soft, and of so wonderful a structure, that the eye, though armed with a microscope, can neither discern their true make nor use. First indeed there appears a membrane, enveloping the whole head; in the middle and anterior part, which is not covered by the said hemicrania, it appears as if raised by a tubercle C, and in that place it is of a red colour; but the lower ligamentous edge firmly adheres, both to the small process, and to the lower edge of the hemicranium.

This membrane carefully separated and removed, A, A, fig. 12, in the middle of the subjacent pulp is a small pear-like body, perfectly pellucid, somewhat protuberant above the other parts, which made the tubercle in the membrane. It is much harder than the other contained parts of the head and body; so that it will bear cutting with the scalpel. It is of a red colour, as perfectly pellucid as a drop of water; of the shape of a pear, from a larger basis terminating in a point. It cannot be better resembled to any thing, than to the crystalline lens of the eye: yet in spirit of wine it preserved its transparency, but its bulk was diminished, as B, fig. 12.

At the sides, where the lower edges of the hemicrania do not touch each other, there is a sort of cavity; and in these sides the harder fibres may be distinguished, disposed in such a manner, as perfectly to resemble the gills of fish: and through them the worm seems to breathe.

The extreme softness of the other parts of the head prevents our coming at the knowledge of the use of the membranes, furnished with fibres of different tendencies, or inquiring by what organs the worm takes the wood shaved off by the hemicrania, or rough shells; whether it does this by suction, or not; by what muscles, or how acting, this wonderful head is moved. It is probable indeed, that its motion consists in the opening and closing these shells, that shave off the wood; and that the inner parts have a power to move on all sides, as the ball does in the socket of the eye; and perhaps to come forth of these shells, and re-enter, after taking their food. But of these things there can be no certainty, because the parts dissolve between the fingers.

The body, viewed forward, fig. 7, is of a reddish colour. In the middle appears a line, often dark-brown, often blackish, sometimes not visible, sometimes running near half the length. The rest of the animal is of a whitish or grey colour. 1. If you intend to dissect it, and examine the inside, you must first remove a thin membrane surrounding the whole body, which for that reason may be called the cutis or cuticula. When this is removed, there appears an oblong vessel placed in the middle, of a reddish colour, from the shaved wood, of which it is full: hence it seems to be the stomach, or at least the first organ of digestion. 2. In the lower part you will find another vessel, appearing like a dark-brown line, which contains the excrements, of which it is often found full, and discharges them at the end of the tail. 3. At the sides of the reddish vessel, or stomach, is placed a white, clammy, fat substance, sticking to the fingers, and perhaps constituting the flesh of the animal.

Where the body ends, the tail begins, thicker than the body, and rendered stronger by circular fibres. At its end it has two small hard bodies, containing and defending the tender extremities of the tail. This tail, thicker than the body, terminates in two ends, the thickest of which certainly serves for the discharge of the excrements, the slenderest doubtless for generation: and this it can stretch out to an incredible length, so that in worms that seemed to be in copulation, it appeared above an inch out of the pile. The two small bodies, that contain these ends of the tail, are of a harder substance than even the hemicrania. The outer part is gibbous, the inner hollowed. The lower end is bifid; whence it is conjectured, that they serve the animal for feet, when it is mounting upright, or corroding the wood; by leaning on them as on a prop. fig. 13.

The above-described worm dwells now very securely in a testaceous tube, of a white colour, which it exactly fills, yet so as to be able to move with freedom. That tube, like the coverings of snails, &c. daily grows with the animal, from the matter which perspires from its body; whence it is sometimes found straight, sometimes bent, according to the course which the worm steered in corroding.

As to their generation, it is probable enough, that, analogous to that of other insects, it is performed by copulation of male and female: for they can so lengthen one end of their tail, and thrust it out of the pile, that they may copulate by that means. Then they lay their eggs in the water, close to the piles, to which they stick by their clammy viscid matter, such, for example, as frog's spawn; and afterwards, by the heat of the sun, hatch the worm, which immediately endeavours to get into the pile.

Dr. B. could not observe the difference of sex, either with the eye, or a microscope. Some think them hermaphrodites, as snails, and that they copulate in the same manner: but these conjectures are not very probable.

Many remedies and secrets for destroying these dangerous enemies were immediately boasted of, which for the most part were preparations of arsenic or mercury, and are not worth enumerating: the following is the best and surest of all. Take an iron plate, of an oblong figure, and of the width of the pile, with a strong handle at each end. One end of this plate must be armed with thick nails, half an inch long, and about an inch asunder. The nails of this plate must be driven into a pile of any slight wood, with a hammer, and then the plate pulled off by means of its handles. And this is to be repeated, until the pile is perforated every where with small holes: then it must be daubed over with varnish in the hottest sun, the varnish being imbibed by the soft wood with so many holes in it; and while the varnish is yet hot, let it be strewed over with brick-dust. And this is to be repeated 3 or 4 times, after the preceding varnish is quite dry, till the pile is entirely surrounded with a stony crust, which will be impenetrable to all insects, and last many years.

But Providence has already so far destroyed these pernicious insects, which multiplied so prodigiously for 8 or 9 years past, that there is great room for hope, that our country will in a short time be entirely freed from them.

An Explanation of the Figures.—Fig. 6, pl. 8, is the pile-worm, of its natural middle size, lying on its belly.

Fig. 7, the same lying on its back. A is the stomach; B the duct, full of excrements; c the tail, with its defences dd, and its point e, which it can stretch out.

The Six following Figures are represented much larger than Life.—Fig. 8, A, A, the first series of fibres running straight down; BB the second series running transversely; CC the third taking a different course; DD the lower edge, which is infixed to the head.

Fig. 9, the shell or hemicranium, seen on the inside, with the process running across it, one end of which A is fixed, the other a is moveable.

Fig. 10, A, B, C, D, the same as in fig. 8; E the hinge, by which these are connected, and may easily dilate or open.

Fig. 11, AA the membrane covering the head, freed from the hemicrania, which were attached to this membrane; B the place, where the hemicrania were connected; C the middle anterior part, in which the tubercle was prominent.

Fig. 12, AA the membrane of fig. 11, separated and turned back; B the pellucid pyriform body, lying in the middle of the head, and which formed; C the tubercle.

Fig. 13, the two defences of the tail, of which the exterior part A, is gibbous, the other or interior B, is, as it were, hollowed: these extremities are bifid. C, the part by which they are joined to the tail.

Two Observations of Explosions in the Air; *one heard at Halsted in Essex, by the Rev. A. Vievar*; *the other by Sam. Shepheard, Esq. of Springfield.* N° 455, p. 288.

On Sunday the 12th of March 1731-2, between 1 and 2 o'clock in the afternoon, walking in the garden, Mr. V. heard as it had been a loud clap of thunder from the north-east. While looking into the air, the noise was repeated very loud, but seemed more like the violent fall of a house, so that he expected every moment an out-cry from the town: but he was soon undeceived, when it began again, and he found it made towards him, with a different noise from the former, being like the grinding of flint stones, but very loud. Its dimensions seemed to be about 3 feet wide. He found it sink in the air, and as it seemed to point directly at his head, he laid himself down on a glass-slope, to let it pass over. However, at the upper end of the walk it fell to the ground, and came rolling down the grass-walk; and he can compare it to nothing better than to that of a violent grinding of flint-stones, or a coach and six at full speed on a causeway of loose stones. He lay attentive, expecting to see something, and saw a piece of wood came running before it. When the phenomenon came to the water-side, it twisted up a large stake that stood in its way, and tossed it towards him with much violence, and immediately fell into the water, with the violence and noise of a red-hot mill-stone. He has seen the seas break against a rock in a storm, but never saw a greater ferment caused by the boiling of the waters. It staid about a quarter of a minute in the water, and then mounted again into the air, and went rattling away, but with much less violence. He heard it for about a quarter of a mile, and lost it. It came against the wind, and not faster than a man may walk. The froth and foam on the water remained 30 hours after.

The other Account. By Sam. Shepheard, Esq. P. 289.

On Tuesday, Aug. 15, 1732, between 11 and 12, the sun shining very bright and hot, without the least cloud, the wind so calm, that the water was as smooth as glass, Mr. Shepheard was in a little room next the garden, about 40 yards from the canal, when he heard a very surprising noise of fire, as if a very large quantity of oil had been thrown into a great bonfire, burning in its greatest rage. He stepped immediately to the window, which was open, where he saw the middle of the canal, which in the dry season had sunk about 6 inches, in extreme agitation, as rough as the Thames in a storm, foaming and smoking, and forced up, to appearance, full 2 feet above the surface, but it might be far more, the window being much higher than the canal; and the man, who was at work, protests he saw the water, like the spray of the sea, above the dwarf-trees, which must necessarily be 5 or 6 feet. The duration of the phenomenon might be half a minute, and it made such a stench in the house, as if a gun had been fired in it.

The canal bears east and west, and the man says he heard it coming from the west, bringing the leaves of some tall trees from an adjacent field in its passage; but he could not discover any material or substantial body to fall in the water, where the hissing was very loud and violent; neither was there any lightning or thunder before or after, but the day remained bright, still, and hot. The space of the canal that was affected by it, might be 12 or 15 yards.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1738, *pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. Præs. By Isaac Rand, Apothecary, F. R. S. Hort. Chel. Præf. ac Prælec. Botan.* N° 456, p. 291.

This is the 17th presentation of this kind, amounting to 850 plants.

Some Reflections on Generation, and on Monsters, with a description of some particular Monsters. By Daniel de Superville, Physician to the Margrave of Brandenburg-Bareith, &c. Translated from the French by Phil. Hen. Zollman, F. R. S. N° 456, p. 294.

On the subject of generation, this author espouses Leuenhoeck's theory; according to which he thinks monstrous births may be easily accounted for. He mentions having in his collection a pig, that had 8 feet; the 2 bodies, that were separated, reunited themselves by the spina dorsi below the diaphragma, and had but one visible neck supporting a head, larger than it should be, on which there appeared 4 ears, 3 eyes, and the snout seemed double. He had also the head of a foal, which was double, and had 3 eyes. He had a Turkish duck, which was double, the 2 bodies being joined by the breast; each body had 2 wings, and 2 legs; but they had only 1 neck with 1 head. He had a chicken, which had a second rump fixed to its breast, with the 2 legs, and 2 paws. He had a frog, which besides its 4 paws, had a 5th as well formed as the others, which came out at the right shoulder. The production of all these monsters that are double, or have superfluous members, may very well be occasioned, he thinks, by 2 animalcula entering into the same egg; they touch, they close, they unite, they croud each other; the parts of the weakest, being too much crouded, cannot extend nor display themselves; so they vanish, as it were, so much the easier as they are extremely tender, and without any sensible consistency.

It is not more difficult to find plausible reasons for imperfect monsters, or that have an odd conformity, as to the whole, or as to some of the members. He had the fœtus of a sheep, which had no nose; the part where the nostrils should be, seemed to be flayed, and the 2 eyes were one by the side of the other. On the forehead there was a small trunk, of about $1\frac{1}{4}$ inch long, and pierced at the end by 2 nostrils. He had another, which had but 1 eye, in the middle of the forehead. He had a human fœtus, of about 7 months, which had no mark of the sex, and instead of the legs there was a bag that ran to a point, the extremity of which was cartilaginous; in that bag there was a bone 3 inches long, covered with a muscular flesh; it was articulated with the os sacrum; the osea innominata was wanting, and below the anus, which was on the middle of the os sacrum, there was a small tail like that of a pig.

When he was at Stetin in Pomerania, about 12 or 14 years before, a midwife came to tell him, that a serjeant's wife was delivered of 3 dead children, one of which had no head. He immediately went, and observed, that these fœtuses had died at different times. One began already to corrupt, and the epidermis severed itself at the least touch. The monster without a head was also already quite flabby, and the third seemed to have died but a few hours before. He examined the monster: there was no appearance of any head; and instead of the navel, there was a small lump of spongy flesh, of the size of a large strawberry. About the secundines he found but 2 placentas, and 2 coats; so that this monster must absolutely have been in one of those coats with another fœtus. The midwife was not skilful enough to give him an account of the delivery; he put questions to the mother, who assured him she felt one child dying 3 weeks before, and that the last died the evening before. He offered a good sum of money to have all she was delivered of, but they would not let him have it. He still offered money to have only permission to dissect the monster, but the superstition of the parents deprived him of that satisfaction.

He had in his collection a monstrous fœtus, which deserved particular attention. It was of 8 months, without head or arms; the figure outwardly seemed to be nothing else but the abdomen with the legs; these were well-shaped and proportioned, with the toes, and the beginning of the nails; the right foot however was, as it were, crooked, and bending inwards. Having opend it, he found indeed but one cavity, which in the upper part contains a small bladder. There was not in all the cavity any thing besides a bit of intestine, the 2 kidneys, the bladder, and the right testicle, which lay upon the ring. The flesh was hard, and, as it were, carcinomatous. The navel-string went in a little higher than naturally, and a little towards the right side, entering into the intestine. There was a slender intestine, of about 14 lines in length, proceeding from the same place, where the navel entered into the cavity; next came the cæcum with its vermicular appendix, the colon and the rectum, the whole together of the length of about 2 feet. These intestines went from above to below in zig zag, and were attached to the spina dorsi. There was no trace of the heart, the lungs, the stomach, the liver, the spleen, the pancreas, the mesentery, all were wanting. The small bladder was fleshy, and contained some serosity; it was attached to the first of the vertebræ of the neck. This beginning of the spina was bent forwards like a bow, and formed the monster's roundness from above. The bended extremity kept the little bladder, as it were, under, and shut up in the cavity closed up by the ribs. This cavity was to form the thorax, but the sternum was wanting, as well as the diaphragm.

The opinions of most of the natural philosophers concerning the origin and formation of monsters may, he observes, be reduced to two hypotheses: 1. That monsters are original, that is, that even in conception the monster is conceived.

2. That they are not produced but by accident. It may be concluded from what he had said about double monsters, that he believed them accidental; and he believes, rigorously speaking, they are so, whatever they be; for supposing every animalculum to be an embryo created, he cannot imagine them to be created imperfect. Their imperfection, their deformity, may proceed from a thousand accidents, either in the reservoirs where they are contained, or in the different routes they are obliged to take, going from father to son. In this case it may easily happen, that they are monsters, even in the moment of conception, though they be such by accident. To how many accidents are they not subject afterwards in the venter of the females? A fall of the mother, a strong pressure, a contusion, &c. may disorder the nice and tender structure of that little creature so far, that a great many of its parts do not unfold themselves any longer, are destroyed, or have their order and natural situation quite changed.

The disturbed and disordered imagination of the females ought also to be ranged among the accidental causes of monsters. He had seen in a sow, just slaughtered, 7 pigs, which all had the bloody mark of the knife about their necks. About 20 years ago, a cloth-shearer, in Holland, had the misfortune to fall into the hands of some drunken young fellows, who murdered him, and stabbed him with more than 20 wounds with their swords. He was to be married that very week; his intended bride saw his corpse naked with all those wounds, and was 2 days after delivered of a dead child, which had the marks of the wounds in the same places of its body, where the mother had observed them on her dead lover.

He very well knew, that these sorts of instances, of which one might allege some hundreds, would not go down with certain people, who deny the effect of the mother's imagination on the fœtus. They lay stress on two principal reasons: 1. It is pretended, that the fœtus has no immediate connexion with the mother who carries it. But this is ridiculous, for it cannot be denied, that the secundines are closely united to the matrix, and receive from the mother a humour, or a liquid, which by the navel-string it remits to the fœtus. It is by that way it receives its nourishment, that is, the matter necessary for its increase. Accordingly one may say, that the fœtus owes part of its being to the mother; and that the liquid which runs in the vessels of the mother, runs likewise in the vessels of the fœtus. 2. It is said, that it is incomprehensible, how the soul of the mother can have an effect on the child. He owns he does not comprehend it neither. It does not follow from thence, that we ought to reject as false all that our reason cannot penetrate into. When once the existence and the nature of the soul has been demonstrated, when once we have a perfect knowledge of the manner how an immaterial being acts upon matter, we shall then reason in consequence about what the soul can do, and cannot do. Daily observations demonstrate, that the disordered and disturbed imaginations of women often hurts the infants. And this is a reason, which he adds to all the others, to think he had good grounds to conjecture, that all monsters were accidental; and to believe, that by the hypothesis of animalcula one may better explain the phænomena which are observed in generation, than by any other.

On a Bregma of a Gigantic Magnitude; with a Problem to determine the Size of the Giant according to the Rules of the Art of Drawing. By James Theodore Klein, Secretary to the Republic of Dantzic, and F. R. S. N° 456, p. 308. *From the Latin.*

Having obtained, from Wittsen's museum, at Amsterdam, a bregma of a gigantic size, in height 9 English inches, and its breadth 7, with a description and figure by Ruysch, representing the height of the head, from the chin to the crown, 20 inches, and the breadth at the temples 12 inches; and also another bone of the same kind, the height of which was $5\frac{6}{8}$ inches, and breadth 5 inches, but without a figure and reference to the head, it is easy to find, according to the rules of painting, by taking 8 lengths of the head, that the giant's stature was 13 feet 4 inches. But being desirous also to know the just proportion of the other bregma, according to strict mathematical rules, M. Klein proposed the following problem to Dr. Henry Kühn, professor of mathematics at Dantzic, viz.

If, in two human bodies of different stature, the height of the bregma in the former, be 9 inches, the breadth 7, the height of the whole head 20, the breadth 12; and in the latter, the height of the bregma $5\frac{3}{4}$, and the breadth 5; to determine the height and breadth of the whole head of the latter, and the proportion of its stature to that of the former.

Now the stature of the first body being $20 \times 8 = 160$ inches, or 13 feet 4 inches, if the bodies were similar, the question would be easily answered, by making a simple proportion, viz. as any dimension of the one is to the like dimension in the other, so is the stature of the former, to the stature of the latter. But because 9 to 7 and $5\frac{3}{4}$ to 5 are dissimilar ratios, the bodies are not similar. Therefore we must take a kind of mean between the stature required, as determined both by comparing the lengths and breadths of the bregmas together. Which may be done in three different ways, as follows:

1st. As $9 : 160 :: 5\frac{3}{4} : 102\frac{2}{9}$ inches $= 8$ feet $6\frac{2}{9}$ inches, the stature of the latter body as determined by the heights of the bregmas.

And, as $7 : 160 :: 5 : 114\frac{2}{7}$ inches $= 9$ feet $6\frac{2}{7}$ inches, the stature of the same as determined by the breadths of the bregmas.

The mean between these two is 9 feet $\frac{1}{4}$ inch nearly.

2dly. As $9 + 7 : 5\frac{3}{4} + 5 :: 160 : 107\frac{1}{2}$ inches $= 8$ feet $11\frac{1}{2}$ inches; differing only $\frac{3}{4}$ of an inch from the former way.

3dly. By the geometrical means, as $\sqrt{9 \times 7} : \sqrt{5\frac{3}{4} \times 5}$, that is, as $\sqrt{63} : \sqrt{28\frac{3}{4}} :: 160 : 180$ inches or 9 feet; which being nearly a mean between the two former, it may be accounted the most accurate of all.

Hence then the statures being 13 feet 4 inches, and 9 feet, or 160 inches and 108 inches, are to each other as 40 to 27.

An Account, by the Rev. Zachary Pearce, D. D. F. R. S. of a Book, entitled, Reflections Critiques sur les Histoires des Anciens Peuples, &c. Paris* 1735, *4to. in* 2 *vols.* N° 456, p. 313.

The general design of M. Fourmont, the author of this book, is to rectify the history of the most ancient nations, particularly the Chaldeans, Hebrews, Phœnicians, Egyptians, Greeks, &c. down to the time of Cyrus, the founder of the Persian empire. The work consists of 3 books.

In the first of which, he gives, at length, the famous fragment of Sanchoniathon the Phœnician, as translated by Philo Byblius, and preserved by Eusebius, in his Præparatio Evangelica, lib. I. cap. 9. With this fragment he has published a French version of it, in which he endeavours to distinguish between the account given by Sanchoniathon the author, and what he supposes to be the additions of Philo the Greek translator. After this he examines into the reasons brought by several of the learned, for and against the genuineness of the fragment, and determines in favour of it, with as much weight of argument as the question will admit. He then takes notice of a treatise, written on the same subject as his own, by our learned countryman Bishop Cumberland; and having examined and declared his dislike of the Bishop's scheme

* Dr. Zachary Pearce, a celebrated English bishop, and F. R. S. was born in 1690, and educated at Westminster; whence he was elected to Trinity College, Cambridge; while there, he wrote several papers in the Spectator and Guardian. In 1724 he published an edition of Longinus; and his next work was a treatise, "On the Origin and Progress of Temples."

In 1739, Dr. Pearce was made dean of Westminster, and in 1748, bishop of Bangor. In 1756, he was translated to the see of Rochester, with the deanery of Westminster. He died in 1774; and an elegant monument to his memory is erected in Westminster Abbey.

Bishop Pearce published an edition of Cicero de Officiis; also A Review of the Text of Milton; an Account of Trinity College, Cambridge; and several other esteemed works. And, since his death, there have been published his Sermons, in 4 vols. 8vo.; also his Commentary on the Gospels and the Acts of the Apostles, in 2 vols. 4to.

in the main, he prepares his reader to expect full satisfaction from his own, which makes the subject of his second book.

In the second book, he undertakes to reconcile the generations of men, set forth in Sanchoniathon's fragment, with those which are recorded by Moses, of the patriarchs before, and for some time after the flood. By the help of Hebrew, Phœnician and Egyptian etymologies, he often makes the names, which at first sight are almost all quite unlike, to be the same in sound, or at least in sense. And by this application of his skill in the ancient languages, he readily finds out a coincidence between Moses's and Sanchoniathon's earliest generations.

But his main work, and what he appears most pleased with, is his discovery of Abraham and his family, among the latter generations recorded by Sanchoniathon. Having laid down, that Ouranos is Terah, the father of Abraham, he undertakes to prove, that Abraham is the Chronus of Sanchoniathon, and the Saturnus of the Latins; that Sarah, his wife, is the same with the goddess Rhea; that Ishmael, Abraham's son, is the Mûth of Sanchoniathon, and the Dis or Pluto of the Greeks and Romans: that Isaac, Abraham's other son, is the same with the Sadid of Sanchoniathon, with Jupiter among the Latins, and Ζεὺς among the Greeks, his wife Rebecca being Juno; that Esau, Isaac eldest son, is Osiris and Bacchus; and that Jacob, the youngest, is Typhon. And, in like manner, he finds a very great part of the Grecian theology in Abraham's family.

In the mean while his readers will, perhaps, make two very material observations on this extraordinary discovery of his: the one, that Chronus's character in Sanchoniathon's fragment, is the most immoral and tyrannous of any recorded there: and how to reconcile this with the character given in scripture to Abraham, as the friend of God, the father of the faithful, &c. is no easy task: it requires, to be sure, more than a resemblance of two or three circumstances, common to Chronus and Abraham, when their historians in fifty other circumstances make their characters essentially different. The other consideration, which occurs, when we read this treatise, is, that Abraham had ill luck indeed, if, when he left his native country because of the rise of idolatry there, all the grosser idolatry of the heathen nations after his time took its rise from him and his family: the very crime which he took pains to avoid, he was the accidental occasion of, if he and his are to be thus placed at the head of the heathen theology.

The author, having finished this remarkable part of his work, enters into a very learned detail of the particular Gods of the several heathen nations, who are the most celebrated in history; and he has shewed a great compass of reading on this occasion. Hardly any writer has been more copious on the subject, or has given better hints for clearing up many passages of sacred and profance story.

In his third book he has treated at large on the dynasties of Egypt, and the shepherd kings who reigned there: both of them, perhaps, the darkest spots in the whole face of antiquity. He has taken great pains to fix the epochs of the kings of Sicyon, Sidon and Tyre, of Arabia, Assyria, Lydia, of the Medes and Babylonians; concerning all which, he has laid together the most remarkable testimonies of the ancients. At length he comes to his favourite point, the Chinese history, and gives, as he says, a complete list of their kings, from the flood down to the present monarch of that empire, and shows that the chronology of the Chinese, may be made pretty nearly consistent with the true chronology of the Old Testament.

And for this part of the work the author seems well fitted, being skilled, as he tells us in the preface, in the learned characters of that country, which he has studied for near 20 years, and has for some time taught in the royal college at Paris; and having composed 5 dictionaries and a grammar, of that language, with a translation, almost intire, of the geography of Tamim, which contains no less than the whole history of that empire: on which occasion he applies to himself, and the progress he has made in the Chinese learning, those expressive verses of Virgil in his 6th book of the Æneid:

——— Pauci, quos æquus amavit
Jupiter, aut ardens evexit ad æthera virtus,
Dii geniti, potuere.

On the Scurvy-Grass that grows in Greenland. By Mr. David Nicholson, Surgeon. N° 456, p. 317.

Mr. N. communicates this as matter of truth, and not hypothetic, viz. that the scurvy-grass in Greenland, equally the same with ours in England, as to the figure of the plant, and all its appearance to the eye, changes its nature much, as it approaches the sun; for in that climate, its principal quality, the volatile salt, is neither pungent nor perceivable; but to the taste, the whole plant is quite as insipid as the colewort or beet. Mr. N. preserved some plants with their natural earth, and brought them to London alive; and he observed the remarkable change produced by the sun's heat on them; for the saline matter in Greenland, which certainly was analogous to a fixed salt, became, in a month's time, almost to the same volatility as that which naturally grows in England.

*Concerning two Species of Lines of the Third Order, not mentioned by Sir Isaac Newton, nor by Mr. Stirling. By Mr. Edmund Stone *, F. R. S.* N° 456, p. 318.

Mr. Stone having for some time past been reading and considering the little treatise of Sir Isaae Newton, intitled, Enumeratio Linearum tertii Ordinis, as also the ingenious piece of Mr. Stirling, called, Illustratio Tractatus Domini Newtoni Linearum tertii Ordinis; he observed, that they have neither of them taken notice of the two following species of lines of the third order; and he ventures to affirm, that the 72 species mentioned by Sir Isaac, with the 4 more of Mr. Stirling, and these 2, making in all 78, is the exact number of the different species of the lines of the 3d order, according to what Sir Isaac has thought fit to constitute a different species.

The 2 species are to be reckoned among the hyperbolo-parabolical curves, having one diameter, and one asymptote, at N° 8 of Newton's Treatise, or p. 104 of Mr. Stirling's; its equation being $xyy = \pm bx^2 \pm cx \pm d$; which will give, not 4, as in these authors, but 6 species of these curves: for,

1. If the equation $bx^2 \pm cx + d = 0$, has 2 impossible roots, the equation

* Mr. Edmund Stone was a remarkable instance of the effect of industry united to good natural talents; having raised himself to an eminent rank in the mathematical sciences, as well as in the languages, by his own application alone; all the instructions he ever received, being only to know the 24 letters of the alphabet, which he was taught at 8 years of age, by a servant in the duke of Argyle's family, where young Stone's father was gardener; with whom, also, at an early age, the son became a servant; in which situation he spent a considerable part of his life. When about 18 years of age, his extraordinary talents were accidentally discovered by the duke, who found him in the garden reading Newton's Principia in the Latin language; when, on inquiry, the duke learned that by procuring books, he had made himself master of arithmetic, geometry, &c. as well as the Latin and French languages. Delighted with his conduct and conversation, the duke drew him from obscurity, and placed him in a situation to pursue his favourite studies.

The time of Mr. Stone's birth is unknown, though it was probably towards the latter end of the 17th century, as the first edition of his Mathematical Dictionary was printed in 1726. After which, several other useful works, both translations, and books of his own composition, follow at certain intervals of time. As, 2.—A Treatise on Fluxions, in 1 vol. 8vo. 1730; the first part being a translation, from the French, of l'Hopital's Analyse des Infiniments Petits; and the 2d part, or inverse method, being supplied by Stone himself.

3. The Elements of Euclid, in 2 vols. 8vo. 1731: being a neat and useful edition of those elements, with an account of the life and writings of Euclid, and a defence of his elements against modern objectors.

4. Dr. Barrow's Geometrical Lectures, translated from the Latin, in 1 vol. 8vo. 1735. Besides several other smaller works.

The time of his death is also unknown.

$xyy = bx^2 \pm cx + d$, will, as they say, give 2 hyperbolo-parabolical figures, equally distant on each side the diameter A B. See the 57th figure in Newton's Treatise, and this is his 53d species, and Stirling's 57th.

2. If the equation $bx^2 - cx + d = 0$, have 2 equal roots, both with the sign $+$; the equation $xyy = bx^2 - cx + d$, will, as they say, give 2 hyperbolo-parabolical curves, crossing each other at the point τ in the diameter. See fig. 58 in Newton; and this is his 45th species, and Stirling's 58th.

3. But if the equation $bx^2 + cx + d = 0$ have 2 possible unequal negative roots $A\rho$ and $A\tau$, the curve given by the equation $xyy = \pm bx^2 + cx + d$, will consist of 2 hyperbolo-parabolical parts, as also of an oval on the contrary side the asymptote or principal absciss, as fig. 1, pl. 9. And this is one of the species omitted by Sir Isaac and Mr. Stirling, which is really the 59th species.

4. Also if the equation $bx^2 + cx + d = 0$, have two equal negative roots $A\rho$ and $A\tau$; the curve given by the equation $xyy = \pm bx^2 + cx \pm d$, will consist of 2 hyperbolo-parabolical parts, and also of a conjugate point on the contrary side the asymptote or principal ordinate, as fig. 2. And this is the other species of these curves omitted by Sir Isaac and Mr. Stirling, which is really the 60th species.

5. If the roots of the equation $bx^2 - cx + d = 0$, be real, and unequal, having both the sign $+$; the curve given by the equation $xyy = bx^2 - cx + d$, will, as they say, consist of a conchoidal hyperbola and a parabola, on the same side the asymptote or principal ordinate. See fig. 59 in Newton; and this is really the 61st species.

6. If the roots of the equation $bx^2 \pm cx - d = 0$, have contrary signs, the equation $xyy = bx^2 \pm cx - d$, will, as they say, give a conchoidal hyperbola with a parabola on the contrary side the asymptote or principal ordinate. See fig. 60 in Newton; and this is really the 62d species.

An Account, by Mr. Harris, of several Alterations and Contrivances about the Terrestrial Globe, to render it, as he thought, more commodious and useful in Practice. N° 456, p. 321.

A New Method of improving and perfecting Catodioptrical Telescopes, by forming the Speculums of Glass, instead of Metal. By Caleb Smith. N° 456, p. 326.

The imperfections of telescopes are attributed to two causes; viz. the unfitness of the spherical figure to which the glasses are usually ground, and the different refrangibility of the rays of light.

The first of these defects only, was known to the writers on dioptrics, before Sir Isaac Newton; for which reason, as he informs us, Opt. Lect. 1, 2, "they imagined, that optical instruments might be brought to any degree of perfection, provided they were able to communicate to the glasses, in grinding, what geometrical figure they pleased; to which purpose various mechanical contrivances were thought of, whereby glasses might be ground into hyperbolical, or even parabolical, figures; yet nobody succeeded in the exact description of such figures; and had their success been answerable to their wishes, yet their labour would have been lost (continues this incomparable mathematician); for the perfection of telescopes is limited, not so much for want of glasses truly figured, according to the prescriptions of optic authors, (which all men have hitherto imagined) as because that light itself is an heterogeneous mixture of differently refrangible rays; so that were a glass so exactly figured as to collect any one sort of rays into one point, it could not collect those also into the same point, which having the same incidence upon the same medium, are apt to suffer a different refraction." Phil. Trans. N° 80. And again,—"Diversa diversorum radiorum refrangibilitas impedimento est, quo minus optica, per figuras, vel sphæricas, vel alias, perfici possint; nisi corrigi possint errores illinc oriundi, labor omnis in cæteris corrigendis imperite collocabitur." Principia, &c. Scholium ad finem Libri Primi.

Now, for this principal and last-mentioned defect, no one has proposed any remedy; apprehending perhaps the difficulty of attaining such to be insuperable; inasmuch as the great author of this discovery himself had not shown any method to correct those errors which arise from this inequality of refraction; but rather discouraged any such attempts, by declaring, "that on this account he laid aside his glass-works," (Phil. Trans. N° 80) "and looked upon the improvement of telescopes, of given lengths, by refraction, as desperate." Optics, 2d Edit. p. 91.

However, as it has been proved by incontestible experiments, that this dissipation of the rays of light, from whatever cause it proceeds, in passing out of one medium into another, is not accidental and irregular; but that every sort of homogeneal rays, whether more or less refrangible, considered apart, are refracted according to some constant uniform and certain law; and as the removal of so great an impediment as this, of unequal refraction in the rays of light, is of great importance to the science of dioptrics, and absolutely necessary to its further advancement; we have thought it worthy of a careful examination, whether, in some cases at least, it might not be possible for contrary refractions so to correct each other's inequalities, as to make their difference regular; and if this could be conveniently effected, Sir Isaac Newton has acknowledged, "there would be no further difficulty." Phil. Trans. N° 88.

Now, on a due consideration of this subject, we have found it possible, by proper methods and expedients, to rectify those errors which proceed from the different degrees of refrangibility in different rays, passing from one medium into another; admitting only this well-known and established principle, on which we ground our reasoning, viz. "That the sines of refraction of rays, differently refrangible, are one to another in a given proportion, when their sines of incidence are equal." Optics, 2d edit. p. 66. And our present design is, to show what advantage this will yield towards improving and perfecting catodioptrical telescopes, by making the speculums of glass, instead of metal, in the following manner: let A B C D E F, fig. 3, pl. 9, represent the section of a concavo-convex speculum, whose two surfaces are segments of unequal spheres; call the radius of the sphere, to which the concave side is ground, a; and the radius of the convex surface, which must be quicksilvered over, e; let B R be the axis of the speculum, or a line perpendicular to both the surfaces; where let P be the principal focus, or point where parallel rays of the most refrangible kind are collected, by this speculum; and Q the focus, or point of concourse, of such rays as are least refrangible; viz. after they have suffered two refractions, at entering into, and passing out of, the concave surface D E F, and also one reflection from the convex surface A B C. If the radius of concavity be greater than the radius of convexity, as we will in the first place suppose, then P will fall nearer the vertex of the speculum than the point Q; and the interval Q P will be the greatest aberration, or error, occasioned by the separation, or unequal refraction, of the greatest and least refrangible rays, after their emergence from the concave surface F E D. Call the common sine of incidence, n; the sine of refraction of the least refrangible rays, out of a dense medium into a rarer, m; and of the most refrangible, μ; then, according to the known and received laws of refraction and reflection, the focal distance of the most refrangible rays, from the vertex of the speculum, neglecting its thickness, as of little or no moment in the present case, will be found $= \frac{nae}{(a-e)\,2\mu+2ne} =$ P B. And the quantity of the greatest aberration, occasioned by the different refrangibility of the most and least refrangible rays, P Q, will be to the focal distance just mentioned, P B, as $(a-e)\times(\mu-m)$ to $(a-e)\,m+en$; which quantity, or error, thus obtained, to abbreviate the calculation, call ι; and now let it be required to form a lens, if possible, which, placed at some given point in the axis, between the focus of the most refrangible rays P, and the vertex of the speculum, as H, shall refract not only the rays of the most refrangible kind tending to the point P, but also the rays of the least refrangible kind tending to Q, in such a manner, that both sorts shall concur, after such refraction, in some other point of the axis R: let HP, the given distance of the point in the axis H, from the focal point P, be called d; and then if the point H has been assumed, so that the said given quantity, or distance, d, is greater than $\frac{(\mu-n)t}{\mu-m}$, but less than $\frac{mt}{\mu-m}$, the refracting superficies GHI, that shall perform what was required, will be part of a concave sphere, whose radius is $=\frac{(dd+dt)\times(\mu-m)}{mt-(\mu-m)d}$; and HR, the distance of the given point H, from R, the point to which all the rays will tend, after refraction at the said concave surface, (whose radius being found, as above, we call v) will be $=\frac{\mu dv}{(d+v)n-\mu d}$. Lastly, upon the point R thus obtained, as a centre, with an interval a little less than HR, describe the circumference KLM, and the figure GHIMLK will denote the section of a double concave lens, which, placed at the given point in the axis H, (taken nevertheless within the limits above mentioned) will collect all sorts of rays proceeding from the speculum, into one and the same focus, or point of the axis R, as was required; for the surface GHI, which first receives those rays, will refract the most refrangible sort converging to the point P, and also the least refrangible converging towards Q, so that both sorts, after such refraction, will concur in the point R; but the rays tending to R, it is manifest, will suffer no refraction at their emergence from the superficies KLM, because R is the centre thereof, by construction; which point R, where a perfect image of an object infinitely distant will be formed, we call the focus of the telescope, to distinguish it from the point P, which we have before called the focus of the speculum.

In this manner a lens, (or instead thereof a triangular prism with two of its sides ground concave, and the third plain, if that be found as practicable) may be formed and situated, so as to correct the errors of the speculum arising from the different refrangibility of the rays of light. But in order to render this kind of telescopes absolutely perfect in their construction, the errors also that result from the spherical figure, must be rectified; and with regard to this, we assert, that it is possible to assume a point in the axis, between the focus of the speculum and its vertex, (as we have taken the point H, in the following example, see fig. 4,) at which, if a refracting superficies, or lens, be constituted, according to the method already delivered, it will not only correct the errors occasioned by the unequal refraction of the rays of light, but also rectify such as proceed from the spherical figure of this speculum, to a much greater degree of exactness than is requisite for any physical purpose, meaning always the errors of those rays which respect the axis. Now to find or determine this point, affords a problem not easy to be solved; and we recommend it, as worthy of the consideration of geometricians.

Seeing therefore it is possible, and we believe also practicable, to remedy the imperfections of this kind of speculums, from whatsoever cause they arise, by the method we have here proposed; it seems to follow, that catodioptrical telescopes may be carried, by this means, to as great a degree of perfection, as they are capable of receiving; provided spherical figures can be truly given, with an exquisite polish, to glasses of a large aperture, and a foil of quicksilver made also to retain that figure accurately, and without any inequality; for the object-glass or speculum being rendered perfect, so as that all sorts of rays, proceeding from one lucid point in its axis, shall be collected by means of the lens exactly in another point, its aperture may then be extended to its furthest limits; and that is, till the whole pupil of the eye, or the whole portion of the eye-glass to be used, when that becomes necessarily less than the pupil, be filled with rays proceeding from the speculum, and flowing from one point of the object, but no farther; because this is a limitation made by nature in the structure of the eye itself: and in telescopes whose construction is such as we have now described, the largest aperture of the speculum that can ever be of use, will be to the diameter of the pupil of the eye, very nearly, in a ratio compounded of the ratios of the focal length of the speculum, to the distance of that focus from the lens, and of the distance of the lens from the focus of the telescope, to unity: that is, of BP to PH, and of RH to 1; which proportion holds, whatever be the charge or the power of magnifying.

But if inquiry be made as to the charge most proper and convenient, that will be best determined by experience, in these, as well as in all other sorts of telescopes: however, on supposition that one of a given length has its aperture and charge rightly ordered and proportioned, the rule for preserving the same degree of brightness and distinctness, in all others of a like construction, will be, to make the apertures, and magnifying powers, directly as the focal lengths of the speculums; which shows the vast advantage and perfection of these telescopes, above the common reflecting ones; where, according to Sir Isaac Newton's rule, the apertures, and powers of magnifying, must be as the biquadrate roots of the cubes of their lengths. See his Optics, 2d edition, p. 97.

It is likewise a considerable advantage in this construction, that the reflection from the concave side of the speculum will do no sensible prejudice; because the image of any object there made, is removed to so vast a distance from the principal image, formed by the convex surface, as to create no manner of confusion or disturbance in the vision; which necessarily happens, in some degree, from the vicinity of those images, when the glass is ground concave on one side, and as much convex on the other; according to the method propounded by Sir Isaac Newton, in his most excellent book of Optics.

It may be imagined perhaps, at first view, that, if our reasoning is just, the errors of refracting telescopes, occasioned by the different refrangibility of light, may be corrected by a like artifice: but the aberration of the rays from the principal focus is there so great, and bears so considerable a proportion to the focal length of the telescope, that the error cannot be rectified by the interposition of any lens, until the rays are, by a contrary refraction, collected again at an infinite distance, which renders this expedient quite useless: however, there is no need to despair of accomplishing even this, by other methods: and, by the way, we may observe, if it were worth while to seek a remedy for the errors occasioned by the spherical figure of the object-glass only, in dioptrical telescopes; that might be obtained by the proper application of a suitable lens, between the focus and the vertex of the object-glass; which is much more easy and practicable, than the grinding of glasses to hyperbolical or elliptical figures.

For a further illustration of the foregoing, it may be proper to exhibit the several parts and proportions of a telescope in numbers, computed according to the theorems already delivered; and in practice we judge it will be most convenient, that the radii of the spheres, to which the concave and convex sides of the speculum are ground, be nearly in the ratio of 6 to 5; as in the following example; where, in fig. 4,

ABCDEF represents the great speculum of glass, ground concave on one side, and convex on the other; quicksilvered over the convex side, and of an equal thickness all round its circumference.

The radius of concavity $= a =$ 48 inches.

The radius of convexity $= e =$ 40 inches.

Then putting n, the sine of incidence $=$ 100; m, the sign of refraction of the least refrangible rays, out of glass into air, $=$ 154; and μ, the sine of refraction of the most refrangible rays, $=$ 156; as Sir Isaac Newton found them by experiments; we shall have,

PB, the focal length of the speculum with regard to the most refrangible rays $=$ 18.2926 $+$, which will be somewhat increased by the thickness of the glass, when that is considerable.

PQ, the greatest aberration of the rays, occasioned by their different degrees of refrangibility, $=$ .05594 $+$, which quantity, in practice, should be a very little augmented, rather than otherwise; therefore we put it here $=$ .056 $= \iota$.

The radius of the concave surface of the lens, turned towards the speculum, viz. of GHI, $= \nu =$ 2.8 inches.

The radius of the concave surface of the lens, turned from the speculum, viz. of KLM, = 6.7 inches.

The thickness of the lens at the vertex LH = $\frac{1}{10}$ of an inch.

The aperture of the lens must be about $\frac{1}{6}$ of the aperture of the speculum.

HP, the distance of the focal point P from the point H, where the abovesaid lens is to be placed, so as to correct the errors arising from the different refrangibility of the rays, and also the errors of the spherical figure, = $2\frac{2}{7}\frac{4}{3}$ inches.

HR, the distance of H, the vertex of the lens, from R, the focus of the telescope, = 6.8 inches.

And if we suppose the diameter of the pupil of the eye to be $\frac{1}{8}$ of an inch, though it has not one certain measure; then the diameter of the greatest aperture of the speculum, that can ever be of use, will be $6\frac{4}{3}$ inches, nearly.

The small plano-convex eye-glass O must always have one common focus with the telescope, viz. the point R translated to r, by reflection from the base of the prism N; for which reason it must retain, at all times, an equal and invariable distance from the lens GHIKLM; which distance will be the focal length of the said eye-glass + HR = HN + Nr, the distance of the lens from the focus of the telescope R.

The form and position of the prism N, and the contrivance of the other parts necessary, will be much the same as in the Newtonian telescope.

If the focal length of the eye-glass be $\frac{1}{4}$ of an inch, the telescope will magnify about 200 times.

This telescope may be contrived in the Gregorian way, by using, instead of a lens and prism, a small speculum spherically concave on one side, and convex on the other; but we think it not worth while to attempt this construction, as an investigation of the proportion between the two surfaces necessary, in this small speculum, to unite the rays proceeding from the great one, into one point, would be intricate, and the practice also very difficult; because a little inaccuracy will, in this case, occasion errors much more considerable than a like imperfection in the refracting lens.

We have hitherto supposed the radius of the concavity greater than that of the convexity; as being most convenient and useful, on several accounts, in forming this kind of telescopes; however, it may be proper to remark, that the same method may be used for correcting the errors of the speculum, when the radius of its concavity is less than that of the convexity; only the refracting superficies of the lens, placed between its vertex and focus, will be convex, and not concave, as in the former case. And there is another thing worthy of remark, that the focus, or point P, where the most refrangible rays are collected, will fall farther from the vertex of this speculum, than the focus of the least refrangible Q; a circumstance which never happens by refraction alone, in glasses of any figure whatever, or however they be disposed.

Now all things being put as before, and making fig. 5, HQ $= d$, then the convex superficies GHI, of a lens placed at H, that shall correct the errors arising from the different refrangibility of rays, in this kind of speculum, will be part of a sphere, whose radius is $= \frac{(\mu - m) \times (dd + d\iota)}{(\mu - m)d + n\iota} = \upsilon$. And HR, the distance of the point R, where the rays of all sorts will unite, after this refraction, from H the given point in the axis, will be $= \frac{d\upsilon}{(\mu - n)d + n\upsilon}$; which point R being taken as a centre, describe on it the arch KLM, and the figure GHIMLK will represent the section of a meniscus-glass, or lens, which, placed at the point H, assumed between the vertex and focus of the speculum, will collect all sorts of rays proceeding from it, into one and the same point, or focus R. We might also show, how this error may be rectified by one or more glasses, placed in the axis, at a distance further from the vertex than the focal point P; but the former speculum is so much preferable to this, for the constructing of telescopes, that we think it not worth while to prosecute this matter further. To conclude this essay;

Whoever shall think fit to put the method here proposed in execution, we dare venture, from a trial that has been made, to assure him of success; provided the same diligence, care, and accuracy, be applied, in choosing, figuring, polishing and foiling, the glass, that has of late been employed for the forming speculums of metal; and let none be discouraged, though the first and second attempt should fail; for that must be expected, if the ordinary way of grinding and polishing be used: greater exactness is here required, than is usually thought sufficient for the object-glasses of refracting telescopes: let it be also considered how many essays, for a long term of years, were made by Mr. Gregory, Sir Isaac Newton, and others, to reduce their constructions of the reflecting telescope into practice, without answering, in any tolerable degree, what their theories promised: the workmen they employed were chiefly optical instrument-makers, and had it been left to such persons only to perform by themselves, we have reason to think, that it would have been pronounced impracticable to this day, to make a reflecting telescope that should equal or excel refracting ones of 10 times its length; though we now see, that most of these artificers are capable of making them to such a degree of perfection as was formerly despaired of.

April 5, 1739.

Concerning an Earthquahe at Naples. By the Hon. Henry Temple, N° 456, p. 340.

Naples, Dec. 12, *N. S.* 1732.

—They say, the last earthquake here has made a great crack in the side of Mount Vesuvius, above 30 yards long. But what seems much more extraordinary, is, that the second shock, which was a very slight one, had a great effect upon the nerves: I and all the company where I was, as soon as the shock was over, were seized with a shaking, just as if we all had the palsy, our teeth chattering in our heads to such a degree, that we could hardly speak; and I find, that half the town felt the same effect from it. It would be natural to imagine, that this shaking was caused by the fright, but it is easy to prove the contrary; because, in the first place, the first shock, which was much more terrifying, had not that effect: 2dly, many people who were not sensible of the earthquake, found themselves seized in the same manner: 3dly, Mr. ——— who used to be troubled with convulsive fits, and had got quite cured of them here, was immediately seized with them again, after the earthquake; and, 4thly, every body, more or less, complained of head-achs for some days after.

Concerning a Monstrous Child, born of a Woman under Sentence of Transportation. By Mr. Timothy Sheldrake. N° 456, p. 341.

Elizabeth Spencer, being tried at the Assizes for the city and county of Norwich, for shop-lifting, and being found guilty of the crime, received sentence for transportation; for respiting of which sentence, she pleaded her belly, which plea, as she was a married woman, appearing what was very probable, she was favoured by the mayor and the other magistrates, by being allowed the full time that she said she had to go; at the expiration of which she was delivered of a child, which Mr. S. saw a few hours after it was born, and was exactly, in every part, according to the following account. The head had a rising on the top of it, and the nose was as if one nose was on the top of another, but only 2 nostrils, and those at the bottom of the lower nose. The arms were without the elbow-joint; the 2 bones, which make the lower joint of the arm, in common, were in this extended to the shoulder. Just under the ribs, and above the hips, was a deep place, as if a cord had been tied very strait, so as to sink down below the reach of the eye: this girding-in of the body, he believes might go almost round: he did not turn it, to see whether it did or not, but it was continued as far about the body as he could see, without turning it. By this girding-in of the body, the lower part of it was almost round, it being without either legs or thighs; but had 2 feet joined unto the lower part of the body, the heels inward, the toes, of which it had not the full number, pointing towards the sides. As to sex, this creature was a female, and born alive. It was the opinion of the women about her, that the midwife had injured the head in the birth, by which the rising in the head was produced; and this surprising creature that was born alive, was thus soon deprived of life. This woman, who had been the mother of several children, before this strange production, and all in perfect form, was by some free-speaking persons, charged with having been guilty of some practices both unnatural and unlawful, which she very positively always denied; and said that she knew nothing that could give any change to the natural form of this creature, but the strange apprehensions that her sentence had put her under, from the uncommon creatures the country to which she was sentenced might bring in her sight. These odd ideas that she had formed to herself, was all and the only thing, that had occasioned so great a change from the natural form the child might otherwise have had, as she often asserted.

Concerning the Mola Salu, or Sun-fish, and a Glue made of it. By the Rev. Mr. William Barlow.* N° 456, p. 343.

There was brought to this place, struck the day before in our river, a sun fish, weighing about 500lb. The form of it nearly answers that given by Mr. Willoughby, except that the tail of this was scolloped. It differed very much in one thing from that described by Mr. Willoughby, whose flesh, he says, was very soft:- on the contrary, the flesh of this was hard and firm, rather a gristly substance, than soft flesh.

A commander of a vessel told Mr. B. that his people took a sun-fish, south of Newfoundland, which, by his description, was considerably larger than that brought hither. They made no use of the flesh; but he remembers it was a gristly substance, hard and firm.

A piece of the flesh boiled, to try how it would look and taste, was all turned into a jelly. Being soft and tender, it could not be taken out of the saucepan with a fork, but only with a spoon; in colour and consistence nearly resembling boiled starch when cold. It had little or nothing of the fishy, but a grateful and pleasant taste.

By the sticking together of his lips, and from what he observed by touching

* Diodon mola. *Bloch.*

it with his fingers, Mr. B. observed that this boiled flesh was clammy and glutinous; which brought to mind, that what the ancients made use of to serve the purposes of glue, was made from fish. He then tried it on paper and leather, and found it to answer the use of paste very well: and it was owing in part to neglect, and partly to accident, that it was not also tried on wood.

From the descriptions given of the ichthyocolla, by Dioscorides and Pliny, the glue-fish seems not to be the same as this sun-fish. Whether the fish from which isinglass is made, be the same as the ichthyocolla of the forementioned authors, as the name usually given to it seems to import, he cannot tell: but neither the ichthyocolla of Rondelitius or Bellonius, nor the huso taken in the Danube, from the bladder of which fish-glue is made, can, by the descriptions given of them, be the same as the sun-fish.

Discovery of the Remains of a City under-ground, near Naples. Communicated to the Royal Society by William Sloane, Esq. F. R. S. N° 455, p. 345.

At Resina, about 4 miles from Naples, under the mountain, within half a mile of the sea-side, there is a well, down which about 30 yards is a hole, which some people have the curiosity to creep into, and may afterwards creep a good way under-ground, and with lights find foundations of houses and streets, which, by some it is said, was in the time of the Romans a city called Aretina, others say Port Hercules, where the Romans usually embarked for Africa. Mr. S. has seen the well, which is deep, and has a good depth of water at the bottom, that he never cared to venture down, being heavy, and the ropes bad. This city, it is thought, was overwhelmed by an eruption of the mountain Vesuvius, not sunk by earthquakes, as were Cuma, Baia, Trepergola, &c.

Of a Meteor seen in the Air in the Day time, Dec. 8, 1733. By Mr. Crocker. N° 456, p. 346.

On Saturday, Dec. 8, 1733, between 11 and 12, the sun shining bright, the weather warm, and wind at south-east, some small clouds passing, Mr. C. saw something in the sky, which resembled a boy's paper kite, which appeared towards the north, which soon vanished from sight, being intercepted by the trees which were near the valley where he was standing. Its colour was a pale brightness, like that of burnished or new-washed silver. It darted out of sight with a seeming coruscation, like that of star-shooting in the night; but it had a body much larger, and a train much longer, than any thing of that kind he had ever seen before. On coming home, one Brown said, he had seen the same thing, for the continuance of a minute; and that the body and train appeared to be about 20 feet long, and seemed to fall to the ground somewhere about the kennel-garden, whither Mr. C. accompanied him in expectation of finding some of those jellies which are supposed to owe their beings to such meteors: but we might have searched long enough, as the next day, when Mr. Edgcombe informed him, that he and another gentleman had seen the same appearance at the same time about 15 miles from us, steering the same course from east to the west, and vanished from them between Walkhampton and Oakhampton: they gave the same account of its figure, length and colour.

Of a Luminous Appearance in the Sky, seen at London on Thursday, March 13, 1734-5. *By John Bevis, M.D.* N° 456, p. 347.

When observing Mars near a small fixed star, then in the west, on the top of his house in Buckingham-street, about 5 minutes after 8, equal time; happening to turn his face southward, Dr. B. was surprised with an uncommon bright glade of light. It was straight, about 2½ degrees broad, and 110 or 120 degrees long, ill defined at either end, but pretty well at the sides, much as the common rainbow, or one of those pyramids which are used to dart up from the horizon in an aurora borealis, which light it resembled in all respects, except in its place and position, and that this was steady, and altogether without that tremulous kind of motion, which usually accompanies that. Besides Saturn, Mars, Venus, and the fixed stars, there was then no other light in the sky, nor the least cloud, nor any of that horizontal blackness which we see northward in the aurora. The stars were as discernible through it, as if nothing had been there. A gentleman who was with the Doctor fancied it to be the tail of a comet; but as neither he nor the Doctor had ever seen one, he gave but little heed to that conjecture: however, he carefully directed a 17-foot glass to all parts of its western extremity, but could discern nothing like a nucleus. When first seen, it extended from about the midway between Aldebaran, and Orion's left shoulder, through Gemini a little under β, and so on through Cancer and Leo, just above Cauda Leonis, till it arrived between Vindematrix and Coma Berenices, where it ended very dilutedly. In about half an hour it grew dim about the middle, where in a short time it separated in two, or rather became quite dark there; then the disjoined parts were more luminous than before; but they too in a little while after grew dimmer, and shortened away, on to their remote extremities, which remained visible the longest; the western one about 9 o'clock, the time of its extinction, being near Orion's right shoulder, and the other near the left knee of Boötes; so that this meteor seems pretty nearly to have accompanied the earth in its diurnal motion, and to have had little or no motion besides. The Doctor looked for this light afterwards, but could find nothing like it.

Of a Calculus making its Way through an old Cicatrix in the Perinæum. By David Hartley, M. A. F. R. S. N° 456, p. 349.

William Jarman, of the parish of Bayton in Suffolk, was cut for the stone about 15 years before, and a large stone taken from him. The patient continued easy for about 4 years after he was cut; the wound was quite healed up, and he made water in the natural way, without any leakage at the wound. In July last, he felt great pain at the place where he was cut, and it was much swelled. It looked black, and a little hole broke open there, out of which the water came; and a stone appearing, the hole grew wider by the force of the water, and his frequently touching it, till at last the stone came away whole. It was broken afterwards by a fall.

As soon as the stone was come away he became easy, and the swelling abated. The wound was afterwards reduced to a small compass; but his water came away through the wound, and very little the natural way. The patient was about 30 years of age. The great end of the stone came away first, which he suffered to lie at the mouth of the wound near a fortnight; but he applied to no surgeon.

Of a Stone, or Calculus, making its Way out through the Scrotum. By Mr. John Sisley, Surgeon. N° 456, p. 351.

Robert Swann, of East-Malling, Kent, a hard working man in the woods, sent for Mr. S. one day to see him. He found him with a large swelling on his testicles; on the upper part of the scrotum, there was a small hole or two, and he told Mr. S. his urine oosed out sometimes. Mr. S. passed the probe in, and found a hard substance, which seemed to be large, he told him, he had a large stone lodged there, at which the poor man was much surprised. He said he would make an incision and take it out; but he refused to be cut. Mr. S. dilated it in another manner, made the orifice pretty large: the swelling of his testicles assuaged, he went to work, as usual; about a week's time after, coming home at night with a large bundle of wood at his back, he found himself more in pain than usual; as soon as he got home, he complained to his wife, and told her he was very much in pain, he went to-bed, and desired Mr. S. to be sent for immediately; but before he could get to him, the stone forced its way out; and as soon as he came, the poor man seemed much rejoiced, and told him, (as he expressed himself) the Swan had laid an egg: its weight at at first was ℥v and ʒij, now almost 4¾ oz. This man lived about 7 years after, in a good state of health, and lived to the age of 60 or upwards. He said the stone had been growing there for near 30 years; but he never apprehended it to be a stone, though he used to complain of a weight, as if it were half a pound, carried between his legs.

Account of the Petrifactions near Matlock Baths in Derbyshire; with Conjectures concerning Petrifaction in General. By Mr. Moreton Gilks, F. R. S. N° 456, p. 352.

In the mountainous part of Derbyshire, about Cromford, is a valley of at least a mile and half long, walled on each side with high craggy rocks; the east side cliffy, the west more reclining, but extremely rough and difficult of ascent; being composed of large loose pieces of the lime-stone rock, of 5, 10, or 20 ton weight; that seem some time to have broken off from the top of the cliffs, and fallen down into the valleys. At the bottom of the valley, which seems to be a great gaping fissure of the rock, runs the river Derwent harshly along its rocky bottom. About the middle of the valley, at near 50 feet perpendicular height from the river, issue forth several rivulets of a lukewarm water, falling into the Derwent below. Some of this water, being collected in a reservoir, on account of its agreeable warmth, has of late years been much used for bathing, and is called Matlock Bath. Now for about the compass of 500 or 600 yards, near where this water gushes out, the stone appears of a very different texture and complexion; and proves, on examination, to be a perfect incrustation, formed on the original rock; composing a fictitious stone of earth, vegetables, &c. of various kinds, such as usually grow in rocky places, as polypody, tricomanes, and other species of the capillary tribe, mosses, brambles, ivy, hazle, &c. There are several large grottos, at about 15 feet above the level of the river, lined most curiously with the stalactitæ, lapides stillatitii, &c. Some of them nearly resemble large bunches of grapes, and other clusters of fruit, and very beautiful. The farther you penetrate into this mountain, the closer and more compact the stone appears; the interstices in the petrified matter being at the depth of 15 or 16 feet, almost filled up, and nearly as solid as the lime-stone, of which the original rock is composed; and even within 4 or 5 feet of the surface, though very open and porous; yet is it so hard, as to be used in the building about the bath.

The mountain in several places jets out almost over the brink of the river; under these protuberances are the grottos, very dangerous and difficult to get at; but it is impossible to give an idea of the natural beauty of the place. The frost-work, and incrusted plants, are some of them so very delicate and tender, as to make it impracticable to bring them away with half their beauty, by the most careful conveyance. In one place there is an ivy creeping along the rock, part of it entirely petrified, another part only incrusted, and a third still vegetating. In another place is a hazle-tree, its root composing part of this petrified mountain, the branches some petrified, and some tenderly incrusted. As these are changed, others spring up, and in time will undergo the same fate. In short, nothing in nature can give a more clear idea, or more beautiful representation, of the whole business of petrifaction, than a curious observer will see, and frame in his mind from this mountain. He will see, that not only the water, as it distils out of the rocks, is capable of incrusting and petrifying the bodies it meets with in its passage, but that even the streams and exhalations, being highly saturated with these mineral particles, will produce the same effect; as is evident in the place under consideration, and will generally best account for the supply of petrifying matter, brought to fill up the vacuities left by the decay and waste of vegetables incrusted over; and which in course of time are constantly filled with it. For though the water of some springs may be so loaded with mineral matter, as, perhaps by penetrating the pores of wood and other lax bodies, to increase greatly their specific gravities; yet surely it is contrary to the laws of matter, and absurd to say, there is any hidden property in such waters, capable of changing the parts of one body into another, specifically different. It may in time, no doubt, lose its texture and coherency, by the admittance of heterogeneous particles of different attractions; but the cause of coherency in the parts of the original body must entirely cease, and be dissolved, before it can be said to become a part of any other body whatever. Afterwards, indeed, the space that was possessed by the parts of the original body, may be supplied by those of the new one, so as to make in time a uniform stone, in the shape of the original plant: but if this petrified plant be still kept in the place where the same petrifying quality continues to act upon it, it will lose even that shape, and become a part of the body it is contiguous to; and so a great many of these petrified plants, and other bodies united together, will compose large masses, and whole strata of stone. This is clearly the case in the instance now before us, and perhaps it might be carried so far as to strengthen our conception about the general formation of the strata of lime-stone or marble; that appearing to be every where, (notwithstanding Dr. Woodward dispatches them much more expeditiously) but especially in the Peak of Derbyshire, such a petrifaction as above described, quite finished. I could urge many reasons for my supposition, but I will not trouble you with them here, the compass of this letter not permitting me; nor do I know how far such conjectures are capable of being used, with regard to the received opinion of the world's age; but if we had as good authority to suppose it 60,000 years old, as we have 6000, it would be worth the while to trace the origin and source of these petrifying exhalations a little deeper than seems to have been done by Dr. Woodward; and might either perfect his history, or produce a more rational system of the earth, than has yet appeared.

You will find, among the things now sent, some land-coral found in a lime-pit, where is a great quantity of it, between two strata of lime-stone, of at least 3 feet thick. Also some few pieces of pseudo-sapphirus, and other kinds of spar; such as were picked out of the fissures of the rocks, above-described. There is a vast variety of these things in the peak, much greater than has been noticed by any one.

*Concerning the Smut of Corn. By the Abbe Pluche.** N° 456, p. 357.

The Abbe having passed some months in the country, where he had the satisfaction to read in the great book, nature, which far exceeds all our libraries; he made several observations, among which are the following:

1. Having with the assistance of the microscope viewed the smut of corn, he observed the stalks were all spotted and pricked with small burnings: now as the smut happens after a fine rain, followed by a bright sun-shine, the cause of this evil is, that the focus of those very small drops is just near them, and on the stalks that supports them; therefore the sun's rays, collected in this point, must there burn; which dries up the stalk, and prevents the ear from graining.

The second remark is on the corn that grows up into ears, the grains of which are for the most part full of meal quite black. With the microscope he saw, all round or above these black grains, small long bodies, rolled up, and having each a pedicle; which he found to be the flowers, that could not reach their due form, or come forth and ripen; so that the grain, being deprived of this help, could not develope its germ, but produced only a black meal, for want of the unfolding of certain vessels.

The third remark is, the reason that invites thrushes or starlings under the legs of black cattle grazing in a pasture. Not being able to get near them, he observed them at a distance with a good glass. He saw all these birds thrust their head and half their body down into the grass, in such manner that their tails remained erect in the air, as that of a duck when diving; which made him think, that those birds seek after worms in the earth; and that they gather about the cattle, because as they are large animals, by trampling on the ground, they oblige such worms to come forth, as happen to be pressed under the weight of their hoofs.

* Author of the Spectacle de la Nature, and some other popular works. He died in 1761, aged 73.

Concerning a Cluster of small Teeth observed at the Root of each Fang, or great Tooth, in the Head of a Rattle-snake, on dissecting it. By John Bartram, M. D. of Philadelphia. N° 456, p. 358.

Near German-town, about 6 miles from Philadelphia, Dr. B. found a rattle-snake, which is now become a rarity so near our settlements. He took it home, and dissected it; in the head he met with what has not been observed before by any, viz. a cluster of teeth on each side the upper jaw, at the root of the great fangs, through which the poison is ejected. In the same case, that the two main teeth were sheathed in, lay 4 others at the root of each tooth, in a cluster together, of the same shape and figure with the great ones, and he thinks for the same use and purposes, if by accident the main teeth happen to be broken. May not these clusters of teeth be placed to supply such a defect successively, for the support and defence of this creature?

Notices of some Meteors observed at Philadelphia, in North America. By Joseph Breintnall. N° 456, p. 359.

The remarkable aurora borealis, that was seen in Europe the beginning of December, 1737, was not seen here. But we had a visible aurora borealis the 29th of December 1736. The day was clear, with a brisk cold north-west wind, the evening calm and serene, and about 7 was a red aurora borealis.

On Nov. 17, 1737, about sun-set, many people in this town saw a fiery meteor in the air, large and bright; it seemed in the zenith, and so it seemed to others some miles from town: it was observed to be higher than the lower clouds.

Dec. 7, 1737, a minute or two before 11 at night, were two shocks of an earthquake, greater than ever felt here before. The second evening after, and for several evenings in this month, a red vapour appeared to the south and south-west, like the aurora borealis.

A Description of the Cave of Kilcorny, in the Barony of Burren in Ireland. By Mr. C. Lucas, Apothecary, Dublin. N° 456, p. 360.

The place where this cave lies, is called Kilcorny; it is a pretty low valley, in comparison to the hills that surround it; the entrance is by the east end, for it lies east and west. There are the ruins of an old church, and, a little westward of it, an even plain of about an acre of ground; on the north side of which, under a steep rugged cliff, lies the cave.

The mouth of it is level with the plain, about 3 feet diameter; it has been much larger, but was blocked up with lime and stone, which plainly appears still, but to what purpose is not known. Within this narrow entrance, it grows much wider and loftier. The floor is a pretty even rock, from 2 to 4 or 5 yards broad: the sides and top are rugged and unequal, from 6 to 12 or 14 feet high.

About 40 yards from the door, there is a pretty deep pit, 7 or 8 yards over; but, when passed, the floor is plain and even, as before, for about 200 yards, which is the farthest that any one known has ventured into it. Most people that have gone into it, went by a thread or clue; others have carried a bundle of straw, and dropped it by the way, to guide their return; which seems altogether unnecessary, there being no windings or chambers throughout of any extent. It is all over, even in the depth of winter, as dry as any place of the kind under ground can be; and, what seems very strange, it often pours forth such a deluge as covers the adjacent plain, sometimes with above 20 feet depth of water.

The times of its overflowing are uncertain and irregular; sometimes it does not happen above once in a year or two, but most commonly 3 or 4 times a year; it is sometimes observed to succeed great rains and storms, though it often happens without either. The neighbouring inhabitants are alarmed at its approach, by a great noise, as of many falling waters at a distance; which continues for some hours before, and generally all the time of the flood. The water comes forth with extreme rapidity from the mouth of the cave, and likewise from some smaller holes in the low ground, attended with a surprising noise; it flows for a day or two, and always returns into the same cave, and partly into the small holes, from whence it was observed to come before, but with a more slow and tardy course. The water is of a putrid quality, like stagnated pond-water, insipid as spring-water. It always leaves a filthy muddy scum on the ground it covered, which greatly enriches the soil. It has been known sometimes to overflow and ebb in 6 or 8 hours time, but in a much less quantity.

There is neither river nor lake any where in that part of the country, and it is above 6 miles from the sea. There are very near it several much lower valleys, in which there is no appearance of water, unless a little rain-water collected in a pit, in the fissure of a rock, or the like.

Of an extraordinary Tumour on the Thigh. By Mr. Mizael Malfalguerat, Surgeon, St. Edmund's-bury. N° 456, p. 365.

Grace Lowdell, a woman of the parish of St. James, in Bury St. Edmund's, Suffolk, aged 60, was naturally of a gross, fat, and relaxed constitution, and constantly given to drinking strong liquors; she laboured for many years under an ill habit of body, as the rheumatism, which had caused a contraction of some of her fingers, with some nervous affections in her head, often causing some fits of vertigo, &c. And though she had formerly some child-bearing weakness, viz. a procidentia uteri, yet there could not be found any other scrophulous symptoms, than that she observed, when about 30 years of age, soon after her delivery of a son, a little hard swelling on the muscle biceps, and posterior, inferior and external lateral part of the thigh, a little above the ham, without her knowing any cause of it; which at first went on slowly, but after proceeding more quickly, till it increased to the bulk of near a foot in circumference, being somewhat of a globulous and a little longish figure from its basis, which was lax, like a peduncle, or stalk, and about half the circumference of the tumour, like a neck to the head of a child hanging down.

From the first appearance of this tumour to the excision of it, there were more than 30 years; she had excessive pains and uneasiness in it, and at last its bulk and weight had in some measure intercepted the nourishment to it, so that an ulcer had affected the inferior part of it, very putrid and sinuous, of about 6 months standing.

Mr. M. wished to have made a total extirpation of this excrescence; but being near large vessels, and among the tendons of the muscles, he was content, as Dr. Turner advises, "To level it, by escharotics, repeated as the sloughs throw off, till as much of the gland or substance shall have been consumed as may be safely adventured: when some powerful desiccative may induce a cicatrix," &c. &c.

Therefore, July 7, 1735, Mr. M. made a ligature about its basis, with a slip-knot, which he gradually constringed once or twice a day, as the patient could suffer it, without causing any ill symptoms, till the 17th of the same month, when she was taken with strong convulsions, a slow fever, syncope, her teeth set in her head, and a loss of her senses, which lasted that whole day and the night following; from which time he did no more constringe the tumour, but prescribed cordials, volatile drops, a purging enema, and a paregoric draught at night, which had so good an effect, that by the next day she was much recovered, and came to her senses. The ligature began to make a separation in the neck of this preternatural sprouting excrescence; and on the 20th he extirpated the whole outer tumour, without any great hæmorrhage. He was induced to use the ligature, in order to prevent the too great effusion of blood, which might otherwise have happened, thinking it not very safe to make a ligature of the body of so large an artery as is in the ham, from fear of intercepting afterwards the nourishment to the leg, as happens often after the operation of the aneurisma.

The remains, though sordid at first, by a peculiar method of dressing, and proper applications of strong digestives, detersives, &c. cleansed, and the ulcer soon digested, the substance came even to the skin, and, September the 21st, it was all perfectly cured, without any hardness, or any inconvenience to her walking, and was likely always to remain so.

On a Remarkable Aurora Borealis. By Mr. James Short, College, Edinburgh. N° 456, p. 368.

On Saturday last, Nov. 13, 1736, about 6 o'clock, there was one of the most remarkable auroræ boreales that ever Mr. S. saw. At first there appeared the ordinary luminous arch, the vertex of which was about 30° above the horizon, and had its centre somewhere in the meridian circle. After this was perfectly well formed, there appeared little or none of the purple and red colours which are usually in that arch; but immediately there broke out, from the western extremity, a great deal of that northern light which formed this arch, and, rushing along with rays directed to the zenith, formed another aurora borealis above the first, the centre of which was to the east of the meridian. After this was formed, there followed, from the same extremity, a great deal of purple and red-coloured light, quivering and shaking towards the zenith, with a flapping noise in rushing along, till it formed a third aurora borealis, above the second, the centre of which was somewhere on the east-side of the meridian. Looking again to the western source of these arches, he perceived, as it were, a huge pillar of a dull red-coloured light, rising out of the same place whence the arches took their beginning, extending in a direction towards the zenith, till it rose almost 60° high. These arches and the pillar lasted very near an hour; the 2 uppermost arches were continually quivering and shaking, and the pillar always turning to a paler red.

The night before the aurora borealis, there was an amazing hurricane of wind, which lasted till the Saturday morning; and all that day it continued to blow, though not so hard. The arch from whence the wind blew, was from the north-west, the same quarter from whence the arches took their rise. To the 18th, ever since the hurricane of wind, there has been a most intense frost. It froze so hard, that in less than 24 hours after it began, the lake on the north-side of this city was so strong, as to bear people on it.

Of an Extraordinary Exostosis on the Back of a Boy. By Mr. John Freeke, F.R.S. N° 456, p. 369.

There came to St. Bartholomew's Hospital a boy of a healthy look, and about 14 years old, to ask what should be done to cure him of many large swellings on his back, which began about 3 years since, and had continued to grow as large on many parts as a penny-loaf, particularly on the left side. They arose from all the vetebræ of the neck, and reached down to the os sacrum; they likewise arose from every rib of his body, and joining together in all parts of his back, as the ramifications of coral do, they made, as it were, a fixed bony pair of bodice. Mr. F. considered this as an extraordinary case of exostosis. It is added that the boy had no other symptoms of the rickets on any joint of his limbs.

An Account, by John Eames, F.R.S. of a Dissertation, containing Remarks on the Observations made in France, to ascertain the Figure of the Earth, by Mr. Celsius, entitled, De Observationibus pro Figura Telluris determinanda, in Gallia habitis, Disquisitio. Auct. And. Celsio, Upsal, 1738, 4to. N°. 457, p. 371.

That the figure of the earth is spheroidical is agreed on by all: but whether it be an oblong or oblate spheroid, i. e. whether the axis be longer or shorter than a diameter at the equator, has been for some time a matter of doubt. Three several methods have been proposed to determine this controversy by experiments; as, by the different lengths of pendulums vibrating seconds, in different latitudes; by the figure of the earth's shadow in lunar eclipses; and by the actual measurement of the lengths of a degree on the meridian in different latitudes.

It is certain, if the lengths of the degrees of latitude decrease as we go from the equator towards the poles, then the axis is greater, and the figure an oblong spheroid; but, on the contrary, if these lengths increase as we remove towards the poles, the axis is less than a diameter at the equator, and consequently an oblate spheroid.

Mr. Cassini and others, judge the earth to be of an oblong spheroidical figure; and the observations made in France, if entirely to be depended on, prove this hypothesis to be a matter of fact. Our late illustrious president, Sir Isaac Newton, Mr. Huygens, and others, make the earth to be an oblate spheroid, higher at the equator than at the poles; and this figure of the earth is undoubtedly the true one, if the observations lately made near the arctic circle be admitted as certain and exact. So that since both sets of observations have been taken by persons of known skill, dexterity, and integrity, it is now become absolutely necessary to inquire into this matter, to find out the occasion of so great a difference in their conclusions.

Mr. Celsius, in the treatise before us, proposes to consider this matter more closely, and begins with a defence of the observations made at Tornea, near the north polar circle; and then takes notice of some things, proper to be considered, relating to the instruments, astronomical observations, and trigonometrical operations, performed in France; which, in his judgment, render the observations uncertain; at least so far as not to be accurate enough to be depended on, in determining the matter in question.

To begin with the defence of the observations made at Tornea: perhaps it may not be improper to premise a short account of them. They were undertaken at the charge of the king of France, by 5 skilful gentlemen; three of them members of the Royal Academy at Paris, who were joined by Mr. Celsius and the Abbe Authier. The trigonometrical part of the work was performed near the river of Tornea, which is in the direction of the meridian of Tornea; the coasts of the gulph of Bothnia being found very inconvenient for that purpose. By the favourable situation of five mountains, they formed 8 triangles, which took in space enough for their design. All the 5 gentlemen observed, one after another, each angle of these triangles, setting them down in writing separately.

They afterwards determined the distance between Tornea and Mount Kittis, under the same meridian, by a basis, measured on the river when frozen over, the length being 406 toises 5 feet by the first measurement; and when measured again, it was barely 4 inches over. This distance between them they found to be 55,234 toises.

The first part of their work being thus finished, the next was to find the difference of latitude of these two places. This they did by the help of a telescope, fixed to a sector of 9 feet, made at London, by the care and direction of Mr. George Graham, to whom the lovers of astronomy are indebted for the curious and well-contrived instruments he has supplied them with. The star they observed at Tornea was α Draconis. They repeated their observations 3 times, and the greatest difference between them was only 2 seconds. Removing to Mount Kittis, they took the same number of observations, of the same star, without finding more than one second difference. The result was, that the amplitude of the arch, in the heavens, between Tornea and Mount Kittis, allowing for the precession of the equinox, and the time elapsed between the two observations, according to Mr. Bradley's theory, was 57′ 26″. Hence the magnitude of a degree, on the earth, intersecting the polar circle, was found to be greater than a mean degree of France 377 toises; and to differ 900 toises from what it should have been, according to M. Cassini's hypothesis. And if the correction according to Mr. Bradley's theory, were omitted, the difference would have amounted to above 1000 toises: the consequence of which, say the curious observers, is, that the earth is not only flatted towards the poles, but that it is much more so than Sir Isaac Newton or M. Huygens thought it. * This unexpected difference being so very great, made them resolve on a careful, as well as new kind of verification of the whole. In the first place, they repeated their astronomical observations 3 several times, at Tornea and Kittis, with the same instrument, but on another star, viz. δ Draconis. The difference of latitude between the two places was found to be the same, within $3\frac{1}{2}$ seconds, with the first. They then not only examined the truth of their meridian line, the exactness of their sector, in the different divisions on the limb, chiefly in the two degrees employed in observing α and δ Draconis, but supposed that, in their trigonometrical operations, they had erred in each triangle, by 20 seconds in each of the two angles, and 40 seconds in the third; and that all these errors tended to diminish the length of the arch; the calculation, on this supposition, gives but $44\frac{1}{10}$ toises for the greatest error that could be committed.

When a particular account of all these observations was read before the Royal Academy of Sciences at Paris, and inquired into; the main exception taken to them was, that the observers, omitting to make a proof of the line of collination, by means of double observations, with the face of their instrument turned contrary ways, have not duly ascertained the truth of their observations. But this objection was fully answered by M. Maupertius, as Mr. Celsius hopes and believes, to the entire satisfaction of M. Cassini, who made it. He allows M. Cassini had very good reason to mention this, as a thing proper to be done in instruments of common use, for this purpose, which generally stand in need of such a method of verification. But it was not at all necessary in the instrument used at Tornea and Mount Kittis: the very make of it was such, that no alteration could easily be made in it, so as to create any perceptible error in the observations. The whole apparatus of the telescope and sector is all framed together; the object-glass and cross-wires, as well as the limb, so firmly fixed to the tube, as not to be dislocated without great violence. Besides, the utmost care was taken in transporting it from one place to another; being placed in a chest, that the Laplanders, to use his own words, in illa cista idolum quoddam servari facile sibi persuaderent. He adds, the same objection may be made to M. Picard's observations, who does not seem to have used this precaution,

* This happened in consequence of certain errors committed, notwithstanding all that is here said to the contrary. See the note at p. 207 of this vol. of these Abridgments.

as M. Cassini himself acknowledges, who nevertheless approves and extols his observations for their accuracy: so that those at the arctic circle may be very good, notwithstanding the want of this, supposed necessary, operation. And indeed, that they were so, sufficiently appears from this fact—The difference of latitude between Tornea and Mount Kittis, found in September, was observed again in March following, by the help of the same star δ Draconis, and did not differ from the former above $3\frac{1}{4}$ seconds, though the instrument had been twice carried from one place to the other. This is a degree of exactness not easy to be met with; no not in M. Cassini's observations, made on different stars, which differ sometimes 40″, in determining the amplitude of an arc in the heavens, though the instrument was carefully examined in the way above mentioned.

The author then proceeds, in his turn, to inquire into the accuracy and certainty of the two sets of observations made in the north and south parts of France, in respect of the royal observatory at Paris.

As to the measures of the degrees in the northern parts of France, between Paris and Dunkirk, he owns that they cannot be much out of the way; being in some measure confirmed by M. de la Hire, in the year 1683, and by M. Cassini himself. Yet Mr. Celsius observes, that the basis on the sandy plain shore, near Dunkirk, when measured again, differed three feet from the former measurement; which is a much greater difference than that M. Celsius and the other gentlemen found, in measuring a much longer line twice over, which was but 4 inches.

As to the astronomical observations taken by the 6-foot sector, whose limb of 12 degrees was divided only at every 20″; it is true, M. Cassini examined the instrument several ways, at Paris, after his return thither; but that a correction, owing to the change of centre, might be safely applied to the observations at Dunkirk, the examen of the centre should also have been taken at Dunkirk; it being uncertain, whether this alteration or aberration of the centre, was caused by the journey to or from Dunkirk.

The difference of 41″ between the observations taken to settle the true measure of the arc of the heavens, seems to be enormous. Perhaps the stars were not lucid enough to be well observed by the 3-foot tube; but might they not, for a due degree of accuracy, have been viewed through the 9 or 10-foot telescope?

Our author prefers the observations of 1719, made after the return to Paris, to those made before; because made at the same time of the year with those of Dunkirk, and so not standing in need of Mr. Bradley's correction: though this caution perhaps may be thought not necessary here, where the errors of the observations are greater than the correction itself. Mr. Celsius remarks further, if the difference of latitude between Dunkirk and Paris be supposed to be 2° 12′ 12¼″, which is a mean between 4 others he mentions, the length of a degree will amount to but 56,395 toises. And if the observations at Malvoisine and Amiens, be counted according to Mr. Bradley's theory, for the interval of a month between the observations, the length of a degree will come out to be 56,926 toises: which is 135 toises less than the length of a degree, found by measuring the whole length of France; and 134 less than that of Mr. Picard, so highly approved of by Mr. Cassini, as confirming his own.

Mr. Celsius having finished his remarks on the observations made in the north part of France, extending from Paris to Dunkirk, proceeds to examine those taken in the south, from Paris to Collioure, near the borders of Spain, and the Pyrenean mountains. By the former, a mean degree was found to consist of 56,960 toises, by the latter 57,097; and consequently the earth is an oblong spheroid.

Mr. Celsius, in examining these observations, which were taken under the conduct and direction of the late M. Cassini, in 1700, first considers the structure and goodness of the instruments used; then the accuracy of the astronomical observations for finding the difference of latitude; and, in the last place, the trigonometrical operations for determining the distances of places; especially the two extremes under the same meridian.

The principal instrument M. Cassini carried with him, was, a limb of 12 degrees, whose radius was indeed 10 feet, but divided only into degrees and minutes; the other parts were added to it at Perpignan. Here Mr. Celsius observes, that the finding the true centre of this limb was, and still is, a very difficult and troublesome problem to a good artist; that no mention is made, whether the position or place of this centre, and the divisions of the limb, were ever examined at Paris or Collioure, though the carriage of the instrument through so long and rough a way, could not but make some alteration in the place of the centre.

It is true, the zenith distance of Capella, taken by it at Paris, was confirmed to be right by another instrument; but it cannot be concluded that the zenith distance of the same star, taken at Collioure by this instrument, and not confirmed there by another instrument, must be true also. For the point of division, answering to this distance in the limb, was not examined; and a centre wrong placed may by accident give the true zenith distance, viz. when the true and erroneous centre happen to lie in the same perpendicular to the horizon.

The exceptions taken to the astronomical observations, for finding the difference of latitude between Paris and Collioure, are, in the first place, that though 5 stars were observed at Collioure and Paris, yet one only was made use of, viz. Capella. That the difference of latitude by Capella is 6° 18′ 57″. If Lucida Lyra had been used, the difference would have been but 6° 17′ 7″; but by the right shoulder of Auriga, 6° 19′ 25″. Hence arises the uncertainty or difference of 2′ 18″ between the greatest or least of their observations; that the late M. Cassini makes the difference 57″ less than M. Cassini, who accounts for this difference from the observations being taken by an ordinary instrument; but the instrument is the same which was used to take the altitude of the pole of Amiens, which was very near that found by Mr. Picard.

As to the trigonometrical operations for finding the distance of places, M. Celsius thinks they labour under considerable uncertainties; not only on account of the many difficulties they met with, viz. the mountainous countries, want of proper signals, &c. so that convenient triangles could not be formed; but add to all these, that several of the triangles had but two angles observed, and some of these angles too acute; whence, as M. Cassini himself very justly observes, in his examination of Snellius and Riccioli's observations, great errors may arise. M. Picard thinks all angles less than 20° ought to be avoided; as also that the triangles should be contrived so as to have sides of a due length, neither too great nor too small. Then follow 16 triangles, wherein one or more of these inconveniencies are to be found.

It may be said, the whole of these observations and measures of M. Cassini seem to be sufficiently confirmed, if not ascertained; since the principal base in Roussillon was found, when computed, to differ but 3 toises from the same as it was actually measured; and that, after some due corrections, it was made to agree with the greatest exactness. M. Celsius replies, why are we not told what those corrections were, that we may see whether they were really necessary or no? Why were they not taken notice of in the calculations of each triangle? Besides, the real length of the base, or the fundamental line in Roussillon, is not fully ascertained, it not being measured more than once; whereas that at Dunkirk, and that of M. Picard, were measured twice; and there was more reason for doing so here than at Dunkirk, on account of the uneven and almost ever-changing shore in Roussillon, from the restless overflowing sea.

The great number of the triangles, joined with the numerous small errors of the angles, is another ground of uncertainty; for the errors in the angles, though small, may make the distance of the parallels, of the two extreme places, greater than it ought to be; and yet the principal sides, that is, those that are made bases to the following triangles, continue the same. This made it necessary to verify the sides, at least at every second degree, by measuring the principal base twice over with due care; which might have been done, and therefore should have been done, in a matter of so much nicety, as an attempt to find the difference between 2 degrees so near one another, under the same meridian.

To show what bad consequences may arise from small errors committed in observing the angles of several triangles, Mr. Olaus Hiorter, a curious and ingenious friend of Mr. Celsius, has taken the pains to form the triangles of M. Cassini between Bourges and Collioure; so that the distance between their parallels shall be considerably lessened; and yet the base in Roussillon, found by computation, shall not, after due correction, differ sensibly, if at all, from the same actually measured. In consequence of this, Mr. Celsius concludes with observing, that the distance between the Royal Observatory, and the perpendicular to the meridian of Collioure, deduced from the triangles of Cassini, corrected after Mr. Hiorter's Method, &c. will amount to but 358,980 toises. This, divided by the mean difference of their latitudes, 6° 19′ 11″, will give 56,803 toises, for the length of a degree, one with another, between Paris and Collioure, which is less than the length of a mean degree found by M. Picard, and pretty near the truth: so that the degrees decrease as you go towards the equator; and consequently the earth is higher at the equator than at the poles, as Sir Isaac Newton and Mr. Huygens believed.

The distance of the parallels of Paris and Collioure, by this method, is indeed less than that computed by M. Cassini; but this cannot reasonably be complained of, since these computed measures of M. Cassini seem very capable of being lessened; and it is no more than what M. Cassini himself has done to the measures published by his father, which he has shortened by 325½ toises. But however that matter be, whether this particular correction of M. Cassini's distance, and consequently length of a mean degree, be admitted or not, Mr. Celsius is fully persuaded, on the whole, that he has made it plain to every unprejudiced reader, that these two sets of observations in France, are not taken with such a degree of exactness, as to be depended on, in determining so nice a matter, in dispute for 50 years, as the true figure of the earth; which was the thing proposed to be done by them.

Concerning a Place in New-York for measuring a Degree of Latitude. By Mr. J. Alexander. N° 457, p. 383.

The mention of the French endeavours, to discover the figure of the earth by observation, put Mr. A. in mind, that a very exact observation for that purpose might be made here, because Hudson's river is frozen over, from New-York up to Albany, and its course is very straight, almost due north, and the distance between New-York and Albany is above 150 miles; New-York is in latitude of 40° $40'$, nearly; so that the length of above 2° of latitude on the earth might be measured here, with much more exactness than it was possible in England or France, because of the ascents and descents, and curved lines, which they would continually be obliged to make allowances for. From all which difficulties the mensuration here on the ice would be entirely free.

On the Antiquities of Prussia. By James Theodore Klein, Secr. to the Republic of Dantzic, and F. R. S. N° 457, p. 384. *From the Latin.*

This paper relates chiefly to a kind of antique, copper bracelet, dug up in Prussia, and supposed to have been buried with its owner, some noble personage. It consists of twisted elastic copper wire, coiled into the form of a helical spring, of about 7 coils or rounds.

This opinion is confirmed by the learned Bartholin, who gives a figure of a bracelet, composed of several rings connected together, from the museum of Olaus Wormius; and calls it a monument of stupendous antiquity, worthy the memory of posterity.

On account of affinity with the bracelet, M. Klein adds a silver ring, found about a year before in a Prussian urn. It had threads twisted together in like manner, to form the jewel; the rest running out into two ends, not joined, but only lying close together, and forming a circle; so that it would, by its elasticity, fit either a larger or smaller finger.

Observations and Experiments with Madder-Root, which has the Property of tinging the Bones of living Animals of a red Colour. By M. Du Hamel du Monceau, F. R. S. &c.* N° 457, p. 390.

The fact of the bones of hogs and fowls coloured very red, by feeding on

* Henry Louis du Hamel du Monceau may be numbered among the most active and useful members of the French Academy of Sciences during the 18th century. The subjects to which he particularly directed his attention were, 1. Agriculture; including the cultivation of trees, the management of forests, and the preservation of timber; the preservation of corn, &c. 2. The Arts, in the descriptions of which, published under the superintendance of the Parisian Academy of Sciences, and illustrated by an expensive set of engravings, he bore the principal part. 3. Naval Architecture and the fisheries, with other matters relative to the marine, of which he was inspector. He died in 1782, aged 82. His treatises on the various subjects abovementioned, amount to a great number of volumes, most of them adorned with plates.

food mixed with the juices of madder-root, as related in the Philos. Trans. N° 442 and 443, having been communicated to the Royal Acad. of Sciences at Paris, M. Du Hamel du Monceau undertook to make a variety of experiments of the same nature, which he did with the same effect, on many animals.

This however it seems is not a new discovery; for Mizaldus, in a work published in 1566, with this title, Memorabilium, utilium ac jucundorum Centuriæ novem, Cent. 7, has these words: "Erythrodanum, vulgo Rubia tinctorum dictum, ossa pecudum rubenti et sandycino colore imbuit, si dies aliquot illud depastæ sint oves, etiam intacta radice, quæ rutila existit, &c."

First, M. Du H. took 4 strong pullets, which he shut up in coops; and fed them with a paste made of wheat-meal and powder of madder-root; and gave them an infusion of the same root to drink. The first days they eat their paste pretty well; but afterwards disliked it much, and eat always less and less. As to the infusion of the rubia tinctorum, they never would drink it, and he was obliged to give them pure water, which they drank plentifully; for this root made them thirsty. At the end of some days they could not relish the mixture, of which they eat but very little, and wasted away visibly.

On the 10th day, one of them died; and another 2 days after: and both of them had their bones tinged of a rose-colour. To prolong the lives of the other two, he diminished the dose of the madder, and sometimes he gave them the paste without it. The root had already produced its effect; for notwithstanding the new regimen, they continued to waste; which obliged him to kill the third 5 days after the death of the first 2. The colour of its bones was not different from that of the 2, which died 5 days before. As to the 4th pullet, which seemed not quite so sick, he set it at liberty. It recovered by degrees, by choosing food to its taste in the yard. But at the same time the tincture its bones had received, went off gradually, and almost entirely disappeared in a month's time.

He next chose some strong young pigeons. Two of these had no other food given them but wheat-meal, others were fed with the meal and madder mixed and made into pellets, of a convenient size, given them 3 times a day till their crops were full. But they could never be made to drink the infusion of the madder. The two young pigeons fed with the meal alone were lively and fat, digested their food, and throve as well as if fed by the old ones. But on the contrary, those that were fed with the paste of meal and madder, took this food only by force, digested ill, were dull and very thirsty. And though care was taken to keep their crops constantly full, as well as the others, yet they grew leaner daily. They were always shivering, and endeavouring to get into the sun, or near the fire, to warm themselves: and the strongest of them was very sick by the 10th day, and the bones of their wings were already turned red. By a change of diet however, the red colour gradually decreased and vanished.

All these creatures, that had been fed with the mixture, were dissected; and the following observations made on them. Neither the feathers, the horn of the bill, nor claws, had changed their colour, even where they are inserted into the skin. The skin of the whole body had preserved its natural colour. The brain, nerves, muscles, tendons, cartilages, epiphyses, and membranes, showed nothing contrary to the usual state of these parts. But the long bony tendons, that run along the great bone, which is improperly called the leg of fowls, were red about the middle of their length, which is their hardest part. All the true bones, even to the very thinnest of them, were as red as carmine; and in some places this red was so deep, that they appeared almost black.

In these young birds, all the bones do not take the red tinge alike. The hardest are generally more coloured than those that are tenderer. A difference of this kind is perceivable even in the same bone; for the middle, which has more solidity than the ends, is almost always the reddest. Not but there are sometimes found little pale spots in the part where the red is deepest; and sometimes spots of a very deep red in those parts which have taken but a carnation tinge.

The great bone of the foot, which is commonly called the bone of the leg, was visibly less red than the others. The little bones of the larynx and of the apophyses tinged of a fine red; though these are as small as a thread in young pigeons. The rings of the trachea, which are entirely cartilaginous, had not taken the least tinge; but the ring nearest the division of the trachea was red in these pigeons; and even the first ring of each branch of the bifurcation had in several taken the tincture, in the middle at least of its outside.

There was nothing remarkable in the thorax or the viscera; but the inner membrane of the crop and intestines, especially the large ones, appeared red. Having washed pieces of these crops and intestines, their outer membrane continued white, and the inner, or tunica villosa, only was tinged by the madder. At first sight it appeared as if injected; but on examining it with a glass, it distinctly appeared, that it was not a coloured liquor that was contained in vessels, as in parts injected; but that it was only a sort of fæcula detained in the villose part of these membranes. It is doubtless the adhesion of these tinging particles of the root to the small villi of the inner membranes of the organs of digestion, that is the source of all the distempers with which these creatures appeared to be seized, while fed with the madder. Their crop especially was relaxed and flabby, as if it had been macerated several months in water; it was easily torn, and its inner or villose membrane adhered so little to the others, that it was detached from them in pieces. A certain quantity of this fæcula, being accumulated there, retarded digestion, and those animals died hectic, though with a full stomach.

The eyes of these animals, while alive, seemed as red as those of some parrots. The capsula of the crystalline, and that of the vitreous humour, were of a crimson red, though neither the vitreous humour nor the crystalline were dyed.

The tinged bones being broken, while fresh, or before drying in the air, seemed somewhat larger and fuller of marrow; but also more spongy, or of a looser texture, and easier to break, than the white bones of the pigeons fed with meal only. The parts of these bones that had the least degree of hardness, broke between the fingers, which remained coloured from them; and this tincture does not come from the marrow, which continues in its natural state, like all the other soft parts. The same parts in the white bones were not to be broken in this manner.

On viewing these bones with a good glass, their smoothest surface appears bored with a vast number of small holes, in which the colouring fæcula is perceived. And with a microscope that magnifies still more, there appears a sort of net-work of fibres, which divide, and reunite, to form this net. Under the first order of this net-work, which appears white, another is seen somewhat red, and under this a third and a fourth, still deeper coloured; in fine, the ground under all these reticular strata is of a very deep red; and the whole may be justly enough compared to a piece of wood stripped of its bark.

In other experiments, some young pigeons, fed with the paste mixed with madder, died the third day; yet all that had the consistence of bone in their skeletons, was become as red as scarlet. Mr. Belchier was surprised to see the bones of his cock tinged red in 16 days, yet here are bones so coloured in 3 days. But all that should in course of time have turned to bone in one of the young pigeons, and as yet was but cartilage, as the epiphyses, the great apophysis of the sternum, &c. had not taken the least colour. In the other, there were some spots of a very weak red on the cartilage of the sternum, which probably began to ossify. Other experiments, since tried, have taught with greater certainty, that the cartilages in general are not tinged red by the madder, except when they begin to acquire the consistence of bone.

It appears that the bones of animals that are still growing, are dyed better and quicker than those of full-grown animals. Two turkeys had the same ailments with the pullets of the first experiment, they fell into a decay like those, and they were obliged to be killed in 15 days.

Here we see young pigeons, whose bones were dyed of a fine carmine-red in 3 days, which is nearly the time they must have for acquiring this degree of tincture. By other experiments on young pigeons of the same age, in 36 hours their bones were of a lively rose-colour, and in 24 hours they were at least of a flesh-colour.

These last experiments prove with what expedition the distribution of the nutritious juice is performed in animals of this kind, which acquire all their growth in a few months; and how rapid the distribution is, even in those parts where the blood's circulation meets with the greatest obstacle, as in the substance of the bones.

The rubia probably is not the only vegetable substance that can change the colour of the bones; and yet the log-wood, the anchusa and curcuma, have been tried without success. Probably it must be a substance less susceptible of alteration; and it is well known, that the rubia is of that sort, seeing that clothes dyed with this root bear very well the action of the air, and of boiling.

M. du H. put the coloured bones of the animals to several proofs; first, as Mr. Belchier, to that of boiling water, and of spirit of wine, without the least change of colour. It also resisted soap-suds. A strong lixivium of salt of tartar discharged a little of the colour, and made it look brighter. Vinegar made it take a yellowish brown and obscure tinge. In fine, alum-water discharged the colour pretty considerably, and the water remained somewhat vinous. Thus these bones perfectly well resist the same boilings as the clothes dyed with the same root; but the air acts on them much sooner than on these clothes; for the bones of the pullets in the first experiment, those of the turkeys in a third, and those of the young pigeons, that had eaten of the madder but 3 days, became entirely white in less than a year, and the reddest bones lost much of their colour. And he thinks, that the dew, to which he exposed some of them for a few days, will finish the blanching of them.

A Catalogue of fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1739, *pursuant to the Direction of Sir Hans Sloane, Bart. By Isaac Rand, F. R. S.* N° 457, p. 406.

This is the 18th presentation of this kind, completing the number of 900 different kinds of plants.

A Physico-mathematical Demonstration of the Impossibility and Insufficiency of Vortices. By M. de Sigorgne. N° 457, p. 409.

M. de S. here takes great pains to refute the existence, and even the possibility of the Cartesian vortices. He undertakes to prove, 1. That the mechanical formation of a vortex is impossible; 2. That a vortex, though once formed, cannot be lasting; 3. That it is not sufficient for explaining the celestial phænomena. After labouring these points, at great length, in the details of which it is now of no use to follow this author, he at last concludes that, therefore the vortex is every way impossible, and insufficient in natural philosophy. Its mechanical generation is impossible; it has only an axifugal force, and not a centrifugal and centripetal force, as it should have; and even if it had, it cannot defend itself equally on all sides. It is not sufficient for explaining gravity, and its properties; it destroys Kepler's astronomical laws. What more can be desired, in order to conclude with Sir Isaac Newton? 'Itaque hypothesis vorticum 'est impossibile, et cum phænomenis astronomicis omnino pugnat, et non tam 'ad explicandos quam ad perturbandos motus cœlestes conducit.' Q. E. D.

An Account, by David Hartley, M. B. F. R. S. of Dr. Trew's Dissertation concerning the Differences of a Human Body before and after Birth, intitled, Diss. epistolica de differentiis quibusdam inter hominem natum et nascendum intervenientibus, deque vestigiis Divini Numinis inde colligendis. Jo. Georgio Kramero inscripta. Cum Tab. Æn. Aut. Christoph. Jacobo Trew, Noribergiæ, 1736, *4to.* N° 457, p. 436.

There are, according to Dr. Trew, 2 remarkable observations, which animal bodies suggest, 1st. That the same general ends are accomplished in different animals by all the possible varieties of means. 2dly, That animal bodies are machines, which produce in themselves all those changes, that are necessary for their preservation and well-being. Thus the same general ends of chylification, circulation, secretion of bile, &c. are accomplished in different animals by organs that differ considerably from each other; and in the same animal the body of the fœtus is very different in its structure from that of the adult, at the same time that this difference is effected by the body itself, each subsequent variation, the natural and mechanical consequence of that which immediately preceded, and the whole conducted in the best possible manner for the welfare and happiness of the animal.

The author's design in this dissertation, is to consider those differences of a human body before and after birth, which affect the circulation of the blood. And for this purpose he has given us 78 very curious and accurate figures of the parts relating thereto, such as the heart, and trunks of the great blood-vessels, the liver, the vena portarum, the umbilical chord, &c. subjoining to them a very minute and precise explanation of each. The work contains numerous anatomical disquisitions; which will be best read in the book itself.

There is a short dissertation, with four figures of the tongue, its vessels, glands, muscles, and nerves annexed, by the same author; whose principal intent is to show, that the vessels called salival ducts by Coschwitzius, are not salival ducts, but veins.

Some curious Experiments and Observations on a Beetle, that lived three Years without Food. By Mr. Henry Baker.*† N° 457, p. 441.

In the middle of the month of June 1737, while Mr. Baker was at a relation's house at Tottenham in the county of Middlesex, a large cistern of lead, that was placed in the coach-house-yard, to receive by pipes the rain-water from some out-buildings, fell down. Curiosity led him to examine into this cistern; and at the bottom he observed several black beetles, plunging in a muddy slimy sediment, which the water had left. Taking out 2 or 3 of them, he found them of a middling size, somewhat above an inch in length, having 6 pretty long legs, with 2 little hooks at the extremity of each, in the manner of the common beetles: they were all over of a rusty black colour, with antennæ long and jointed; a body covered with one strong shell, forming an appearance of case-wings, but undivided, and without any filmy wings underneath, and a tail turning up a little: in short, they resembled very much a sort of beetle that is sometimes seen in houses, but were of a stronger and much more firm contexture.

As Mr. B. had preserved most of our English insects, he chose one of the

* The insect, whose strength of constitution was thus tried by Mr. Baker, is the *tenebrio mortisagus* of Linnæus.

† Mr. Henry Baker, F. R. S. was a learned antiquary and naturalist, on which subjects he communicated a great number of curious papers to the R. S. which were published in the Philos. Trans. in the several volumes, from the 41st to the 56th; as well as some ingenious separate works; as, the Microscope made easy, in 8vo, 1742; and Employment for the Microscope, 8vo, 1764. He wrote also Original Poems, Serious and Humourous, 8vo, 1725; also the Universe, a Poem, intended to restrain the pride of man.

Mr. Baker was born in London, and brought up to the business of a bookseller; but he quitted that profession, and undertook to teach deaf and dumb persons to speak and read, &c.; by which he acquired a decent fortune. In 1740 he was elected Fellow of the Royal and Antiquarian Societies, from the former of which he received, the same year, the annual gold medal, for his microscopical experiments on saline particles. Mr. B. married a daughter of the celebrated Daniel De Foe, by whom he had two sons, one of whom, Henry, had some pretentions to mathematical and philosophical learning, and had received lessons in the former of these branches from the celebrated Wm. Jones, Esq. father of the late Sir Wm. Jones, chief judge in India; but taking a turn to the theatre, he married a lady eminent in that profession, and became a comedian himself; in which profession he published a useful little book, called A Companion to the Playhouse. Our author, the father, died in 1774, being upwards of 70 years of age.

largest of these beetles, and threw it into a cup full of common lamp-spirits, that being the way of killing and preparing them for his purpose, and in a few minutes it appeared to be quite dead. He then shut it up in a round pill-box, of about an inch and half diameter, and carried it in his pocket next day to London, where he tossed it into a drawer, and thought no more of it for above 2 months after; when, opening the box, he found it alive and vigorous; though it had nothing to eat for all that time, nor received any more air than what could be met with in so small a box, the cover of which shut very close. Having however no intention of keeping it alive, he again plunged it into spirit of wine, and let it lie considerably longer than the first time, till supposing it dead beyond any possibility of recovery, he put it into the box again, and locked it in a drawer, without looking any more at it for a month at least, when he found it again alive.——And now he began to imagine there must be somewhat extraordinary in this creature, since it could survive the force of spirit of wine, which soon kills most other insects, and live for 3 months, without taking in any sustenance.

A few days before this, a friend had sent Mr. B. 3 or 4 cock-roches, or as Merian calls them, kakkerlacæ, brought alive from the West Indies: these he had placed under a large glass of 6 or 7 inches diameter, made on purpose to observe the transformation of caterpillers: he put the beetle among them, that it might enjoy a greater share of liberty than for 3 months before. He fed them with green ginger moistened in water, and they eat it greedily; but he could not find, nor does he believe, that the beetle ever tasted it during the whole 5 weeks they lived under the glass together. He often took notice, that the cock-roches would avoid the beetle, and seem frighted at its approach; but never observed any tokens of its liking or dislike of them, for he usually stalked along, without regarding whether they came in his way or not. Perceiving the cock-roches begin to decline in vigour, Mr. B. was afraid they would lose much of their beauty, if he permitted them to die of sickness, and would become unfit to be preserved: he therefore put them into spirit of wine, and the beetle their companion with them. They appeared dead in a few minutes, and he believed were really so: the beetle seemed likewise in the same condition: after they had lain in the spirits about an hour, he took them out, and whelmed the glass over them, till he should have leisure to dispose of them as he intended. This was about 10 o'clock in the morning, and he saw them no more till evening, but found the beetle then creeping about as strong and vigorous as ever: and therefore he resolved to put him to a trial he imagined he could not possibly survive, which was to let him remain a whole night in spirits; but here too

Mr. B. found himself mistaken, for after he had been taken out a day, he appeared as lively as if nothing had happened to him.

After that time Mr. B. put him no more in spirits, but kept him under the glass aforementioned, where it was still living after 2 years and half, and Mr. B. has never been able to discover, that he had drank or eaten any thing.

However, by way of experiment, Mr. B. put under his glass, at different times, water, bread, fruits, &c. but he never found them in the least diminished or touched by the beetle. These trials too were always made at many months asunder, and he is pretty certain, there has been at least a year together, during some part of the aforesaid time, in which nothing has been offered him either to eat or drink.

The question will then be, how this creature has been wonderfully kept alive for 2 years and a half, without taking any visible food? The supposition, that it finds its nourishment in the air, carries with it the highest probability: since there are particles in the air which evidently supply a growth to plants of some particular kinds, such as the sempervive, orpine, house-leek, &c. And the same or some other particles in it may possibly be likewise able to afford a nourishment to animals of some certain kinds.—There is a further reason also to believe, that something like this must be the case; for, in the amazing plan of nature, the animal, vegetable, and mineral kingdoms are not separated from each other by wide distances, or broken off by sudden starts, but differ from each other, near their boundaries, by such minute and insensible degrees, that it is impossible to find out certainly where the one begins, or where the other ends.—As the air, therefore, yields nourishment to some kinds of plants, it may probably do the same to some kinds of animals; for otherwise a link would seem wanting in the mighty chain of beings. And that cameleons, lizards, snakes, &c. can live for months together without any visible sustenance, is a fact generally allowed to be true; the cause of it too has been attributed to an exceedingly slow digestion, circulation, and distribution of nourishment, in those creatures; but as their agility seems to imply a brisk motion of their animal spirits, Mr. B. thinks the circulation of their other fluids cannot be so sluggish as commonly is supposed: and perhaps it may not be unreasonable to believe, that their being able to live so long without visible food, is rather owing to some other nourishment they receive from the air, which supplies the want of more substantial diet.

This beetle walked not much about under the glass that covered it, but was usually found with its nose thrust close down to the bottom, perhaps to suck in air. On removing the glass, it appeared robust and vigorous, and would willingly run away. A strong aromatic kind of smell issued from it, agreeable enough when there is not too much of it; and the same scent hangs about the fingers a long while after touching it. In the exhausted receiver, where it was kept sometimes for half an hour, it seemed perfectly unconcerned, walking about in vacuo as briskly as in the open air; but, on admission of the air, it shrinked its legs together, and appeared in a surprise for near a minute.

This beetle, after being kept half a year longer, was permitted to get away, by the carelessness of a servant, who took down the glass to wipe it.

See the figure of this insect, in fig. 6, pl. 9.

*The Discovery of a perfect Plant in Semine. By Mr. Henry Baker.**
N° 457, p. 448.

Since the ancient supposition of equivocal generation has been rejected, for a more reasonable belief, that every thing proceeds from parents of its own kind, numbers of curious people have busied themselves in search of experiments, whereby to demonstrate the truth of the latter, and consequently the falsity of the former opinion. For this purpose the animal and vegetable worlds have been examined, and such analogy found between them, as proves convincingly, that their generation and increase are brought about in a manner pretty much alike. The animal and vegetable semina are found to be alike the rudiments of their future offspring; and both alike require only a proper repository to preserve them from injuries, and proper juices to advance their growth, and bring them to perfection.

Glasses are the means by which these secrets in nature are discovered to us. The eye, assisted by a good microscope, can distinguish plainly, in the semen masculinum of animals, myriads of animalcules alive and vigorous, though so exceedingly minute, that it is computed 3000 millions of them are not equal to a grain of sand, whose diameter is but the 100th part of an inch: and the same instrument will inform us beyond all doubt, that the farinæ of vegetables are nothing else but a congeries of minute granula, whose shapes are constant and uniform as the plants they are taken from. And as the seeds of plants are found by repeated experiments to be unprolific, if the farina be not permitted to shed, it has been supposed, that all its granula contain seminal plants of their own kind.

The growth of animals and vegetables seems to be nothing else but a gradual

* This is one of those papers which have not escaped the animadversions of Sir John Hill, who very properly observes that Mr. Baker's supposed embryo plant is in reality no other than the germen with its feathered stigmata. If the paper be worth preserving, it is that it may operate as a proper caution to unguarded observers.

unfolding and expansion of their vessels, by a slow and progressive insinuation of fluids adapted to their diameters, until being stretched to the utmost bounds allotted them by Providence at their formation, they reach their state of perfection, or, in other words, arrive at their full growth.—If this be granted, the consequence must be, that all the members of a perfect animal exist really in every animalcule of the semen animale masculinum, and all the parts of a perfect plant in every little grain of the farina plantarum, however minute either of them may be.

According to this theory, it is supposed by some, that, in animals, the semen of the male being received into the matrix of the female, some of the animalcules it contains in such abundance, find an entrance into the ovaria, and lodge themselves in some of the ova placed there by Providence as a proper nidus for them. An ovum, becoming thus inhabited by an animalcule, gets loosened in due time from its ovarium, and passes into the matrix through one of the Fallopian tubes. The veins and arteries that fastened it to the ovary, and were broken when it dropped from thence, unite with the vessels it finds here, and compose the placenta: the coats of the ovum, being swelled and dilated by the juices of the matrix, form the chorion and the amnion, integuments needful to the preservation of the little animal, which, receiving continually a kindly nourishment from the same juices, gradually stretches and enlarges its dimensions, becoming then quickly visible with all the parts peculiar to its species, and is called a fœtus.

In plants, say they, which are incapable of removing from place to place, as animals can, it was requisite a repository for their farina should be near at hand to prevent its being lost; and accordingly we find, that almost every flower, producing a farina, has likewise in itself a proper ovary for its reception; where the ova thereby impregnated, are expanded by the juices of the parent plant, to a certain form and bulk, and then, becoming what are called ripe seeds, they fall to the earth, which is a natural matrix for them.

According to the above supposition, a ripe seed, falling to the earth, is in the condition of the ovum of an animal getting loose from its ovary, and dropping into the uterus: and, to go on with the analogy, the juices of the earth swell and extend the vessels of the seed, as the juices of the uterus do those of the ovum, till the seminal leaves unfold, and perform the office of a placenta to the infant included plant; which, imbibing suitable and sufficient moisture, gradually extends its parts, fixes its own root, shoots above the ground, and may be said to be born.

Others disapprove of this hypothesis, and insist that no animalcule can possibly enter the ovum animale, nor any particle of the farina get into the embryo of. a seed. But, say they, in animals, either the finest part of the semen is taken in by the vessels of the vagina and uterus, circulated with the fluids, and carried into the ovaria, and even into the ova, by the vessels that run thither; or else fecundation is occasioned by a subtile spirit in the semen masculinum, which passes the uterus, enters the ovaria, pervades the female ova, actuates and enlivens the seminal matter contained in them, and produces all the various symptoms of conception. In plants, too, say they, the same is effected by penetrating effluvia from the male semen or farina.

This account of animal and vegetable generation is intended to introduce a discovery, which may possibly some way lead to a greater certainty about it.

Among numberless inquirers, whom the opinion, that every seed includes a real plant, has set at work to open all kinds of seeds, and try by glasses to find evident proofs of, Mr. B. was not the least industrious. But after repeated experiments, in every manner he could think of, and with the utmost nicety, he began to despair of ever attaining an ocular demonstration of it. If by moistening the seed it began to vegetate, he could indeed discern the seminal leaves, and the germen or bud, whence the future plant should arise; but he was not able to go further, unless he waited till the moisture, gradually extending its vessels, made the little root shoot down, the stalk rise up, and the minute leaves expand, and bring themselves to view. This, however, was not the thing he sought for; but at length mere accident favoured him with a discovery he had so often searched after to no purpose.

Endeavouring with a fine lancet to dissect a seed of the gramen tremulum, with intention to examine the several parts of it with a microscope, imagining he might find somewhat curious in the contexture of its husk, the edges of which were transparent, he opened it the long way exactly in the middle, and took notice of something exceedingly small between the two sides, which he had separated. He stuck the point of the lancet into it, with no other design than to take it up, and place it in the microscope to see what it might be; which he had no sooner done, than he found the lancet had fortunately opened a membranous case, that included a perfect plant, arising from a double root in the basis of its case, with two stems of an equal height, each having many leaves on it, like the grass from whence it was produced. He afterwards cut open a great many seeds of the same kind, in hopes to be able to separate one of these minute plants entirely from its theca; which at last he successfully effected.

On the Analogy between English Weights and Measures of Capacity. By the Rev. Mr. William Barlow of Plymouth. N° 458, p. 457.

The analogy between ancient English weights and measures seems for many ages to have been entirely forgotten and unknown.

Our ancestors supposed, as Mr. B. thinks, a cubic foot of water (assumed as a general standard for liquids) to weigh $62\frac{1}{2}$ lb.; the exactness of which supposition is confirmed by modern observation. From a cubic foot of water, multiplied by 32, is raised a ton weight, or 2000 pounds, luckily falling into large round numbers, and for that reason made choice of.

Agreeably to this were liquid measures accommodated, viz. 8 cubic feet of water made a hogshead, and 4 hogsheads a ton, in capacity and denomination, as well as weight.

Dry measures were raised on the same model. A bushel of wheat, assumed as a general standard for all sorts of grain, was supposed to weigh $62\frac{1}{2}$ lb. equal to a foot of water; 8 of these bushels a quarter, and 4 quarters a ton weight.

Coals were sold by the chaldron, which was supposed to weigh a ton or 2000 pounds. See Chambers's Dictionary.

Therefore, though the measures containing a liquid ton, 4 quarters of wheat, a chaldron of coals, &c. be all of different capacities; yet the respective contents are every one of the same weight. A ton in weight is the common standard of all.

Afterwards, through ignorance of this analogy, a variety of weights and measures were introduced, incommensurate, and not reducible to any common standard, or analogous relation. Whereas, had the original analogy been kept up, it would have prevented that disorder and confusion so justly complained of at present, concerning the subject of weights and measures.

From the foregoing scheme it is reasonable to suppose, that corn, and several other commodities, both dry and liquid, were first sold by weight; and that measures, for convenience, were afterwards introduced, bearing some analogy to the weights before made use of.

From the modern experiment before mentioned, a cubic foot of water weighing $62\frac{1}{2}$ lb. it appears that the measure of a foot, and the weight of a pound, are the same now as they were in use many ages before the conquest.

The foregoing scheme assigns a reason, why the word ton is applied both to weight and liquid measure, viz. because the same quantity of liquor is a ton both in weight and measure. Probably 4 quarters of grain had formerly the same appellation, till the significancy of it was lost in the use of the avoirdupois ton.

The word quarter, as applied to grain, is also here explained. Most writers have supposed it the 4th part of some measure, but what that measure was, could never satisfactorily be made out. The learned Fleetwood guessed nearest the truth, supposing it the 4th part—not of any measure, but of some load or weight. (Chron. Pretios. p. 72.) It is strange that he stopped here, and did not observe what that load or weight was, viz. a ton or 2000 lb. But the avoirdupois ton, in use at present for all gross weights, threw such a mist over the subject as could not easily be seen through.

From the original and natural signification of the word hundred, it plainly appears, that twenty hundred, or a ton, must be exactly two thousand weight.

Account of a Tract entitled, Jo. Frederici Weidleri Commentatio de Parheliis Mense Januario Anni 1736, *prope Petroburgum Angliæ et Vitembergæ Saxonum visis. Accedit de rubore cœli igneo Mense Decembri Anni* 1737, *observato Corollarium, Vitembergæ,* 1738, 4to. N° 458, p. 459.

An Attempt to examine the Barrows in Cornwall. By Stephen Williams, M. D. F. R. S. N° 458, p. 465.

These barrows are conical hillocks, generally situated on places of eminence, on or near the summit of downs, and so capable of being seen at a great distance; and often near the most public or greatest roads, though sometimes in inclosed or fenced lands. They are sometimes single, and other times a number together.

The height and dimensions of the barrows in Cornwall, are various, from 4 to 30 feet high, and from 15 to 130 broad; but they always bear a regular proportion in their form. Some have a ditch round their circumferences; some a small circle of stones at the top; some a circle of stones round the extreme verge of their basis.

The barrows, which are the subject of the present inquiry, lie on the summit of St. Austle Downs, about a mile from the town, and half a mile from the sea. They are mostly composed of adventitious earth, with commonly a circular hole, of about 2 feet diameter and depth, sunk under the centre of the base, containing at bottom a small portion of a fat black earth, about 1 inch thick, and covered by pretty large stones to sustain the superincumbent weight of the earth.

Though they had hitherto found no urn in searching several barrows, yet being persuaded by the unctuous black earth, and the cylindrical pits, in the centre of every one of them, the artful position of the stones to cover and guard them, and the foreign earth, that these barrows were erected for sepulchres; they resolved to proceed further, and pitched on one somewhat different from the rest, both as its situation seemed to regard a greater number of barrows, and as its circumference appeared to have a very large circle of stones round it, without any ditch or fossa.

They began a passage through the outer circle of stones, of 5 feet broad, and 2 high; then passed through adventitious earth till they came to a second circle of stones, of 3 feet high, and 3 feet broad: after them appeared nothing but foreign earth, till at the centre of the barrow was found an oblong square pit, of the depth of one foot and half, and breadth 2 feet, and length 5 feet; in the bottom appeared a black greasy matter, as in the other barrows, about an inch thick; but the pit was not covered or defended by any stones. However, being not satisfied, they examined the uttermost circle of stones, and on the inside of it they struck on a large flat stone, about 5 feet broad, and 1 foot thick, under which, when lifted up, were found two other thin flat stones, and under them a smaller flat stone, which covered an urn, fig. 7, pl. 9, which also stood upon another flat stone in a small pit, deeper than the circle of stones, and carefully wedged in, as well as supported, with many small stones round it. This urn is made of burnt or calcined earth, very hard, and black in the inside; it has four little ears or handles; its sides are not half an inch thick; in it were seven quarts of burnt bones and ashes. The urn will hold 2 gallons and more; its height is $13\frac{1}{2}$ inches, diameter at the mouth 8, at the middle 11, and at the bottom $6\frac{1}{2}$.

Mr. W. describes also some ancient pillars and encampments; and then subjoins remarks on several nations that practised urn burials accompanied with tumuli, &c.

It would be tedious and needless, he adds, to enumerate all the nations that burned their dead, and erected tumuli over them; we need only remember, that it was the custom among most eastern nations, and continued with them, after their descendants had peopled the most western and northern parts of Europe. Hence it is easily traced in Greece, Latium, Iberia, Gallia, and Britannia, as well as Germany, Sweden, Norway, Denmark, till Christianity appeared, and abolished it.

That the Celtæ and Britains inhabited here, need not be proved; the relics or remains of Druidism being traced in carneds, cromleches, meini gwyrs, fortifications, and the like.

That the Phenicians first, and after them the Grecians, knew these islands, and traded here for tin, long before the Romans' knowledge of them, is plain, and easily proved by Grecian and Roman authors, as Strabo, Polybius, Pliny, &c.

That the Romans conquered great part of Britain, is not disputed; but whether they possessed the most western part, now Cornwall, many learned doubt.

Mr. W. states it as a vulgar error, that the Roman soldiers made the highways in Britain; when it is plain, he says, that the poor conquered Britains under them, as masters and overseers, et inter verbera et contumelias, caus-wayed the bogs, and pared woods; paludibus et sylvis emuniendis, are Tacitus's words. This was the unhappy state of our conquered ancestors the Britons. He makes many remarks on the mixture of words in our provincial dialects; also on various Roman coins, &c.

The Danes, he adds, certainly landed here in Cornwall, but by invitation from the Britons, to assist them to overcome the Saxons, and probably never had any settlement here. They, as friends, did not want fortifications for their defence in Cornwall, since they went as far as Exeter with the Britons against the Saxons, who could never penetrate Cornwall till the 9th century, when, by one fatal battle, the Britons were obliged to become tributaries.

Having, however, endeavoured to trace all the nations, which could be supposed to have known Cornwall, Mr. W. leaves it to learned gentlemen to conjecture and discover what nation erected these tumuli.

Extracts of two Letters from Sign. Camillo Paderni at Rome, to Mr. Allan Ramsay, Painter, Covent-Garden, concerning some ancient Statues, Pictures, and other Curiosities, found in a subterraneous Town, lately discovered near Naples; dated Rome, Nov. 20, 1739, *and Feb.* 20, 1740. *Translated by Mr. Ramsay.* N° 458, p. 484.

The king of Naples has lately made a discovery of a subterraneous town at Portici,* a small village at the foot of Mount Vesuvius; and our old friend Sign. Gioseppe Couart, as sculptor to the king, has the care of the statues found there, with orders to restore them, where they are damaged. He tells me, they enter into this place by a pit, like a well, to the depth of 88 Neapolitan palms; † and then dig their way, after the manner of our catacombs, under the bituminous matter, thrown out of the mountain in the time of great eruptions, and called by the people of the country, the lava, which is as hard as a flint. And when they meet with any thing that seems valuable, they pick it out, and leave the rest. They have already found the following things:

* See these Transactions, N° 456.—Orig. † A Neapolitan palm contains near 9 inches.—Orig.

An amphitheatre, with its steps; an equestrian statue, but all broken to pieces; a chariot and horses of brass, which have had the same fate; a large brazen dish, said to be found in a temple. They have also dug out many other bronzes, with several statues and bas-relieves, which Sign. Gioseppe is now restoring. There have been found likewise 8 rings, with their cornelians engraved, and a bracelet of gold. And they have already taken up about 30 or more pieces of ancient painting, some of which are exceedingly beautiful.

As soon as I arrived at Naples, Sign. Gioseppe met me, and carried me to Portici. The first thing he showed me, was the pictures they had dug out, such as were never seen in our days; being finished to the highest pitch, coloured to perfection, and as fresh as if they had been done but a month ago.

Particularly one piece, 8 palms broad by 9 high, the figures as large as the life, representing Theseus after having killed the Minotaur, which is wonderfully fine. The figure of Theseus is naked and standing, which, in my opinion, cannot be more properly resembled to any thing, than the Antinous of the Belvidera, both for the attitude and air of the head. It is drawn and coloured with astonishing elegance. The Greek boys, who are represented as returning him thanks for their deliverance, seem, for their noble simplicity, the work of Dominichino; and the composition of the whole is worthy of Raphael. Another piece represents Chiron teaching Achilles to touch the lyre. Another large one, like that of Theseus, the figures as large as the life; but we could not comprehend the design of it. There is a woman sitting, dressed in white, with one hand resting on her head, adorned with a garland of flowers, and several deities, as they appear to be, in the air, with a black figure of Hercules leaning on his club. This figure is not of a piece with the rest, which are really prodigies of the pencil; but yet it is a fine picture. Under the woman is a deer, which gives suck to a child. This sitting figure, and the heads of those whom I take to be divinities, are exquisitely drawn and coloured.

Two other pieces of greater height than breadth, in which there are two figures, half human and half fish, which fly in the air. Four landscapes, with temples, and other buildings. Another figure, which seems to be Mercury, with a child in his hand, delivering it to a woman sitting. A tyger, with a boy upon it; and another boy, who plays on a tympanum. With many others.

After having viewed all these things, which are already taken out, I went down into the pit. The part where they are at work must have been a stupendous building; conjectured to have been an amphitheatre, by the circumference of the walls, and the large steps, which are still preserved. But it is impossible to see the symmetry of the whole; because one must travel through strait passages, like our catacombs in Rome. After having gone a good way underground, I arrived at the place in which the paintings had been discovered, and where they are daily discovering more.

The first mistake those men, they call intendants, have committed, is, their having dug out the pictures, without drawing the situation of the place, that is, the niches, where they stood: for they were all adorned with grotesques, composed of most elegant masques, figures, and animals: which, not being copied, are gone to destruction, and the like will happen to the rest. Then, if they meet with any pieces of painting not so well preserved as the rest, they leave them where they are found. Besides, there are pillars of stucco extremely curious, consisting of many sides, all variously painted, of which they do not preserve the least memory. It is very curious, to see these paintings all covered with earth, which when taken off, they appeared to have suffered nothing by it. I believe this may be accounted for, by there being no damp or moisture in the place; and that the dry earth has been rather preservative than hurtful to them. The ancient beams are yet discernible, but they are become like charcoal. And I have seen there a place where anciently they kept lime for building; a great quantity of which yet remains as fresh as if made but yesterday.

Extract of a Letter from Mr. George Knapton to Mr. Charles Knapton, on the same Subject. N° 458, p. 489.

The ancient city of Herculaneum, which was swallowed up by an earthquake, is now under the town Portici, a quarter of a mile from the sea, at the foot of Vesuvius; and has no other road to it but that of the town-well, which is none of the most agreeable, being in some parts very strait, in others wide, and cut in a most rude manner. Toward the bottom, where you go into the city, it is very broad, which they have made so, to turn the columns, which were brought up; they began this excavation 27 years ago, and worked 5 years.

The principal things found were, two columns of oriental alabaster, which were sold for 50,000 ducats; also many fine statues, the best of which were sold, and some were sent to Lorraine. Five are set up in the market-place, all clothed figures, one in a consular habit, the others women; they are all well dressed, and in a fine taste, but want the heads. In the duke's villa, which is near and by the sea-side, are two others entire, both women; one seems to be a Livia; also the fragments of a naked figure, which wants the head and arms, of a good style. These, with some ornaments and fragments of various sorts of marbles, are all that is to be seen there, of what has been dug up.

Having descended down the well, Mr. K. says, the place gives one a perfect idea of a city destroyed in that manner: for one there sees great quantities of timber, in the forms of beams and rafters, some lying one way, some another; some, as they broke in the fall, others entire; these are sticking in the sides of the ways, and are become a perfect charcoal; but those in moist places, and where the water ouses, you may run your hand into, and work like a paste, and they have more the colour of rotten wood. The walls are some tumbled slanting, others crossing them, and many are upright. One sees great quantities of marble, as bits of window-cases and other ornaments, sticking out in all parts. There seem to be, in one place, the ruins of some magnificent building, which they have dug round; for there appear the bases in white marble of square and round columns, which are all of a size; and, what is surprising, they have not examined whether they have any columns on them, which one stroke of the pick-axe would have done. I scraped away the earth at the side of the base of a pilaster, and found the wall covered with a very beautiful marble, but could not reach to discover what was on the top of it. There are but two columns that appear, one of a red marble, the other of brick covered with stucco, and fluted. In one place there are about 14 steps, which resembled the seats of a theatre. Some of the walls have the plaster remaining, and are painted, the colours still fresh. One sees nothing but pure earth mixed with these ruins; whereas the surface of all that part of the country, quite to the sea, is covered with the cinders of Vesuvius. The buildings were of brick covered with marble; for I found no other sort of stone there, but thin plates of marble of all sorts in great quantity. Neither are there any bases or capitals of large columns; 2 feet diameter is the most. Captain Emmory brought away a small capital of a pilaster, which is very curious, it being much the same as was used by the Goths in Italy. This makes me think, that they revived the ancient barbarous style, used before the introduction of the Greek for the capital. This is certainly more ancient than the time of the Goths in Italy. It was the only one of the kind we saw there.

Extract of a Letter from Mr. Crispe to Mr. George Knapton, on the same Subject as the two preceding Papers. Dated Rome, April 24, 1740. N° 458, p. 493.

At Portici I saw some antique paintings, which have lately been taken out of the ruins of Herculaneum; two of them, about 12 feet square, with their painted frames or borders round them, are as fresh and perfect as if done yesterday; much more so, than some of Raphael's in the Vatican; and for excellence, and fine taste, they are beyond any thing I have seen. One of these is called the Pomona, because, among other figures, there is a woman sitting crowned with fruits and blossoms. The other is Theseus, having just killed the Minotaur, who lies dead at his feet; a figure of a youth is kissing his right hand; Ariadne and another figure stand at his left. The figures in both these are as large as life. There is a third, somewhat less, of Chiron teaching Achilles on the harp, if possible, still beyond the two former. There are above 50 other pieces, some whole figures, some heads, some mascheras, some landscapes, some architecture.

I went to visit the ruins under-ground, where I saw several pieces that were taking down; particularly one 15 feet wide, and 8 high; it consists of the front of a large temple, with buildings of the same architecture projecting on each side, in the nature of the wings of a house. There are houses also adjoining to this temple, with windows divided into squares, which squares are painted of a greyish colour. In this architecture the perspective is very exact; the architecture is very rich and noble: the clare-obscure likewise in the other pictures, is well understood: particularly in the Pomona, where there are 6 figures, which are very agreeably grouped, and the eye is immediately pleased and reposed.

They have dug up a good many statues, but not above one or two that are tolerably good. There is, however, a perfect bust of Agrippina, mother of Nero, which was found standing in its niche, it is as clean as if just finished, has not the least damage, and is equal to most things of that kind in the world. I should not stick to say, it is altogether as fine a portrait as the Caracalla of the Farnese. There are two equestrian statues in bronze, broken all to pieces, but which, by the parts, one may judge to be as large as the Marcus Aurelius: they are soon to be put together. They have found several antique rings, with cameos and intaglias set in them; a fork, a silver spoon, made in the handle like a modern one; the bowl is pointed like an olive-leaf; a case of surgeon's instruments; several kitchen utensils; mouse-traps, vessels full of rice, a triumphal car of bronze, &c.

A remarkable Cure performed by John Cagua, Surgeon, Plymouth, of a Wound of the Head, complicated with a large Fracture and Depression of the Skull, the Dura Mater and Brain wounded and lacerated. N° 458, p. 495.

The patient was a boy 10 years of age, who fell down from the top of a high wall, June 11, 1729. Mr. C. found him speechless, comatose, his eyes bloated, his face wan, there was a bleeding at the nose and ears, and a great hemorrhage and vomiting; on examination, a large, long, deep, and contused wound appeared, from the eyebrow all over the left side of his head; and after having shaved him, Mr. C. was surprised to feel, with his fingers, so many rugged splinters of the cranium, confusedly depressed through the dura and pia mater, into the substance of the brain; the extremities of which appearing above the dura mater, he extracted to the number of 5, besides several other bits and small pieces; in taking out the last splinter, being part of the superior and interior part of the orbit, containing some of the basis and inferior part of the os frontis, joining by the sutura transversalis to the superior part of the os malæ, with part of the said suture and the upper extremity of the sphenoides, almost to the lower end of the sutura coronalis and squamosa; this splinter was, the major part of it, depressed under the superior part of the great depression of the os frontis, on extracting of which, 2 pieces of the substance of the brain, with clotted blood, came out with it, one as large as a kidney bean, and the other as large as a pea; at which time the patient fainting and vomiting, brought up most of what was contained in his stomach, mixed with bilious and bloody matter.

The dura mater was very much contused, lacerated, and bare, upwards of 3½ inches in length, and at one end 1½ inch over, the remainder about 1 inch, and the edges rugged; from the upper part of the fracture, there was a depression of the os frontis, which reached up to the sutura sagittalis, near the coronal suture; one part of the cranium lapped over the other, which he sawed off on the 3d or 4th day, it being an inch long, and occasioned a great deal of trouble, before it could be raised up with the elevator, the inferior part of the fracture being so thin and weak; the depressed part terminated in a long fissure about an inch behind the coronal suture in the bregma. The scalp was so much contused and lacerated, that the next day it began to mortify, which obliged him to lay bare all that side of the coronal, and the greatest part of the bregma, home to the lamboid suture, from the upper part of his head down to his ear.

The dura and pia mater were very livid, and insensible to the touch, except those parts where the brain was wounded, in the dressing of which the motion or pulsation of the brain was very strong, and sometimes to that degree, that it would rise considerably above the surface of the cranium; which obliged him to keep it down sometimes more than 2 or 3 minutes with his fingers, and a large and thick sindon dipped in a warm detergent lotion, before it would cease, introducing it between the dura mater and the edges of the fracture.

The upper eyelid in a week's time imposthumated; and formed a tumour as large as a hen's egg, which he opened, and kept it so a considerable time, because there was a plentiful discharge of matter from it, which was at first very fetid, but afterwards became laudable, giving likewise a good discharge from the wounded brain through the fracture of the upper part of the orbit. In about a fortnight's time there was a very laudable suppuration from all the wound, and the symptoms ceasing, the dura mater began to regenerate, looking very red and fresh: the livid and lacerated parts sloughed off, and the extremities of the fracture began to throw out their ossifications from the diploe and both tables of the cranium, like small excrescences, or proud flesh, which in a month's time spread over the whole fracture; and it grew harder sooner at the extremities of the fracture than in the centre.

The motions or pulsations of the brain still continued, and were very visible for a long time after, and were felt for some time after the wound was cured; especially in the inferior part of the coronal and bregma, over the inferior part of the coronal suture, near the squamosa. Except the first 3 or 4 days, the boy continued very sensible; but during the first 6 weeks he would very often complain of a violent pain in his head, attended with a comatose, and fever; which would soon go off again, by giving him an emollient and laxative clyster, or a gentle laxative draught.

The 6th of October following, before his wound was quite well, he was taken very ill with the small-pox, of the flux kind; and though he had them very severely, and was delirious on their coming out, yet he recovered. Nov. 11, the wound was perfectly cured; but in the latter end several exfoliations were taken out of the upper part of the coronal. The beginning of March 1736, he was very well, strong and healthy; had his sight in both eyes, was a very sensible and forward boy for his age, and had been upwards of 4 years at sea, in his Majesty's and merchants' services.

Of an extraordinary Stone voided by the Anus. By Mr. J. Mackarness, Apothecary, Chipping-Norton. N° 458, p. 500.

Mrs. Mary Smith, wife of John Smith, of Chadlington in the county of Oxon, aged about 31, a tall well-shaped strong-made woman, was seized with a violent fever, accompanied with great heat, restlessness, pain in the head, twitchings of the tendons, pale urine, unequal pulse, difficulty of breathing, great costiveness, but without thirst. She had a hard labour about 3 weeks before. This fever seized her the 2d of January 1727, and lasted till the 17th, during which time she was very costive, and continued so till she had another child, which was the latter end of February 1728, and was frequently subject to attacks of a fever, notwithstanding she observed a most regular temperance. Her labour was always difficult, and she bred her children very fast. She lay in again in December 1728, and in May 1731, and this last had a hollow dent above the temples, on the left side of the head, and was living at the date of this communication. She lay in again in September 1732, and in October 1733. These last 2 labours were the most violent, and the children had both dents in the same place of the head, the last the largest, the hollow being large enough to contain half a small orange; and the 2 children were still-born, but alive till the moment they came to the birth.

In December 1733, she was seized with a fever, and violent pains across her loins and back, great costiveness, pain at the neck of the bladder, and a pain and heaviness about the region of the os pubis. Blood was taken away, and purges, clysters, &c. were given; notwithstanding which, she continued costive, and the excrements that came from her were formed in a very odd figure, like the leaves of the great house-leek, in strata, one on the other. And thus she was for several months; then her urine began to grow fetid, and a slimy substance fell to the bottom of the pot. Her pains still continued: she found no relief from any medicines, except opiates; and these Mr. M. was obliged to use but seldom, because of her costiveness. The stench of her urine increased, and now a purulent matter was discharged in great quantity, he concluded she had an ulcer in the bladder. Mr. Wisdom, a neighbouring surgeon, passed a catheter into the bladder, and he perceived a swelling just above the groin, in the left hypochondre, which was very hard. We advised her to patience and resignation, in hopes nature might point out some method for her relief; and gave her no more medicines, but a soluble electuary to procure her stools, which she took every night.

After some time, the fetid purulent matter ceased from its discharge in the urine, but came away through the vagina, after the manner of the whites. She was quite emaciated by continual pain, and those discharges.

In April 1735, that purulent fetid matter, discharged at the vagina, now came through the anus; she complained of a prodigious weight there, and about the middle of June 1735, she had frequently very bloody stools, and once a discharge of more than $\frac{1}{4}$lb. of fresh blood. On the 2d of July, having occasion to go to the close-stool, as she sat there hard straining, but to no purpose, she thought she felt a hard substance ready for expulsion, and sent to her neighbours, who found a large substance hard and rugged, so much, that it tore one of the women's fingers, and made it bleed, in the lower part of the gut rectum, close to the sphincter ani. Mr. Wisdom, the surgeon, was immediately called, who endeavoured to extract this substance, and broke some part of it off; but he was forced to dilate the rectum, and so extract it that way. It was a hard unequal ragged flinty stone, was $10\frac{1}{2}$ inches round, and weighed $8\frac{1}{2}$ oz. after it was extracted. The woman was easy from that moment; the wounds, by the care and skill of her surgeons, healed; and she recovered perfectly, except a numbness and contraction she had in some of the fingers of both hands, and both feet and toes.

An Account of M. Leuwenhoek's Microscopes. By Mr. Henry Baker, F. R. S. N° 458, p. 503.

Martin Folkes, Esq. in N° 380 of these Transactions, gave such an exact and full description of the structure and uses of these glasses, as left little more for Mr. B. to offer, than a calculation of their magnifying powers, some reflections arising from such calculations, and a brief account of what improvements in microscopes had lately been made.

In order to this, he first viewed attentively the objects applied to these microscopes by M. Leuwenhoek himself, which Mr. Folkes has given a list of in his account; but the greatest part of them were destroyed by time, or struck off by accident; which indeed is no wonder, as they were only glued on a pin's point, and left quite unguarded. Nine or ten of them, however, are still remaining; which, after cleaning the glasses, appeared extremely plain and distinct, and proved the great skill of M. Leuwenhoek, in adapting his objects to such magnifiers as would show them best, as well as in the contrivance of the apertures to his glasses, which, when the object was transparent, he made exceedingly small, since much light in that case would be prejudicial; but, when the object itself was dark, he enlarged the aperture, to give it all possible advantage of the light. The lens being set so as to be brought close to the eye, is also of great use, since thereby a larger part of the object may be seen at one view.

All these microscopes are of one and the same structure, and that the most simple possible, being only a single lens, with a moveable pin before it, on which to fix the object, and bring it to the eye at pleasure.

Though it must cost much trouble to measure the focal distances of these 26 microscopes, and thence ascertain their powers of magnifying, yet without that it would be impossible to form a right judgment of them, or make any reasonable comparison between them and others. This task therefore Mr. B. performed, with as much care and exactness as he was able; and has shown, in the following table, how many of them have the same focus, and consequently magnify in the same degree; how many times they magnify the diameter, and how many times the superficies of any objects applied to them. The calculations are given in round numbers, the fractions making but an inconsiderable difference.

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

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

This cabinet is only the second in Mr. Leuwenhoek's collection, and is very far from containing all the microscopes he had, as many wrongly have imagined. We find here indeed, 26 microscopes in 13 little boxes. Each box contains a couple of them, and is marked in two places with a number, to distinguish it from the rest. But as the first of these boxes is marked 15, and the rest with following numbers on to 27; it necessarily implies there were 14 preceding boxes. Mr. Leuwenhoek, then, had another cabinet, that held 14 boxes before ours in numerical order, and probably each box contained a couple of microscopes, as our boxes do. But, besides these two cabinets, he had several other microscopes of different sorts, as appears by his own writings.

Many of Mr. L.'s microscopes must certainly have been much greater magnifiers than any in our possession. And we are assured by himself, in many places, that such he had.

While looking over these microscopes of M. Leuwenhoek, an opportunity presented of examining and comparing with them a curious apparatus of silver, with 6 different magnifiers, belonging to Mr. Folkes, and then newly made for him by Mr. Cuff, in Fleet-street. The body of this instrument, into which the glasses are occasionally to be fastened, is after the fashion of Wilson's pocket microscope, and contrived to screw into the side of a scroll fixed on a pedestal, from which a turning speculum reflects the light upwards upon the object: it is likewise contrived to be used with the apparatus of the solar microscope. Descriptions and figures of both of which are given in Mr. B's book intitled, The Microscope made easy. Edit. 2d. Lond. 1743, 8vo.

Mr. B. measured the focal distances, and magnifying powers, of the 6 glasses, and found them to be as follows:

A Table of the Six Magnifiers belonging to Mr. Folkes's Microscope, calculated by an Inch Scale divided into a hundred Parts, with a Computation of their Powers, to an Eye that sees Objects at 8 Inches.

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

The above calculation shows, that Mr. Folkes's first glass magnifies the superficies of an object 6 times as much as the greatest magnifier of M. Leuwenhoek. And that the animalcula, a million of which, he says, scarcely equalled a grain of sand, would, if viewed with this magnifier, appear as large as 16 grains of sand do to the naked eye. And M. Leuwenhoek must have had glasses to magnify even more than this, though they are not come to us. For we cannot otherwise conceive, how he could observe the animalcules in the semen masculinum of a flea, and of a gnat, as we find he did, or assert, as he does in the strongest terms, that he could see the minutest sort of animalcules in pepper-water, with his glasses, as plainly as he could swarms of flies or gnats hovering in the air with his naked eye, though they were more than ten millions of times less than a grain of sand. And lest this should be imagined only a random guess, he gives immediately a regular arithmetical calculation to prove his computation right. But we may all be sensible, that no glasses in this cabinet are able to render such minute objects distinguishable.

One thing alone (which, when slightly considered, may appear but trifling) has conduced greatly to the modern improvements; viz. the making use of fine transparent Muscovy talc or isinglass, placed in sliders, to inclose objects in. Had M. Leuwenhoek known this way, it would have saved him a vast deal of expence and trouble. For then, we may reasonably suppose, instead of making an entire and separate microscope for every object, he would probably have secured his objects in sliders, as we at present do, and have contrived some such means as ours, of screwing his several glasses of different magnifying powers, occasionally, to one and the same instrument, and of applying his sliders to which of them he judged best. A few good glasses, gradually magnifying one more than another, would, by such a method, have answered all the purposes of his great number, and his objects would have been preserved in a much better manner.

Passing over the different microscopes invented by Wilson, Marshal, Culpepper, Scarlet, and others, though all deserving praise, Mr. B. notices two extraordinary improvements lately made; viz. the solar or camera obscura microscope, and the microscope for opaque objects. But these inventions are by the ingenious Dr. Liberkhun. Mr. Cuff, in Fleet-street, has taken great pains to improve and bring these to perfection; and therefore the apparatus prepared by him is what are described below.

This solar microscope is composed of a tube, a looking-glass, a convex lens, and a microscope. The tube is of brass, near 2 inches in diameter, fixed in a circular collar of mahogany, which, turning round at pleasure, in a square frame, may be adjusted easily to a hole in the shutter of a window, in such a manner, that no light can pass into the room but through the tube. Fastened to the frame by hinges, on the side that goes without the window, is a looking-glass, which, by means of a jointed brass wire coming through the frame, may be moved either vertically or horizontally, to throw the sun's rays through the brass tube into the darkened room. The end of the brass tube without the shutter has a convex lens, to collect the rays, and bring them to a focus; and on the end within the room, Wilson's pocket microscope is screwed, with the object to be examined applied to it in a slider. The sun's rays being directed by the looking-glass through the tube upon the object, the image or picture of the object is thrown distinctly and beautifully on a screen of white paper, and may be magnified beyond the imagination of those who have not seen it. Mr. B. assisted lately in making some experiments by means of this instrument, and a particular apparatus contrived by Dr. Alex. Stuart, for viewing the circulation of the blood in frogs, mice, &c. and had the pleasure of beholding the veins and arteries in the mesentery of a frog magnified to near 2 inches diameter, with the globules of the blood rolling through them as large almost as peppercorns. They examined also the structure of the muscles of the abdomen, which were prodigiously magnified, and exhibited a most delightful picture.

The microscope for opaque objects remedies the inconvenience of having the dark side of an object next the eye; for by means of a concave speculum of silver, highly polished, in whose centre a magnifying lens is placed, the object is so strongly illuminated, that it may be examined with all imaginable ease and pleasure. A convenient apparatus of this kind, with 4 different specula, and magnifiers of different powers, has lately been brought to perfection by Mr. Cuff.

M. Leuwenhoek says, (in his 2d vol. part ii. p. 93,) that sometimes, to throw a greater light on his objects, he used a small convex metal speculum; and it is highly probable, that our double reflecting microscope may be owing to this hint. Also, in the 4th volume of his works, p. 182, after describing his apparatus for viewing eels in glass tubes, M. Leuwenhoek adds, that he had another instrument, to which he screwed a microscope set in brass; on which microscope, he says, he fastened a little dish, that his eye might be assisted to see objects better; for he says, he had filed the brass which was round his microscope, as bright as he could, that the light, while he was viewing objects, might be reflected from it as much as possible. This microscope, with its dish, seems so like our opaque microscope with its silver speculum, that, after considering his own words, it is probable he may properly be esteemed the inventor of it.

An Inquiry into the Causes of a dry and wet Summer. Anonymous. N° 458, p. 519.

This writer concludes, that a frosty winter produces a dry summer; and a mild winter a wet summer. And he finds, from these and some other observations, casually made, that the weather depends very much on the wind. He therefore begins with inquiring what is the cause of winds, and then proceeds to guess why the wind influences the weather.

Wind is a stream of air; air an unmixed fluid encompassing our globe, with a shell of at least 60 miles thick. Every particle of air gravitates equally towards the centre of the earth. Air is capable of being compressed and expanded. The more air is compressed, the heavier it is; the more it is expanded, the lighter. Cold and heat, whatever they be, or however they act, produce these contrary effects in the air: that is, cold compresses the air, and heat expands it; therefore cold and heat, in different parts of the air, will make it flow: for cold making the air heavy, and heat making it light, the lighter must give way to the heavier; as, in a balance, a greater weight makes a smaller rise. We daily see a proof of this in a stove.

The sea and land-breezes, and the trade-wind, owe their origin to these causes. The sea-breeze, when regular, begins at 9 o'clock in the morning, approaches the shore gently at first; increases till 12; retains its full strength till 3; then gradually decreases till 5, when it dies away. At 6 in the evening the land-breeze begins, and continues till 8 next morning. The interval between these two breezes, at morning and evening, are the hottest parts of the day.

The way of accounting for this vicissitude of sea and land-wind, is thus: the sun, as it ascends, sheds its heat equally on the land and sea; but the earth receives the heat sooner than the water, or else reflects it stronger. For one or both of these reasons, the air that hangs over the land, is heated more than the sea air, it becomes thereby more rarefied, and consequently lighter: and therefore the sea air, with its superior weight, flows in upon it every way. The intervals between are owing to the air of both places being in an equal degree of heat, and consequently of equal weight.

The trade-wind never varies, which is thus accounted for: the air just under the sun is the hottest: the cold air presses upon the hot, as the hot air follows the sun; and therefore it makes a perpetual flow of wind between the tropics from Africa to America, and from thence to the East-Indies.

With regard to the wind influencing the weather; though air be an unmixed fluid, yet it is capable of receiving many vapours, which float in it, as we see other bodies float in water. Sometimes the vapour ascends, and sometimes it falls to the ground. All which is probably effected by heat and cold in this manner: heat separates water into small particles, and the incorporated air, rarefied by the same heat, blows up those particles into bubbles; by which means the swoln vapour, becoming specifically lighter than a like space of ambient air, ascends, swift at first, affording a pleasant sight in a warm summer's day, and then gradually slower, till it gets up to that part of the air which is of equal lightness with itself; and there it remains, as long as the air continues in the same state. But whenever the air cools, in which these watery bladders float, the cold contracts the bladder, which becoming thereby specifically heavier than the air, down it falls in dew, or rain. A common alembic sufficiently shows the operation of heat and cold on the ascending and descending vapour.

Thus in a calm evening, when there is no wind to waft the air, as the heat of the sun declines, the cold arrests some few of the last ascending vapours, and, by its own force, without any other change in the state of the air, compels them to return, in dew, to the very spot from whence they arose; whilst their brethren escape, who go out of the reach of the cold a little before the approach of night.

Since therefore the same air, in different states of heat and cold, affects vapour in this manner, it thence follows, that vapour, wafted from air of one temperament to another, must be affected in the same manner also: so that vapour, carried from a colder to a warmer air, will ascend; and, on the contrary, vapour carried from a warmer to a colder air, will descend.

Now if cold condenses the air, and thereby makes it press on the warmer; and if vapour, carried by a stream of air from a colder to a warmer region, ascends; we have the reason why the north-east blows, and why it blows dry.

Let us fix upon some spot in the continent of North-Europe, whence this wind comes to us: suppose Archangel, which lies on our north-east point, and is in 65 degrees northern latitude: when the frost is intense, the incumbent air there must needs be very heavy; that air will press every way: quà data porta, ruit. Let us consider which way this condensed air can burst out from thence: it cannot go to the north, where the cold is greater; nor to the east, for the air over the large continent of Tartary is at least of equal coldness with itself. To the west, the air might find a free passage over the ocean, were not the colds of North-America too near. The main outlet is between both, towards the Atlantic-Ocean: the warm air over which being able, of itself, to make but a feeble resistance, yields to the superior force; the conqueror eagerly pursues his victory, and we, happening to lie directly in the way, feel then a cold dry north-east wind: this is the wind that brings us frost in the winter. When the winter is severe, it continues to blow all the spring, and its influence reaches to the end of the summer.

This it seems sufficiently proves, that air, flowing from a cold to a warmer quarter, will blow dry: but, like a willing witness, it proves too much; for, if wind proceeds only from cold air pressing on hot, and if heat makes the vapour ascend, it follows from thence, that wind can never bring rain; whereas we find the contrary by sad experience.

How can this be accounted for, on the principles commonly received? That vapour, wafted from a warmer to a colder region of air, should precipitate, is what we have already shown. But the question is, why does the south-west blow? What is the cause why a stream of air should be carried, for so long a time, and with so great violence, as we have often felt, from a warmer to a colder, from a rarer to a denser, from a lighter to a heavier quarter? To the north-east of us lies the continent of North-Europe, great part of which is, in the winter, deprived of the sun's heat, and consequently very cold; on the other side, to the south-west, lies the vast Atlantic ocean. We find by experience, that the sea-shore is warmer than the inland; that the sea is warmer than the shore; and that the ocean is still warmer than the sea. Besides, the more you go from hence towards the south, the nearer you go to the sun; and the more north, the farther from it: this must make the south-western ocean much warmer than the continent, that lies at an equal distance, on the opposite point: from this very warm place, the wind blows to a place much colder; and yet there must be a natural cause of all this apparent contradiction to the laws of nature.

It will be in vain to seek for the cause of the wind in this ocean itself, or in the air over it, influenced only by the sun, and the surface of the sea. But there may be tornados in those seas: our seamen often meet them between the tropics, seldom in the ocean to the north of the northern tropic. But were they more frequent and violent than they really are, yet they are not lasting, and therefore cannot produce a long steady course of south-west winds with us.

Probably our south-west wind is no other than an eddy of the trade-wind, reflected from America to us. There are also some other facts which strongly support this hypothesis; viz. currents of the sea, and the wind in the Atlantic ocean, to the northward of the trade-wind.

With regard to the currents, Dampier tells us, it is generally observed by seamen, that, in all places where the trade-wind blows, the current moves the same way with the wind; and that though it be perceived most near the shore, yet it makes no sensible rising in the water, as the tides do. He says, there is always a strong current setting from Cape St. Augustin westward, occasioned, as he remarks, by the south-east trade-wind driving the surface slanting on the coast of Brasil; which, being there stopped by the land, bends its course northerly, towards Cape St. Augustin; and, after it has doubled that promontory, it falls away towards the West-Indies, down along the coast westward, till it comes to Cape Gratia de Dios; from thence north-west towards Cape Catoch in Jucatan, thence to the northward between Jucatan and Cuba. He says, that in the channel, between Jucatan and Cuba, he has found the currents extraordinarily strong; that it is probable, that the current which sets to leeward, on all the coast from Cape St. Augustin to Cape Catoch, never enters the bay of Mexico, but bends still to the northward, till it is checked by the Florida shore; and then it wheels about to the east, till it comes near the gulph's mouth, and passes with great strength through the gulph of Florida, which is the most remarkable gulph in the world for its currents, because it always sets very strong to the north.

Let us then suppose the wind, which drives this water before it, to follow it much in the same course; and that, instead of striking against one plain surface, with such an inclination as would direct it to us, it strikes against a million, yet still bending this way: let this natural supposition be admitted, and we have the very thing sought for, viz. a proper direction.

The other fact is this: that when our ships return from the West Indies through the gulph of Florida, and are got into the wide ocean, they have a regular wind at south-west, or near that point, which sometimes attends them to their very port. This wind cannot have its rise in that ocean, nor can it come from any continent that lies to the north, or even west of it; therefore probably it must be an eddy of the trade-wind.

From America to the west of England this wind glides over the ocean, a plain field, that gives no opposition, and which, with its natural warmth, encourages the waft, by making the air over it more ready to yield to the impelled force. Having thus opened a passage for the trade-wind to flow even to us, with a back stream; and admitting this conjecture to be right, we have the cause why the south-west wind blows with us; and then there can be no great difficulty in finding out the reason why it brings so much rain. For this wind blowing over a warm ocean, which sends up many vapours, by the time it reaches us, it comes charged with an infinite mass of watery bladders, which the cold of this climate condenses, and then they descend in showers of rain.

Notabilia quædam in Itinere Alpino-Tyrolensi observata per Balthasarem Ehrhartum, M.D. Memingensem in Epistola ad C. Mortimerum, R. S. Secr. missa.* N° 458, p. 547.

This paper contains some observations (not interesting in the present advanced state of geology) relative to the structure and formation of the Tyrolese Alps, together with an enumeration of the plants found growing on that chain of mountains.

The Figure of a Machine for grinding Lenses spherically. By Mr. Samuel Jenkins. N° 459, p. 555.

Fig. 8, pl. 9, represents this machine, which is contrived to turn a sphere at one and the same time on two axes, which cut each other at right angles, with equal velocity and pressure on each of them. Without any skill or care in the workman, it will produce a segment of a true sphere, barely by turning round the wheels.

A represents a globe covered with cement, in which are fixed the pieces of glass to be ground.

* Author of several works on natural history, the chief of which is an account of plants used in the arts, in agriculture, and in medicine, written in German, and amounting to 12 vols. 8vo. Of the 12 vols. seven were edited by Gmelin after the author's decease.

This globe is fastened to the axis, and turns with the wheel B. C is the brass cup, which polishes the glass: this is fastened to the axis, and turns with the wheel D. So that the motion of this cup C is at right angles with the motion of the globe A.

Of an immoderate and fatal Use of Crabs' Eyes and other earthy Absorbents, producing Calculi in the Stomach and Kidneys.† By J. Philip Breyne, M. D., F.R.S. Addressed to Sir Hans Sloane, President of the Royal Society, &c.* N°. 459, p. 557. *From the Latin.*

In this communication Dr. Breyne relates the case of Sir Robert Hacket, Kt. who resided in Barbadoes. He was of a robust constitution, and enjoyed very good health, except that he was sometimes attacked with the gout, in consequence of indulging too freely in wine. Another consequence of his occasional intemperance was that he was troubled with cardialgia or heartburn, for the removal of which he frequently made use of crabs'-eyes, chalk and other earthy absorbents, which afforded a temporary relief; but as after some time he became subject to a daily recurrence of this symptom (the heartburn) he continued taking the beforementioned absorbents in large doses every day, for a number of years; until at length they not only failed to relieve the cardialgia, but brought on a most distressing sense of weight under the diaphragm, accompanied sometimes with vomiting, and most acute nephritic pains. These symptoms proved fatal in 1694, when the patient was in his 56th year.

On opening the body a great number of calculi of different sizes, and branched after the manner of corals, were found in the stomach. The largest and most remarkable of these ramified calculi, though the extremities of some of the branches had been broken off at the time they were shown to Dr. Breyne, by Mr. W. Hacket, (son of the deceased, and who communicated to the Doctor the particulars of this case), yet it then weighed 2 oz. 5 drs. The next in size weighed 1 oz. 1 dr. The others were much smaller, from the size of a poppy seed to that of a pea, and for the most part of a spherical shape.

All these calculi were inveloped in the stomach in a thick slimy fluid, which by drying upon paper in the air became converted into a powder, that very much resembled the substance of which the aforesaid calculi were composed. Nevertheless the substance of these calculi was not exactly alike in all. In the

* The lapilli cancrorum, vel oculi cancrorum are stony concretions found in the stomach of crabs.

† In renibus. But in the history of this case mention is made of one kidney only.

greater number it was white or cineritious; in others it was of the colour and consistence of the occidental bezoar; while in a few, it resembled the oriental bezoar.

Moreover there was found in the kidney,* a stone weighing 3 drs, seemingly formed of 6 spheres, as represented in fig. 4, and in substance similar for the most part to the calculi found in the stomach.

For other instances of calculi in the stomach, see Phil. Trans. N° 250,† and Eph. Nat. Curios. Dec. 1, Ann. 2, Obs. 181.

Concerning Two Pigs of Lead, found near Ripley, with this Inscription on them, IMP. CÆS. DOMITIANO AUG. COS. $\overline{\text{VII}}$. *By the Rev. Mr. S. Kirkshaw.* N° 459, p. 560.

These were found by a countryman, whose horse's foot slipping into a hole covered with ling, he dismounted, and, thrusting his stick into the hole, perceived something hard, and of the sound of metal; then, by digging, he found these two pieces of lead, standing upright, and near each other, about 2 feet under-ground. They are of the same shape and dimensions, and have the same inscription. One of them weighs 11 stone, the other 11 stone and 1 pound. The letters of the inscription are raised, and very bold. There have been 4 other letters on the side of each of them; but they are become so obscure, that they cannot be discovered with any certainty.—They seem to have been B. N. I. G. . . . The great Roman causeway leading from Aldborough, in this neighbourhood, into Lancashire, passes within a little way of the place where the leads were found. There have been no lead-mines, as far as can be known, within some miles of it: but a countryman speaks of a large rock, about half a mile from it, on the top of which there is an impression similar to either of the leads, only so much larger as to admit of a pan, wherein they might be smelted, if in so early time they knew the modern art of smelting by the air.

Camden mentions 20 pieces of lead of this kind, found in Cheshire, part of them with this inscription, IMP. DOMIT. AUG. GER. DE. CEANG. . . . . Camden's Britan. fol. edit. p. 679.—However, among the Duke of Parma's medals, published by Paolo Pedrusi, we do not find any struck in the 7th consulate of Domitian, but what have the addition of divi filius, or the like. That author too says, that the first year of Domitian's being emperor was the 8th of his consulate; nether of which agree with the inscription on the leads.—

* In rene; but in which of the kidneys it is not specified.

† Vol. iv, p. 357, of these Abridgments.

The dimensions of the pieces of lead were, the greatest length 23¼ inches, the mean breadth 4½, and the depth or thickness 4 inches.

The Description and Draught of a Machine for reducing Fractures of the Thigh. By Mr. Henry Ettrick, Surgeon. N° 459, p. 562.

This machine consists of no more than a wheel and pinion, with their axles; the roch, or snagged wheel, being herein accounted as part of the great wheel, fixed in a light frame of about 2 feet long, (see fig. 1, pl. 10) the whole not exceeding the weight of 15 pounds; and when taken to pieces, by unscrewing the frame-pieces, may be packed up in a common rush-basket, belted to the side, and conveyed to any distance. The room it takes up in working is not a full yard, and it may be set up and fixed for use in a few minutes. In using this machine, the surgeon needs only one assistant; whereas, in most other methods, their number is most troublesome and inconvenient: the business of this assistant is only to mind the surgeon's orders, and move the winch according to his direction. When the extension is sufficient, the engine stays itself, and continues the tension of the limb, by the assistance of this roch, or toothed wheel, whose teeth are cut fine enough to stay the engine at every line of an inch, and which is fixed on the back of the aforesaid great wheel, both to the cross by the help of screws, and on its arbor by having its centre squared out, so as to fix tight on it, and so near the frame as only to allow a bare clearage: its teeth, standing counter to the former, admit the spring or catch, fixed on the inside of the frame, to slip over its vertex, without interruption; but in a reverse rotation, or when the engine is about to come up, it flies into the spaces, and stops it. The upper part projects about an inch from the frame, so that being pressed on by the finger of one hand, the inferior part is elevated above the range of the teeth, to admit the coming up of the engine, which is to be directed by the other hand being applied to the winch in any degree. This engine has its power so commanded, that it may be used without restriction, from the most robust to the most tender frame, as it acts and exerts its power in proportion to the resistance made. This extension is made deliberately, steady, equally, and in one continued line, without the least variation. And, in oblique fractures of the thigh, where the bones are apt to ride, and therefore require a continued extension in a certain degree, to prevent the limb's shortening after the cure, such a machine must be of excellent service; having the property of increasing or decreasing the extension at pleasure, and to be perfected without the least jar or tremor.

The necessary appendages are bands, by which the engine extends the limb.

Immediately from the axle of the great wheel comes a girth, at the other end of which is a hook, which links into a swivel-ring at the bottom of a sole-plate: this plate answers the shape of the foot, and is made of well-hammered brass, the inside of which is padded, to sit easy to the foot: the upper part has a strap, which clasps over the upper part of the metatarsal bones; and to keep the straps ending in the sole-plate from galling or pressing the sides of the foot and ancle, there project 2 arms from the sides of this sole-plate, to which the straps coming from the ancle-band are fastened. That the whole limb may be kept in a line with the machine, the leg is suspended by bands, one of which is placed at the ancle, from the sides of which pass 2 straps, to join the inferior knee-band: from this band pass 2 straps to the superior knee-band: all these straps are designed to divide the extension, so that all parts may equally bear alike, and so to secure the joints of the limb from the violence of the extension. The inside of these straps are lined; the bands encircling the limb are contrived in the same manner as the bow or spring of a truss, having strong clasps at the ends, after the manner of those for pocket-books, to fit any dimensions. The band embracing the part above the fracture, and from which pass 2 straps to the head of the bed, to make the counter-extension, is of the same kind as the former, and is to be kept on during the whole time of decumbiture, to prevent the patient's body sinking on the fracture, and thereby contracting the limb. The exterior of the 2 last mentioned straps presses just beneath the great trochanter on its outside; the other comes from the anterior part of the same band, and in such a scite, as to give the patient liberty to raise himself at discretion. To preserve the natural curvity of the thigh, it would be necessary to have a large broad band arising from the bedside, to encompass the fractured part, and keep it steady.

Of petrified Oysters, by Cornelius le Bruyn, illustrated by James Theodore Klein, F.R.S. and Sec. Rep. Dantzic.* N° 459, p. 568.

That indefatigable traveller Cornelius le Bruyn, among other things worthy of notice, relating to natural history, mentions oysters, of which not only the valves, but even the animals themselves were petrified within the shells.

" At some miles from Nicosia, he says, there is a hill, which consists wholly of petrified oysters. The shells are close shut, and when they are

* *Cornelius,* or *Corneille le Bruyn,* a painter, was born at the Hague, and travelled into the Levant in the years 1674—1708. At his return he published his travels, which are considerably valued on account of their accompanying plates. He must not be confounded with a more eminent artist, viz. *Charles le Brun,* the celebrated French painter, born in 1619.

opened, there appears an oyster on each side, so well consumed, that one might say it was well engraved there. These shells are also petrified, or turned to stone. I opened one, in the middle of which there was an oyster quite entire, and at the same time, as it were, engraved in the other shell."

I did not wonder at the shells being turned to stone, but it seemed strange, that the animal oyster should be petrified; nor did the author's reason of this phenomenon appear to be sufficient.

"When we take sand out of the first shell, we see the oyster, which is in like manner consumed by time; whence we must conclude, that these oysters have been alive there, and that the water running out, the sand has insensibly supplied its place, and that the oyster, as it died, left the print of its shape in the shell. Thus there are some of these oysters, like those stones in which we see a fish."

It will hardly be understood what is meant by the shape of the oyster, a soft and corruptible animal, being impressed on its shells, before the shells themselves, by nature hard, were turned into stone; nor will you easily come into the author's opinion, that it should be possible for an oyster to imprint its shape on the shells, in like manner as the skeletons of fishes leave their impression in soft earth, which is afterwards turned to a stone, for the most part flaky. Therefore I thought it not amiss to explain this account by schemes of a lithostreum, which I got whole out of a very hard stone of the mountain Zijanken-Berg, near Dantzic, in 1736.

It is to be noted, 1. That the figured stones of Dantzic, containing many extraordinary vegetable and fossil substances, especially of the mountains Hagels-Berg, and Zijanken-Berg, are formed of potter's earth and clay, mixed with a little sand, grey, and generally very hard; so that being beaten with an iron hammer, they fly asunder like the vitrum fossile imperati. 2. That they contain abundance of shells of cochlidæ or conchæ, very often entire, petrified, but very distinguishable by their natural colours; sometimes, when the matrix, as it is called, is less compact or hard, partly calcined, and partly petrified.

Now in the abovementioned lithostreum, if I mistake not, the same phænomena appear, which le Bruyn has endeavoured, however obscurely, to describe; therefore I took care to have an exact draught of this lithostreum, the valves being opened with great circumspection.

In these, the form of the animal remained entire; but the whole substance of it was changed to a smooth, hard clay. This perhaps is what le Bruyn meant, when he said, "In the middle - - - we see the oyster entire, and at the same time it looks as if it was engraven in the opposite shell."

Now it is well known, that in the inner part of most oysters, especially in the concave valve, there is a sort of cistern, containing the water which is greedily drawn in, closed with a thin shelly plate, and from the hinge generally equalling the whole bed of the animal; and I have learned by experience, that this cistern, when it is distinct from the hard shell, is apt to deceive the unskilful: for it has happened more than once, that some have pronounced the cavity covered with a transparent plate in fossil oysters, to be the figure of the oyster rudely inscribed on either shell.

Perhaps such a shell of Mount Nicosia might impose upon Le Bruyn, "That the oyster has imprinted its shape on the shell," when he boldly appeals to his figure, "as may be seen in the figure:" whereas even his figure, considered attentively, shows nothing but the mere shell, representing only an imaginary shape of the oyster. I could prove this assertion by many schemes.

Observations of the Planet Mars, made at Berlin, in the Autumn of 1736. *By Christ. Kirch, Astronomer of the Royal Academy there.* N° 459, p. 573.

Some observations of that planet, as to its places and situations, with respect to the sun and fixed stars, &c.

A Collection of the Observations of the remarkable Red Lights seen in the Air on Dec. 5, *or* 16*th N. S.* 1737, *sent from different Places to the Royal Society.* N° 459, p. 583.

1. *As observed at Naples by the Prince of Cassano, F.R.S.* p. 583.—Dec. 16, 1737, N. S. in the evening, the sun being about 25 degrees below the horizon, a light was observed in the north, as if the air was on fire, and flashing; the intenseness of which gradually increasing, at the 3d hour of the night it spread much westward. Its greatest height was about 65°; for it occupied the whole extent of both the Bears and the polar star; yet at the sides it was not so high; for in some places near the north it rose only to 50°, and it gradually diminished, so as to become insensible at the true horizon.

The abovementioned light at its extremities was unequally jagged, and scattered, and followed the course of the westerly wind; so that for a few hours it spread considerably wider, yet without ever reaching the zenith. About the 6th hour of the night the intenseness of the colour disappeared: some small traces of the inflammation still remaining towards the north-east and the west, which were all vanished at $7\frac{1}{4}^h$ of the night.

The inflamed matter, in the greatest part of its extent, gave a free passage to the rays of the stars, even of the 3d and 4th magnitude, situate behind it. About the 4th hour of the night, a very regular arch, of a parabolic figure, was seen to rise gently, to 2° of rectangular elevation, and to 20° of horizontal amplitude. This phenomenon was seen all over Italy, as appears by several accounts of it, though with some disagreement between them.

The most probable opinion as to the cause of this phenomenon, ascribes it to the simple firing of a bituminous and sulphureous matter, on account of its very little specific gravity, raised to the upper parts of the atmosphere, and there, by the clashing of contrary winds, broken, comminuted, and at last set on fire. This opinion has been defended with strong arguments in the Petersburg commentaries, by Mayer, on occasion of the appearance of a similar phenomenon in those northern countries. And indeed the preceding eruption of Vesuvius, the contrariety of the moving forces, the readiness of the matter to take fire, the unequal intenseness of the light, the streaks, and all the other circumstances observed in this meteor, are plain arguments of a genuine and real accension. And Wolfius, on the appearance of a phenomenon much like this, which was seen all over Germany on the 17th of March 1717, is of opinion, that it should be called imperfect lightning, as being produced by the inflammable matter of lightning.

2. *Observed at Padua, by the Marquis Poleni, F. R. S.* p. 587.—At the time of this meteor, the air was calm, and the barometer was remarkably high.

At $5\frac{1}{4}^{h}$, there appeared near the horizon a blackish zone, with its upper limb of a sky-colour, somewhat obscure. Above this zone was another very luminous, resembling the dawn pretty far advanced. The highest zone was of a red fiery colour. The altitudes of the zones seemed to bear such proportion, that the second was double the first, and the third triple; and in many places they rose somewhat above the 40th degree of altitude. Eastward they extended to the 55th degree on the horizon, and westward to the 70th.

It is remarkable, that after sun-set on the preceding days, as well as this, there appeared in the west a remarkable redness expanded on each side; and on the ensuing evening, the same bright red colour, appearing near the horizon, deceived the common people into a belief, that a new phenomenon, like the foregoing, was breaking out of the horizon. Near our zenith there appeared some thin lucid clouds, partly of a whitish red, in such a manner, that they seemed as if occasioned by the burning of houses at some distance to the north. Others of this sort had happened before, and some were seen afterwards.

A little after 6, the upper parts began to emit red streamings, or rays, in plenty; but in these the red was now and then intermixed with whitish and darkish colours. In a few seconds after, there issued out from the very equinoctial west, a red and very bright column, which ascended to the third part of the heavens, and a little after, it became curved in the shape of the rainbow.

At half after 8, almost in an instant of time, the bright zone, from the 8th degree west to the 50th east, became more vivid, and rose higher; and above this appeared a new large one, of a red fiery colour, with several successive streamings tending upward, and passing 60 degrees of altitude; the western part had assumed the form of a thin cloud. At 12, the light of the aurora was nearly extinct, there appearing only a very weak light along the tops of the mountains. Twenty minutes after, there appeared a white brightish beam, at 30° west, and 60° high; but it soon became invisible. In half an hour after, a very weak light remained in the west, near the horizon; which had not been observable, if the brightness of the preceding phenomenon had not invited to continue the observation.

3. *Observed at the Observatory of the Institute of Bononia. By Dr. Eustachio Zanotti, Deputy Professor of Astronomy,* p. 593.—The aurora borealis, which was formerly a rare phenomenon, and almost unknown in this climate, is now become very frequent. In Bononia a great number have been observed for some years past. This time it was so very remarkable, that no one remembers to have ever seen the like. As to its extent, it spread so as to occupy about 140° of the heavens; and, as to its light, it was so vivid, as by it to distinguish houses at a great distance; which seemed of a red colour, which made some people attribute this light to a fire in the neighbourhood.

It continued at times variously increasing and decreasing.

About 8^h, the aurora formed itself into a concave arch towards the horizon. The polar star was near the top of its convexity, and some stars shone bright in the midst of the light; and, among these, δ and γ of Ursa major. The concave part was terminated by a basis somewhat dark; which separated the red light of the arch from a white and very bright light that remained within it. The arch, which was 15° broad, was of a deeper colour towards the horizon than towards the pole. The western limit, which was interrupted by clouds, was wider and more irregular than the eastern limit. Fig. 2, pl. 10, exhibits the phenomenon conformable to the description now given.

At 8^h 34′, the red light continued spreading, and made, as it were, a basis of a weaker redness. At this time the aurora appeared unsettled and curious, as in fig. 2. At its eastern limit, the pyramid continued visible, but of a more intense colour towards the north, and from its middle there shot up vertically a streak of light, between a white and a yellow colour. A very dark narrow cloud crossed the whole phenomenon, and went to terminate in the pyramid.

At the upper part, a considerable tract of the heavens was enlightened with a very vivid red light, which was interrupted by several streaks or columns of a bright yellowish light. These streamings shot up vertically, and parallel to each other, and the narrow cloud seemed to serve them for a basis. Under the cloud there issued forth two tails of a whitish light, hanging downward on a basis of a weak red, and it seemed as if they kindled and darted the light downward. There was likewise seen a white streak, which passed across these two tails, and extended from one end of the phenomenon to the other, in a position almost parallel to the abovementioned cloud.

At $9^h\ 4^m$, there now remained but a little reddish light at the north pole; all the rest was collected near the zenith, not extending lower than the star α of Ursa major. In the south, where the sky was clear, there were seen some of those meteors, called falling stars.

Several persons have positively asserted, that, in the evening of the 16th day, they perceived a certain stench in the air, like that which is sometimes occasioned by a fog. The same has been taken notice of at other times, when such phænomena have appeared.

There was a very thin fog in the air not only on the 16th day, but also on the preceding and ensuing days. The mornings of the 17th and 18th, before and a little after sun-rise, the air appeared of an uncommon fiery colour. The evening of the 17th, the crepusculum was of an extraordinary height. Between the north and west, there was seen a very thin red vapour, which lasted almost till night.

4. *Observed at Rome. By S. de Revillas, Math. Prof. and F. R. S.* p. 601.—These observations are similar to the foregoing.

5. *By Mr. James Short, at Edinburgh,* p. 605.—We were surprised, on looking out at the windows, about 6 o'clock, to find the sky, as it were, all in a flame; but on further inquiry, it was nothing but the aurora borealis, composed of red light. There was an arch of this red light reached from the west, over the zenith, to the east; the northern border of this light was tinged with somewhat of a blue colour. This aurora did not first form in the north, and after forming an arch there, rise towards the zenith, as they commonly use to do; neither did the light shiver, and by sudden jerks spread itself over the hemisphere, as is common; but gradually and gently stole along the face of the sky, till it had covered the whole hemisphere; which alarmed the vulgar, and was indeed a strange sight. A great circle of this light came from the west to the zenith, which seemed to be the magazine whence all the rest were supplied. It is but about a year since Mr. S. first observed this red light in the aurora borealis, and only then in very small quantities.

6. *At Rosehill, Sussex. By John Fuller, Esq. jun. F.R.S.* p. 606.—It was a strong and very steady light, nearly of the colour of red ochre. It did not seem to dart or flash at all, but continued going on in a steady course against the wind, which blew fresh from the south-west. It began about north north-west, in form of a pillar of light, at about 6^h 15^m in the evening; in about 10 minutes, a 4th part of it divided from the rest, and never joined again; in 10 minutes more it described an arch, but did not join at top; exactly at 7, it formed a bow, and soon after quite disappeared. It was all the while lightest and reddest at the horizon. It gave as much light as a full moon.

At 8^h it began again exactly north; it was very light then, but not near so light as before: in half an hour it made an arch from east to west, and went quite away to the south, when it ended much with the same appearance as it began in the north, but not quite so red.

A short Account of Dr. Jurin's ninth and last Dissertation, De Vi Motrice. By Mr. John Eames, F. R. S. N° 459, p. 607.

The last* dissertation is new, and treats of the motive forces of bodies, whether they are to be estimated by the velocities, or the squares of the velocities, when the masses are equal. The original of this dispute among the mathematicians, the author ascribes to a slip committed by the celebrated Mr. Leibnitz, in the year 1686; and the continuance, to the neglect of the times, in which equal effects are produced. The one side asserts all causes to be equal, whose effects are so, whether the times, during which the causes act, are shorter or longer. The other, on the contrary, maintains, that equal effects may arise from unequal causes, if the times of action are unequal; that consequently the times, as well as the effects, ought to be taken into the account.

He wishes the gentlemen on the other side of the question would produce some experiment in their favour, where the equality of the times is preserved; since all the experiments they have hitherto made, and argued from, may justly be set aside, as incompetent, on account of the inequality of the times of action.

The author then proceeds to prove the truth of the common opinion of the forces in equal bodies, being proportional to their velocities. This he does by three mediums, the first taken from the action of a single spring on the same body; the second from some experiments of Mr. Mariotte; the third from the joint action of several springs on two unequal bodies.

* The eight preceding dissertations had been before printed separately; but were now all collected together, with the addition of this ninth, and published in one volume in 8vo. London, 1732.

1. A single spring, fixed to a moveable horizontal table, is made to communicate to the same body, degrees of force unquestionably equal, while the degrees of velocity communicated at the same time are also undoubtedly equal; therefore the forces are proportional to the velocities.

2. In Mr. Mariotte's experiments, the impressions made upon equal surfaces in the same point of time, are found to be in the duplicate ratio of the velocities; but the masses or numbers of impinging particles are in the simple ratio of the velocities; consequently, the masses and velocities conjunctly being in the duplicate ratio, i. e. as the impressions, must also be as the forces which made them; which is the old opinion.

3. A complicated or bent spring interposed between two unequal bodies, acting on each with an equal pressure, and during an equal time, must communicate equal moving forces to each; but their velocities are by experiment reciprocally proportional to their masses: therefore their masses, drawn into their respective velocities, are also equal, as were their moving forces; and by consequence their moving forces are as the masses and velocities conjunctly; which is the generally received opinion.

In the appendix, the author answers some of the principal arguments brought in favour of the contrary side.

The last of these arguments is founded on Poleni's experiments, in which equal cavities are formed in soft substances, by equal bodies falling from heights reciprocally proportional to their masses. This the author sets aside, as insufficient, since the times of forming these equal cavities are unequal, and unequal causes may produce equal effects in unequal times. Poleni does indeed reply, and say, that the formation of these cavities seems to be instantaneous; but the ingenious author shows the contrary, and that from a position allowed of by Poleni himself, in his reply.

Of some remarkable Stones, taken out of the Kidneys of Mrs. Felles, on opening her Body after her Decease. By Noah Sherwood, Surgeon. N° 459, p. 610.

Mr. S. found nothing amiss in any of the viscera, till he came to the kidneys, both of which were considerably enlarged, and of an oblong figure, and had several protuberances bunching out, which made the surface appear almost like a beeve's kidney. On feeling them externally, he could plainly perceive they were caused by stones; he took them out of the body, and laid them open longitudinally, and found in the right kidney several stones of an irregular figure, branched like coral: they had extended themselves beyond the capacity of the pelvis on every side, though that was greatly enlarged, so as each of them to contain half a pint of pus, or more, forming for themselves cells in the parenchyma of the kidneys, which cells were all ulcerated within, and full of matter, communicating with the pelvis; the whole substance of the kidneys was scirrhous. The patient had long been troubled with grievous pains of the back, and had voided great quantities of pus with all the urine she made, so that there was no doubt of there being ulcers in her kidneys; and she herself often declared there were stones in the kidneys, which, on any motion, she could feel grate against each other.

The left kidney was likewise full of matter, and contained only one stone, larger than any of those in the right, nearly of a triangular figure, with the angles pointed at their extremities.

Of a large Lake, called Malholm Tarn, near Skipton in Craven, in the County of York. By John Fuller, Esq. Jun. F. R. S. N° 459, p. 612.

This tarn, or lake, of between 3 and 400 acres, is situated among high hills, by which it is supplied with water, which, after quitting the lake, runs in one stream for 2 or 300 yards, then sinks into the ground at two different places, which it is probable afterwards emerge, and immediately join the river Air.

Concerning a File rendered Magnetical by Lightning. By M. de Bremond, M. D. N° 459, p. 614.

In N° 437 of these Transactions, is given an extraordinary effect of lightning, which communicated magnetism to several iron tools. The following, from the coast of St. Andre in Dauphine, in 1739, gives an account of a fact of the same nature.

Some lightning fell on the house of a clock-maker. The thunder broke one of his files, 4 inches from the end; so that there still remained 7 inches of it in the handle; and the piece of 4 inches long, that was broken off, remained on the shop-board. Using the broken file afterwards, it was found to attract iron very strongly, like a magnet. But it was remarkable that this quality was given only to the inside of the file, at the broken part; for bits of iron applied to every side of it, had no effect, the virtue residing no where but in the place that was broken. Having broken in two the same piece of 4 inches, one of the two pieces attracted iron at both ends, the other only at its broken end. Rubbing the point of a knife on one of these two bits of the file, it communicated to the knife a degree of magnetism sufficient to raise needles, and hold them suspended.

An Account of Tumours, which rendered the Bones soft. Communicated to the Royal Society by Mr. Pott, Surgeon.* N° 459, p. 616.

In Nov. 1737, a gentleman, aged 27, complained of a swelling in the inside of his right thigh. On examination it appeared to be an encysted tumour of the steatomatous kind, lying loose between the sartorius and vastus internus muscles. Mr. P. took it out, and he got well in 6 weeks.

After this he continued well for near a year, except that he complained at times of a slight pain in the joint of that hip, which went off and returned at different times. He then fell into such a disposition to sleep, that no company or diversion, nor his own endeavours to the contrary, could keep him awake after 8 or 9 o'clock in the evening, if he sat down. This continued on him for 3 or 4 months, and then the pain in his hip became worse; for which he used the cold-bath, flesh-brush, and riding on horseback, but without any effect.

He then asked the advice of Dr. Beaufort, who put him into a course of the Æthiops mineral, cinnabar of antimony and gum guaiacum, with the Spa-water, and purging with calomel, by intervals: this method he pursued for a considerable time, but without any benefit. After this, by the advice of some

* The name of Percival Pott holds a conspicuous place in the catalogue of English chirurgical authors. He was one of Mr. Nourse's pupils, and not many years after his establishment in business he was elected surgeon to St. Bartholomew's Hospital, and got into very extensive practice. Thus circumstanced, he had abundant opportunities of making observations conducive to the advancement of his profession, and of his own reputation; which rose to a degree seldom equalled; insomuch, that for many years preceding his death, he was consulted in the treatment of most cases accompanied with difficulty, or requiring any great or nice operation, not only in the metropolis and its vicinity, but even in very remote parts of the country. The result of his experience on these and other occasions he communicated to the world in various tracts, published at different periods of his life, and which have since been reprinted collectively by Sir James Earle; to whom the public is indebted for an excellent account of Mr. Pott's life. This edition of his works contains the author's Treatise on Ruptures; his observations on Fistula Lachrymalis; his Treatise on Injuries to which the Head is liable from external Violence; his Remarks on Fractures and Dislocations; his Practical Remarks on the Hydrocele; his Treatise on the Fistula in Ano; his Observations on the Cataract; his Observations on the Mortification of the Toes and Feet; his Treatise on the necessity of Amputation in certain cases; his Remarks on Palsy of the Limbs from Curvature of the Spine; and some other tracts.

Mr. Pott was born in London in the year 1713, and died of a fever in 1788, at the age of 75. At this advanced age he had, in consequence of his temperate mode of living, so little infirmity of body, and so much activity of mind, that he continued to be busily engaged in the exercise of his profession, till within a few days previous to his death.

acquaintance, he took half a drachm of salt of hartshorn night and morning, in a draught of warm whey, for some time; but without any sensible effect, even by perspiration. Some little time after this, he began to complain of a slight periodical heat and thirst, which returned every night, with a quick hard pulse, but which was not so great as to make him uneasy.

It was Sept. 1739, when, having an opportunity of going with some friends, he determined to try what Bath would do for him: in his journey thither, the nocturnal heat and thirst increased so much, as to prevent his sleeping; but in the few days that he spent in recovering from the fatigue of the journey, they seemed to go off again. He then began to use the waters, both internally and externally; on which the last-mentioned symptoms again appeared, and he was obliged to desist, and use cooling medicines. His physicians then advised him to bathe the affected limb only; on which they returned again, and with such violence, that the further use of the waters was thought highly improper, and he left them off.

During this time the sight of his left eye became dim, and the dimness increased gradually for some little time, till he became quite blind of that eye; the bulb of it being considerably enlarged, and thrust forward out of the orbit.

For the most part of the time he had been at Bath, he had generally been very costive; and, on leaving off the water, had no stool for some days; for which reason a common clyster was given, and produced so profuse a discharge of serous matter, and continued for so many hours, almost incessantly, that he was reduced as low as possible.

For some time past, several small tumours had appeared in different parts of him, viz. 5 or 6 on his head, 2 or 3 in his back, and one in the neck, all lying just under the skin, and sensibly increasing every day, till they came to a considerable size.

December 2, 1739, he returned to London. His chief complaints now were an excessive languor, an inability to move his right hip, and when moved by another person, a very acute pain in it; an incapacity of sleeping when in bed, and an intense thirst in the night, with a quick hard pulse.

He now took the advice of Dr. Hartley and Dr. Shaw, who prescribed him the cinnabar of antimony 3 times a day, to drink the Seltzer water, and keep to a cooling regimen; and allowed him a moderate dose of the pill Matthæi every night; by means of which he got some sleep, of which he had for some time been absolutely deprived.

When he had taken the cinnabar 5 or 6 days, and during that time had no stool; a clyster was given, which brought away all the medicine, without the least alteration; nor was there ever after this time any appearance of any mucus being secreted by the intestinal glands, he never going to stool above once in a week, and then there came away a few lumps of excrement as hard as pieces of wood; which were expelled with much labour and fatigue; though he generally took an oily clyster to render it more easy, and washed down his medicines with a soapy draught.

The joint of the hip was now become quite stiff, all the inguinal glands being loaded with the same kind of matter of which the other tumours seemed to be composed; and a large cluster more of them might be felt under the glutei muscles, and behind the trochanter.

The cinnabar was now left off, and mercurial unction used; a proper quantity being rubbed in every night, stopping now-and-then to see what turn it would take; and in this course he continued for more than a month, but without any benefit, nor did the mercury produce any visible effect on him.

Sir Edward Hulse, being called in, directed the burnt sponge, which he took for some time, till, growing worse and weaker, he determined to try Mr. Ward. He took his sweating and purging medicines 2 or 3 times, but found no sort of effect from them; and being now quite tired of physic, and reduced extremely low, he determined to pass the rest of his time as easily as he could, by gradually increasing his opiate; and in this manner languished, incapable of stirring or helping himself, till the 2d of May 1740, and then died.

For a considerable time before he died, he was nourished by fluids only: yet, as soon as ever they were received into the stomach, in however small a quantity, they gave him an acute pain at the bottom of his belly, just above the pubes. For 2 months, or more, before his death, he could never make any water while he was up, but always made a good deal at different times when in bed. Soon after his return to London, Mr. P. opened the tumour he had taken out of his thigh 2 years before, and found the inside of it ossified.

. . . .

On dissection, the first thing that offered was a large tumour on the sternum, which had been perceived about 3 months before he died: it was as large as a turkey's egg, and so hard and immoveable, that Mr. P. was in doubt whether it was upon or under the bone. On removing the skin, it appeared covered by the expansion of the tendons of the intercostal muscles, and the periosteum: this coat being taken off, it was of a suetty kind of substance for about half an inch deep; and below this was a kind of cartilage, intermixed with a great many bony particles. Mr. P. then shaved off all this diseased body even with the surface of the rest of the sternum, but found no bone, it being quite dissolved and confounded with the mass of matter that composed the tumour, which was equally protuberant within the thorax, and composed of the same materials. Part of the 5th and 7th ribs were dissolved in the same manner, into a kind of substance between bone and cartilage, with a thick coat of steatomatous matter.

Within the cavity of the thorax were 37 of these diseased bodies, most of them attached either to the vertebræ or the ribs; and wherever they were attached, the cortex of the bone was destroyed, and its internal cellular part filled with the diseased matter.

Immediately above the diaphragm was a large scirrhous body, lying across the spine and the aorta, the latter of which lay in a sinus formed in its lower part; it had no attachment to any other part, and weighed $13\frac{1}{8}$ oz; and from its situation, must have taken its rise from some of the lymphatic glands lying about the thoracic duct.

From the origin of the aorta, from the heart, quite up to the basis of the cranium, all the blood-vessels were surrounded with these scirrhous bodies, and the thyroid gland was diseased in like manner, and bony within. On the left side was another of these bodies, made out of the glandula renalis, weighing $9\frac{3}{4}$ oz.

On the right, the glandula renalis was in a natural state; but the cellular membrane, which surrounds the kidney, was filled with a large cluster of these bodies of different sizes, some of them entirely suetty, others intermixed with bony particles: three or four of them were attached to the body of the kidney, and these were a sort of cartilage, beginning to ossify.

The pancreas was quite scirrhous, and very large. One very large tumour sprung from the spongy body of the third vertebra of the loins, the bony texture of which was so dissolved, and mixed with the matter of the tumour, that the knife passed through it with great ease.

The inner side of the os ilium, all the ischium and pubis, were covered with these appearances; and on removing them, the bone was found in the same state as the sternum and ribs. The middle of the right os femoris was surrounded with a mass of the same matter, and the bone underneath in the same state.

In the bottom of the orbit, surrounded by the recti muscles, was a pretty large steatoma, which occasioned the protrusion of the eye; and, by pressing on the optic nerve, probably caused the blindness.

A Case in which Part of the Lungs were coughed up. Presented to the Royal Society by William Watson, F. R. S. N° 459, p. 623.

Thomas Halsey, about 70 years of age, of a short make, and pretty fat, being in a tolerably good state of health, except that for some years before he was troubled with frequent coughing on any motion, was seized, Sept. 23, 1740, with a violent fit of coughing, in which he fell down, as if dead, and discharged near a quart of blood at his mouth, in a very large stream, mixed with many portions of a seemingly grumous matter. His coughing fit continued near 3 minutes. He revived upon bleeding at the arm, and being put to bed, recovered his senses, and was quite easy, and free from pain, except on coughing, which as often as he did, he spit blood visibly arterial from its florid colour. About 4 hours after the first fit, he was taken with a 2d, attended with the same symptoms as the first; and expired in it. On examining the blood, which he brought up at his death, Mr. W. found, in pieces of different sizes, near 3 oz. of the substance of the lungs, not ulcerated, or any ways distempered; and he had reason to believe there was near the same quantity of the lungs thrown up during the first fit of coughing. The pieces were easily distinguishable from grumous blood, by their connecting membrane, the acini in the internal part, and their specific gravity.

On examining the coutents of the thorax after death, the right lobes of the lungs were sound, of a good colour, and nowise injured. In the left cavity of the thorax, there was a large quantity of extravasated blood; the inferior left lobe adhered strongly both to the pleura and mediastinum, and was somewhat decayed; but of the superior left lobe, the upper part next the trachea adhered to the pleura for about 2 inches; and the remainder, where there had been no adhesion, he could perceive from the smooth surface of the pleura, was torn away by pieces, and discharged in coughing. As the greatest part of the left side of the lungs was tied down to the circumjacent membranes, the person being old, and the whole force of the parietes of the abdomen, diaphragm, &c. in the action of coughing, was unequally exerted on that part that did not adhere, and which, by the violence of the pressure, was torn off from the rest, and discharged.

It is worthy observation, how small the degree of sensibility is in the lungs; that a person should lose so much of their substance, as in this instance, on the first fit; and yet, on recovery of his senses, to complain of little or no pain from such dilaceration, when even the bite of an insect on the surface of the body is attended with so much. The adhesion of this man's lungs explained likewise the cause of his frequent coughing for some years before his death.

An Account of several Meteors, communicated by Thomas Short, M. D. Dated Sheffield, March 18, 1740-1. N° 459, p. 625.

The whole of 1737 was the most irregular year of any in my time; not one month but what had the weather of all the seasons in it, and that not by gradual transitions, but by sudden jerks; summer was dry, August was as cold as winter, September full of great changes; hence that sudden and general catarrh in October; succeeded in the latter end of the month, and all November, by a fatal diarrhœa among the poor.

Dec. 5, at 5 o'clock, afternoon, and all the evening, appeared a remarkable brightness in the sky, of a blood-red colour, very bright, and attended with many curious appearances.

This meteor was seen at Venice at the same time; and over Kilkenny in Ireland, it appeared like a great ball of fire; which burst with an explosion that shook great part of the island, and set the whole hemisphere on fire; which burned most furiously, till all the sulphureous matter was spent.

This meteor put an end to the remains of both the catarrh, and watery diarrhœa; and restored general health, till the next epidemic catarrh among infants in February 1738, 2 months after.

The next meteor was on August, 1733, a clear, calm, excessive hot day, at 9 at night, a frightful glade of fire, or draco volans, from east to west.

October 1, 1736, day cloudy, wind S.W. clear evening, six at night, fell a great ball of fire out of the air to the earth; no rain 15 days before, and only a few drops 2 days after.

August 28, 1738, at 5 P.M. wind S.W. sky clear, the sun bright shining, a fiery meteor appeared N.E. ran north, like a spear of fire, with a great round head, which burst like a rocket, spread about in a large fire, and vanished suddenly. This was a great drought, which continued without rain to September 7.

The next was December 2, 1739, at 6 at night, wind north, sky clear, a white frost, a great halo about the moon. This meteor appeared like a large round body of fire, of about a foot and a half diameter; seemed very low, therefore could not be observed far, though it went all over this country from north to south; pretty sharply, but nothing near so quickly as a glade of lightning, was succeeded instantly by a most dismal sound in the air, like carts, drums, and groans mixed. It kept the tract of the meteor, but in an opposite course, viz. from south to west. This was a most frightful time of rains, snow, storms, &c.

A Conjunction of Venus and Mercury, May 17, 1737, observed at Greenwich. By J. Bevis, M.D. N° 459, p. 630.

At $9^h\ 44^m$, the planet Mercury appeared immediately under Venus, their distance asunder not more than a 10th part of the diameter of the latter.

An Occultation of Aldebaran by the Moon, Dec. 12, 1738, P.M. observed in Fleet-street with a reflecting Telescope of 15 Inches in Length. By Mr. G. Graham, F.R.S. N° 459, p. 632.

The occultation at	5^h	27^m	6^s
Emerged at	5	29	59
Duration	1	2	53

An Eclipse of the Sun, Dec. 19, 1739, in the Morning, observed by Mr. Short in Surry-street, with a Reflecting Telescope of 16 Inches Focus, that magnified about 40 times. N° 459, p. 633.

The beginning could not be seen for clouds about the horizon. About 35^m after 8 o'clock, there was an opening, when the sun seemed to be about 2 or 3 digits eclipsed. The end was exactly observed at $9^h\ 1^m\ 45^s$, t. app.

An Eclipse of the Moon, Jan. 2, 1740, observed at Mr. Graham's house in Fleet-street, by Mr. Short, with a reflecting Telescope of 9 Inches Focus, that magnified about 40 times. N° 459, p. 633.

Beginning about	8^h	25^m	0^s t. app.
Beginning of total darkness at . . .	9	31	10
End of total darkness	11	15	20
End of the eclipse at	12	22	0

But the beginning and end could not be distinctly seen for clouds.

Some Remarks and Experiments concerning Electricity. By the Rev. J. T. Desaguliers, LL.D., F.R.S. N° 459, p. 634.

1. Bodies electric per se, are such in whom a virtue of attracting and repelling small bodies at a distance is inherent, though it is not always in action, so as to produce that effect. But by rubbing, patting with the hand, hammering, warming, and sometimes only exposing to dry air, such bodies exert the virtue above mentioned; otherwise they are in a non-electric state.

2. Non-electric bodies, are such in which no electrical virtue can be excited

by any action on the bodies themselves, such as rubbing, warming, &c. But an electric per se, when excited, can communicate its virtue to a non-electric, and that virtue will be received by all the parts of the non electric, be the body ever so long or large, and be strongest, being as it were collected at that end of the non-electric, which is farthest from the place where the electricity is first received.

3. A non-electric, having received electricity, will communicate to another body brought to touch it, or only brought pretty near, and that often with a snapping noise, and a small flash of light, losing by that means all its own electricity.

4. An electric per se will become a non-electric for a time, if it be made wet or moist, and become receptive of electricity, which it will receive at one end, and carry to the other, where the electricity will go off with a small explosion, to impregnate any other non-electric, which is brought near.

5. An electric per se, in which electricity has been excited, may become non-electric by being exposed to moist air, whose humid vapours it attracts; and then, brought to the fire, or into very dry air, recover its electricity when the moisture is exhaled again.

6. An electric per se may be made strongly electric in part of its length, while the other part remains in a non-electric state.

7. A body in a state of electricity, whether a non-electric having received electricity, or an electric per se, excited to electricity, will attract all non-electrics, and repel other bodies that are in a state of electricity, provided the electricity be of the same kind.

8. A non-electric body will not retain the electricity which it receives from an electric per se, unless it be free from touching any other non-electric body; but must be suspended or supported by electrics per se touching only them and the air.

9. An electric per se, when it is not reduced to a non-electric state, will not receive electricity from another electric per se, whose electricity is excited, so as to run along its whole length; but will only receive it a little way, being as it were saturated with it.

10. An electric per se will not lose all its electricity at once, but only the electricity of such parts of the body as have communicated it to other bodies, or near which non-electrics have been brought.

11. When a non-electric, which has received electricity, communicates its electricity to another, it loses all its electricity at once; and the effluvia, in coming out, strike the new body brought near, as well as the body first made electric.

12. Excited electricity exerts itself in a sphere round the electric per se; or rather a cylinder, if the body be cylindrical.

13. The electricity which a non-electric of great length, for example, a hempen string 800 or 900 feet long, receives, runs from one end to the other in a sphere of electrical effluvia. But all the supports of this string must be electrics per se.

14. If this string be branched out into many strings, the electricity will run to all their ends.

15. If the non-electric string, which is to receive and carry on the electric effluvia, be not continuous, but has between its ends some electrics per se, the effluvia will stop at the first of them, unless the interruption or discontinuation of the non-electric be short; because in that case the electricity jumps from the end of the first non-electric to the beginning of the next, especially if the air be very dry, even though the ends of the string should be about a foot distant, and no body but the air between. Sometimes indeed the distance must not be above an inch or two.

There are two sorts of electrics per se, known by what follows. A body impregnated with electricity from one sort will repel all bodies that have that sort of electricity, till they have lost their own electricity by coming to some non-electric. But an electric per se of the other sort, though excited, will attract all those bodies, though in a state of repulsion on account of the other electricity; and so vice versa.

Some Electrical Experiments made before the Royal Society, Jan. 22, 1740-1. By the same. N° 459, p. 637.

It being a matter in dispute, whether there is any difference between the electricity of glass, and that of gums and resins, Dr. D. made the following experiments to settle that point.

He fastened a string of dry cat-gut, which when dry is an electric per se, from one pillar to the other, at the end of the table in the meeting-room of the Royal Society, about seven feet from the floor; and to the middle of that cat-gut fastened a silken thread about 2 feet long, which hung down, and at its lower end had a down feather. Then rubbing the end of a stick of wax pretty quick and strongly against his cloth waistcoat, the wax became electrical, and attracted the feather, which stuck to it awhile, and then was repelled from it, as long as it retained the electricity it had received from the wax: but having touched the feather with his finger, it lost its electricity; and, becoming a non-electric, was again attracted by the wax, which gave it fresh electricity; and then it was repelled from it, and so toties quoties. When the feather was in its electric state, he applied to it another stick of wax, which was first rubbed; and it repelled the feather, though it had not touched it before, and did the same as the other stick of wax had done.

After that he rubbed a glass tube, which first attracted and then repelled the feather, as the wax had done. And another tube, being rubbed, repelled the feather, when it was put into an electric state by the first tube, without first attracting it. But non-electrics, such as the finger, or a stick, attracted the feather, when it had first been made electric; and not only so, but electrics per se, when they were become non-electric, as the tube unrubbed, or the wax unrubbed; nay, the rubbed tube also, when its end was moistened, or that end of it turned to the feather, which had been held in the hand.

Then he made the feather electric, by the application of the excited tube; and having rubbed the wax, to give it electriciiy, he brought it near the feather, which it attracted strongly, though it had repelled it before, when the feather had been made electric by wax.

Afterwards he made the feather electric by the wax, which first attracted and then repelled it: and, having applied the rubbed tube to the feather, it attracted it strongly, though it repelled it when the feather was made electric by another glass tube.

Electrical Experiments made before the Royal Society, on Thursday, March 15, 1740-1. *By the same.* N° 459, p. 639.

Dr. D. having showed lately by some plain experiments, that the electricity of glass is different from that of sealing-wax; because the wax attracted a feather suspended in the air by a fine silk, when the rubbed glass tube repelled it, he made the experiment with a cake of rosin, instead of sealing-wax; and it appeared to have the same kind of electricity as the wax. Then considering that the supporters of any non-electric conductors of electricity, must themselves be electric, he tried whether bodies, endued with either kind of electricity, were in any-wise different in that case, by the following experiments.

He laid a piece of wood, 4 feet long, on 2 glass plates, whose ends stood 1 foot beyond the side of the table on which they were laid; then applying the rubbed tube to one end of the wood, the other attracted leaf-brass, or a thread hanging down from a stick. Then, instead of the glass plates, he laid the long piece of wood on 2 cakes of rosin, and applied the rubbed tube to the end of the said wood, which conducted the electricity to the other end, where leaf-brass and the thread were attracted in the same manner.

This shows that, in order to conduct electricity along any non-electric body, it is indifferent what kind of electricity its supporters are endowed with, provided they are but electric.

Concerning an Extraordinary Hernia Inguinalis. By John Huxham, M. D. F. R. S. N° 459, p. 640.

Mr. Burman, a taylor of Plymouth, about 40, had from his childhood laboured under a small inguinal rupture on the right side; but about 6 years before his death, from a blow received in his groin, the hernia became very large, and the gut always remained down in the scrotum; for he wore no bag, truss, or the like, to support it. The day before his death, he was following his work, as usual, with his pressing-iron, without any violent jerk or straining; when, about 10 in the morning, all at once, he felt a very great pain in his right inguen; which, continually increasing, in 2 or 3 hours threw him into vomitings, cold sweats, &c. His apothecary gave him a clyster, which brought off a small matter of thin stool; but gave no relief, though it had been formerly very serviceable to him in the like disorder.

About 8 in the evening Dr. H. was sent for, and found him in cold sweats, with scarcely any pulse. The hernial tumour was prodigiously large, and exceedingly hard; the pains extremely violent, which caused excessive languors. He ordered, that he should be placed in a proper posture, that a warm aromatic emollient fomentation should be frequently and long applied, and that a reduction of the intestine should be attempted; or, if that did not succeed, that the operation for the bubonocele should be performed. The fomentation was tried a long while, emollient terebinthinate clysters injected, and the reduction attempted, for an hour or two, by a skilful surgeon, but in vain; nay, the swelling increased considerably during the application; and the pain became, if possible, more aggravated all over the hernia, which before was chiefly at, and near the rings of the abdominal muscles; and this too, though he took, with an easy cordial, and mulled wine, Laudan. Solid. gr. ij ʒiis horis.

Early the next morning, Dr. H. saw him again; and finding that he had not slept a moment, the tumour considerably increased, and excessively hard, though not discoloured, and the patient exceedingly weak and pained, he advised the operation forthwith, as the only possible means of saving him: but the patient was unwilling to admit of it, and we were all indeed diffident of the success. While a fresh fomentation was getting ready, the poor man expired in agonies.

About an hour or two after they opened the scrotum, which in so short a space of time appeared all livid, and the blood vessels were extremely turgid and varicose. On cutting through the teguments, part of the colon and ilium thrust out with great force; they were both prodigiously distended with wind, highly inflamed, and in several places very livid. That part of the guts commonly called cæcum, was blown up into a kind of globular figure, as large as a child's head. It was remarkable, whether it was in the original conformation, or by the vast distention, that there was no manner of appearance of the appendix vermiformis to be found, though diligently sought for. And further, that the cæcum was vastly thicker set with glands, and these much larger than he had ever seen before in any subject. The convolutions of the ilium and colon were so immensely distended with wind, that the valvular corrugations in both almost totally disappeared. Yet exactly at the valvula Tulpii, *alias* Bauhini, there was a very great constriction of the intestinal canal, as if tied strongly with a cord; and though we opened the colon about a hand's breadth beyond the valve, and let out the flatus, we could not possibly press any wind from the ilium into the colon through the valve. Dr. H. suspected indurated excrement, as an obstacle; but on a careful inquiry, he only found the whole valvular production, and the end of the ilium, at its insertion into the colon, highly inflamed, and quite shutting up the passage. On dilating the rings of the oblique and transverse muscles, the wind rumbled up out of the ilium into the cavity of the belly very readily. They found pretty much bloody sanies in the guts, on slitting them open, but little or no indurated fæces: a manifest proof, that the exceeding hardness of the tumour was owing only to the excessive flatulence, and great inflammation; and shows how much we may be deceived in our conjecture on like occasions. The tumour of the scrotum was 28 inches round. There was no adhesion of the intestines to the containing parts, though he had so long laboured under the hernia.

This unhappy case gave Dr. H. a severe reflection, and he thought the malady was much increased by the repeated application of the hot fomentations; as it rarefied the air greatly, and, by relaxing the parts, gave further room to the vast expansion.—At that time he had never seen Belloste's second part to his Hospital-Surgeon, where he advises, in such cases, the most cold astringent fomentations. In this and the like, they might have been very proper; especially if a portion of spirit of wine camphorated had been added to prevent mortification.

It sometimes happens, that though the annular perforations of the abdominal muscles are dilated by the operation, yet the hernia cannot be reduced.—Dr. H. believes, as the guts were distended to so enormous a bulk in this man, it would have been impracticable. In such cases may it not be proper to prick them with a needle, to let out the flatus, as is commonly practised in small wounds of the abdomen, where the intestine thrusts out, and becomes so turgid with wind, that it cannot otherwise be returned?—In some ventral ruptures, as they are called, this also may be necessary. He finds Mr. Sharp, in his late excellent piece of surgery, approves of this method, from an old English practitioner, who had often used it with success.—Dr. H. was persuaded, punctures in this manner are much less dangerous than the operation; and believes, in such cases, may be more effectual. It is a common thing with graziers and cattle-doctors, to prick the guts of their sheep and bullocks with great success, when, by feeding on clover, or fresh young grass, their guts become so vastly distended with wind, as would otherwise certainly kill them. May not a very small hollow needle with perforations, as in that used by some instead of the trocar for a paracenthesis, be more proper than a common needle? May not the hernial tumour be perceived to be chiefly flatulent by its being in some degree transparent on applying a candle, as used in the hydrocele? and may not that direct the proper place for punctures?

An Observation on the Planet Venus, with regard to her having a Satellite. Made by Mr. James Short, F.R.S. at Sun-rise, Oct. 23, 1740. N° 459, p. 646.

Directing a reflecting telescope of $16\frac{1}{2}$ inches focus, with an apparatus to follow the diurnal motion, towards Venus, Mr. S. perceived a small star pretty near her; on which he took another telescope of the same focal distance, which magnified about 50 or 60 times, and which was fitted with a micrometer, in order to measure its distance from Venus; and found its distance to be about 10′. Finding Venus very distinct, and consequently the air very clear, he put on a magnifying power of 240 times, and, to his great surprise, found this star put on the same phasis with Venus. He tried another magnifying power of 140 times, and even then found the star under the same phasis. Its diameter seemed about a third, or somewhat less, of the diameter of Venus; its light was not so bright or vivid, but exceedingly sharp and well defined. A line, passing through the centre of Venus and it, made an angle with the equator of about 18 or 20 degrees.

Mr. S. saw it for the space of an hour several times that morning; but the light of the sun increasing, he lost it altogether about a quarter of an hour after 8. He looked for it every clear morning after, but never had the good fortune to see it again.

Cassini, in his astronomy, mentions much such another observation. Mr. S. likewise observed two darkish spots on the body of Venus; for the air was exceedingly clear and serene.

An Occultation of Jupiter and his Satellites by the Moon, Oct. 27, 1740, *in the Morning; observed at Mr. George Graham's, in Fleet-street. By Dr. Bevis and Mr. James Short, F. R. S.* N° 459, p. 647.

15^h	51^m	2^s	The moon's centre passed the meridian.
15	52	28	Jupiter's centre passed the meridian.
15	54	36	Jupiter's 3d satellite eclipsed by the moon.
16	0	31	Jupiter's 2d satellite eclipsed by the moon.
16	8	25	Jupiter's preceding limb immerged.
16	10	41	Jupiter's subsequent limb immerged.
16	14	15	Jupiter's 1st. satellite eclipsed by the moon. These immersions were taken with a reflecting telescope, of 16.5 inches focus, that magnified 120 times.

None of the emersions could be seen for clouds. While Jupiter was immerging, the sky was perfectly serene; and, at his nearest approach to the moon, he did not appear to alter his figure in the least, nor to be tinged with any prismatic colours; neither did he, as is said to have been sometimes observed through refracting telescopes, seem to enter at all on the moon's body.

That part on the moon's limb where Jupiter entered, was a hollow; and though some are of opinion, that the circumference of the moon, as it is bounded to our eye, is a perfectly smooth circle, and that no hills or hollows appear there, as on the surface of the moon; yet if it be looked at in a clear night with a good telescope, that magnifies about 100 times, or even less, it will be seen rugged and uneven all round.

Notwithstanding Jupiter's light seems to be more vivid than that of the moon, when he is seen at a good distance from her, and far more so when the moon is away; yet the contrary is plainly discerned when they are near each other: and in this observation, while Jupiter was immerging behind the moon, his disk appeared much dimmer, and of a more faint and dusky complexion, than the disk of the moon.

A Letter from James Parsons, M. D., F. R. S. to the R. S. giving a short Account of his Book intitled, A Mechanical Critical Inquiry into the Nature of Hermaphrodites. London, 1741, *in* 8*vo.* N° 459, p. 650.

This treatise was written at the time when an Angolan was publicly exhibited as an hermaphrodite. The intent is to prove contrarily to common opinion, that there are no such things as hermaphrodites in the human race. 1. The introduction, which is chiefly historical, lays down the manner of this error's being propagated among Jews, Pagans, and Christians, at all times; with an account of Jewish, civil, and canon laws made against such as were reputed hermaphrodites, as well as those that were always in force at Rome, by which great numbers of people were destroyed from time to time.

2. The 1st chapter exhibits many reasons against a possibility of their existence in human nature; with a true discovery of such diseases as have been the cause of men and women being called hermaphrodites.

3. The 2d chapter is a critical account of the causes authors have assigned for the produce of hermaphrodites; wherein it is proved, that no such effects could arise from those causes; and several absurdities are exposed in the arguments advanced for the support of this error.

4. The 3d chapter is a critical view of the histories of hermaphrodites given by several authors; showing that those so reputed were either perfect men or women, having only some deformity or disease in the parts of generation.

5. The conclusion describes the state of all female fœtuses, with some observations which the author laid before the R. S.; which prove that every female fœtus may as well be thought an hermaphrodite, as any that were ever so called.

Of an ancient Date in Arabian Figures, on the North Front of the Parish Church of Rumsey in Hamphire. By the Rev. Mr. William Barlow. N° 459, p. 652.

As the knowing how long the Arabian or Indian figures have been used in the west, may sometimes be a means of distinguishing spurious from genuine dates; so a wrong hypotheses may possibly induce us to suspect genuine dates to be doubtful or spurious. To give some light to this subject, Mr. B. has sent a draught of part of the north front of the abbey (now parish) church of Rumsey, in the county of Southampton, with an inscription on it, represented fig. 9, pl. 9. That this inscription is a date, 1011, is evident from the figures. That it is a genuine date, the apparent antiquity of the building plainly demonstrates. A spurious date in this place would have expressed the time when the abbey was founded by King Edward, grandfather of Edgar, above 100 years before the time here mentioned.

There is something very remarkable with regard to the time when this church was built. Not only during the year of this date, 1011, but for several years before, many parts of England were laid waste by the revenging Danes, justly incensed against the English by the massacre of their countrymen in the year 1002. The Saxon Chronicle, p. 141, acquaints us, that the county of

Hants, Damtun-ꝼcıꞃe, among others, was miserably harassed by these cruel invaders this year of the date. It is therefore very extraordinary, that so fine a pile, according to the age when it was builded should be raised at a time when every thing else, sacred and civil, was plundered and destroyed by these merciless ravages. But probably the devastation was not quite so general as represented.

If this be a genuine date, it is probably the oldest, Indian or other, that has yet been noticed in England, perhaps in Europe; and quite destroys the opinions advanced by Scaliger, Vossius, F. Mabillon, Dr. Wallis, and other learned men, concerning this matter.

Observations concerning the Virtue of the Jelly of Black Currants, in curing Inflammations in the Throat. By Henry Baker, F. R. S. N° 459, p. 655.

In this paper Mr. B. states, that being frequently attacked with inflammation in the throat, and not finding sufficient relief from the usual remedies, he was at length advised by a clergyman of his acquaintance to swallow leisurely a small quantity of black currant jelly, or if the jelly could not be got, a decoction of the leaves in milk, or even of the bark (if it should happen in winter) used by way of gargle. He tried the jelly prepared from the juice, and it had the desired effect, and he afterwards recommended it to many of his friends, who obtained similar relief from it. From a particular observation of its effects during the attack of inflammatory angina, it operated by perspiration, and in that way carried off the disorder.

Several Electrical Experiments, made at various Times, before the Royal Society. By the Rev. J. T. Desaguliers, LL. D., F. R. S. N° 460, p. 661.

The first of these experiments were made before the R. S. May 14, 1741; and were adapted to prove what the Doctor had mentioned in one of his former papers concerning electricity, that electrics per se would not receive the electricity of a rubbed tube, so as to carry on to a distance; but that, if those bodies were changed into non-electrics, they would then receive and convey the electricity of the rubbed tube, in the same manner as all other conductors of electricity do.

The 2d set were made before the Society, on Thursday, May 28, 1741; in order to prove that it is not the quantity of matter in bodies, that makes them more or less receptive of electricity, and conductive of it, but entirely their quality.

The 3d set were performed before the Society, Thursday, Aug. 29, 1741, to the following effect.

Dr. D. having found, by several of Mr. Gray's experiments, as well as some of his own, that water is receptive of electricity, so as to be raised up in a little cup, to emit a vapour towards the rubbed tube, to snap, and to give light; having also found, that when a dry tube, suspended horizontally, will not conduct the electricity of the rubbed tube applied to one of its ends; and yet, when blown into, will conduct it strongly all its length, because the electricity runs along from one moist particle to another, though those particles are not contiguous——he thought that electricity might impregnate a whole jet of water, whether perpendicular, oblique, or horizontal: and supposed also, that if at any time there be electrical effluvia in or above a cloud, that virtue may be communicated by the falling rain, to any thing that the rain falls upon. How far this conjecture is true, will appear by the following experiment.

Having properly suspended, that is, suspended by some electric body, as here cat-gut, a copper fountain with the spout downwards, the Doctor opened the cock, and let the water spout into a vessel underneath: then, having excited a great tube to electricity, he held it over the copper fountain, while an assistant held the thread of trial, that is, a thread hanging from a stick, near several parts of the jet, which attracted it sensibly: he then applied the rubbed tube near the falling jet, which attracted it strongly, so as to bend it into a curve, and sometimes cause it to fall out of the vessel below.

Concerning an Extraordinary Venereal Case. By John Huxham, M. D., F.R.S. N° 460, p. 667.

Mr. R. B. aged about 27, of a bilious, dry constitution, had for some years before his death, contracted a virulent gonorrhœa, which was scarcely well cured before he got a 2d, and at length a 3d. To complete his misery, being in the fleet at Portobello, he had frequent impure conversation with some of the negro women, who probably laboured under the worst species of pox, called the yaws.

He returned with a very troublesome itching all over him, though no pustules appeared; was much thinner than usual, and had a horrible stinking breath, and spit frequently a foul, corrupt matter. As he had no running, ulcer, bubo, or nodes, he thought all safe. But not many days after his arrival at Portsmouth, post impurum cum impurâ coitum, a violent green-coloured gonorrhœa appeared. For this he put himself under the care of a surgeon, who, after much pains to no purpose, endeavoured to salivate him, but that also in vain.

The gonorrhœa indeed was much abated, but a bubo was risen in his left groin, and some small verrucose eruptions about the anus.

In this condition he returned here, and put himself under the hands of Mr. St——, an ingenious surgeon, who endeavoured to bring the bubo to suppuration, but without effect; for it soon receded, and presently violent pains seized him in and about the fundament, which soon produced an exceedingly painful phyma near the verge of the anus on the left side.

Dr. H. was now consulted, who advised to bring it to suppuration as soon as possible, which was done in 2 days; from whence issued abundance of purulent bloody matter.—In a day or two more, another appeared on the other side, which soon vented the like matter.—The verrucæ also now grew more numerous and larger, and many pustular and scaly eruptions appeared all over him.

Dr. H. ordered him to be fumigated with cinnabar, and advised him forthwith to enter on a salivation. But, antecedent to it, as his humours were exceedingly tough and acrid, put him on a course of very plentiful dilution; and this the rather, as he was naturally of a dry and hot constitution, and besides had lately been roasted in the torrid zone.

Dr. H. began, as usual, by giving him calomel; which, though it neither purged or vomited him, yet, after having taken 5 drachms, produced no degree of salivation, nor did it make his gums sore.—However, it brought on his gonorrhœa again: Dr. H. then ordered him once and again, 8 or 10 grs. of turbith mineral, which scarcely puked him, and gave him only 2 or 3 stools. Dr. H. now found indeed that mercury and he, as well as Venus, had been old acquaintance; so he greatly augmented the dose of the mercurials, ordering immense quantities of thin watery diluents: notwithstanding this, there was very little operation by stool, and scarcely any by salivation. Though his gums and fauces were very sore and swoln, he scarcely spit 1 pint in 24 hours, and that excessively tough and fetid. Even under this strong mercurial course, the pustular and leprous eruptions increased daily, so as to cover almost his whole body, even his very face. His hands and feet were vastly swoln, as in an elephantiasis, with horrid fissures, from whence issued a very stinking ichorose matter.

Dr. H. was quite confounded at this dreadful state of things, and seriously bethought what further method could be taken against so terrible an enemy. He had recourse to a warm emollient bath, in which his whole body was immerged; after which he was well anointed with a strong mercurial ointment. This was done for 3 days successively: notwithstanding which, though his chaps grew exceedingly sore, and his throat so much inflamed and pained, that he swallowed with extreme difficulty what he sucked through a pipe or quill, yet the spitting was very little increased, and as tough as ever: nor did the fistulous ulcers seem in the least disposed to heal up, but vented a vast deal of stinking, oily, sanious matter; and even new ones broke out under each axilla, and a very large phyma rose on the coccyx, which soon discharged the same kind of virulent matter; though we found the bone, and even the periosteum, quite sound and untouched.

The scales were now grown so hard and stiff, that he could scarcely bend a limb, or finger: also, abundance of ulcers, from whence flowed great quantities of greasy, purulent, and somewhat bloody matter, were broke out in his thighs and buttocks. A very large tumour was also risen in his right breast, and soon after on the left, voiding prodigious quantities of the same kind of matter.

It was observable, that wherever any of these ulcers appeared, they ran only under the skin, being entirely seated in, and feeding on, the membrana adiposa; so that the muscles and tendons underneath appeared as fair and florid as in the most healthy constitution.

Dr. H. now unfortunately found, though too late, there was nothing to be done by mercury in any form; and therefore determined to run it off, and try the guaiacum method and sweating, so much recommended of old, and in some cases so justly, by Sir Ulric Hutten, and others; at the same time keeping up a most plentiful dilution, attempting withal to detach the scaly cuticle by continued emollient baths, which at the same time also would partly act by dilution. By this means the scales came off apace, just in the manner usual in the confluent small-pox; only the exuviæ were here much larger, several being above 4 or 5 inches over. In about a week's time, this coat of mail was pretty well cleared off, and his breath, from the most horribly nauseous he ever smelt, became as sweet as that of an infant. Nor was the matter spit, though still very viscid, any way fetid: for the mercury was pretty well run down by lenient cathartics, and the sloughs of his mouth cast off.

He was now become exceedingly emaciated: wherefore he ordered him plentiful liquid nourishment with vipers, and large dilution, avoiding every thing that was in the least gross or fatty. But with all this he still kept to his three pints of strong decoction of guaiacum every 24 hours, sweating at least 2 or 3 of them.

Under this method Dr. H. conceived some hopes of his recovery, as he seemed now to gain some small degree of strength and spirit; but still his ulcers rather increased than abated, and continually discharged a vast quantity of matter, though by no means so thick, putrid, or bloody; and, indeed, in a most profuse manner from under each axilla.

But, what is vastly surprising, notwithstanding all the past methods and medicines, 2 very large chancres now appeared on the glans penis, and a very considerable bubo in the left groin. A troublesome cough soon also seized him, with shortness of breath; and be began to expectorate a purulent, and sometimes bloody kind of matter. As the whole membrana adiposa without, had been consumed by the disease, it was now falling on that part of it that invested the more vital parts. But nature could support no longer. He died in the extremest degree of a pocky consumption. But not one single bone of any part of his body appeared to be touched, though he died with near 40 ulcers upon him.

An Account of Coal-balls made at Liege. By William Hanbury, Esq. F. R. S. N° 460, p. 672.

This method of making coal-balls, has been much used at Liege, as also sometimes in England and other parts. The way is, to collect the small dust of pit coals, which would otherwise be useless or thrown away among the ashes, &c. and temperate it up with water and some smooth fat clay, working it up into balls or bricks, or pieces of any size and shape.* After being dried, these burn easily and pleasantly, making a fire that is strong, clear, and very durable.

The proportion of the two ingredients is various, from $\frac{2}{3}$ of coal and $\frac{1}{3}$ of clay, to the reverse, or $\frac{1}{3}$ of coal and $\frac{2}{3}$ of clay, according to their different natures; or most usually the medium proportion between those, viz. half and half, or an equal quantity of each.

A short Account of Dr. Alexander Stuart's Paper concerning the Muscular Structure of the Heart: which was read at several Meetings of the R. S. in May and June 1735. *By Cromwell Mortimer, M. D. Sec. R. S.* N° 460, p. 675.

Dr. Mortimer premises that the sketch exhibited in this paper of Dr. Stuart's discoveries is drawn from memory, he not having any of the Doctor's papers by

* Probably the above practice gave rise to the present modern way of making bricks, now commonly used about London, &c.; which is, to mix with the clay, or brick earth, a large portion, about a 4th, of the fine dust sifted out of the common refuse of fires, coal ashes, which usually contains a considerable portion of the small fine coal, which runs through the grate among the ashes. This fine dust intimately incorporated through the brick, not only helps it to burn readily and thoroughly, but also gives it on the outside a pleasant grey colour, which would otherwise naturally be a deep red.

him, except some drawings. He does not undertake to give a description of all the parts belonging to the heart, supposing them already sufficiently known from the anatomical writers; but only explains the surprising simplicity of its muscular structure, as the ingenious Dr. Stuart has demonstrated it from various preparations of boiled hearts; viz. that the heart is nothing else but a single muscle of nearly a semicircular form, whose fibres are all parallel.

This structure the Doctor endeavours to imitate, by certain lines described on a plane, which being cut out in a circular form, and then rolled up into the shape of a truncated cone, gives a rude idea of the position of the fibres in the heart, though perhaps not near so clear and intelligibly, as by proper preparation and exhibitions of the heart itself.

The several courses of the fibres may be easily traced in a boiled heart; and if they are not found to answer to the directions of the lines on the paper-cone with the strictest mathematical exactness, when rolled up, it must be ascribed to the form of the heart, which is not exactly conic, though nearest reducible to that figure; and because the base is not a plane as in the paper-cone, but of a convex round form; and the tendinous circle round it is of a smaller diameter than the middle part of the heart.

By this structure and circumvolution of the fibres, the muscle which constitutes the heart, by a simple contraction of its length, by those external fibres which encompass both ventricles, contract the diameter of the heart, while by the internal fibres, that form the septum and inside of the left ventricle, it shortens its length, or draws the apex up nearer to the base: this is done without any contrariety in the action of these fibres, or destroying the force of each other; but, on the contrary, they being all parallel to each other, and a continuation of the same fibres, assist each other in their action.

The Doctor supposes this contraction is not caused so much by the influx of the nervous spirits, as by the influx of the arterial blood, through the coronary arteries into the substance of the heart; and that the contraction of the auricles comes from the same cause; which will be alternate with that of the heart, because the lateral branches, which arise out of the trunk of the coronary artery, that encompasses the base of the heart and both auricles, are on one side distributed into the substance of the heart, and on the other side into the coat of the auricles; and will be alternately compressed, and alternately free, as the auricles and ventricles are alternately full or empty of blood.

Concerning the Foramen Ovale being found open in the Hearts of Adults, and of the Figure of the Canal of the Urethra. By M. le Cat, M. D., F. R. S. &c.* N° 460, p. 681.

In the winter of 1734 M. le Cat opened a great number of dead bodies of men grown, and did not find the foramen ovale open in any of them. The oldest of the male subjects, in which he found it open, was a lad of 15 years of age. Of 20 bodies of women, which he examined, in 7 he found the foramen ovale open.

Among the number of openings that remain of this foramen, there is a great variety in their shape, and in that of the cicatrices or adherences of the valve: however, they may conveniently be reduced to three sorts.

The second sort of foramen ovale open in the adult, differs from the first sort, in being more sunk in, or more approaching the shape of a funnel. The same foramen ovale of the second sort, seen on the side of the left auricle, differs from the same side of that of the first sort, by the valve beginning to make the goose-foot by its different attaches, which much resemble the columns of the mitral valves of the heart. The foramen ovale of the third sort open in the adult, differs from the preceding two, by the foramen ovale, nearly forming a funnel. The same foramen ovale viewed on the side of the other or left auricle, differs from the preceding ones, by the goose-foot formed by the valve being much more compounded.

The women in whom was found the foramen ovale of the second and third sort, were about 60 years of age.

The necessity M. le Cat was under of sounding frequently, and the difficulties he sometimes met with in this nice operation, made him resolve scrupulously to examine the figure of the canal of the urethra. On this he made a number of experiments, two of which are here described.

1. He melted resin with wax, and injected this liquid through the urethra. He filled the bladder but half way with it, in order to preserve all the wrinkles of the canal. When the injection was cold and solid, he cut through the ossa

* Claude Nicholas Le Cat taught and practised surgery at Rouen. He was, as Haller has remarked, a man of considerable ingenuity, but was rather too confident in his own abilities, and too fond of inventing new hypotheses. He cultivated anatomy, physiology, and pathology, with great assiduity; nevertheless, it is suspected that some of his anatomical observations were made inaccurately, and many of his opinions are regarded as paradoxical. He wrote a number of dissertations, such as a Treatise on Muscular Motion; another on the Cause of Menstruation; another on the Cause of the black Colour in Negroes; another on Lithotomy, &c. but his principal works, are his Traité des Sensations et des Passions, and his Theorie de l'Ouie. A pension et des lettres de noblesse were conferred upon him by Lewis XV. M. le Cat died in 1768, aged 68.

innominata. He dissected the left side of the canal and bladder, and the section of these parts gave him fig. 4, pl. 10, the explanation of which is as follows:

A represents the glans; B an elbow, which the ligamentum suspensorium causes the penis to make; C folds, or wrinkles, of the bulb or of the gulf of the urethra; D the entry or straits of the prostate; E the gulf of the prostate, or the verumontanum; F elbow, or straits of the entry into the bladder; G a section of a portion of the bladder; H a section of the pubis; I the root of the left corpus cavernosum cut through.

2. He injected another subject with very thick glue, entirely filling the bladder with it through the canal of the urethra, till it was somewhat stretched. He let this injection remain till the next day, and then found it solid and elastic. He cut the parts round it, as he had done in the preceding subject; and afterwards he made an exact division of the injection: he put one half of it on paper, in order to have its shape exactly; and thus he obtained fig. 5, having added, in pricked lines, a pretty exact section of the adjacent parts.

A represents a section of the bladder; B a section of the pubes; C the cavity of the abdomen; D the peritonæum; E the integuments of the abdomen; F the space between the pubes and the peritonæum, taken up by the cellular membrane, being the place of the incision in the high operation of lithotomy; G the rectum; H the glans; I the corpus cavernosum; K the urethra; L the elbow of the ligamentum suspensorium; M the bulb or gulf of the urethra; N the straits and elbow at the entry of the gulf of the prostate; O the gulf of the prostate; PPP sort of elbows, or blind cavities, found in it; Q the straits of the entry into the bladder.

Remarks on the Weather, and accompanying three Synoptical Tables of Meteorological Observations for 14 *Years, viz. from* 1726 *to* 1739, *both inclusive. By Geo. Lynn, Esq.* N° 460, p. 686.

Mr. Lynn having, for 14 years, kept a constant register of the altitudes of the barometer and thermometer, the quantity of rain, the course of the winds, &c. the first 5 years of which have been already communicated to the Royal Society. He now sends the remaining 9 years at large, ending Dec. 1739, in the same method as formerly. But, believing it would be of good use, both here and abroad, if the mean heights of the barometer, thermometer, and quantity of rain in every month of the whole 14 years, with the collateral means, both of the months and years, were brought all into one view together, he has ranged them accordingly in a small table. On the whole it appears, that the mean height of the barometer, for the whole 14 years, is 29.58 inches; the mean quantity of rain annually, 23 inches; and the mean altitude of the thermometer 56,52,48, that is, at the coldest time of the day 56, at the hottest 48, and their mean 52. The thermometer made use of, was that of Mr. Hauksbee, and kept constantly in the same place. And the altitudes of the thermometer are taken but twice a-day, viz. at the coldest, which is at sun-rise, or sometimes a little after; and at the hottest, viz. between 2 and 4 in the afternoon: by which method are gained the proportional heats for every month in the year, and their difference, as also between that of day and night, for 13 years together.

Mr. Lynn was not a little surprised to find, in casting up the column of the mean altitudes of the thermometer collaterally, that as those for July, being the hottest month, are 41,35½,30, so the altitudes of June and August, on each side of it, come out exactly equal to each other, and also those of May and September; these last only differing in their morning and evening heats or altitudes, which does not alter their medium of 44½. The following are some few remarks added on the weather.

When there is a haziness in the air, so that the sun's light fails by degrees, and his limb is ill defined, it is a pretty certain sign of rain, especially if the mercury falls. The like haziness, at night, is still more a sign of it. It is observable, that though the mercury, in the summer months, does not so much vary in its altitudes, as at other times of the year; yet in that season we have the most rain; it should seem therefore, that the different warmths, and consequently rarefaction of vapours, in the upper and lower currents of the air, and those currents mixing, and sometimes wholly interchanging, are then the more immediate causes of the rains, if not also of thunder and lightning.—Black fleecy clouds, formed on a sudden flurry of the wind, are generally succeeded by a shower: and, the shifting of the wind in a little time almost round the compass, in hot weather, is often succeeded by a thunder-shower.—Several times, when the mercury has been a good while high, and so continues, there has fallen misling rain; especially about the new and full moon, with an easterly breeze, which the borderers on the coast of Lincolnshire and Norfolk call tide-weather, and may be occasioned by the vapours arising from the tides, which then cover a vast wash of sands in their neighbourhood.—Those vapours sometimes reach us here in Northamptonshire, but seldom further west.—The nights are for the most part calmer than the days; and the winds seldom settled in their quarter, or at their strength, till some hours after sun-rise, and generally die away again before sun-set.

On the Case of a poor Woman who had a Fœtus in her Abdomen for nine Years, opened May 6, 1739. *By William Bromfeild,* Surgeon.* N° 460, p. 697.

This woman, about 9 years since, was with child, and, at the expiration of the usual time, was attempted to be delivered. The child was so far advanced in the passage, that the midwife declared, that in less than 2 minutes the child would be in the world; but, on the woman's suddenly turning herself, the child slipped from the midwife, and could not be found by her again.

Previous to her being pregnant, she had been afflicted with the venereal disease, and had had a violent discharge of a fetid matter from the uterus, and was then under the care of Mr. Balgay, surgeon. She had been salivated once or twice in our hospitals, but to no purpose. After the time of attempting to deliver her, to the hour of her death, she had prodigious discharges of a fetid gleet, and frequently indigested matter with blood from the uterus. There appeared a tumour on the right side, which was moveable to the other, though its attachment was chiefly to the right. She was troubled with a suppression of urine, ever since the attempt of delivery, and within the last twelvemonth went to stool in a cloth insensibly, and what fæces descended into the rectum, were immediately discharged. She gradually wasted, from a hale lusty woman, till she was reduced to a mere skeleton.

On opening the body, the omentum was found entirely wasted; the peritonæum was greatly inflamed, and adhered to the subjacent tumour, which Mr. B. expected, not being acquainted with the case, to be a tumour of the same kind he had before seen, which was chalky; but, on cutting into it, there appeared the os frontis, and, on proceeding farther, the arm, leg, and ribs, on the left side, with some viscid matter in the interstices. It was seemingly contained in a thick membranous cyst, which, on dissection, proved to be the containing membranes of the fœtus, contracted to the shape of the fœtus in utero, and gave the tumour an oval form. The situation of the fœtus was in the concave part of the right ilium, and by its cyst was attached to the intestines, colon, and cæcum. It had some vessels that ran on the surface of the cyst, that was sent from the internal iliacs of the contrary side. By its pressure on the righ ureter, it had hindered the descent of the urine, and had greatly enlarged both the ureter and pelvis, of the right kidney, which was greatly distended with urine, so that what descended into the bladder, must steal in by drops.

The uterus and Fallopian tubes appeared of their usual size, only inflamed.

* Mr. Bromfeild was a surgeon of considerable eminence, and author of two volumes of Chirurgical Cases and Observations, and of some other tracts, such as a Treatise on the English Nightshades; a pampblet on the Treatment of Persons under Inoculation, &c. It is said that the Lock-Hospital was projected by him. Mr. B. was born in 1712, and died in 1792.

The fimbriæ were loose and fluctuating. On examining farther into the pelvis, there was near 6 oz. of fetid matter, lying between the rectum and uterus, which near its neck was perforated, and the parts were very rotten. From its neck almost to the extremity of the vaginæ, the muscles of the anus were nearly destroyed. There were some few indurated little tumours adhering loosely to the cyst of the fœtus. There were several little parts appeared like carious bones found in the matter contained in the pelvis.

A Letter from Mr. John Powell, to Sir Hans Sloane, Bart. Pr. R. S. &c. concerning a Gentlewoman who voided, with her Urine, hairy crustaceous Substances. To which is annexed Sir Hans Sloane's Answer, containing several Observations of extraordinary Substances voided by the Urinary Passages. N° 460, p. 699.

A worthy divine's daughter of the neighbourhood of Pembroke, had voided for nearly 2 years preceding this account, with her urine, various hairy crustaceous substances. She was near 40 years of age, and had been married about 17 years, and had a child about 12 years before, that lived about 9 weeks.

About two years before, she was seized with a stoppage in her urine, a small pain in her bladder, and a great pain in the bottom of her feet, with the making of whitish water like whey; and she had then a great weakness in her limbs, and a pain in her bowels; for which a gentleman ordered her to go into the cold bath, by which she found great benefit for the pains in her limbs; but the pain in making water rather increased, and then her urine began to grow fetid; and about Christmas 2 years preceding, she voided the largest of the things in the box;* without any very great pain then, as she had taken a quieting draught that night to compose her; but, almost ever after, they put her to most exquisite pain before she can get them off; and she was commonly obliged to take the small part of the hairy part between her fingers, before she could get them off; and often a good deal of blood came off with her plucking them, which made her very sore inwardly.

On using gentle evacuations, with emollient decoctions, &c. she got much better; insomuch, that she undertook a journey into Herefordshire, and staid there near 2 months; but her pains became worse, and more troublesome, after her return home; and she then voided great quantities of those large substances, as well as small, contained in the box, and her urine got extraordinarily ropy and fetid, notwithstanding all the endeavours of another gentleman and Mr. P.; and very often the substance she voided would be so stiff and ropy,

* Mr. P. sent a quantity of the hairy crustaceous substances, voided by this woman, in a box to Sir Hans Sloane.

that they could scarcely separate it from the pot; at other times so pliant, that you might take it up a great height with a sprig of a broom, or a feather, and so fall down again like a lump into the pot.

She had for a considerable time voided one or more of these hairy crustaceous substances every day or night; looking, when first voided, like hair and coralline; and her pains were so exquisite, that they were obliged, every 3d night at furthest, to give her an anodyne to quiet her; and that often cannot be done, her pain being so very great.

The continuance of this severe pain brought her to great weakness, and almost a total loss of flesh; and unfortunately milk by no means agreed with her. She often tried to conquer it, but never could, it constantly making her very sick in her stomach, and she vomited it up in large lumps.

They used injections of 2 or 3 sorts, but she could not well bear them; she had her menses very regularly, till within the 2 or 3 last times: and for 10 or 12 days before this account was sent, she had complained of a swelling in her belly, but none in her thighs nor legs.

She often found a crepitus, or a breaking of wind, as it were, in her bladder; which would make one believe, that there was an aperture from the intestinum rectum to the bladder. Her bladder has been searched, but the surgeon could discover no stone.

She had for 4 or 5 days preceding this account, complained, at times, of asthmatic fits, which were attributed to the heat of the weather. The voiding of these hairy crustaceous substances never occurred to him before; though more than once he had had persons who voided large bladders, like the hydatids in fish, in large quantities, and had cured them.

She drank the hot-well waters both at Bristol and here, but with little success; and took cantharides inwardly, as prescribed by Dr. Groenvelt, in ulcers of the bladder, and all other things that Mr. P. could think of.

Sir Hans Sloane's Answer to Mr. Powell, p. 703.

In this reply, Sir H. S. states, that he is persuaded that the hairy excretions were generated in her kidneys. He had seen, in his practice, some instances of the like, and had by him what was brought off by urine from some of them. The first he remembered, was from a gentleman near the Exchange, who would frequently, 40 years since, void with his urine long hairs, which were received on white paper; and, by their transparency and angles, yielded, on viewing by a microscope, the finest colours imaginable, such as we find by a prism. This gentleman did not suffer much, though he complained of a sharpness of urine. The person who was affected the most, was a brewer, who had such hairs matted or woven together, voided by urine with great pain; but then there was no calculous matter, or very little, added to them. It is very likely, that that matter is added to those of your patient in the bladder, by being retained there. He had a pin, that a young woman had swallowed, and was afterwards taken out of her groin from an apostem after a tumour; which pin was covered or incrusted, as these hairy substances, with such calculous matter, and got there from the urine in her bladder, where in all likelihood it had contracted that crust. He had a silver bodkin, the broad end of which is covered with a pretty large stone. A poor gentlewoman thought, by thrusting this bodkin up the meatus urinarius, to remove a stone which pressed on the neck of her bladder, and it slipped past recovery into her bladder; whence, after 3 years, it was taken, and on which, as on a centre, was bred the stone. He had other instances of the same, where an extraneous body, passed into the bladder, has proved as a centre to attract, or have affixed to it, such matter.

As to the cure, dilution seemed to him the best. The brewer was cured by drinking plentifully of soft liquors, which he often poured down; and twice a week he took the purging waters. His opinion was, that the less is generated of this matter, and the less time it remains in either kidneys, ureters, or bladder, the disease will be mitigated, and, he hoped, cured. He believed Bath-waters drank warm, mallow-tea, linseed-tea, oil of sweet almonds, syrup of marshmallows, little and often taken, with baths of emollient herbs, might be of great use; and perhaps moderate exercise might help them off. Opiates, in excessive pain, he judged necessary; and now and then bleeding, to take off the inflammations that must of necessity attend such a distemper. He also thought, that some balsamics, such as Locatelli's balsam, might be useful; and perhaps, with the emollient method, take off that disposition in the kidneys, which produced this uncommon distemper. The pains in her feet, and about her, seemed not to have any relation to this distemper; and he was of opinion, that violent diuretics or exercise would rather hurt than help her.

A Letter from Mr. T. Knight to Sir Hans Sloane, Bart. Pr. R.S. &c. concerning Hair voided by Urine. N° 460, p. 705.

The hairy substance, or fine capillamenta, inclosed in the pill-box, were discharged along with the urine of a gentleman, during a severe fit of ardor urinæ; the gravel that came away was inconsiderable, so that the cause of the dysury was chiefly owing to the hairy substance with the gritty matter that adheres to it, inflaming, by their irritations, the ureters and sphincter vesicæ, and parts adjacent. For, notwithstanding phlebotomy, lenient clysters, emulsions, opiates, and such like remedies, were strictly used, all proved ineffectual, till all this extraneous substance was come away.

These fine capillamenta seem to be the tegument of an animal, which had got into the primæ viæ, and passed the venæ lacteæ, and, by circulation, passed also the glandulæ renales. For it is more probable, that they were extraneous, than that they were generated in the urinary passages, in an equivocal manner.

The greatest objection, that offered to him was, that it was judged absolutely necessary, that the venæ lacteæ should be smaller than the finest artery in the body, that nothing might enter, which might stop circulation of the blood. Also, that the mouths of the lacteals, which are open into the cavity of the intestines, from whence they receive their chyle, are so small as not to be seen by the best microscope in dead bodies.

To obviate these objections, may not the mouths of the lacteals be perceptible in living bodies, when dilated, distended, and turgid with chyle? And may not these capillamenta, when relaxed with any humidity, become very flexible, pliable, and susceptible of being contorted, and of assuming any figure;* and, when thoroughly relaxed, disseminated and floating in a fluid, enter the lacteals; and consequently may pass through the convolution of small arteries, whereof the glands and secretory vessels are formed; for a gland is said to be nothing else but a convolution of small arteries.

N. B. This gentleman has kept a strict regimen of diet for many years, being subject to frequent fits of the gout, an incontinency of urine, &c. In the morning early, a draught of cow's milk, statim ab ubere; which often does not pass a colatorium, whereby some of the downy hair about the udder might get along with the milk into the primæ viæ.

Concerning a large Quantity of Matter or Water contained in Cystises or Bags adhering to the Peritonæum, and not communicating with the Cavity of the Abdomen. By Walter Graham, M.D. Mansfield. N° 460, p. 708.

In the middle of Feb. 1735, Jane Dawson, of Mansfield, in Nottinghamshire, an unmarried woman, aged 30, received a violent strain by lifting a tub of water, and immediately complained of great pain in her left side. In March following, she found a lump, or little round swelling, in that side of her belly;

* The capillamenta, whilst in the urinal, and till the urine was decanted, appeared only like a gross turbid liquor, the filaments being so diffused.—Orig.

Dr. Mortimer remarks, in a note, that he doubted of these substances being *real hairs;* he imagined they were rather slender grumous concretions, formed only in the kidneys by being squeezed out of the excretory ducts into the pelvis.—Orig.

and soon after the whole abdomen swelled, but more in the left than in the right side. She complained frequently of severe pains in her bowels, which in time became so violent, that she had neither ease nor sleep, but by taking large quantities of opium. During her illness, she made very little urine, and was so costive, that she had seldom any stools but by the help of purges or clysters: the former gave her always pain, and the greatest relief she found was from emollient clysters that emptied the intestines. Her thighs and legs, were not swelled, but these and other parts of the body were much emaciated, In this unhappy condition the poor woman lived about 2 years 9 months, and died on the 17th of November. Dr. G. adds, that before this accident of the strain, she had always enjoyed a tolerable good share of health; and seldom made any complaint, but of missing her menstrua.

On viewing the naked body, the abdomen was vastly distended, and most at the navel. The swelling was unequal, the left side being more swelled than the right; and there appeared a very distinct protuberance all along the left epigastrium: this protuberance was much softer than the other parts of the belly, which were so hard, that on pressure, they did not pit.

On opening the body, the following observations were made:

The membrana adiposa was very thin, and the abdominal muscles were much extenuated by the great distention, as is usual in like cases.

The peritonæum, which was the chief seat of the distemper, and the principal part to be taken notice of, was grown to so monstrous a thickness, that its section at the navel was 5 inches and $\frac{8}{10}$ths; and it was of the same thickness below, but somewhat thinner above it. All over the peritonæum, and throughout the whole, there appeared a prodigious number of glands; and the space between one gland and another was filled with a white spongy flesh. Some of these glands were round, others oblong; many of them were as large as a goose's egg, others about the size of a pigeon's egg, and some less; the largest being on the left side. Their internal substance was destroyed, and only the external membranes left, whose cavities were full of liquors of different colours and consistence: some contained a thin whitish humour, others a pellucid viscous gelly, like the white of an egg, and some a white thick matter, like pus. As the contents of these glands thus differed, so did their membranes; some were very thin, others thicker, and many of them were become cartilaginous. In general, those having thin membranes, contained a thin whitish liquor; and those that were cartilaginous, a thick white matter like pus. Their internal surface was quite smooth, and none of their cavities had any communication with each other; nor could the matter be pressed out, without opening them with a knife.

The protuberance of the left epigastrium, was occasioned by a quantity of liquor lodged in a cavity formed by the peritonæum, which in this place was about $\frac{2}{10}$ths of an inch thick. This cavity extended over the kidney and spleen, and there was found in it above 2 quarts of thin liquor of a darkish colour. The whole quantity of matter taken out of that cavity, and those of the glands which were opened, was about 4 gallons.

In the cavity of the abdomen there was found no matter, or water.

The omentum was very white, and much decayed.

The coats of the stomach and intestines were very thin and tender, and inflamed in several places. The intestines lay in the right side, and were filled with hard excrements, forced into that situation by the large protuberance on the left.

The liver was very large, of a colour more red than common, and full of blood, which on the smallest incision flowed freely out of it; and the greatest part of the blood in the whole body seemed to be accumulated in this viscus, and was of a darker red colour than usual.

The gall-bladder was not larger than natural, nor did it contain any stones, or concreted matter; and, on gentle pressure, the bile moved easily through the ductus cysticus.

The pancreas was smaller than common, and adhered closely to the duodenum.

The kidneys were a little inflamed, and of a flatter figure than usual; occasioned probably by the pressure of the peritonæum.

The cavity of the thorax was greatly lessened by the diaphragma being pressed upwards, by which the lungs were likewise much compressed, and they adhered in several places to the pleura and mediastinum. The heart was of a paler colour than common: from the middle to its apex, it was pressed flat, and little or no water was found in the pericardium.

On Hydatids voided per Vaginam. By Mr. William Watson, F. R. S. N° 460, p. 711.

A gentlewoman, aged about 48, the mother of many children, after a respite of 6 years, had, in Nov. 1739, the symptoms of conception, which left her in February; from which time to the end of March, she every night discharged per vaginam uteri a considerable quantity of blood; and, not perceiving an increase in her belly, nor (which in cases of conception is the pathognomonic sign of something preternatural) her breasts, she concluded her menses were leaving her at their usual period. But, on the first of April, being taken with great pains in her back, and having other symptoms antecedent to delivery, there came away at short intervals, a very large number of hydatids, of all the intermediate sizes, from a nutmeg to a pin's head, some filled with clear, others with bloody lymph; all of them propagated in the manner of a cluster of grapes from a spongeous substance, answering the purposes of a placenta. After the discharge of these, in a few days she recovered her accustomed health.

On boiling some of these hydatids, they appeared like the ovary of a boiled hen, with this difference; in the hen, the contents of the ova concrete; in this case, not; but the transparency was changed to the colour of bile diluted with water.

Two Medico-Chirurgical Observations. By M. Le Cat. N° 460, p. 712.

1. *An Observation on Hydatids, with Conjectures on their Formation.*

Sept. 21, 1739, a woman died in our Hotel-Dieu, who had an abscess in the right hypochondrium, through which she discharged hydatids; with a considerable tumour at the left hypochondrium.

Her body was opened. The abscess of the right hypochondrium was between the proper and common membrane of the liver. The tumour on the left side was almost as thick as one's head, and twice as long. It was between the common and proper membrane of the spleen. It ran between the floating parts of the abdomen; had displaced them; and went so far as to push against the integuments of the belly, in its passage adhering to the stomach.

M. Le C. laid this tumour open, and found it filled with hydatids of all sizes, with clear water, and mucilaginous membranes, which were the remains of large hydatids, that were burst by the motions of the patient. He examined with care both the hydatids, and their bag. The hydatids were composed of 2 mucilaginous, transparent, and yet very elastic membranes. The inner membrane had on its concave surface a sort of villosity wrinkled and mamillated, that pretty much resembled the surface of a rough skin, or what is called a goose's skin. The softest and most gelatinous of these membranes, were very like the vitreous humour of the eye. The water contained in all these hydatids, was entirely like the aqueous humour of the eyes.

There were clusters of these hydatids quite resembling the ovary of a hen, or a bunch of grapes, which were made up of globules of all sizes. The bag that contained them was pretty smooth on the side opposite to the spleen; that is, that part of the bag formed by the common membrane of the spleen, or by the peritonæum, was pretty smooth; but on the side next the spleen, the bottom of the bag was very thick, and composed of several lamellæ half destroyed, which fell off in bits or scales, and in slime, at the least touch.

It appeared plainly on the inspection of these remains of the bottom of the bag, that that was the source of the hydatids; and, on considering what sort of parts are found on the surface of the viscera, under their integuments, it seems evident, that these lymphatic globules were nothing else but the glandulous and lymphatic grains of the surface of the spleen, dilated into excrescences by the disease, and puffed up by the lymph, which the distemper caused to accumulate there.

He had proved, in his Physiology, then in the press, that these glandulous grains are nothing but the ends of the nerves, or nervous papillæ, which receive the ends of the lymphatic vessels into their spongy texture: and he had, among others, instanced in the papillæ of the tongue, called glandulous papillæ, which are at the same time the organ of taste,* and the receptacle of the salival lymph.

A part of the nerves, which are distributed into the substance of the liver and spleen, terminate in the surface of those viscera, under the form of glandulous or pulpous grains. This same surface is the seat of a great number of those lymphatic vessels. And it is not to be doubted but those glandulous grains are as necessary for those lymphatics, as the parotid gland is necessary for the lymph of the salival duct, and the glandulous papillæ of the tongue for the liquor that distils from them. In quality of glands, they are the receptacle of those liquors: as nervous papillæ, they furnish the spirits necessary for the functions of those liquors. All these truths are proved in the work above cited. Let us now come to the consequences of this structure.

As long as the glandulous papillæ are sound, their excretory pores pour forth the lymph according as their cavities receive it from the lymphatics; but if these pores happen to be obstructed by a disease; if the surface of these grains be altered by any erosion; or if the natural tone of these solids be perverted; the lymph brought into these grains will be retained there: it will stretch these globules; their substance having lost its elasticity, will easily give way; the nutritious juice, which they will not be able to drive further, will be there assimilated, and will contribute to the dilatation. In fine, a vesicle will be formed filled with lymph, or an hydatid, such as those we have examined.

This congestion of lymph, or hydatids, will not fail to soften, relax, and raise up the membrane that covers them; and thus a bag will be formed.

* See his Traité des Sens, Rouen, 1742, 8vo.—Orig.

When an hydatid swells to a considerable size, the volume of the fluid will become disproportioned to the force of the teguments; these will be burst by the shaking of the contained fluid, on the least motion of the body. This fluid will extravasate into the common bag, on opening which, the waters and membranes, which result from that rupture, will be found.

Most part of the glandulous grains are distributed into clusters, as is well known to anatomists; therefore hydatids are also found disposed in clusters, like ovaries.

Yet the greatest number of this heap will be composed of separate hydatids; because, when one of these globules has acquired a certain bulk, it will generally break the too feeble pedicle, which held it attached to the cluster; and thus it will fall into the common cavity.

This kind of eruption, or general disengagement from the surface of the bowel, must destroy its natural texture, and reduce it exactly to the state in which we found the bottom of the bag of hydatids.

2. *An Observation on the singular Consequences of an incomplete Hernia, and on the Functions of the Intestines exposed to sight.*

Catharine Guilmatre, of St. Adrian, near Rouen, aged 50, had a rupture in the right groin, for 7 years before. At Easter 1739, there happened a strangulation in her rupture; and, having no assistance, the tumour suppurated, and opened of itself. The excrements followed the pus, and the patient escaped at the expence of vomitings, and a little fever.

The intestine cicatrized with the integuments, but there remained externally an opening, through which the excrements passed. The anus ceased to perform its usual functions; and, that excepted, the patient was cured.

Towards Witsuntide, there issued out at the wound, besides the excrements, a gut 3 or 4 inches in length; but this gut was turned inside out, that is, the villous coat was outward, and it conveyed no excrements; these were always discharged through the wound, on one side, and below the gut that was come out.

In the month of August of the same year, 1739, there came forth at the wound another gut, turned as the first, making with it a continuous canal, but at its end supplying fæces, which had before been discharged through the fistula; so that, instead of the fistula, there was found, as it were, the trunk of 2 intestines, which made a kind of fork.

The woman, tired of this inconveniency, resolved at length to seek relief. Fortune presented her with no other than the Hôtel-Dieu of Rouen. She was brought thither in December. Mr. Le C. was then in the country: she was told, that her distemper was incurable; and yet she was kept there till his return, to show her to him by way of curiosity.

In effect, he found her case deserved his utmost attention; and he had her carried to his house, in order to examine it more at ease, and to have drawings taken of her distemper.

What was curious in this distemper, was not an anus formed contrary to nature in the groin, that accident being pretty common; but it was the 2 guts turned inside out, their villous coat, and their functions, demonstrated to the very eye; as also the ænigma occasioned by these 2 guts, which were both of one piece, and which notwithstanding had 2 openings, the lower of which voided the excrements, but the upper discharged nothing. He knew of no other person, but Mr. Cheselden, who had observed an inverted gut in a living body: but his observation added to his, 1st, Experiments on the action of purgatives: 2dly, The singular figure of this hernia, the discovery of which has an influence on the radical cure of this disease, and on those of the same kind which may possibly happen, as will be seen by the sequel.

He thought he might give the epithet of singular to this sort of hernia; because, on inspection, we instantly conceived, that the gut which voided the excrements, was continuous to the stomach, and the other to the anus; but how was it possible, that these 2 inverted guts should be of one piece? Let us imagine a gut cut through by a strangulation: there remain 2 orifices, one that runs to the stomach, the other to the anus: if the canal of each of these orifices turns inside out, and prolapses, as it happens, to the anus; we then have 2 guts prolapsed and turned, but they are distinct one from the other, far from being of one piece. It must be allowed, that the ænigma is puzzling: and indeed many surgeons saw this singularity, but not one of them accounted for it. The reader, if he be an anatomist, has but to attempt the solution, in order to be sensible of the difficulty.

The villous coat, and the functions of these intestines, being exposed to the eye, afforded a circumstance still more curious and useful. These 2 portions of guts seemed to be 2 large living worms. They moved here and there, twisting, shortening and lengthening themselves like reptiles. The lower gut was much more active and sounder. Once that he handled it, it twisted round his fingers like an eel. The upper gut, that answered the anus, had less motion, and was beset with pustules.

The expulsion of the fæces engaged particular regard: we remarked in its mechanism 2 sorts of motion. The first is the vermicular motion, allowed by most authors. In this, the gut first swells, and becomes smooth; then grows narrower, running into wrinkles, and forming waves the whole length of the gut, where these two motions happen alternately. The straitening is performed behind, and on the excrements, to drive them down; the dilatation happens before these fæces, in order to open them a passage: for example, when the fæces were at the orifice, through which we saw them issue, this orifice was spread open.

The 2d sort of motion observed in the guts, generally preceded the one above described. In this motion the surface of the gut, being swelled and smooth, was rendered uneven by many small impressions, or hollows, distributed here and there, and which seemed to be formed by little local convulsions, circumscribed by the intestinal fibres. These convulsive impressions resembled, in little, those that are made in the abdomen, on contracting some one of its muscles. They made the surface of the intestine a little pale, and so formed a sort of undulation on its surface. It was chiefly in this sort of motion, that there was squeezed out of the villous coat of the intestines, a mucilage and serosity, which flowed from it in abundance. Both these seem to serve for diluting the fæces, and preparing them an easier passage. The cold air did not fail to excite these motions, and the woman felt some touches of the colic.

After having made these observations on the natural functions of the intestines, it occurred to him to observe the effect of cathartics in them. We do not often see the inside of the guts of a living person in good health, and freely performing its functions: he was therefore willing to make use of so uncommon an occasion.

First, he put a little pulp of cassia on several places of these 2 portions of gut. This medicine made very little impression on those parts; they stirred very little, especially the upper gut. Next, he laid on manna. This, when somewhat dissolved; formed a sort of froth, and then the gut was agitated by vermicular motions, and by small convulsive contractions, much more distinct than in the conditions he had examined it before. He took off the manna, and strewed powder of jalap on the gut. At first it had no effect; but, when it was moistened, the gut was violently agitated, discharged much serosity, and the patient complained of gripings. He removed the powder, and under it he found a great quantity of mucilage, already gathered there.

He thought it needless to harass the woman by further trials, which would prove much the same with the foregoing; and therefore turned his whole attention on the means of curing her of this accident, and so rewarding her for the services she had rendered us.

At first sight of this disease, he was as far as the other surgeons from comprehending the ænigma of the figure of the 2 ends of the gut continuous, or of one piece. He plainly saw that they were portions of the ileum; but he was obliged to meditate on it a 2d time, in order to guess at the rest; and yet nothing so easy when a person has hit it off.

The hernia which this woman had at first, was one of those named an incomplete hernia properly so called; that is, a hernia where there was but a portion of the side of the gut pinched within the ring. This strangulated portion mortified; the sound lips cicatrized with the integuments; the rest of the canal remained within the belly; and the excrements, which this remainder of the canal received, issued at its outlet towards the groin.

The patient, being recovered, quitted her bed, and by little and little occasioned the turning inside out, and fall of the portions of the intestinal canal, situated above and below the open part. By this inversion, the remaining coats of the opened gut came out likewise. This part is situated between the 2 portions, one of which answers to the stomach, and the other to the anus; and with these 2 portions it makes but one and the same part, or a continued plane: it was therefore found, out of the belly, between these 2 portions, and formed, as it were, the trunk of these 2 branches.

The portion, or branch, corresponding with the anus, must have had less motion, and be less sound; because it is deprived of the share of life that would come to it from the continuity of the fibres that were pinched and carried off by the strangulation, and that it is continually exposed to the air. The other portion is full of life, because its continuity with the stomach makes it enjoy all the life that this communication can furnish it with; and that besides it remains within the abdomen, while the patient is in a recumbent posture.

In order to give the pupils of our Hôtel-Dieu a clear notion of the formation of this singular rupture, he made one just like it on a dead body. For that purpose he made an incision in the abdomen, at the place of the rings. He passed into it a gut, in which he made an opening. He sewed the lips of this opening to those of the wound of the belly; and having turned inside out the portions of gut placed above and below this opening, they afforded a bifurcation of guts continuous, and entirely like that of the observation.

A disease well known is sometimes half cured. This same portion of gut that supplied the fæces, and that was so lively, was drawn back into the belly, when the patient lay down, as already said; and the other only constantly continued out. This circumstance made him conceive hopes of curing this accident.

He reasoned in this manner: it is but first making this last gut enter in, and bringing the disease to its first state: then, seeing there was a pretty large portion of a canal still remaining between these 2 guts, as appeared by the size of the trunk of the branches formed by them; what remained to be done, after the whole was reduced, was to close the exterior orifice of this demolished canal; that is, to close the opening made by the strangulation and mortification; and he conceived that this last operation was very feasible. The next thing to be done was to refresh the lips of the fistula formed by the integuments of the abdomen, which are thick enough, and on which shall be afterwards made a gastroraphia proportionate to these parts.

The great difficulty was to reduce this end of gut, which was become hard, and full of tubercles. He had already made a fruitless attempt, both with cataplasms to repair the damages, and with manual operations proper for making it re-enter. He was actually watching a favourable moment for this operation. If he succeeded he intended to stay for making a 2d operation, till this gut has remained long enough in the belly to repair itself, and resume its functions. In order to that, he would content himself for the first 8 days, with keeping it in the belly, applying resolving fomentations, and giving proper clysters. Then he would put into the opening of the intestinal canal, that would answer to the fistula, a silver canula of the same bore with the gut; in order to push this portion of a canal into the belly, to support it therein, and re-establish its communication with the portion newly reduced. This silver canula would be fixed by a plate of the same metal, guarded with plaster and linen, and placed on the fistula, where it should be secured in its situation by a bandage. He would then redouble the use of the clysters, and when he should be certain of the re-establishment of the communication of the 2 guts, and the functions of the portion continuous to the anus; then he would withdraw the silver canula, and would perform the operation, as abovesaid.

Concerning the Circulation of the Blood, as seen in the Tail of a Water-Eft, through a Solar Microscope. By the Rev. Mr. Henry Miles. N° 460, p. 725.

This paper is now of no consequence, since it is well known that the common water-newt is one of the most favourable subjects for exhibiting a general view of the circulation, which appears to peculiar advantage in the tail of the male animal.

Concerning the true Delineation of the Asterisms in the ancient Sphere. By the Rev. Ebenezer Latham, V. D. M. and M. D. N° 460, p. 730.

This is a method of fitting up the celestial artificial globe, by furnishing it with a moveable and temporary axis, which may be shifted occasionally, and set in a position answerable to any former age and position of the heavens, allowing after the rate of 50″ a year for the precession of the equinoxes. In such situations of the sphere, we may contemplate many of the astral descriptions given by the ancient poets; thus explaining and illustrating many obscure passages in their writings. Mr. L. gives instances of many such quotations, and concludes, the inspection of the globe, when it is fixed in a proper position, will convey the best idea of all these appearances; for we derive this advantage from the new construction of it, that it will enable us to place the several phænomena before every eye; by which means those who have the least acquaintance with these studies, must be greatly surprised and pleased to observe the ancient accounts minutely verified. It is a sort of living over again the former ages, allowing $1^{\circ} 23' 30''$ for every 100 years, according to Ricciolus and Flamsteed, which is a sort of mean between the other computations.

I shall not now suggest some other purposes, that might be served by this method. It is sufficient to recommend the invention, that it throws so much light on the common classics, to which this examination is confined.

An Aurora Australis, seen at Rome, Jan. 27, 1740. By the Abbot Didacus de Revillas, Prof. Math. et F.R.S. N° 460, p. 744.

The sky being all over cloudy, at 6^h afternoon a reddish light appeared between 45° and 55° of S. E. amplitude. It was about 8° high, and 10° broad. About half an hour after, the light became more vivid, and then sensibly diminished again; which it did several times till 9 o'clock.

A new Plotting-Table, for taking Plans and Maps in Surveying: invented in the Year 1721. *By Henry Beighton, F. R. S.* N° 461, p. 747.

This new plotting-table, is nothing more than the old plain-table, with some new contrivances or improvements, which are here described at great and unnecessary length.

Mr. B. says one of the chief objections against the plain-table, was the difficulty in shifting of papers; for you were almost necessitated, when you were at work on a sheet on the table, to put in all the work that is to be contained in it, because it is very impracticable to put it on the table in the same precise position again, and this although it were with the utmost inconveniency, in pursuing some grand station, on circumscribing the whole. This is entirely obviated; and I have remedied all the other objections against it, in a very simple and easy manner, which I contrived in the year 1721, for making a correct map of the county of Warwick; by which, with good success and expedition, I completed and published the same in the year 1728; and call the instrument the plotting-table.

Concerning a large Piece of the Thigh-bone, which was taken out, and its Place supplied by a Callus. By Mr. Wm. Wright, Surgeon, at Bradford in Yorkshire. N° 461, p. 761.

This was part of the os femoris, taken out of the thigh of a young man, 20 years of age, about the end of March 1738. His name was Hird Ramsden, and he lived at Braithwait, near Kighley. His lameness was occasioned by a fever, which fell into his thigh, where it imposthumated, and was afterwards opened; but, not healing again, left 3 or 4 carious or fistulous ulcers, which discharged a great quantity of sanies, and fetid matter, by which he was reduced almost to a skeleton. In this condition he had been 6 or 7 years before Mr. W. was concerned for him, and was considered as incurable. He examined his ulcers with his probe, and found in one of them, which was on the inside of the thigh, a rotten bone: he dilated the orifice with gentian and sponge tents, and afterwards laid it open about 3 or 4 inches: he then dressed it with tincture of myrrh, and dossils of dry lint; and at every dressing, over the carious bone the powder of rad. aristol. myrrh, and euphorb. in order to promote exfoliation: with these applications the bone began to loosen, which looked much larger than he expected. He was afraid of making another incision because of the crural artery, which lay very near the place where the bone was taken out: he therefore chose rather to do it gradually by dilating the orifice, than run the risk of another incision. The same dressing was continued, and the spongy flesh kept down with the powder of mercur. præcipit. rub. et alum. ust. $\bar{aa}$. At every dressing he raised the bone with a hooked instrument, and in about 4 months time he got it quite out. The cavity was afterwards kept open for some time, with dossils of dry lint, to make way for some loose pieces that were left behind. The ulcer, after it was well digested, healed up in a little time. During this time his knee was very much contracted, which was afterwards extended by the use of emollient fomentations. At the date of this account he was perfectly sound, and in a good state of health, walked straight, and his thigh was not shorter than the other.

Of a monstrous Fœtus, resembling a hooded Monkey. Communicated by Mr. William Gregory of Rochester. N° 461, p. 764.

A woman, aged 44, of an athletic body, conceived with child a little before Christmas 1730; on which ensued all the usual symptoms of pregnancy. Soon after conception, some fellows who travelled the country, with a bear and a monkey, placed themselves before the woman's door, to amuse the populace.

The monkey had a hood on, which reached to his shoulders, of which the woman took great notice; and all the time the monkey was playing his tricks, in turning over a stick, &c. the woman could not keep her eyes off him. A short time after, the woman met a man of a thin, pale, dismal aspect, on whom she looked very earnestly, and thought his face exactly like the monkey's. When the woman was quick with child, and the fœtus began to move, the woman felt it turn over and over, many times successively, just as the monkey turned over the stick, and always in the same manner. In the 7th month of her pregnancy, she was taken ill, with a vomiting, gripes, and looseness, which soon ceased without the help of medicine; on which her belly decreased, and the fœtus did not move so often, nor so strong, as before. The woman began to be very uneasy, thought her case dangerous, and that she was not with child; on which she consulted Mr. G. who was of opinion that she was with child. After this time, the child within her stirred always less and less, till at the end of 10 months from the time of her supposed conception, and when she had not felt the child move for 6 weeks before, she was delivered of the fœtus, having the appearance abovementioned, and the navel-string twisted as if by the fœtus turning over.

The Case of Mary Howell, who had a Needle run into her Arm, which came out at her Breast. N° 461, p. 767.

Mary Howell, late of Oswestry, Shropshire, spinster, had, on the 3d day of March 1732, a small needle, which she had stuck on the sleeve of her gown, by her accidentally running against a door, driven, with some thread twisted about it, into her left arm, about 6 inches below her shoulder; and a young woman endeavouring to draw it out, broke off its eye, and left the needle in her arm; on which she directly applied to Mr. Tomkins, a surgeon, in the same town, who endeavoured to extract it, but could not, without laying her arm open, which she would not suffer. About a month after which, she felt a gnawing pain above the place where the needle ran in, and up to her left shoulder, which continued 3 or 4 days, and so returned by fits, till at length, after about 7 years, she felt a gnawing pain at her stomach, which made her very sick, and retching to vomit, and continued to afflict her, especially in the mornings, for some days, after which she fancied a pin was got into her right breast, in the under part: and 2 days after applied to Mr. Robert Nanney, surgeon, in Fetter-lane, who the same day lanced the breast, and extracted the needle.

Mr. Alexander Orme's Pectoral Syrup, sent in a Letter to Sir Hans Sloane, Bart. &c. from Calcutta, dated Jan. 25, 1733. N° 461, p. 769.

℞ *Nantsjera Patsja Horti Malabarici cum toto q. v. incis. et contus. coq. ex aquæ font. q. s. colaturæ fortiter express. adde sacchari par pondus, et coque ad syrupi consistentiam absque clarificatione.*

Some uses of the pectoral syrup.—A drop or two, with a little honey, given to new-born infants, greatly helps the necessary cleansing of the bowels. Three or four drops are a safe puke for them, and cleanse the stomach and bowels from that phlegm that causes their gripes.—It is of great service in most asthmas, and has relieved, when the best remedies have failed. When the fit is violent, give a large spoonful of it, which will soon procure a vomit or two. When the fit is moderate, 2 tea-spoonfuls 3 times a-day will be sufficient.—In fevers that are attended with a laborious breathing, it has been found serviceable.—It is excellent in the small-pox, as well to vomit in the beginning, as to help on the necessary salivation in the confluent sort.—It helps coughs, and promotes expectoration.

From these few hints, a physician will be able to adjust its use in other distempers. Mr. O. would not have recommended it, had not repeated experience convinced him of its usefulness: and that it might be of benefit to posterity, he meant to physicians that were really such, he gave the receipt of it to be sent to the president and censors of the College of Physicians, London.

Concerning the Seed of Fern. By the Rev. Mr. Henry Miles, of Tooting. N° 461, p. 770.

In Boerhaave and Gaubius's edition of Swammerdam's Biblia Naturæ, sive Historia Insectorum, in Dutch and Latin, 2 vols. fol. at Leyden 1737 and 1738, is a dissertation on the seed of the male fern, with a very curious cut, representing the seed-vessels, their mechanism, and the seed, as viewed by a good microscope.

The author claims to himself the first discovery of fern-seed, in his dissertation, at the beginning: "You rightly judge," says he to his friend, "me to have been the first," &c. Boerhaave says, that he showed them to the botanic professor at Leyden Anno 1673, and that he had drawn the figures of them. But Dr. William Cole sent an account of the seeds of divers of the plants called dorsiferous, to * Dr. Robert Hook, in a letter dated September 30, 1669,

* Who was the first Englishman that discovered the seed of the fern by the help of a microscope.—Orig.

and gives a pretty just description of the seed-vessels, and the manner in which they grow, and intended a delineation of the figures. Swammerdam perhaps spoke what he thought to be true; and possibly he might have made the discovery many years before the time when he showed the seeds to the professor. However, I am humbly confident of this, after numberless trials made with all kinds of microscopes, and in all positions, and with different lights, that Swammerdam's account is just and accurate, in every point. I have viewed the several kinds of fern, English maidenhair, other sorts of maidenhair, wall-rue, harts-tongue, and find the seed-vessels of the same form in all, some little difference being between some of them in the size only; and in the manner of their being inserted on the back of the leaf, with the numbers in various plants, there is a more considerable difference. Where there are fewer seeds, there are more of a sort of fungus, or tubercle, very like what is called Jews-ears, which seems to me designed to shelter the seed, which grow, as under covert, round about them.

In the female fern, and English maidenhair, the whole surface of the leaf on the inside seems covered: so that the seeds guard each other in some measure; though in these, after the seed-vessels are shook off, small membranes are found here-and-there on the surface, a little curled, looking as if they had been raised with the edge of a sharp pen-knife, from the skin of the leaf, not altogether unlike the pieces of skin we are wont to raise in trying a pen-knife on one's hand.

The plant here attempted, as exhibited in fig. 6, pl. 10, with its seed-vessels, &c. is the filix mas Dodonæi; on the inside of the leaves of which are usually seen several spots placed in a regular manner, of a light-brown or russet-colour. In this plant the principal part of these spots is the fungus beforementioned, around which the seed-vessels are inserted.

The seed-vessels consist of a stalk, by which they are inserted into the leaf, as cc, fig. 7, of a springy ribbed chord ee, having a great number of annular ribs, exactly resembling the annular cartilages in the aspera arteria; and I know nothing in nature so aptly resembling this chord, as the aspera arteria of a small bird, as a robin or nightingale, &c. This chord incircles the globular membraneous pod, where the seed lies, adhering to it, and dividing it into two hemispheres. The pod ff is, in appearance, composed of a fine whitish membrane, somewhat like that which lines the inside of a pea-shell. The seeds, fig. 8, k, are irregular in shape, and in their surface a little resembling a sort of net-work.

In viewing this admirable production of divine wisdom in this plant, I use a single lens, and no deep magnifier, that I may have the advantage of the light falling on the objects. I throw a quantity of seed-vessels on a circular plate of ivory; and, if the plant be newly gathered, the proper time being about the beginning of September, I often have the pleasure of seeing the seed-vessels burst; the motion of which at that time may be seen by a good eye unassisted. But, when I happened to light of a pod not thoroughly crisp, I have had the satisfaction of seeing the gradual procedure of the bursting of the vessel, in order to the scattering the seed, in the following manner: first, the chord breaks, and by expanding rends the folliculum or pod in two parts; by going on to expand itself, as it departs from a curve, and approaches to a right line, it gradually rends itself away from the globular pod, till it be wholly discharged from it; when, as there can be no further resistance made to the chord in expanding itself, it naturally gives a sudden jerk, which in this case is very gentle; and thus the seeds are shed on the surface of the plate, in the same manner as if you were to cast some grains of corn out of a bowl on the plane of a table-board: this I have several times seen with unspeakable pleasure; but where the vessel is more crisp, its motion in bursting wholly escapes the sight, flying away with great violence beyond the field which the lens takes in. Sometimes I have observed the pod to be 10, sometimes 20 minutes in bursting; in which time you may have a distinct view of the procedure. I have more than once seen the pod broke in the side by some accident, as at l; and the seed lodged within, while the chord has been whole, and still embraced it.

We might have the opportunity of seeing this curious piece of divine mechanism to greater advantage, if we could find a way to get the seed-vessels from the leaves in a less rude manner than by rubbing them; for they will not easily be discharged from the leaves, for I believe they continue a month after the seeds are dispersed, so as to collect any number of them together, and this method bursts them. When I have been attempting this, they fly about like exceedingly fine vapour or smoke, and are very troublesome to the hands, &c. by getting into the pores like cowidge.

In fig. 10 is a representation of a small piece of the leaf of harts-tongue magnified, taken from Dr. Grew's Anatomy, or History of Plants, pl. 72, referred to book 4, p. 200. I was surprised to see that cut so little resembling the true figure; indeed the Doctor says it was a cloudy day when he viewed the object; and I am sure he had no just notion at all of the spring which embraces the pod, as to its texture: for it is by no means spiral, or like a screw, nor do the seeds grow in that regular manner, as represented in the figure.

Whatever use may otherwise be made of this discovery, a moral one naturally presents itself to us; viz. to admire the infinite wisdom and skill of the wonderful Creator; for what thinking mind can help being struck with astonishment, when he considers the seed vessels of a coarse plant so minute as to fly about in the air like vapour, but a little remove from being invisible to a naked eye, framed with such curious mechanism, containing a great number of seeds, too fine to be seen by the acutest sight without the help of glasses.

In pl. 10, fig. 9 represents a branch of the plant; fig. 7 the seed-vessels; fig. 8 the seeds; αα a branch of the male fern; ββ refer to the leaves, on the backside of which, the excrescencies, like Jews-ears, grow, around which grow the seed-vessels; cc the stalks of the seed-vessels; d a shoot from the stalk, producing sometimes another seed-vessel on the same stalk; ee the springy chord, embracing the pod, which contains the seed; ff the pod; g the pod with a crack or chink in it, to represent its being about to be divided into two hemispheres; hh the chord expanded, approaching to a right line; ii the two hemispheres, when the pod is divided in two; k the seeds; l seeds in the pod, the membrane being broken and turned up.

An extraordinary Case of the Heart of a Child turned upside down. By Jos. Ignat. de Torres, M.D. of Gandia in Valentia. N° 461, p. 776.

The following is a new and surprising prodigy, concerning the heart, the like of which was never hitherto observed, till Dr. T. saw it on the 29th of December 1736, in a new-born female infant, of the town of Almoyna. Innumerable phenomena have been observed in the human heart, some few of which he here mentions.

Balloni saw a heart so large, that its monstrous size alone, without any defect in the lungs, occasioned an asthma. Bartholin found caruncles in the ventricles. Spilemberger observed a small bone in one, which occasioned a phthisis.

Zacutus Lusitanus relates, from the report of another person, that a * worm was found in the left ventricle, which brought on dreadful symptoms. Its head was yellow, its body white, and its tail split. Riolan opened the body of a man, whose heart was cartilaginous. According to Rayger, the aorta with the valves was found ossified; which was the cause of sudden death. Genesius of Valentia, a very able physician, has apprised me in one of his letters, that on opening his young son, he found the heart inverted; that is, the left ventricle on the right, and the right on the left side. Amorosius saw a heart with 2 points, which on the outside showed the 2 ventricles. Sirenarius found a heart with its cone in the right side, and there the pulsation was constantly felt. Martinezius, first physician to the king of Spain, observed in a new-born male infant, the heart pushed out of the breast, with its cone and basis lying hori-

* Rather a polypus. C. M.—Orig.

zontal, and without a pericardium; a new and remarkable phenomenon; as if the heart, not bearing so close a confinement, burst through the breast, and, having broke the sternum, appeared on the outside.

Dr. T. omits Benivenus, Muretus, Scultetus, and Giersdorf, who observed the heart hairy, and found stones, polypuses and abscesses in its ventricles.

He then states that he had observed, in a new-born female infant, the heart without a pericardium, and turned upside down, so that its basis, with all the vessels, had fallen down as low as the navel; and its apex, still on the left side, lay hid between the 2 lungs. It is amazing how the circulation could be carried on, the heart being thus inverted; and yet the child lived several days after birth. He observed the heart from its basis, whence the aorta and pulmonary artery spring, and where the cava and pulmonary vein enter it, to its cone, surround loosely with several windings of these vessels, through which the blood's circulation must necessarily be performed.

Of the Curve called, from its Form, a Cardioide. By M. John Castillion, of Montagny, Prof. Philos. in the Acad. of Lausanne, and F. R. S. N° 461, p. 778. *From the Latin.*

The diameter AB of the semicircle AMB, fig. 1, pl. 11, touching the circumference in the point B, so as always to pass through the point A, will generate this curve.

From this genesis of the curve, it appears, 1. That DAα, perpendicular to AB, is equal to double the diameter.

2. That the periphery of this curve ADN$a\alpha$NA terminates in A.

Now through a and A draw aE and AQ perpendicular to aA, and EN any where perpendicular to aE. Then, from the genesis, it follows that AN = BA $\pm$ AM; and, by the similar triangles QAN and MBA, AQ = BM $\pm$ MP, and NQ = MA $\pm$ AP.

This is the chief property of the curve. There is also another pretty property, that the line NN is always equal to double the diameter, and is always bisected by the circle in M.

Now put BA $= a$, aE $= x$, EN $= y$; then will QN $= \mp y \pm 2a$, and AN $= \sqrt{x^2 + y^2 - 4ay + 4a^2}$, and MA $= \mp a \pm \sqrt{x^2 + y^2 - 4ay + 4a^2}$; which 4 lines being compared by analogy give this equation of the curve,

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

Hence the subtangent of the curve is easily found by the common fluxionary method. But an easier way of drawing the tangent may be deduced from the generation of the curve. Let MAN come into the nearest place to the first

mAn; then take AR = AM, and Ar = AN, and joining MR, Nr, draw AT parallel to them, and M, m, and N, n, being joined, draw MT, nt. Now nA : At :: nr or mR : rN :: mR × MA : rN × AM :: mR × MA : MR × AN :: MA × Am : AN × AT; but in the ultimate ratio mA = MA, and TA is perpendicular to MN, therefore nA : At :: MA² : AN × AT; now if, through the centre of the circle F, there be drawn the line MF, produced to meet TA, also produced in G, that is to the circumference of the circle, then will MA² = TA × AG; therefore nA : At :: AG : AN; therefore describe a semicircle through G and N, which will cut the line AT in t, from which tN being drawn, it will be a tangent to the curve, to which also NG is perpendicular; hence MO being joined, a parallel to it, drawn through N, will be the tangent to the curve.

And here it may be observed, that this method of drawing tangents agrees with most curves. Thus let AB, fig. 2, be the conchoid of Nicomedes; then, supposing the former preparation, BP : Pt :: BR or cr : Rb :: cr × CP : Rb × CP or rC × PR :: CP² : TP × PR; hence the former construction is derived.

Again, let a line CPB, fig. 3, of a given length, have its extremity C moved along the line CDT, perpendicular to DA, and always pass through the point P in the same given line DA, and so generate a curve AB. Now applying the former preparation and reasoning to this, then we shall have BP : Pt :: bR or rC : RB : cr × CP : RB × CP or BP × rC :: CP² : BP × PT, as before.

Also the method de maximis and minimis gives the greatest ordinate = $\frac{3}{4}a$, and its absciss = $\frac{1}{4}a\sqrt{3}$. In like manner the greatest absciss might be investigated, but by a tedious process; therefore find it thus: because EN, fig. 1, is a tangent to the curve, the line MG, drawn from the point M through the centre F, determines the point G, from which GN being drawn perpendicular to EN, therefore also to Aa by the hypothesis. But NQ = AV = MA + AP; therefore VP = MA; but BA : AM :: MA : AP; therefore BA : PV :: VP : PA; but PF = FV = $a - 2z$; therefore $a : a - 2z :: a - 2z : z$. Hence is easily deduced $z = \frac{1}{4}a$; EN = $\frac{3}{4}a$; AQ = $\frac{3}{4}a\sqrt{3}$. Where it is to be noted, that the same point M, which gives, in the line NAMN, the point of the greater ordinate, gives also the point of the greater absciss.

A Machine to represent Eclipses of the Sun. By J. And. Segner, Med. Physic, et Mathem. Prof. Goetting, R. S. S. N° 461, p. 781. *From the Latin.*

A projection of the arches and circles of the earth's illuminated surface, on a plane, may serve well enough to show any solar eclipse; and if the places on the earth's surface, as cities, islands, &c. be inserted in the projection, and a circle be added, to express the position and magnitude of the lunar penumbra, and some smaller circles concentric with it; we have then in one view those places where the sun is hid by the moon, or any part of it concealed from our sight.

But such an image is momentary; and though it shows well enough what happens at any precise moment of time, as for instance when the centre of the lunar penumbra first enters the earth's disk, yet it cannot exhibit the other phenomena, which depend partly on the earth's rotation, and partly on the moon's motion.

While the earth turns round, the circles of latitude indeed, and consequently their projection, remain the same; but the meridians, or circles of longitude, are continually changed, and consequently their projection, and the situation of places on the earth, as far as depends on them.

But the artificial globe of the earth shows the illuminated hemisphere at any point of time, with very little trouble. For the pole being elevated above the horizon, or depressed below it, so that the elevation or depression may be equal to the sun's declination at that given time; or, which comes to the same thing, the sun's place in the ecliptic of the globe being brought to the zenith, the artificial horizon becomes the boundary of light and shade; and nothing remains, to exhibit plainly the illuminated hemisphere, but to turn the globe on its axis, till it obtains the position adapted to the hour of the day.

Thus, what is very difficult in projections, is easily performed by the globe, and also more conformably to nature. Considering this, it occurred that we still wanted, for representing all the phenomena of a solar eclipse, to project the lunar penumbra on the globe, and to make an instrument to represent its situation at any time, and to refer it to those places on the earth which are marked on the globe. Hence Mr. Segner devised such an instrument, which is as follows.

Fig. 4, pl. 11, represents a common terrestrial globe, furnished with its horizon, meridian, and hour circle. To the horizon are connected 2 wooden arms, AB, ab, of a length a little exceeding the semidiameter of the globe; and one end of each arm is made to clasp the horizon, to any part of which it may be fastened by screws, as shown at D.

On the opposite extremities, B, b, are wooden columns, BE, be, erected perpendicular to the horizon, of the height of the semidiameter of the globe, and of the breadth of the brazen meridian; so that a right line being drawn through the tops of the columns, cannot touch the meridian.

On the top of each column is a small brazen ball, each perforated by an iron axis, projecting on both sides, and firmly joined to the ball, the lower parts of the axes being fixed into the columns, so that the balls are held fast in a position parallel to the horizon of the globe.

The upper parts of the axes are round and polished, as well as the upper surfaces of the balls; and they receive round plates of brass, EFG, efg, resting on the balls in such a manner, that, being turned on the axes, they always remain parallel to the plane of the horizon. The plates are about 3 inches diameter, and have each a notch in the circumference, to receive a thread, but the plate efg a little less than the other plate EFG; and this latter has a circle inscribed on it, divided into degrees, with an index H, to point to any part.

There are 3 brazen rays, ik, il, im, connected at i, containing equal angles, kil, lim, mik; the place i being perforated with a very small hole. The rays are elastic, and very thin, and in length nearly equal to a 4th part of the diameter of the globe. They have also small perforations at l and m, through which a thread being drawn, is brought round the plates in the direction mEFGgfel, the ends being fastened together between l and m: therefore the skeleton of the penumbra is also rendered immoveable at the part of the thread elmE. Hence the skeleton is turned either way, in a right line, by turning the plate EFG or efg.

The following circumstances must be changed according to each particular eclipse. The chief of these is the disk of the penumbra With regard to this, having found, by the tables of the eclipse to be represented, the semidiameter of the lunar penumbra on the earth's disk, as also the moon's horizontal parallax, it is, as the moon's horizontal parallax, is to the radius of the disk of the penumbra, so is the semidiameter of the terrestrial globe used, to the quadrant, which expressed the radius of the penumbra by the magnitude of the globe.

To set every thing in order for any moment of a given eclipse, we must proceed in the following manner. Having found by calculation the points of the bounds of light and shade, by which the moon's centre first enters the earth's disk, and departs from it again, they are to be marked on the horizon of the globe, and the arms AB, ab, are to be placed so, that the plate EFG being turned round, the centre i of the disk of the penumbra klm, may pass over them; which when done, will be shown by the pendulum in. Then find the time when the centre of the penumbra is in any remarkable place, as when it first enters the earth's disk, and place the globe, by means of the meridian and equator, in such a manner, that the part above the horizon may show the earth's hemisphere, at that time illuminated by the sun. Then turn the plate EFG till the centre of the penumbra, i, is over that particular place. This done, move the index H of the plate to the beginning of the division. Thus every thing is rectified for that time, and its phenomena may be collected.

Now the horary motion of the moon from the sun being taken from the tables, it will be, as the moon's horizontal parallax, is to her horary motion, so is the number of the parts of the plate EFG, which answer to the semidiameter of the globe, above found, to the quadrant which shows how many parts, on turning the plate round, are to be drawn through the place of the index, to have the situation of the disk of the penumbra, an hour before or after the time which answers to the primary situation. The disk therefore being brought to this place, and the globe turned on the axis, the phenomena of this time may be had in like manner.

The places marked on the surface of the globe, lying perpendicularly under the disk of the penumbra, in any situation of it, may be found by the pendulum. But they are seen at one view, when the whole apparatus is exposed to the sun's rays reflected from a plain speculum, in such a manner, that the rays may fall perpendicularly on the horizon of the globe. For then such shadows will be projected from the disk of the penumbra, on the globe, as are like the penumbræ cast on the earth by the moon, by which may be seen the phases of the eclipse for any place.

But this motion of the sun is inconvenient. Perhaps those who have a large burning-glass will make use of a lamp, the rays of which may be thrown on the globe from the glass, in a position perpendicular to its horizon. Otherwise, by viewing the globe from a distance through a perspective glass, then the disk klm, being brought on the surface of the globe, exhibits the penumbra. Various other contrivances may also be devised, for exhibiting the several phases of the eclipses.

Samuelis Christiani Hollmanni, Logic. et Metaph. in nova Gottingensi Academia P.P.O. Observatio de Sceletorum, ex Foliis virentibus paratorum, quorumcunque Duplicatura. N° 461, p. 789.

Samuelis Christiani Hollmanni, Logic. et Metaph. in nova Gottingensi Academia P.P.O. de Duplicaturæ Fibrarum, in Foliis quibuscunque conspicuæ Usu, aliisque huc pertinentibus, Conjecturæ. N° 461, p. 796.

The observations of Mr. Hollman add nothing of importance to the discoveries in vegetable anatomy and physiology made by Grew, Malpighi, and others, detailed in the preceding vols. of these Transactions.

An Earthquake at Scarborough, Dec. 29, 1737, *communicated by Maurice Johnson, Esq.* N° 461, p. 804.

The ends of several inclosures or fields behind the cliff, on the back of the spa, sunk down very low into the ground, making a large valley of a vast length, and considerable breadth, with 5 cows then grazing on it, the weight of which shook and opened the hill behind the house, after a frightful manner, and forced up the sands 100 yards in length on each side the space, and 27 broad, to the height of 6 yards, and in some places 10 yards high.

The pier, entire as it was, moved sideways out of its place, and rose up about 5 yards in the air; the house fell down, and at the same time took fire. The flag-house, and wooden rails, which were about the mouth of the well, were forced up in the air above 10 yards high, so that it is thought the spa-water is entirely lost for ever.*

No persons were hurt, the people of the house getting away in time.

An Examination of Sea-water frozen and melted again, to try what Quantity of Salt is contained in such Ice, made in Hudson's Straits by Capt. Christopher Middleton, F. R. S. N° 461, p. 806.

Dr. Hales, in his learned paper lately read at the Royal Society, where he proposes a method of rendering sea-water fresh, and wholesome to drink, mentions a diversity of saltness of the water at the Nore in the mouth of the Thames, and the water taken up in the Mediterranean Sea, this containing $\frac{1}{27}$ of salt, the former $\frac{1}{29}$. Mr. Boyle, in his Observations on the Saltness of the Sea, says, that about Holland the salt in the sea-water has been found to be $\frac{1}{40}$. In the English Channel, he found sea-water $\frac{1}{45}$ heavier than conduit-water, and, by immersing a lump of sulphur in it, he found the difference $\frac{1}{38}$; but by distillation ad siccitatem, he found the salt to be $\frac{1}{30}$, and in another trial $\frac{1}{37}$. It is certain the sea differs in saltness in different parts. It is in general observed, that in hottest climates the water is the saltest. At Mosambique Mr. Boyle relates an instance of a ship drawing two handsbreadth less water than usual. On the contrary, when salt water freezes, it has been thought to let fall all its salt; the ice of sea-water, and the water melted from it, tasting fresh, and being good for boiling meat and peas in. Capt. Middleton, being in Hudson's Straits in July 1738, took ice from under the surface of the sea, which he melted till he got 40 quarts of water, which he evaporated to dryness, and out of that quantity had only 6 ounces of salt, or about $\frac{1}{213}$ part.

* The spa was soon after recovered as good as before.—Orig.

A Rule for finding the meridional Parts to any Spheroid, with the same Exactness as in a Sphere. By Colin Mac Laurin, F. R. S. N° 451, p. 808.

It was demonstrated long since, that in a sphere the nautical meridian line is a scale of logarithmic tangents of the half complements of the latitudes. The same may be computed with no less exactness to any spheroid by the following rule.

Let the semidiameter of the equator be to the distance of the focus of the generating ellipse from the centre, as m to 1. Let A represent the latitude for which the meridional parts are required, s the sine of this latitude, the radius being unit; find the ark B, whose sine is $\frac{s}{m}$; take the logarithmic tangent of half the complement of B from the common tables; subtract this logarithmic tangent from 10.000000, or the logarithmic tangent of 45°; multiply the remainder by $\frac{7915.7044678978}{m}$ &c. and the product subtracted from the meridional parts in the sphere, computed in the usual manner for the latitude A, will give the meridional parts expressed in minutes, for the same latitude in the spheroid, provided it is oblate. When the spheroid is oblong, the difference of the meridional parts in the sphere and spheroid for the same latitude, is then determined by a circular ark; but it is not necessary to describe this case at present.

Example.—If $mm : 1 :: 1000 : 22$, then the greatest difference of the meridional parts in the sphere and spheroid, is 76.0929 minutes. In other cases it is found by multiplying the remainder above mentioned by 1174.078.

The Parabolic Orbit for the Comet of 1739, *observed by Signor Eustachio Zanotti at Bologna.* N° 461, p. 809.

The motion in its own proper orbit was retrograde.

The perihelion was in .... ♋ 5° 11′
The descending node in .... ♈ 25 18
The perihelion from the node 69 53
The comet was in the perihelion June 9ᵈ 9ʰ 59ᵐ
——————— descending node July 18 4 57

The perihelion of the comet's orbit was within the sphere of the orbit of Venus, and without that of the orbit of Mercury; being distant from the sun 0.69614 parts of the earth's mean distance from the sun. The plane of the orbit was inclined to the plane of the ecliptic in an angle of 53° 25′. The diurnal mean motion, according as it is interpreted by Dr. Halley in his Elements of cometical Astronomy, was 1° 5707.

Concerning an Extraordinary Skeleton, and of a Man who gave Suck to a Child. By Robert Lord Bishop of Cork. N° 461, p. 810.

The bishop of Cork here gives an account of a skeleton of a man, whose bones, during his life-time, were almost all grown into one entire bone, so that now his flesh is taken from them, he is one entire skeleton. The only bones he could move before his death, were the wrist of his right hand, and the bones of his knees, so that he could move his legs a little; and, when set upright, could in about a quarter of an hour get a foot forward.

For many years before his death, he could not alter his posture in the least. He was valued by his master on account of his honesty. The only use he was capable of being put to, was that of watching the workmen; for when he was once fixed in his station, it was impossible for him to desert it.

At about 18 years of age he began to be unwieldy, and so continued growing more stiff, till he lost all use of his limbs, and died in the 61st year of his age. The posture into which he fixed at last, was somewhat like that of the Venus of Medicis, only that his right hand was the lowest, and the left hand did not rise higher than the elbow of the right. He was originally deformed, his left shoulder rising higher than his right; the vertebræ of his back were exceedingly bent inwards towards the lower part, with an inclination towards the left hip. The os sacrum was so bent outwards, that you had no sight of it all. His left knee did not come down so low as the right by 3 or 4 inches. There was hardly one bone in his body in the figure it ought naturally to be, except the bones of his legs, which were not much distorted.

He was one entire bone from the top of his head to his knees.

His head seemed regular, and the sutures pretty distinct, though more united than in common skulls. His jaw-bones seemed entirely fixed, and grown together, as were also the teeth in the hind part of the jaw. His fore-teeth were very irregular, which left a vacancy for him to suck in his food at. Out of the back of his head there grew a bone, which shot down towards his back, and passed by the vertebræ of the neck at about an inch distance: this bone united to the vertebræ of the back, and the scapula of the left shoulder, from whence it disengaged itself again, and continued distinct, till it divided into 2 towards the small of the back, and fixed itself into both the hip-bones behind. The vertebræ of the neck and back were one continued bone.

In the fleshy part of his thighs and buttocks Nature seemed to have sported herself, in sending out various ramifications of bones from his coxendix and thigh-bones, not unlike the shoots of white coral, but infinitely more irregular; some behind, and some before; some in clumps and clusters, and others in irregular shoots of 8 or 9 inches in length. You could not pass your hand between his two knees, which inclined much towards the right, his left shoulder having been the highest. One of the bones of his left arm was once broken by a fall, and nature had shot out another bone a little above the bending of the arm, which united to the broken bone, and made it much stronger than it was before, though the bone seemed more liable to decay about the place where it was formerly broken. All the cartilages of his breast, 4 only excepted, were turned to bone. These 4 served to move his breast in respiration.

Out of his heels there frequently grew bones like the spurs of a cock, 2 or 3 inches long, which he shed as a deer does his horns. When he was dissected, there was a bone found in the fleshy part of his arm, quite distinct and disengaged from any other bone; it was very thin, about 4 inches long, and $\frac{1}{4}$th part of an inch broad, with several ramifications. Yet while these bones were growing, he never complained of any pain in his muscles.

The bishop met with a man at Inishanan, about 10 miles from Cork. He was about 70 years of age, by birth a Frenchman, but was a refugee on account of his religion; was bred a gardener, and had been industrious, till deprived of his strength by age.

This man affirmed he had once suckled a child of his own. His wife, he said, died when the child was about 2 months old: the child crying exceedingly while it was in bed with him, he gave it his breast to suck, only with an expectation to keep it quiet; but he found that the child in time extracted milk; and he affirmed, that he had milk enough afterwards to rear the child. The bishop looked at his breasts, which were then very large for a man; and the nipple was as large or larger than any woman's he ever saw.*

Concerning the Cæsarian Operation performed by an ignorant Butcher. N° 461, p. 814.

Sarah Mc Kinna, who now lives at Brentram, 2 miles from the city of Clogher, in the county of Tyrone, was married at the age of 16 years. Before her marriage she never had the appearance peculiar to women; but in a month after her marriage, those appearances showed themselves properly. Ten months after her marriage, she found the symptoms of pregnancy, and bore a child at the expiration of the usual time. Ten months after she was delivered of another; and each time had a speedy and easy delivery.

* No mention is made that the organs of generation were examined, so as to afford positive proof that this person was a man. But admitting the person to have been a man, there appears to have been no other evidence but the man's own assertion, that a child had been supplied with milk from his breasts. The largeness of the breasts and of the nipples can by no means be admitted as a proof thereof.

Two months after her second delivery, symptoms of pregnancy appeared again, and increased in proportion to the time; but at the end of 9 months those symptoms began to abate, and in a little time she had no other reason for thinking she was with child, but an absolute stoppage of her catamenia. Nor had she, during the space of 6 years and some months, any one return of them; but for the greatest part of that time, especially the first 4 years, she was perpetually afflicted with most violent pains in the middle region of the abdomen.

Some time in the 7th year after her last pregnancy, which ended in such an unusual manner, a swelling in her belly, and other symptoms, made her conclude she was again pregnant. About 7 months after this uncertain account, a boil, as she thought, appeared about an inch and a half higher than her navel. During this time of her pregnancy she often found the symptoms of her being quick with child, till about 6 weeks before this boil, as she called it, appeared. It was attended with very great pain.

She sent for one O Neill, a butcher. This man came to her the Sunday after her message, and found her in an expiring condition. By this time the imposthumation had broken, and an elbow of the child had forced itself through it, and appeared in view. At the request of herself and friends, he undertook to administer relief to her, and made so large an incision above and below the navel, as enabled him, by fixing his fingers under the jaw of the fœtus, to extract it; in which operation he met not with the least impediment. He afterwards looked into her belly, and seeing something black, he put in his hand, and extracted, by pieces, a perfect skeleton of a child, and several pieces of black putrefied flesh. After the operation, he swathed her up; and in 6 weeks she pursued her business about the house.

She had been in good health ever after; only she had a navel rupture, owing to the ignorance of the man in not applying a proper bandage.

Of a new Invention of expanding Fluids, by their being conveyed into certain heated Vessels, where they are immediately rarefied into an elastic impelling Force, sufficient to give Motion to Hydraulopneumatical and other Engines, for raising Water, and other Uses, &c. By John Payne. N° 461, p. 821.

To produce a great power at a small expence, is what every body desires in moving machinery: and is what, by this new invention, we have proved by experiments and practice, to be a great improvement, when applied to that noble invention the fire-engine. The following is a short description of the vessels and machinery contrived for that purpose; viz.

A pot or vessel made of wrought or cast iron, nearly the figure of a cone, its diameter at the base being 4 feet, with holes round the edge, for nails or screws to fasten a globular end of copper of about 5¼ feet diameter. There is then placed in the inside a small vessel, which Mr. P. calls a disperser. This vessel has spouts round the sides fixed to it, and the bottom resting on a centre-pin; and in the middle of this basin is a socket, with holes near the bottom, to let the water pass from above, through an iron pipe of about 7 feet long, the lower end of which is placed in the socket, so as the end of the pipe will be always immersed in water in the basin, to prevent the expanded fluid from returning up the pipe; and the other end of this pipe goes up through the copper-head, which is inclosed very tight, but so as it may easily be moved with a circular motion, in order that the water which is conveyed through this iron pipe down into the disperser, may be dispersed or showered round, on the sides of the red-hot pan, or ignited vessel, in a very exact manner.

This evaporating vessel being completed, they then take 1, 2, or more of these vessels, thus furnished, and place it or them in a reverberatory arch or canal, for conveying the intense heat of a strong fire, the flame of which encompasses the metal pot or pots, and brings them to a red heat; and in that condition they are continually kept, while in use, with the water running from a cistern or vessel, where the water is heated, through a guage-cock, down the iron pipe, into the disperser, which conveys it to the sides of the ignited vessel, when it is immediately rarefied or expanded into an elastic steam or vapour, fit for application to give motion to sundry sorts of machinery, &c.

In fig. 5, pl. 11, A represents a copper globe, 12 inches diameter; B, B, two brass cocks, one opposite to the other, fitted very tight; C, a handle or bale, fastened to the globe, by which it may be hung or held up; D, a small valve or clack, fitted to the upper cock, of one inch diameter.

The whole thus fitted, weighed 12lb. 9¼ oz. avoirdupois; and, filled with water, it weighed 45lb. 7 oz. from which deduct the metal, the weight of water is 35lb. 13¼ oz. which is about 4 gallons.

This vessel Mr. P. hung over the large vessel F, in which water was rarefied into steam; and by the pipe E, at the large cock G, which being open, as also the other cocks B, B, the stream had a free passage through the globe A, by which the stream excluded or forced out the air that was in the globe, and by its elastic quality supplied its place; when both cocks B, B, were suddenly shut, and the globe A taken down and hung over a vessel of cold water, with the lower cock B, immersed in water, and then opened under water; on which the water rushed violently into the globe till it had supplied the vacuum, when the cock was again shut, and the globe, with the water, put in the scales, and then found to weigh 44lb. 9 oz. which take from 45lb. 7 oz. the whole weight, as before, there remains only 14 oz. the difference, which shows that all the air was nearly excluded out of the globe by the steam: in ounces it stands thus $\frac{727}{715}$, which is very nearly a perfect vacuum.

2dly, He again excluded the air out of the globe with steam as before, and both cocks B, B, being closed with the globe full of steam, he put the globe in the scales, and it weighed 12 lb. $10\frac{1}{2}$ oz. He then opened one of the cocks, and let in the air, on which the scale descended; and, by adding weight in the other scale, it was found to weigh 12 lb. 11 oz.; which showed that the weight, not the pressure, of the air the globe contained, was $\frac{1}{2}$ an ounce avoirdupois.

3dly, the globe being filled with steam, as before, and condensed with cold water on the outside of the globe, and the metal again made very dry, and the air let into the globe, the water from the condensed steam was found to weigh 4 penny-weights.

4thly, The globe filled with steam, as before; only now he continued the globe longer with the steam passing through it, by which it acquired a greater degree of heat; for he found by those experiments, that the least degree of cold less than the steam, a part would be condensed again into water, by which the quantity could not be certainly ascertained, which would exclude the air out of a certain space, which is the chief end of this experiment. But in this experiment he succeeded better; for, on weighing the globe, when the steam was condensed, the air let in, and all cold, it was as follows, viz. 15 lb. 3 oz. 2 dwts. Troy, the weight without the steam being 15 lb. 3 oz.; so that the weight of the water condensed from the steam, or the water converted into a strong elastic steam to fill the space of this little globe, is but 2 dwts, or $\frac{1}{10}$ of an ounce Troy of water, by which $\frac{1}{10}$ of an ounce Troy of water fills, when converted into steam, 925 cubic inches of space in a vessel, so as to exclude nearly all the air.

He repeated this experiment several times, and found it nearly the same; and by immersing the cock in water, and opening it again, as in the first experiment, he found the weight of water to be nearly as above, and to make about $\frac{15}{16}$ void or vacuum; so that 1 ounce Troy of water makes 9250 cube inches of steam, of equal force with the like number of inches of air; and with this remark, that the weight of the steam is much less than the weight of common air; for in this globe the air weighed $\frac{1}{2}$ oz. avoirdupois or 9 dwts. Troy; and the steam, which filled the same space, only 2 dwts. Troy, which is but little more than $\frac{1}{5}$th part, and shows how very small the particles of water are when so divided by the force of fire, and of what force. From which he concludes, that 1 cubic inch of water will discharge or force out 4000 inches of air from a vessel of that content.

5thly, Proceeding as before with steam in the globe A; he condensed it, as in the third experiment; and then tried the pressure of the atmosphere on the clack or valve D, and found it required about 10lb. Troy, to lift the clack from its tube of 1 inch diameter.

6thly, He excluded the air with the steam, and instead of the clack he screwed on very tight a plate, on which he placed a glass receiver, as usual, with the air-pump; and then, turning the cock, the air under the glass receiver expanded itself into the globe, by which he had equally a share of the vacuum partly made in the globe, and could thereby make many experiments that are made with the air-pump, &c.

Observations from Experiments made by J. Payne.—1. That a pot or vessel, of the size and shape here mentioned, will (being kept to a dark-red heat, and the water regularly dispersed) rarefy or expand 50 gallons of water, wine measure, per hour.

2. That a cubic inch of water will make in practice 4000 inches of steam; or that the elastic steam of one cubic inch of water is sufficient to exclude the air out of a vessel that is in content 4000 inches.

3. That the above 50 gallons will produce 46,000,000 cubic inches of elastic steam per hour, which is per minute 770,000.

4. That the 2d pot or vessel, as in fig. 7, will rarefy or expand 40 gallons of water, wine measure, per hour, and will produce 36,960,000 cube inches of elastic steam, per hour, which is per minute 616,000 inches.

5. That both, being united together, make 1,386,000 cube inches of steam every minute, from 346 inches of water.

6. That, by an experiment made by a fire-engine, 40 gallons of water per hour, made into elastic steam in this method, will effectually give motion to a 24 inch cylinder fire-engine.

7. That, by true experiments, made at Wedgbury and Newcastle on Tyne, 112lb. of pit-coals, will, and is sufficient in this method, to expand or rarefy 90 gallons of water per hour into an elastic steam or vapour.

8. That, by the best accounts and observations he could get and make, they consume under their boilers, to make the same quantity of steam, 3 cwt. of pit-coal, in working a fire-engine one hour.

9. That 95 gallons of water per hour, expanded or rarefied into steam, will work a 36 inch cylinder engine.

10. From these observations Mr. P. concludes, that this new invention will save at least 60 per cent. in pit-coals, to work a fire-engine.

In fig. 6, pl. 11, A, A, represent the two pots; B, B, the two copper heads or globes; C, C, the two sink pipes, for waste water, not evaporated; D, D, clacks or valves, to keep out the air; E, E, the two dispersers and spouts; F, F, the stools with a centre-pin, on which the disperser rests; G, G, the two iron pipes, in which the water is conveyed to the cistern; H, a cistern of hot water; I, I, two cog-wheels, to turn the disperser; K, a steam-pipe, in which is conveyed the steam to the cylinder; L, the cylinder of the fire-engine; M, M, leaden pipes, that convey hot water from the cistern to the disperser.

An Examination of Westashton Well-waters, a Well about 4 Miles from that of Holt. By Ambrose Godfrey Hankewitz. N° 461, p. 828.

Obs. 1.—Mr. G. H. took 4 oz. of the Westashton water, with as much milk, and set them on the fire; as soon as they boiled, the milk began to curdle, which denotes a brackish salt of a neutral nature. The water changed syrup of violets green.

2. Some powder of galls infused in this water, gave it a tinge of a brown purple; by which it appeared that this water was chalybeate.

3. A fixed alcali, as ol. tartar. per deliq. and a volatile one, as sp. sal. ammoniaci, caused a white precipitation; which denotes an aluminous cretaceous earth.

4. A solution of salt of lead, caused a cream-like, or a troubled milky colour.

5. The usual acid spirits, viz. spirit of salt, nitre, and vitriol, caused no alteration; which shows that the water is itself impregnated with an acid.

6. The water, being evaporated to a pellicle, deposited saline crystals of a rough or austere taste, being of a styptic nature; and separated a martial yellowish ochre, which was attracted by the loadstone, and was an absorbent, for it fermented with acids. The remaining brine, being evaporated to dryness, left a salt of a lixivious alcaline taste.

7. Some of these salts being put into water, 3 parts out of 4 dissolved very readily; but $\frac{1}{4}$th part would not dissolve at all, but was of a talcky nature, and unalterable in the fire.

Hence he observes that chalybeate waters, as long as they retain their natural sulphureous gas, are capable of keeping suspended, or floating in them, these talcky substances; but that boiling drives away that sulphureous gas, on which this talcky substance subsides, and cannot again be dissolved in water, but remains fixed against the power of the fire; for it suffers no alteration on a red-hot iron, neither emitting flame, nor melting, as neither does talc itself.

8. These chalybeate-waters contain somewhat of the same nature as our cathartic Epsom salt, only not so mild on the tongue; for by this examen, when their gas is gone, they are found to contain 2 sorts of such like earths; the one absorbent, fermenting with acids; and the other fixed, or talcky: and that this substance is really talcky, is confirmed by the digging up of much talc in sinking this well.

All the salts of the medical waters are more generally alcaline than acid, being of a martial nature, impregnated with sulphur, which gives them a muriatic taste.*

Hence Mr. G. H. concludes that this Westashton water is a very good chalybeate water; and, by report, more plentiful and more constant all the year round, than the well at Holt, which spring diminishes much at a certain time of the year; but both seem alike for their virtues, and physical use, being both alike martial.

An Examination of the Cheltenham Mineral Water. By Conradus Hieronymus Senckenberg. N° 461, p. 830.

From his experiments Mr. Senckenberg inferred that the Cheltenham mineral-water contains Glauber's salt, (sal mirabile Glauberi) mixed with common salt. As for the bitterness of this water, there is (his words are) no other reason for it than the terra cretacea, which is proved by the Epsom salt, where the terra alkalina salis communis, is joined with the acidum vitriolicum; and after the same manner in the sal mirabile, the alkaline earth causes the bitter taste. He adds that from 1 lb. Troy of this water, he obtained 29 grs. of the said salt, and 3 grs. of earth.†

Of a new Purging Spring discovered at Dulwich in Surrey. By Mr. John Martyn, F.R.S. N° 461, p. 835.

Dulwich is a village about 6 miles south of London, at the foot of that ridge of hills which divides the counties of Kent and Surrey. The purging springs, which have been esteemed for about 100 years, and are commonly known by the name of Dulwich waters, have been improperly so called; those springs arising in a valley on the south side of the hills, in the middle of a large common belonging to the parish of Lewisham in Kent; whereas Dulwich is on the north side of the hills, in the parish of Camberwell in Surrey.

In the autumn of 1739, the master of the Green Man public-house, at Dulwich, lying about a mile beyond the village, was desirous to dig a well for the service of his house, there being no spring of good water near it. The well being digged to the depth of 60 feet, and no water appearing, the owner caused it to be covered up, and gave himself no further trouble about it that winter. The following spring, on Mr. M. going down, it was opened, and he

* In most chemical processes Mr. G. A. Hankewitz was allowed to be sufficiently expert; but he appears to have been unequal to the task of examining mineral waters, and accordingly he deduced, from a very imperfect analysis, many erroneous conclusions respecting their composition.

† It is now known that the Cheltenham mineral water contains a great variety of saline ingredients, such as Glauber's salt, Epsom salt, common salt, magnesia muriata, selenite, &c. besides iron, carbonic acid, and other gases.

found 25 feet of water, of a sulphureous smell and taste, which went off, after the well had been opened some days. As he had a strong suspicion, that this water was impregnated with some mineral, he examined it by the following experiments:

1. It curdled milk.—2. It became green, when mixed with syrup of violets, which colour disappeared in a few days.—3. Being poured on green tea, it did not acquire any colour.—4. Being mixed with powdered galls, it acquired a deeper brown colour than rain-water did, and continued turbid; whereas the rain-water continued clear, after the galls were subsided.—5. Being shaken in a close-stopped phial, it disploded a vapour on opening the phial before the commotion ceased, with a more audible noise than common water did.—6. Being mixed with oil of vitriol, and oil of tartar, a much more considerable ebullition was raised, than by the mixture of those liquors with rain-water.—7. Six quarts of this water, being boiled to a pint, let fall a large quantity of a fine, whitish, insipid powder; and the water so boiled had a very strong saline taste, with a mixture of bitterness, not unlike the sal cartharticum amarum.—8. It let fall a copious white sediment, on the addition of the oil of tartar, which has the same effect on a solution of alum, or of sal cartharticum amarum.—9. The boiled water, after it had deposited its earth, precipitated large white flakes, on the addition of oil of tartar.—10. It differs from a solution of common salt. For the oil of tartar, being dropped into that solution, caused only a slight precipitation, which was soon afterwards absorbed again by the water.—11. It does not lather with soap.

Having made these experiments, Mr. M. was satisfied, that this new spring was really a purging water, as it was afterwards found by experience. Several persons tried it, to their great advantage. Being drank fresh, in the quantity of 5 half pint glasses, it purges quickly, not sinking, but raising the spirits. It is found to be very diuretic.

These properties of the Dulwich water do not seem to be owing to any of the materials found in digging the well. The pyritæ are known to be a mixture of iron and sulphur; but this water seems to have hardly any parts of iron in it [Exp. 3 and 4.] The spirit, with which it abounds, [Exp. 5 and 6] may, perhaps, be owing to a fermentation of the sulphur, which is continually flying off, as appears by its strong smell, after it has been for some time covered up. And a silver cup, which has been often used in drinking this water, has acquired a yellowish colour.

The ludus helmontii affords nothing but iron. Nor does the clay, through which they dug, discover any salt in its composition. We may therefore conclude, that the hill, which lies between the old wells and this new one, contains the purging salt, with which these waters are impregnated.

Mr. M. did not find any material difference between the old and new waters, except in the convenience of drinking them. The old wells are at a distance from any house, except some few huts, and exposed to the rain and land-floods, by which they are often injured: the new well is a mile or two nearer to London, and well secured from any injuries of the weather.

Of the Lights seen in the Air, an Aurora Australis, on March 18, 1738-9, *at London. By Cromwell Mortimer, M.D. Sec. R.S.* N° 461, p. 839.

March 18, 1738-9, about half past 7, there appeared a bright column arising about the east north-east, which reached up with its point near to the zenith, but going a little south of it. This column seemed to be the boundary of the clear and obscure regions of the sky. It had a uniform steady light, without any dartings or shiverings; but it sometimes vanished for a few minutes, and then returned again all at once, not proceeding from the bottom, but from the side next the misty part of the sky, as if it were only the border of the mist illuminated. About 8 this column was become much wider, and all of a breadth, extending in the same direction beyond the zenith to the west south-west; the addition to its breadth seemed to be all on its southern edge; this whole band was of a most beautiful pink-colour. A quarter after 8, the phenomena remained the same; but to the north north-west there appeared some whitish clouds about 20° from the zenith: out of these arose 3 beautiful pyramids of light, which extended very near the zenith; their middle of a beautiful sea-green, which went off gradually in lighter shades towards the edges, which were of a bright white; the colour of these very much resembled the light of phosphorus. Half an hour after, it was all over; but it returned again about 10; when the redness spread, almost universally, over the southern parts of the heavens.

Concerning the same Aurora Australis, seen at Chelsea, near London. By Mr. John Martyn, F.R.S. N° 461, p. 840.

At half past 8, being informed that there was a great fire towards London, Mr. M. made haste towards an upper window that looked to the north north-east: he found an extraordinary redness in the air, but of too determined a figure to arise from the burning of a house: a broad red band extended to the northward of the east; in the middle of which he plainly saw Arcturus, then about 25° high; and its northern edge touched Cor Caroli. It seemed to be fixed and permanent; not radiating, or fading, as in a common aurora borealis.

This red band, or arch, was bounded on the north by streams of a greenish blue, in the same direction.

After considering this phenomenon for some little time, Mr. M. retired into his garden, where he saw a great brightness almost in the zenith, but declining to the south-west; which he found to be a centre, from which proceeded many luminous radii, of which the red band was much the most considerable. This crown, or centre, seemed at that time about the place of Cancer; for it effaced all the neighbouring stars, and he could but just see the 2 stars in the heads of the Twins; when the brightness was most faded. It would sometimes almost disappear for near a minute, and then kindle again, and dart rays on all sides; but those to the west and north were short, pale, and soon disappeared. Those which shot southward, were of a fiery red; and the whole southern part of the atmosphere was tinged with a red brightness, which did not however reach quite down to the horizon. About 9 the red band had covered the tail of Ursa Major, having moved considerably towards the north, the centre continuing in the same place; and by degrees it faded so as not to be distinguishable from the common redness which was spread over so considerable a part of the heavens. About 10, he went to the river-side, where he had a large prospect to the south-east; and found all that part covered with a dusky red, quite down to the horizon. There were afterwards some faint rays darted, sometimes from the centre of this phenomenon, which had the appearance of a common aurora borealis.

Concerning the same Aurora Australis. By the Rev. Mr. Timothy Neve. N°. 461, p. 843.

We had from about half an hour after 7, till almost 9 o'clock, an aurora australis, which spread with variety of colours all over the horizon, meeting in a centre almost vertical, but somewhat inclining to the south. The original colours were so mixed and blended in the common centre, as, by the vast variety easily distinguished, made a beautiful appearance. The fainter colours came from the two opposite points of the north-west and south-east. The blood-red crimson, &c. were seen chiefly in the north-east and south-west.

Description of a Catheter, made to remedy the Inconveniencies which occasioned the leaving off the high Operation for the Stone. By Archibald Cleland, Surgeon. N° 461, p. 844.

The high operation for the stone was left off very precipitately, in order to introduce that method now called the lateral operation, which has been practised for some time with good success; but had the operators at that time had the benefit of this instrument, Mr. C. is persuaded the advantage would have been more than equal in favour of the high operation, and preferable to any other method yet practised. And he hopes that the description, and the method of using this catheter, will be a means of reviving an operation so happily begun, and calculated for the good of those that are afflicted with the stone in the bladder.

This catheter is made either of silver or steel, of different sizes, to suit different ages; and has the outward appearance of a common catheter, and will answer the same uses. But in respect to this operation, it differs from the common in this, that it is composed of two legs, with blunt points, a long tube, a sliding bolt; and a handle, which serves to open and shut the legs. The bolt, which is fixed to the extremity of the tube, goes into two holes, fixed in the plate of the handle: the one serves to keep the legs close during the time it is to be introduced into the bladder, the other to extend the points at the distance of an inch or more, during the time the operation is performing.

The method of using this catheter is, after having taken the necessary precautions, and filled the bladder, first to introduce the catheter into the bladder, then unbolt it at the handle, and by holding the tube in one hand, and the handle that moves the legs in the other, turn or open the legs, till the bolt becomes opposite to the second hole on the plate, into which the bolt must be thrust; then by pressing gently the handle downwards between the patient's legs, the 2 blunt points will be easily felt above the os pubis, in the protuberance made by the injection into the bladder.

The advantages he proposes, by using this instrument, are these: First, to be a director for the operator, in determining the place where the puncture is to be made in the bladder. It also serves as a support to the bladder, when the water flows out; and keeps it from subsiding during the time of the operation, and till the stone is extracted: it serves also to resist the pressure of the abdominal muscles and peritoneum, and hinders the intestines from being forced down upon the knife; and keeps the orifice open, till the stone or stones are brought away. And lastly, by the help of this instrument it may be discovered, whether the bladder is indurated or scirrhus.

The method of performing this operation with safety, is, after having introduced and fixed the catheter with its legs open, to feel for the 2 points above the os pubis, and place the finger and thumb gently upon them; then give the handle to an assistant, to keep it firm in that position; and with the knife in the right hand, make a puncture at once into the bladder, exactly in the middle between the points; but for the more security, somewhat lower nearer the os pubis; then without drawing out the knife, make a large incision downwards inclining under the arch of the pubis, in proportion to the size of the stone, taking care not to wound the cartilage that joins the bones together, when the knife is withdrawn. The bladder being thus supported, the stone may be extracted with the fingers, or with a small pair of tenets, there being little danger of breaking it in this method. When the operation is finished, raise the handle of the catheter, and unbolt it; shut it close, and fix it so; then withdraw the catheter, and dress the patient.

Fig. 1. pl. 12, represents the catheter as it is to be introduced into the bladder, the 2 legs A and B being closed together.

Fig. 2. The catheter, its 2 legs A, B, being open. C, D, the tube; E, the sliding-bolt; F, the two holes into which the bolt is to be slid; G, the ears fixed to the tube C, D, which is all of one piece with the leg A; H, the handle, which opens the legs; this handle is all of one piece with the leg B, which B is a continuation of a wire, that runs through the tube C D, and is fastened to the handle H, and turns with it.

Of Needles made for Operations on the Eyes, and of some Instruments for the Ear. By the Same. N° 461, p. 847.

The first differs from a common couching-needle (fig. 3, pl. 12) in this, that it is made of two pieces of steel soldered together, and fixed in a handle (fig. 4): at a little distance from the handle they separate, and have, in each lamina, a button fixed, which passes through a hole in the other; from this part to the points, they are so nicely applied, and polished together, that they cut, and have the shape of a common needle. On pressing the buttons, the points are separated, and in the inside of the broad part of the points are several small indents, to prevent any thing from slipping, after it has once got hold.

The use of this needle is, either to depress a cataract; or, if it should be found of such a nature as to bear to be taken hold of, then, by opening the points, to engage it, and carefully bring it out of the eye.

If it should happen, that in dressing the cataract, or in bringing it out of the eye, some of the small vessels are wounded, and some drops of blood diffuse themselves in the aqueous humour; this second needle (fig. 5.) is intended to remedy this inconvenience.

It is a long, small, round stilet (fig. 6.) gradually decreasing from the handle to the point; and is fitted to a long silver tube of the same shape, (fig. 7.)

into which the needle is put, and the point comes out at the end $\frac{1}{4}$ of an inch. This is to be introduced into the eye at the orifice the other needle had made; when it is so far introduced, as the end of the tube is within the posterior chamber of the aqueous humour, the needle is to be withdrawn, leaving the tube in the eye; and then, with the mouth, may be sucked into the tube, all the blood, and watery humour that is contained there, or any other floating particles; then the tube is to be withdrawn, and the eye left to replenish itself with the aqueous humour again; which will take but 12 or 18 hours at most.

The following Instruments are proposed to remedy some kinds of Deafness proceeding from Obstructions in the external and internal auditory Passages.

In order to discover with more exactness, whether the disorder lies in the outward ear, Mr. C. made use of a convex glass, 3 inches in diameter, fixed in a handle, (fig. 8) into which is lodged some wax candle, which comes out at a hole near the glass, and reaches to the centre; which, when lighted will dart the collected rays of light into the bottom of the ear, or to the bottom of any cavity that can be brought into a straight line; therefore, when it is discovered by the help of this glass, and lighted candle, that the ear is full of hard wax, which will not bear to be taken out with the forceps, the method is to have a small boiler, containing some proper herbs; and by different tubes of various sizes, the steam is conveyed to the bottom of the ear. In a short time, the wax will dissolve, and the person find great ease. In one of these tubes, are placed 2 valves, to regulate the heat to the person's inclination. If this has not the desired effect, and the person still remains deaf, the following instruments are made to open the Eustachian tube. If, upon trial, it should be found to be obstructed, the passage is to be lubricated, by throwing a little warm water into it, by a syringe joined to a flexible silver tube, which is introduced through the nose into the oval opening of the duct at the posterior opening of the nares, towards the arch of the palate. The pipes of the syringe are made small, of silver, to admit of bending them, as occasion offers; and, for the most part, resemble small catheters: they are mounted with a sheep's ureter (fig. 9); the other end of which is fixed to an ivory pipe; which is fitted to a syringe, by which warm water may be injected: or they will admit to blow into the Eustachian tube, and so force the air into the barrel of the ear, and dilate the tube sufficiently for the discharge of the excrementitious matter that may be lodged there. The probes, (fig. 10) which are of the same shape with the pipes, have small notches near the points, which take in some of the hardened and glutinous matter, contained in those tubes, which is distinguished by the fetid smell, when the probes are withdrawn.

There is another kind of deafness, which proceeds from a violent clap of thunder, noise of a cannon, or the like. In this case, it is probable, that the position of the membrana tympani is altered, being forced inwards on the small bones, and so becomes concave outwardly. In this case no vibration of sounds will be communicated to the drum, till the membrane has recovered its natural position. The means proposed to remedy this disorder are, first, (if the person heard very well before, and it be not too long after the accident has happened) to oblige the patient to stop his mouth and nose, and force the air through the Eustachian tube into the barrel of the ear, by several strong impulses; which will probably push the membrane back to its natural state.

But if, by any accident, the excrement is hardened in the tube, or its orifice, which opens into the barrel of the ear, should be stopped up, so that no air can be forced that way, the 2d method proposed, is to introduce into the meatus auditorius externus, an ivory tube (fig. 11) as near to the drum as can be done, and so exactly fitted, that no air can go in or out, between the skin of the internal meatus and the tube. When it is thus fixed, take the farther small end in your mouth, and by degrees draw out the contained air; and it will act like a sucker on the membrane, and draw it back to its natural state; then the person will hear as before. If this should fail, probably the violent shock this membrane has suffered, may have dislocated some of the small bones; in which case there is scarcely any remedy. And for the diseases that are called nervous, he leaves them to the learned gentlemen of the faculty.

In this ivory tube may be fixed a brass cock, (fig. 12) which, being turned, will hinder the rushing in of the air, while the person who sucks, takes breath, and can renew his suction.

The flexible silver tube, for injecting the Eustachian tube, may be used without the sheep's ureter, by being screwed on to a small silver syringe, as at fig. 13.

Concerning a violent Hurricane in Huntingdonshire, Sept. 8, 1741. By Mr. Stephen Fuller, Fellow of Trin. Col. Camb. N° 461, p. 851.

This was the most violent hurricane of wind in these parts, that ever was known since the memory of man. Cambridge was not in the midst of the hurricane, so that it has escaped very well. Mr. F. happened to be at Bluntsham in Huntingdonshire, about 10 miles north-west of Cambridge. They were there in the midst of the hurricane. The morning, till half an hour after 11, was still, with very hard showers of rain. At half after 11 it began to clear up in the south, with a brisk air, so that they expected a fine afternoon. The south-west cleared up too, and the sun shining warm drew them out into the garden. They had not been out above 10 minutes, before the storm was seen coming from the south-west: it seemed not to be 30 yards high from the ground, bringing along with it a mist, rolling along with such incredible swiftness, that it ran about a mile and half in half a minute. It began exactly at 12 o'clock, and lasted about 13 minutes, 8 minutes in full violence: it presently uncovered the house, and some of the tiles, falling down to windward, were blown in at the sashes, and against the wainscot on the other side of the room; the broken glass was blown all over the room; the chimneys all escaped; but the statues on the top of the house, and the balustrades from one end to the other, were all blown down. The stabling was all blown down, except two little stalls. All the barns in the parish, except those that were full of corn quite up to the top, were blown flat on the ground, to the number of about 60. The dwelling-houses escaped best; there was not above 12 blown down, out of near 100. If the storm had lasted 5 minutes longer, almost every house in the town must have been down; for they were all, in a manner, rocked quite off from their underpinnings. The people all left their houses, and carried their children out to the windward side, and laid them down on the ground, and laid themselves down by them; and by that means all escaped, except one poor miller, who went into his mill to secure it against the storm, which was blown over, and he was crushed to death between the stones and one of the large beams. All the mills in the country are blown down. Hay-stacks and corn-stacks are some quite blown away, some into the next corner of the field. The poor pigeons that were caught in it, were blown down on the ground, and dashed to pieces. Wherever it met with any boarded houses, it seemed to exert more than ordinary violence on them, and scattered their wrecks above a quarter of a mile to the north-east, in a line: Mr. F. followed one of these wrecks; and about 150 yards from the building, he found a piece of a rafter, many feet long, and about 6 inches by 4, stuck upright 2 feet deep in the ground; and at the distance of 400 paces from the same building, was an inch board, 9 inches broad, 14 feet long: these boards were carried up into the air; and some were carried over a pond above 30 yards; and a row of pales, as much as 2 men could lift, were carried 2 rods from their places, and set upright against an apple-tree. Pales, in general, were all blown down, some posts broke off short by the ground, others torn up by the stumps. The whole air was full of straw: gravel-stones, as large as the top of the little finger, were blown off the ground in at the windows; and the very grass was blown quite flat on the ground. After the storm was over, he went out into the town, and such a miserable sight he never saw: the havoc above described; the women and children crying, the farmers all dejected; some blessing God for the narrowness of their escape, others wondering how so much mischief could be done with one blast of wind, which hardly lasted long enough for people to get out of their houses. Two people, that were out in it all the time, said, that they heard it coming about half a minute before they saw it; and that it made a noise resembling thunder, more continued, and continually increasing. A man came from St. Ives, who says, the spire of the steeple, one of the finest in England, was blown down, as was the spire of Hemmingford, the towns having received as much damage as Bluntsham. There was neither thunder nor lightning with it, as there was at Cambridge, where it lasted above half an hour, and consequently was not so violent. Some few booths in Sturbridge-fair were blown down. The course of the storm was from Huntingdon to St. Ives, Erith, between Wisbeach and Downham to Lynn, and so on to Suetsham. Very few trees escaped: the barns that stood the storm, had all their roofs more damaged to the leeward side than to the windward. The storm was succeeded by a profound calm, which lasted about an hour; after which the wind continued pretty high, till 10 o'clock at night.

Concerning the Remains of a Roman Hypocaustum or Sweating-room, discovered under-ground at Lincoln, Anno 1739. By Mr. T. Sympson. N° 461, p. 855.

Some labourers being employed to dig a cellar in an outhouse, fronting the west end of the Minster, and adjoining to the Chequer-gate, they found 2 or 3 stone coffins, which had probably lain there ever since the demolition of the ancient parish church of St. Mary Magdalen, to make way for the foundation of the cathedral, and its appendages; but going lower, about 10 or 11 feet deep, they found some building; and at 13 feet they struck into the corner of a vault. Mr. Sympson took it to be a Roman hypocaustum; he had the dimensions of it taken, as in the plan, pl. 13, fig. 1, and the profile, fig. 2.

A is the præfurnium, stoking-place, entrance or place where the fornacator, the stoker, stood to manage the fire. It is 3 feet 6 inches square, its height not certainly known, because of the rubbish which lay at the bottom.

B, the fornax, furnace, or fire-place, built of brick, and arched over with the same. Its length from E to G, 5 feet 6 inches; its height 3 feet at E, but 4 feet at F, rising gradually; 3 feet 6 inches long from E to F, and 2 feet wide between E and F; 2 feet long from F to G, and but 19 inches wide between F and G.

C, the alveus, or body of the kiln, 21 feet 4 inches long; 8 feet 4 inches broad; and 2 feet 4 inches high. The floor is made of a strong cement composed of lime, sand, brick-dust, &c. which the masons of that country call terrace-mortar. On this floor stand four rows of low pillars, made of brick, 11 in a row; the outside rows round, the two inner rows square; the round ones are about 11 inches diameter, the others 8 inches square; each standing on a brick 11 inches square, as at fig. 4, and 2 inches thick; the shaft 2 feet high, on which lies another brick likewise 2 inches thick, some 17, 18, and others 19 inches square, as at fig. 3, which represents the profile of two square pillars with the square bricks at top and bottom, which make the whole height of the alveus 2 feet 4 inches. The pillars, both round and square, are jointed with mortar, and that very clumsily; the round pillars being composed of 10 courses of semicircular bricks, as at fig. 4, A, laid by pairs; the joint of every course crossing that of the former at right angles, as at fig. 4, C; with so much mortar between, that the two semicircles rather form an oval, and so the pillars look at first sight as if they were wreathed; the square pillars are composed of 13 courses of bricks, as at fig. 4, B; 8 inches square, as at fig. 4, D; these bricks being thinner than those which compose the round pillars.

On the top of these pillars rests the testudo or floor of the sudatorium or sweating-room, fig. 2, HI, which is composed thus: first, there is a floor of large bricks, 23 inches long, and 21 broad, which lie over the square bricks on the tops of the pillars, as at fig. 3, the four corners of each brick reaching to the centres of four adjoining pillars, as at fig. 5, where only one of these larger bricks is represented, as it bears upon 4 of the smaller bricks with their pillars under them. On this course of bricks is a covering of cement 6 inches thick, and on that is set a tessellated pavement; the tessellæ of the corner uncovered, K, in fig. 1 and 2, are of a whitish colour.

L and M, in fig. 1 and 2, are two tubuli or flues, 12 inches wide, and 14 deep, for carrying off the smoke; the bottoms of them are even with the bottom of the alveus, and they are carried on the level about 15 feet, under another room by the side of the hypocaustum, and then it is presumed they turn upwards. The walls of this room were plastered, and the plaster painted red, blue, and other colours, and its floor tessellated white; no figures are discernible in either painting or pavement. This pavement, which is on a level with the testudo of the hypocaustum, is about 13 feet below the present surface of the ground, so deep is old Lindum buried in its ruins!

The workmen, in digging up this pavement, struck into the flue M, 3 feet from the north-east corner of the hypocaustum; and opened it to the very corner K, which showed one of the round pillars, and thus the whole was discovered. In sinking the hole NK, at 5 or 6 feet depth, they came to the wall, which was dug up by pieces with the rubbish, before they came to the pavement. This had been the wall of a room under which the tubuli ran, by the side of, and not over the alveus, but on the east side of it.

Mr. Sympson got a youth to creep in at the opening made at K, and take the dimensions of the several parts, who, the alveus being quite black with smoke, returned like a chimney-sweeper, but could not take the exact measures of the fornax and præfurnium, on account of rubbish he found in them; therefore Mr. Sympson, being desirous to inform himself thoroughly of all the parts of this curious piece of antiquity, with the leave, and at the expence, of the proprietor, caused another hole to be sunk 16 feet deep, and by driving a level OP, fig. 1 and 2, he broke into the middle of the fornax; and, having cleared it of rubbish, found its dimensions as above, and that the bottom of the narrowest part between F and G, was raised 18 inches higher than the bottom of the part between E and F.

The præfurnium was covered over at top with a large flat stone. The fornax, and the two square pillars in the alveus fronting the opening of the fornax, were greatly impaired by the fire, which must have been very violent; some small fragments of wood-coal were thrown out among the rubbish in the bottom of the fornax; whence probably it was heated with wood.

At the conclusion of the account, Mr. Sympson gives the following remark on a passage in the second letter from Mr. Baxter to Dr. Harwood, concerning the hypocausta of the ancients, printed in these Transactions, N° 306. "Mr. Baxter says, the hypocausis was called alveus and fornax; but, with due deference to that learned gentleman, says Mr. Sympson, I humbly apprehend them to have been distinct parts of the whole, which was called hypocausis: the ground of this conjecture is; in the first place, it would hardly be possible to make a fire in that part of this hypocaust, which he calls the alveus; much less to come at it to manage it, being so low, and so crouded with pillars, as to admit only a slender person to crawl among them, and that not without difficulty. In the next place, the floor does not seem designed for it, nor are there any appearances of ashes on it; and further, that the fornax was where he has placed it in this, appears not only from the structure of that part, but from the bricks being much burnt, and pieces of wood-coal being found in it; whereas in the alveus, the bricks are only black with the steam and smoke being drawn through it by the tubuli." He might have added, that only those pillars in the alveus, which faced the mouth of the fornax, had suffered much by the fire, the others not.

That hypocaust, described in N° 306, abovementioned, must have been a much hotter room than this; for, instead of the flues being carried under another room, the walls of the sweating-room itself were hollow or double, and a great number of flues carried up between them all round the room. A curious model of this was to be seen in the museum of the Royal Society.

This hypocaust may serve as a model for malt-kilns, or for drying hops, &c.

Of a Capricorn Beetle, found alive in a Cavity within a sound piece of Wood, and of the Horn of a Fish struck several Inches into the side of a Ship. By C. Mortimer, M. D. Secr. R. S.* N° 461, p. 861.

About Michaelmas 1728, Dr. M. went to Portsmouth with some friends, where having taken a view of his majesty's yard and docks for building ships of war; and satisfied his curiosity in examining several ingenious contrivances used in naval architecture; Mr. Bankley, the clerk of the survey, invited him to his house, where he showed him the insect as represented in fig. 1 and 2, pl. 14. The people of the yard were much alarmed at it, none knowing what to make of it, and all imagining it was venomous. On opening the piece of wood, which was tied together with a packthread, Dr. M. found this animal yet alive, and moving in a large cavity in the middle of the wood, which appeared otherwise sound, having no visible entrance into it. This beetle being turned out on a sheet of paper, crawled about. Mr. Bankley gave the following account of it: " This insect was found August 26, 1728, in splitting a piece of exotic wood into two pieces, cut across the grain 4½ inches thick, taken up in the hold of his majesty's ship Bredah, when in the dock at Portsmouth, after her return from the West Indies: it lived upwards of a month afterwards. The hole in which it was nourished, was 5 inches deep, and 2¼ inches by 1¼ inch broad, in the great piece; 2 inches deep, and 2¼ inches by 1¼ inch broad, in the smaller piece. There was not the least sign of any defect on the outside of the wood, but it appeared very fair and sound; the inside was porous, having a grain like cedar, but in colour not unlike yellow sanders."

On examination, Dr. M. found this insect to be a sort of scarabæus called capricornus from its long horns; which in this were very much crumpled, and partly broken off against the wood in its confinement; its wings were likewise crumpled on the same account. The females of these insects usually lay their eggs in the crevices of the bark of trees; so it is probable, that as soon as this insect was hatched in form of a worm, it gnawed its way through the bark into the wood; and that afterwards the hole it had made in the wood, closed towards the outside; and the worm, still continuing to gnaw deeper, formed the large cavity; and then taking its perfect form of a beetle, remained in that hollow place, where the sap of the tree arising, might have supplied it with nourishment, and even air, since it is known, by various experiments, that air will insinuate itself whereever such fluids, as contain air in them, can penetrate.

* This beetle was a species of the Linnæan genus *cerambyx,* and perhaps the female of *cerambyx cervicornis.*

Dr. M. had seen in the magnificent museum of Sir Hans Sloane, Bart. a piece of wood, sound without, having a cavity within, wherein was found alive a sort of beetle, but he thought of a different species. It came from Jamaica, if he remembered right.

At the same time, that curious gentleman, Mr. Bankley, showed him the horn of a fish* that had penetrated above 8 inches into the timber of a ship, see fig. 3; and gave the following account of it: " His majesty's ship Leopard, having been at the West Indies, and on the coast of Guinea, was ordered by warrant from the Navy Board, dated Aug. 18, 1725, to be cleaned and refitted at Portsmouth for Channel service; pursuant thereto, she was put into the great stone-dock; and, in stripping off her sheathing, the shipwrights found something that was uncommon in her bottom, about 8 feet from her keel, just before the foremast; which they searching into, found the bone or part of the horn of a fish of the figure here described; the outside rough, not unlike seal-skin, and the end, where it was broken off, showed itself like coarse ivory. The fish is supposed to have followed the ship, when under sail, because the sharp end of the horn pointed toward the bow; it penetrated with that swiftness or strength, that it went through the sheathing 1 inch thick, the plank 3 inches thick, and into the timber 4½ inches."

With what prodigious force must this fish have moved? for had it met the ship, the motion of the ship would have assisted the penetration of the horn; but the direction of it pointing from the stern towards the head, shows that the fish struck against the ship, either while at anchor, or that it overtook it, while under sail; in which case the force of the fish must have been still greater; and this was probably the case, because nobody in the ship remembered the shock. Several able workmen on the spot assured Dr. M. that, with a hammer of a quarter of a hundred weight, they could not drive in a pin of iron, of the same form and size, into such sort of wood, and to the same depth, in less than 8 or 9 strokes.

Abstracts of the original Papers communicated to the Royal Society by Sigismond Augustus Frobenius, M. D. concerning his Spiritus Vini Æthereus. Collected by C. Mortimer, M. D. Secr. R. S. N° 461, p. 864.

Dr. Frobenius being dead, and some learned chemists at Paris, in Germany, and in Italy, having endeavoured in various manners, and with different contrivances, to make this ethereal spirit; Dr. M. thought it would be acceptable

* This fish was probably the *scomber gladius*, Bloch. *Xiphias platypteris.* Shaw's General Zoology.

to the curious in England, to give them an abstract of the three papers the Doctor communicated to the Royal Society concerning his Spiritus Vini Æthereus. The first he gave in on Feb. 19, 1729-30, along with what is printed in N° 413 of these Transactions, but was desired by the author not to be published at that time. In this paper he says, you must "take of oil of vitriol, and the highest rectified spirit of wine, equal parts by weight, not by measure; that the oil of vitriol was to be poured by little and little into the spirit of wine, because they will grow hot on mixing; that they should be shaken often, that they may mix thoroughly; then to be digested gently in a glass retort, and a large receiver to be applied and luted on, lest the subtile spirits should fly away: then distil them in an athanor, in gentle digestion, for 3 days; and pour back the distilled liquor, till the liquor in the recipient appears double, or of two sorts. Thus far, he says, Sir Isaac Newton was acquainted with the process."*

He then proceeds almost in the very words of the late Mr. Godfrey [Hanckewitz] as printed in the Transaction quoted above.

He concludes, by telling us, that the first part of the process, till we come to the separation of the two liquors, is mentioned by Caneparius, in his book de Atramentis, first printed at Venice, and afterwards at London, then by the great Mr. Boyle, afterwards by Sir Isaac Newton; that Dr. Stahl, and Professor Hoffman, were the first in Germany who knew the first operation from Kunckel; but neither of them brought it to perfection, or knew the effects of it.† In France M. Homberg undertook an experiment somewhat analogous to this, with sulphur and oil.

The second paper was communicated on the 12th of February 1740-1, in Latin, and contains an ample account of the whole process, with improvements and additions: but as the author in his third paper, given in Feb. 19, 1740-1, in English, says that that is the truest and most advantageous process, Dr. M. presents it as follows, only subjoining the differences and additions in the second paper, by way of note or explication.

Take 4 lb. in weight of the best oil of vitriol, and as much in weight of the best alcohol, or the highest rectified spirit of wine.

1. First, pour the alcohol into a chosen glass retort; then pour in, by little and little, 1 oz. of oil of vitriol; then shake the retort, till the two liquors are thoroughly mixed, when the retort will begin to grow warm; then pour in more of the spirit of vitriol, and shake it again; then the retort will become very

* So long ago as the time of Raymund Lully this process was in use; see his Epist. Accursatoria, p. 327, and Weidenfeld's Secrets of the Adepts, p. 251.—Orig.

† But Baron ****, at Vienna, knew the whole process, and it is said Frobenius learned it of him. —Orig.

hot. Do not pour in the spirit of vitriol too fast, or too much at a time, lest the glass retort, by being heated too suddenly, should burst. You must allow about an hour's time for pouring in the spirit of vitriol, not pouring in above 1 oz. at a time, and always shaking the retort, till the whole quantity of the ponderous mineral spirit is intimately united with the light inflammable vinous spirit.

2. In the next place, examine with your hand the heat of the glass retort, and have a furnace ready, with the sand in the iron pot, heated exactly to the same degree as the retort has acquired by the mixture of the 2 liquors; take out some of the sand, and, having placed the retort in the middle of the iron pot, put in the hot sand again round the retort, and apply a capacious receiver to it; set it into cold water, and wrap it over with double flannel dipped in cold water.

Raise the fire gradually,* that the drops may fall so fast, that you may count 5 or 6 between each, and that beside this quick discharge of the drops, the upper hemisphere of the receiver appear always filled with a white mist or fumes: continue this heat as long as they emit the scent of true marjoram. †

As soon as the smell changes to an acid, suffocating like that of brimstone, take out the fire, and lift the retort out of the sand, and change the receiver; for all that arises afterwards is only a mere gas of brimstone, and of no use. ‡

If you do not use the greatest precaution, the liquors in the retort will run over; the fire must cease, as soon as the æthereal spirits are gone over; for there remains behind an oleum vini, which is extracted by the force of the acid out of the spirits, which will arise, run over, and often cause explosions. §

* Force it from the beginning with a pretty strong fire, that not only the spirit of wine be carried over, but the oil of vitriol along with it; which will most certainly happen, if a middle degree of heat be kept up, between a reverberatory heat, and the other degrees of fire. For the spirit of wine being mixed with the vitriolic acid in equal weight, but by unequal measure; the spirit taking up double the room of the oil, does in a wonderful manner make up the deficiency of the highest degree of heat.—Orig.

† Towards the end, the scent will more resemble that of arrack; continue this heat for about 3 hours, till the scent becomes offensive, and like that of gas sulphuris.—Orig.

‡ At this time you will see black froth arising, which will certainly burst the glasses, and destroy the work, if continued.—Orig.

§ The retort with its receiver being removed, set them by in a cold place; and when all are thoroughly cold, separate the receiver from the retort. There will be two different liquors in the receiver, which pour off through a glass funnel into a glass bottle, which stop up very carefully.

The liquor will be of two sorts; that which swims at top, inflammable, of the nature τȣ̃ Φλογιςȣ̃;

The 2d day, when the glass is cold, infuse the remainder, with half as much alcohol;* and distil again as before, and you will have the same. The 3d day again, with as much, and proceed as at first, it gives it again. Go on as long as you can obtain any of the æthereal spirit, till all turns to a carbo. Then separate it, and alcalize it with spirits of salt ammoniac made without spirits of wine, till all effervescence ceases; and distil it once more e balneo mariæ. So is it ready for experiments.†

There are more products to be got from this process; as, 1st. a balsamic oil. 2dly, a terra foliata tartari of a glittering nature, not fusible, as is the common, prepared with wine-vinegar, and fixed salt, which is of great use in medicine. And, 3dly, a purple earth out of the caput mortuum.

The Doctor proposed at some subsequent meeting, to exhibit 4 other simple

that which sinks to the bottom, like gas sulphuris, a sulphureous acid. Separate the one liquor from the other, by the separating funnel (per tritoreum).—Orig.

* I suppose he means, pour in half as much fresh alcohol, as you did at first, that is 2lb. weight, to the liquor remaining in the retort.—Orig.

† The above-mentioned liquors are to be purified from the strong-smelling sulphur, and superfluous acid, which is performed in the following manner: Pour the liquor, which swam at top, into a phial; drop into it, drop by drop successively, a sufficient quantity of spirit of sal ammoniac, prepared either from salt ammoniac with quick-lime, or from salt ammoniac, and salt of tartar, with common water, and not with spirit of wine: every operator knows the quantity; viz. continue dropping in of such spirit upon the liquor of the phlogiston, till all effervescence ceases, and all the acid taste, with the sulphureous smell, vanishes, being precipitated by the volatile alcali to the bottom.

3dly, Let the whole liquor be rectified in a fresh retort by a most gentle heat of a balneum mariæ, or of a sand bath as hot as that of a person in a fever; and then keep it for chemical uses.

4thly, The inferior liquor is to be purified as well as that which swam on the top; but it must be done by oil of tartar per deliquium, till all ebullition entirely ceases. By evaporating all the humidity of this liquor, you will have a peculiar terra foliata tartari, which, being reduced into a calx, shines in the crucible like oriental pearls, or a peacock's tail. This earth has nothing of a pungent taste, and is to be esteemed as a sheet-anchor in the most ardent fevers.

This earth is of diverse colours, but it is not the common vulgar terra foliata tartari; for it does not flow in the fire, nor has the same taste as the common. The common is made by pouring distilled vinegar on fixed salt of tartar, till an entire saturation is made. The uses of this were formerly known, and I know not by what fate (says the doctor) it is coming into use again now. I thought proper to mention the difference of these preparations, because I am able, from innumerable experiments, to demonstrate a real diversity in them. I shall seem to have dwelt too long on one thing, but I hope I shall be the less blamed, since I design to show, that there are several ethereal liquors besides this above described; for there are not only such (Φλογιστικα,) or combustible fluids, but there are likewise saline fluids, and also some quite insipid, being a mixture of combustibles differently graduated, and extracted by no other heat unless their internal fire. In a word, as many spheres as there are of the elements, so many ethereal, or (if you rather chuse to call them so) aërial liquids, viz. the æther of the earth, of the water, of the air, and of the fire: which, with the leave of the Royal Society, I intend shortly to lay before them.—Orig.

æthereal spirits, but of saline origins, equally subtile with this æthereal spirit of wine.

Soon after this the Doctor died, and never discovered any thing relating to these elementary æthereal liquors; only in a paper he left in Dr. M.'s hands, he gave these few hints of their nature.

There are 4 spheres opened, one of the earth, one of the sea, one of the air, and one of the heaven. Whoever therefore knows how to extract the essences out of vitriol and nitre, whose centres are salt, (and the surface of the earth is salt), possesses

1. The salt of the earth. 2. The salt of the sea is made from the sphere of the sea, and common salt. 3. That of the air is made of sal ammoniac and salts of vegetables. 4. The essence of fire is made soon and easily from a concentrated spirit of wine, or of vegetables. Thus the four genuine elements of nature are obtained.

Of the Fire-ball seen in the Air, and of the Explosion heard, on Dec. 11, 1741. By Lord Beauchamp. N° 461, p. 870.

Being then on the mount in Kensington gardens, at a quarter past 10 o'clock, the sun shining bright, in a serene sky, Lord B. saw towards the south, a ball of fire, of about 8 inches diameter, and somewhat oval, which enlarged to the size of about a yard and a half diameter. It seemed to descend from above, and at the distance of about half a mile from the earth, took its course to the east, and seemed to drop over Westminster. In its course it assumed a tail of 80 yards in length; and before it disappeared, it divided into 2 heads. It left a train of smoke all the way as it went; and from the place where it seemed to drop, there arose a smoke, which continued ascending for 20 minutes; and at length formed into a cloud, which assumed different colours.

Concerning the same Meteor, seen in Sussex. By John Fuller, Esq. F. R. S. N° 461, p. 871.

Between 12 and 1 o'clock, all this part of the country was alarmed with a most terrible clap of thunder, as it is generally imagined. The sound came from the north, where the weather appeared very black and dark all the morning. The sound was double, as if 2 very large cannons had been discharged at the distance of about a second from each other. Most people thought, just at the first hearing, that it was the discharge of cannons, till by the rolling and echoing of the sound afterwards, they were convinced it was not. Some thought powder-mills had been blown up; and they are no bad judges in such kind of blasts, having been more than once alarmed with them, by the powder-mills in the neighbourhood. A countryman, at work in the fields, about 7 miles north of us, saw a flash of lightning before he heard the noise, but Mr. F. cannot answer for the truth of it. It is very easy to imagine, that fancy and fear in a poor countryman on such an uncommon occasion, might conjure up the idea of lightning. If it was thunder and lightning, the effects of it must be very terrible somewhere; for it gave the same report, and shook all the houses just in the same manner, that were about 20 miles distant from one another north and south; which is an argument that it was more general than thunder can possibly be.

Concerning the same Meteor, seen in Kent. By the Rev. William Gostling.

On the 11th Dec. 1741, about one in the afternoon, Mr. G.'s house was violently shaken for some seconds of time, as if several loaded carriages had been driving against his walls; and he heard a noise, which at first the family took for thunder, but of an uncommon sound; but Mr. G. concluded it an earthquake: and, going immediately to the top of the house, found the sky cloudy, but nothing like a thunder cloud in view; only there was a shower of rain from the eastward presently after, and the coldest that he has felt. But since he finds it was attended (and he supposes caused) by a ball of fire, which passed with great rapidity over the country, from westward to eastward. It began with two great blows, like the reports of a cannon, which the jumbling of the sashes prevented him distinguishing; and then rolled away till it was heard no more. The appearance, it seems, was as that of a very large shooting-star; and it left a train of light, which soon disappeared, it being noon day. He met a pilot 2 days after, coming from Deal, who told him he saw no fire-ball, but heard the noise, and that it made the ship shake he was in, going from Gravesend to the Nore.

END OF THE FORTY-FIRST VOLUME OF THE ORIGINAL.

Concerning the Fire-ball seen in the Air, and a great Explosion heard, Dec. 11, 1741. *By Mr. Christopher Mason.* N° 462, p. 1. *Vol. XLII. Anno* 1742.

On that day, at Bucksteep, Sussex, about a quarter before one o'clock in the afternoon, Mr. M. observed a very dark uncommon appearance in the north, and at the same time the sun shone bright at his back; when, on a sudden, there was an explosion as violent as the report of a mortar-piece, attended with a rumbling echo, which ran eastward; and he judges it came from about 40° elevation. Several people saw a ball of fire, which ran nearly eastward, leaving a train of light, which continued some time. The ball of fire was seen, and the report heard very loud, at Sompting, beyond Shoreham.

A Letter from Edward Milward, M.D. to Martin Folkes, Esq. P.R.S. concerning an Antidote to the Indian Poison in the West-Indies. N° 462, p. 2.

Dr. M. here gives an account of an antidote against the Indian or Negro poison, which was first purchased from a famous Negro poisoner, at a great expence, by one who styles himself, Isaiah Burgess, Doctor of Physic; and the secret devolved to Dr. M., by means of a MS. of the Doctor's, which, among others, he had procured, for his History of the Physical and Chirurgical Writers of this Kingdom. The author intended this little tract, which contains observations on the most considerable distempers in America, should be made public; he wrote it at the request of his friends, when an expedition was designed into America; and particularly declares, that he purposed the divulging of this specific antidote, that such as should go to the West-Indies, among the Spaniards, might meet with a remedy in case of necessity. What prevented the Doctor from executing this laudable design, was not known; but as it was plainly his intention it should be made public, and as the knowledge of such a remedy may be of the greatest benefit to mankind, it is here communicated to the Royal Society.

"The Negroes," says he, "use a poison of a strange and extraordinary nature. The dose is very small, and it has no ill taste; so that mixed with meat or drink, it is not perceivable. It causes divers symptoms, and the effect is various, according as the dose is large or small. It kills sometimes in a very few hours, sometimes in some months, and at others in some years. The symptoms are according to the quantity given: if great, it causes evacuations upwards and downwards; of excrements first, then of humours, and lastly of blood, with fainting-fits, and sweatings. Death follows in 6 or 7 hours. The Negroes turn white.

"If the dose is but small, the sick loses his appetite, feels pains in his head, arms and limbs, a weariness all over, soreness in his breast, and difficulty of breathing, so that he appears as being in a consumption, and at last dies languishing.

"All remedies yet publicly known, are of no force nor virtue against this poison; and the patient certainly dies. Nay, he questions whether the best cordial remedies can put the least stop to the efficacy of its venom, or retard death, and put it off, longer than the intention of the cunning poisoner had fixed it, in proportioning the dose.

"He adds, that the Spaniards have knowledge of this very poison, and was satisfied that he had seen several Bucaneers die of it, given them by Spanish women. He was also persuaded, that the same poison is used in Spain and Italy.

"This poison has but one specific antidote yet known; the knowledge of which cost him very dear: and it was with much difficulty he could persuade a famous negro poisoner to part with his secret.

"The antidote is, the root of the sensible weed, as it is commonly called, or herba sensitiva. It grows like a shrub, has no prickles, blossoms yellow, and bears little cods, full of small black pretty seeds, of which the women make necklaces and bracelets. Take none of the root but what is in the ground; wash it well, and split it in two. Take a good handful of these roots so split, and steep them in 3 quarts of good clear water in an earthen glazed pot, having a cover. Use but a moderate fire, that it may boil very gently. The decoction has no ill taste, and you may either give it so, or add sugar, as you shall think best. Give to the patient a good glass of this decoction, as warm as he can drink it; an hour after give another, and so for some time, as you shall think it necessary to make a perfect cure. There is no danger of giving too much; it can do no harm at all. Several people have taken this decoction, though they have not been poisoned, thinking it would do them good in other distempers; so that one who any ways suspects he has had some of that poison given him, may drink it very safely, and in what quantity he pleases. The rest of the plant is to be rejected as bad and noxious."

The Doctor enforces his observations by remarking, that he had been a practitioner in those parts for above 25 years. Many Negroes, he says were wonderfully preserved and cured by taking this antidote, though, for brevity's sake, he gives but one instance; which is, "of a strong negro man, about 30 years of age, and in perfect health, who being one night at a plantation 4 miles distant from that where he lived, was invited to drink a dram of rum, by another negro, who mixed poison with it. The fellow drank it up, perceiving nothing to be in it; but as he was taking leave, on the other's bidding him farewel, and telling him he should never see him again, he suspected he was poisoned; and putting his finger in his mouth, vomited up great part of the poison, though there remained enough of it to cause continual evacuations in him upwards and downwards; of excrements first, then of humours, and lastly of blood.

As he was coming home, he fainted away several times, and calling at length to some neighbour's negro houses, was brought home extremely altered; turned white, and was, as it was thought, expiring. The root was immediately sent for, and the decoction made, and given him in quantity. He continued taking it for 3 or 4 days, and on the 5th went to work along with the rest of the negroes."

That the sensible plant is endowed with the property of resisting poison has been formerly taken notice of. For Sir Hans Sloane, the late worthy president of the R. S. whose writings will always remain an honour to his country, has observed from Piso, that the root of this shrub is an antidote against the shrub itself, which is very poisonous, and kills by degrees, making the unhappy sufferers cachectical, short-winded, and melancholy, till they die. (Nat. Hist. of Jamaica, Vol. II. p. 57.) This greatly corroborates what our author has advanced; and it is observable, that he likewise directs all parts of the plant, except that part of the root which is in the ground, to be rejected, as bad and noxious: though whether this be exactly the same plant with what our author mentions, he dares not determine; as Sir Hans Sloane enquires whether it be not the Æschynomene, seu Mimosa arborescens Americana, &c. flore albo; whereas Dr. Burgess expressly says, that its flowers are yellow: though this may, possibly, be a mistake in him.

Dr. M. is sensible it may be objected, that the negro poison is of various kinds; and that therefore, though this remedy may be so extraordinary a specific in some cases, it may be unavailable in others. That the negroes may have the knowledge of different sorts of poison, he denies not; but it would appear, from the universality of the effects of this medicine, (as the Doctor affirms many have been wonderfully cured and preserved by it, and does not mention a single instance of its miscarriage,) as though the negroes in the West-Indies used but one kind of poison, or, if different, yet such as comes within the power of this remedy. Besides, as we cannot be assured but by the consequence, whether the poison be of that sort, as to be within the reach of this remedy, or not, he thinks there is all the reason in the world it should be administered under any suspicion of the Indian poison: especially, as the Doctor assures us of its great innocence; and he believes every one will readily agree, that it is no small recommendation of a medicine, that let what will become of its good effects, it can do no harm.

Of several Stones found in Bags formed by a Protrusion of the Coats of the Bladder, as appeared on opening the Body of Mr. Gardiner. By Mr. Edward Nourse, F.R.S. N° 462, p. 11.

Mr. Gardiner was, on the 5th of March 1739, before the trustees appointed by the Parliament to inquire into the efficacy of Mrs. Stephens's medicines, produced as an instance, where they had been effectual in dissolving the stone in the bladder.

He was searched by Mr. N. on Saturday the 30th of December 1738, who felt a stone the moment his instrument was introduced; which was likewise felt by Mr. Wall, his apothecary, then present. Tuesday following Mr. G. began to take Mrs. Stephens's medicines, and continued them 8 months.

On the 30th of November 1739. Mr. N. saw him at Child's Coffee-house, when he told him he was quite free from his usual disorders. He there searched him again, in the presence of several physicians and surgeons, who likewise felt for the stone, but none could be found.

Mr. Gardiner dying on Saturday the 2d of January 1741-2, the next morning in the presence of Mr. St. Hill and Mr. Wall, Mr. N. opened his bladder, and therein observed 6 preternatural apertures, of different sizes, the largest, capable of admitting the top of his finger. Each of these openings led to a separate bag, formed by an enlargement of the internal membrane of the bladder, protruded between the fibres of its muscular coat.

These bags were to be seen on the back part of the bladder, a little above the vesiculæ seminales; and when viewed on the outside, seemed to be but two; though they were in number equal to the openings within, already mentioned; and divided from each other by the duplicature of the internal membrane, which formed a septum between each of them.

In these sacculi, or bags, were contained 9 stones; the largest about the size of a small nutmeg; and with what facility some of them moved out of, and returned into, the sacculi, the following circumstance will clearly evince.

When Mr. N. had opened the abdomen, Mr. St. Hill, handling the bladder, brought 2 of these stones up to its fundus, where they were felt by Mr. Wall and himself. They then examined the kidneys: the right contained a little matter, otherwise it was as it should be: but of the left, two-thirds were wasted; its pelvis was contracted in proportion, and the ureter almost impervious. Rehandling the bladder, neither of them could feel any stone; Mr. N. therefore laid it open, and they found them all in the sacculi. The stones that are in one of these sacculi, had been so much enlarged since their lodgment, that without force and laceration they could not be got out.

Some further Observations concerning Electricity. By J. T. Desaguliers, LL. D. F. R. S. N° 462, p. 14.

Electrics per se, which Dr. D. has heretofore defined, bodies in which an electrical virtue may be raised by some action on them, such as rubbing, patting, warming, &c. are reduced to a non-electric state by being in contact with non-electric bodies, especially water, which is the greatest non-electric, even when it becomes vapour.

A non-electric, which though it cannot be made electrical by any action upon it, receives electricity from an excited electrical body; but does not retain it while it touches any other non-electrical body. An electric per se, when it is become non-electrical, differs from the non-electric per se in this; that it may be so restored to electricity, by applying a rubbed tube to it, as to repel all other electrics of the same kind of electricity as the tube; till it meets with some non-electric body, which brings it back to non-electricity, or at least to such a languid state, that its electricity is scarcely perceptible.

The electricity may be also restored in the same manner by wax, &c. But in both cases, an electric body, in a languid state, cannot be restored to electricity while it adheres to a non-electric per se.

Experiments to illustrate these assertions.—From an horizontal cat-gut, which is an electric per se, as most animal substances are, he suspended two feathers, the one by a thread, and the other by a silk, about 2 feet long each: then applying the rubbed tube to the feather hanging by the silk, which silk is an electric per se, the feather came to the tube, and stuck to it, as all non-electric bodies do, till it was so impregnated with the virtue from the tube, as to come out of its languid state, and become strongly electrical; which appeared by its flying from the tube, and being repelled as often as the tube was brought near it; till it had touched some non-electric body, or was left so long as to imbibe the moist particles floating in the air; by which it became non-electric, and was again attracted by the tube.

On applying the tube to the other feather that hung by the thread, which, like most vegetable substances, is generally non-electric per se, the feather was constantly attracted, and never repelled; because the virtue communicated from the tube to the feather, lost itself along the thread; which would have been retained by the feather, if it had floated in dry air, or been suspended by an electric body.

These properties of electric bodies show the reason of that phenomenon, by which a rubbed tube, after having attracted a feather, repels and chases it about a room in the air, and does not attract it a second time, till the feather has touched some other body; and also shows the reason why the experiment does not succeed in moist weather.

Pure air, that is dry, may be ranked among the electrics per se, because it repels all bodies in a state of electricity, whether they have been excited to it by wax or glass; that is, by either of the two sorts of electricity.

Watery vapours, that float in the air, are non-electric; from which mixture the air becomes more languid in its electricity, when most impregnated with vapours; so that dry air is more electric than moist; but cold air in frosty weather, when vapours rise least of all, is more electric than air in summer, when the heat raises vapours; which renders that state of the air more fit for making electrical experiments.

The rubbed tube retains its electricity a long time, because it repels, and is repelled by, the dry air; and the feather, which has been attracted by the tube, after adhering to it a while, is raised out of its languid state to a strong electricity; by which it flies from the tube, repels and is repelled by the air, where meeting with very few vapours, it retains its electricity a long time; till touching a non-electric, that is brought to it, it loses its own electricity by communicating it, becomes a non-electric, and is re-attracted by the tube, to which adhering some time, it receives so much virtue from the tube, as to be restored to its electricity, and again repelled.

In a moist state of the air, the feather, after it has been made electrical, and repelled by the tube, it attracts to it the moist vapours floating in the air; by which losing its electricity, it is attracted by the tube, without touching any other body first.

Sometimes, when the feather flies off from one part of the tube, it immediately returns to another part, generally the top of the tube, because the top of the tube has attracted the moist vapours, and is become a non-electric, and therefore attracts the feather; which having become electric, flew off from the electric part of the tube.

That this is true, appears from an experiment to be made in dry weather. At that time, when every part of the tube repels the feather, strongly, after having attracted it, if you wet 2 or 3 inches of the upper end of the tube, the feather will come to that end. Wetting the silk by which the feather hangs from the cat-gut, the feather will be always attracted, and not repelled.

When the silk is dry, the feather once made electrical, so as to be repelled by the tube, retained that virtue above 2 hours in frosty weather; but in moist weather lost it in half a minute.

An Observation of the Eclipse of the Moon, Dec. 21, 1740, *at the Island of St. Catharine on the Coast of Brasil. By the Hon. Captain Ed. Legge, F.R.S.* N° 462, p. 18.

Capt. Legge observed that this eclipse of the moon began very nearly at 7^h 5^m; but the horizon being hazy, he could not observe exactly the beginning: however, it ended exactly to a moment at 9^h 50^m.

This eclipse was observed at the island of St. Catharine, on the coasts of Brasil; and the Captain places the island in latitude 27° 30′. Mr. Gael Morris calculated the said eclipse; and the middle of it, apparent time, at Greenwich, was

wich, was	11^h	44^m	50^s
By the Captain's observation, supposing the beginning exact	8	27	30
Difference of meridian	3	17	20
	= 49°	20′	
The end of it, by calculation at Greenwich	13	06	57
—— by Capt. Legge's observation	9	50	00
Difference of meridian	3	16	57
	= 49°	14′	

Capt. Legge observes, that in attempting to pass Cape Horn, they thought themselves to have been more to the westward than they really were: by which mistake, turning too soon to the north, they fell in with high lands, and met with those misfortunes, which, if they had kept out more at sea, might probably have been avoided. By comparing the longitude at St. Catharine's as above settled, with Senex's maps, the coasts appear to be placed about 6 degrees too much eastward; and if the other parts of America about the Cape are laid down as faultily in the charts, this error will probably account for their misfortunes.

An Observation of extraordinary Warmth of the Air in January 1741-2. *By the Rev. Mr. H. Miles. Dated Tooting, Surrey, Jan.* 20, 1741-2. N° 462, p. 20.

The mercurial thermometer abroad, was last night, at 10 o'clock, 20° above the freezing point; which is higher than it was 16 mornings of the 31 in May last, and higher than in any morning in April, one excepted.

The Description and Uses of the Steel-yard Balance Swing, invented and made by Mr. Timothy Sheldrake. N° 462, p. 20.

Where crookedness is caused by bad accidents, as falls, breaking of bones, or any such causes, attended with neglect; there it is to be feared no help can be given. But where a deformity of body is owing to some defect of health, ill habit of body, or some internal cause, it may be in the power of art and care to prevent growing worse; or with good care and endeavours, to recover entirely. For which end Mr. S. communicates this steel-yard swing, for restoring such crooked persons, whose bones are tender, and capable of having their form a little altered.

The body, being composed of bones with joints, covered with muscles, &c. for moving the body, as necessity requires, so if any of these muscles that are of use for bending the body forward, backward, downward, or raising it upward, or for turning part of the body to the right or left side, have by illness, or want of proper nourishment flowing so freely to one side as the other, or by a careless way of sitting or lying, been contracted on one side of the body, by which the bones are braced closer together than nature intended; in this case, the hip generally rises, the shoulder on the same side falls lower: the great support of the body, the vertebræ of the back, are altered from their natural uprightness to a curve, and the other side extended to too great a length: thus the viscera are pressed too close on the contracted side, and probably hindered from performing their due office; while on the contrary side, which is extended beyond its due bounds, there is too much room for them, that may give too large a growth to them, or render them too lax and weak. From this united ill state of the viscera, it is possible that crooked persons are generally unhealthy.

For removing this distorted form, and recovering a better, this steel-yard swing is proposed, as a mechanical method, for stretching the contracted side, and giving liberty to the too-much extended side to contract; that the sides may thus be brought to their original and regular form, by suspending the crooked person with cords properly covered for ease, and put under each arm, and then placed at equal distances from the centre of the beam. The gravity of the body will, in great probability, immediately affect the contracted side of the body, so as to put the muscles a little upon the stretch; and if the cord under the arm on the longest side of the body be removed farther from the centre, the longest side will become a weight continually increasing, as the point of suspension is removed farther from the point of motion; by which means the shortest side must be lengthened. Thus the vertebræ of the back will be gradually brought from their irregular form, to a perpendicular; and the head, that probably leaned too much to one side, will rise upright.

The child, or crooked person, may hang suspended much longer on this swing, than by the head in one of the semicircular swings, which cannot extend the contracted side in such manner as this can. It may be necessary to keep the arms down, by a small bandage round the body and arms a little above the elbow.

By this method of swinging a child, its own weight must necessarily stretch the contracted muscles, &c. that draw the shoulder and hip too close together, and give liberty to the ribs to extend themselves to a greater distance from each other; and at that very moment of time, the too much extended side, by the weight of the body, will be pressed closer together; and by daily increasing the time that the person is on the swing, the desired effect may be produced, an agreeable form of body recovered, and a healthy constitution restored, to the satisfaction of the parents, and great benefit of the once crooked person.

ABC, fig. 4, pl. 14, represents the steel-yard balance swing; D, one of the square iron loops to which the cords are to be fixed, and which loops, one on each arm of the balance, are moveable from one notch to another; E, a weight to be hung upon the arm C at F, to add to the weight of the too much extended side, as occasion requires.

Concerning a golden Torques found in England. By Sir Tho. Mostyn, Bart. N° 462, p. 24.

This torques is a wreath of gold, weighing nearly 9 ounces. It seems to be without alloy, being pliable. It being joined here with the pharetra, and being very proper for carrying a quiver, makes it probable that the Gauls, from whom the Romans took it, used it for that purpose; but among the latter it seems to have been worn as an ornament, rather than a thing of use. There are several passages in the historians, which mention its being given as a reward for military service. It is sometimes described as a chain consisting of several links; but this is all one piece, without any link or joints, and takes its flexibility from the pureness of the metal.

Of the Fire-ball seen Dec. 11, 1741. By Mr. Benj. Cooke, F.R.S. Dated Newport, in the Isle of Wight, Jan. 25, 1741-2. N° 462, p. 25.

A gentleman was on a hill about 3 miles west of that town, and had a very advantageous view of the fire-ball. He says, that at that time the brightness of the sun was a little obscured by the interposition of some thin clouds, when he saw it pass by to the eastward, at about the distance of a quarter of a mile, and apparent height of 30 feet above the level of the place were he stood. Its colour was that of a burning coal; its figure a cone, whose length might be 8 feet, and diameter at the base 18 inches. From about its apex, which was its hinder part, issued several bright streams sparkling with fiery drops, to the length of about 4 or 5 feet, something after this manner . Its motion was nearly parallel to the plane of the horizon, and its direction about from south-west by south to north-east by north, without any noise, wind, or motion of the earth attending it. The time of its appearance did not happen to be taken notice of with the desired exactness; but by the best observation we can make, must be about a quarter before 1 o'clock at noon.—There were a few others who saw it, to whom it appeared different in shape, according to the point it was seen from; and perhaps its shape might change as it became nearer consuming, and only its head, in the form of a bell, remain at last.

An Account, by Mr. John Eames, F. R. S. of a Book intitled, Jacobi Theodori Klein Historiæ Piscium Naturalis promovendæ Missus primus Gedani, 1740, 4to. Or, The first Number of An Essay towards promoting the Natural History of Fishes. By Mr. Klein, Secretary of Dantzic, and F. R. S. N° 462, p. 27.

Though the natural history of animals has been much improved, since several of the worthy Members of the Royal Society have taken it under their consideration; yet there still remain some things to be known, in order to render it full and complete. As particularly, concerning the hearing of fishes, it is remarked, that in no fishes beside the cetaceous kind, have hitherto been found any auditory passages, or ear-holes; and whether all fish hear or not, is a question not yet fully determined, notwithstanding the experiments alleged to prove the affirmative. It is with this view, and in order to set this matter in a clearer light, that the ingenious author has obliged the world with the book before us. It consists of a dedication addressed to this Honourable Society, a preface, an essay, and a double appendix.

Having considered the auditory organs, with the seat of them, in the cetaceous, cartilaginous, and spinose kinds of fishes, it appears, that these lapilli or ossicula differ from each other both in structure and substance; for in cetaceous fishes, whose skeletons are truly bony, and which, in certain respects, may be compared to truly lignous trees, both the os petrosum, and auditory organs, are in these, as in other animals, perfectly osseous or bony: whereas the cartilaginous fish, whose skeletons are elastic and cartilaginous, they may be compared to the keratophyta species of sea plants; and these fish, instead of an os petrosum, have something analogous, but cartilaginous; and the auditory bones are of a tartareous kind of friable and easily macerable substance.*

A Journal of the Shocks of Earthquakes felt near Newbury in New-England, from the Year 1727, *to the Year* 1741. *By the Rev. Mr. Matthias Plant.* N° 462, p. 33.

Oct. 29, 1727, about 40 minutes past 10 in the evening, there was heard a great rumbling noise; and before that, the bricks on the hearth rose up about three quarters of a foot, and seemed to fall down and roll the other way, which was in half a minute attended with the said noise or burst. The tops of the chimneys and stone-fences were thrown down; and in some places the earth opened, and threw out some hundred loads of earth, of a different colour from that near the surface, something darker than the white marl in England; and in many places, opened dry land into good springs, which remain to this day; and dried up springs, which never came again. It continued roaring, bursting, and shocking our houses all that night. Though the first was much the loudest and most terrible, yet 8 more, that came that night, were loud, and roared like a cannon at a distance. It continued roaring and bursting 12 times in a day and night, till Thursday in the said week, and then was not so frequent; but on Friday in the evening, and about midnight, and about break of day on Saturday, 3 very loud roarings: we had the roaring noise on Saturday, Sunday, Monday, about 10 in the morning, though much abated in the noise.

Nov. 7, about 11, it roared very loud, and gave our houses a great shock; and continued shocking from 3 times to 6 every day and night, till the 12th of November, when it was heard twice in one hour in the afternoon.

Nov. 13, two hours before day-break, the roaring was loud, and shook the houses; and so for several days after.

January 3, 1727-8, about 9 at night, an easy shock.

January 6, there were 5 shocks, attended with the roaring.

January 24, it roared exceedingly loud.

January 28, 29, 30, several more shocks, with the roaring.

February 21, 29, the same.

March 17, 19, also the same.

April 28, 1728, a small noise.

May 12, a long and loud roaring, that shook the houses.

May 17, 22, 24, several more shocks.

* For further information respecting the organ of hearing in fishes, the reader is referred to the writings of Camper, Comparetti, (de Aure interna comparata. Patav. 1789.) and Scarpa; also to a paper of Mr. J. Hunter's inserted in the 72d vol. of the Phil. Trans. and to Dr. Monro's work on the Physiology of Fishes.

June 6, 8, and 11, the same.

July 3, and 23, very loud and long, and shook the houses.—Also March 19, 1729.—Sept. 8, and 29, the same. And the same in the months of October and November.

Similar shocks and noises were also in most of the months of the following years 1730, 1731, 1732, 1733, 1734, 1736, 1737, 1739, 1740, 1741.

An Account of Mr. Sutton's Invention and Method of Changing the Air in the Hold, and other close Parts of a Ship. By Richard Mead, M. D., F. R.S. N° 462, p. 42.

It is found by daily experience, that air shut up and confined in a close place, without a succession and fresh supply of it, becomes unwholesome, and unfit for the use of life. This is more sensibly so, when any stagnating water is pent up with it. But it becomes still worse, when such air is used in respiration, that is, becomes moister and hotter, by passing and repassing through the lungs.

These bad effects, in different degrees, according to the different manner in which air is inclosed, are observed in many cases; particularly in deep wells and caverns of the earth, in prisons or close houses, where people are shut up with heat and nastiness: but most of all in large ships, in which, with the stench of water in the hold, many men being crouded up in close quarters, all the mentioned circumstances concur in producing greater mischief than would follow from any of them singly.

The reason of these bad effects, Dr. Mead thinks, is partly by the air losing its elasticity in such circumstances, and partly by being impregnated with noxious effluvia,* either from unwholesome substances of any kind, or from the infectious breath of diseased bodies; hence it will become quite poisonous and deadly. It is proposed at present to find out a remedy for this evil in ships only: but by making alterations according as particular places require, the same may be applied to any houses or parts of them, as prisons, the sick wards in hospitals, &c.

Now it is a natural consequent of the elasticity of the air, that when it is rarefied in any part, (which is most effectually done by heat) the neighbouring air will rush that way, till this part is brought to be of an equal density and elasticity with itself; and this again will be followed by the air next to it: so

* Air under the circumstances abovementioned becomes unfit for being respired, not only in consequence of being loaded with noxious effluvia, [and with carbonic acid gas] but also in consequence of the abstraction of a portion of its oxygen, which is absorbed by the blood during the respiratory action.

that, if a conveyance for air be laid from the hold or well of the ship, and a rarefaction of the air therein be made; the foul air from this place will run or be drawn out that way, and fresh air from the adjacent parts will succeed in its room.

It is on these principles that the following scheme was offered to the Lords of the Admiralty, and Commissioners of the Navy. And the means to be employed are shortly the following: that whereas in every ship, of any bulk, there is already provided a copper or boiling-place, proportionable to the size of the vessel, it is proposed to clear the bad air by means of the fire used under the said coppers or boiling places, for the necessary uses of the ship.—Under every such copper or boiler, there are 2 holes, separated by a grate; the first of which is for the fire, and the other for the ashes falling from it; and there is also a flue from the fire-place upwards, by which the smoke of the fire is discharged at some convenient place of the ship. Now the fire once lighted in these fire-places, is only preserved by the constant draught of air through these 2 holes and flue; but when the said 2 holes are closely stopped up, the fire, though burning ever so briskly before, is immediately put out.

But if, after the shutting up the abovementioned holes, another hole be opened, communicating with any other room or airy place, and with the fire; it is clear, that the fire must again be raised and burn as before; there being a like draught of air through the same, as there was before the stopping up of the first holes: this case differing only from the former in this, that the air feeding the fire will now be supplied from another place.

It is therefore proposed, in order to clear the holds of ships of the bad air, that the 2 holes abovementioned, that is, the fire-place and ash-place, be both closed up with substantial and tight iron doors; and that a copper or leaden pipe, of sufficient size, be laid from the hold into the ash-place, for the draught of air to come in that way to feed the fire. And thus it seems plain that there will be from the hold a constant discharge of the contained air; and consequently, that the air so discharged must be as constantly supplied by fresh air down the hatches, or such other communications as are open into the hold; by which the same must be continually freshened, and its air rendered more wholesome, and fit for respiration.

And if into this principal pipe so laid into the hold, other pipes are let in, communicating respectively either with the well or lower decks, it must follow, that part of the air consumed in feeding the fire, must be respectively drawn out of all such places, to which the communication shall be so made.

A Representation of the Parhelia seen in Kent, Dec. 19, 1741, communicated by the Rev. Mr. H. Miles. N° 462, p. 46.

This paper contains no additional observations on parhelia; nor does the representation or figure exhibit any new appearance.

Experiments, by Way of Analysis, on the Water of the Dead Sea; also on the Hot Spring near Tiberiades; and on the Hammam Pharoan Water. By Charles Perry, M. D. made on his Journey through the Holy Land, &c N° 462, p. 48.

I. *On the Water of Asphaltites, commonly called the Dead Sea.*

Exper. 1.—On steeping or infusing some scrapings of galls in it, after standing a long time, it turned of a bright purple colour.

Exper. 2.—On the instillation of ol. tartari per deliq. it immediately became troubled or muddy, and seemed as if goblets of fat were fluctuating in it. This unctuous matter, on long standing in repose, came gradually into closer contact, and at last subsided.

Exper. 3.—On the instillation of sp. of vitriol, it deposited a milk-white greasy sediment; which, after 12 hours repose, occupied a 5th part of the vehicle or liquor.

Exper. 4.—Being mixed with a solution of sacch. saturni, it let fall a small quantity of a greyish powder.

Exper. 5.—Being severally and separately mixed with solution of sublimate, with sp. sal. ammoniac, and with sugar of violets; it neither fermented, deposited any sediment, became turbid, nor changed colour; except only from the sugar of violets, which turned it of a dark green.

Observations.—This water is highly saturated with salt, insomuch that any measure of it preponderates fresh water under equal surfaces, in the ratio of 5 to 4. It has also a wonderful acrity, insomuch that being held in the mouth for a short time, it constringes it as alum does.

Dr. P. could not, from the above experiments, and the appearances which resulted from them, conclude, that this water is impregnated with any thing more than mere salt,* which is of a very acrid, alkaline nature; and something else, which may be of a compound nature, partly sulphureous, and parly bituminous. But, it may be presumed, that it neither partakes of steel, alum, nor vitriol, nor yet of a pure, genuine sulphur: and consequently it can afford

* The water of the Dead Sea is remarkably salt and bitter. Besides common salt (muriate of soda) it contains a very large proportion of muriate of lime and muriate of magnesia, to which last salts its bitterness is owing. See Memoirs of the French Academy of Sciences for 1778.

no other, nor better effects, to such as may bathe in it, than other sea-water; except only, that its greater degree of salt, and superior weight, may somewhat heighten the same effects.

II. *On the hot Spring Water near Tiberiades.*

Exper. 1.—Oil of tartar. per deliq. ʒss being mixed with ℥iss of the water, it became troubled and muddy; and after standing 12 hours in repose, $\frac{3}{4}$ths of the whole, from the bottom upwards, appeared like white wool: but this woolly water, being separated by filtration, and left to dry, seemed no other than a yellowish ochre.

Exper. 2.—Dr. P. mixed ʒss sp. vitriol with ℥iss of the water, and, after 12 hours standing still, he found a large sediment of a white unctuous matter.

Exper. 3.—Solution of sublimate ʒss being mixed with ℥iss of the water, it became turbid and yellowish, and yielded an earthy sediment in small quantity; whence he concluded it contains a sal murale.

Exper. 4.—One ounce and half of the water, mixed with ʒss of a solution of sacch. saturni, deposited a greyish sediment of a lateritious matter, in small quantity.

Exper. 5.—One ounce and half of the water, mixed with ʒss sp. sal. ammoniac, turned turbid, of a colour between green and blue; and after 12 hours repose, yielded a woolly sediment of 4 digits deep.

Exper. 6.—One ounce and half of the water, mixed with ʒss sacch. violar. became troubled, and of a dark-yellowish colour.

Exper. 7.—One ounce and half mixed with ʒss of scrapings of galls, became of a fine violet colour; but when shaken, was as deep as ink.

Observations.—This water then appears to contain a good deal of a gross fixed vitriol, some alum, and a mural salt of a limy quality.* It is too salt and nauseous for internal use; but by bathing in it, must be good for all cutaneous distempers, and especially for the scurvy and leprosy: for it will powerfully deterge, scour, and clean the excretory pores; and it may, by its weight and stimulus, restore them to their natural state, strength, and elasticity. It may, by the same means, restore the lost or impaired tone of the solids in general: in consequence of which, it may thin the blood, help its circulation, and promote the natural digestions and secretions; and thus, finally, it may be useful in rheumatisms, dropsies, jaundices, and nephritic diseases.

III. *On the Hammam Pharoan Water, near Corromondel, on the Way to Mount Sinai.*

Exper. 1.—This water being mixed with the scrapings of galls, manifested

* These conclusions respecting the composition of this mineral water are extremely vague and unsatisfactory. The same remark will apply to this author's observations on the Hamman Pharoan water.

no sensible change at first; but after long standing it became somewhat greenish.

Exper. 2.—On the instillation of sp. sal. ammoniac, it became turbid; and on standing some time in repose, deposited a dark-greyish powder, in small quantity.

Exper. 3.—Four ounces of the water, being mixed with ʒiss sacc. violar. manifested no change, except what would necessarily result from the tincture of violets.

Exper. 4.—Being mixed with a solution of sacch. saturni, it became immediately very turbid; but on standing some time in repose, it deposited a large dark-brown sediment, leaving the vehicle troubled and whitish.

Exper. 5.—On mixing a solution of sublimate with it, it became immediately yellow; but, after standing at rest, it deposited a woolly unctuous matter, in small quantity.

Exper. 6.—Being mixed with ol. tartari per deliq. it became of a chyly colour and substance, or of a turbid pearly colour.

Exper. 7.—Being mixed with sp. of vitriol, it manifested no change, either of colour or transparency.

Observation. — He concludes from the phenomena which appeared on analyzation, that this water is impregnated with a good deal of a gross earthy sulphur, a neutral salt, a small quantity of alum, but no proportion of vitriol.

This cannot be used inwardly, it being nauseous beyond expression: it smells somewhat like rotten eggs, but much worse. But, used by way of bath, it may cleanse the skin of all foulnesses, purge and deterge the cutaneous glands from all foul noxious humours: it may reinforce the natural heat and vigour, where they are decayed, and may restore the impaired digestions: and hence, finally, it may promote virility in men, and fecundity in women. It may likewise be useful in the gout; as also in epilepsies, and other diseases of the nervous class.

An Account of the Case of William Payne, with what appeared on examining his Kidneys and Bladder, when his Body was opened. By Mr. George Bell, Surgeon. N° 462, p. 54.

William Payne, aged about 71, had been afflicted with the stone in his bladder, and other calculus complaints, for several years; he had taken Mrs. Stephens's medicines for 15 months. He was subject also to a scrotal rupture on the left side, from which however he suffered no great inconveniency, unless on neglect of his truss, which he had been directed to wear; and even then, if the intestines came down, he used to return them with ease.

About the beginning of January last, he was attacked with a severe fit of the stone, attended, on every attempt to make water, with a strong tenesmus, that forced into the scrotum a considerable quantity of the intestines, which exceeded his skill to reduce. Mr. B. found the tumour large and unequal, but without much tension or inflammation; his pulse low, with clammy sweats; he complained of violent pains in his back, propagated through the whole length of the ureters, accompanied with nausea and vomitings; he felt exquisite pain about the neck of his bladder and glans, with an unusual weight in perinæo; he had frequent inclinations to make water, but seldom made above a spoonful at once, and that drop by drop, with much pain, and sudden stoppings; the urine was extremely fetid, sometimes mixed with purulent matter, at others tinged of a coffee colour.

He had received, just before, a clyster, which produced 2 stools, and encouraged him to hope might facilitate the reduction of the rupture. He attempted it by all necessary means possible, but without success; for though the largest part receded and gave way, yet a considerable portion remained, which he could not possibly return. He therefore concluded, as the intestines performed their office, and were free from tension, inflammation, &c. that the parts adhered: so left him, with directions for a bag-truss to support them.

January the 22d, being informed of his death, he applied for leave to open him, which was granted. In examining the contents of the abdomen, he found the left kidney quite wasted, scarcely any thing remaining except the coats, and those filled with blood and purulent matter; the ureter very much enlarged above its natural capacity, and full of the same. The right kidney was ulcerated in several places, and full of purulent matter, mixed with grit; several hydatids appeared on its external surface, the ureter was somewhat enlarged.

He next examined the bladder, which was exceedingly large, and contained above 3 pints of clear urine: on opening it, and introducing his hand he found 2 smooth flattish stones, somewhat larger than common Windsor beans; he discovered a third in the neck of the bladder, which probably had been forced there during the paroxysm, and appeared to be the immediate cause of his death; it was about the size of a filbert, and had quite blocked up the passage.

On dissecting the hernial bag, the first part that presented was a large piece of fat, about $\frac{1}{4}$ lb. and immediately underneath it lay a large portion of the colon, about 10 inches in length: the internal surface of the peritoneum was strongly attached to the colon by several filaments, and to the scrotum by its cellular substance. All the other viscera were in a natural state.

A Method of preparing Specimens of Fish, by drying their Skins, as practised by John Frid. Gronovius, M. D. at Leyden.* N° 463, p. 57.

There are requisite for this purpose, a pair of scissars, with very fine blades, and sharp points. Small wooden plates of the lime-tree, or wooden trenchers. A very fine needle. Slips of parchment as large as the fishes. Minikin or small pins.

Take hold of the fish with your left hand, so as that the belly may be towards the hollow of your hand, and its head pointed to your breast. Then with the needle make a wound behind its head, into which introduce one of the points of your scissars, cutting gently from thence along to the tail. If you would preserve the right side, the scissars are to be conducted on the left side of the fin. This being done from the head to the tail, the scissars are to be pointed deeper, and the flesh divided quite to the back-bone. Then turn the fish with its back downward, and its belly upward, and proceed in the same manner, cutting with the scissars through both head and jaws. Take away the brain and gills. The fish then easily parts, the intestines appear, which may be easily taken away. The back-bones are then to be cut asunder, the fish is to be washed, rubbed till it is dry with a linen cloth, and placed on a board, in such a manner, that the skin, covered with its scales, may lie uppermost, and all the fins and tail are to be expanded with pins. Let it then be exposed to the sun, if in summer, or, if in winter, to the fire, till the skin grows quite dry and hard, when it must be turned, and the flesh exposed to the sun or fire, till it also is dry; and then the skin may be separated from the flesh with very little trouble, and, being put between papers, must be pressed flat. But as a sort of glutinous matter, in pressing, is always forced out from between the scales and the skin, a piece of parchment is to be laid under the fish, which is easily separated from the scales, as paper always sticks; for this reason it is necesary, that after an hour or two, a fresh piece of parchment should be applied: and thus, in the space of 24 hours, the fish is prepared.

Account of the Fire-ball seen Dec. 11, 1741. *By Capt. William Gordon.* N° 463, p. 58.

On Friday the 11th of December 1741, about 1 P. M. coming by water from the city to Whitehall, and near to Hungerford-stairs, there appeared to Capt. G. between Vauxhall and Lambeth, a body of fire: it sprung upwards in its ascent almost perpendicular to the horizon, to the height of about 35°, in the

* Dr. John Frid. Gronovius, a physician at Leyden, is chiefly known by his work entitled Zoophylacium, in folio, containing good descriptions, accompanied by figures, of many rare and curious animals, particularly fishes, insects, and amphibia.

space of a few seconds, and nearly in form of a large paper kite, projecting a long tail towards the north-west, not unlike those of slips of paper set on fire; in this state it continued so long, that he made the waterman lay his oars in, that he might the more easily observe whether it was the work of art or nature, as he was in some doubt. It had from its first appearance expanded itself considerably, so that the extreme breadth was seemingly equal to the diameter of a full moon arising from a dusky horizon. In this form it continued ascending for the space of 2 minutes, gently shooting to the north-east, till it arose to about 45°; then suddenly quitting its tail, which vanished, colouring the neighbouring clouds with a yellow on their separation, it formed itself first into a ball of fire; then shooting quickly to the south-east in a stream of light, disappeared, making a noise like a clap of thunder at some distance, and leaving behind it a smoky substance in its track.

The weather was moderate and cloudy, wind nearly west south-west. It continued in sight upwards of 5 minutes.

Concerning the Fire-ball seen Dec. 11, 1741. *By the Rev. Mr. William Gostling, of Canterbury.* N° 463, p. 60.

As the fire-ball appeared at noon-day, and the sun shining, few people saw it, and they could only guess at the course. The best account he had is at second-hand, from two farmers who saw it together, and make its course from north-west by north to south-east by south, and right over Littleborn, which is the first village in the road from Canterbury to Deal. Their way of telling its course was by saying, it went from Westbere towards Ratling, and they heard only one explosion.

Observations on Mr. Sutton's Invention to extract the foul Air from the Well and other parts of Ships, with Critical Remarks on the Use of Windsails. By William Watson, F. R. S. N° 463, p. 62.

As nothing is more conducive to the health of the human body, than taking a sufficient quantity of wholesome air into the lungs, so the contrary is attended with pernicious, and often with destructive consequences.

One of the great uses of air in inspiration is, to cool* the blood passing through the lungs; where nature has provided, according to the excellent Malpighi, that the blood should be distributed through a vast number of exceedingly fine arteries, which are applied all round the thin vesicles of the lungs;

* A very different theory of respiration has been adopted in later times, founded on a more accurate knowledge of the composition of atmospheric air, and on the changes produced in it and upon the blood during its inhalation into the lungs.

and by this means the blood is exposed to the air under a prodigious large surface, by which the putrefaction is prevented, which, from the alcalescent quality of that fluid, would otherwise be speedily destructive.

It has been frequently tried, that if a gallon of air be contained in a bladder, and by means of a blow-pipe inspired and expired by the lungs of a man, without having any communication with the external air; in the space of a minute, or little more, it becomes heated, and unfit for respiration; and without the addition of fresh air, the person would speedily be suffocated. The diving-bell is another instance of the same kind, where a constant supply of fresh air must be had, to keep out the water, and refresh the people included.

Though air is absolutely necessary to our existence, and necessity constrains us inevitably to breathe in it, it may be made a vehicle of most malignant poisons; as the famous Grotto del Cani in Italy, poisoning air by charcoal, air impregnated with the fumes of fermenting vegetable liquors, stagnant air, either alone or mixed with water, soon becomes pernicious and very offensive; as in wells dug for supply of water, and disused for some time; also in the wells and in the holds of ships, where what is usually called the bulge-water, if the ship is tight, and the water not pumped out often, it soon becomes so extremely poisonous, as frequently to suffocate those seamen, who, as the pumps are subject to be clogged with filth, venture down to cleanse them; and also to affect persons at a distance with violent head-achs, cold sweats, and frequent vomitings, which continue more or less, in proportion to the distance from the well of the ship when the injury was received, and the degree of putrefaction in the water and air.

The air, in ships particularly, is very liable to be vitiated; not only from the bulge-water, but from too many people breathing in the same atmosphere; especially in ships of war, hospital ships, and those used in the Guinea trade for negroes, where a number of uncleanly people, being stowed too close together, heat the air, make it replete with noxious effluvia, destroy the particles adapted to cool the lungs, particularly the acid nitrous gas, which is so abundant in cool air, and manifests itself not only by the quantity of nitrous crystallizations, which may be collected from caverns of the earth, especially those open to a northerly aspect, but by exposing pieces of the flesh of animals fresh cut, or their blood, by which the colours of their surfaces are soon changed from a dark deep red, to a more lively and florid one. Air deprived of this valuable property, and replete with hurtful ones, not only from the people, but from the stinking water in the well and lower parts of the ship, must produce the most putrid, if not pestilential fevers.

Though the equilibrium within places confined is maintained by the external air, yet unless, by openings properly adapted, the air be suffered to pass freely through, the external air proves as a stopple to the internal, and only mixes with the next in contact; as is evident from the common occurrence in privies, which are scarcely offensive in clear weather, but are much so in foul or windy, from a diminution of the incumbent pressure, when the vapours that have been pent up, expand to a considerable distance.

To prevent the above-mentioned inconveniencies, and preserve the health and lives of that valuable part of the nation, the seafaring people, many schemes have been thought of; particularly the machines of those two very worthy ingenious and industrious members of this Society, the Rev. Dr. Hales and the Rev. Dr. Desaguliers; the first by an instrument which he calls the ship's lungs, in his treatise on Ventilators, and the latter by a machine, which is an improvement of the Hessian bellows, Philos. Trans. N° 437; but as these have been laid before the Society by the gentlemen themselves, Mr. W. passes them over, and proceeds to mention the contrivance commonly made use of, viz. the wind-sails.

These are made of the common sail-cloth, and are usually between 25 and 30 feet long, according to the size of the ship, and are of the form of a cone ending obtusely. When they are made use of, they are hoisted by ropes to about two-thirds or more of their height, with their basis distended circularly by hoops, and their apex hanging downwards in the hatch-ways of the ship; above each of these, one of the common sails is so disposed, that the greatest part of the air, rushing against it, is directed into the wind-sail, and conveyed, as through a funnel, into the upper parts of the body of the ship. These must be hung up and taken down every time they are used, and the supply by this method is not constant. Though custom has given a sanction to this device, it is subject to many inconveniencies: 1st, Each ship having commonly three of these, one to each mast, the seamen are a considerable time in getting their apparatus ready, and hoisting them up, to make use of. 2dly, They can only be used in mild weather. 3dly, Near the equator, where fresh air is most wanted, there sometimes happen such dead calms, that they are useless, by not having air enough to distend them. 4thly, The air thus admitted passes only into the upper and more open parts of the ship, so that the well, &c. receive no change from it; and it is observed, that sometimes, on using them after some discontinuance, they drive offensive air into the cabin, and more airy parts of the ship, like as the pouring some fresh into stinking water makes more water stink, though in a less degree. 5thly, They are improper to be used in the night-time, when the people are sleeping between decks. And, lastly, admitting they had none of the former inconveniencies, their use must be destructive in hospital ships; where, though fresh air imperceptibly received is absolutely necessary to preserve the crew, as free as possible, from the infectious breath and exhalations of the diseased and wounded seamen, yet blasts of wind, pouring impetuously into the very places where the sick lie, must be attended with such consequences as are too obvious to mention.

To remedy these inconveniencies, to prevent air proving foul even in the wells and holds of ships, and to cause imperceptibly a large circulation of fresh air into every part of the ship at all times, Mr. Sutton has invented the following scheme, in the preceding N° of the Trans. which is useful not only in these cases, but, by altering some parts, as particular places require, may be applied to houses, close parts of prisons, wells at land, privies, hospitals, &c.

Nothing rarefies air so considerably as heat, and whenever it causes a diminution of the density of the air, that part next in contact will rush in, and be succeeded by a constant supply, till the air becomes of an equal degree of elasticity. Therefore, if a tube be laid in the well, hold, or any other part of the ship, and the upper part of this tube be sufficiently heated to rarefy the impending column of air, the equilibrium will be maintained by the putrid air from the bottom, which being drawn out this way, a supply of fresh air from the other parts of the ship will succeed in its place; which operation, being continued, will entirely change the air in all the parts of the ship. This principle, exactly conformable to the doctrine of pneumatics, is the basis of Mr. Sutton's machine, which being put in execution on board the Hulk at Deptford, before the lords of the Admiralty, commissioners of the navy, the very learned and ingenious president M. Folkes, Esq. Dr. Mead, &c. performed to their satisfaction, in bringing air from the bread-room, horlop and well of the ship at the same time, in such quantity, that large lighted candles being put to the end of tubes, the flame was immediately sucked out as fast as applied, though the end of one of the tubes was above 20 yards distant from the fire. The method is as follows:

To boil the provisions of the ship's company, they have a copper, larger or smaller in proportion to the size of the ship, and number of the crew: this copper is fixed in ships in the manner as on land, having under it two holes divided by an iron grate. The first hole, having an iron door, is for the fire; the ashes from the grate drop through into the bottom of the other; the smoke passes through a chimney, and is discharged as usual. After the fire is lighted, it is supported by the air from the parts next the ash-pit; but having, contrary to the usual custom, adapted an iron door, like the former, made very tight, to prevent the ingress of air, the fire would soon be extinguished, if not supplied by some other aperture; in order to which, one or more holes are made through the brick work in the side of the ash-pit; and tubes of lead or copper fitted closely in the holes, and made fast, are laid from thence into the well, and other parts of the ship; by which means the air next the bottom of the tubes rushes through them, and the foul air succeeding is transmitted through the fire, and passes off, without offending, by means of the chimney; and a supply of fresh air from the other parts of the ship continually fills the place of the former, the fire requiring a constant support, which support will be wanting, not only during the continuance of the fire, but while any warmth remains in the fire-place, copper, or brick-work, as was observed on board the hulk at Deptford, where the draught of air through the tube lasted above 12 hours after the fire was taken away. This being considered, as the dressing the provisions for a number of people will take up some hours every day, the warmth of the brick-work and flues will continue a draught of air from one day to the next. Mr. Sutton proposes thus to circulate the air by the same, and no greater expence of fire than is used for the necessities of the ship. The operation of the machine will be equally useful in large as small ships; for the greater the number of people they have on board, the larger quantity, and longer continuance, of the fire will be necessary to dress the provisions; and therefore there will be required a greater quantity of air to support that fire.

There is also, especially in large ships, not only a copper, but also a fire-grate like those used in kitchens: that the heat and smoke of this also may not be useless, an iron tube may be fixed behind the grate, and inserted quite through the deck, so that one end of it will stand about a foot, or little more, in the chimney above the brick-work, and the other will enter into the hold, or any other part of the ship; so that the upper end being heated, the draught of air will be supplied from below, as in the other case. This likewise was tried on board the hulk, with an iron tube about 2 inches and a half in diameter, and the lighted candles held at the bottom of this tube were extinguished as fast as by any of the other.

It may be objected, that a number of tubes take up too much room, especially in merchants' ships, and are subject to be broken or injured by loading or unloading: to remedy which, it is advisable, that only one tube of a convenient size be made fast unto the side of the ash-pit, and, as soon as it comes through the main deck, to compress it, not too close; and it may be divided into as many ramifications as may be thought necessary, especially as the bread-room, store-room, &c. cannot be kept too sweet, a branch for each of these, and these branches be carried between the beams which support the deck, till they come to the side of the ship, and there let down likewise between the beams into the places intended; by which contrivance their operation will not in the least be obstructed, and the tubes be secured from any accident.

The simplicity of this machine, it being so little cumbersome, its operation without any labour to the seamen, the small expence to put it in execution, and maintain it, besides the before-mentioned considerations, are other arguments for its general use.

The Method of making Soap-lees and Hard Soap, for Medicinal Uses. By M. Claud. Joseph Geoffroy, of Paris, F.R.S. N° 463, p. 71.

To make the lye, take, (says Dr. G.) of the best calcined lime, that has been the least exposed to the air, 5lb; of good salt of kali or glass-wort of Alicant, pulverized, and passed through a fine sieve, 10lb. Divide the lime and the salt of glass-wort, called in England barilla, into 2 equal parts; then put the lime, broken into pieces not larger than an egg, into new stone pans, and cover it with as much salt of glass-wort as is designed for each pan. Pour afterwards on these several mixtures hot water, by little and little, to give time to the lime to open itself, till it turns into a sort of meal, which will happen after having poured 3 half-pints * into each pan. Then add to it the rest of the water that is required, stirring this mixture with a stick of white wood; when there are 18 or 19 quarts of water in each pan, there is enough for dissolving the salts. In this state the pans are left for 12 or 15 hours; after which this lye is filtrated through brown paper, supported by a coarse cloth, fixed to the 4 corners of the filtering frame. When the whole mass of the lye and of the lime is well drained, put it into a clean iron pot, with 10 quarts of water, to the quantity taken out of each pan, and let it boil an hour; then filtrate it a 2d time. Afterwards it is put into another clean iron pot, and as it evaporates by degrees, it is filled up again with the first lye prepared, without boiling. Let it continue to evaporate till the 28 quarts of water, that have been used for making the lye of the mixture that was at first put into each of the pans, be reduced to 2 quarts and half a pint, or so long till a small salinous film forms itself on the top of the lye. This liquor turns almost black, because it corrodes the iron; but this is no inconvenience, as will appear hereafter. In this state of concentration, if we let a drop of it, while hot, fall on a piece of glass, it will be very quickly covered with a fine and greasy film, which makes it look as if it was congealed. At the bottom of this lye is found a salt in flakes, which, being melted in a crucible, produces a lapis infernalis of a strong caustic

* The Paris pint is near a quart English.—Orig.

power. We may know also, that the lye has acquired the necessary degree of concentration, when, becoming more active, we see, that the edge of the pot that has been wetted by it, turns red, while the lower part of the side all around, down to the surface of the liquor, takes a greenish colour. Then the pot must be taken from the fire, and the liquor left to cool so far as to be put into glass bottles without cracking them: the bottles ought to be carefully corked, not only to prevent the salts contracting a dampness from the air, which would lessen the degree of forced concentration, which has been acquired by the evaporation, but also to preserve what is sulphureous, which would exhale, if the liquor remained long exposed to the air; for probably that sort of hepar, formed by the union of the caustic salt with the sulphur of the ashes of the glass-wort, ought not to be neglected.

Now, the better to direct those who wish to work after these processes, and to furnish them with the degrees of concentration this lye is to have, in order to make with oil a solid soap out of it as speedily as possible, take a glass phial with a narrow neck, and fill it with clear water up to a mark made on the neck. That now used by the Dr. being filled up with water to that mark, contains just 3 oz.: afterwards empty it carefully, and instead of clear water, fill it with that concentrated lye as far as the foresaid mark, and then weigh it. If the weight be increased 8½ or 9 drs., that is, near 3 drs. in each ounce, this shows that the lye is neither too much nor too little concentrated. An hydrostatical balance, a water-poise, and other instruments, might also give this degree; but in the country they are not at hand, and he judged it best to point out only what is most easy Soap-boilers use for this end a fresh egg; if one half of it sinks into the lye, they judge the latter to be of the first strength, that is to say, that this is the lye which they ought to employ last of all in their manufacture; if the egg sinks in to 2 thirds, the lye is called the second; and, lastly, if the lye covers the whole surface of the egg, it will be called the first, and will be that with which they begin their operation or boiling. But this way of trying has not all the exactness which can be desired, because all hens' eggs have not the same specific gravity. Besides, as he makes his soap without fire, he must take the lye that is most concentrated.

Lest the iron, which is corroded by the lye, should enter into the composition of the soap, we need only evaporate the lyes in earthen pans put over a balneum mariæ: but as this evaporation is slower, it will consume much more coals. We may even see in those pans by different marks, that the liquor approaches the desired degree of concentration, partly by a piece of wood marked with notches, partly because if there is the least ferruginous speck in the earth of those pans, the liquor will penetrate that ferruginous place, and make a spot there. By using earthen pans you will get a very limpid liquor, and which will only have a very pale straw-colour, even after its perfect concentration.

The lye prepared in iron, being kept for some time, clears up, and leaves a black sediment, which is that part of the iron which it has separated by corroding the sides of the pot. And yet this ferruginous lye, together with the oil, will form a white soap, if we let that black sediment precipitate. This sediment is true iron. The Dr. has made himself sure of it, by calcining it in a crucible, after having moistened it with oil.

One ounce of concentrated lye to the degree above mentioned, contains 3 drs. 18 grains of salt: when this salt is dissolved again in distilled rain-water, and filtrated, it yields 3 grains of coarse earth, which cannot penetrate the pores of the filtre.

To use it to make soap, take 1 part of it to 2 parts of the best oil: mix them gently in a China bowl, stirring them with a spathula of white wood, till both liquors are come to the consistence of butter that is churning: this thickening is much sooner done in winter than in summer. Keep the vessel in a dry place, that the moisture of the air may not diminish the strength of the lye. The mixture daily grows to a body, and when it is in the sun in summer, and on the chimney mantle in winter, the phlegm evaporating sooner, it becomes perfect soap in 4 or 5 days, provided the lye be sufficiently concentrated. It will be well however, that during the time the 2 liquors are binding together, the mixture be stirred with the spathula, that the water may not be kept in, but evaporate the sooner. When the soap is made, it easily comes out of the vessel, but it has not yet lost all that moisture it should lose; so that though it may be used in that state, it is better to keep it 12 or 15 days longer; at the end of which time, if decompound, you always find the whole oil employed; that is to say, out of 18 drs. of this perfect soap, you get $1\frac{1}{2}$ oz. of oil, and 2 drs. 23 or 24 grs. of salt of glass-wort. So after this method a patient may easily make his own soap, and be sure of the ingredients; perhaps even in the great manufactories, one day or other, they may prefer this to that which is now in use.

As to what relates to the oil of lime, (huile de chaux) spoken of in his experiments, it is the caput mortuum of the sal ammoniac, after distillation of the volatile spirit by means of quick-lime; it is exposed in a flat vessel to the moisture of the cellar, whence a deliquium is formed, which is called oil of lime. It is lime dissolved by means of the acid of the sea-salt, which is contained in the sal ammoniac; other chymists call it the fixed liquors of sal ammoniac. Your soap-boilers are obliged to add sea-salt to their soap, which probably comes from their making use of pot-ash in their lyes, which they would have no occasion to have recourse to, if they employed true salt of glass-wort, seeing this strong lye of salt of glass-wort makes soap immediately; besides, the salt of glass-wort contains sea-salt, as he has demonstrated by making Glauber salt with pure salt of glass-wort and oil of vitriol. If instead of salt of glass-wort we make use of pot-ash with oil of vitriol, it will not make Glauber salt, but instead of it produce tartar vitriolate.

In describing this sort of soap, the Doctor had no other view, than not to deviate from the way of making Alicant soap, and to know well the proportions, in order to apply them to the making of the soap proposed, and to fix them with regard to the lime and the salt of glass-wort, which for many and various reasons is preferable to other fixed salts, as being that which forms the best, the most detersive, and the mildest soap, as it has been found by experience in all our manufactures.

The observations he had laid before the Academy, prove that the oil which has passed through the lyes of lime and salts, is perhaps easier to digest than any other. He there demonstrates, that the oil separated from the soap by means of acids, as he has pointed out, is found to have acquired a property which it had not before; for it dissolves in spirit of wine, and perfectly unites with it; which it could not do while it was crude, that is, before it had formed soap, or had been boiled with metallic limes.

Account of the Earthquakes felt in Leghorn, from the 16*th to the* 27*th of Jan.* 1742. *By the Rev. Sig. Pasqual R. Pedini, Principal of the Clergy of the College of Leghorn.* N° 463, p. 77.

Jan. 16 was a very temperate day, with a gentle breeze between south and west. A little after 24 hours, (about 6 at night, according to our English way of reckoning) Sig. P. observed a certain dark cloud, which passed with a bad smell, settle within a foot and a half over the tops of the houses, like the smoke that the peasants make in an evening, when they burn their garden rubbish, or such like. At 2 hours in the night, (8 o'clock English) they thought the pavement gave way, and the chamber shook. On going to the window, he found a small air from the south; the dark cloud was no longer to be seen, but a thin slight obscurity in the air. Scarcely a quarter of an hour passed, when the chamber received a more violent motion than the former, and the candles on the table moved from west to east. He smelt the stink no longer, but observed the clouds increasing and thickening on every hand, but always with a white hue, like the circle which is often seen round the moon, but of a prodigious extent. A few minutes after 4 in the night, another violent shock began, far superior to the two former, which lasted about the time one might say an Ave Maria; the motion was sudden, and the shaking of the house was from east to west. At 10 hours and a half, were perceived 2 other shocks, with a small intermission of time, much like the 2 former. Another shock waked him at 11¼, and another about an hour after.

In the morning of Jan. 17, after the last shock, there fell a little small rain, like hail, which turned to snow in about 14 or 15 hours, which fell in such abundance for an hour, that the streets and tops of the houses were quite covered; and a little more after mid-day, which continued all the remainder of the day.

On the 18th there were no sensible shocks of the earthquake, but now and then visible undulations of the ground, though of no consequence.

The 19th in the morning, at sun-rise, there were between the east and south certain clouds very thick, which dispersed as they came nearer to the solar disk; but there always remained a particular uncommon whitish thickness in the air, till 16 hours, when it was entirely dissipated. At 18¼ hrs. was heard a rumbling noise, the house began to shake, and the motion continued 8 or 10 seconds: it came like a blow, and the house waved from west to east. At 19 hours exactly, followed another shock, which lasted about 3 seconds. All the remainder of the day, quite till 23 hours, the earth was in continual motion; and exactly at 23 hours followed another shock, like that at 18¼. At 2 hours, and at 3½, and 3 hours 50 minutes, were 3 small shocks; and from that time to the 20th of January at 23½ hours, nothing was felt: at this hour there was a small shuddering, which was not universally observed. At 5 hours 25 minutes in the night, followed a shock like that of 18¼ hours of the 19th day, with this difference only, that the house waved from south-east, and continued between 10 and 12 seconds; there followed a strong undulation of the ground till 21 hours of Jan. 21; at 23 hours of the said day, a small motion of the ground was observed by many.

Some fishermen told him, that at the same hour on the 19th day of January, that they had the terrible shock at Leghorn, they, being at sea between Meloria and Gergona, saw a small part of the sea rage violently, and raise itself to a great height in a white foam, with a dreadful roaring, which frightened them so far as to imagine themselves lost, though it did not directly beat upon them, but they felt it on one side only; which made them imagine some violent mischance at shore; and keeping their eyes always on that troubled part of the sea, perceived it made towards Leghorn, and broke on the old fortress, which for a little while was hidden from them. The captain of a ship, who came to this port, says, that he saw, to his great surprize, a few miles distant from Capo Corso, several streams running with great impetuosity different ways, and so very rough, that though he had a very fair wind, he expected every moment to be lost. This must have happened just before the earthquake of the 19th of Jan. above mentioned.

From the 20th to 23 hours of Jan. 25, the ground was in a continual agitation. And on the 26th, at 23 hours, there was a much greater motion than that of the 20th day; but from that hour till $18\frac{3}{4}$ hours on the 27th, there was not the least motion perceptible. At this time, however, was heard a most dreadful noise, followed by a treble shock of the earthquake in the most frightful manner, and beyond measure violent; it began by a successive motion, and followed by a sort of blow with dreadful violence; and at last came another successive motion, more horrible than the former. There was heard from under-ground a hollow terrible rumbling, as if the whole earth had broken to pieces: it had a motion like turning and continued moving; the houses waved 30 or 32 seconds, from east to west; part of the door-case fell, and the partition walls cracked; the mortar fell all about like rain, the furniture and cloaths hung to the walls fell all down. He without being able to seek safety out of the house, stood fixed, and nailed up, as it were, by the surrounding crowd of frightened wretches that flocked in upon him. At last, however, he got out, and could hardly believe his eyes when he found the houses all standing, having figured things much worse. Every thing suffered in some degree, there not being a single edifice but what was damaged: but those which are well built have suffered scarcely any thing: some must inevitably be rebuilt, chiefly those which remain leaning on one side: which proceeds chiefly from the load occasioned by their being raised so high. What was most surprising, was the number of cracks in the walls of the collegiate church, which were built without sparing any cost, to make them a complete piece of workmanship, and are of an extraordinary thickness, as might be observed in some of the openings in the building and vaulting, which was esteemed superior to any in the town: hence we may conceive a just idea of the extreme violence of the roaring earthquake. Had not the houses been in general very good, they must have come to the ground. The ruins consist in the roof of the church of St. John Baptist, the convent of Augustine Friars, the roof of a palace called Rosciano, belonging to the family of Borghesi of Sienna. Besides these, there are few others of consequence, and but 13 people killed. There is an immense quantity of iron chains used, to keep the walls of the houses together.

On account of the inconveniences attending this earthquake, an infinite number of people went out of the town; abandoned the houses and shops instantly, to seek refuge in the great piazza: so great was the consternation, that no one knew what he was about. It was an object of the greatest compassion, to see the astonishment and general confusion that prevailed; every body looked pale as death, without knowing what he did or said.

Before the earthquake on the 19th, the waters swelled, and then fell again; soon after they swelled half a yard higher than ever they were used to do. The same night and the following, there was a strong smell of sulphur in the streets. This smell was likewise found in the water of some wells. The sea was seen in sundry situations, now high, and then presently very low again; sometimes strongly agitated, and at others on a sudden calm. On the 27th, the waters were observed to rise as high or something higher than the 19th.

It is said here, that the sea roared with such violence and smartness, that its noise was like the firing of large cannon. A fisherman, a Frenchman by nation, being then in his boat, found it of a sudden raised up a prodigious height, and then it fell down so low, that he thought it had touched the bottom of the sea, and concluded himself lost. During this uncommon motion he affirms to have heard one of these noises resembling the firing a cannon, and afterwards felt no storm.

A Demonstration of Newton's Method of raising a Binomial to any Power. By J. Castilion, Profes. of Philos. in the Academy at Lausanne, and F.R.S. N° 464, p. 91.

Every index is either an integer or fraction, and these either positive or negative. 1. When it is a positive integer; then to raise the binomial to any power of the index m, is nothing more than to write down the given binomial as often as there are units in m, and to draw or multiply all these binomials into one another.

2. When the index is a positive fraction, as $\frac{r}{n}$; then to raise the binomial to this power, is to raise the given binomial to the r power, and, this power being given, to find the quantity which raised to the n power, equals the r power of the given binomial.

3. But when the index is negative, either an integer or a fraction; then to raise the power, we must first proceed as in Art. 1 or 2, and then divide a unit by the power so found.

M. Castilion then assumes $p + q$ for any given binomial, to be raised to any power m. He remarks that p^m and q^m will be the extreme terms of the power, and the intermediate terms will be the $m - 1$ intermediate terms between those, without the co-efficients, but as to the finding of these co-efficients, in which consists the chief difficulty, his process is so illogical and embarrassed, and so far short of a demonstration of Newton's celebrated binomial theorem, that it is undeserving of being reprinted on the present occasion.

Two Histories of Internal Cancers, and of what appeared on Dissection. By William Burton, M. D. of Windsor. N° 464, p. 99.

Bartholomew Collins, a labourer in the king's works at Windsor, of low stature, pale complexion, slender and active, about 36 years of age, temperate in his manner of living, had, for some years, been afflicted at different times with wandering arthritic, colic, and nephritic pains, none of which were periodical or constant. During this term, when in best health, he was usually costive, and his urine, as soon as made, deposited a calculous sediment.

In March 1733, he received a violent blow by a ponderous and obtuse instrument on his loins, and the spine of the os innominatum, towards the left side, which disabled him for that day. On the next, the pain abating, he continued so well for 6 months after, as not even to recollect this accident, till about a month before his death, though he was often asked by the physician, whether that part had ever suffered a contusion.

In January following, he complained of an excruciating pain, extending from the said spine to the spurious ribs on the left side, which sometimes attacked also the intestines; whence he became continually restless, especially in the night, and, tossing the bed cloaths off, frequently lay naked. He could not now lie on his back or left, viz. the affected side, but lay always on his right side, leaning on his right elbow. In April 1734, his left knee, from a contraction of the muscles elevating it, was always drawn up towards the abdomen, insomuch that he could not stand upright. His left testicle, formerly less than the right, was now become scirrhus, and increased to double the magnitude of this, and the left spermatic vessels felt like a knotty cord. A sort of hectic fever attended him, the exacerbation of which, as well as of his pains, was generally about noon and 6 in the evening.

After various medicines had been tried in vain, Dr. B. was consulted, April 4th, 1734. The patient at that time complained of an intolerable pain, on any pressure, about the region of the left kidney; whereupon a maturating cataplasm was applied in the day-time, and a plaster at night. Emulsions, whey, and such like, were the chief internals he used till April 8th, when crude mercury was recommended to him, of which he took 1 oz. night and morning, which gave so much relief as to encourage the continuance of that medicine only to the 17th, when the pains returning, he was bled once in 2 or 3 days, to 4 or 5 oz. and treated with the subacid, cooling regimen, and paregorics, till April 29th, when he first mentioned a scirrhous tumour, as large as a hen's egg, situate on the left mastoid muscle of the neck. On comparing this with the testicle, neither of which tumours were in the least diminished after applications for that purpose, it was conjectured, that either the pancreas or mesenteric glands were cancerated. Various other remedies were tried; but he became more and more emaciated, till May 21st, the day of his death.

May 22d, on removing the integuments of the abdomen, the musculi recti appeared livid. The omentum was destitute of fat. The intestine contiguous to the left os innominatum was tinged with green. Nothing besides appeared morbid in the viscera in situ, at first view. The situation of the pylorus seemed lower than usual. The colour or texture of the liver were not remarkably preternatural. The spleen was of the largest size, and adhered in its hinder part so strongly to the peritonæum, that it could not be separated without laceration, and there remained in the place of adhesion, a thick, callous, and almost horny membrane, as large as a half-crown. The pancreas was very small, and seemed composed of small scirrhi. The left kidney was twice as large as natural: its substance about the pelvis was corroded by a semipurulent cancerous sanies, that was in part collected between the surface of the kidney and its containing bag. Its internal structure was not much amiss: but the fomes morbi, the most singular and surprizing phenomenon in this subject, was a number of large conglobate, steatomatous, cancerated glands, reaching from the receptaculum chyli to the lowest vertebræ of the loins, so connected together as to represent a pancreas affixed to the vertebræ of the loins, and upper anterior part of the left psoas muscle: it was 4 times as large as his pancreas, and as large as the right or sound kidney. The aorta descendens pervaded the middle of this preternatural substance lengthwise. From this mass, as a fountain, flowed that cancerous sanies, which had made its way to the left kidney, and also corroded the superior carneous part of the left psoas major, and iliacus internus, so that one might easily rend their gangrened flesh like rotten linen. Some of this green ichor collected near the os innominatum had laid the spine of it quite bare. The left spermatic vessels were knotty, tumefied, and livid. The mesenteric glands were scirrhous. The descending trunk of the aorta was smaller than usual; and, dividing it, we extracted a small polypus. The examination of the other cavities was not permitted.

Thomas Trinder, a taylor, at Windsor, in his 29th year, was of a pale complexion, with red hair, of a middle stature, and thin habit, addicted to smoking from morning to night, and now and then to hard drinking. Eight years before his death, he was thrown in wrestling, so as to pitch the small of his back on the corner of a chair, by which at first he was much hurt in that part;

but on the abatement of his pain, he became from that time subject to fits of the colic, in which he said his bowels seemed to be drawn to his back-bone, and usually received ease by binding his waist as tight as he could. He had also frequent recourse to Geneva, and such liquors, for relief, but seldom found any, till a swelling, as large as a hen's egg, appeared like a rupture in his right groin. These fits were not of above 24 hours duration, but the inguinal tumour lasted 2 or 3 days. He was often afflicted with stitches under his left breast, which were removed by bleeding.

But in the middle of Nov. 1735, his colic became so violent, that he could not lie in his bed, nor sleep without opiates. Nov. 21, Dr. B. found him in the use of some carminative pills sent by an apothecary. He had frequent retchings to vomit, and was very costive. His pains seemed confined to the intestinal region, and were most acute in the evening, continuing to harass him till 5 or 6 in the morning. His pulse beat seldom under 100 in a minute, at night generally above. He was not very hot nor thirsty. His urine at this time was rather defective in quantity, than amiss in respect of colour or separation. His tongue was foul towards the root, but not very white.

After this, his disorder resembled a nephritic one, his chief complaint being of pain about the region of the kidneys, and along the descent of the ureters. The pain removing from the loins fixed itself at the os pubis and in the thighs Nov. 29, but being abated about the os pubis by proper medicines, the pain afterwards mostly afflicted the left thigh and hip. From this time he generally sat up in his bed, leaning forward to the left, and for the most part cross-legged, finding himself easiest in this posture. He could not lie any time on his right side. The quantity, colour, and sediment of his urine, much the same now as when he was in health. It was made without the pain, which, soon after the nephritic symptoms commenced, he complained of at the root of the penis. And now his disorder resembled the lumbago and sciatica, affecting the left side mostly; whereupon, Dec. 2, he was put in the use of a diuretic and aperient electuary, with terebinthinate clysters. To this time he had very few stools without clysters, and those generally very small and fetid. Dec. 5, the fever and pain increasing, a cooling aperient apozem, with a paregoric draught pro re nata, were continued till Dec. 9, when examining the thigh where the pain now afflicted him most, Dr. B. found some small scirrhi in the groin, which were sensible enough to the touch, though not to the sight; and from that time apprehending him of a scrophulous habit he prescribed accordingly.

Dec. 18, he first mentioned a tumour he had discovered near the navel. It appeared not as he sat, but when laid on his back, there was a protuberance larger than a turkey's egg, 4 finger's breadth on the left side of the navel, extending 2 above it, and 4 below it. By its situation, resistance to pressure, and the unevenness that from under the skin was communicated to the touch, its disappearing when he was in an erect posture, and its not being diminished by discutient fomentations, it was judged to be a scirrhous tumour, which had long existed there unobserved by the patient, till it increased too much to be longer undiscovered. The emplast. de ran. et cum mercur. was applied outwardly; and concluding there was an internal cancer, Dr. B. was encouraged, from the preceding case, to order him hydrargyr. ℥j every morning. On which there was such a remission of his pains, that during almost a fortnight, he got more rest without opiates than before with them; insomuch that being greatly revived, and regaining some appetite, he got down stairs 2 or 3 times. Thinking the plaster increased his pain, indigo blue linen was applied in its stead. The mercury came away by stool, and he had now one almost every day, and sometimes twice a day, without a clyster. His grand complaint now was of a most troublesome cardialgia, especially when he lay down, which was somewhat mitigated by powders of cret. britan. cum pauxillo sal. absinth. From the first use of the mercury he seemed to be recovering, till after about 12 days, when omitting it for a few days, he relapsed into his former or a worse condition; and though he was somewhat easier on the repetition of it, the good effects lasted not long. He drooped daily from the 4th of January, and on the 13th died, emaciated and almost exanguis.

On dissection, nothing preternatural appeared in the integuments, abdominal muscles, or peritoneum immediately under them. But under all these, where the protuberance had been observed, and immediately under the omentum, which was destitute of fat and its lower part mortified, there came in view an anomalous substance in situ, seemingly as large as a very large potatoe; which, when the circumambient viscera were removed, was found to be a scirrhous, fungous, cancerated excrescence, rooted, as it were, to the left side of the vertebræ, quite from the diaphragm down to the pelvis, of a monstrous bulk, occupying near one-half of the abdomen, lying like a tortoise with its head towards the pelvis, and its back to the umbilicus. It was in the upper part covered by and firmly cohered with the colon, which in the whole contiguity was black and mortified. It was strongly attached to the peritoneum on the left side of the lumbal vertebræ, having displaced the left kidney, and brought it forwards to the left side of the navel, so that it came in view as soon as the omentum was removed. It likewise removed the aorta descendens, the left emulgent, and meseraic vessels, quite out of their natural situation; all of which were found pervading the centre, nearly, of this excrescence, and smaller than natural. It adhered to the kidney strongly where the emulgent vessels enter it, and it had detruded most of the small guts into the pelvis. Nothing was preternatural in the stomach or spleen, excepting that the latter, as well as the left kidney, seemed paler than usual, and this kidney also more flaccid: the gall-bladder was shrunk to the size of a nutmeg, and empty. The liver had a preternatural lobule, as large as a hazel-nut, adhering to it by a small pedicle. But otherwise all these viscera, as well as the right kidney, bladder, &c. discovered nothing morbid.

This cancerated excrescence could not be eradicated without laceration, and on its removal, 2 or 3 large trunks of nerves appeared naked, passing over the iliacus internus to the thigh; which had been compressed by this tumour. The weight of this excrescence was 4 lb. 14 oz, and allowing for what remained on laceration, and the effusion on cutting into it, it doubtless exceeded 5 lb. On bisection, it appeared, to the depth of half an inch from its surface, black and gangrened, and below that it was all spongy, with cavities as large as those of a honeycomb; and from it had issued a cancerous sanies, draining to the pelvis.

On opening the thorax, the right lobe of the lungs was full of scirrhous cancerated tubercles, whence a sanies had flowed between it and the pleura; the left lobe was much smaller than the right, was firmly attached to the pleura and mediastinum, and inseparable without dilaceration. It had some tubercles also. The heart appeared sound, but a large polypus was taken out of its right ventricle, at the orifice of the arteria pulmonalis.

Another case occurred to Dr. B. cotemporary with the first of these, and so like to both of them in the antecedent cause and symptoms, that, could he have obtained leave to inspect the corpse, he was persuaded some such immediate cause would have discovered itself. Crude mercury was the only medicine in this case also, which palliated for about 10 days successively.

The diagnostics of a cancer within the abdomen, deduced from the preceding histories, seem to be as follow: a naturally slender habit of body, accompanied with some scrophulous or scirrhous tumour, with a pale complexion, and costive disposition; if such an one, at an age above 20, has received a violent contusion on the loins, and, neglecting all remedies, is some time afterwards attacked with excessive pains, afflicting now the colon, then the urinary passages, spine of the os innominatum, and pubes, at various times, always increased by all internals or externals, by which the heat of the body is increased, especially by terebinthinate clysters; but mitigated by some singularity of posture, in which the patient constantly abides; if these be attended with a hectic fever, without the usual degree of heat in the skin, of whiteness or dryness of tongue, or complaint of thirst, and also without cough, high-coloured urine, or vitiated respiration; if accompanied likewise with an affection of the spermatic vessels, of the thighs, and frequent pleuritic pains; the blood always abounding with tough size; if opiates soon lose their effect, and only, as all other new remedies not heating, seem to give relief for 2 or 3 days; if cathartics take place, and by frequent repetition do not produce a colliquative diarrhœa, and the most palliative remedies are nitrous salts and mercurial; may it not be concluded with much probability, that such a case is owing to some such cause? may it not be pronounced an internal cancer?

Observations on Falling Dew, made at Middleburg in Zealand, on a Leaden Platform, in the Night between the 25th and 26th of July, 1741, N. S. with Figures of the Flakes of Snow observed Jan. 1742. By Leonard Stocke, M.D. N° 464, p. 112. *From the Latin.*

On glass of various kinds, there fell much dew, so as to wet it all over.—On polished brass, but little, and only a thin vapour.—On rough unpolished brass a little more.—On lattin, or iron tinned, a little: on the same of a blue colour, much: on the same rough, very much; on the same smooth, scarcely any: on the same rusty, none.—On pure quicksilver, none.—On smooth tin, none.—On rough lead, much: on polished lead, a little.—On white silver, none; on polished silver, none: on silver gilded, none.—On blue porcelaine, none.—On a stone slab, much.—On a basket, made of Indian cane finely woven, a little. —On a smooth white oaken plank, very much; on the same of a black colour, much less.—On a smooth fir plank, but little.

On shifting those bodies, which received much dew, a little higher, 2 or 3 inches above the leaden platform, the lead dried, and the bodies themselves became wet beneath, as well as above. But the tin and silver being placed in like manner, continued dry, though their place, which before was bedewed, dried up.

Jan. 2, 1742, N. S. early in the morning, there fell flakes of snow, like fig. 5, pl. 14; their diameters, from the extremities of their points, being $\frac{3}{4}$ of a line.—Jan. 10, before noon, as in fig. 6, also $\frac{3}{4}$ of a line in diameter; in the middle of which was a hexagonal rose, like that in fig. 5, only that the oval parts were empty.—Jan. 20, about noon, like fig. 7, being 1 line in diameter; and fig. 8, of $1\frac{1}{4}$ line diameter; which last shone like Muscovy glass.

Concerning the Vegetation of Melon Seeds Forty-two Years old. By Martin Triewald, F. R. S. Milit. Arch. to the King of Sweden. N° 464, p. 115.

Secretary Hæreus, of Stockholm, having a large collection of natural curiosities, among which was a great number of foreign seeds, and finding he had melon seeds that were laid up in a paper in the year 1700; Mr. T. was curious to try if they had retained their vegetative quality, and accordingly the 21st of Feb. 1741, he planted 24 of them in a separate hot-bed, from which he had 21 good plants, which, after they were planted in a new-made hot-bed, showed flowers before they began to branch themselves, and their branches were very narrow, yet produced early and plenty of good melons. This experiment shows not only how very long melon seeds will retain their vegetative quality, but also that good melon seeds cannot well be too old. It is no new thing to make use of old melon seeds rather than new, but he has never heard of any person trying so old as these.

On the Differences of the Heights of Barometers. By M. Samuel Christian Hollman, Leg. Met. and Theol. Natural. in Regia Georgia Augusta, P. P. O. N° 464, p. 116. *From the Latin.*

In July and Aug. 1741, M. Hollman made several observations with barometers, in a visit he made to the mountains of Hercynia in Sweden. Having prepared a new barometer for this purpose, he divided its scale of ascent and descent into Rhinland inches and lines, or 12ths, from the 20th to the 32d inch. On applying it to this barometer, and comparing it with 6 others, which he had by him, he found that they all showed different heights of quicksilver, the differences extending from 2 to 12 lines, the new one being 2 lines higher than any of the others.

On his return to Stockholm from the mountains, he compared the barometers again, and finding still the same differences, he constructed several other new ones, with upright tubes, of different apertures, among which he found also differences of from 1 to 4 lines; those which rose the highest, exceeding one which he called his best, by full 6 lines, or half an inch.

Aug. 12 he repeated the experiments again with his 15 barometers, and finding again nearly the same differences, he then prepared 10 other new ones, with upright tubes, some of them having bent glass cisterns to hold the quicksilver, and some without. Among these the 10 heights differed from 1 to $1\frac{1}{2}$ lines, and they exceeded the height of his best barometer by 4 lines.

These barometers were all made in the same manner, and great care was taken to free the tubes and the quicksilver from all air. There was indeed some difference in the glass of the tubes; the best barometer, in which the quicksilver had always the least height, had its tube of green glass, with a separate cistern made of the same glass; this barometer is N° 1 of class 1 following; N° 2 being that which was used in the mountains of Hercynia. But that tube in which the quicksilver rose the highest, and often a full inch higher than in N° 1, was made of the whitest glass; this shows a remarkable phosphoric light, and has a wooden cistern; but its heights change slower than any other, and is N° 7 in the same class. N° 3 is a diagonal one, with a single bend, and a bent glass cistern. N° 4 is Bernoulli's, the tube of which, is to the cylinder fastened above, as 1 to 8. N° 5 is Huygens's. N° 6 is another diagonal one, but with a double bend, one of which is received by that part of the tube to which the scale is applied, intercepting an angle of about 25°, with the perpendicular part of the tube, in the double angle of which, because of the narrower width of the tube, the quicksilver must be greatly retarded in its ascent and descent.

The barometers in the 2d class have their tubes of a different kind of glass; which when melted at a lamp, partly loses its transparency, and its surface becomes covered with very small scales. In these tubes the quicksilver rises the highest in the upright and simple barometers, excepting only that which produces the phosphorus.

The 3d class contains those barometers having very white glass tubes, that suffer no alteration from the fire, and are all straight and simple. The differences among these amount only to $1\frac{1}{4}$ line; and their greatest height only exceeds the least of the others by about 4 lines.

Are we therefore, asks M. Hollman, to seek for the cause of this difference in the diversity of the glass tubes? Is not the surface of one more rough and uneven than that of another, and does it not therefore more or less resist the ascent of the quicksilver by its friction? or is it from any other cause?*

CLASS I. Baromer used July 27 and Aug. 12.

N°	Aperture	Height
1	$\frac{1}{3}$ line	27 inch. 11 lin.
2	$\frac{1}{2}$	28 1
3	$\frac{1}{2}$	27 11
4		28 4
5		28 5
6	$\frac{1}{2}$	28 .... $7\frac{1}{2}$
7	$\frac{1}{2}$	28 9

CLASS II. 8 Barometers new made July 27.

N°	Aperture	Height
1	2	28 .... $1\frac{1}{4}$
2	2	28 4
3	$1\frac{2}{3}$	28 4
4	$1\frac{1}{2}$	28 .... $2\frac{1}{4}$
5	$1\frac{1}{3}$	28 2
6	$1\frac{1}{4}$	28 2
7	1	28 2
8	$\frac{2}{3}$	28 4

CLASS III. 10 Barometers made Aug. 12, the first 5 having no annexed cisterns, but the last 5 having bent glass cisterns.

N°	Aperture	Height
1	$\frac{1}{3}$ line	27 inch. $11\frac{1}{2}$ lin.
2	$\frac{1}{3}$	27 .... $11\frac{1}{2}$
3	$1\frac{3}{4}$	27 .... $11\frac{1}{2}$
4	$2\frac{1}{4}$	28 0
5	$1\frac{1}{2}$	27 .... $11\frac{1}{2}$
6	$1\frac{2}{3}$	28 0
7	$\frac{1}{2}$	28 0
8	1	28 1
9	$1\frac{1}{2}$	27 .... $11\frac{1}{2}$
10	$\frac{1}{4}$	27 .... $11\frac{1}{2}$

Now, since different barometers have such different heights, M. H. concludes we ought to think about harmonic barometers as earnestly as about thermometers, before we can accurately collect

* Probably the diversity and smallness of the tubes have a great effect on the heights.

from their annual observations their mean heights in different places, and thence, among other things, the elevations of those places above the sea.

Concerning Polypi taken out of the Hearts of several Sailors just arrived at Plymouth from the West-Indies. By John Huxham, M. D. N° 464, p. 123.

During the very dry, cold weather in February and March last, several of the men brought home in the Deptford and Dunkirk men of war, from the West Indies, were seized with short, importunate, asthmatic coughs, without any expectoration, violent and almost continual palpitation of the heart, with a perpetual intermitting, trembling, fluttering pulse, and a constant anxiety, pain and sinking of the heart, as they expressed it. They breathed with excessive difficulty, and could scarcely lie down in bed without suffocation. Their heads, as it were, sunk between their shoulders, and they had very dead, heavy countenances. Some had pains of the side, though very little apparent fever.

Upwards of 20 persons were in a very short time carried off towards the end of March in this manner, notwithstanding the most proper and diligent care, by bleeding, vomiting, blistering, attenuants, diluents, &c.

On this, Mr. Wyatt, first surgeon of the hospital, ordered 2 of the dead to be opened. They were about 40 years old. He found monstrous polypi in both their hearts, and directly had the hearts carried to his own house, and soon acquainted Dr. H. with the whole matter; when they very carefully examined them. The polypi were very nearly of the colour of the buff formed on the surface of highly pleuritic or rheumatic blood, when quite cold or rather whiter. They were vastly tough, and seemed to be formed of various lamina very closely connected, though here and there a bloody vein, as it were, was interspersed. They were not only firmly attached to the fleshy columnæ of the heart, but were also sunk and inserted strongly into the intercolumnia, or sulci, and that even to the very bottom of the ventricles. These roots, if we may so call them, were of a whiter colour than the body of the polypus.

One of these polypi weighed a full ounce, not including its ramifications in the arteria pulmonaris and the cava, but as it was taken out of the right auricle and ventricle; for it was one continued mass, and strongly adhered to both. The polypus taken out of the left ventricle of the same heart, was also very considerable, and rather more firm and compact than that of the right, but of the very same colour, and firmly implanted into the sides of the ventricle, quite down to the mucro cordis. Its branches were shot a great way into the subclavian and carotid arteries, but very little down the aorta. One of the semilunar valves of the aorta was become bony.

There were likewise found very great polypi in the right and left cavities of the other heart, of the same colour, firmness and tenacity, but not quite so large; and they respectively branched their appendices a great way into the pulmonary artery, aorta, &c.

More of the sailors dying in the very same way soon after, the thorax of another was opened, that of a young man about 20. In the right auricle and ventricle of his heart was found a large tough subrubicund polypus, not quite so white as those mentioned before; but there was no such concretion in the left.

Now though Kerkringius and others have endeavoured to explode the notion of the formation of true polypi in the heart and blood-vessels; yet Malpighi, Bartholine, Tulpius, Pechlin, and others, have given us incontestable instances of the existence of true polypi in the heart, in the strictest sense; and we have here 3 unquestionable evidences of the like nature: such, indeed, especially the 2 former, as Dr. H. never has before met with amidst the very numerous dissections he had ever been present at.

Dr. H. adds that he had the first lieutenant and purser of the Dunkirk under his care in very severe pleuro-peripneumonies, whose blood was as viscid as he ever saw; and they were with very great difficulty saved, nor could they be brought to expectorate till the 7th day of the fever.

It may be observed also (Dr. H. continues) that the above ships came home from a very hot climate into a very cold one, in the midst of winter, and that a long continued course of north-easterly winds kept on, and even increased, the cold to a great degree; that pleurisies, peripneumonies, &c. are commonly the effects of such a constitution of air; that the blood of such as labour under these disorders is always extremely sizy; and that the heat of the weather in the West Indies, and large and long continued use of spirituous liquors, had greatly condensed the blood of these men; and that, in the blood-vessels of the thorax of such as die of these distempers, polypous concretions are not uncommonly found.

An Extract of a Topographical Account of Bridgnorth in the County of Salop; containing an Account of the Situation, Soil, Air, Births and Burials of that Place, and of some Tumuli Sepulchrales near it. Communicated by the Rev. Mr. Stackhouse.* N° 464, p. 127.

Bridgnorth is situated on the River Severn, on the west of the ancient forest

* Taken from the original papers of the Rev. Mr. Richard Cornes, late minister of the parish of St. Mary Magdalen in Bridgnorth.—Orig.

of Morfe, and was built, according to Camden, by Edelfleda, lady of the Mercians; but encompassed with a wall, and fortified, by Robert de Belesme, Earl of Shrewsbury; and afterwards favoured by King John, and other kings, with many and great privileges granted in their respective charters.

The town is divided by a stately stone bridge, of 7 arches, over the Severn into two unequal parts; the lesser part, that lies upon the east of the river, is called the low Town, and consists of two streets, one extending from the bridge to the very foot of Morfe, and goes by the name of St. John's-street, from a religious house there in times of popery, dedicated to St. John the Baptist.

The high town lies on the western bank of the river; and rises gradually to a considerable height.

A Table of Births and Burials for 12 *Years, in the Parish of St. Mary Magdalen, which contains about* 520 *Families; and of St. Leonard, containing about* 550 *Families; which, allowing* 5 *to each Family, amounts to* 2600 *Inhabitants in the Parish of St. Mary, and to* 2750 *in the Parish of St. Leonard; in all* 5350.

In the Parish of St. Mary Magdalen.				In the Parish of St. Leonard.		
Births.	Burials.			Births.	Burials.	
54	119	Small Pox.	1727	68	100	Small Pox.
72	77		1728	72	61	
52	74		1729	54	78	
65	78		1730	84	65	
75	36		1731	70	53	
64	41		1732	47	49	
70	46		1733	79	65	
69	77		1734	64	90	
46	56		1735	72	57	
60	32		1736	79	39	
67	22		1737	71	56	
61	53		1738	62	65	
755	711			822	778	

Total increase 88.

In July 1740, he inspected several tumuli on Morfe, where the soil is strong gravel. Montfaucon, in his Antiquities, tells us, that the old Cimbri, or old inhabitants of Denmark, were wont to throw up heaps of gravel on their graves; and that the more remarkable the persons were, the larger were the tumuli over them. It was therefore imagined, that this might possibly be a burying-place of the Danes, who, it is generally owned, were descendants of those people. On opening some of these tumuli, some few bones were found, but mostly in a petrified state. The tumuli are from 8 to 9 yards in diameter, at their bases.

Concerning some extraordinary Effects of Lightning. By the Right Hon. Robert James Lord Petre, F. R. S. N° 464, p. 136.

One Tuesday morning, June 22, 1742, between 3 and 4 o'clock, we had at Thorndon some of the most terrible thunder ever heard. It beat down a chimney at a farm-house just by, and the lightning also struck two large oaks in the park, which stand about 40 or 50 feet apart. It seems remarkable, that the greatest damage appears to be done on the east side of one tree, though it is certain that the storm all came from the south-west. This tree is extremely shattered, and split from the top to the bottom; and on the south-west side, just by the root, there is a large hole made in the ground, about 6 or 7 inches diameter, and about 12 or 15 inches deep. But in the other tree still there is something more particular; for there, without shattering or splitting the tree in the least, or so much as disturbing a single branch, though there are a great many on it, the lightning has taken off the bark about 5 inches wide, in a complete spiral line, from about 40 feet high, down to within about a foot of the ground, where the width diminishes to about 2 inches, and so goes quite off: in the centre of the 5 inches, it has entered the wood about $\frac{3}{4}$ of an inch deep, and about an inch and half wide: this hollow it has in great part cleared out entirely, and the rest is left hanging like pieces of broken or untwisted ropes; this hollow also diminishes near the ground, and dies quite out exactly at the ground: the spiral line is exactly regular, and goes just once round the tree, or but very little more, and is exactly of an equal width all the way. The surface of the bark of both the trees is remarkably touched for about 10 feet from the ground, as if it were shot all over with small-shot, each of which seems to have taken off little scales or outside pieces of the bark, from an inch and half or 2 inches broad and long, to a quarter of an inch.

Of a Meteor seen at Peckham, Dec. 11, 1741. By Thomas Milner, M. D. N° 464, p. 138.

Dec. 11, 1741, at 7 minutes past one in the afternoon, a globe of light, somewhat larger than the horizontal full moon, and as bright as the moon appears at any time while the sun is above the horizon, instantaneously appeared, in a clear blue sky, about the S. S. E. moving towards the east with a continual equable motion, and leaving behind it a narrow streak of light, whiter than the globe itself, throughout its whole course. Towards the end it appeared less than at the beginning of its motion; and within 3 or at most 4 seconds, it suddenly vanished. Its apparent velocity was nearly equal to half the medium velocity of those usual meteors commonly called falling or shooting stars.

The narrow luminous streak remained very distinct after the globe was gone; and gave a fair opportunity for taking the elevation of this phenomenon above the horizon, at the beginning and end of its motion, &c. which was found to be 20°. This luminous tract, or path, seemed a right line, not quite parallel, but a little inclined to the plane of the horizon, viz. highest towards the east. It was at first very narrow, and pointed at each extremity; but soon grew broader, and within 20 minutes after the appearance, it was exactly like a long bright rare cloud, discontinued in two places, above 3 times its first breadth, and a little more inclined to, and elevated above the horizon, than it was immediately after the motion of the globe.

Some Conjectures concerning Electricity, and the Rise of Vapours. By J. T. Desaguliers, LL. D., F. R. S. N° 464, p. 140.

It is proper first to mention, by way of preliminary, that Mons. Du Faye's assertion, of 2 sorts of electricity, is found to be true by observations and experiments, viz. that bodies endowed with the vitreous electricity repel each other, and attract those that have the resinous electricity: on the contrary, those that are endowed with the resinous electricity repel each other, but attract those that have the vitreous electricity.

Dr. D. supposes particles of pure air to be electric bodies always in a state of electricity, and that of the vitreous kind. 1st, Because particles of air repel each other without touching, as has been deduced from experiments and observations.

2dly, Because, when the air is dry, the glass tube rubbed, or only warmed, throws out its effluvia, which the air drives back to the tube, whence they dart out anew, and so move backwards and forwards with a vibratory motion, which continues their electricity.

3dly, Because the feather made electric by the tube, and darted from it, keeps its electricity a long time in dry air; whereas, when the air is moist, the moist particles, which are non-electric, being attracted by the feather, soon make it lose this electricity, which also happens even to the tube in a little time.

Hence it will be easy to account for a famous experiment of the late Mr. Hauksbee, which is this—Having pumped out all the air from a glass globe, he caused it to turn on its axis very swiftly, by means of a rope with a wheel and pully; then rubbing the glass with his hand during its motion, there appeared a great deal of light of a purple colour within the globe, without any light or attraction observed on the outside of the glass, which is observed when the air has not been pumped out. Then turning the cock, so as to re-admit the air gently into the globe during its motion, the light was broken and interrupted, diminishing gradually, till at last it appeared only on the outside of the glass, where it was accompanied with attraction. Does it not appear, that at first the external air by its resistance drives back the electric effluvia, which go then to the inside of the globe, where there is the least resistance? for we observe, that as the air comes in, it repels the electric effluvia, that go inwards no longer, when all the air is come in. If the fact be so, as the experiment shows, is not my conjecture proved, viz. that the air is electrical?

In Dr. Hales's Vegetable Statics, several of his experiments show, that air is absorbed, and loses its elasticity by the mixture of sulphureous vapours, so that 4 quarts of air in a glass vessel will be reduced to 3. Will not this phenomenon be explained by the different electricity of sulphur and air? The effluvia of sulphur, being electric, repel each other; and the particles of air, being also electric, likewise repel each other. But the air being electrical, of a vitreous kind, and sulphur of a resinous, the particles of air attract those of sulphur, and the moleculæ compounded of them, becoming non-electric, lose their repulsive force.

It has for a great while been thought, that watery vapours, that rise in air to form clouds, used to rise, because the water which is of itself specifically heavier than air, being formed into little hollow spherules or bubbles filled with an aura, or thinner air than the ambient air, in this new state made a fluid of little shells, specifically lighter than the ambient air in which it must rise. But philosophers have rejected that opinion; and such as have implicitly come into it, may find it refuted in the Philosophical Transactions, N° 407.

Now may not this phenomenon of the rise of vapours depend upon electricity in the following manner? The air which flows at top of the surface of the waters is electrical, and so much the more as the weather is hotter. Now in the same manner as small particles of water leap towards the electric tube, may not those particles leap towards the particles of air, which have much more specific gravity than very small particles of water, and adhere to them? Then the air in motion having carried off the particles of water, and driving them away as soon as it has made them electrical, they repel each other, and also the particles of air. This is the reason that a cubic inch of vapour is lighter than a cubic inch of air; which would not happen, if the particles of vapour were only carried off in the interstices of air, because then a cubic inch of air, loaded with vapour, would be made specifically heavier than an inch of dry air; which is contrary to experiments, which show by the barometer, that air which is moist, or full of vapours, is always lighter than dry air.

Account of Margaret Cutting, a young Woman, at Wickham Market in Suffolk, who speaks readily and intelligibly, though she has lost her Tongue. By Mr. Henry Baker, F. R.S. N° 464, p. 143.

A brief account of this young woman's case, in a letter from Mr. Benjamin Boddington, of Ipswich, to Mr. Henry Baker, F. R. S. was communicated to the Royal Society in the month of February last, and appeared so extraordinary, that Mr. Baker was desired to make all possible inquiries into the reality of the fact, and lay before the Society what information he should receive on it.

In pursuance of this, he wrote to Mr. Boddington, requesting him to make the strictest inquiry into this affair, not only by viewing the young woman's mouth, and examining her himself, but also by calling to his assistance some skilful gentleman in the physical way, and any other learned and judicious person whom he might judge most likely to contribute towards discovering the real truth, and detecting any error, fallacy, or imposition. He also desired they would heedfully observe her manner of speaking and articulating the sounds of those letters and syllables, in the formation of which the apex of the tongue seems more particularly needful: and, in order to render their examination more easy, as well as more satisfactory, he sent a list of letters and sounds, with several such sentences as he imagined would be most difficult to be pronounced without the help of the tongue.

Mr. Boddington, soon after this prevailed on Mr. Notcutt, a minister, a learned and curious gentleman, and Mr. Hammond, who perfectly understands anatomy, to accompany him to Wickham Market, about 12 miles from Ipswich, where the young woman lives; whose case, after they had inspected her mouth, and examined her in the strictest manner, is set forth in the following certificate signed by them all.

Ipswich, April 9, 1742.—We have this day been at Wickham Market, to satisfy our curiosity concerning Margaret Cutting, a young woman, who, we were informed, could talk and discourse without a tongue.

She informed us, that she was now more than 20 years of age, born at Turnstal, a village within 4 miles of Wickham Market in Suffolk, where she lost her tongue by a cancer, being then about 4 years old. It first appeared like a small black speck on the upper superficies of the tongue, and soon eat its way quite to its root. She was under the care of Mr. Scotchmore, a surgeon of Saxmundham, who soon pronounced the case incurable: however, he continued using the best means he could for her relief. One day when he was syringing it, the tongue dropped out, and they received it into a plate, the girl, to their amazement, saying to her mother, "don't be frighted, mamma; it will grow again." It was near a quarter of a year after, before it was quite cured.

We proceeded to examine her mouth with the greatest exactness we could, but found not the least appearance of any remaining part of a tongue, nor was there any uvula. We observed a fleshy excrescence on the under left jaw, extending itself almost to the place where the uvula should be, about a finger broad: this excrescence, she said, did not begin to grow till some years after the cure: it is by no means moveable, but quite fixed to the parts adjacent. The passage down the throat, at the place where the uvula should be, or a little to the right of it, was a circular open hole, large enough to admit a small nutmeg.

Notwithstanding the want of so necessary an organ as the tongue was generally supposed to be, to form a great part of our speech, and likewise to be assisting in deglutition, to our great admiration, she performed the office of deglutition, both in swallowing solids and fluids, as well as we could, and in the same manner: and as to speech, she discoursed as fluently and well as other persons do; though we observed a small sound, like what is usually called speaking through the nose; but she said she had then a great cold, and she believed that occasioned it. She pronounced letters and syllables very articulately; the vowels she pronounced perfectly, as also those consonants, syllables, and words, that seemed necessarily to require the help of the tongue. She read to us in a book very distinctly and plain; only we observed, that sometimes she pronounced words ending in ath as et; end as emb; ad as eib; but it required a nice and strict attention to observe even this difference of sound. She sings very prettily, and pronounced her words in singing as is common. What is still very wonderful, notwithstanding the loss of this useful organ the tongue, which is generally allowed by anatomists, and natural philosophers, to be the chief, if not the sole organ of taste, she distinguishes all tastes very nicely, and can tell the least perceivable difference in either smell or taste.

We the underwritten do attest the above to be a true account.

Benjamin Boddington.
William Notcutt, Minister.
William Hammond, Apothecary.

Mr. Baker received along with the foregoing certificate, by letter from Mr. Boddington, some farther particulars, which he supposed less material. He says, if she were among 30 people in a room, he thinks it would be impossible for a stranger by any means to guess her being the person without a tongue, for she has no odd motion of her mouth or lips in speaking: she sings with an easy air, and modulates her voice prettily. He asked her, if she did not miss her tongue, or find any inconvenience from the want of it? She answered, no; not in the least; nor could she imagine what advantage he had in the use of his. He inquired, how she did to guide her food in her mouth to eat; she replied, very easily, she could eat before, or on one side or the other as she pleased, but could not explain the manner how. He was very observing to see her eat, but could discern no difference from others in the moving of her jaws, or other motions of her face, nor in her swallowing food, or in drinking: she did both very neatly, and had exactly the same motion in her throat as we have in its passing down.

He was apprehensive the excrescence mentioned in the certificate, might, in some measure, supply the use of a tongue; but she assured him it never moved in the least, and that she spoke as well before it began to grow, which was several years after the cure; and Mr. Hammond convinced him, by trying with their fingers and a spoon, that it was quite fixed and immoveable. He observes further, that she is no ways assisted by a good set of teeth; for she has but few, those bad, and scarcely so high as her gums. He asked her in what part of her mouth her most sensible taste lay? She said it was all over alike; and, smiling, added, she was afraid she was too nice in that; for if her butter was not curious, she eat dry bread.

Mr. Boddington, in another letter to Mr. James Theobald, F.R.S. dated the 14th of April 1742, after giving an account of this young woman in the manner as before, adds, he can recollect nothing more, except her telling him, that though she was able to speak from the very first losing of her tongue, she was not so happy as to her deglutition; for she was unable to swallow any thing solid for many months after, without its being minced very fine, and then thrust into her throat by a finger. But by degrees, she knows not how, she became able to manage without that help, and could eat any thing in the same manner as other persons do. He adds that, in his own mind, he thinks the fleshy excrescence is of great service to her, though he cannot make out in what manner: that for his own part, he had formerly supposed it as impossible to speak without a tongue, as to see without eyes; and therefore expects many who shall hear this account will continue unbelievers, and think he and his friends are all mistaken, that they do not know what they see, and that their ignorance is the only ground of their admiration.

After reciting several more testimonies of the truth of the cases, it is added, that there are several examples of like nature to be met with in medical writers, and those of the greatest authority; one of which, as it has the attestation of a whole university, it cannot be improper to mention here. M. Drelincourt, a very noted physician, tells us, in his Treatise on the Small Pox, of a child 8 years of age, who had lost his tongue by that distemper, and was yet able to speak, to the astonishment of the University of Saumur in France; and that the University had attested it, by drawing up a particular account of the fact, that posterity might have no room to doubt concerning the validity of it. The account is to be met with at large, in the 3d vol. of the Ephemerides Germanicæ, under the title of Aglossostomographia.

Tulpius too makes mention of a man who had the misfortune to have his tongue cut out by the Turks, and yet, after 3 years could speak very distinctly. He says, he went himself to Wesop, a town in Holland, to be satisfied of the truth of it, and found it to be as it was reported. Nay, he does not so much as mention any defect in his speech, but assures us that he could pronounce those letters which depend on the apex of the tongue, even the consonants, very articulately. And this case is still the more worthy attention, because the patient could not swallow even the least quantity of food, unless he thrust it into the œsophagus by means of his finger.

If we go back to earlier times, the emperor Justin, in Cod. Tit. de Off. Præf. Præt. Af. says, he has seen venerable men, qui abscissis radicitus linguis, pœnas miserabiliter loquebantur, whose tongues having been cut out by the roots, they miserably spoke, or complained of the punishments they had suffered. And again. Nonnullos alios, quibus Honorichius Vandalorum rex linguas radicitus præciderat, loquelam tamen habuisse integram, that some others, whose tongues Honorichius, king of the Vandals, had cut out by the roots, yet perfectly retained their speech.

A remarkable Conformation, or Lusus Naturæ, in a Child. By C. Warwick, Surgeon, Truro. N° 464, p. 152.

About April 1741, one Sarah Allen, of the parish of St. Blazy, near Truro, having been married near 4 years, and mother of 2 children, well-formed and living, was brought to bed of the present subject, but of so remarkable and preternatural a constitution, as must render its whole life inevitably miserable, the particulars of which are as follow:

The umbilicus was nearly in its natural site, but somewhat large and prominent, having more the appearance of a tumour, than the ordinary irregular shape of that organ. Immediately below this umbilicus, was a large fungous excrescence, nearly the size of a small egg, but somewhat depressed, of a fiery aspect, and exquisitely sensible. The surface was irregular, being composed of divers granulations or small lobes of flesh; and the basis of it Mr. W. could not well discover, his endeavours being attended with much pain and difficulty; however, from its branchy top, he was inclined to think it somewhat pendulous.

Beneath, adjoining to this fungus, was another pretty large excrescence, neither sensible nor spongy, as the former, but of a solid uniform contexture. Its projection from the abdomen was about one-third of an inch, and if a section were there made parallel to its basis, it would be of an elliptical figure. In shape and dimensions it somewhat resembled the glans penis, its surface being covered with the same fine membrane, and had a small indenture in the top of it, but it was not so large, and had no aperture in it.

Suspended to this glans, like the omentum to the ventricle, was a large membrane of a semilunar figure, loose, flexible, and, when turned up, capable of covering some part of it. Its texture nearly resembled that of the præputium, or was somewhat thicker. There was likewise a small cord or frænum, which arising from the circumference of this membrane, and bisecting the above glans, terminated under the fungus. About half an inch below this membrane, was a wrinkled extuberance resembling a scrotum, but of an uncertain magnitude, great or small, as the descent of the infant's intestines, which having broken their natural confines, form an unseemly roll from one inguen to the other. Its situation was about the upper edge of the os pubis, which, on examining this part, he found greatly deficient, and he was apt to believe, from the great chasm which he perceived there, it must be entirely wanting.

The next thing to be observed was the anus. He found the situation of this part more forward than usual, at least by 2 inches; and he conjectures that the rectum, from this position, must take its course nearly through the chasm of the os pubis.

Besides all these inconveniencies, to complete the child's misery, there was a perpetual distillation of urine from some unseen passages under the fungus, exciting by its acrimony, every moment, pains and excoriations.

To conclude: its sex was so imperfect, and obscurely represented, that it received no baptism till 4 months after it was born; when its parents, flattering themselves that nature might take a turn some time or other for the child's advantage, gave it an appellation applicable to either sex, as time and circumstances should require.

A true Copy of a Paper, in the Hand-writing of Sir Isaac Newton, found among the Papers of the late Dr. Halley, containing a Description of an Instrument for observing the Moon's Distance from the fixed Stars at Sea. N° 465, p. 155.

In fig. 9, pl. 14, PQRS denotes a plate of brass, accurately divided in the limb DQ, into $\frac{1}{2}$ degrees, $\frac{1}{2}$ minutes, and $\frac{1}{10}$ minutes, by a diagonal scale; and the $\frac{1}{2}$ degrees, and $\frac{1}{2}$ minutes, and $\frac{1}{12}$ minutes, counted for degrees, minutes, and $\frac{1}{6}$ minutes. AB is a telescope, 3 or 4 feet long, fixed on the edge of that brass plate. G is a speculum, fixed on the brass plate perpendicularly, as near as may be to the object-glass of the telescope, so as to be inclined 45 degrees to the axis of the telescope, and intercept half the light which would otherwise come through the telescope to the eye. CD is a moveable index, turning about the centre C, and, with its fiducial edge, showing the degrees, minutes, and $\frac{1}{6}$ minutes, on the limb of the brass plate PQ; the centre C must be over-against the middle of the speculum G. H is another speculum, parallel to the former, when the fiducial edge of the index falls on $0^\circ\ 0'\ 0''$; so that the same star may then appear through the telescope, in one and the same place, both by the direct rays and by the reflexed ones; but if the index be turned, the star shall appear in two places, whose distance is showed, on the brass limb, by the index.

By this instrument, the distance of the moon from any fixed star is thus observed: view the star through the perspicil by the direct light, and the moon by the reflexed (or on the contrary); and turn the index till the star touch the limb of the moon, and the index shall show, on the brass limb of the instrument, the distance of the star from the moon's limb; and though the instrument shake, by the motion of the ship at sea, yet the moon and star will move together, as if they did really touch one another in the heavens; so that an observation may be made as exactly at sea as at land.

And by the same instrument, may be observed, exactly, the altitudes of the moon and stars, by bringing them to the horizon; and thereby the latitude, and times of observations, may be determined more exactly than by the ways now in use.

In the time of the observation, if the instrument move angularly about the axis of the telescope, the star will move in a tangent of the moon's limb, or of the horizon; but the observation may notwithstanding be made exactly, by noting when the line, described by the star, is a tangent to the moon's limb, or to the horizon.

To make the instrument useful, the telescope ought to take in a large angle: and to make the observation true, let the star touch the moon's limb, not on the outside of the limb, but on the inside.

The Effects of Cold; with Observations of the Longitude, Latitude, and Declination of the Magnetic Needle, at Prince of Wales's Fort, on Churchill river in Hudson's Bay, North America. By Capt. Christopher Middleton, F.R.S. 1741-2. N° 465, p. 157.

Capt. M. observed, that the hares, rabbits, foxes and partridges, in September, and the beginning of October, changed their native colour to a snowy white; and that for 6 months, in the severest part of the winter, he never saw any but what were all white, except some foxes of a different sort, which were grizzled, and some half red, half white.

That lakes and standing waters, which are not above 10 or 12 feet deep, are frozen to the ground in winter, and the fishes in them all perish. Yet in rivers near the sea, and lakes of a greater depth than 10 or 12 feet, fishes are caught all the winter, by cutting holes through the ice down to the water, and putting lines and hooks in them. But if they are to be taken with nets, they cut several holes in a straight line the length of the net, and pass the net, with a stick fastened to the head line, from hole to hole, till it reaches the utmost extent; and what fishes come to these holes for air, are entangled in the net; and these fish, as soon as brought into the open air, are instantly frozen as stiff as stock-fish. The seamen freshen their salt provisions, by cutting a large hole through the ice in the stream or tide of the river, which they do at the beginning of the winter, and keep it open all that season. In this hole they put their salt meat, and the minute it is immersed under water, it becomes pliable and soft, though before its immersion it was hard frozen.

Beef, pork, mutton, and venison, that are killed at the beginning of the winter, are preserved by the frost, for 6 or 7 months, entirely free from putrefaction, and prove tolerably good eating. Likewise geese, partridges, and other fowl, that are killed at the same time, and kept with their feathers on, and guts in, require no other preservative, but the frost, to make them good wholesome eating, as long as the winter continues. All kinds of fish are preserved in the like manner.

In large lakes and rivers, the ice is sometimes broken by imprisoned vapours; and the rocks, trees, joists and rafters of our buildings, are burst with a noise not less terrible than the firing of a great many guns together. The rocks which are split by the frost, are heaved up in great heaps, leaving large cavities behind; which may be caused by imprisoned watery vapours, that require more room, when frozen, than they occupy in their fluid state. Neither is it wonderful that the frost should be able to tear up rocks and trees, and split the beams of our houses, when we consider its great force and elasticity. If beer or water be left in mugs, cans, bottles, or copper pots, though they were put by our bed-sides, in a severe night they are surely split to pieces before morning, not being able to withstand the expansive force of the inclosed ice.

The air is filled with innumerable particles of ice, very sharp and angular, and plainly perceptible to the naked eye. Capt. M. several times tried to make observations of some celestial bodies, particularly the emersions of Jupiter's satellites with reflecting and refracting telescopes: but the metals and glasses, by the time he could fix them to the object, were covered a quarter of an inch thick with ice, which rendered the object indistinct, so that it is not without great difficulties that any observations can be taken.

Bottles of strong beer, brandy, strong brine, spirits of wine, set out in the open air for 3 or 4 hours, freeze to solid ice. He tried to get the sun's refraction to every degree above the horizon, with Elton's quadrant, but to no purpose, for the spirits froze almost as soon as brought into open air.

The frost is never out of the ground; how deep cannot be certain. They have dug down 10 or 12 feet, and found the earth hard frozen in the 2 summer months; and what moisture is found 5 or 6 feet down, is white like ice. The waters or rivers near the sea, where the current of the tide flows strong, do not freeze above 9 or 10 feet deep.

All the water used for cooking, brewing, &c. is melted snow and ice; no spring is yet found free from freezing, though dug ever so deep down. All waters inland are frozen fast by the beginning of October, and continue so till the middle of May.

The walls of the house they lived in are of stone, 2 feet thick, the windows very small, with thick wooden shutters, which are close shut 18 hours every day in the winter. There are cellars under the house, where are put the wines, brandy, strong beer, butter, cheese, &c. Four large fires are made in great stoves, built on purpose, every day. As soon as the wood is burnt down to a coal, the tops of the chimneys are close stopped with an iron cover: this keeps the heat within the house, though at the same time the smoke makes their heads ach, and is very offensive and unwholesome; notwithstanding which, in 4 or 5 hours after the fire is out, the inside of the walls of the house and bed-places will be 2 or 3 inches thick with ice, which is every morning cut away with a hatchet. Three or 4 times a day they make iron shot of 24 pounds weight red-hot, and hang them up in the windows of the apartments. Though a good fire be in the room the major part of the 24 hours, yet all this will not preserve the beer, wine, ink, &c. from freezing.

For a winter dress, they make use of 3 pair of socks of coarse blanketing, or Duffield, for the feet, with a pair of deer-skin shoes over them; 2 pair of thick English stockings, and a pair of cloth stockings upon them; breeches lined with flannel; 2 or 3 English jackets, and a fur or leather gown over them; a large beaver cap, double, to come over the face and shoulders, and a cloth of blanketing under the chin; with yarn gloves, and a large pair of beaver mittens, hanging down from the shoulders before, to put the hands in, which reach up as high as the elbows; yet notwithstanding this warm cloathing, almost every day, some of the men that stir abroad, if any wind blows from the northward, are dreadfully frozen; some have their arms, hands and face blistered and frozen in a terrible manner, the skin coming off soon after they enter a warm house, and some have lost their toes And their confinement for the cure of these frozen parts, brings on the scurvy in a lamentable manner. Many have died of it, and few are free from that distemper. And notwithstanding all endeavours, nothing will prevent that distemper from being mortal, but exercise and stirring abroad.

Coronæ and parhelia, commonly called halos and mock-suns, appear frequently about the sun and moon here. They are seen once or twice a week about the sun, and once or twice a month about the moon, for 4 or 5 months in the winter, several coronæ of different diameters appearing at the same time. Five or six parallel coronæ, concentric with the sun, are seen several times in the winter, being for the most part very bright, and always attended with parhelia or mock-suns. The parhelia are always accompanied with coronæ, if the weather be clear; and continue for several days together, from the sun's rising to his setting. These rings are of various colours, and about 40 or 50 degrees in diameter.

The frequent appearance of these phenomena in this frozen clime seems to confirm Descartes's hypothesis, who supposes them to proceed from ice suspended in the air.

The aurora borealis is much oftner seen here than in England; seldom a night passes in the winter free from their appearance. They shine with a surprising brightness, darkening all the stars and planets, and covering the whole hemisphere: their tremulous motion from all parts, and their beauty and lustre, are much the same as in the northern parts of Scotland and Denmark, &c.

The dreadful long winters here may almost be compared to the polar parts, where the absence of the sun continues for 6 months; the air being perpetually chilled and frozen by the northerly winds in winter, and the cold fogs and mists obstructing the sun's beams in the short summer they have; for notwithstanding the snow and ice is then dissolved in the low-lands and plains, yet the mountains are perpetually covered with snow, and incredible large bodies of ice continue in the adjacent seas. When the wind blows from the southern parts, the air is tolerably warm; but very cold when it comes from the northward; and it seldom blows otherwise than between the north-east and north-west, except in the two summer months, when they have light gales between the east and the north, and calms.

The northerly winds being so extremely cold, is owing to the neighbourhood of high mountains, whose tops are perpetually covered with snow, which exceedingly chills the air passing over them. The fogs and mists, brought here from the polar parts in winter, appear visible to the naked eye in innumerable icicles, as small as fine hairs or threads, and pointed as sharp as needles. These icicles lodge in the cloaths; and if the faces or hands be uncovered, they presently raise blisters as white as a linen cloth, and as hard as horn. Yet if they immediately turn their backs to the weather, and can bear a hand out of the mitten, and with it rub the blistered part for a small time, they sometimes bring the skin to its former state: if not, they make the best of their way to a fire, and get warm water, with which they bathe it, and so dissipate the humours raised by the frozen air; otherwise the skin would be off in a short time, with much hot, serous, watery matter coming from under along with the skin; and this happens to some almost every time they go abroad for 5 or 6 months in the winter, so extremely cold is the air when the wind blows any thing strong.

It is observed, that when it has been extreme hard frost by the thermometer, and little or no wind that day, the cold has not near so sensibly affected them, as when the thermometer has showed much less freezing, having a brisk gale of northerly wind at the same time. This difference may perhaps be occasioned by those sharp-pointed icicles before mentioned striking more forcibly in a windy day than in calm weather, thereby penetrating the naked skin, or parts but thinly covered, and causing an acute sensation of pain or cold. And the same reason will probably hold good in other places.

It is not a little surprizing to many, that such extreme cold should be felt in these parts of America, more than in places of the same latitude on the coast of Norway; but the difference seems to be occasioned by wind blowing constantly here, for 7 months in the 12, between the north-east and north-west, and passing over a large tract of land, and the exceedingly high mountains, &c. Whereas at Drontheim in Norway, as Capt. M. observed some years ago in wintering there, the wind all the winter comes from the north north-west, and crosses a great part of the ocean clear of those large bodies of ice found here perpetually. At this place they have constantly every year 9 months frost and snow, and unsufferable cold from October till the beginning of May. In the long winter, as the air becomes less ponderous towards the polar parts, and nearer to an equilibrium, as it happens about one day in a week, they then have calms and light airs all round the compass, continuing sometimes 24 hours, and then back to its old place again, in the same manner as it happens every night in the West Indies, near some of the islands.

The snow that falls here is as fine as dust, but never any hail, except at the beginning and end of winter. Almost every full and change of the moon, very hard gales from the north. The constant trade-winds in these northern parts he thinks undoubtedly to proceed from the same principle, which Dr. Halley conceives to be the cause of the trade-winds near the equator, and their variations. For that the cold dense air, by reason of its great gravity, continually presses from the polar parts towards the equator, where the air is more rarefied, to preserve an equilibrium or balance of the atmosphere, is very evident from the wind in those frozen regions blowing from the north and north-west, from the beginning of October till May; for when the sun, at the beginning of June, has warmed those countries to the northward, then the south-east, east and variable winds, continue till October again; and doubtless the trade-winds and hard gales may be found in the southern polar parts to blow towards the equator, when the sun is in the northern signs, from the same principle.

The limit of these winds from the polar parts, towards the equator, is seldom known to reach beyond the 30th degree of latitude; and the nearer they approach to that limit, the shorter is the continuance of those winds. In New England it blows from the north near 4 months in the winter; at Canada, about 5 months; at the Dane's settlement in Davis's Straits, in the 63d degree of latitude, near 7 months; on the coast of Norway, in 64°, not above $5\frac{1}{2}$ months, because blowing over a great part of the ocean, as before-mentioned; for those northerly winds continue a longer or shorter time, as the air is more or less rarefied, which may very probably be altered several degrees, by the nature of the soil, and the situation of the adjoining continents.

The vast bodies of ice met with in the passage from England to Hudson's-bay, are very surprising, not only as to quantity, but magnitude, and as unaccountable how they are formed of so great a bulk, some of them being immersed 100 fathom or more under the surface of the ocean; and a 5th or 6th part above, and 3 or 4 miles in circumference. Some hundreds of these are sometimes seen in a voyage, all in sight at once, when the weather is clear. Some of them are frequently seen on the coasts and banks of Newfoundland and New England, though much diminished. When becalmed in Hudson's-straits for 3 or 4 tides together, Capt. M. has taken a boat, and laid close to the side of one of them, sounded, and found 100 fathom water all round it. The tide flows here above 4 fathom; and he has observed, by marks on a body of ice, the tide to rise and fall that difference, which was a certainty of its being aground. And in a harbour in the island of Resolution, where he continued 4 days, 3 of these isles of ice came aground. He sounded along by the side of one of them, quite round it, and found 32 fathom water, and the height above the surface but 10 yards; another was 28 fathom under, and the perpendicular height but 9 yards above the water.

Capt. M. accounts for the aggregation of such large bodies of ice in this manner: all along the coasts of Davis's-straits, both sides of Baffin's-bay, Hudson's-straits, Anticosh, or Labradore, the land is very high and bold, and 100 fathoms, or more, close to the shore. These shores have many inlets or fuirs, the cavities of which are filled up with ice and snow, by the almost perpetual winters there, and frozen to the ground, increasing for 4, 5, or 7 years, till a kind of deluge or land-flood, which commonly happens in that space of time throughout those parts, breaks them loose, and launches them into the straits or ocean, where they are driven about by the variable winds and currents in the months of June, July, and August, rather increasing than diminishing in bulk, being surrounded, except in 4 or 5 points of the compass, with smaller ice for many hundred leagues, and land covered all the year with snow, the weather being extremely cold, for the most part, in those summer months. The smaller ice that almost fills the straits and bays, and covers many leagues out into the ocean along the coast, is from 4 to 10 fathom thick, and chills the air to that degree, that there is a constant increase to the large isles by the sea's washing against them, and the perpetual wet fogs, like small rain, freezing as they settle on the ice; and their being so deeply immersed under water, and such a small part above, prevents the winds having much power to move them; for though it blows from the north-west quarter near 9 months in 12, and consequently those isles are driven towards a warmer climate, yet the progressive motion is so slow, that it must take up many years before they can get 5 or 6 hundred leagues to the southward; probably some hundreds of years are required; for they cannot well dissolve before they come between the 50th and 40th degree of latitude, where the heat of the sun consuming the upper parts, they lighten and waste in time; yet there is a perpetual supply from the northern parts.

Observations of the longitude, latitude, and the declination of the magnetic needle, at Prince of Wales's Fort, Churchill River.

Having observed the apparent time of an emersion of Jupiter's first satellite at Fort Churchill, on Saturday the 20th of March last 1741-2, at $11^h\ 55^m\ 50^s$.

And as the same emersion happened at London, by Mr. Pound's tables, compared with some emersions actually observed in England near the same, at $18^h\ 15^m\ 10^s$.

Hence the hoary difference of meridians between Fort Churchill and London, comes out $6^h\ 19^m\ 20^s$.

Which converted into degrees of the equator, gives for the distance of the same meridians 94° 50′.

And several other observations gave nearly the same difference of longitude.

The variation of the magnetical needle, or sea-compass, observed at Churchill in 1725, as in N° 393 of the Philosophical Transactions for the months of March and April 1726, was at that time north 21° westerly, and this winter Capt. M. carefully observed it at the same place, and found it no more than 17°; so that it has differed about 1° in 4 years; for in 1738, he observed it here, and found its declination 18° westerly.

The Report of the Committee of the Royal Society appointed to examine some Questions in Gunnery. N° 465, p. 172.

Dr. Jurin having proposed 2 questions in gunnery to be examined, the Society was pleased to appoint a committee for that purpose. The questions were, 1. Whether all the powder of the charge be fired, before the bullet is sensibly moved from its place? 2. Whether the distance to which the bullet is thrown, may not become greater or less, by changing the form of the chamber, though the charge of powder and all other circumstances continue unchanged?

At the meeting of the committee, it was proposed to divide the 1st question into 2 parts. 1. Whether all the powder of the charge be fired? 2. Whether all the powder that is fired, be fired before the bullet is sensibly moved from its place?

As to the 1st part of the 1st question, the committee are of opinion, that all the powder of the charge is not fired. And they found their opinion on the following experiments:

Pieces of paper used for hangings were laid close together on the ground, to the breadth of 10 feet, in the line of a fowling-piece, between it and a frame of 10 feet square, covered over with paper. On pointing the piece towards the middle of the frame, and discharging it several times with and without ball, some powder was always collected, but mixed with a great deal of dirt.

It is however to be observed, that in 2 experiments made the 22d of July, near the Artillery-ground, before the president and some of the fellows of the Society, with a finer sort of powder, in a barrel of 3 feet 9 inches in length, and $\frac{3}{4}$ of an inch bore, with 12 dwts. of powder the 1st time, and 24 dwts. the 2d time, without ball or wadding, no powder could be found scattered on the paper laid before the piece, nor sticking to a board at the distance of about 10 feet, against which the piece was pointed. But when the same powder was fired in a short barrel of $5\frac{1}{2}$ inches in the chace, either with or without ball, some quantity of powder was always collected.

Other experiments were afterwards made before the committee by firing a fowling-piece charged with 5 dwts. of powder, against a sheet of whited-brown paper, at the distance of 2 or 3 yards; the paper was found pierced with several hundred holes, and the jags of the paper appeared on the backside. In a 2d trial with 10 dwts. the paper had more holes in it. A 3d trial was made with 5 dwts. of powder and ball, and then few holes appeared in the paper. In a 4th experiment, made with a short screw-barrel pistol, with a charge of 1 dwt. 2 grs. of powder and a ball, several holes were found in the paper.*

But the irregularities in this manner of collecting the powder unfired, giving reason to suspect, that some powder escaped sideways, beyond the paper laid to receive it, it was proposed to have a machine made, which being close everywhere, except at the end where the muzzle of the piece was to be placed, might thereby hinder the powder from being dissipated. Such a machine was contrived by Mr. Ellicot, and by him presented to the committee, being a frame of wood in shape like a truncated quadrangular pyramid; at the smaller end was a board to receive the shot, and the 4 sides of the machine were covered with thick paper strongly pasted together, and so prepared as to prevent its taking fire. This machine, supported by props, was placed on one of its angles, the carriages for fixing the barrels was placed close to the greater base, which was left open. The results of the several experiments were as follows:

The first 3 experiments were made with a barrel $\frac{4}{5}$ of an inch diameter of the bore, and the length of the chace $5\frac{1}{5}$ inches. The charge each time was 6 dwts. of powder, without ball; the quantities of powder collected were respectively, 1 dwt. 19 grs. 1 dwt. 21 grs. and 1 dwt. 20 grs.

Three other experiments were made with the same piece, and with a 12 dwt. charge, without ball. The quantities of powder collected were 4 dwts. 18 grs. 4 dwts. $2\frac{1}{2}$ grs. and 4 dwts. 22 grs.

The next 3 trials were with the same piece, the charge 6 dwts. with a ball weighing 1 oz. 4 dwts. being a mixture of lead and tin, and fitting the piece exactly. The quantities of powder collected each time were respectively, 1 dwt. 5 grs. 1 dwt. 5 grs. and 1 dwt. 11 grs.

The last 3 experiments with the same piece, were made with a charge of 12 dwts. the weight of the ball as before; and the quantities of powder collected, were found to be 1 dwt. 12 grs. 1 dwt. 9 grs. and 1 dwt. $8\frac{1}{4}$ grs.

The waddings used in all these and the following experiments were of thick leather cut round, to fit the bore of the piece.

* That the paper in these experiments was pierced by the unfired powder, appears, because several grains were found lying behind the frame, to which the paper was fixed, and some few stuck in the paper.—Orig.

The committee then proceeded to examine what alteration might arise from a greater length of chace. The experiments in this case were made with a barrel 3 feet 9 inches in length, and $\frac{3}{4}$ of an inch in the bore; the charges of powder, and weight of leaden balls, were as before.

In the first 3 experiments, with 6 dwts. charge, without ball, the quantities of powder collected were, 3 grs. 9 grs. and 9 grs. respectively. In the next 3 experiments, with 12 dwts. charge, without ball, the quantities of powder collected were 13 grs. 9 grs. and $16\frac{1}{2}$ grs. The 3 following experiments were with 6 dwts. charge, and a ball. The powder collected was 2 grs. 3 grs. and 2 grs.

The last experiments were made with 12 dwts. charge, and ball, as before; the quantities of powder collected from 2 discharges were respectively, 2 grs. and $4\frac{1}{2}$ grs. The frame being broken, a 3d experiment could not be made.

The powder collected after the several discharges was put into separate boxes; it seemed much bruised, and mixed with dirt. Yet several of the parcels being tried, fired with brisk explosions; and some of the powder collected from the experiments with the short barrel, amounting to 6 dwts. 16 grs. being put into the long barrel, and fired with ball, went off with a strong report; and the ball pierced the deal-board, at the end of the frame, and penetrated 2 inches deep into an elm-plank, placed to receive the balls.

Some gentlemen, present at these experiments, suspecting that part of the powder might escape at the open end of the frame; the short barrel was fired with 12 dwts. of powder and ball, as before, through a very large funnel; the quantities found, after 3 discharges, were severally 1 dwt. 2 grs. 16 grs. and 15 grs. Whereas on removing the funnel, and discharging the piece, as before, 1 dwt. 11 grs. was collected, agreeably to former experiments; it seems that the funne had a like effect as lengthening the piece.

Some experiments were also made with the short barrel, filled up with lead, so as to leave but $3\frac{3}{4}$ inches for the chace; the piece being then charged with 12 dwts. of powder, and ball, as before: the surface of the ball was but $\frac{4}{5}$ of an inch within the mouth of the piece, and the powder collected, after 3 discharges, was respectively, 2 dwts. 2 grs. 1 dwt. 17 grs. and 1 dwt. 11 grs.

The barrel being further filled up, so as to leave only $2\frac{4}{5}$ inches for the chace, and charged as before, the ball rising about $\frac{1}{5}$ of an inch beyond the mouth of the piece, the powder collected, after the discharge, was 2 dwts. 6 grs. On a 2d trial, the ball being as much within the mouth, 1 dwt. 16 grs. were collected. And at the 3d trial, the ball being level with the mouth, 2 dwts. 6 grs. were again found.

The committee also caused some experiments to be made of the effect of a touch-hole near the fore-part of the charge. They found, on discharging the short piece of 5¼ inch chace, the charge 12 dwts. and ball, as before, the touch-hole being near the fore part of the powder; the quantities of powder, severally collected, were 1 dwt. 7½ grs.; 1 dwt. 6 grs.; and 1 dwt. 4 grs. And on a discharge made with a little more powder, which filled the barrel exactly to the edge of another touch-hole, the former being screwed up, the quantity collected was 1 dwt. 9 grs.

The effect of firing with heavy slugs was also examined: the weight of the slugs and quantities of powder collected, were as follow; the charge in the short barrel being 12 dwts.

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

The powder used in all these experiments, made before the committee, was presented to them by Mr. Walton, and is such as he makes for the king's service. To ascertain as nearly as possible, that the powder had not undergone any considerable alteration by damps or otherwise, a standard experiment was previously made at every meeting, with the short barrel charged with 12 dwts. of powder, and with a ball of 24 dwts.; and the quantity of powder collected was from 1 dwt. 8 grs. to 1 dwt. 12 grs.; which is as great a regularity as can well be expected. This powder of Mr. Walton being sifted, and divided into a fine and a large sort, the following discharges were made with 12 dwts. of each, and ball as usual:

Discharges with fine powder.	Powder collected.		Discharge with large powder.	Powder collected.	
I	1 dwt.	4 gr.	I	1 dwt.	11 gr.
II	0	21	II	1	16
III	0	12	III	1	21

In the third experiment the bullet, not being so exactly turned as the others, was rammed down with great force.

And the powder being bruised in a mortar, and sifted through a lawn sieve, the charge and ball being as before, what was collected after 3 discharges, was 1 dwt. 10 grs.; 1 dwt. 8 grs; and 17 grs.

Mr. Watson having had two parcels of powder delivered to him, the one fresh, and the other collected after discharges with ball, gave an account of the quantity of nitre he had separated from them, viz.

Separated from 9 dwts. of fresh powder:			From 9 dwt. of powder collected after having been discharged with ball:		
Nitre	6 dwt.	2 gr.	Nitre	4 dwt.	18 gr.
Residuum	2	7	Residuum	2	15
Loss	0	15	Sand, &c.	0	11
			Loss	1	14

Twelve grains of the powder gathered and put into separate boxes, after firing with ball out of the short piece, as before mentioned, being fired in the exhausted receiver, sunk the mercurial gage from $29\frac{1}{10}$ inches to $23\frac{6}{10}$. And the same weight of fresh powder being fired in the same manner, sunk the gage to $22\frac{3}{4}$; the difference being $\frac{17}{20}$ of an inch.

From these experiments the committee are of opinion, that the 1st part of the 1st question, Whether all the powder of the charge be fired? is sufficiently determined in the negative.

As to the 2d part of the 1st question, Whether all the powder that is fired, be fired before the bullet is sensibly moved from its place? The committee are of opinion, that the bullet is sensibly moved from its place before all the powder that is fired, has taken fire.*

This, indeed, has not been determined by any direct experiment, but seems a consequence of the determination of the first part of the question, that the whole of the charge is not fired.

For let it be considered, that from the moment any part of the powder within the barrel takes fire, the flame of the powder already fired is always contiguous to some part of the powder as yet unfired; and consequently some part of this last must be continually taking fire, so long as any unfired powder remains within the barrel; that is, the firing of the powder cannot be over, till all the unfired powder is driven out of the gun: but before any part, how small soever, of the unfired powder is driven out of the gun, the bullet which lies between the charge and the muzzle, must necessarily have been driven out of the gun. Therefore the firing of the powder is not over, or all the powder that is fired, is not fired till after the bullet is driven out of the gun. And consequently the bullet must be sensibly moved from its place, before all the powder that is fired has taken fire.

As to the 2d question, Whether the distance to which the bullet is thrown, may not become greater or less, by changing the form of the chamber, though

* Mr. Robins afterwards determined that the ball is not sensibly moved from its place, when the powder is fired.

the charge of powder and all other circumstances continue unchanged? The committee are of opinion, that the change of the form, in the chamber, will produce a change of the distance to which the bullet is thrown. Their opinion is grounded on the following experiments, in which the longest chamber of equal capacity drove the ball farthest.

Three brass chambers were made, whose depths were respectively 3 inches; $1\frac{1}{2}$ inch; and $\frac{3}{4}$ inch; so turned as to fit the chamber of Mr. Hauksbee's mortar; each of these chambers contained, when full, 1 oz. Troy of powder. The ball was of brass, weighing 24lb. $6\frac{1}{2}$ oz. Avoirdupois, that is, nearly 356 ounces Troy.*

The ball touched the powder of the charge in all these experiments. W the first chamber of 3 inches deep, the elevation of the mortar being 45°, the ranges at 4 different trials were found to be as below.

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

In the 2d of these experiments, the brass chamber, not being sufficiently thrust home before the discharge, was by the violence of the powder driven in so, that it could not be got out again without the help of an iron screw, and a vast force applied to iron wedges. This was doubtless the cause of the great irregularity observed in this case. The mean distance, collected from the other 3 experiments, is nearly 741 feet.

Then 3 discharges were made with the chamber $\frac{3}{4}$ of an inch deep, with ball, powder, and elevation, as before. The ranges were,

Shot.	Chains.	Links.	
I.	7	6	or 466 feet nearly,
II............	7	2	463
III............	7	2	463

The mean distance to which the ball was thrown in these 3 experiments is 464 feet.

The chamber $1\frac{1}{2}$ inch deep, was also tried; but this not fitting the mortar so well as the other 2, the ranges were found to be very irregular, being

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

* Supposing 14 oz. 11 dwts. and 15 grs. and a half Troy, equal to 1 pound Avoirdupois.—Orig.

The last shot, falling so much short, may be ascribed to the damp, it being late in the evening when it was fired. That moisture greatly weakens the effect of powder, is commonly known; and the committee found by an experiment, that powder dried by means of a phial in balneo, and put warm into the chamber, threw the ball twice as far as the same quantity of powder taken out of the same barrel, before it was dried.

Of a Meteor seen near Holkam in Norfolk, August 1741. *By the Right Hon. Thomas Lord Lovell, F. R. S.* N° 465, p. 183.

Some of Lord Lovell's ploughmen, being at work, about the middle of August 1741, on a fair day, at 10 o'clock in the morning, saw on a heath about a quarter of a mile from them, a wind like a whirlwind, come gradually towards them, in a straight line from east to west. It passed through the field where they were at plough, tore up the stubble and grass in the ploughed ground, for 2 miles in length, and 30 yards in breadth. When it came to some closes at the top of a rising ground, some men there saw it appear like a great flash or ball of fire. To some others it appeared as a fire, and some saw only a smoke, and heard such a noise as fire makes when a barn is burning, and the wind making a terrible noise, like that of a violent fire, or like carts over stoney ground, which passed by a house, tearing up the stones in the road; it tore up a rank of pales, sprung several of the posts out of their places and carried a pewter plate that stood on the outside of the window about 40 yards from the house; also a large box-cover, about an inch and a half thick and 4 feet square and cross-barred, was carried away much farther, and torn all to pieces; and the gravel and stones flew about like feathers. It also broke down some fences, and frightened the cattle. And, what is very remarkable, every where else but in this place, the weather was clear and fine, and no sign of any storm or disturbance whatever. There was a strong smell of sulphur, both before and after the wind passed, and the noise was heard a great while after seeing the smoke. They said it moved so slowly forward, as to be near 10 minutes in coming from the closes to the house.

On the Proportions of the English and French Measures and Weights, from the Standards of the same, kept at the Royal Society. N° 465, p. 185.

Some curious gentlemen, both of the Royal Society of London, and of the Royal Academy of Sciences at Paris, thinking it might be of good use, for the better comparing together the success of experiments made in England and in France, proposed some time since, that accurate standards of the measures and weights of both nations, carefully examined, and made to agree with each other, might be laid up and preserved in the archives both of the Royal Society here, and of the Royal Academy of Sciences at Paris: which proposal having been received with the general approbation of both those bodies, they were pleased to give the necessary directions for bringing the same into effect. In consequence of which, Mr. George Graham, Fellow of the Royal Society, did, at their desire, procure from Mr. Jonathan Sisson, instrument-maker in Beaufort-buildings, two substantial brass rods, well planed and squared, and of the length of about 42 inches each, together with 2 excellent brass scales of 6 inches each, on both of which one inch is curiously divided by diagonal lines, and fine points, into 500 equal parts: and on each of the rods Mr. Graham did, with the greatest care, lay off the length of 3 English feet from the standard of a yard kept in the tower of London. He also at the same time directed Mr. Samuel Read, scale and weight maker near Aldersgate, to prepare, in the best manner he could, 2 single Troy pound weights, with 2 piles of the same weight, decreasing from 8 ounces to one quarter of an ounce respectively, 2 parcels of the less corresponding weights, that is, from 5 dwts. to half a dwt., and grain weights from 6 grains to one-fourth of a grain; with 2 single Avoirdupois pound weights: all which, when made, were carefully examined, and found to agree sufficiently with each other. Things being thus provided, the 2 brass rods, one of the 6-inch scales, and one set of all the weights, were sent over to Paris, one of the rods to be returned, and all the other particulars, to be presented for their use, to the Royal Academy of Sciences there: who, on receipt of them, desired the late M. Du Fay, and Abbe Nollet, both members of the Academy, and also fellows of the Royal Society, to see the measure of the Paris half-toise, containing 3 Paris feet, accurately set off on both the brass rods, in like manner as the length of the English yard, containing 3 English feet, had already been set off on the same. After which those gentlemen returned over one of the rods to the Royal Society, with a standard weight of 2 marcs, or 16 Paris ounces, accompanied with a proces verbal, or authentic certificate from the proper office, of the due examination of them.

The rod being returned, Mr. Graham caused Mr. Sisson to divide both the measure of the English yard, and the Paris half-toise, each into 3 equal parts, for the more ready taking off both the English and Paris foot from the same: after which, both this rod and 2 marc weight, sent over from France, were, together with the other particulars before mentioned, carefully laid up in the archives of the Royal Society, where they now remain, as their duplicates do in those of the Royal Academy of Sciences at Paris: but as, before they were so laid up, an accurate examination and comparison of them was made by direction of the council of the Royal Society, the result of the same is here subjoined as follows: that is,

1. The Paris half toise, as set off on the standard in the Royal Society, contains English inches by the same standard 38.355. Whence it appears, that the English yard and foot, is to the Paris half toise and foot, nearly as 107 to 114. For as 107 to 114, so is 36 to 38.35514.

2. The Paris 2 marc, or 16 ounce weight, weighs English Troy grains 7560. Whence it appears, that the English Troy pound, of 12 ounces, or 5760 grains, is to the Paris 2 marc or 16 ounce weight, as 16 to 21: that the Paris ounce weighs English Troy grains 472.5, and that consequently the English Troy ounce is to the Paris ounce, as 64 is to 63.

3. The English Avoirdupois pound weighs Troy grains 7004; hence the Avoirdupois ounce, of which 16 make a pound, is found equal to 437.75 Troy grains: and it follows of consequence, that the Troy pound is to the Avoirdupois pound, as 88 to 107 nearly; for as 88 to 107, so is 5760 to 7003.636; that the Troy ounce is to the Avoirdupois ounce, as 80 to 73 nearly; for as 80 to 73, so is 480 to 438; and lastly, that the Avoirdupois pound and ounce, is to the Paris 2 marc weight and ounce, as 63 to 68 nearly; for as 63 to 68, so is 7004 to 7559.873.

4. The Paris foot, expressed in decimals, is equal to 1.0654 of the English foot, or contains 12.785 English inches.

A Method of making a Gold-coloured Glazing for Earthen Ware; communicated by M. Godofrid Heinsius, Astron. Prof. at St. Petersburg. N° 465, p. 188.

Take of litharge 3 parts; of sand or calcined flint 1: pound and mix these very well together, then run them into a yellow glass with a strong fire. Pound this glass, and grind it into a subtile powder, which moisten with a well saturated solution of silver; make it into a paste, which put into a crucible, and cover it with a cover. Give at first a gentle degree of fire; then increase it, and continue it till you have a glass, which will be green. Pound this glass again, and grind it to a fine powder; moisten this powder with some beer, so that by means of a hair pencil you may apply it on the vessels, for any piece of earthen ware. The vessels that are painted or covered over with this glazing, must be first well heated, then put under a muffle, and as soon as the glass runs, you must smoke them, (afflare debes fumum) and take out the vessels.

Excerpta ex Ephemeridibus Meteorologicis Romanis Anni 1741, observante Didaco de Revillas, Abbate Hieronymiano, in Romana Acad. Math. Prof. et R.S.S. N° 466, p. 193.

This monthly register of the weather at Rome is of no further use now.

Concerning a Water-insect; which, being cut into several Pieces, becomes so many perfect Animals. By J. F. Gronovius, M.D. at Leyden.* N° 466, p. 218.

Some Conjectures concerning the Position of the Colure in the Ancient Sphere. By the Rev. Ebenezer Latham, M.D. and V.D.M. N° 466, p. 221.

This paper is on a draught of the constellation Aries, as it was exactly copied by Dr. White, from a book in the library of Samuel Sanders, Esq. Possibly it may be of some use for determining the famous controversy with respect to Sir Isaac Newton's chronology. Dr. Halley observes, (Philos. Trans. N° 397) "that the dispute is chiefly, over what part of the back of Aries the colure passed. Sir Isaac Newton takes it to be over the middle of the constellation; P. Souciet will have it, that it passed over the middle of the Dodecatemorion of Aries, which by consequence would make it pass about mid-way between the rump and first of the tail;" which situation could never be said to be over the back: whereas, if the ring in this cut was designed, as I apprehend, to image the colure in the ancient sphere, it exactly answers Hipparchus's description—ἐν δὲ τῷ ἑτέρῳ κολȣ́ρῳ φησὶ κεῖσθαι τȣ̃ κριȣ̃ τὰ κατὰ πλάτος, and justifies the construction Sir Isaac put on those words beyond exception. The sculptures from whence this was taken, have the title of Aratæa, sive Signa Cœlestia, in quibus Astronomicæ Speculationes Veterum ad Archetypa vetustissimi Aratæorum Cæsaris Germanici Codicis (44) ob oculos ponuntur a Jacobo de Geyn ex Biblioth. Acad. Lugd. Bat. Amstel. 1652.†

Of an Extraordinary Dropsy. By Tho. Short, M.D. of Sheffield. N° 466, p. 223.

Jan. 1742, Dr. S. was called to visit a woman 30 years of age, who about 7 years before, had a complaint like a severe fit of the stone in her left kidney, with all the common symptoms of a stone, but she recovered again. Three years afterwards she had another fit, but got better in a few days; though she mostly complained of a dull pain in that place ever after. Her menses had been very irregular, and small, since her last paroxysm, and totally obstructed since September; her pulse was very small and quick, her countenance pale and languid; a pain at the pit of her stomach, towards the spleen, besides that in the kidney; her whole stomach and belly full, and somewhat swelled, but harder on

* This is the Polype, (at that time commonly called an *Insect*) of which a sufficient description is given in a paper by M. Trembley, in the following number of the Philos. Trans.; on which account it is not deemed necessary to preserve this article.

† Hug. Grotii Batavi Syntagma Aratæorum: ex offic. Plantin. 4to. See Germanicus's Interpretation, p. 25, the figure of the constellation Aries.—Orig.

the left side than the right; a fluctuation of water or matter among the abdominal muscles, and the peritonæum very hard under it: the right side was full, and softer. She had no appetite, little sleep, a small cough, a little thirst, slight fever, and much pain. He ordered her some laxative, aperient, attenuating, diuretic pills, with an antihydropic stomachic mixture, the country air, and daily moderate riding. She pursued this method a few weeks with some advantage, but not so much as she expected and desired. She then took the advice of another physician, to no better purpose. In April he was consulted again by her. Her flesh was now much shrunk, the belly fuller, pulse quicker, pain the same, urine scanty but pale, appetite languid. He prescribed other things to the same purpose as above, but with no better success. In June he put her on drinking Nevil-Holt water (which last year had cured three of dropsies, which were all that had used it for that purpose) and riding. On this she made water freely, slept tolerably well, had a better appetite, less pain, and was much more chearful; but the swelling of the belly was still the same. Always on turning in bed, she heard and felt a jolting and fluctuating of water in the belly: this put her on being tapped, not doubting but she would recover then. Next day he was sent for to see the water drawn off, July 23, but to his surprize, on the perforation, between 3 and 4 pints of very thick, ropy mixed matter came away; some was matter, some a thick white slime, but the greatest part was a thick reddish-brown liquor, like liver mashed with a little water: it could not get through the canula, without often clearing it with a goose-quill. After this came near 6 pints of clear water or serum, as in a dropsy. She seemed much easier then, and all the afternoon and night; next forenoon not so easy, though she came down stairs to dinner. Quickly after it, she was most severely and violently seized with such excruciating pain all down the left side to the foot, as threw her into the most profuse sweat, often fainting, vomiting, &c. At 4 o'clock, she wholly lost both sense and motion of that thigh and leg; at 5 she was insensible, and at 6 she died, July 24th.

Next day the body was opened, when a monstrous tumour on the left side of the belly showed itself, and a large bag of water on the right side appeared, which two filled the whole cavity. The abdominal muscles on the left side were very large, flabby, bloated, and a livid pale. The peritonæum uncommonly hard, thick, and scirrhous; the liver and spleen both much emaciated; the first not above 2lb. the last about 2 or 3 oz.; the stomach and intestines, from the cardia to the anus, full of small, hard, white, scirrhous knots, like small peas, or hail stones; the intestines of a dusky yellow colour: the remains of the omentum were mortified: the kidneys were sound: the pancreas very small and yellow. The tumor on the left side, which was the ovarium, being cut up, some pints of the same matter as was first drawn off in tapping, ran out: it was divided into innumerable cells, full of different matter, some as above; some white, thick and slimy, some fatty, some purulent, &c. The partitions between the cells very strong, cartilaginous in the middle, so as to resist the knife; like muscular flesh below and above this cartilage, so was each cell. The whole ovarium, before it was first broken, might weigh about 20lb. The bag of water on the right side, was the other ovarium, wherein was nothing but like a large ox bladder, containing 9 or 10 pints of water; like a bladder at the lower end, and rising up like a crooked horn at the other end; the skin was very thin and smooth. The vesica urinaria and uterus were both sound.

From this account, it appears, 1st. that here was a triple dropsy, viz. One intermuscular on the left side of the abdomen; one in the cavity of the belly; one, and the largest of all, in the right ovarium. 2dly, in barren women, and stale maids, tapping should be very cautiously undertaken; especially when the whole belly is not equally distended, and not a free fluctuation of the water heard and observed from side to side, as the sick turn in bed; but especially if there was, or is, a sensible difference to be felt in the hardness or softness of the parts of the belly, before it is greatly distended.

Concerning the Insect mentioned by Gronovius in page* 218 *of this Number of the Original*; *and alluded to in page* 607 *of this Volume of our Abridgment. By a Gentleman of Cambridge.* N° 466, p. 227.

The last news from Paris mentions something very surprising in the account of M. Reaumur's late memoir, read in the Royal Academy of Sciences there, concerning an animal called a polypus, in which life is said to be preserved, after it has been cut into several pieces, so that one animal seems by section to be immediately divided into 2 or 3 more complete animals, each separately enjoying life, and continuing to perform the proper offices of its species.

Such an account would have been less regarded, had we not been informed before, that two letters had been communicated to the Royal Society, some months since, both which mentioned the same thing, and related it as a fact averred, and carefully examined, by one of the greatest judges, and most indefatigable promoters of natural history, and especially of that part of it, which leads to the knowledge of what is most particular and remarkable in the insect and reptile part of the creation.

The most common operations, both of the animal and vegetable world, are all in themselves astonishing; and nothing but daily experience, and constant observation, make us see, without amazement, an animal bring forth another of the same kind; or a tree blossom, and bear leaves and fruit.

* Improperly so called, since it belongs to the class of worms.

The same observation, and daily experience, make it also familiar to us, that besides the first way of propagating vegetables from their respective fruit and seed, they are also propagated from cuttings; and every one knows, that a twig of a willow particularly, cut off and only stuck into the ground, presently takes root and grows, and becomes as much a real and perfect tree, as the original one from which it was first taken.

Here is then, in the vegetable kingdom, the very thing quite common, that M. Reaumur's memoir is said to give a rare example of in the animal. The best philosophers have long observed very strong analogies between these two classes of beings: and the moderns, as they have penetrated farther into nature, have every day found reason to extend that analogy; some have even with great probability talked of a scale of nature, in which she, by an insensible transition, passed from the most perfect of animals, not only to the most imperfect, and thence to the most imperfect of vegetables, but even through coralline bodies, and minerals, to the very earths and stones, which seem the most inanimate parts of our globe.

Now in such a scale, who is the man that will be bold to say, just here animal life entirely ends, and here vegetable life begins? or, just thus far, and no farther, one sort of operation goes, and just here another sort quite different takes its place? or, again, who will venture to say, life in every animal is a thing absolutely different from that which we dignify by the same name in every vegetable? And might not a man even be excused, if he should modestly doubt, whether plants and vegetables may not themselves be considered as a very low and imperfect tribe of animals; as animals might, in like manner, be considered as a more perfect and exalted kind of vegetables?

We see the two sexes of male and female run through all the higher parts of the animal creation; yet would he have gone a great deal too far, who should have thence asserted, there were no exceptions to this general economy; or that this was one of the general and distinguishing affections of all the animal kind: for modern discoveries have informed us, that there is somewhat very analogous to this in the vegetable creation also; and even in the animal it has been found, that snails, earthworms, and some others, are really hermaphrodites, having in themselves the organs of both sexes: while the working bee is truly of no sex at all, nor anywise employed in the production of that species, it labours so hard daily to provide with food.

But whereas in animals, the division of the sexes is almost general; and the union of them in one subject appears but in a few instances; contrarywise, in vegetables, almost all have the whole apparatus of generation in each individual, while only a few sorts seem to emulate animals in what is analogous to the division of them.

I seem perhaps to wander too much from the point first-mentioned; but as I am only offering loose hints, and such wild conjectures as come in my way, hope to be excused, though I yet hazard another observation, which is, that what appears chiefly to be new in the subject of this memoir, is, that the animal or animals live and do well after their separation, and that they are capable of re-producing such parts as the head and the tail, which seemed essentially wanting.

I say, that the animal's living and doing well again, is what is chiefly new; for that an animal, after separation of some of the principal parts, seems for some time to retain life in each part, must have been observed by every body;* and though people generally say, from their prejudice in favour of some of the principles above hinted at, that to be sure only one of the parts, though they know not always which, feels and has the sensation of pain; yet have all that I have ever talked with on the subject, as freely acknowledged, that the phenomena appeared on the other side.

A chicken, or a pigeon, whose head is suddenly struck off, shows in both parts, if no preconceived opinion led us to think otherwise, strong signs of pain and suffering, and the very same signs, that the respective parts of the animal show of that sensation, while it is surely living and entire; and I have been told by some, who have seen the heads of malefactors suddenly severed from their bodies, that the same observation holds also in our own species. But we have all seen it hold much stronger in the more imperfect animals, as they are commonly called, such as worms, where, on the separation of the body

* The ancients have taken notice of this, and some even seem to have had no doubt, that life continued some time in the parts of a divided insect. Aristotle observes in the 4th book of his History of Animals, that almost all insects live some time when pulled asunder, wasps particularly; and that those live longest, when so separated, which have a long body, and many feet; so that the scolopendra being cut asunder, one part moves on forwards, and the other backwards. But a passage of St. Austin is so remarkable on this head, that I cannot help transcribing it. Me revocat quod his hausi oculis ...... Cum enim nuper in agro essemus Liguriæ, nostri illi adolescentes, qui tunc mecum erant studiorum suorum gratia, animadverterunt humi jacentes in opaco loco reptantem bestiolam multipedem, longum dico quemdam vermiculum; vulgo notus est; hoc tamen quod dicam nunquam in eo expertus eram; verso namque stylo, quem forte habebat unus illorum, animal medium percussit: tum ambæ partes corporis ab illo vulnere in contraria discesserunt, tanta pedum celeritate, ac nihilo imbecilliore nisu, quam si duo hujuscemodi animantia forent. Quo miraculo exterriti, caussæque curiosi, ad nos, ubi simul ego et Alypius considebamus, alacriter viventia illa frusta detulerunt. Neque nos parum commoti, ea currere in tabula quaquaversum poterant, cernebamus; atque unum ipsorum, stylo tactum, contorquebat se ad doloris locum, nihil sentiente alio, ac suos alibi motus peragente. Quid plura? tentavimus quatenus id valeret; atque vermiculum, imo jam vermiculos, in multas partes concidimus; ita omnes movebantur, ut nisi a nobis illud factum esset, et comparerent vulnera recentia, totidem illos separatim natos, ac sibi quenquam vixisse crederemus. Aug. Lib. de Quantitate Animæ.—Orig.

into two parts, life has continued seemingly in both, and with strong signs of it, longer than we have had the patience to attend and examine. We have been, indeed, quite uncertain, in which of the parts this seeming life has been most conspicuous; and as both parts have seemed to endeavour to get away, and have frequently soon after been found missing, boys and ordinary people are generally possessed of an opinion, that they unite and grow together again after their separation.

Now, if it could once be allowed, that animal life and sensation might subsist but an instant, in both parts of the creature, after its section; the whole remaining difficulty would be only as to the cure of the wounds, and the reproduction of the necessary organs that are wanting. And, for the first of these, we know very well, that the more imperfect animals are killed with much greater difficulty than the more perfect, their vitals being more diffused, and their general organization being, I suppose, far more simple than that of the higher tribes; and as to the other, I think no one will see any impossibility in the reproduction of certain parts, after what we have seen and read of, in the lobster and cray-fish kinds, who when they chance by any misfortune to lose a claw, reproduce it in a short time with all its joints, and the proper muscles for moving them: all which appears as difficult as the regaining of a mouth and a tail to some of the worm-kind; whose general organization being simple, and consisting chiefly of only one straight gut, or passage, from the mouth to the vent, they seem to want little more to reproduce either, than a contraction of the wound, with the assistance of the muscles that move the several rings of which the body is composed; and every one of which, in its first and natural state, performs almost the same motions as are necessary for suction or ejection: the latter of which we have even sometimes seen very wonderfully supplied in our own species, in those cases, where grievous wounds of the intestines have put nature on trying to perform her operations in a new way.

On the whole, we are all very desirous to see M. Reaumur's memoir on this curious subject; we hope it will soon be published, when, as his curious and exact experiments will afford infinite entertainment, so his judicious remarks upon them will doubtless be no less instructive; but will, in all probability, give a light into these matters we do not yet think of. In the mean time I could not help just mentioning what came into my own head on the occasion, hoping that however you may look on my thoughts as the dreams, perhaps, of a man bewildered in his inquiries into nature, you will still believe me to be a firm and constant lover of truth, and ready at all times to receive and embrace whatever is really such, however odd and surprising it may at the first chance to appear.

I shall therefore only add one or two facts I should indeed have mentioned before, when I was speaking of the difficulty of killing some of the tribes of insects and reptiles; which are, that I have myself seen the heart of a viper continue its regular beats more than 6 hours after it had been taken out of the body; that I have seen that body move and seem alive to all purposes for a great part of the same time, after having lost the heart; and that I have seen wasps, whose heads had been taken off, creeping in the window the next day; and butterflies that have lived, and attempted even to fly, several days after undergoing the same severe operation.

Insects seem at first to suffer but little from the loss of their hinder parts, though these contain most of their viscera; nor does the loss of limbs seem to affect them in any proportion to the more perfect animals. But even in our own kind, in infancy, before the parts have lost all their softness, much greater wounds may be received without loss of life, than afterwards. If we go yet further back to our embryo state, it is very probable, that yet vastly greater hurts are recoverable; and it is on that principle chiefly, that the best and most likely account has been given by modern writers in anatomy, of some ve y remarkable monsters that have appeared in the world, where even some of the most essential parts of two fœtuses have been seen wonderfully united in one and the same body.

A Synopsis of the Calculation of the Transit of Mercury over the Disk of the Sun, Oct. 25, 1743. By Mr. John Catlyn. N° 466 p. 235.

The equal time of the true ☌ at Greenwich	Oct. 24^d	22^h	15^m	58^s
The equation of natural days add			16	11
Apparent time of the true ☌	Oct. 24	22	32	9
At which time the true place of the sun and of Mercury seen from the earth	♏	12°	36′	44″
The geocentric latitude of Mercury	south		9	37
Elongation in 5 hours, i.e. the 2½ immediately preceding and following the ☌			29	16
Difference of latitude in the same time			4	24
Therefore the angle of the apparent way of ☿ with the ecliptic		8	33	0
And the distance of their centres at the time of their nearest approach			9	31
And the motion of interval between that and the ☌......			1	26
And the hourly motion of Mercury in his path over the sun's disk			5	55$\frac{1}{11}$
And the motion of the ½ duration from the first to the last exterior contacts of the limbs......................			13	15

Motion of the same for the interior contacts		13′	4″
Hence, the time of the interval from the ☌ to the middle		14	32
Of ½ the exterior transit	2°	14	22
Of ½ the interior transit	2	12	30
Hence, the first exterior contact of the limbs, Oct. 25, morn	8h	32m	19s
The first interior contact	8	34	11
The nearest approach of the centres, or middle	10	46	41
The last interior contact, afternoon	0	59	11
The last exterior contact, or end of the transit	1	1	3

This computation is made from tables * which give the ascending node of Mercury at the time of this transit 6′ 17″ too forward, according to the result of very accurate observations made of that in the year 1723, by Dr. Halley, Dr. Bradley, and Mr. Graham. Therefore making the calculation with this correction of the place of the node, the times of the several circumstances of the transit will be as follows:

The first exterior contact	8h	29m	21s	
The first interior contact	8	31	5	
The nearest approach of the centres	10	46	6	
The last interior contact	1	1	7	afternoon.
The last exterior contact	1	2	51	afternoon.

This transit may be very aptly compared with that which happened on the 24th day of October 1697;† as happening at the end of a remarkable period in Mercury's motion, by which he is nearly in the same situation, with respect to the sun, at every completion of it. Dr. Halley, in his series of moments, in which Mercury is joined to the sun, &c. published in the Philos. Trans. N° 193, makes the middle of this transit at 11m past 6 in the morning the 24th day, or the 23d day at 18d 11m p. m. and the distance of the centres of the sun and Mercury 10′ 4″.

Only the egress of Mercury, in the transit of 1697, could be observed in Europe;‡ which was done at Nuremberg in Germany, by Mr. Wurtzelbaur, and at Paris by M. Cassini: at Greenwich clouds prevented it. At Nuremberg Mr. Wurtzelbaur observed Mercury to go off the sun's disk § at 8h 45½m mane, about 73½ degrees from the vertex of the sun to the right hand; and M. Cassini observed the same accurately at 8h 10m 24s mane; therefore from

* Philos. Trans. N° 386.—Orig.

† Mean period 46 years 1d. 5h. 43′ 42″.—Orig.

‡ Flamsteed's Hist. Cœlest. lib. ii, fol. 32.—Orig.

§ Vertex to the right, it says, a nadir solis ad dextras; but it is a manifest mistake, as any one on trial may find.—Orig.

the known difference of meridians of these places, the egress must have happened at Greenwich at $8^h\ 1^m$ mane.

The observation of Mr. Wurtzelbaur will greatly avail at coming at the duration of the transit. It is mentioned, that Mercury left the sun's limb 73° 30′ from his vertex to the right. Now at that time at Nuremberg, the angle of the ecliptic with the vertical passing through the sun's centre, was 42° 3′ 5″; therefore the last point of contact on the sun's limb was observed 31° 26′ 55″ from the ecliptic to the south, and consequently his latitude was 8′ 28″ south at that time.

To find the point on the sun's limb of the ingress, in order to come at the duration of the transit, we must be beholden to computation and the theory of Mercury's motion: I have therefore, from the tables from which the above times of the transit of 1743 are drawn, carefully computed his motion along his path crossing the sun's disk, and find that he moved along it after the rate of 5′ 53½″ in an hour, and the difference of latitude in 5 hours 4′ 21″, and his elongation 29′ 7″: therefore the angle of his visible way was 8° 29′ 50″, which doubled, and added to 31° 26′ 55″, gives 48° 26′ 35″, his distance, on the limb of the sun, from the ecliptic, also to the southward at his ingress on it: therefore the nearest approach of his centre to that of the sun was 10′ 19″, and the length of the path run during the transit 25′ 14″, and consequently the time of running it $4^h\ 17^m$, the half of which $2^h\ 8\frac{1}{2}^m$, subtracted from $20^h\ 1^m$, the end of the transit at Greenwich, gives the middle there at $17^h\ 52^m\ 30^s$, earlier by $18\frac{1}{2}^m$ than the series of moments, &c. give it.

Now as the said series makes the middle of the transit of 1743, at $11^h\ 2^m$ mane, and as it corresponds with that of 1697; and the computation of that is $18\frac{1}{2}^m$ too late by the series of moments, &c. it may be reasonably expected, that the same computation for this of 1743 will be so much too late also; and if so, the middle may be set down at $43\frac{1}{2}^m$ past 10, or 44^m at farthest, Oct. 25 in the forenoon.

By computation from the tables abovementioned, with the correction of the node, I make the distance of the centres at the nearest approach in 1697, to be 10′ 33″, but by the observations of Mr. Wurtzelbaur it turns out only 10′ 19″, less by 14″. Should therefore their distance in 1743, computed in the same manner at $9^m\ 10^s$, be as much diminished, the duration of the transit will be protracted no less than $5^m\ 24^s$, and the first contact will be $2^m\ 42^s$ earlier, and the last so much later, than the times abovementioned for them.

In the computation of the transit of 1743, the semidiameter of the sun is supposed 16′ 14½″, and that of Mercury 4½″; but in that of 1697, have taken Mercury's only 3½″, imagining the precise moments of the first and last exterior contacts are not observable; but that the ingress is seen some little time later, and the egress sooner, than the true times. I have all along spoken of the motion of Mercury, without mentioning that of the sun, whereas, in reality, it is that of them both jointly; but as we may suppose the sun to stand still during the transit, it will then be considered as the apparent motion of Mercury alone for that time.

Concerning a Man who lived 18 *Years on Water. By Mr. Robert Campbell of Kernan.* N° 466, p. 240.

About 18 years before, viz. about 1724, John Ferguison, of the parish of Killmelfoord, in Argyleshire, happened to overheat himself on the mountains, in pursuit of cattle, and in that condition drank excessively of cold water from a rivulet, near by which he fell asleep; he awaked about 24 hours after in a high fever; during the paroxysm of the fever, and ever since that time, his stomach loaths, and can retain, no kind of aliment, except water, or clarified whey, which last he uses but seldom, there being no such thing to be had by persons of his condition in that country during many months in the year.

Archibald Campbell of Ineverliver, to whom this man's father is tenant, carried him to his own house, and locked him up in a chamber for 20 days, and supplied him himself with fresh water, to no greater quantity in a day, than an ordinary man would use for common drink; and at the same time took particular care, that it should not be possible for his guest to supply himself with any other kind of food without his knowledge; yet after that space of time, he found no alteration in his vigour or visage.

He is now about 36 years of age, middle stature, a fair and fresh complexion, with a healthy, though not seemingly robust, fresh complexion; his habit of body is meagre, but in no remarkable degree; his ordinary employ is looking after cattle, by which means he needs must travel 4 or 5 miles a day in that mountainous country.

He uses no tobacco; yet seems to discharge as much saliva as others, who do not use stimuli to provoke that evacuation.

If we may judge of his insensible perspiration by the softness and freshness of skin, he is in that respect like other men, and like them sweated with violent exercise; as to the grosser excrements, it did not occur to Mr. C. to inquire about them, but he concluded he discharged none; because the country people, who strongly fancied him supported by supernatural means, would not forget to object this to him, if he evacuated any quantity of gross fæces, with which water is not charged.

This history of this abstemious person he had from Mr. Campbell of Ineverliver, his neighbour in that country, a gentleman of great candour and ingenuity, neither credulous himself, nor anywise inclined to impose on the credulity of others. He had the same account from several others, and confirmed by the belief of the whole country. The man himself he never saw, but the bearer, Mr. Charles Campbell, preacher, has conversed with him, whose veracity might be depended on.

The case appeared very singular, and worthy the notice of men of letters; being an instance to convince us, that a great part of the gross meats which we greedily destroy, is not necessary for the support of animal life; and that there must be some other qualities in the pure element of water, than what have fallen under common observation, since they have supported this man in health and vigour for so many years, and supplied the evacuations necessary in the animal economy.

An Account and Abstract of the Meteorological Observations communicated to the Royal Society, for the Years 1731, 1732, 1733, 1734, *and* 1735. *By Geo. Hadley, Esq. F. R. S.* N° 466, p. 243.

The diaries that continue throughout the said 5 years, are only those kept at Crane-court, Southwick, and Coventry. The Kentish diary for the year 1731 is wanting, and ends with the year 1734. In the former account of the years 1729 and 1730, Mr. H. gives an account of the method and contents of the two first. Mr. Henry Beighton's, from Griff near Coventry, contains the height of the barometer at several times of the day, in inches and decimals, and the weather. That from Upsal by Mr. Celsius, from Hudicksval by Mr. Broman, and from Abo by Mr. Sporing, go no farther than the year 1731; for which year there is also one from Lunden by an anonymous author; for it appears not to be Mr. Conrad Quensel's, whose diary ends in the year 1730, from the same place: it contains observations on the barometer twice a-day, the wind and thermometer, which is a particular one of his own.

Wr. Weidler's diary from Wirtemberg continues to the end of the year 1734. In the year 1732, he alters his method of the barometrical heights, from Paris to London measure, and the days of the month from the new style to the old one, to make them the better correspond with our observations. He gives a very accurate account of the phenomena of several northern lights in the ends of the years 1731 and 1733, and the beginning of the year 1734. His diaries also contain some few astronomical observations, and extraordinary occurrences.

Captain Christopher Middleton's journal of his voyage to Hudson's bay is published already in Philos. Trans. N° 418. The Naples Diary, by Dr. Cyrillus, ends in the year 1732, and also that from New-England by Mr. Dudley.

For the year 1734, that from Dr. Pack, at Canterbury, exhibits at one view, by a table for every month of the year, in the first column, the quantity of rain, and the evaporation: in the 2d, 3d, and 4th, the greatest and least and middle heights of the barometer, thermometer, and hygrometer: in the 5th, the meteors, by variety of marks: in the 6th, the direction and strength of the winds. He gives also a description of the instruments he invented, and made use of, for observing the quantity of rain and evaporation, and the hygrometer, with a draught of each. For the month of January, there is a particular table, containing great variety of observations for every day of that month. There is a letter of his, relating to a chart of the levels of Kent, which, he thinks, are so contrived as to cause a circulation of air from the sea, which is of great use. Mr. Forth's Diary, at large, from Darlington, begins in the year 1737; but he has given an abstract for the 3 preceding years: in which the greatest, least, and middle height of the barometer is given for every month. By a letter of his it appears, that his thermometer stands at 45° when Mr. Hauksbee's stands at 33, which is 12 difference; and I suppose he means they differ so much throughout the scale; so by that rule are his observations reduced to the table. Quere, at what time of the day the observations were made, and where the thermometer was placed; for the mean heights differ but little from those at London, as he observes in his letter. There is an extract of a letter from Signor Didacus de Revillas to Dr. Mortimer, containing an account of the rain that fell at Rome, beginning with August 1734, and ending with July 1735, in Paris measure.

Marquis Poleni's diaries at large, from Padua, end in the year 1730; but he sent an abstract of his observations for the 6 following years, which was published in the Philos. Trans. N° 448.

These are all the manuscript observations communicated to the Royal Society, relating to meteorological observations. Mr. H. has added the observations of the barometer, thermometer, and rain, at Edinburgh, from the 4 volumes of Medical Essays; and Mr. Doppelmaier's Barometrical Observations, from the printed ones at Norimberg, to make the tables as general as he could.*

* For want of knowing the particular heights and situations of the different places of observation, as well as the width and forms of the barometers, and the other instruments, which were then not accurately made; the tables cannot be depended on for any useful purposes; and they are therefore here omitted.

A short Account, by James Parsons, M.D. F.R.S. of a Book entitled, Traité des Sens, &c. By M. le Cat, M.D. F.R.S. Printed at Rouen, 1740, *8vo,* N° 466, p. 264.

This treatise appears to be a part of a physiological work, which the author says is not likely to be soon published; and he has therefore exhibited this part for the use of lovers of philosophy, who might not be so agreeably entertained by the rest of the work, as treating chiefly of the human body, and therefore calculated rather for those of the faculty of medicine.

He says, that he has before established certain general principles of sensation, and that now he proceeds to recount the particular parts with which nature has furnished the animal economy, serving to our different senses; and then expatiates a little on their general utility.

His first chapter treats of the sense of feeling, in which he has compiled all the different phenomena that regard this sense, as those of heat, cold, and other objects of feeling, with the structure of the skin; to which he subjoins some curious histories of peculiar and exquisite feelings.

Tasting is his next subject, where, as in the foregoing chapter, the author has drawn together the several sections relating to it; as, an account of the organs of taste, the mechanism of savours, and the manner of their being varied into compound tastes.

The sense of smelling is discussed in his 3d chapter, where he observes the same method as in the two former, in describing the mechanism of the organs serving to that sense, and accounting for the conveyance of odours to those organs; and for the stimulus of some odoriferous particles causing tears to flow, as well as sneezing caused by a glaring light; and, after making some reflections on the many effects of smells on the human body, and the exquisite sense of smelling in some animals, he recites some curious stories of its peculiar effects.

He proceeds next to treat of hearing, and brings under that head the whole mechanism and doctrine of sounds; the vibrations of all sounding bodies: and from the experiment of holding a candle near any vibrating or sounding body, without the flames being moved or otherwise affected, he argues, that the common air does not produce the sound, but a more subtile fluid better proportioned to the organs of hearing. He then comes to his last section, which treats of seeing: including the structure of the eye, and all the phenomena of vision. He begins it with the doctrine of lights and colours, making use of many experiments and explanations of Sir Isaac Newton.

The principal writers besides, on anatomy and physiology, which our author seems to have had in view, are Du Verney, Willis, Senac upon Heister, and Verduc's excellent book, L'Usage des Parties.

Various Medico-chirurgical Observations. By John Daniel Schlichting, M. et Chir. D. &c. N° 466, p. 270. *An Abstract from the Latin.*

Of these observations, the first relates to the spina ventosa, between which and the lues venerea, Dr. S. thinks there is a great analogy. He has found that mercury used internally and externally (but particularly mercurial inunction over the affected parts), to be as efficacious in many cases of the spina ventosa, as it is in the venereal disease. He directs a small portion of the mercurial ointment to be rubbed upon the affected part twice a day, with the occasional use of a gentle purge to prevent a ptyalism, which, contrary to what is observed respecting the venereal disease, is not beneficial in these cases. This mercurial friction is to be continued (if necessary) for 2 or 3 months. Where, after using the mercurial inunction for some weeks, no material advantage appeared to be gained, in consequence of the carious bone not exfoliating, Dr. S. caused the ulcers to be dilated, and pledgets moistened with tincture of myrrh, and other stimulant and antiseptic dressings to be applied, and kept on with proper bandages; leaving the fulness and hardness, which often remained after the separation of the bone, and the healing of the ulcers, to disperse of itself, or using for this purpose nothing more than the gum-plaster.

In the 2d observation, an account is given of a girl 3 years old, who was attacked with a pleurisy, which terminated in an abscess of the lungs, and difficulty of breathing, which continued for some months; after which there suddenly came on a discharge of pus from the vagina (per vulvam), which discharge was almost continual day and night for nearly 4 months; at the expiration of which time, the patient got perfectly well, with very little assistance from medicine.

In the 3d observation, mention is made of an abscess of the spleen, terminating favourably in a purulent discharge from the vagina.

The 4th observation relates to an emission of blood in coitu, instead of semen. This happened in a young man 26 years of age, who, 4 years before, had laboured under a virulent gonorrhœa, and who, for the space of 12 months, had taken not only strong cathartics, but strong diuretics also. This discharge of blood from the urethra was not accompanied with pain, and never happened extra coitûs aut pollutionis tempus. Finding that the most powerful astringents and various other medicines (among which was mercury given until it produced a salivation) were of no use, the disorder was deemed incurable, and was accordingly left to itself. Dr. S. asks, whether the hemorrhage might not be owing to a ruptured blood-vessel, from an ulcer in the prostate, or in one of the vesiculæ seminales?

In the 5th observation, an account is given of an abscess in the hip-joint, with a separation of the head of the os femoris. This happened in a girl 14 years of age. The hip-joint swelled, suppurated, and burst. The aperture was dilated by the surgeon, who extracted the whole head of the os femoris; after which he introduced into the cavity of the abscess tincture of myrrh, and the fuscum ung. fel. W. applying a tight bandage, and seldom removing the dressings. In the course of 6 weeks it healed up, so that the girl could walk easily, but not without limping.

The 6th observation gives an account of a spurious aneurism, without pul sation, containing liquid blood. This occurred in the right arm of a woman, and was consequent to phlebotomy, which had been performed in that arm a year before. At the time when Dr. S. saw the patient, the arm was exceedingly inflamed, and so much swelled as to measure 32 inches in circumference. The tumour extended from the lower part of the shoulder almost to the wrist. On the inside there appeared a small ulcer in a gangrenous state, which showed that the tumour was ready to burst: it felt like a bladder distended with water or other fluid, was without pulsation, and was so dense as not to yield in the least to pressure. It was scarcely possible to feel any pulsation at the wrist. Some were of opinion that this enlargement of the arm was occasioned by a steatomatous tumour (fungus adiposus); but Dr. S. and others who were assembled in consultation, suspected it to be a spurious aneurism. A roller was wrapped round the whole arm; and it was agreed to wait for the breaking of the tumour, which happened 3 days after; when on taking off the roller, there instantly spirted forth more than a pint (1lb.) of blood. The hemorrhage was stopped by the application of the fungus bovist. and proper bandages. Two hours afterwards it was determined, in consultation, either to amputate the arm, or to take up the artery. The last of these measures being preferred, a tourniquet was applied, and an incision made in the sound part above the aneurism, and nearly in the middle of the arm, through the integuments and belly of the biceps muscle, and a needle and thread were passed under the artery, which was thus secured by ligature. The aneurism was then laid open from top to bottom, whereupon there flowed out a vast quantity of liquid blood, amounting to as much as 4lb. No arterial sac was discovered, as in the case of a true aneurism, nor any polypus or coagulated blood, as in the case of a spurious aneurism; but a cavity of a very different kind had been found between the cutis and muscles, and among the muscles themselves; and the muscles of the lower arm were displaced from each other, as though they had been separated by art; they had become pale, having lost their natural colour and appearance; and there was found adhering to them a small quantity of a gelatinous or mucous matter, which after being scraped off with the fingers and washed in water, became white. At the sides of the aforesaid cavity, the blood was seen to issue from 6 or 7 different orifices, to close which the strongest styptics were applied, and the whole cavity was filled with lint, secured by sticking-plaster and a roller. By these means the hemorrhage was soon stopped; nevertheless, a train of unfavourable symptoms (the consequence of the previous disease) came on, and continued increasing until the 3d day, when the patient died.

Two Observations, by Job Baster, M. D. F. R. S. N° 466, p. 277. *An Abstract from the Latin.*

In the first of these observations, mention is made of a male infant, who was born with a pendulous tumour formed on the back, where the os sacrum begins, and reaching from thence down to the heels. On handling, it appeared to contain a watery fluid. Although from its rosy complexion the infant had the appearance of health, yet it died a few days after it was born. Dr. B. could not obtain permission to examine the tumor after death.

The 2d case was that of a child who died of hydrocephalus, at the age of 2 years and a half: during all which time it took no other sustenance besides its mother's milk. The father was healthy, but the mother was of a bad habit of body. From the time of its birth the head was unusually large, and went on gradually increasing, till it became so exceedingly bulky, that the child was unable to support its weight, and was therefore obliged to be constantly in a recumbent posture. The dimensions of the head, as taken after death, were as follow: from the right meatus auditorius over the ossa bregmatis, to the left meatus auditorius, 20½ Rhinland inches; from the root of the nose to the first vertebra 20 inches; a thread drawn all round the head (beginning from the root of the nose, passing across the os frontis, the temporal bones and occiput, and meeting again at the same point in the forehead) measured above 25 Rhinland inches. On opening the head, and carefully removing the dura mater, the pia mater appeared exceedingly thin and quite transparent, inclosing a great quantity of a watery humour as clear as crystal, through which the basis of the brain could be seen; the basis of the brain, for the substance of the brain was so compressed that there was no appearance of brain, but only a strong membrane, thicker in some places, and thinner in others. The 3 cavities (ventricles) of the brain formed one cavity, where the medulla oblongata and cerebellum, but incredibly small, were seen. No vestiges appeared of the nates, testes, cerebri vulva; nor of the protuberantiæ cerebelli; nor of the medulla spinalis. The contained fluid weighed 6lb. 11 oz. While the child lived, the vital and natural actions were performed, but it seemed incapable of any animal action. It was constantly quiet and drowsy, without crying, was deaf, and died without any convulsion or apparent struggle.

Some Papers lately read before the Royal Society concerning the Fresh-water Polypus; an Insect which has this surprising Property, that being cut into several Pieces, each Piece becomes a perfect Animal, as complete as that of which it was originally only a Part. Collected and published by Cromwell Mortimer, M.D. &c. Sec. R. S. N° 467, p. 281.

[The history of the Polype (which is not an insect, but an animal of the zoophyte tribe), is now so well known to all naturalists, that it cannot be thought necessary to repeat all that has been said of it in the Philos. Trans. It will be sufficient to give Mr. Trembley's paper only; he being (after Leuwenhoek) the first observer of the animal.]

Observations and Experiments on the Fresh-water Polypus, by M. Trembley, at the Hague. Translated from the French, by P. H. Z. F.R.S.* N° 467, p. 283.

The animal in question is an aquatic insect, and was mentioned in the Philos. Trans. for the year 1703, N° 283, and N° 288.

It is represented in fig. 10, pl. 14, as sticking to a twig. Its body AB, which is pretty slender, has on its anterior extremity A, several horns AC, which serve it instead of legs and arms, and which are yet slenderer than the body. The mouth of the polypus is in that anterior extremity; it opens into the stomach, which takes up the whole length of the body AB. This whole body forms but one pipe; a sort of gut, which can be opened at both ends.

The length of the body of a polypus varies according to its different species, and according to many other circumstances, to be mentioned hereafter.

M. Trembley knows 2 species, of which he has seen some individuals extend their bodies to the length of an inch and a half; but this is uncommon. Few are generally found above 9 or 10 lines long; and even these are of the larger

* M. Trembley, an ingenious observer at Geneva, is eminent for his discovery of the green and long-armed polypus, with some other species, of which he first observed the extraordinary power of reproduction by cuttings, and of which he published the natural history, illustrated by elegant plates.

kind. The body of the polypus can contract itself, so as not to be above a line or thereabouts, in length. Both in contracting and extending itself, it can stop at any degree imaginable, between that of the greatest extension, and of the greatest contraction.

The length of the arms of the polypus differs also according to the several species: those of one of the species can be extended to the length of 7 inches at least. The number of legs or arms is not always the same in the same species. We seldom see in a polypus, come to its full growth, fewer than 6. The same may be said of the extension, and of the contraction of the arms, as was said concerning the body. The body and the arms admit of inflexions in all their parts, and that in all manner of ways. From the different degrees of extension, contraction, and inflexion, which the body and the arms of the polypus admit of, result a great variety of figures, which they can form themselves into.

These insects do not swim; they crawl upon all the bodies they meet with in the water; or on the ground, on plants, on pieces of wood, &c. Their most common position is, to fix themselves by their posterior end B, to something, and so stretch their body and arms forwards into the water.

They make use of their progressive motion, to place themselves conveniently, so as to catch their prey. They are voracious animals: their arms extended into the water, are so many snares which they set for numbers of small insects that are swimming there. As soon as any of them touches one of the arms, it is caught. The polypus then conveys the prey to its mouth, by contracting or bending its arm. If the prey be strong enough to make resistance, he makes use of several arms. A polypus can master a worm twice or thrice as long as himself. He seizes it, he draws it to his mouth, and so swallows it whole. If the worm come endways to the mouth, he swallows it by that end; if not, he makes it enter double into his stomach, and the skin of the polypus gives way. The size of the stomach extends itself, so as to take in a much larger bulk than that of the polypus itself, before it swallowed the worm. The worm is forced to make several windings and folds in the stomach, but does not keep there long alive; the polypus sucks it, and after having drawn from it what serves for his nourishment, he voids the remainder by his mouth, and these are his excrements. According as the weather is more or less hot, the polypus eats more or less, oftener or otherwise.

They grow in proportion to what they eat; they can bear to be whole months without eating, but then they waste in proportion to their fasting.

The observations in the Philos. Trans. principally concern the manner in which these insects multiply. What is there said of them, is true and exact.

The more we search into the manner how a polypus comes from the body of its parent, the more we are persuaded, that it is done by a true vegetation. There is not on the body of a polypus any distinguished place, by which they bring forth their young. M. T. had some of them, that greatly multiplied under his eyes, and of which he can almost say, that they have produced young ones, from all the exterior parts of their body.

A polypus does not always put forth a single young one at a time; it is a common thing to find those which produce 5 or 6: he had some which put forth 9 or 10 at the same time, and when one dropped off, another came in its place. These insects seem so many stems, from which issue many branches. He learned by a continual attention to 2 species of them, that all the individuals of these species produce young ones.

He had for 2 years under his eye thousands of them; and though he observed them constantly, and with attention, he never observed any thing like copulation. On supposition, that this copulation is performed in some secret manner, he tried at first to be sure it had not place between 2 of them, after they were severed from the body of their parent. To this end, he took young ones, the moment they came from the parent, which was alone in a glass; or he even parted them with scissars. Each of these young ones he put into perfect solitude, and fed them every one separately in a glass; they all multiplied, not only themselves, but also their offspring, which from generation to generation, as far as the 7th, were all confined to solitude with the same precaution.

Another fact, which he observed, has proved that they have the faculty of multiplying, before they are severed from their parent. He has seen a polypus, still adhering, bring forth young ones; and those young ones themselves have also brought forth others. On supposition, that perhaps there was some copulation between the parent and young ones, while they were yet united; or between the young ones coming from the body of the same parent; he made divers experiments, to be sure of the fact; but not one of those experiments ever led him to any thing that could give the idea of a copulation. The polypus multiplies more or less, as he is more or less fed, and as the weather is more or less warm. If plenty of food, and a sufficient degree of warmth concur, they multiply prodigiously.

He next proceeds to the singularities resulting from the operations he tried upon them. If the body of a polypus be cut into 2 parts transversely, each of those parts becomes a complete polypus. On the very day of the operation, the first part, or anterior end of the polypus, that is the head, the mouth and the arms; this part lengthens itself, it creeps and eats.

The second part, which has no head, gets one; a mouth forms itself, at the anterior end, and shoots forth arms. This reproduction comes about more or less quickly, according as the weather is more or less warm. In summer, he has seen arms begin to sprout out 24 hours after the operation, and the new head perfected in every respect in a few days. Each of those parts thus become a perfect polypus, performs absolutely all its functions. It creeps, it eats, it grows, and it multiplies; and all that, as much as a polypus which never had been cut.

In whatever place the body of a polypus is cut, whether in the middle, or more or less near the head, or the posterior part, the experiment has always the same success. If a polypus be cut transversely, at the same moment, into 3 or 4 parts, they all equally become so many complete ones.

The animal is too small to be cut at the same time into a great number of parts; he therefore did it successively. He first cut a polypus into 4 parts, and let them grow, next he cut those quarters again; and at this rate he proceeded, till he had made 50 out of one single one: and here he stopped, for there would have been no end of the experiment. He has several parts of the same polypus, cut into pieces about a year before; since which time, they have produced a great number of young ones.

A polypus may also be cut in two, lengthways. Beginning by the head, one first splits the head, and afterwards the stomach: the polypus being in the form of a pipe, each half of what is thus cut lengthways forms a half-pipe; the anterior extremity of which is terminated by the half of the head, the half of the mouth, and part of the arms. It is not long before the two edges of those half-pipes close, after the operation. They generally begin at the posterior part, and close up by degrees to the anterior part. Then each half-pipe becomes a whole one, complete: a stomach is formed in which nothing is wanting, and out of each half-mouth a whole one is formed also.

He has seen all this done in less than an hour; and that the polypus, produced from each of those halves, at the end of that time did not differ from the whole ones, except that it had fewer arms; but in a few days more grew out. He has cut a polypus lengthways, between 7 and 8 in the morning; and between 2 and 3 in the afternoon, each of the parts has been able to eat a worm as long as itself.

If a polypus be cut lengthways, beginning at the head, and the section be not carried quite through; the result is, a polypus with two bodies, two heads, and one tail. Some of those bodies and heads may again be cut lengthways, soon after. In this manner he has produced a polypus that had 7 bodies, as many heads, and one tail. He afterwards at once cut off the 7 heads of this new hydra: seven others grew again; and the heads, that were cut off, became each a complete polypus.

He cut a polypus transversely, into 2 parts: he put these 2 parts close to each other again, and they reunited where they had been cut. The polypus, thus reunited, eat the day after it had undergone this operation: it afterwards grew, and multiplied.

He took the posterior part of one polypus, and the anterior of another, and brought them to reunite in the same manner as the foregoing: next day, the polypus that resulted, eat: it had continued well 2 months after the operation: grew, and put forth young ones, from each of the parts of which it was formed. The two foregoing experiments do not always succeed; it often happens, that the 2 parts will not join again.

To comprehend the following experiment, we should recollect, that the whole body of a polypus forms only one pipe, a sort of gut, or pouch. He has been able to turn that pouch, that body of the polypus, inside-outwards; as one may turn a stocking. He had several by him, that have remained turned in this manner; their inside is become their outside, and their outside their inside: they eat, they grow, and they multiply, as if they had never been turned.

Facts like these, to be admitted, require the most convincing proofs. He asserts he is able to produce such proofs. They arise from the detail of his experiments, from the precautions he took to avoid all uncertainties, from the care he used to repeat the same experiment several times, from the assiduity and attention with which he observed them.

These animals are to be looked for in such ditches where the water is stocked with small insects. Pieces of wood, leaves, aquatic plants, in short, every thing is to be taken out of the water, that is met with at the bottom, or on the surface of the water, on the edges, and in the middle of the ditches. What is thus taken out, must be put into a glass of clear water, and these insects, if there are any, will soon discover themselves; especially if the glass is let stand a little, without moving it; for thus the insects, which contract themselves when they are first taken out, will again extend themselves when they are at rest, and thus become so much the more remarkable. In order to feed them, we must know how to provide ourselves with insects fit for their food.

Some Considerations for determining whether Pendulums are disturbed in their Motions by any Centrifugal Force. By the Marquis John Poleni, F.R.S. N° 468, p. 299. *From the Latin.*

Sig. Poleni observes that the method used in discovering the centrifugal force, has always been, to compare observations made in countries at a vast distance from each other. But he here considers whether the same end may not be obtained in the same country, or without change of place.

To this end, he first relates what the learned Huygens has laid down, in his "Dissertation on the Cause of Gravity," when he endeavoured to discover, how much a pendulum ought to be shortened, which is carried from France to the equator. And then he states his own contrivance. But Huygens's contrivance need not be repeated here, as it can be seen in his book. Nor does it seem proper to detail the particulars of Sig. Poleni's construction, as nothing has ever resulted from it, and as the matter has been long since settled, as to the variation in the length of pendulums depending on the cause in question.

Observationes Astronomicæ habitæ in Collegio Pekinensi a Patribus Societatis Jesu, a Mense Novembri 1740, *a Do. Jacobo Hodgson, R. S. S. cum Regia Societate communicatæ.* N° 468, p. 306.

These Chinese observations are of no use now.

Account, by John Van Rixtel, F. R. S. of Mr. W. Kersseboom's Second and Third Treatise, confirming the Manner how to know the probable Quantity of People in the Provinces of Holland and West-Friezland, besides a Foundation on which to prove the probable Lives of Widows, and likewise a Rule to know the Duration of Marriages.* N° 468, p. 315.

Mr. Kersseboom having advanced in his first Treatise, printed anno 1738, that the provinces of Holland and West-Friezland contained 980,000 souls, of all ages, on a well-grounded supposition, that annually are born in the said two provinces 28000 children alive; but it having been the opinion, that this should be more clearly demonstrated, he has thought it necessary to comply with the same. In order to which, the author has divided the provinces into 3 general divisions, distinguished with the letters A, B, C; and supposes on good grounds, that in the first division marked A, are born alive annually 3890 children, B ditto 19070, and C ditto 5040, making all together annually 28000 children.

And, as it has been proved in his first treatise, that for every child that is born, the whole number of people is 35 times as many; so it will prove, that these numbers being multiplied together, it renders 980,000 souls.

But as it was impossible for the author to get an exact account, from all places, of the births, weddings, and burials, (from which last two the first is

* See an account of the first part, Philosophical Transactions, N° 450.—Orig.

to be cited and proved) he gives the chief observations he was able to obtain; and believes that these, joined with those contained in his first treatise, will be a sufficient proof of his general calculations.

Mr. Kersseboom then gives an account how many people were buried in the city of Dort every year, from 1700 to 1739 inclusive, amounting, in 40 years, to 28977 persons; which is annually, on an average, 724.—The marriages are 202 couple annually, during the same time, which should produce (according to the author's calculations in his first treatise) 325 children per 100 marriages, and consequently 656 children per annum; but he has found it, on an average, to be 651.—This city being a sea-port, and driving a large trade to Scotland, and on the Rhine, and consequently many of the people, whose traffic brings them to Dort, may die there, it is supposed, that about 680 children are born annually there, and that consequently this city may contain 24000 souls.

Next to this, the author gives an account of Haerlem, how many people died there in 84 years, from 1656 to 1739 inclusive, namely, 132132 persons, which is annually, on an average, 1573.—The next is, how many marriages, from anno 1690 to 1739 inclusive, namely, 21910, is annually 438, on an average. —As to the births, Mr. Kersseboom refers to his first treatise, p. 54, where he supposes, that 1450 children may be born alive annually; and endeavours to demonstrate it further, by giving an account of the births for 60 years, namely, from 1680 to 1739, and finds it to be 1453; from which it is calculated, that this city contains 50500 souls, as mentioned in his first treatise.

The next account is that of the burials of Delft and Delftshaven, from the years 1724 to 1739, being 15 years, and is found to be annually, on an average, 723 persons; but there is subjoined, for the greater certainty, an account from the year 1696 to 1739, which proves it to be 748 persons annually.

The marriages are, in the same time of 44 years, on an average, 224 per annum, which should produce 728 children, according to the rule laid down before, namely, 100 marriages producing 325 children; but is found to produce from 1690 to 1739 inclusive, to be 648 per annum, on an average; from whence it is supposed those two places contain 25000 souls.

The city of Leyden comes next in consideration. It appears by a list for 50 years, namely, from the year 1690 to 1739 inclusive, that there have been buried in that city annually, on an average, 1919 persons; and married during the same time, annually, on an average, 558 couple, which, agreeable to the former rule, would produce 1813 children per annum, but is found to have been 1834 per annum, on a medium, as aforesaid; the author concludes consequently, that this city contains 63000 souls.

The next city is Amsterdam: it appears by a list, that since the year 1696 to

1738 inclusive, there have been buried in this city 7323 persons annually (Jews excepted); and there having been married, during the same time of 43 years, 2311 couple annually, produced, according to the author's computation, 7134 children annually, at a medium; and takes it thence for certain, that Amsterdam contains (including 20000 Jews, as observed in his first treatise, p. 21) 241000 souls.—The author proceeds, in the like manner, about other places. He then gives a table showing how long 432 widows lived during a century, and finds it to have been near 14 years each on a medium; and then subjoins a list how many years married people of different ages continue to live probably together, before the bonds of matrimony, by the death of either party, are dissolved; namely,

		live between
Those whose ages together are	40 ..	24 and 25 years.
....	50 ..	22 and 23.
....	60 ..	20 and 21.
....	70 ..	19 and 20.
....	80 ..	17 and 18.
....	90 ..	14 and 15.
....	100 ..	12 and 13.

And finishes with rejecting the method of calculating the quantity of people after the manner of Vossius, Auzout, Petty, and others.

The third treatise contains, 1st, A copy of a letter written by the author in the beginning of the year 1741, to Mr. John Eames, F.R.S. 2dly, A demonstration, in 29 tables, that Mr. Simpson's calculation of lives, as 1 to 26, is a mistake, and his own hypothesis, as 1 to 35, right; and proves, from Mr. Maitland's Observations, that children in London, of 2 years old, continue to live, on a medium, above 37 years; and observes, that Dr. Halley's table has it full 38 years and a half.

The author supposes, 3dly, That out of every 100 children born, 5 come dead into the world; and that out of every 100 children born alive, near 20 die under a year old; and he shows, 4thly, how much Mr. Simpson differs in his calculation; namely, That full 32, out of 100 children, die under a year old.

The rest of this treatise consists in divers calculations and tables of interest, and the value of annuities for life on different ages and interest; and concludes with an explanation of the same, and the usefulness thereof.

Concerning the wonderful Increase of the Seeds of Plants, e. g. of the Upright Mallow. By Mr. Joseph Hobson of Macclesfield. N° 468, p. 320.

In the upright mallow, the seeds being disposed in rings, Mr H. counted those which were on the principal stems, and found them as follows.

Rings in all	10199	
Multiply by seeds in one ring	12	Seeds.
Number of seeds	122388	
Allow for two large stems destroyed	7612	
Seeds in all	130000	

He then counted the seeds in several particular rings, and found them commonly 14 in each, but has confined himself to multiply the rings by 12, which is moderate, yet makes the number of seeds amount to 130000, allowing 7612 seeds for 2 large stems cut down and destroyed, a moderate allowance, considering 2 of the stems alone contain each above 1000 rings: some of these stems were above 2 yards and a half high. This plant was a seedling last year, transplanted out of the fields on the end of a sloping strawberry-bed; and he counted the rings in the middle of July, when it had thousands of flowers upon it, which, with thousands that must still succeed, might very probably produce more than 50000 seeds more, even supposing many of the flowers to produce no seed, considering 1000 rings contain 12000 seeds and more; and if we multiply the number of rings actually counted, by 14, the number of seeds contained in one ring, instead of 12, we shall have an addition of 20000 seeds, all which, added together, amount to 200000, the possible increase of one seed.

On the Nature of Amber. By John Ambrose Beurer. N° 468, p. 322. *From the Latin.*

M. Beurer cannot admit that amber is the resinous juice of a tree, for these reasons: Is it probable that amber should pass out of the earth into the sea? or whence is that passage? since the trees are not near the sea.

Again, can this resin pass through the earth like water, or diffuse itself so copiously through it? If this were possible, would it not rather grow stiff, and adhere to the surface of the earth.

Besides, the heat of the sun can never cause such a flood of resins, as to fill several subterraneous tracts. For resins exude by drops, the least part of which only reach the ground, the greater part adhering to the bark of the tree. Besides, amber is often found on mountains, and in pits, where trees were never planted. And as to the arguments from the distillation of vitriolic acid with turpentine, they do not prove the case; for though something bituminous is then produced, it is not real amber, as it wants the equal mixture, transparence, elasticity, and hardness. This may be easily and quickly produced, by the mixture of any distilled ethereal oil, coagulated with vitriolic acid; from which mixture there presently arises something bituminous, but not amber.

Amber probably derives its origin from a mineral, viz. from a soft bitumen (oleum naphtæ) and a sulphureous vitriolic acid, which mix in the form of steam, and presently harden.—This is also proved by the fossil amber; for wherever this is dug up, there are also found, among the blue clay, bituminous wood, coal, vitriol, and often alum. And the amber found in the sea, is produced in the same manner as that formed in the mountains, being only washed out of the earth by the beating of the waves, and partly lost in the deep, partly thrown up on the banks.

Further, as the vitriolic acid, with the bitumen, produces the form and semblance of amber; that acid will quite dissolve it again, and leave it in the same state, without destroying any constituent part, reducing its hardness, transparence, and elasticity.

An Account of a Book intitled, A Treatise of Fluxions, in Two Books. By Colin Maclaurin, A. M., F. R. S. 2 Vols. 4to.* N° 468, p. 325.

The author's first design, in composing this treatise, was to establish the method of fluxions on principles equally evident and unexceptionable with those of the ancient geometricians, by demonstrations deduced after their manner, in the most rigid form, and by illustrating the more abstruse parts of the doctrine, to vindicate it from the imputation of uncertainty or obscurity. But he has likewise comprehended in this work the application of fluxions to the most important geometrical and philosophical inquiries. It consists of an introduction, and two books. In the introduction he gives an abstract of the discoveries of the ancients in the higher parts of geometry, with observations on their method, and those that first succeeded to it. The first book treats of fluxions in a geometrical method, and the second treats of the computations.

In the introduction we have an abstract not only of the discoveries of the ancients in the higher parts of geometry, but likewise of their demonstrations. After an account of the propositions of this kind, that are to be found in the

* This very able and masterly account was probably the composition of the excellent author himself.

twelfth book of Euclid, there follows a summary of what is most material in the treatises of Archimedes, concerning the sphere and cylinder, conoids and spheroids, the quadrature of the parabola and the spiral lines. The demonstrations are not precisely in the same form as those of Archimedes, but are often illustrated from the elementary propositions concerning the cone, or corollaries from them, after the example of Pappus, (Coll. Math. Prop. 21st, lib. 4) from whom a proposition is demonstrated, and rendered more general, concerning the area of the spiral generated on a spherical surface by the composition of two uniform motions, analogous to those by which the spiral of Archimedes is described on a plane. This area, though a portion of a curve surface, is found to admit of a perfect quadrature, and this proposition concludes the abstract. He takes occasion from these theorems to demonstrate some properties of the conic sections, that are not mentioned by the writers on that subject; and there are more of this kind described in the 11th and 14th chapters of the first book.

It is known, that if a parallelogram, circumscribed about a given ellipse, have its sides parallel to the conjugate diameters, then shall its area be of an invariable or given magnitude, and equal to the rectangle contained by the axes of the figure; but this is only a case of a more general proposition. For if, upon any diameter produced without the ellipse, you take two points, one on each side of the centre at equal distances from it, and the four tangents be drawn from these points to the ellipse, those tangents shall form a parallelogram, which is always of a given or invariable magnitude, when the ellipse is given, if the ratio of those distances to the diameter be given; and when the ratio of those distances to the semidiameter is that of the diagonal of a square to the side, (or of $\sqrt{2}$ to 1) the parallelogram has its sides parallel to conjugate diameters. It is likewise shown here, how the triangles, trapezia, or polygons of any kind are determined, which, circumscribed about a given ellipse, are always of a given magnitude.

There is also a general theorem concerning the frustum of a sphere, cone, spheroid, or conoid, terminated by parallel planes, when compared with a cylinder of the same altitude on a base equal to the middle section of the frustum made by a parallel plane. The difference between the frustum and the cylinder, is always the same in different parts of the same, or of similar solids, when the inclination of the planes to the axis, and the altitude of the frustum, are given. This difference vanishes in the parabolic conoid. It is the same in all spheres; being equal to half the content of a sphere of a diameter equal to the altitude of the frustum. In the cone it is $\frac{1}{4}$th of the content of a similar cone of the same height with the frustum; and in other figures it is reduced to the difference in the cone.

In the remarks on the method of the ancients, the author observes, that they established the higher parts of their geometry on the same principles as the elements of the science, by demonstrations of the same kind; that they seem to have been careful not to suppose any thing to be done, till by a previous problem they had shown how it was to be performed: far less did they suppose any thing to be done, that cannot be conceived to be possible, as a line or series to be actually continued to infinity, or a magnitude to be diminished till it becomes infinitely less than it was. The elements into which they resolved magnitudes were always finite, and such as might be conceived to be real. Unbounded liberties have been introduced of late, by which geometry, wherein every thing ought to be clear, is filled with mysteries, and philosophy is likewise perplexed. Several instances of this kind are mentioned. The series 1, 2, 3, 4, 5, 6, 7, &c. is supposed by some to be actually continued to infinity; and, after such a supposition, we are puzzled with the question, whether the number of finite terms in such a series is finite or infinite. In order to avoid such suppositions, and their consequences, the author chose to follow the ancients in their method of demonstration as much as possible. Geometry has been always considered as our surest bulwark against the subtleties of the sceptics, who are ready to make use of any advantages that may be given them against it;* and it is important, not only that the conclusions in geometry be true, but likewise that their evidence be unexceptionable. However, he is far from affirming, that the method of infinitesimals is without foundation, and afterwards endeavours to justify a proper application of it.

The grounds of the method of fluxions are described in chap. 1, book 1, and again in chap. 1, book 2. In the former, magnitudes are conceived to be generated by motion, and the velocity of the generating motion is the fluxion of the magnitude. Lines are supposed to be generated by the motion of points. The velocity of the point that describes the line is its fluxion, and measures the rate of its increase or decrease. Other magnitudes may be represented by lines that increase or decrease in the same proportion with them; and their fluxions will be in the same proportion as the fluxions of those lines, or the velocities of the points that describe them. When the motion of a point is uniform, its velocity is constant, and is measured by the space which is described by it in a given time. When the motion varies, the velocity at any term of the time is measured by the space which would be described in a given time, if the motion

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

was to be continued uniformly from that term without any variation. In order to determine that space, and consequently the velocity which is measured by it, four axioms are proposed concerning variable motions, two concerning motions that are accelerated, and two concerning such as are retarded. The first is, that the space described by an accelerated motion is greater than the space which would have been described in the same time, if it had not been accelerated, but had continued uniform from the beginning of the time. The second is, that the space which is described by an accelerated motion, is less than the space which is described in an equal time by the motion which is acquired by that acceleration continued afterwards uniformly. By these, and two similar axioms concerning retarded motions, the theory of motion is rendered applicable to this doctrine with the greatest evidence, without supposing quantities infinitely little, or having recourse to prime or ultimate ratios. The author first demonstrates from them all the general theorems concerning motion, that are of use in this doctrine; as, that when the spaces described by two variable motions are always equal, or in a given ratio, the velocities are always equal, or in the same given ratio; and conversely, when the velocities of two motions are always equal to each other, or in a given ratio, the spaces described by those motions in the same time are always equal, or in that given ratio; that when a space is always equal to the sum or difference of the spaces described by two other motions, the velocity of the first motion is always equal to the sum or difference of the velocities of the other motions; and conversely, that when a velocity is always equal to the sum or difference of two other velocities, the space described by the first motion is always equal to the sum or difference of the spaces described by these two other motions. In comparing motions in this doctrine, it is convenient and usual to suppose one of them uniform; and it is here demonstrated, that if the relation of the quantities be always determined by the same rule or equation, the ratio of the motions is determined in the same manner, when both are supposed variable. These propositions are demonstrated strictly by the same method which is carried on in the ensuing chapters for determining the fluxions of the figures.

In chap. 2, a triangle that has two of its sides given in position, is supposed to be generated by an ordinate moving parallel to itself along the base. When the base increases uniformly, the triangle increases with an accelerated motion, because its successive increments are trapezia, that continually increase. Therefore, if the motion with which the triangle flows, was continued uniformly from any term for a given time, a less space would be described by it than the increment of the triangle which is actually generated in that time by axiom 1, but a greater space than the increment which was actually generated in an equal time preceding that term, by axiom 2, and hence it is demonstrated, that the fluxion of the triangle is accurately measured by the rectangle contained by the corresponding ordinate of the triangle, and the right line which measures the fluxion of the base. The increment which the triangle acquires in any time, is resolved into two parts; that which is generated in consequence of the motion with which the triangle flows at the beginning of the time, and that which is generated in consequence of the acceleration of this motion for the same time. The latter is justly neglected in measuring that motion, or the fluxion of the triangle at that term, but may serve for measuring its acceleration, of the 2d fluxion of the triangle. The motion with which the triangle flows, is similar to that of a body descending in free spaces by a uniform gravity, the velocity of which, at any term of the time, is not to be measured by the space described by the body in a given time, either before or after that term, because the motion continually increases, but by a mean between these spaces.

When the sides of a rectangle increase or decrease with uniform motions, it may be always considered as the sum or difference of a triangle and trapezium; and its fluxion is derived from the last proposition. If the sides increase with uniform motions, the rectangle increases with an accelerated motion; and in measuring this motion at any term of the time, a part of the increment of the rectangle, that is here determined, is rejected, as generated in consequence of the acceleration of that motion.

The fluxions of a curvilineal area (whether it be generated by an ordinate moving parallel to itself, or by a ray revolving about a given centre) and of the solid, generated by the area revolving about the base, are determined by demonstrations of the same kind; and when the ordinates of the figure increase, the increment of the area is resolved in like manner into two parts, one of which is only to be retained in measuring the fluxion of the area, the other being rejected as generated in consequence of the acceleration of the motion with which the figure flows. An illustration of 2d and 3d fluxions is given by resolving the increment of a pyramid or cone into the several respective parts that are conceived to be generated in consequence of the 1st, 2d, and 3d fluxions of the solid, when the axis is supposed to flow uniformly.

In chap. 5, a series of lines in geometrical progression are represented by an easy construction. The first term being supposed invariable, and the second to increase uniformly, all the subsequent terms increase with accelerated motions. The velocities of the points that describe those lines being compared, it is demonstrated, from the axioms by common geometry, that the fluxions of any two terms are in a ratio compounded of the ratio of the terms, and of the ratio of the numbers that express how many terms precede them in the progression.

In the 6th chapter, the nature and properties of logarithms are described after the celebrated inventor; and it is observed, that he made use of the very terms fluxus and fluat on this occasion. A line is said to increase or decrease proportionally, when the velocity of the point, that describes it. is always as its distance from a certain term of the line; and if in the mean time another point describes a line with a certain uniform motion, the space described by the latter point is always the logarithm of the distance of the former from the given term. Hence the fluxion of this distance is to the fluxion of its logarithm, as that distance is to an invariable line; and the fluxions of the quantities that have their logarithms in an invariable ratio, are to each other in a ratio compounded of this invariable ratio, and of the ratio of the quantities themselves. Some propositions are demonstrated, that relate to the computation of logarithms; but this subject is prosecuted further in the second book. The logarithmic curve is here described, with the analogy between logarithms and hyperbolic ratios.

In the 7th chapter, after a general definition of tangents, it is demonstrated, that the fluxions of the base, ordinate, and curve, are in the same proportion to each other, as the sides of a triangle respectively parallel to the base, ordinate, and tangent. When the base is supposed to flow uniformly, if the curve be convex towards the base, the ordinate and curve increase with accelerated motions; but their fluxions at any term are the same as if the point which describes the curve had proceeded uniformly from that term in the tangent there. Any further increment which the ordinate or curve acquires, is to be imputed to the acceleration of the motions with which they flow. A ray that revolves about a given centre, being supposed to meet any curve and an arc of a circle described from the same centre, the fluxions of the ray, curve, and circular arc, are compared together; and several other propositions concerning tangents are demonstrated from the axioms. The next chapter treats of the fluxions of curve surfaces in a similar manner.

The 9th chapter treats chiefly of the greatest and least ordinates of figures, and of the points of contrary flexure and cuspids. The fluxion of the base being given, when the fluxion of the ordinate vanishes, the tangent becomes parallel to the base, and the ordinate most commonly is a maximum or minimum, according to the rule given by authors on this subject. But if the second fluxion of the ordinate vanish at the same time, and the third fluxion be real, this rule does not hold, for the ordinate is in that case neither a maximum nor minimum. If the first, second, and third fluxions vanish, and the fourth fluxion be real, the ordinate is a maximum or minimum. The general rule demonstrated in this chapter, and again in the last chapter of the second book, is, that when the first fluxion of the ordinate, with its fluxions of any subsequent successive orders, vanish, and the number of all these fluxions that vanish is odd, then the ordinate is a maximum or minimum, according as the fluxion of the next order to these is negative or positive. The ordinate passes through a point of contrary flexure, when its fluxion becomes a maximum or minimum, supposing the curve to be continued on both sides of the ordinate. Hence the common rule for finding the points of contrary flexure is corrected in a similar manner. Such a point is not always formed when the second fluxion of the ordinate vanishes; for if its third fluxion likewise vanishes, and its fourth fluxion be real, the curve may have its cavity turned all one way. The same is to be said, when its fluxions of the subsequent successive orders vanish, if the number of all those that vanish be even. Other theorems are subjoined relating to this subject.

The 10th chapter treats of the asymptotes of lines, the areas bounded by them and the curves, the solids generated by these areas, of spiral lines, and the limits of the sums of progressions. The analogy between these subjects, induced the author to treat of them in one chapter, and illustrate them by one another. He begins with three of the most simple instances of figures that have asymptotes. In the common hyperbola, the ordinate is reciprocally as the base, and therefore decreases while the base increases, but never vanishes, because the rectangle contained by it and the base is always a given area, and it is assignable at any assignable distance, how great soever. The points of the conchoid are determined by drawing right lines from a given centre, and on these produced from the asymptote, taking always a given right line; so that the curve never meets the asymptote, but continually approaches to it, because of the greater and greater obliquity of this right line. The third is the logarithmic curve, wherein the ordinates, at equal distances, decrease in geometrical proportion, but never vanish, because each ordinate is in a given ratio to the preceding ordinate. Geometrical magnitude is always understood to consist of parts; and to have no parts, or to have no magnitude, are considered as equivalent in this science.* There is, however, no necessity for considering magnitude as made up of an infinite number of small parts; it is sufficient, that no quantity can be supposed to be so small, but it may be conceived to be diminished further; and it is obvious, that we are not to estimate the number of parts that may be conceived in a given magnitude, by those which in particular

* See Euclid's Elements, Def. 1, lib. i.—Orig.

determinate circumstances may be actually perceived in it by sense; since a greater number of parts become visible in it by varying the circumstances in which it is perceived.

It is hardly possible to give a tolerable extract of this or the following chapters, without diagrams and computations: we shall therefore observe only, that after giving some plain and obvious instances, wherein a quantity is always increasing, and yet never amounts to a certain finite magnitude (as, while the tangent increases, the arc increases, but never amounts to a quadrant); this is applied successively to the several subjects mentioned in the title of the chapter. Let the figure be concave towards the base, and suppose it to have an asymptote parallel to the base; in this case the ordinate always increases while the base is produced, but never amounts to the distance between the asymptote and the base. In like manner a curvilineal area, in a second figure, may increase, while the base is produced, and approach continually to a certain finite space, but never amount to it: this is always the case, when the ordinate of this latter figure is to a given right line, as the fluxion of the ordinate of the former is to the fluxion of the base; and of this various examples are given. A solid may increase in the same manner, and yet never amount to a given cube or cylinder, when the square of the ordinate of the latter figure is to a given square, as the fluxion of the ordinate of the first figure is to the fluxion of the base. A spiral may in like manner approach to a point continually, and yet in any number of revolutions never arrive at it; and there are progressions of fractions that may be continued at pleasure, and yet the sum of the terms may be always less than a given number. Various rules are demonstrated, and illustrated by examples, for determining when a figure has an asymptote parallel or oblique to the base; when the area terminated by the curve and the asymptote has a limit which it never exceeds, or may be produced till it surpass any assignable space; when the solid generated by that area, the surface generated by the perimeter of the curve, the spiral area generated by the revolving ray, the spiral line itself, or the sum of the terms of a progression, have such limits or not; and for measuring those limits. The author insists on these subjects, the rather that they are commonly described in very mysterious terms, and have been the most fertile of paradoxes of any parts of the higher geometry. These paradoxes, however, amount to no more than this: that a line or number may be continually acquiring increments, and those increments may decrease in such a manner, that the whole line or number shall never amount to a given line or number. The necessity of admitting this is obvious enough, and is here shown from the nature of the most common geometrical figures in Art. 292, 293, &c. and from any series of fractions that decrease continually, in Art. 354, 355, &c.

The 11th chapter treats of the curvature of lines, its variation, the degrees of contact of the curve and circle of curvature, and of various problems that depend on the curvature of lines. This subject is treated fully, because of its extensive usefulness, and because in this consists one of the greatest advantages of the modern geometry above that of the ancients. The author on this, as on former occasions, begins by premising the necessary definitions. Curve lines touch each other in a point, when the same right line is their common tangent at that point; and that which has the closest contact with the tangent, or passes between it and the other curve through the angle of contact formed by them, being less inflected from the tangent, is therefore less curve. Thus a greater circle has a less curvature than a less circle; and since the curvature of circles may be varied indefinitely, by enlarging or diminishing their diameters, they afford a scale by which the curvature of other lines may be measured. As the tangent is the right line which touches the arc so closely, that no other right line can be drawn between them; so the circle of curvature is that which touches the curve so closely, that no other circle can be drawn through the point of contact between them. As the curve is separated from its tangent in consequence of its flexure or curvature, so it is separated from the circle of curvature in consequence of the variation of its curvature; which is greater or less, according as its flexure from that circle is greater or less.

The tangent of the figure being considered as the base, a new figure is imagined, whose ordinate is a third proportional to the ordinate and base of the first. This new figure determines the chord of the circle of curvature by its intersection with the ordinate at the point of contact, and by the tangent of the angle in which it cuts that circle, measures the variation of curvature. The less this angle is, the closer is the contact of the curve and circle of curvature, of which there may be indefinite degrees. When the figure proposed is a conic section, the new figure is likewise a conic section; and it is a right line when the first figure is a parabola, and the ordinates are parallel to the axis; or when the first figure is an hyperbola, and the ordinates are parallel to either asymptote. Hence the curvature and its variation in a conic section are determined by several constructions; and among other theorems, it is shown, that the variation of curvature at any point of a conic section, is as the tangent of the angle contained by the diameter which passes through that point, and by the perpendicular to the curve.

When the ordinate at the point of contact is an asymptote to the new figure, the curvature is less than in any circle; and this is the case in which it is said to be infinitely little, or the ray of curvature is said to be infinitely great. Of this kind is the curvature at the points of contrary flexure in the lines of the third order. When the new figure passes through the point of contact, the curvature is greater than in any circle, or the ray of curvature vanishes; and in this case the curvature is said to be infinitely great. Of this kind is the curvature at the cuspids of the lines of the third order.

As lines which pass through the same point have the same tangent when the first fluxions of the ordinate are equal, so they have the same curvature when the second fluxions of the ordinate are likewise equal; and half the chord of the circle of curvature that is intercepted between the points wherein it intersects the ordinate, is a third proportional to the right lines that measure the second fluxion of the ordinate, and first fluxion of the curve, the base being supposed to flow uniformly. When a ray revolving about a given point, and terminated by the curve, becomes perpendicular to it, the first fluxion of the ray vanishes; and if its second fluxion vanishes at the same time, that point must be the centre of curvature. The same is to be said when the angular motion of the ray about that point is equal to the angular motion of the tangent of the curve; as the angular motion of the radius of a circle about its centre is always equal to the angular motion of the tangent of the circle. Thus the various properties of the circle suggest various theorems for determining the centre of the curvature.

Because figures are often supposed to be described by the intersections of right lines revolving about given poles, three theorems are given in prop. 18, 26, and 35, for determining the tangents, asymptotes, and curvature of such lines, from the description, which are illustrated by examples. A new property of lines of the third order is subjoined to prop. 35. The evolution of lines is considered in prop. 36. The tangents of the evoluta are the rays of curvature of the line which is described by its evolution; and the variation of curvature in the latter is measured by the ratio of the ray of curvature of the former to the ray of curvature of the latter.

Sir Isaac Newton, in a treatise lately published, measures the variation of the curvature by the ratio of the fluxion of the ray of curvature to the fluxion of the curve; and is followed by the author, to avoid the perplexity which a difference in definitions occasions to readers, though he hints in art. 386, that this ratio gives rather the variation of the ray of curvature, and that it might have been proper to have measured the variation of curvature rather by the ratio of the fluxion of the curvature itself to the fluxion of the curve; so that the curvature being inversely as the ray of curvature, and consequently its fluxion as the fluxion of the ray itself directly, and the square of the ray inversely, its variation would have been directly as the measure of it, according to Sir Isaac Newton's definition, and inversely as the square of the ray of curvature; according to this explication, it would have been measured by the angle of contact contained by the curve and circle of curvature, in the same manner as the curvature itself is measured by the angle of contact contained by the curve and tangent. The ground of this remark will better appear from an example: according to Sir Isaac Newton's explication, the variation of curvature is uniform in the logarithmic spiral, the fluxion of the ray of curvature in this figure being always in the same ratio to the fluxion of the curve; and yet while the spiral is produced, though its curvature decreases, it never vanishes; which must appear strange to such as do not attend to the import of his definition.—It is easy, however, to derive one of these measures of this variation from the other, and because Sir Isaac Newton's is, generally speaking, assigned by more simple expressions, the author has the rather conformed to it in this treatise, but thought it necessary to give the caution we have mentioned.

The greatest part of this chapter is employed in treating of useful problems, that have a dependence on the curvature of lines. First, the properties of the cycloid are briefly demonstrated, with the application of this doctrine to the motion of pendulums, by showing that when the motion of the generating circle along the base is uniform, and therefore may measure the time, the motion of the point that describes the cycloid, is such as would be acquired by a heavy body descending along the cycloidal arc, the axis of the figure being supposed perpendicular to the horizon. In the next place, the caustics, by reflexion and refraction, are determined. If perpendiculars be always drawn from the radiating point to the tangents of the curve, and a new curve be supposed to be the locus of the intersections of the perpendiculars and tangents, then the line, by the evolution of which that new curve can be described, is similar and similarly situated to the caustic by reflexion. The doctrine of centripetal forces is treated at length from art. 416 to 493.

First, a body is supposed to descend freely by its gravity in a vertical line, and because the gravity is the power which accelerates the motion of the body, it must be measured by the fluxion of its velocity, or the second fluxion of the space described by it. When the vertical line is supposed to move parallel to itself with an uniform motion, the body will descend in it in the same manner as before; and the gravity will be still measured by the second fluxion of the descent, or the second fluxion of the ordinate of the curve that is traced in this case by the body on an immoveable plane, and therefore is as the square of the velocity, which is measured by the fluxion of the curve, directly, and the chord of the circle of curvature that is in the direction of the gravity inversely, by a proposition mentioned above. When the gravity acts uniformly, and in parallel lines, the projectile, in describing any arc, falls below the tangent drawn at the beginning of the arc, as much as if it had fallen perpendicularly in the vertical; and, the time being given, the gravity may be measured by the space which is the subtense of the angle of contact. In other cases, when the gravity varies, or its direction changes, it may be measured at any point by the subtense of the angle of contact, that would have been generated in a given time, if the gravity had continued to act uniformly in parallel lines from that term, that is, by the subtense of the angle of contact in the parabola that has its diameter in the direction of the force, and has the closest contact with the curve; which leads us to the same theorem as before.

In general, let the gravity, that results from the composition of any number of centripetal forces, which are supposed to act on the body in one plane, be resolved into a force parallel to the ordinates, and a force parallel to the base; then the former shall be measured by the second fluxion of the ordinate, and the latter by the second fluxion of the base, the time being supposed to flow uniformly, so that the velocity of the body may be measured by the fluxion of the curve. When the trajectory is not in one plane, the force is resolved in a similar manner into three forces, which are measured by three second fluxions analogous to them.

Whether the body move in a void, or in a medium that resists its motion; the gravity that results from the composition of the centripetal forces which act upon the body, is always as the square of its velocity directly, and the chord of the circle of curvature that is in the direction of the gravity inversely.

When a body describes any trajectory in a void or in a medium, by a force directed to one given centre, the velocity at any point of the trajectory, is to the velocity by which a circle could be described in a void about the same centre, at the same distance, by the same gravity, in the subduplicate ratio of the angular motion of the ray drawn always from the body to the centre, to the angular motion of the tangent of the trajectory; and, if there be no resistance, the velocity in the trajectory at any point, is the same that would be acquired by the body, if it was to fall from that point through one-fourth of the chord of the circle of curvature that is in the direction of the gravity, and the gravity at that point was to be continued uniformly during its descent.

If the centripetal force be inversely as any power of the distance whose exponent is any number m greater than unit, there is a certain velocity (viz. that which is to the velocity in a circle at the same distance as $\sqrt{2}$ to $\sqrt{m-1}$) which would be just sufficient to carry off the body upwards in a vertical line, so as that it should continue to ascend for ever, and never return towards the centre. If the body be projected in any other direction with the same velocity, it will describe a trajectory which is here constructed; it is a parabola when $m = 2$, a logarithmic spiral when $m = 3$, an epicyloid when $m = 4$, a circle that passes through the centre of the forces when $m = 5$, and the lemniscata when $m = 7$. In general, it is constructed by drawing a perpendicular from the centre of the forces to a right line given in position, and any other ray to the same right line, then increasing or diminishing the angle contained by this ray and the perpendicular in the given ratio of 2 to the difference between 3 and m, and increasing or diminishing the logarithm of the ray in the same given ratio. The trajectories described in analogous cases by centrifugal forces, are constructed in a similar manner. These are the figures in which the perpendicular, from a given centre on the tangent, is always as some power of the ray drawn from the same centre to the point of contact, which are afterwards found to arise in the resolution of the most simple cases of problems of various kinds.

When the area described about the centre of an ellipse is given, the subtense of the angle of contact, drawn through one extremity of the arc parallel to the semidiameter drawn to the other extremity, is in a given ratio to this semidiameter; and therefore, when an ellipse is described by a force directed towards the centre, that force is always as the distance from the centre. When the force is directed toward the focus, it is inversely as the square of the distance. And these two cases are considered particularly, because of their usefulness in the true theory of gravity. To illustrate which, the laws of centripetal forces that would cause a body to descend continually toward the centre, or ascend from it, are distinguished from those which cause the body to approach towards the centre, and recede from it by turns. A body approaches from the higher apsid toward the centre, when its velocity is less than what is requisite to carry it in a circle; and if its velocity increase, while it descends, in a higher proportion than the velocities requisite to carry bodies in circles about the same centre, the velocity in the lower part of the curve may exceed the velocity in a circle at the same distance, and thereby become sufficient to carry off the body again. But while the distance decreases, if the velocities in circles increase in the same, or in a higher proportion, than the velocity in a trajectory can increase, the body must either continually approach toward the centre, if it once begin to approach to it, or recede continually from the centre, if it once begin to ascend from it; and this is the case, when the centripetal force increases as the cube of the distance decreases, or in a higher proportion. But though, in such cases, the body approaches continually towards the centre, we are not to conclude, that it will always approach to it till it fall into it, or come within any given distance; for it is demonstrated afterwards, in art. 879 and 880, that it may approach to the centre for ever, in a spiral that never descends to a given circle described in the same plane, and that it may recede from it for ever in a spiral that never arises to a given altitude. An example of each case is given when the centripetal force is inversely as the fifth power of the distance.

When the trajectory is described in a medium, let z be to a given magnitude as the centripetal force is to the force by which the same trajectory could be described in a void; and if the area be supposed to flow uniformly, the resistance will be in the compound ratio of the fluxion of z, and of the fluxion of the curve; and the density of the medium, supposing the resistance to be in the compound ratio of the density and of the square of the velocity, shall be as the fluxion of the logarithm of z directly, and the fluxion of the curve inversely. Hence, when any figure that can be described in a void by a force that varies according to any power of the distance from the centre, is described in a medium, the density of the medium must be inversely as the tangent of the figure bounded by a perpendicular at the centre to the ray drawn from it to the point of contact.

After giving some properties of the trajectories that are described by a body when it gravitates in right lines perpendicular to a given surface, and their application to optical uses, the author proceeds to consider the motion of a body that gravitates towards several centres. In such cases, that surface is said to be horizontal, which is always perpendicular to the direction of the gravity that results from the composition of the several forces; and it is shown, that the velocity which is acquired by descending from one horizontal surface to another is always the same, whether the body move in right lines, or in any curves; the square of which is measured by the aggregate of several areas which have the distances from the respective centres for their bases, and right lines proportional to the forces at these distances for their ordinates.

The force which acts on the moon is resolved into a force perpendicular to the plane of the ecliptic, and a force parallel to it. This last is again resolved into that which is parallel to the line of the syzigies, and that which is parallel to the line joining the quadratures. The first measures the second fluxion of the distance of the moon from that plane, the second and third measure the second fluxions of her distances from the line of the quadratures, and from the line of the syzigies, respectively. Hence a construction is derived of the trajectory which would be described by the moon about the earth, in consequence of their unequal gravitation towards the sun, if the gravity of the moon towards the earth was as her distance from it. From this a computation is deduced of the motion of the nodes of the moon, and of the variation of the inclination of the plane of her orbit. It is sufficient here to observe, that these motions are found to agree nearly with those which have been deduced from other theories, and from astronomical observations.

A fluid being supposed to gravitate towards two given centres with equal and invariable forces, it is shown, that the figure of the fluid must be that of an oblong spheroid, and that those two centres must be the foci of the generating ellipse. The nature of the figure is also shown, when the fluid gravitates towards several centres, or when it revolves on its axis; but these are mentioned briefly, because such theories are of little or no use for discovering the figures of the planets.

In the 12th chapter, the author proceeds to consider the more concise methods, by which the fluxions of quantities are usually determined, and to deduce general theorems more immediately applicable to the resolution of geometrical and philosophical problems. In the method of infinitesimals, the element, by which any quantity increases or decreases, is supposed to become infinitely small, and is generally expressed by two or more terms, some of which become infinitely less than the rest, and therefore being neglected as of no importance, the remaining terms form, what is called the difference of the quantity proposed. The terms that are neglected in this manner, are the very same which arise in consequence of the acceleration or retardation of the generating motion, during the infinitely small time in which the element is generated; and therefore these differences are in the same ratio to each other as the generating motions or fluxions. Hence the conclusions in this method are accurately true, without even an infinitely small error, and agree with those that are deduced by the method of fluxions.

It is usual in this method to consider a curve as a polygon of an infinite number of sides, which, being produced, give the tangents of the curve, and, by their inclination to each other, measure its curvature. But it is necessary in some cases, if we would avoid error, to resolve the element of the curve into several infinitely small parts, or even sometimes into infinitesimals of the second order; and errors that might otherwise arise in its application, may, with due care, be corrected by a proper use of this method itself, of which some instances are given. If we were to suppose, for example, the least arc that can be described by a pendulum to coincide with its chord, the time of the vibration derived from this supposition will be found erroneous; but by resolving that arc into more and more infinitely small parts, we approach to the true time in which it is described. By supposing the tangent of the curve to be the production of the rectilineal element of the curve, the subtense of the angle of contact is found equal to the second difference or fluxion of the ordinate; but in this inquiry, the tangent ought to be supposed to be equally inclined to the two elements of the curve that terminate at the point of contact: and then the subtense of the angle of contact will be found equal to half the second difference of the ordinate, which is its true value.

Sir Isaac Newton, however, investigates the fluxions of quantities in a more unexceptionable manner. He first determines the finite simultaneous increments of the fluents, and, by comparing them, investigates the ratio that is the limit of the various proportions which they bear to each other, while he supposes them to decrease together till they vanish. When the generating motions are variable, the ratio of the simultaneous increments that are generated from any term, is expressed by several quantities, some of which arise from the ratio of the generating motions at that term, and others from the subsequent acceleration or retardation of these motions. While the increments are supposed to be diminished, the former remain invariable, but the latter decrease continually, and vanish with the increments; and hence the limit of the variable ratio of the increments, or their ultimate ratio, gives the precise ratio of the generating motions or fluxions. Most of the propositions in the preceding chapters may be more briefly demonstrated by this method, of which several examples are given, and the author makes always use of it in the sequel of this book.

It is one of the great advantages of this method, that it suggests general theorems for the resolution of problems, which may be readily applied as there is occasion for them. Our author proceeds to treat of these, and first of such as relate to the centre of gravity and its motion. In any system of bodies, the sum of their motions, estimated in a given direction, is the same as if all the bodies were united in their common centre of gravity. If the motion of all the bodies is uniform and rectilineal, the centre of gravity is either quiescent, or its motion is uniform and rectilineal. When action is equal to reaction, the state of the centre of gravity is never affected by the collisions of the bodies, or by their attracting or repelling each other mutually. It is not, however, the sum of the absolute motions of the bodies that is preserved invariable in consequence of the equality of the action and reaction, as they seem to imagine, who tell us, that this sum is unalterable by the collisions of bodies, and that this follows so evidently from the equality of action and reaction, that to endeavour to demonstrate it would serve only to render it more obscure. On this occasion the author illustrates an argument which he had proposed in a piece that obtained the prize proposed by the Royal Academy of Sciences at Paris in 1724, against the mensuration of the forces of bodies by the square of the velocities, showing that if this doctrine was admitted, the same power or agent, exerting the same effort, would produce more force in the same body when in a space carried uniformly forwards, than if the space was at rest, or that springs acting equally on two equal bodies in such a space, would produce unequal changes in the forces of those bodies.

Various problems concerning the collision of bodies are resolved in a more general manner than usual. Mr. Bernouilli had determined the motions when the elasticity is perfect, and one body strikes two equal bodies in directions that form equal angles with its direction; or when there are any number of bodies impelled by it on one side in various directions, providing equal bodies be impelled by it on the other side, in directions equally inclined to its own direction. But the problem is resolved here without these limitations; some others of this kind are subjoined, and this doctrine is applied for determining the motions of bodies that act on each other while they descend by their gravity.

The general principle derived from these inquiries is, that if there be no collision, or sudden communication of motion from one body to another, while they descend together, and in any case, if the elasticity be perfect, the sum of the products, when each body is multiplied by the square of the velocity acquired by it, is the same as if all the bodies had descended freely from the same respective altitudes to their several places; only in collecting that sum, if any body is made to ascend, the product of it multiplied by the square of its velocity is to be subducted; and if the bodies be supposed to ascend from their places with the respective velocities acquired by them, then their common centre of gravity will rise to the same level from which it descended. In other cases, however, the ascent of the centre of gravity will be less than its descent, but is never greater.

After demonstrating the usual rule for finding the centre of oscillation, the author treats of the motion of water issuing from a cylindric vessel. The effect of the gravitation of the whole mass of water is considered as threefold. It accelerates, for some time at least, the motion with which the water in the vessel descends; it generates the excess of the motion with which the water issues at the orifice above the motion which it had in common with the rest of the water: and it acts on the bottom of the vessel at the same time. Then supposing the last two parts of the force to be in any invariable ratio to each other, when the diameters of the base and orifice are given, he determines by logarithms the velocity with which the water issues at the orifice; and shows that this velocity will approach very nearly to its utmost limit in an exceedingly small time. When the water is supposed to be supplied in a cylinder, so as to stand always at the same altitude above the orifice, there is an analogy between the acceleration of the motion of the water that issues at the orifice, and the acceleration of a body that descends by its gravity in a medium which resists in the duplicate ratio of the velocity. For when the utmost velocities, or limits, are equal in those two cases, the time in which the issuing water acquires any less velocity, is to the time in which the descending body acquires the same velocity, as the area of the orifice to the area of the base; and if a cylindric column be supposed to be erected on the orifice equal to the quantity of water that issues at the orifice in the former of those times, the height of this column will be to the space described by the descending body in the latter time, in the same ratio as the orifice to the area of the base. The ratio of the force that acts on the bottom of the vessel to the force that generates the motion of the water issuing at the orifice, is deduced from Sir Isaac Newton's cataract, and is the same that follows from the principle concerning the equality of the ascent and descent of the centre of gravity, which was first applied to this inquiry by Mr. Daniel Bernouilli, comment. Acad. Petrop. tom. 2. But there are several precautions to be taken in applying this doctrine.

After some other theorems concerning the centre of gravity, and several observations concerning the curvature of lines, and the angles of contact; the author represents 4 general propositions in one view, that the analogy between them may appear. The first gives the property of the trajectories that are described by any centripetal forces, how variable soever these forces, or their directions, may be. The second gives a like general property of the lines of swiftest descent. The third gives the property of the line that is described in less time than any other of an equal perimeter. And the fourth gives the property of the figure that is assumed by a flexible line or chain, in consequence of any such forces acting upon it. If we suppose a body to set out from any point in the trajectory, or in the line of swiftest descent, with the velocity which it has acquired there, and to move in the right line which is the direction of the gravity, that results from the composition of the centripetal forces, then shall its velocity, and its distance from the point where the perpendicular from the centre of curvature meets that right line, flow proportionally, i. e. the fluxion of the velocity, or of the right line that measures it, shall be to the velocity, as the fluxion of that distance is to the distance. When the velocity and direction of the motion is the same in the line of swiftest descent as in the trajectory, their curvature is the same. Thus in the common hypothesis of gravity, the curvature in the cycloid, the line of swiftest descent, is the same as the parabola described by a projectile, if the velocities in those lines be equal, and their tangents be equally inclined to the horizon. In order to find the nature of the catenaria in any hypothesis of gravity, suppose the gravity to be increased or diminished in the same proportion as the thickness of the chain varies, and to have its direction changed into the opposite direction ; then imagine a body to set out with a just velocity from a given point in the chain, and to describe the curve. The tension of the chain at any point will be always as the square of the velocity acquired at that point, and if a body be projected with this velocity in the direction of the tangent, the curvature of the trajectory described by it will be $\frac{1}{2}$ of the curvature of the chain at that point. We must refer to the book for a fuller account of these and of other theorems.

In the 13th chapter, the problems concerning the lines of swiftest descent, the figures which among all those that have equal perimeters produce maxima or minima, and the solid of least resistance, are resolved without computations, from the first fluxions only. There are also easy synthetic demonstrations subjoined, because this theory is commonly esteemed of an abstruse nature, and mistakes have been more frequently committed in the prosecution of it, than of any other relating to fluxions. To give some idea of the author's method, suppose the gravity to act in parallel lines, a to denote the velocity acquired at the lowest point of the curve, and u the velocity acquired at any other point of the curve. Suppose the element of the curve to be described by this velocity u, but the element of the base to be always described by the constant velocity a. Then it is easily demonstrated, without any computation, that the element of the ordinate being given, the difference of the times in which the elements of the curve and base are thus described is a minimum, when the ratio of those elements is that of a to u; i. e. when the sine of the angle, in which the ordinate intersects the curve, is to the radius in this ratio. Supposing therefore this property to take place over all the curve, the excess of the time in which it is described by the body descending along it, above the time in which the base is described uniformly with the velocity a, must be a minimum ; and this latter time being given, it follows that the time of descent in this curve is a minimum. When the gravity tends to a given centre, substitute an arc of a circle described from that centre through the lowest point of the curve in the place of the base in the former case ; and the property of the line of swiftest descent will be discovered in the same manner. The nature of the line that among all those of the same perimeter is described in the least time, is discovered with great facility, by determining from the former case the property of the figure when the sum or difference of the time in which it is described by the descending body, and of the time in which it would be described by any given uniform motion, is a minimum ; for the latter time being the same in all curves of the same length, it follows that the figure, which has this property, must be described in less time than any other of an equal perimeter. The general isoperimetrical problems are resolved, and the solutions are rendered more general, with like facility by the same method; which is also applied for determining the property of the solid of least resistance, and serves for resolving the problem, when limitations are added concerning the capacity of the solid, or the surface that bounds it.

The last chapter of the first book treats chiefly of gravitation towards spheroids, of the figure of the planets, and of the tides. The author, having occasion in those inquiries for several new properties of the ellipse, begins this chapter by deriving its properties from those of the circle, by considering it as the oblique section of a cylinder, or as the projection of the circle by parallel rays on a plane oblique to the circle. In this manner the properties are briefly transferred from the one to the other, because by this projection the centre of the circle gives the centre of the ellipse; diameters perpendicular to each other in the circle with their ordinates, and the circumscribed square, give conjugate diameters of the ellipse with their ordinates, and the circumscribed parallelogram; parallel lines in the plane of this circle are projected by parallels in the plane of the ellipse that are in the same ratio; any area in the former is projected by an area in the latter, which is in an invariable ratio to it; and concentric circles give similar concentric ellipses. It is likewise shown how properties of a certain kind are briefly transferred from the circle to any conic section with the same facility.

After demonstrating the properties of the ellipse, it is shown, that if the gravity of any particle of a spheroid being resolved into two forces, one perpendicular to the axis of the solid, the other perpendicular to the plane of its equator, then all particles, equally distant from the axis, must tend towards it with equal forces; and all particles at equal distances from the plane of the equator, gravitate equally towards this plane; but that the forces with which particles at different distances from the axis tend towards it, are as the distances; and that the same is to be said of the forces with which they tend towards the plane of the equator.

From this it is demonstrated, that when the particles of a fluid spheroid, of a uniform density, gravitate towards each other, with forces that are inversely as the squares of their distances, and at the same time any other powers act on the particles, either in right lines perpendicular to the axis, that vary in the same proportion as the distances from the axis, or in right lines perpendicular to the plane of the equator, that vary as their distances from it, or when any powers act on the particles of the spheroid, that may be resolved into forces of this kind; then the fluid will be every where in equilibrio, if the whole force that acts at the pole be to the whole force that acts at the circumference of the equator; as the semidiameter of the equator to the semiaxis of the spheroid;

and that the forces with which equal particles at the surface tend towards the spheroid, will be in the same proportion as perpendiculars to its surface, terminated either by the plane of the equator, or by the axis. Because the centrifugal force with which any particle of the spheroid endeavours to recede from its axis, in consequence of the diurnal rotation, is as the distance from the axis, it appears that if the earth, or any other planet, was fluid, and of a uniform density, the figure which it would assume would be accurately that of an oblate spheroid generated by an ellipsis revolving about its 2d axis.

Afterwards the gravity towards an oblate spheroid is accurately measured by circular arcs, not only at the pole, but also at the equator, and in any intermediate places; and the gravity towards an oblong spheroid is measured by logarithms. The gravity at any distance in the axis of the spheroid, or in the plane of the equator produced, is likewise accurately determined by like measures, without any new computation or quadrature, by showing that when two spheroids have the same centre and focus, and are of a uniform density, the gravities towards them at the same point in the axis or plane of the equator produced, are as the quantities of matter in the solids.

This theory is applied for determining the figure of the earth, by comparing the force of gravity in any given latitude, derived from the length of a pendulum that vibrates there in a second of time, with the centrifugal force at the equator, deduced from the periodic time of the diurnal rotation, and the amplitude of a degree of the meridian; or by comparing the lengths of pendulums that vibrate in equal times in given unequal latitudes; or by comparing different degrees measured on the meridian. By the best observations it would seem, that there is a greater increase of gravitation, and of the degrees of the meridian from the equator towards the poles, than ought to arise from the supposition of a uniform density. Therefore the author supposes the density to vary from the surface towards the centre; and, in several cases he has considered, he finds that a greater density towards the centre would account for a greater increase of gravitation towards the poles, but not for a greater increase of the degrees of the meridian; and that the hypothesis of a less density towards the centre would account for the latter, but not for the former, supposing (after Sir Isaac Newton) the columns of the fluid to extend from the surface to the centre, and there to sustain each other. On this account he determines the gravitation towards the earth, when it is supposed to be hollow with a nucleus included, according to the hypothesis advanced by Dr. Halley, with the difference of the semidiameters that might arise from such a disposition of the internal parts. But in this case, and when the density is supposed variable, the spheroidical figure is only assumed as an hypothesis. He adds, that by imagining the density to be greater in the axis than in the plane of the equator at equal distances from the centre, an hypothesis perhaps might be found, that would account for most of the phenomena; but that a series of many exact observations is requisite, before we can examine with any certainty the various suppositions that may be imagined, concerning the internal constitution of the earth. This doctrine is likewise applied for determining the figure of Jupiter.

It follows from the same theorem, that if we suppose the earth to be fluid, and abstract from its motion on its axis, and the inclination of the right lines in which its particles gravitate towards the sun or moon, the figure which it would assume, in consequence of the unequal gravitation of its particles towards either of those bodies, would be accurately that of an oblong spheroid, having its axis directed towards that body. The ascent of the water, deduced from this theorem, agrees nearly with that which Sir Isaac Newton found, by computing it briefly from what he had demonstrated concerning the figure of the earth. Several observations are subjoined concerning the tides, and the causes which may contribute to increase or diminish them, particularly the inequality of the velocities with which bodies revolve about the axis of the earth in different latitudes.

This chapter concludes by demonstrating briefly, that if the attraction of the particles decreased as the cube of their distance increases, or in any higher proportion, then any particle would tend towards the least portion of matter in contact with it, with a greater force than towards the greatest body at any distance, how small soever from it. The true law of gravity is better adapted for holding the parts of each body in a proper union, while it perpetuates the motions in the great system about the sun, and preserves the revolutions in the less systems nearly regular; and the author concludes with observing, that a remarkable geometrical simplicity is often found in the conclusions that are derived from it.

Of a very large Calculus voided by a Woman with her Urine. By Antonio Leprotti, Physician to the Pope, and F. R. S. N° 468, p. 363. *From the Latin.*

A poor woman, aged 50, and who had laboured under a strangury between 3 and 4 years before, one night was seized with a discharge of bloody urine, to the quantity of about 3 lb. during which she voided a stone, which, after being dried, weighed ℥ij gr. 29, or 1 oz. 17 dwts. 4 gr. avoirdupois.

A Machine for dressing and curing Patients, who are very unwieldy, and are under the Surgeon's Hands for some complaint on the Back, the Os Sacrum, &c. or are apprehensive of it. By M. le Cat, F.R.S. Abstracted from the French by P. H. Z. F. R. S. N° 468, p. 364.

Finding the usual methods for supporting unwieldy patients, who required surgical assistance for some ailment in the back, insufficient, M. le Cat was led to contrive a sort of hanging cradle or hammock, as represented in the figure hereto annexed.

Pl. 14, fig. 11, represents the bed without the bedding. On which lies a sort of boat of Turkey leather, full as long as the bed, with very strong hems all round, and eilet-holes for receiving hooks, that serve to lift up this hammock. The hooks are fastened to several ropes, all which depend on as many cross-beams of very solid wood. The cross-beams consist of one beam of the length of the whole bed, running lengthwise over the middle of it, and 4 transverse beams, the 2 middle ones somewhat longer than the others. The ropes on which the hammock hangs, are fastened to the extremities of these beams, which keep the hammock displayed; and on the same extremities are also fastened all the ropes, which unite in one that passes through the testern of the bed, and above it hangs on a pulley, fixed to the cieling of the bed-chamber. The rope from this pulley passes into another pulley, corresponding to it, hanging at some distance from the bed, where a man is placed to pull it, and raise the hammock.

What we chiefly intend in dressing a patient in question, are, 1st. To dress and refresh him, that is, gently to place him in a proper position, easy both for himself, and those who attend him. 2dly, To put him into an easy situation, that may also promote his recovery: the making of his bed often, is of great ease to him; but at the same time it is necessary that his wounds or ailments may not bear on any the least thing possible; and therefore his bed ought to be composed of several small mattresses, or of mattresses of several pieces, each with its tick over it; these mattresses ought besides to be supplied with numbers of pillows, each with its pillow-bier, so that he who waits on the patient, may place them where it is proper, for the ease of the person, and of the part affected. Nothing is more proper for this purpose than this hammock; the patient may be lifted up from his bed, and suspended just above those pillows, and higher yet, if necessary.

The bottom of the hammock is pierced in those places which answer to the anus, or any part affected, so that the evacuations may find their passage into receptacles between the pillows ranged accordingly. When the patient is to be dressed or refreshed, the borders of the hammock are taken up, and the several hooks passed through, by which he is to be suspended, as appears in the figure; and then a man, being placed at the rope that runs over the pulleys, lifts the patient up to the height necessary for the surgeon to search and dress the wound, and for the assistants to make his bed, which, even for the greater conveniency, may be pulled out from under the hammock. When all is done, the bed is pushed back again to its former place, the patient is gently let down upon it, the cross-beams are lowered and detached both from the hammock and the block, and put out of the way into a corner of the room; instead of it, a rope is fixed to the hook of the block, tied into an eilet at the end, coming down towards the bed within the patient's reach, in order to help himself when he wants to stir a little.

The hammock being displayed, and the cross-beams taken away, the patient is wrapped up in napkins as much as possible, to supply the sheet he wants between his body and the leather of the hammock; he is afterwards covered with an upper sheet, and other necessary bed-clothes.

This machine may be further improved by use. For instance: after M. le Cat had contrived this, he thought that instead of the border or hem of the hammock, might be made strong cylindrical iron rods, like curtain-rods, formed into a square, somewhat larger than the bedstead, as fig. 12, to the 4 corners of which are fastened as many ropes, which meet at the pulley; in which case the cross-beams, and the ropes depending on them, become useless; and instead of a hammock all of one piece, might be fixed 4 broad straps of Turkey leather to 2 sides of the square rod, which may be placed under such parts of the patient's body as will be proper, and which leave a space between each other where it is convenient. These straps may be fastened to the iron rods by several buckles with rings to slide along the rods, by the help of which the straps may be pushed on to such places where there is occasion; they may also thus be stretched or slackened, or even be taken off, or changed as is thought fit. After the patient has been dressed, and the bed made, the 4 ropes may be taken off both from the rod and from the block, and the rod be let drop with the extremities of the straps down upon the floor round the bedstead, which being narrower than the square of the rod, the latter will easily slip over it.

An Account of a Treatise, intitled D. Alberti Halleri Archiatri Regii et Elect. Medicin. Anatomiæ, Botan. Prælect. &c. Enumeratio Methodica Stirpium Helvetiæ indigenarum. Qua omnium brevis Descriptio et Synonymia, Compen-*

* The Rev. Mr. Coxe has favoured the world with a biographical account of Haller in the 2d vol. of his Letters on Switzerland. As most of our readers must be possessed of those entertaining and instructive letters, it cannot be necessary, on the present occasion, to enter very circumstantially into a history of the life and writings of this illustrious character; who, in every department of letters and science to which he directed his attention (and his attention was directed to a great variety of

dium Virium Medicarum, dubiarum Declaratio, novarum et rariorum uberior Historia et Icones continentur. Gottingiæ, 1742, 2 tom. folio, abstracted from the Latin. By William Watson, F. R. S. N° 468, p. 369.

This work is so well known to botanists in every part of Europe, that to reprint Mr. Watson's account of it now cannot be necessary. It will suffice to insert the tabular view of this celebrated author's Methodus Plantarum.

topics), displayed a vastness of conception, a strength of reasoning, a solidity of judgment, rarely equalled. Many instances of persons celebrated for universality of knowledge are recorded by biographical writers; but Haller may be considered as almost the only instance, where, to the most extensive erudition, and the most unwearied assiduity, in acquiring and in teaching science, there was, at the same time, joined the finest originality of genius. This is evinced by his poetical compositions, by his botanical classifications and descriptions, and more than all by his physiological experiments and inquiries.

It was at Berne that Albert Haller was born in 1708. Here his father practised as an advocate, designing his son for the same profession; but after devoting some years to the pursuit of general literature, young Haller made choice of the medical profession; to qualify himself for which, he went first to Tubingen, and afterwards to Leyden; where Boerhaave and Albinus taught. In 1726, he took his degree of M. D., and the year following he visited England, and afterwards Paris, where the botanical and anatomical chairs were at that time filled by very able professors. He returned to Berne in 1729, having previously spent some time at Basle, where, while he received instructions in mathematics from John Bernoulli, and in the practice of physic from Tzinger, he officiated as a teacher of anatomy, being at one and the same time both a pupil and a professor. In 1736, he accepted the professorship of physic, surgery, and botany, at Gottingen. This situation he filled for 17 years, during which, not only by his lectures, but also by numerous useful institutions, which he caused to be set on foot, he brought that newly-established university into great repute. Not long after the period last mentioned, Haller began to distinguish himself as an author, by various publications, the principal of which will shortly be noticed. In 1753, he resigned his professorship at Gottingen, and once more took up his abode in his native city, where he became a member of the government, and performed the functions of a magistrate and a statesman, with as much credit to himself, and advantage to the public, as he had performed those of a university teacher. In the intervals of retirement from his magisterial and political engagements, he prosecuted his inquiries in general philosophy and medical sciences; and in particular put a finishing hand to his great and unrivalled work, entitled Elementa Physiologiæ; the materials for which he had been collecting and accumulating for a series of years. For some years preceding his death, his health and spirits had been much impaired; but his intellectual powers appeared undiminished even to a very advanced age; insomuch, that he continued to correspond with M. Bonnet on literary and scientific subjects, till within a very short time previous to his death, which happened in 1777, when he was in his 70th year.

Passing over his miscellaneous compositions (including his poetry, and some critical and philosophical tracts), we shall only mention his principal works relating to anatomy and physiology; for his great botanical work has been already noticed in the text, to which the present observations are annexed. Among his most valuable labours in anatomy, may be mentioned his Icones Anatomicæ, amounting to many folio fasciculi, and chiefly exhibiting accurate delineations of the arteries of different parts of the human body. Add to these, a number of anatomical tracts, first published separately, and afterwards collectively, under the title of Opuscula, Dissertationes Selectæ et Opera Minora. Besides the text book, entitled Primæ Lineæ Physiologiæ, and his great work entitled Elementa Physiologiæ, in 8 vols. 4to. he also published a number of separate tracts, (some of which have been reprinted collectively with the anatomical disputations before mentioned) on physiological subjects; such as Memoires sur les Partes sensibles et irritables; Memoires sur la Formation du cœur dans le Poulet; Memoires sur la Formation des Os; Memoires sur la Respiration, &c. of all which an account is given by the author himself, in the 2d vol. of his Bibliotheca Anatomica. Besides these, and other works wholly of his own composition, (such as his Bibliotheca Chirurgica, Bib. Med. Bib. Botanica, &c.) he was editor, with large annotations, of Boerhaave's Prælectiones; of Boerhaave's Methodus Studii Medici; of Hippocratis Opera; Celsi Opera, &c. To enumerate all the works of Haller would require a great number of pages; but the above imperfect list will suffice to show how largely he contributed to the advancement of medical science, and particularly of anatomy and physiology.

Conspectus Methodi Plantarum D. Alb. Halleri.

Plantæ.

- I. Sunt flore staminibus et petalis destitutæ, semine solo donatæ; ut Conferva.
- II. Staminibus veris et petalis destitutæ, flore aliquo et semine donatæ
  - 1. Staminibus omnino nullis, ut Lichen.
  - 2. Staminum analogis corpusculis præditæ, ut Musci.
  - 3. Epiphyllospermæ; ut Osmunda, Polypodium.
- III. Petalis destitutæ, semine flore et veris staminibus donatæ,
  - 1. Staminibus coalitis a fructu remotis, coniferæ, ut Larix.
  - 2. ——— a fructu remotis, Juliferæ; ut Salix.
  - 3. Non Juliferæ, isostemones, ut Rhamnoides.
  - 4. Meiostemones; ut Alchemilla.
  - 5. Diplostemones; ut Knawel Raii.
  - 6. Polystemones; ut Tithymalus.
  - 7. Aquaticæ variæ; ut Chara, Limnopeuce.
  - 8. Tristemones, flore plerumque bifolio; ut Gramina.
  - 9. Graminibus adfines; ut Cyperoides.
- IV. Seminibus, flore, staminibus et petalis, donatæ.
  - 1. Monocotyledones.
    - 1. Tuba destitutæ, Orchideæ; ut Orchis, Helleborine.
    - 2. —— præditæ, Liliaceæ,
      - 1. Fructu sub flore tristemone, ut Gladiolus.
      - 2. ——— hexastemone, ut Colchicum.
    - 3. Fructu intra florem,
      - 1. Monopetalum; ut Muscari.
      - 2. Hexapetalum;
        - 1. Solitarium; ut Lilium.
        - 2. Conglomeratum; ut Cepa, Porrum.
  - 2. Dicotyledones petaloideæ.
    - 1. Polystemones,
      - 1. Plerumque multisiliquæ; ut Veratrum, Butomus.
      - 2. Gymnopolyspermæ.
        - 1. Petalis circa ovarium ortis; ut Adonis, Trinitas.
        - 2. ——— e calyce ortis; ut Caryophyllata.
      - 3. Pomiferæ,
        - 1. Umbilicatæ, ut Ribes, Rosa.
        - 2. Non Umbilicatæ; ut Prunus, Cerasus.
      - 4. Multiloculares; ut Tilia, Helianthemum.
      - 5. Staminibus coalitis; ut Malva.
    - 2. Diplostemones; ut Oxys, Geranium.
    - 3. Isostemones,
      - 1. Placentiferæ,
        - 1. Vasculiferæ; ut Evonymus.
        - 2. Flore fructui innato Gymnodispermæ; ut Umbelliferæ.
      - 2. Flore fructui innato,
        - 1. Dipetalo; ut Circæa.
        - 2. Tetrapetaloideæ; ut, Asperula.
        - 3. Quinquefido; ut Opulus.
      - 3. Flore circa fructum posito.
      - 4. Cucurbitaceæ; ut Bryonia.
      - 5. Solanaceæ; ut Alkekengi, Solanum.
      - 6. Asperifoliæ; ut Echium, Symphytum.
      - 7. Dicarpæ; ut Asclepias, Pervinca.
      - 8. Hexapetalæ; ut Berberis.
    - 4. Meiostemones; ut Ligustrum, Veronica.
    - 5. Staminibus ad petala sesquialteris; ut Tetrapetalæ Cruciatæ
      - 1. Siliqua breviori,
        - 1. Uniloculares.
        - 2. Biloculares.
      - 2. ——— longiori.
    - 6. Staminibus ad petala duplis sesquitertiis; ut Papilionaceæ.
    - 7. Flore monopetalo, staminibus quatuor inæqualibus,
      - 1. Capsula uniloculari, ut Orobanche.
      - 2. ——— Biloculari; ut Digitalis.
      - 3. Seminibus quatuor nudis.
    - 8. Floribus uni semini insidentibus congregatis; ut Papposæ, Capitatæ, Corymbiferæ.

Some Account of the Phoca, Vitulus marinus, or Sea-Calf, shown at Charing-cross, in Feb.* 1742-3. *By. Ja. Parsons, M. D. F.R.S.* N° 469, p. 383.

The figures of this animal, given by Aldrovandus, Johnston, and others, being profiles, lead us into 2 errors; 1st, they make a cubit in the fore-limb, which is not visible in any shape, from the surface of the body; and, 2dly, make the posterior parts terminate in two fins, which on the contrary are really webbed feet, like those of water-fowl, consisting of 5 toes, each having 3 articulations, and ending with nails of a darkish colour.

The nails of the fore-paws are very considerable, being like the paws of a mole, contrived for crawling upon land, and partly for swimming, by a narrower web between each toe; but the hinder feet are extensive webs, serving alone to drive or row the creature in the waters.

Rondeletius, as cited by Gesner, blames Aristotle for saying this animal has nails; which is strange, as that historian is one of great reputation; for it has very considerable ones.

The animal was a female; and the viscera, examined after its death, were as follows:

The stomachs, intestines, bladder, kidneys, ureters, diaphragm, lungs, great blood-vessels, and pudenda, were like those of a cow. The hairs of the whiskers are very horny and clear. The spleen was 2 feet long, 4 inches broad, and very thin. The liver consisted of 6 lobes, each hanging as long and lank as the spleen, with a very small gall-bladder. The heart was long and flabby in its contexture in general; having a large foramen ovale, and very great columnæ carnosæ.

In the lower stomach were about 4 pounds weight of flinty pebbles, all sharp and angular, as if the animal chose them of that form for cutting the food. Probably this may be common to all the larger sea-animals, as they swallow many considerable fishes whole, that after some maceration in the first stomach, they may be more easily ground small by these pebbles in the other, for the nourishment of the creature.

The uterus is of the horned kind, each cornu being considerably thicker than the body or duct leading to them: it is very fibrous, and the fibres seem all longitudinal with the uterus and cornua, making a muscular appearance. The ovaria are very large, being granulated on the surface with the ova, under a very thin membrane; and the opening into the tubes leading to the cornua is a great hole. A drawing of this part is annexed, as it is very remarkable.

Dr. P. refers the Society to the under-mentioned authors for the other pro-

* The species here described by Dr. Parsons is the *Phoca barbata,* Lin. Gmel. and the Great Seal of Pennant.

perties of this animal; such as their love to their young, their manner of copulation, inconstancies to their females, virtues in the skin of preserving persons from thunder, who carry part of it, as Suetonius relates of Augustus Cæsar, who dreaded it very much; and also of such consent between the skin of this animal and the sea, that though it be dried and kept in the most secret place, whenever the sea is much disturbed, the hairs rise up on the skin, and lie smooth when it is calm; with many other particulars, which, if not fabulous, are very curious; viz. Aristotle, Pliny, Aldrovandus, Rondeletius, Gesner, Wolfgangius, Johnston.

As to the particular figures of the animal, that of Aldrovandus seems to have been taken from a stuffed skin, having the hinder feet like a fish-tail, and not at all like the creature. Rondeletius's figure has as little truth as the former; and that given by Gesner, in his corollary on Rondeletius, is worse than any; having the fore-parts upright like a Sphinx. This last author has another figure of the phoca, which is rather like a lump-fish, and almost triangular. These could never convey a just idea of the creature to such as delight in natural history.

The animal is viviparous, and suckles its young by the mammillæ, like quadrupeds, and its flesh is carnous and muscular. This was very young, though $7\frac{1}{4}$ feet in length, having scarcely any teeth, and having 4 holes regularly placed about the navel, as appears by the figure, which in time become papillæ.

Fig. 1. pl. 15, represents the phoca lying on the right side, that the belly, and parts of generation may be the better observed. A the fore-feet and breast; B the umbilicus and holes of the mammæ; C the external orifice of the vagina and the anus; D the hinder feet, which are webbed; E the tail.

Fig. 2 shows the uterus taken out and extended. A the body of the uterus or vagina; B the cornua uteri; C the holes leading into the slender tubes that end in the extremities of the cornua; D the ovaria; E the continuations of the peritonæum.

The Ambe of Hippocrates, for reducing Luxations of the Arm with the Shoulder, rectified. By M. le Cat, M.D. F.R.S. Abstracted from the French by P. H. Z. F.R.S. N° 469, p. 387.

M. le Cat first describes the ambe of Hippocrates, which consists of a horizontal lever A, and of a fixed point B, fig. 4. pl. 15, made of a piece of wood standing vertically, to the extremity of which the lever is joined by a hinge. The patient sitting, and his hurt arm being raised, the machine is pushed forward under the arm-pit, so that the vertical piece of wood is applied along the ribs, where the lever enters into the arm pit up to the end of the luxated bone, or even farther. This circumstance is essential, and even recommended by

Hippocrates. Sect. 6, p. 783, Fœsii. The arm is tied to this horizontal piece, and then the assistant bears upon the scapula and the clavicula, as is seen in the figures of Scultetus, pl. 21, while another presses down the lever, and thus makes the bone come into its place again.

And after noticing M. Petit's remarks (in his Treatise on the Diseases of the Bones) on this subject, he proceeds to state, that the capital defect in the ambe of Hippocrates, is that it pushes the head of the bone into its cavity, before the extension and counter extension are made. The dangerous consequences of this defect, are, according to M. Petit, 1st, that the reduction is very difficult, because the bone is not conducted by the same way it took in luxating itself, and that obstacles are met with from the parts that surround it, even the scapula itself, on which it articulates. 2dly, in making such efforts for surmounting those obstacles, we run the risk of turning inwards the cartilaginous edge of the cavity of the scapula, or the capsula ligamentosa. The second defect of the ambe of Hippocrates is, that it cannot move the luxated bone but from below upwards; consequently this machine is only proper in luxations directly downwards; and yet it is certain, that the arm luxates itself both outwards and inwards; and even it is known to all practitioners, that luxations forwards are very frequent. Here we have a great number of luxations of the arm, where the ambe becomes useless. Now, if the ambe of Hippocrates be useless in all luxations outwards, and in luxations inwards, which are very frequent, if it be dangerous in luxations downwards, the only ones it is fit for, it must be owned that this machine, so much cried up by Hippocrates, is yet very imperfect.

These imperfections are real ones, but the advantages it has are so constant, and so superior to those of any other practice, that one naturally inclines not to part with it, but becomes desirous to remove those defects it has, without which it would certainly be, as Hippocrates asserts, the most perfect of all machines used in reducing a luxated arm. For supposing an ambe, which makes a sufficient extension and counter-extension, before it leads the bone into the cavity, or at the same time it does so, and which also might lead it from the right to the left, and from the left to the right, as well as from below upwards, it is certain there can be no method to be compared to this; because there is none in which concur at once so much force and expedition, joined to such simplicity, regularity, and safety, that are quite singular. For that method in which a surgeon only employs his own strength, and that of his assistants, is commonly insufficient; and the other, in which he helps himself with the pulley, is perplexed with a great apparatus, is long, and still very much wants the hands of the surgeon, and of his assistants: all which are circumstances that render the method more complicated, and less sure.

Hence M. le Cat was induced to contrive a new ambe, in which he has endeavoured to rectify all the defects before mentioned.

Description of the new Ambe.—The basis of the whole machine is an elbow-chair all of solid wood, fig. 3, pl. 15. higher than others usually are, in order to give room to the lever to play the more freely, which cannot be lowered any farther than to the floor on which the elbow-chair stands. To prevent any uneasiness to the patient from that height of the chair, it has a foot-stool that makes part of the chair, and brings the seat to its usual height.

Each arm of the chair is pierced with a round hole, to receive the stem or foot of the ambe. If the luxation be on the right side, the foot is run through on the same side; and vice versa. The patient is tied partly to the back of the chair, partly to a piece joined to the chair on that side where the ambe is placed. This solid union of all the pieces of the machine among themselves, and with regard to the patient, furnish its action with all the force and certainty possible. The ambe of Hippocrates can play but to a small extent: it is separate from the chair in which the patient sits, and he is left to the care of the assistants; all disadvantageous circumstances, which are remedied by the new machine.

In that of Hippocrates, the body of the patient has no other support against the extension of the lever than the very vertical piece, fig. 4, on which the lever rests; this piece is narrow, has no proportion, or no union with the figure of the body to which it is applied, and consequently must change his position on that piece on the least effort the patient makes.

The foot of M. le Cat's lever has no connexion with the patient's body: there is between the foot and his body a particular piece called the bodice, represented in fig. 1, pl. 16, made to fit itself to the body; and in order to render that application easy, the part which touches the body is quilted. This bodice is fixed to the arm of the chair between 2 large iron cheeks, a, b, fig. 3, pl. 15, by 2 strong iron pins, which run through them, and are stopped at their extremities with nuts screwed on. The concave part of this piece, where the body enters, is placed perpendicularly under the end of the lever, yet so that the lever be a little farther advanced towards the patient, than the bottom of the bodice, that the lever may thrust itself the better in under the arm-pit. As there are cases where the head of the lever ought to be very short, or very near the point it rests on, and others again on the contrary, where that extremity of the lever ought to be longer, and farther off the point of its rest, the bodice of course ought to be set more backward or forward, as the end of the lever is, the direction of which it follows every where. For this reason 2 rows of holes are contrived along the sides of the bodice, and between these 2 sides a notch is cut out, to make room not only for the foot, or for the point it rests on, which may meet there, but also for a part of the lever, called its spur, which always moves towards that notch when the lever is lowered. The figures and the use of the machine will show the necessity of this construction much better than any description. From the bodice come out 2 broad straps, of the strongest leather, with their buckles. One of those straps is to go about the back of the chair, and round the body of the patient: the other goes over the shoulder, very near the articulation, and keeps the scapula and the clavicula in their situation against the efforts of the lever.

That part of the machine, which may properly be called the ambe, is composed, like that of Hippocrates, of 2 pieces; one vertical, which he called the foot of the ambe; and the other horizontal, which forms the lever. It is chiefly in these 2 pieces that this ambe differs from that of Hippocrates.

The foot is a piece made either of wood, pl. 16. fig. 2, or of iron, fig. 3. Its upper extremity is split into a sort of mortise, which receives the spur or tenant T of the lever AB, fig. 8. It is pierced by several holes, which answer to as many others on the spur. Below this mortise the foot becomes more slender and cylindrical; by this part it enters into a round hole in the arm of the chair; this slender part of the foot is pierced by several holes, in order to run an iron pin through, which lies flat on the arm of the chair, and keeps the foot raised to a height proper for the person that undergoes the operation. For the greater security, 2 pins may be run through; one which rests on the arm of the chair, and the other on the seat itself, through which the foot also passes. The iron foot, fig. 3, may be provided with a sort of large ring C, under the pin, which will render its rotation the easier. If an iron foot should be preferred, the hole for it in the arm of the chair must be made narrower, either by filling up the old one with an iron box or clout, which may be taken away, if a wooden foot be used; or we may even at first fit those holes for the iron foot, setting the wooden one quite aside.

The lever ABHD, fig. 3, pl. 15, is the most compound piece of all, and also the most important. It is made of a real lever AB, and of a piece fitted to it DG. The lever properly so called, AB, is made round on its inferior surface; the upper surface is flat; and all along on the middle of it there runs a rod, forked at the end, which fits to a groove of the same figure in the inferior surface of the sliding-piece FG, fig. 9, pl. 16. This lever diminishes towards the extremity A, where the moving power is to be applied; the other extremity, B, is somewhat rounded off at the end, the better to insinuate itself under the armpit. On this larger extremity is a kind of spur or tenant, T, the upper part of which is joined to the lever by 2 iron pins; so that, on taking out the pins, the spur comes out, and separates itself from the lever, as appears by fig. 4. It was necessary to make this spur moveable, and give it the figure of a square rule, in order to bring it quite close to the end of the lever, or set it back, as it may be necessary. For this reason the upper part of this spur ab, slides along in a mortise, or groove, of the length of one foot, contrived under the lever, beginning from its extremity B, to which answers the shoulder b, of the spur.

The rest of the tenant, or its principal part c, is fitted to enter into the mortise d, which is the uppermost part of the foot, fig. 2, 3. They are both pierced with a row of holes, through one of which must be run an iron pin, to unite them, and to form the point of rest, or the hinge of the lever. Towards the other extremity A of the lever, there is a piece of iron c, fig. 8, pl. 16, made arch-wise, under which passes the elastic tail DF, of the rod, fastened to the sliding-piece FG, and into which catch teeth, made on the said tail, as in fig. 3, pl. 15, or fig. 6 and 9, pl. 16. This iron arch ought to be very solid, because it keeps down the arm, and supports all the effort of the lever. He gives to the sliding-piece FG, which is fitted to the lever, the name of the bracer; it is a groove made of one piece of wood, represented in its situation in fig. 9, pl. 16. This piece is hollow in the upper surface, as above said, to place the luxated arm into; this cavity is quilted, and has 3 girths, H, of strong leather, with buckles, to tie the arm fast and conveniently. It has on its inferior surface a groove with a dove-tail, KK, to lay hold of the rod of the lever, and to slide in it without being separated from it, unless it be in sliding beyond the extremity B, of the lever, where it pulls out like a drawer, which is easily done, if the bracer has nothing to stop it on the lever. The extremity of the bracer, which answers to the thick end of the lever, is rounded, in order to enter jointly with it under the arm-pit; the other gives hold to the piece of iron DE, called above by the name of the elastic tail of the bracer. This latter consists of 4 parts: the fork F, which attaches itself to the inferior lateral surfaces of the bracer; the spring f, which is the piece that follows next, the longest and slenderest of all; the teeth E, and the handle D.

The use of the new Ambe.—The patient, being undressed down to the waist, is placed in the arm-chair, as in fig. 6, pl. 16. Next, the lever, furnished with its bracer, is raised and kept in a horizontal position, taking great care, as Hippocrates recommends, to push this bracer as far as may be under the arm-pit to the end of the bone of the arm, and even beyond, if possible, so that the humerus, supported by the bracer in all its length, may be secure against all the power of this machine, and that its violence may only act on those muscles which keep this bone out of its place. Besides the quilting, which the bracer is lined with, a small cushion is put on its extremity, in order to lodge still more conveniently the head and the neck of the humerus, and to preserve the soft parts from any contusion, which the impulse of the machine might produce, by its greatest forces acting on that part.

The arm being thus placed, and well stretched out on the bracer, you tie about it 2 sliding knots, one above the elbow, and the other over the wrist, after having guarded those parts with a very thick and soft compress; the 2 sliding knots are fastened to the fork of the elastic tail of the bracer; after which you complete the fixing of the arm with the 3 girths of the bracer, under which are also put compresses like those just mentioned.

The arm being thus well adjusted, you endeavour to give to the body, and to the hollow of the articulation of the luxated bone, the proper situation and steadiness necessary for the success of the operation, which is easily executed with this machine, by the girths of the bodice, of which the horizontal one keeps the patient's breast closely applied against this piece, and the vertical girth retains the scapula, the clavicula, in short, all the parts where the bone is to be pushed back, in a situation proper for receiving it, and for not deviating by yielding to the efforts of the machine.

Every thing being thus disposed, the surgeon places himself behind the patient, mounted on something that raises him high enough to inspect the effects of the process; to examine by the touch where it operates: in short, to conduct the whole both by feeling and by the eye. The surgeon being placed, the assistant who is to conduct the extremity of the lever, works it according to his directions, but exceedingly slow, that the extensions may be made with less pain, and more effectually.

When the luxation is below, it is sufficient for its reduction to lower the extremity of the lever, as is done with the ambe of Hippocrates. But here appears a great difference between the working or playing of these 2 sorts of levers. The ambe of Hippocrates is a plain lever AB, fig. 7, pl. 16, the motion of which is from A to a, and consequently has for its extension only the space ca, when it is brought to its last term of becoming perpendicular, ab, while it has all AC, or ia, for its elevation. The ambe of Hippocrates therefore only raises the bone of the arm, without scarcely stretching it; and this is the defect which M. Petit, with reason, blames it for; and which is still more sensible, if we take the action of the lever in D, the point whereabouts it must meet the edge of the cavity, and may cause those mischiefs that are apprehended from it; but instead of placing the fixed point of that lever in 1, lower it to 2, by means of the tenant 1 2; then the direction of the end of the lever becomes VE: its elevation is but 1h; and the extension it produces is AE, or DE: if you lower still the lever's point of rest, as in 3, by a longer spur, the elevation of its extremity is reduced to 1k; and the extension it produces, reaches from A to F, if we carry those levers as far as they will go, which is never necessary In short, it will be in your power to give to this lever an extension as great as you please, joined to a very small elevation. To this end you need only set more backward the lever's point of rest, along the perpendicular marked in fig. 7. Now this is precisely what the spur does, which we have added to our ambe; the holes it is pierced with, as well as the mortise of the foot, are placed in different degrees, as the points 1, 2, 3; and these holes, as has been said, are the places of the pin which forms the lever's hinge or point of rest.

The gradation of those holes therefore enables us to augment at will the extension, while the elevation diminishes in the same proportion; but if we want the elevation to diminish more or less than in the foresaid proportion, for instance, to make a great extension, and a very small elevation, there is nothing easier for it than this machine. We need only push the spur 1 3, which is moveable, towards the end of the lever to L, and stop it there: then the end of the lever AL, being very short, it has but little room to play; on the contrary, to have a great elevation, we need only bring back the said spur to M, or I, or still farther; the farther we remove from the end of the lever, the more it will have room to play, and the more considerable will be its elevation. It is true, the power of the lever will decrease in the same proportion; but this power is so great, that losses like this ought to be reckoned for nothing.

We have it therefore in our power with this sort of ambe, to make, as occasion requires, such extensions and counter-extensions as we please; and we may also vary all the degrees of the elevation, which shall be necessary to give to the bone to be reduced; and these are the perfections which have been hitherto required in this machine.

Commonly, when the bone of the arm is sufficiently stretched and raised, so as to be on a level with the cavity of the articulation, those bones replace themselves as it were, because this level is not always exact; on the contrary, the extension and counter-extension being never regular enough to hinder the scapula, which is a moveable part, from following a little the head of the bone, or its extension, it mostly happens that this head bears pretty strongly against the edge of the cavity, and consequently does not fail to fall into the said cavity, as soon as it has only passed its edge, and even before it has met the level, or the axis of the hollow of the articulation; but it is otherwise after an extension, a counter extension, and an elevation so regular as those which may be performed by this machine; it may happen, that after the 3 preceding operations, the head of the bone, without having touched the edge of the cavity, will be placed over-against this cavity, and on a level with its axis, without being able to enter into it, by reason of the firmness and exactness of the powers for retaining the opposite parts in this state of regular extension; and in this case there will remain for us, in order to finish the operation, to conduct the head of the bone into its cavity, or to let it go into it: but what shall we do then? If we slacken the extremity of the lever, or if we lift the same up, we bring the head back to the same place where we took it up; that is, the luxation to its former state. If we resolve to relax the running knots, the operation will be long, and the patient will have time enough to cry out.

In order to avoid these inconvenients, M. le Cat mounted the bracer on the lever in a groove, and he stopped it in this state by the teeth of its elastic tail; by means of this construction, when the surgeon perceives that the bone is over-against its cavity, he directs the assistant, who attends the extremity of the lever, to press upon the handle D, fig. 6, of the elastic tail of the bracer, that the teeth placed under the arch C, near the said handle, may quit their hold, and that the whole bracer, which is now no longer stopped, may slide on the lever towards the patient, and by this means let the head of the bone enter into its cavity.

The necessity of this management with our ambe, is a demonstration that it is far from having that capital fault, with which M. Petit reproaches the ambe of Hippocrates; viz. "that it pushes the head of the bone into its cavity, before the extension and counter-extension are made."

If it be feared that the re-entering of the head of the bone might be too sudden, and occasion a shock, that might hurt the bones, it will be easy to prevent it, by substituting to the stop, into which the teeth of the bracer catch, a toothed wheel A, fig. 5, having in its centre a handle BD; which handle during the operation will be stopped by the iron C, fixed on this piece by the screw F; the said handle will also stop the teeth E, that catch into the toothed wheel; and when the bracer is to be loosened, the assistant, who holds the lever with one hand, will take the handle with the other, and having got the screw F taken off, he will remove from the piece C, that stops it, the part DB of the handle, by means of its moveable arbor D, so that the handle will come at a right angle, as it is represented by dots: then the assistant's hand, sustaining all the effort of the handle and of the bracer, will moderate by the handle the sliding of the bracer, and the entering of the head of the bone into its cavity, with all the slowness he shall think proper for this operation.

Thus much concerning the reduction of a luxation of the arm below, which is the only kind of luxation in which the ambe of Hippocrates can be used. He has succeeded in remedying this defect by the simplest thing in the world, viz. by giving to the foot, that enters into the arm of the chair, a cylindrical shape, by which means it can turn all manner of ways; so that if the luxation be forwards, we need only turn the extremity of the lever accordingly, lowering it at the same time enough to make the necessary extension and elevation; by this turn of the extremity of the lever forward, the head of the bone is of necessity carried backward, and replaced into its cavity. It is easily conceived, that we must go to work in the opposite way when the luxation is backward, and so on as for the rest; all according to the directions of the surgeon placed at the articulation, who is to be attentive to examine the state of the parts, and to order in what direction, and how much is necessary to be done.

*Continuation of the Account of a Treatise of Fluxions, &c. Book 2. By Colin Maclaurin, F. R. S.** N° 469, p. 403.

In the first book, the author described the method of fluxions, and its application to problems of different kinds, without making use of any particular signs or characters, by geometrical demonstrations, that its evidence might appear in the most simple and plain form. In the second book, he treats of the method of computation, or the algebraic part; to the facility, conciseness, and great extent of which, the improvements that have been made by this method are in great measure to be ascribed. In order to obtain these advantages, it was necessary to admit various symbols into the algebra; but the number and complication of those signs must occasion some obscurity in this art, unless care be taken to define their use and import clearly, with the nature of the several operations. An example of this is given by an illustration of one of the first rules in algebra. As it is the nature of quantity to be capable of augmentation and diminution, so addition and subtraction are the primary operations in the sciences that treat of it. The positive sign implies an increment, or a quantity to be added. The negative sign implies a decrement, or quantity to be subtracted; and these serve to keep in our view what elements enter into the composition of quantities, and in which manner, whether as increments or decrements. It is the same thing to subtract a decrement as to add an equal increment. As the multiplication of a quantity by a positive number implies a repeated addition of the quantity, so the multiplication by a negative number implies a repeated subtraction: and hence to multiply a negative quantity, or decrement, by a negative number, is to subtract the decrement as often as there are units in this number, and therefore is equivalent to adding the equal increment the same number of times; or, when a negative quantity is multiplied by

* See the beginning of this account, N° 468, p. 632 of this vol.

a negative number, the product is positive. When we inquire into the proportion of lines in geometry, we have no regard to their position or form; and there is no ground for imagining any other proportion between a positive and negative quantity in algebra, or between an increment and a decrement than that of the absolute quantities or numbers themselves. The algebraic expressions, however, are chiefly useful, as they serve to represent the effects of the operations; and such expressions are not to be supposed equal that involve equal quantities, unless the operations denoted by the signs are the same, or have the same effect. Nor is every expression to be supposed to represent a certain quantity; for if the $\sqrt{-1}$ should be said to represent a certain quantity, it must be allowed to be imaginary, and yet to have a real square; a way of speaking which it is better to avoid. It denotes only, that an operation is supposed to be performed on the quantity that is under the radical sign. The operation is indeed in this case imaginary, or cannot succeed; but the quantity that is under the radical sign, is not less real on that account. The author mentions those things briefly, because they belong rather to a treatise of algebra than of fluxions, wherein the common algebra is admitted.

In order to avoid the frequent repetition of figurative expressions in the algebraic part, the fluxions of quantities are here defined to be any measures of their respective rates of increase or decrease, while they are supposed to vary (or flow) together. These may be determined by comparing the velocities of points that always describe lines proportional to the quantities, as in the first book; but they may be likewise determined, without having recourse to such suppositions, by a just reasoning from the simultaneous increments or decrements themselves. While the quantity A increases by differences equal to a, 2A increases by differences equal to $2a$, and, supposing m and n to be invariable, $\frac{m\text{A}}{n}$ increases by differences equal to $\frac{ma}{n}$, and therefore at a greater or less rate than a, in proportion as m is greater or less than n. Thus a quantity may be always assigned that shall increase at a greater or less rate than A, i. e. shall have its fluxion greater or less than the fluxion of A, in any proportion; and a scale of fluxions may be easily conceived, by which the fluxions of any other quantities of the same kind may be measured.

Let B be any other quantity whose relation to A can be expressed by any algebraic form; and while A increases by equal successive differences, suppose B to increase by differences that are always varying. In this case, B cannot be supposed to increase at any one constant rate; but it is evident, that if B increase by differences that are always greater than the equal successive differences by which $\frac{m\text{A}}{n}$ increases at the same time, then B cannot be said to increase at a less rate than $\frac{mA}{n}$; or if the fluxion of A be represented by a, the fluxion of B cannot be less than $\frac{ma}{n}$. And if the successive differences of B be always less than those of $\frac{mA}{n}$, then surely B cannot be said to increase at a greater rate than $\frac{mA}{n}$; or the fluxion of B cannot be said to be greater in this case than $\frac{ma}{n}$.

From those principles the primary propositions in the method of fluxions, and the rules of the direct method, with the fundamental rules of the inverse method, are demonstrated. We must be brief in our account of the remainder of this book. The rule for finding the fluxion of a power is not deduced, as usually, from the binomial theorem, but from one that admits of a much easier demonstration from the first algebraic elements, viz. that when n is any integer positive number, if the terms E^{n-1}, $E^{n-2}F$, $E^{n-3}F^2$, $E^{n-4}F^3$, ... F^{n-1}, (wherein the index of E constantly decreases, and that of F increases by the same difference unit) be multiplied by $E - F$, the sum of the products is $E^n - F^n$; from which it is obvious, that when E is greater than F, then $E^n - F^n$ is less than $nE^{n-1} \times \overline{E - F}$ but greater than $nF^{n-1} \times \overline{E - F}$.

The rules are sometimes proposed in a form somewhat different from the usual manner of describing them, with a view to facilitate the computations both in the direct and inverse method. Thus, when a fraction is proposed, and the numerator and denominator are resolved into any factors, it is demonstrated, that the fluxion of the fraction divided by the fraction, is equal to the sum of the quotients, when the fluxion of each factor of the numerator is divided by the factor itself, diminished by the quotients that arise by dividing in like manner the fluxion of each factor of the denominator by the factor.

The notation of fluxions is described in chap. 2, with the rules of the direct method, and the fundamental rules of the inverse method. The latter are comprehended in 7 propositions, 6 of which relate to fluents that are assignable in finite algebraic terms, and the 7th to such as are assigned by infinite series. It is in this place the author treats of the binomial and multinomial theorems, because of their use on this occasion, and they are investigated by the direct method of fluxions. The same method is applied for demonstrating other theorems, by which an ordinate of a figure being given, and its fluxions determined, any other ordinate and area of the figure may be computed. The most useful examples are described in this chapter, by computing the series that serve for determining the arc from its sine or tangent, and the logarithm from its number, and conversely the sine, tangent, or secant, from the arc, and the number from its logarithm.

The inverse method is prosecuted farther in the 3d chapter, by reducing fluents to others of a more simple form, when they are not assignable by a finite number of algebraic terms. When a fluent can be assigned by the quadrature of the conic sections, and consequently by circular arcs or logarithms, this is considered as the 2d degree of resolution; and this subject is treated at length. An illustration is premised of the analogy between elliptic and hyperbolic sectors formed by rays drawn from the centres of the figures; the properties of the latter are sometimes more easily discovered, because of their relation to logarithms, and lead us in a brief manner to the analogous properties of elliptic sectors, and particularly to some general theorems concerning the multiplication and division of circular sectors or arcs. When 2 points are assumed in an hyperbola, and also in an ellipsis, so that the sectors terminated by the semi-axis, and the 2 semi-diameters, belonging to those points, are in the same given ratio in both figures, then the relation between the semi-axis and the 2 ordinates drawn from those points to the other axis, is always defined by the same, or by a similar equation in both figures. This proposition serves for demonstrating Mr. Cotes's celebrated theorem, as it is extended by M. De Moivre, by which a binomial or trinomial is resolved into its quadratic divisors, and various fluents are reduced to circular arcs and logarithms. The demonstrations are also rendered more easy of the theorems concerning the resolution of a fraction, that has a multinomial denominator, into fractions that have the simple or quadratic divisors of the multinomial for their several denominators. These demonstrations are derived from the method of fluxions itself, without any foreign aid; the invariable coefficients being determined by supposing the variable quantities or its fluxions to vanish.

When a fluent cannot be assigned by the areas of conic sections, it may however be measured by their arcs in some cases; and this may be considered as the 3d degree of resolution, or the fluents may be called of the 3d order. On this occasion some fluents are found to depend on the rectification of the hyperbola and ellipsis, which have been formerly esteemed of a higher kind. The construction of the elastic curve, with its rectification, and the measure of the time of descent in an arch of a circle, are derived from hyperbolic and elliptic arcs; and the fluents of this kind are compared with those of the first or second order by infinite series. Because there are fluents of higher kinds than these, the trajectories above-mentioned, which are described by a centripetal force, that is as some power of the distance from a given centre, when the velocity of the projection is that which would be acquired by an infinite descent, or by such a centrifugal force, and the velocity is such as would be acquired by flying from the centre, are employed for representing them. A simple construction of these trajectories had been given above, by drawing rays from the centre to a right line given in position, increasing or diminishing the logarithms of those rays always in a given ratio, and increasing or diminishing the angles contained by them and the perpendicular in the same ratio. From any figure of this kind a series of figures is derived by determining the intersections of the tangents of the figure with the perpendiculars from the centre. Every series of this kind gives 2 distinct sort of fluents; and any one fluent being given, all the other fluents taken alternately from it in the series depend on it, or are measured by it; but it does not appear that the fluents of one sort can be compared with those of the other sort, or with those of any different series of this kind.

The inverse method is prosecuted farther in the 4th chapter, by various theorems concerning the area when the ordinate is expressed by a fluent, or when the ordinate and base are both expressed by fluents. The first is the 11th proposition of Sir Isaac Newton's Treatise of Quadratures. In art. 819, 820, &c. the author supposes the ordinate and base to be both expressed by fluents, and shows, in many cases, that the area may be assigned by the product of two simple fluents, as of two circular arcs, or of a circular arc and a logarithm. This subject deserves to be prosecuted, because the resolution of problems is rendered more accurate and simple, by reducing fluents to the products of fluents already known, than by having immediately recourse to infinite series. One of the examples in art. 822 may be easily applied for demonstrating, that the sum of the fractions which have unit for their common numerator, and the squares of the numbers 1, 2, 3, 4, 5, 6, &c. in their natural order, for their successive denominators, is $\frac{1}{6}$ part of the number, which expresses the ratio of the square of the periphery of a circle to the square of its diameter; which is deduced by Mr. Euler, Comment. Petropol. tom. 7, in a different manner, and other theorems of this kind may be demonstrated from the same or like principles.

The series that is deduced by the usual methods for computing the area or fluent, converge in some cases at so slow a rate, as to be of little or no use without some farther artifice. For example: the sum of the first 1000 terms of Lord Brounker's series, for the logarithm of 2, is deficient in the 5th decimal. In order therefore to render the account of the inverse method more complete, the author shows how this may be remedied, in many cases, by theorems derived from the method of fluxions itself, which likewise serve for approximating readily to the values of progressions, and for resolving problems that are commonly referred to other methods. Those theorems had been described in the first book, art. 352, &c. but the demonstration and examples were referred to this place, as requiring a good deal of computation. The base being supposed equal to unit, and its fluxion also equal to unit, let half the sum of the extreme ordinates be represented by a, the difference of the first fluxions of these ordinates by b, the difference of their 3d, 5th, 7th and higher alternate fluxions by c, d, e, &c. then the area shall be equal to $a - \frac{b}{12} + \frac{c}{720} - \frac{d}{30240} + \frac{e}{1209600} -$ &c. which is the first theorem for finding the area. The rest remaining, let a now represent the middle ordinate, and the area shall be equal $a + \frac{b}{24} - \frac{7c}{5760} + \frac{31d}{967680} - \frac{127e}{154828800} +$ &c. And this is the theorem which the author makes most use of. When the several intermediate ordinates represent the terms of a progression, the area is computed from their sum, or conversely their sum is derived from the area, by theorems that easily flow from these.

These general theorems are afterwards applied for finding the sums of the powers of any terms in arithmetical progression, whether the exponents of the powers be positive or negative, and for finding the sums of their logarithms, and thereby determining the ratio of the uncia of the middle term of a binomial of a very high power to the sum of all the unciæ. This last problem was celebrated among mathematicians some years ago, and by endeavouring to resolve it by the method of fluxions, the author found those theorems, which give the same conclusions that are derived from other methods. They are likewise applied for computing areas nearly, from a few equidistant ordinates, and for interpolating the intermediate terms of a series, when the nature of the figure can be determined, whose ordinates are as the differences of the terms.

In the last chapter, the general rules, derived from the method of fluxions for the resolution of problems, are described and illustrated by examples. After the common theorems concerning tangents, the rules for determining the greatest and least ordinates, with the points of contrary flexure, and the precautions that are necessary to render them accurate and general, which were described above, are again demonstrated. Next follow the algebraic rules for finding the centre of curvature, and determining the caustics by reflexion and refraction, and the centripetal forces. The construction of the trajectory is given, which is described by a force that is inversely as the 5th power of the distance from the centre, because this construction requires hyperbolic and elliptic arcs, and because a remarkable circumstance takes place in this case, and indeed in an infinity of other cases, which could not obtain in those that have been already constructed by others, viz. that a body may continually descend in a spiral line towards the centre, and yet never approach so near to it as to descend to a circle of a certain radius; and a body may recede for ever from the centre, and yet never arise to a certain finite altitude. The construction of the cases wherein this obtains, is performed by logarithms or hyperbolic areas, the angles described about the centre being always proportional to the hyperbolic sectors, while the distances from the centre are directly or inversely as the tangents of the hyperbola at its vertex. The circle is an asymptote to the spiral; and this can never be, unless the velocities requisite to carry bodies in circles increase while the distances decrease, or decrease while the distances increase, in a higher proportion than the velocity in the trajectory; that is, unless the force be inversely as a higher power of the distance than the cube. Next follow theorems for computing the time of descent in any arc of a curve, for finding the resistance and density of the medium, when the trajectory and centripetal force are given, and for defining the catenaria and line of swiftest descent in any hypothesis of gravity.

Then the usual rules are derived from the inverse method for computing the area, the solid generated by it, the arc of the curve, and the surface described by it, revolving about a given axis. The meridional parts in a sphere, and any spheroid, are determined with the same accuracy, and almost equal facility. The attraction of a spheroid at the equator, as well as at the poles, is determined in a more general manner than in the first book, or in a piece of the author's, published at Paris in 1740, which obtained a part of the prize proposed by the Royal Academy of Sciences for that year. Several mechanical problems are resolved, concerning the proportion the power ought to bear to the weight, that the engine may produce the greatest effect in a given time; and concerning the most advantageous position of a plane which moves parallel to itself, that a stream of air or water may impel it with the greatest force, having regard to the velocity which the plane may have already acquired. On this occasion, it is shown, that the wind ought to strike the sails of a wind-mill in a greater angle than that of 54° 44′, against what has been deduced from the same principles by a learned author. The same theory is applied to the motion of ships, abstracting from the lee-way, but having regard to the velocity of the ship; and among other conclusions it appears, that the velocity of a vessel of one sail may be greater with a side-wind, than when she sails directly before the wind; which, perhaps, may be the case of those seen by Captain Dampier in the Ladrone Islands, that sailed at the rate of 12 miles in half an hour with a side-wind.

The remainder of this chapter is employed in reducing equations from second to first fluxions; constructing the elastic curve by the rectification of the equilateral hyperbola; determining the vibrations of musical chords; resolving problems concerning the maxima and minima, that are proposed with limitations, relating to the perimeter of the figure, its area, the solid generated by this area, &c. with examples of this kind concerning the solid of least resistance; and concludes with an instance of the theorems by which the value of the ordinate may be determined from the value of the area, by common algebra, and by observing, that it is not absolute, but relative space and motion, that is supposed in the method of fluxions.

Observations on the Mouth of the Eels in Vinegar, and also a strange Aquatic Animal; in a Letter from the Rev. Mr. Henry Miles to Mr. Baker, F. R. S.* N° 469, p. 416.

This observation was made with the camera obscura microscope: first, in a very small tube, Mr. M. put a small quantity of vinegar, with several anguillæ: at first sight of the image on the screen, one had a motion as if it had been wounded, about the middle of the back; it neither rose nor sunk in the liquor, but lay in this form , wriggling itself, as if giving signs of pain, and would soon expire, which it accordingly did in a minute's time; but it coiled itself up, and stuck to the side of the tube very close. He put out the liquor, after waiting to see whether it would revive, in vain, and viewed it several times in the common light, in which way he had the most distinct appearance. The larger end, which may be called the head, was stretched out from the rest of the body, a little way, as in the figure, which gave an opportunity of examining what mouth it had. On the first view of it in common light, he saw what he thought may be called the mouth: repeated trials in different lights and positions, and with different magnifiers, confirmed the suspicion. After the strictest and most exact observation he could discern it to be nothing more than a transparent tube. Where the instruments of nutrition, and the springs of life, are, he doubts not we shall soon discover.


a

In this figure, *a* is the mouth, which seemed to be as wide open as it possibly could be. The figure is too small to give a just idea of the shape of the mouth, but it had the appearance which a tube, or rather a cone, would make cut slopewise.

Mr. H. Miles also sent some specimens of an odd aquatic animal,† found in standing water: he kept some of them in their own element in the house, but they all died in a day and half's time. They seem to be nothing but skin, and

* The vibrio aceti, Linn. Gmel. is sometimes considered as a variety only of the vibrio glutinis or paste eel, from which it chiefly differs in being more slender and transparent.

† The supposed animal here described by Mr. Miles, is nothing more than the seed of the plant called bidens tripartita, which usually grows in watery places, and the seeds of which have a kind of elastic motion when touched.

seem no thicker when alive: they have the power, as most aquatic insects have, of sinking to the bottom on the approach of a stick, &c. and fall like a piece of rotten wood, or leaf. When taken out of the water, if laid on a paper, &c. they will spring away like a grashopper.

P. S. The animals were caught the day before, and kept in water in a glass; and when he had finished his letter, and went to pack them up in paper, he found none of them left, as he thought, at first; but on a nearer view he found they were all collected together in a knot, which he took for some filth in the water, till he more carefully viewed them, and found them hanging together by the tails.

An Extract from the Books of the Town-Council of Edinburgh, relating to a Disease there, supposed to be Venereal, in the Year 1497, *in a Letter from Mr. Macky, to Mr. Maclaurin.* N° 469, p. 420.

If the venereal disease was never known in Europe till the siege of Naples 1495, it must have made a very quick progress indeed through Europe; for in 1497, I find it raging in Edinburgh, and the king and his council terribly alarmed at this contagious distemper, as appears from a proclamation of King James the 4th, in the records of the Town-Council of Edinburgh. The minute of council is dated the 22d of September. I have taken a copy of it for your amusement, and, if you please, you may communicate it to the Society. I have pretty nearly observed the old spelling, except in numbers.

Sept. 22, 1497.—" It is our Soverane Lords will and the command of the Lordis of his Counsale send to the Provest and Baillies within this bur[t] that this proclamation followand be put till execution for the eschewing of the greit appearand danger of the infection of his leiges fra this contagious sickness callit the grandgor and the greit uther skayth that may occur to his leiges and inhabitans within this bur[t]; that is to say, we charge straitly and commands be the auhority above writtin, that all manner of personis being within the freedom of this bur[t] quilks are infectit or hes been infectit uncurit with this said contagious plage callit the grandgor, devoyd, red and pass fur[t] of this town and compeir apon the sandis of Leith at ten hours before none and thair sall thai have and fynd botis reddie in the havin ordanit to them be the officeris of this bur[t] reddely furneist with victuals to have thame to the Inche,* and thair to remane quhill God proviyd for thair health: and that all uther personis the quilks taks upon thame to hale the said contagious infirmitie and taks the cure thairof that they devoyd and pass with thame sua that nane of thair personis quhilks taks sic cure upon thame use the samyn cure within this bur[t] in pns nor peir

* An island in the Frith of Edinburgh over against Leith.

any manner of way. And wha sa beis foundin infectit and not passand to the Inche as said is be Mononday at the Sone ganging to, and in lykways the said personis that takis the sd cure of sanitie upon thame gif they will use the samyn thai and ilk ane of thame salle be brynt on the cheik with the marking irne that thai may be kennit in tym to cum and thairafter gif any of tham remanis that thai sall be banist but favors."

Some Account of the Insect called the Fresh-water Polypus, beforementioned in these Transactions,* N° 466 *and* 467. *By Martin Folkes, Esq. Pres. R. S.* N° 469, p. 422.

After several experiments and observations on these animals, much of the nature of those in the papers referred to in N^{os} 466 and 467, which it is not necessary here to repeat, Mr. Folkes gives a description of several magnified views of the animal, as referring to the several states of it, described in the paper, as follows:

Pl. 17, fig. 1 represents a polypus as seen in the microscope, when in a state of extension, the arms spread as when feeling for their prey, and the mouth sharp and prominent. Fig. 2 and 3 represent the same insect in its most contracted state. Fig. 4 and 5 show the insect when in a middle state of contraction; the body is then wrinkled, so as to appear somewhat like a grub or earth-worm. Fig. 6 is a polypus with a young one growing from its side, and another from that again: this is not so much extended as that in fig. 1, and is to be supposed to have taken lately some food, by which the cavity of the inside is made more conspicuous, and the communication of the guts of the young ones with those of the parents becomes sensible. Fig. 7 shows the appearance of a polypus, that has already swallowed the best part of a worm endwise. He is grasping the remaining part to draw that in also. Fig. 8 represents a polypus, whose mouth is greatly extended: he has just taken in the middle part of a worm; the opening of the mouth is there remarkable, the arms seem somewhat contracted from the effort in stretching the mouth so wide; the neck also may be there observed between the mouth and the stomach, but which will soon disappear as the worm is sucked further in. Fig. 9 is another polypus, nearly in the same state as the last; but the worm is omitted in the figure, to show the form of the mouth more distinctly. Fig. 10 shows the same polypus when the worm is drawn quite double into his stomach; here the neck entirely disappears, and the whole is like an open bag or purse. Fig. 11 is the same polypus, after he has entirely swallowed his worm; the mouth is now again

* Zoophyte; see the remark subjoined to Dr. Mortimer's paper, p. 623 of this vol. of these Abridgments.

closed and contracted, and the worm may be discovered through the skin, as it lies coiled in his stomach. In these last 5 figures it may be noted, that, however extended and swelled the stomach of the insect appears, the posterior part is not stretched in proportion, but discovers itself every where as a small tail, in which is contained a gut, with which the stomach communicates. Fig. 12 shows one of the horns or arms of a polypus very much magnified, for giving some imperfect idea of the knots or papillæ in the transparent membranous substance, of which it is composed. Fig. 13 represents a polypus that had several young growing from it at once, some of which had also others springing out from them again. Besides those here represented, 8 other young ones had at several times separated themselves from him, since the insects were received.

An Account of a Book intitled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder; and an Investigation of the resisting Power of the Air to swift and slow Motions. By B. R. F. R. S. as far as the same relates to the Force of Gunpowder.* N° 469, p. 437.

This treatise contains 2 chapters. The first treats of the force of gunpowder, and the velocities communicated to bullets by its explosion: the 2d considers the resistance of the air to bullets and shells moving with great velocities; and endeavours to evince, that this resistance is much beyond what it is generally esteemed to be; and consequently that the track described by the flight of these projectiles, is very different from what is usually supposed by the modern writers on this subject.

The principal points endeavoured to be established in the first chapter are these, " That the force of fired gunpowder is no more than the action of a permanent elastic fluid, which is produced by the explosion; that this fluid observes the same laws with common air in their exertion of its pressure or elasticity;" and consequently, " That the velocities communicated to bullets by the explosion may be easily computed from the common rules, which are established for the determination of the air's elasticity."

The first 2 propositions contain the proofs that a permanent elastic fluid is constantly generated in the explosion of gunpowder. The 3d proposition is, that the elasticity of this fluid produced by the firing of gunpowder, is, cæteris paribus, directly as its density. This is proved both by the author's own experiments, and by several of Mr. Hauksbee's; when, by the firing of 26 quantities of powder successively, the mercurial gauge was sunk from $29\frac{1}{4}$ inches, to

* This account was given by the very ingenious author himself, Mr. Benjamin Robins.

$12\frac{4}{7}$; for by comparing these experiments together, and making the necessary allowances, it will be found, that the elasticity was nearly proportional to the density in all that variety of densities.

In this proposition, the analogy between the fluid produced by the explosion of powder and common air, is established thus far, that they exert equal elasticities in like circumstances; for this variation of the elasticity, in proportion to the density, is a well-known property of common air. But other authors, who, since the time of Mr. Boyle, have examined the factitious elastic fluids produced by burning, distillation, &c. have carried this analogy much further, and have supposed these fluids to be real air, endued with all the properties of that we breathe; particularly Dr. Hales, who has pursued this examination with the greatest exactness, in a series of the best contrived processes, constantly affixes the denomination of air to these factitious fluids, he having found that their weight is the same with that of common air, and that they dilate with heat, and contract with cold; and that they vary their densities, under different degrees of impression, in the same proportion with common air; and from hence, and other circumstances of agreement between them, he supposes them to be of the same nature with air, and conceives them to be fitly designed by the same name.

But so perfect a congruity between these factitious fluids and air, is not necessary for the purposes of this treatise. The fundamental positions of this first chapter supposing no more, than that the elasticity of the fluid, produced in the explosion of gunpowder, is always, cæteris paribus, as its density; and that the force of fired gunpowder is only the action of that fluid modified according to this law.

The law of the action of this fluid being determined, 2 methods offer themselves for investigating the absolute force of powder on the bodies it impels before it. The first by examining the quantity of this fluid produced by a given quantity of powder, and thence finding its elasticity at the instant of the explosion: the other by determining the actual velocities communicated to bullets by known charges, acting through barrels of different dimensions. The first is the most easy and obvious, but the 2d the most accurate method; and therefore the author has separately pursued each, and he has found, that their concurrence has greatly exceeded his expectation, and thereby both of them receive an additional confirmation.

By the method described, it is collected, that the elasticity of the fluid produced from fired gunpowder, when contained in the space which was taken up by the powder before the explosion, is about 1000 times greater than the elasticity of common air, or, which is the same thing, 1000* times greater than the pressure of the atmosphere.

But, besides the determination of the quantity of fluid produced from a given quantity of powder, the method on which this deduction is founded, there is another method of discovering the same thing, which, though less obvious, is yet, as hath been already observed, more accurate: that is, by examining the actual velocities communicated to bullets by the explosion of given charges in given cylinders; and this is the subject of the 7th, 8th, and 9th propositions.

And first, it is evident, that this examination cannot take place, unless a method of discovering the velocities of bullets be previously established. Now the only known means of effecting this was, either by observing the time of the flight of bullets through a given space; or by finding their ranges when they were projected at a given angle, and thence computing their velocity on the hypothesis of their parabolic motion. The first of these methods was often impracticable, and in all great velocities extremely inaccurate, both on account of the shortness of the time of their flight, and the resistance of the air. The 2d is still more exceptionable, since, by reason of the air's resistance, the velocities thus found may be less in any ratio given, than the real velocity sought. Now, to avoid these difficulties, the author invented a method of determining the velocities of bullets, which may be carried to any required degree of exactness, and is nowise liable to the forementioned exceptions; for, by this invention, the velocity of the bullet is found in any point of its track. independent of the velocity it had before it arrived at that point, or of the velocity it would have after it had passed it: so that not only the original velocity, with which it issues from the piece, is hence known, but also its velocity, after it has passed to any given distance; and therefore the variations of its velocity from the resistance of the air may be also ascertained with great facility. The machine for this purpose is described in the 8th proposition, and the principle it is founded on is this simple axiom of mechanics; that if a body in motion strikes on another at rest, and they are not separated after the stroke, but move on with one common motion, then that common motion is equal to the motion with which the first body moved before the stroke: whence, if that common motion and the masses of the two bodies are known, the motion of the first

* By more accurate and extensive experiments, since made, I have found that this number, denoting the first strength of the fired gunpowder, varies very much on several accounts, but chiefly on the different quantities that are employed in the experiments. Thus, by firing different quantities at a time, I have found that, by using from 4 to 8 ounces at once, that number varies from 1150 to near 1600; being gradually always larger as the charge is higher. See the end of prop. 18 of my Course of Mathematics, vol. ii. p. 357, ed. 4th. C. H.

body before the stroke is thence determined. On this principle then it follows, that the velocity of a bullet may be diminished in any given ratio, by its being made to impinge on a body of a weight properly proportioned to it ; and hereby the most violent motions, which would otherwise escape our examination, are easily determined by these retarded motions, which have a given relation to them. Hence then, if a heavy body greatly exceeding the weight of the bullet, whose velocity is wanted, be suspended so that it may vibrate freely on an axis in the manner of a pendulum, and the bullet impinges on it when it is at rest, the velocity of the pendulum after the stroke will be easily known by the extent of its vibration, and from thence, and the known relation of the weight of the bullet and the pendulum, and the position of the axis of oscillation, the velocity with which the bullet is impinged will be determined, as is largely explained in the 8th proposition. Where note, that there is a paragraph by mistake omitted in that proposition, which should increase the velocity there found in the duplicate proportion of the distances of the points of oscillation and percussion from the axis of suspension ; but this only affects that particular number, for it was remembered in the computations of the succeeding experiments, the numbers of which are truly stated.

It being explained how the velocities of bullets may be discovered by experiment: the next consideration is, from those velocities to determine the force which produced them. And the author thought, the best method of effecting this was by computing what velocities would arise from the action of fired powder, supposing its force to be rightly assumed by the process in the preceding part; that is, supposing the elasticity of the fluid thence arising to be at first 1000 times greater than that of common air; for then, by comparing the result of these computations with a great number of different experiments, it would appear whether that force was rightly assigned; and if not, in what degree it was to be corrected.

The 7th proposition is employed in computing the velocity which would be communicated to a bullet in a given piece by a given charge of powder, on the principles hitherto laid down, that is, supposing the elasticity of fired powder to be at first 1000 times greater than that of common air.

In the 9th proposition these computations are compared with a great number of experiments, made in barrels of various lengths, from 7 inches to 45 inches, and with different quantities of powder, from 6 dwt. to 36 ; and the coincidence between the theory and these experiments is very singular, and such as occurs in but few philosophical subjects of so complicated a nature.

By this agreement between the theory and the experiments, each part of the theory is separately confirmed; for by firing different quantities of powder in the same piece, and in the same cavity, it appears that the velocities of the bullet, thence arising, are extremely near the subduplicate proportion of those quantities of powder, and this independent of the length of the piece: whence it is confirmed, that the elasticity of fired powder in various circumstances, is nearly as its density; and this does not only succeed in small quantities of powder, and in small pieces, but in the largest likewise, under proper restrictions; at least there are experiments which could not be influenced by this theory, where the quantities of powder were above 100 times greater than what are used by this author, and in these trials this circumstance takes place to sufficient exactness.

It is presumed then, that by this theory a near estimate may be always made of the velocities communicated to shells or bullets by given charges of powder; at least these experiments evince how truly the velocities of small bullets are hereby assigned; and the author can show by the experiments of others, that in a shell of 13 inches diameter, impelled by a full charge of powder, the same principle nearly holds: it is true indeed, that when the charge is much smaller than the usual allotment of powder, there are some irregularities, which are particularly mentioned at the end of the 9th proposition; but in the customary charges, the velocities of bullets resulting from all the experiments hitherto made, are really such as the theory laid down in the preceding part of this treatise requires. And it appears, that these velocities are much greater than what they have been hitherto accounted: and there are reasons from the theory to believe, that in cannon-shot the velocities may still exceed the present computation.

With regard to the 2d chapter of this treatise, relating to the resistance of the air, the author has in his preface mentioned his intention of annexing to it a series of experiments, on the real track of bullets, as modulated by that resistance: and therefore, as he proposes to complete those experiments* this summer, unless unforeseen accidents prevent him, he chooses to postpone any account of the subject of the 2d chapter till that time.

Observations of a Comet, made at Vienna, in Feb. 1743. *By the Fa. Frantz.* N° 470, p. 457.

This comet passed through the constellations Ursa Major, Draco, Leo, Virgo, &c.

* These experiments it seems were never completed.

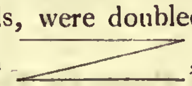
An Abstract of some new Observations on Insects. By M. Charles Bonnet of Geneva. N° 470, p. 458.

[For the observations contained in this long paper, the reader is referred to Mr. Bonnet's Treatise, entitled Insectologie, published a year or two afterwards.]

An extraordinary Case of the Bones of a Woman becoming soft and flexible. By Mr. Sylvanus Bevan, F. R. S. N° 470, p. 488.

The wife of one B. S., in the year 1738, was taken with a diabetes, with the usual symptoms, viz. a frequent and copious discharge by urine, a gradual wasting of the body, a hectic fever, with a quick low pulse, thirst, great pains in her shoulders, back, and limbs, and loss of appetite. She continued thus two years, much emaciated, though using the common medicines; at which time she was attacked with an intermittent, which soon left her; after which the diabetes gradually decreased, so that in a few months she was free from that disorder, but the pains in her limbs still continued. She recovered her appetite, breathed freely, and her hectic much lessened, though she had some appearance of it at times.

About 18 months since she had such a weakness and pains in her limbs, that it confined her to her bed altogether; and in a few months the bones in her legs and arms felt somewhat soft to the touch, and were so pliable, that they were bent into a curve; but, for several months before her death, they were as limber as a rag, and would bend any way, with less difficulty than the muscular parts of a healthy person's leg, without the interposition of the bones.

April 12, 1742, after a tedious illness, she died, near the age of 40: and, with the consent of her friends, Mr. B. had the curiosity to examine more particularly into the several matters beforementioned. On raising the cutis, he found the membra adiposa much thicker than he expected in a person so much emaciated: the sternum and ribs, with their cartilages, were very soft; and all the cartilaginous parts of the ribs, at their articulations, from the clavicle downwards, were doubled over each other on the left side, about an inch, in this form , only flatter. On raising the sternum, he found the lungs adhered very close to the ribs, for 4 or 5 inches on each side; but were more loose and flaccid than usual, and much less in size: her heart was of the common size. Upon viewing the liver, he found it at least a third part larger than common; and the spleen was about $1\frac{1}{2}$ inch in the longest part, and $\frac{1}{2}$ thick: the intestines were very much inflated.

She had appearances of several anchylosseses formed in the small joints, viz. carpal and metacarpal bones; but on laying them open, he found them only like a thin shell: the cartilaginous epiphyses of the bones were entirely dissolved, and no parts of the heads of the bones remaining, but an outside, not thicker than an egg-shell.

On making incisions in her legs and arms, 5 or 6 inches long, he found the outer laminæ of the bones soft, and become perfectly membranous, about the thickness of the peritoneum, containing, instead of a bony substance, a fluid of the consistence of honey, when it is thick, of a reddish colour, not at all disagreeable to the smell: there was no appearance of any bones in her leg and arms, except near the joints, which were in part dissolved, and what remained were very soft, and full of holes, like a honey-comb: also the bones of the head would easily give way to the pressure of the finger.

It is remarkable, that those parts of the bones that are the most compact and hard, were first dissolved, while their heads, which are more spongy and soft, had not so entirely lost their substance.

When she was in health, she was 5 feet high, but after her death she was but 3 feet 7 inches in length, though all her limbs were stretched out straight, which is 17 inches shorter than she was in her health.

Extracts of Two Letters from Dr. John Lining, at Charles-Town in South Carolina, giving an Account of Statical Experiments made on himself, several times in a Day, for one whole Year, accompanied with Meteorological Observations; to which are subjoined Six General Tables, deduced from the whole Year's Course. N° 470, p. 491.

Dr. Lining began the following experiments the first of March 1742, and continued them afterwards, with the loss only of a few days; and proposed to continue them till the year was finished; afterwards to make them a few days in every month, and as constantly as possible in epidemic seasons.

The method he has observed in the tables is this: He weighed himself twice a day, in the morning immediately after he rose, and again before he went to bed at night. As in July 1, his weight at $6\frac{1}{2}$ a. m. was lib. 165 13 0, at 10 at night was 167 5 4, &c. 12 oz. was the quantity of urine excreted from $6\frac{1}{2}$ in the morning, to $10\frac{1}{2}$ that night: and $9\frac{1}{2}$ oz. was the urine from 10 p. m. of the first day, to $7\frac{1}{2}$ in the morning of the second day. The figures placed in the next column, directly opposite to these quantities of urine, express the quantity perspired in the same space of time; thus, 68 oz. 3 drs. was perspired between $6\frac{1}{2}$ a. m. and $10\frac{1}{2}$ p. m. in the first day, and $23\frac{1}{2}$ oz. the quantity perspired from $10\frac{1}{2}$ p. m. of the first day, to $10\frac{1}{2}$ a. m. in the 2d day.

The number of pulses he took in the morning, and immediately before he went to bed at night.

In the column titled excret. alv. the quantity is in oz. and drs. When the figures are placed in the upper part of the column, that excretion was in the morning; when in the middle or lower part of the column, then it was in the middle of the day, or in the night before bed-time. Where 1, 2, or 3, occur in a column, they express the number of stools that day, as in July 6, there were 3 stools. The figures placed in all the rest of the columns, are in oz. and decimals: the calculations he made with a 2-foot sliding Gunter's scale. In the column urina 24 horarum, is the urine of 24 hours calculated each day.

In the column viginti quatuor horarum excreta, is the whole quantity excreted in 24 hours, which is found out by adding together the stools, and the urine and perspiration of 24 hours by calculation; whence the exact quantity retained, or è contra, in every 24 hours, appear in the succeeding 2 columns. By these tedious calculations, he endeavoured, as much as possible, to prepare the tables for use, that just deductions may more easily be drawn from them. In the columns ciborum quantit. et potulentorum quantit. the quantities are in oz. and drs. The weights he used are 60 grs. = 1 dr. 8 dr. = 1 oz. 16 oz. = 1 lb.

TABULA prima exhibet ciborum et potulentorum quantitatem uncialem et denariam, itemque excretorum quorumvis summam in diebus omni mense memoratis, in quibus statica feci experimenta; unde incrementum et diminutio ponderis humani per totum annum abunde patet.

Experiment. Conficiend. Dies.	Cibus.	Potus.	Urina.	Persp.	Excret. Alvin.	Ingesta quam Excreta Major.	Minora.
Mart. 13..	297.87	1282.37	971.50	548.50	43.00	17.25	——
—— 12..	332.12	1026.37	793.37	532.37	46.00	——	13.25
April 13..	310.12	1096.12	798.62	591.62	50.25	——	34.24
—— 10..	244.75	854.12	562.37	506.00	27.87	2.63	——
Maio 16..	424.25	1293.37	880.12	816.37	61.00	——	39.87
—— 14..	367.37	1431.37	804.37	927.00	42.00	25.00	——
Junio 14..	318.12	1447.50	739.87	1000.87	52.75	——	27.87
—— 15..	338.37	1535.87	780.87	1069.50	57.12	——	33.25
Julio 16..	378.37	1787.75	787.00	1301.37	66 50	11.25	——
—— 15..	378.12	1614.50	569.87	1387.37	55.75	——	20.37
Aug. 15..	398.75	1591.00	823.87	1129.37	50.37	——	13.61
—— 15..	357.37	1565.62	838.37	998.12	76.87	9.62	——
Sept. 15..	350.00	1599.25	669.62	1199.00	81.75	——	1.12
—— 15..	352.75	1244.50	532.12	1113.75	52.25	——	100.87
Oct. 16..	368.62	1134.50	749.00	642.75	80.37	31.00	——
—— 15..	373.75	1123.87	729.00	621.50	110.12	37.00	——
Nov. 15..	413.62	1284.00	981.62	609 37	64.00	42.63	——
—— 11 .	284.75	882.00	660.62	442.75	33.25	30.13	——
Dec. 13..	343.25	1186.52	875.12	555.12	47.25	52.01	——
—— 14..	383.25	1285.75	1036.75	593.75	53.75	——	15.25
Jan. 15..	357.62	1320.75	958.50	629.75	50.87	39.25	——
—— 13..	304.75	1328.37	1069.50	489.37	62.50	11 75	——
Feb. 15..	382.00	1381.87	1138.75	563.87	48.62	12.63	——
—— 13..	306 62	1244.75	1041.37	484.75	41.50	——	16. 25

TAB. II.

	Urina 24 Horarum.			Perspiratio 24 Horarum.		
	Max.	Min.	Med.	Max.	Min.	Med.
Mart.	102.20	33.40	70.59	74.75	28.00	43.23
April.	87.50	36.00	59.17	69.40	34.00	47.72
Maius	88.12	25.25	56.15	94.00	30.62	58.11
Junius	85.00	28.70	52.09	106.90	36.75	71.39
Julius	92.90	20.62	43.77	105.00	51.90	86.41
August. ..	76.50	31.00	55.41	107.00	38 90	70.91
Septemb.	78.75	11.15	40.06	130.00	42.37	77.09
Octob. ..	73.40	22.45	47.67	63.10	30.20	40.78
Novemb.	99.00	39.50	63.16	49.30	29.00	40.47
Decemb.	143.50	41.00	70.81	56.60	27.65	42.55
Jan.	121:00	39.75	72.43	49.25	33.10	39.97
Febr.	115.00	45.60	77.86	46.10	24.40	37.45

The above may serve as a specimen of the tables deduced from these very troublesome experiments.

Some further Account of Polypi, in a Letter from his Grace the Duke of Richmond, Lennox and Aubigné, F. R. S., to M. Folkes, Esq. Pr. R.S. Dated Utrecht, May 24, (*June* 4) 1743. N° 470, p. 510.

You will not be sorry to receive some further account of the polypus; and I must tell you what I have seen in M. Trembley's study at Sorgvliet. He has there 12 large glasses, of about a foot high, each holding from a gallon to 6 quarts of water, all well stocked with those insects, to the amount of many hundreds. They are, in general, considerably larger than any I had before seen; and as I was first with him on a Tuesday, and made him a second visit on the Sunday following, I had the opportunity of seeing the prodigious increase they had made in those 5 days. Several single ones that I had left, had in that time put out 5 or 6 young ones each; and those I had seen him perform operations on, were not only recovered, but had most of them produced young ones also. I saw him split the head of one about 2 o'clock in the afternoon, and at about 7 the same evening, each head ate a small worm. I saw him split another from the head to the tail, and each of those parts also ate part of a worm before night. Another operation I saw him make, which I had not before heard of, which was that by putting one of the points of a very small pair of sharp scissars into the mouth of a polypus, and forcing it out at the very end of the tail, he then laid it quite open like a pigeon, or a Barbacute pig to be broiled; yet, in about 5 hours, I saw the same polypus with the parts so reunited again, that I could not perceive any thing had been done to it; and it then ate a worm larger than itself. He then showed me another odd particular, which was one polypus that had fairly two heads, without any tail; that is, with a head at each end, as represented in fig. 1, pl. 18. This was an accidental production, and as follows: two young ones grew, as from one root, out of an old polypus, as in fig. 2. They both dropped off together, and their tails not being separated, they appeared as in the first figure; but, when I saw them, more like fig. 3, with several young ones putting out from their sides. Mr. Trembley said he had seen the like sometimes before, but not often; and that they have then remained 10 or 12 days in that condition, after which they have separated. He had in one of his large glasses upwards of a hundred of these insects all full-grown, and he regaled them all at once before me, with some thousands of what he calls des pucerons d'eau, which are small aquatic animalcules, not unlike fleas, of about the size of large ones, and which move about with great swiftness in the water. These were no sooner put in, but it was a curious and entertaining sight, to observe in how voracious a manner not only every polypus, but every young one also that had arms, though fixed to the side of its parent, seized and devoured these pucerons: and as the body of the polypus is transparent, every one made a very extraordinary appearance, from the number of pucerons in them; for in several I could very plainly, with my bare eye, distinguish and count 5 or 6 of them; and I could plainly discern some very small black spots, which I was assured were the eyes of these pucerons. One extraordinary observation more of M. Trembley's is, that, in the double-headed polypus of the 1st and 3d figure, there was at first but one common gut between them, so that the feeding of one head had the same effect as feeding them both. M. Trembley is particularly handy and dextrous in his operations, and explains himself about them with great exactness and perspicuity. He places some pieces of packthread across his glasses, towards the top: to these some of the insects fix themselves; and I have seen some that in that position have extended their arms almost to the bottom, which must have been above 10 inches.

Of the Structure and Diseases of Articulating Cartilages, by William Hunter, Surgeon.* N° 470, p. 514.

The fabric of the joints in the human body is a subject so much the more entertaining, as it must strike every one, that considers it attentively, with an

* Mr. Wm. Hunter, afterwards Dr. Hunter, so celebrated as a demonstrator of anatomy, author of the splendid plates of the gravid uterus, and founder of the great anatomical museum in Windmill-street.

He was a native of Scotland, and (as his biographer Dr. S. Foart Simmons informs us) after some years spent at the university of Glasgow, he was placed in the year 1737 in the family of Dr. Cullen, who at that time practised physic and surgery at Hamilton. With him Mr. H. continued nearly 3 idea of fine mechanical composition. Wherever the motion of one bone on another is requisite, there we find an excellent apparatus for rendering that motion safe and free: we see, for instance, the extremity of one bone moulded into an orbicular cavity, to receive the head of another, in order to afford it extensive play. Both are covered with a smooth elastic crust, to prevent mu-

years, after which he passed a winter at Edinburgh, and in 1741 he came to London; and was received first into the house of Dr. Smellie, a celebrated practitioner in midwifery, and afterwards into the family of Dr. James Douglas, whom Mr. H. not only assisted in his dissections, but had also committed to him the superintendance of his son's education.

Mr. H.'s first course of anatomical demonstrations was in 1746, but he had before delivered lectures on operations in surgery. Being afterwards elected man-midwife to the Middlesex and British Lying-in-Hospitals, he began to get into considerable midwifery practice. In 1750 he obtained the degree of M. D. from the university of Glasgow. After this period he rose into great repute, both by his lectures and his writings, and in 1768 he had the honour of being appointed physician extraordinary to the queen, having been consulted on occasion of her Majesty's pregnancy 2 years before. He had been elected F. R. S. in 1767. The same year he was appointed anatomical lecturer to the Royal Academy of Arts. In 1781 he succeeded Dr. Fothergill as president of the Society of Physicians in London, to whose publication, entitled Medical Observations and Inquiries, he contributed some valuable papers. About this period, too, he was elected a member of the Medical Society of Paris, and of the French Academy of Sciences. He died in the spring of 1783, in the 65th year of his age, displaying uncommon serenity and strength of mind as his dissolution drew near.

Dr. H. was author of some papers inserted in the Medical Observations and Inquiries before mentioned; of two other communications, besides the above inserted in the Phil. Trans. (vols. 58 and 61) and of a work entitled Medical Commentaries; but his great work is the Anatomy of the Human Gravid Uterus, illustrated by a series of plates, which combine the most accurate exposition of structure on the part of the anatomist, with the most exquisite workmanship on the part of the engravers. A description of the gravid uterus was found among the doctor's papers after his death, and was published some years since by his nephew and successor Dr. Baillie, to whom the medical world owes great obligations for his very valuable works on Morbid Anatomy.

Although the anatomical preparations constitute a principal part of the late Dr. Hunter's museum, yet it further contains a collection of shells, and other subjects of natural history, (once the property of Dr. Fothergill) besides a cabinet of medals, and a library stored with some of the best and scarcest editions of the ancient authors. A description of some of the coins has been published by Dr. Combe, the intimate friend of Dr. H., and a gentleman in whom a large share of classical learning, and a thorough knowledge of antiquities, are united with a great degree of medical skill.

Dr. Simmons informs us, that by his will Dr. H. directed that his nephew Dr. Baillie (or in case of his death Mr. Cruickshank) should have the use of his museum, under the direction of trustees, for the term of 30 years; after which period the whole collection is bequeathed to the university of Glasgow.

When Dr. H. first came to London his finances were so small that they were little more than sufficient to defray his travelling expences; but by great abilities and exertions as a lecturer and a practitioner, joined to a system of strict regularity and economy, he at length amassed a very ample fortune. There was something in his manner peculiarly interesting and impressive, and his language was highly correct and perspicuous. With these qualifications, added to a thorough knowledge of his subject, it is not to be wondered that he could render the most difficult parts of anatomy clear and familiar to his pupils. As a demonstrator he was certainly without an equal.

tual abrasion; connected with strong ligaments, to prevent dislocation; and inclosed in a bag that contains a proper fluid deposited there, for lubricating the two contiguous surfaces. So much in general.

But if curiosity lead us a step further, to examine the peculiarities of each articulation, we meet with a variety of composition calculated to all the varieties of motion requisite in the human body. Is the motion to be free and extensive in one place? there we find the whole apparatus contrived accordingly. Ought it to be more confined in another? here we find it happily limited. In short, as Nature's intentions are various, her workmanship is varied accordingly.

These are obvious reflections, and perhaps as old as the inspection of dead bodies. But modern anatomists have gone further: they have brought the articulations, as well as the other parts of the body, under a narrower inquiry, and entered into the minutest parts of their composition. The bones have been traced fibre after fibre; but the cartilages have not hitherto been sufficiently explained. After some fruitless attempts, by macerating and boiling the cartilages in different menstrua, Mr. H. fell upon the method not only of bringing their fibrous texture to view, but of tracing the direction and arrangement of those fibres.

Now, an articulating cartilage, is an elastic substance, uniformly compact, of a white colour, and somewhat diaphanous, having a smooth polished surface covered with a membrane; harder and more brittle than a ligament, softer and more pliable than a bone.

When an articulating cartilage is well prepared, it feels soft, yields to the touch, but restores itself to its former equality of surface when the pressure is taken off. This surface, when viewed through a glass, appears like a piece of velvet. If we endeavour to peel the cartilage off in lamellæ, we find it impracticable; but, if we use a certain degree of force, it separates from the bone in small parcels; and we never find the edge of the remaining part oblique, but always perpendicular to the subjacent surface of the bone. If we view this edge through a glass, it appears like the edge of velvet: a mass of short and nearly parallel fibres rising from the bone, and terminating at the external surface of the cartilage: and the bone itself is planned out into small circular dimples, where the little bundles of the cartilaginous fibres were fixed. Thus we may compare the texture of a cartilage to the pile of velvet, its fibres rising up from the bone, as the silky threads of that rise from the woven cloth or basis. In both substances the short threads sink and bend in waves on being compressed; but, by the power of elasticity, recover their perpendicular bearing, as soon as they are no longer subjected to a compressing force. If another comparison was necessary, we might instance the flower of any corymbiferous plant, where the flosculi and stamina represent the little bundles of cartilaginous fibres; and the calyx, on which they are planted, bears analogy to the bone.

Now these perpendicular fibres make the greatest part of the cartilaginous substance; but doubtless there are likewise transverse fibrils which connect them, and make the whole a solid body, though these last are not easily seen, because being very tender, they are destroyed in preparing the cartilage.

We are told by anatomists, that cartilages are covered with a membrane named perichondrium. If they mean the cartilages of the ribs, larynx, ear, &c. there, indeed, such a membrane is very conspicuous; but the perichondrium of the smooth articulating cartilages is so fine, and firmly braced on the surface, that there is room to doubt whether it has been often demonstrated, or rightly understood. This membrane, however, Mr. H. had raised in pretty large pieces after macerating; and found it to be a continuation of that fine, smooth membrane that lines the capsular ligament, folded over the end of the bone from where that ligament is inserted. On the neck of the bone, or between the insertion of the ligament, and border of the cartilage, it is very conspicuous, and may be pulled up with a pair of pincers; but where it covers the cartilage it coheres to it so closely, that it is not to be traced in the recent subject without great care and delicacy. In this particular it resembles that membrane which is common to the eye-lids and the fore-part of the eye-ball, and which is loosely connected with the albuginea, but strongly attached to the cornea.

From this description it is plain, that every joint is invested with a membrane, which forms a complete bag, and gives a covering to every thing within the articulation, in the same manner as the peritonæum invests not only the parietes, but the contents of the abdomen.

The blood-vessels are so small, that they do not admit the red globules of the blood; so that they remained in a great measure unknown, till the art of filling the vascular system with a liquid wax brought them to light. Nor even by this method are we able, in adult subjects, to demonstrate the vessels of the true cartilaginous substance; the fat, glands, and ligaments, shall be red with injected vessels, while not one coloured speck appears on the cartilage itself. In very young subjects, after a subtle injection, they are very obvious; and Mr. H. found their course to be as follows: all round the neck of the bone there are a great number of arteries and veins, which ramify into smaller branches, and communicate with one another by frequent anastomoses, like those of the mesentery. This might be called the circulus articuli vasculosus, the vascular border of the joint. The small branches divide into still smaller ones on the adjoining surface, in their progress towards the centre of the cartilage. We are very seldom able to trace them into its substance, because they terminate abruptly at the edge of the cartilage, like the vessels on the albuginea oculi when they come to the cornea. The larger vessels, which compose the vascular circle, plunge in by a great number of small holes, and disperse themselves into branches between the cartilage and bone. From these again there arises a crop of small short twigs, that shoot towards the outer surface; and whether they serve for nourishing only, or if they pour out a dewy fluid, Mr. H. does not pretend to determine. However that be, he observes that the distribution of the blood-vessels to the articulating cartilages is very peculiar, and seems calculated for obviating great inconveniencies. Had they run on the outer surface, the pressure and motion of the two cartilages must infallibly have occasioned frequent obstructions, inflammations, &c. which would soon have rendered our motions painful, and at last entirely deprived us of them. But by creeping round the cartilaginous brim, where there is little friction, or under the cartilage, where there is none, they are perfectly well defended from such accidents.

It were to be wished we could trace the nerves of cartilages: but, in relation to these organs, here, as in many other parts of the body, we are under a necessity, from the imperfection of our senses, of being satisfied with mere conjecture. And though, from the great insensibility of a cartilage, some have doubted of its being furnished with nerves; yet, as it is generally allowed that these are a sine qua non in the growth and nourishment of animals, we have no sufficient reason to deny their existence in this particular part. With regard to the manner of their distribution, we may presume, from analogy, that they follow the same course with the blood-vessels.

The articulating cartilages are most happily contrived to all purposes of motion in those parts. By their uniform surface, they move upon one another with ease: by their soft, smooth, and slippery surface, mutual abrasion is prevented: by their flexibility, the contiguous surfaces are constantly adapted to each other, and the friction diffused equally over the whole: by their elasticity, the violence of any shock, which may happen in running, jumping, &c. is broken and gradually spent: which must have been extremely pernicious, if the hard surfaces of bones had been immediately contiguous. As the course of the cartilaginous fibres appears calculated chiefly for this last advantage, to illustrate it, we need only reflect on the soft undulatory motion of coaches, which mechanics want to procure by springs; or on the difference between riding a chamber horse and a real one. To conclude, the insensibility of articulating cartilages is wisely contrived, as by this means the necessary motions of the body are performed without pain.

If we consult the standard chirurgical writers, from Hippocrates down to the present age, we shall find, that an ulcerated cartilage is universally allowed to be a very troublesome disease; that it admits of a cure with more difficulty than a carious bone; and that, when destroyed, it is never recovered. Hildanus, in considering these diseases, has observed, that when the cartilages of a joint were destroyed, the bones commonly threw out a cementing callus; and thus a bony anchylosis, or immoveable continuity, was formed where the moveable joint had been. So far as he had opportunities of examining diseased joints, either after death or amputation, he had found, according to the nature and stage of the disease, the cartilages in some parts redish and lax; or soft and spongy; or raised up in blisters from the bone; or quite eroded, and perhaps the extremities of the bones carious; or, lastly, a bony anchylosis formed. But he could never see, nor indeed hear of, the least appearance of an exfoliation from the surface of the cartilage. Now, if we compare the texture and morbid phenomena of those cartilages together, all the diseased appearances will admit of as rational a solution, as perhaps any other part of the vitiated economy.

It appears from maceration, that the transverse fibrils are extremely tender and dissoluble; and that the cohesion of the parts of the straight fibres is stronger than their cohesion with the bone. When a cartilage therefore is inflamed, and soaked in purulent matter, the transverse or connecting fibres will the soonest give way, and the cartilage becomes more or less red and soft, &c. If the disorder goes on a little longer, the cartilage does not throw off a slough, but separates from the bone, where the force of cohesion is least, and where the disease soon arrives, by reason of the thinness of the cartilage. When the bone is thus exposed, the matter of the ulcer, or motion of the joint, corrodes or abrades the bony fibres. If the constitution is good, these will shoot forth a callus; which either cements the opposite bones of the articulation, or fills up the cavity of the joint, and for the future prevents motion. But if, unfortunately, the patient labours under a bad habit of body, the malignancy, having got root in the bone, will daily gain ground, the caries will spread, and at last the unhappy person must submit to extirpation, a doubtful remedy, or wear out a painful, though probably a short life.

Explanation of the figure. Fig. 4, pl. 18, represents a view of the patella on the backside, where it is covered with a smooth cartilage. In this we may observe, AAAA, the surface of the cartilage, appearing, when the perichondrium is removed, like velvet. Near the middle, part of the cartilage is taken out, in order to show B, the subjacent surface of the bone; and C, the thickness of the cartilage, where the perpendicular fibres are seen very distinctly; D, the scabrous lower point of this bone, into which the ligament is inserted that binds it to the tibia.

Concerning some Worms whose Parts live after they have been cut asunder. By the Rev. Thomas Lord. N° 470, p. 522.

After having, without success, made several repeated searches for the polypus, in several fish-ponds, and a small stream, Mr. L. collected the different insects of various sorts he had there met with, and which were of more than 30 kinds, putting them all together; but some of them voraciously seized on others, and devoured them, so that in a day's time he had hardly any left, but a few of one sort, which rolled themselves up like millepedes, or hog-lice, but were, on the whole, more of the leech kind, and could extend themselves about an inch in length. These he cut asunder, but the pieces died in about 30 hours after the operation. He then recollected, that in the account published by Dr. Mortimer, Philos. Trans. N° 467, mention is made of a French gentleman, who had discovered water-worms, that would live after cutting: he searched for all he could find fastened either on rotten wood, leaves, straws, or stones, taken out from the bottom of the water, and cut of every sort asunder; but none lived above 48 hours, except one sort, in one glass were 4 pieces that seemed to be complete worms, and the same as the 2 in the other phial: these 4 pieces, 12 days before, were 2 worms: he cut them asunder with a penknife, and found that each part, from the first, continued vigorous and strong; and in 3 days the ends where the wounds were given, were grown sharper, and they moved along like the entire worms.

The other two entire worms were each cut presently after into 2 pieces, which soon after completed themselves, grew longer, and were several weeks after in a vigorous and thriving condition.

*The Natural History of the Rhinoceros. By Dr. Parsons.** N° 470, p. 523.

Albert Durer's figure of this creature has led several natural historians, who have written since his time, into errors; for such have always copied him; and indeed many have exceeded him in adorning their figures with scales, scallops,

* Dr. Js. Parsons was author of a Mechanical and Critical Inquiry into the nature of Hermaphrodites, 1741; of a Description of the Urinary Bladder, with figures, 1742; and of various papers, besides the Croonian Lectures, inserted in the Philos. Trans., which papers chiefly relate to natural history.

He was descended from a good family, and was born at Barnstaple in Devonshire in 1705. After receiving his medical education at Paris, he took his degree of M.D. at Rheims in 1736, and came to settle in London the same year. Here he was befriended by Dr. Jas. Douglas, and was elected F.R.S. in 1740, to which Society he was appointed assistant secretary for foreign correspondence in 1750. He died in 1770. Dr. Parsons was a man of an inquisitive and philosophical turn of mind; and although anatomy, physiology, and natural history, were his favourite pursuits, he nevertheless bestowed considerable attention on the study of antiquities.

and other fictitious forms. Now, from the badness of his figure, Dr. P. was induced to believe that that great man never saw the animal; for he certainly could not have been so mistaken in the performance. Many years after him, one Hendrik Hondius published in Holland an exact copy of Durer's print, counterfeiting the date and mark.

Bontius says he has often seen these animals in the woods and stables abroad; and values himself for having exhibited a figure without the decorations that Albert Durer put upon his; and yet, instead of the hoofs which are proper to the animal, he has drawn a paw not unlike that of a dog, only something bulky.

The figure given by Chardin, in his Voyages, has some truth, as to the folds or plicæ in the skin of the rhinoceros; and likewise as to the feet: but in other respects it is not like the animal. There is also a little truth in the figures of Camerarius, in his emblems taken from animals; but far from a thorough representation of the creature: and, in short, the other originals, as that taken from the rhinoceros in 1685, that published by Carwitham in 1739, and, to look back to the Roman times, those in the pavement of Præneste, and Domitian's medals, are very inaccurate, but have none of Albert Durer's decorations.

When the rhinoceros arrived here in 1739, Dr. Douglas went frequently to see him, for the purpose of correcting the opinions respecting him; and on June 24, of this year, exhibited before the Royal Society a drawing, with a collection of figures of that creature, taken from several authors, who had written of him before. He mentioned also his dimensions; and on the 28th of the same month, he produced a collection of horns, with some account of them, but proceeded no further. Therefore, as another occasion might not offer in many years, Dr. P. gives the following account of the male rhinoceros shown in Eagle-street near Red Lion-square, in 1739, and the drawings annexed to it. In this account, he had no regard to those of other authors, but barely described him as he often saw him both then and afterwards.

The drawings are a side, fore, and back view of the animal fore-shortened: the figures of 2 single horns; and a double one or two sticking to the same piece of skin; the penis; the tail of an old rhinoceros; and an upper and under view of one of the feet.

He was fed here with rice, sugar, and hay: of the first he ate 7 pounds mixed with 3 of sugar every day, divided into 3 meals; and about a truss of hay in a week, besides greens of different kinds, which were often brought to him, and of which he seemed fonder than of his dry victuals; and drank large quantities of water at a time, being then, it seems, 2 years old.

He appeared very peaceable in his temper, suffering himself to be handled in any part of his body; but outrageous when struck or hungry, and pacified in either case only by victuals. In his outrage he jumps about, and springs to an incredible height, driving his head against the walls of the place with great fury and quickness, notwithstanding his lumpish aspect: this Dr. P. saw several times, especially in a morning, before his rice and sugar were given him.

In height he did not exceed a young heifer, but was very broad and thick. His head, in proportion, is very large, having the hinder part, next his ears, extremely high, in proportion to the rest of his face, which is flat, and sinks down suddenly forward towards the middle, rising again to the horn, but in a less degree. The horn stands on the nose of the animal, as on a hill. The part of the bone on which the horn is fixed, rises into a blunt cone, to answer to a cavity in the basis of the horn, which is very hard and solid, having no manner of hollow nor core, like those of other quadrupeds. That of this animal, being young, does not rise from its rough base above an inch high, is black and smooth at the top, like those of the ox-kind, but rugged downwards; the determination of its growth is backwards, instead of straight up; which is apparent, as well in the different horns of old rhinoceroses, as in this of our present subject; for the distance from the base to the apex of this, backward, is not within a third part so long as that before, and it has a curved direction; and, considering the proportion of this animal's size to its horn, we may justly imagine, that the creature which bore any one of those great ones, must have been a stupendous animal in size and strength; and, indeed, it were no wonder, if such were untractable at any rate.

The sides of his under jaw are wide asunder, slanting outward to the lower edge; and backward to the neck, the edges turn outward; from this structure his head naturally looks large. The part that reaches from the fore part of the horn towards the upper lip, may be called the nose, being very bulky, and having a kind of circular sweep downward towards the nostrils: on all this part he has a great number of rugæ running across the front of it, and advancing on each side towards his eyes. The nostrils are situated very low, in the same direction with the rictus oris, and not above an inch from it. If we look at him in a fore view, the whole nose, from the top of the horn to the bottom of his lower lip, seems shaped like a bell, viz. small and narrow at top, with a broad base. His under lip is like that of an ox, but the upper more like that of a horse; using it, as that creature does, to gather the hay from the rack, or grass from the ground; with this difference, that the rhinoceros has a power of stretching it out above 6 inches, to a point, and doubling it round a stick, or one's finger, holding it fast; so that, as to that action, it is not unlike the proboscis of an elephant.

As to the tongue of the rhinoceros, though it be confidently reported by authors, that it is so rough as to be capable of rubbing a man's flesh from his bones; yet that of our present animal is soft, and as smooth as that of a calf; which Dr. P. often felt, having had his hand sucked several times by him. Whether it may grow more rough, as the beast grows older, we cannot say. His eyes are dull and sleepy, much like those of a hog in shape, and situated nearer the nose than that of any quadruped ever seen; which he very seldom opens entirely. His ears are broad and thin towards the tops, much like those of a hog; but have each a narrow round root with some rugæ about it; and rises, as it were, out of a sinus surrounded with a plica. His neck is very short, being that part which lies between the back edge of the jaw and the plica of the shoulder; on this part there are two distinct folds, which go quite round it, only the fore one is broken underneath, and has a hollow flap hanging from it, so deep that it would contain a man's fist shut, the concave side being forward. From the middle of the hinder one of these folds or plicæ, arises another, which, passing backwards along the neck, is lost before it reaches that which surrounds the fore part of the body. His shoulders are very thick and heavy, and have each another fold downward, that crosses the fore leg; and, almost meeting that of the fore part of the body, just mentioned, they both double under the belly close behind the fore leg.

His body, in general, is very thick, and juts out at the sides, like that of a cow with calf. He has a hollow in his back, which is mostly forward, but backwards, the line or ridge rises much higher than that of the withers; and, forming the plica on the loins, falls down suddenly to the tail, making an uneven line. His belly hangs low, being not far from the ground, as it sinks much in the middle. From the highest point in his back, the plica of the loins runs down on each side between the last ribs and the hip, and is lost before it comes to the belly; but, above the place of its being lost, another arises, and runs backward round the hind legs, a little above the joint; this he calls the crural fold, which turns up behind till it meets another transverse one, which runs from the side of the tail forward, and is lost before it reaches within 2 inches of that of the loins. The legs are thick and strong; those before, when he stands firm, bend back at the knee, a great way from a straight line, being very round, and somewhat taper downwards. The hinder legs are also very strong, bending backwards at the joint to a blunt angle, under which the limb grows smaller, and then becomes gradually thicker, as it approaches the foot; so also does that part of the fore leg. About the joint of each of his legs, there is a remarkable plica when he bends them in lying down, which disappears when he stands.

In some quadrupeds, the fetlock bends or yields to the weight of the animal; but in this there is no appearance of any such bending, and he seems to stand on stumps, especially if he is viewed behind. He has three hoofs on each foot forwards; but the back part is a large mass of flesh, rough like the rest of his skin, and bears on the sole or bottom of his foot. This part is plump and callous in the surface, yielding to pressure from the softness of the subjacent flesh. Its shape is like that of a heart, having a blunt apex before, and running backward in a broad basis. The outline of the bottoms of the hoofs are somewhat semicircular.

The tail of this animal is very inconsiderable, in proportion to his bulk, not exceeding 17 or 18 inches in length, and not very thick; it has a great roughness round it, and a kind of twist or stricture towards the extremity, ending in a fatness, which gave occasion to authors to compare it to a spatula. On the sides of this flat part, a few hairs appear, which are black and strong, but short. There is no other hair on any part of this young rhinoceros, except a very small quantity, on the posterior edge of the upper parts of the ears. There is a very particular quality in this creature, of listening to any noise or rumour in the streets; for though he were eating, sleeping, or under the greatest engagements nature imposes on him, he stops every thing suddenly, and lifts up his head, with great attention, till the noise is over.

The penis of the rhinoceros is of an extraordinary shape. There is first a theca, or præputium, arising from the inguinal part of the belly, nearly like that of a horse, which conceals, as that does, the body and glans, when retracted. As soon as the animal begins to extend it, the first thing that is extruded, the theca, is a second sheath of a light flesh-colour, and pretty much in form like the flower of the digitalis floribus purpureis; and then out of this another hollow tube, which is analogous to the glans penis of other creatures, very like the flower of the aristolochia floribus purpureis, but of a lighter or fainter flesh-colour than the former. His keeper, who was a native of Bengal, would make him thus emit his penis when he pleased, while he lay on the ground, by rubbing his back and sides with straw; and, in its utmost state of erection, it never was extended to more than about 8 or 9 inches. Its termination is backward in a curved direction, so that he is a retromingent animal, and consequently, retrogenerative. I have several times seen him staling: he turns his tail to the wall, and, extending his hind legs asunder, crumps himself up, and pushes his urine out in a full stream as far as a cow.

Of the female rhinoceros that came over afterwards, it is unnecessary to say more than that she was exactly like this in all respects, except the sex; and, by the horn, and size, of the same age; and the pudenda like those of a cow.

The skin of the rhinoceros is thick and impenetrable; in running one's fingers under one of the folds, and holding it with the thumb at the top, it feels like a piece of board $\frac{1}{4}$ inch thick. Dr. Grew describes a piece of one of these skins tanned, which, he says, "is wonderful hard, and of that thickness, exceeding that of any other land animal he has seen." It is covered all over more or less with hard incrustations like so many scabs; which are but small on the ridge of the neck and back, but grow larger by degrees downwards toward the belly, and are largest on the shoulders and buttocks, and continue pretty large on the legs all along down; but, between the folds, the skin is as smooth and soft as silk, and easily penetrated; of a pale flesh-colour, which does not appear to view in the folds, except when the rhinoceros extends them, but is always in view under the fore and hinder parts of the belly, but the middle is incrusted over like the rest of the skin. To call these scabbed roughnesses scales, as some have done, is to raise an idea in us of something regular; which in many authors is a great inaccuracy, and leads the reader into errors; for there is nothing formal in any of them.

As to the performance of this animal's several motions, let us consider the great wisdom of the Creator, in the contrivance that serves him for that purpose. The skin is entirely impenetrable and inflexible; if therefore it was continued all over the creature, as the skins of other animals, without any folds, he could not bend any way, and consequently not perform any necessary action; but that suppleness in the skins of all other quadrupeds, which renders them flexible in all parts, is very well compensated in this animal by those folds; for, since it was necessary his skin should be hard for his defence, it was an excellent contrivance, that the skin should be so soft and smooth underneath, that, when he bends himself any way, one part of this board-like skin should slip or shove over the other; and that these several folds should be placed in such places of his body, as might facilitate the performance of every voluntary motion he might be disposed to.

As the great number of horns that are to be found in the museums of the curious, brought from time to time from the East Indies, are single; we may venture to assert, that all those of Asia have really but one horn on the nose; and this is confirmed by many gentlemen, who had seen those creatures in Persia, and other parts of the east. Hence it is easy to conclude, that this was the reason the single horn was imagined the standard of nature for that animal, and that therefore Martial ought rather to have said, that two bears, or, according to Bochart, two wild bulls, were tossed by the strong horn of the rhinoceros, than that a single bear was thrown up by his double horn.

We do not want sufficient proofs to show, that there is a species of those animals in Africa, having 2 horns on the nose. Peter Kolbé, a Dutchman, in his voyage to the Cape of Good Hope, says, there is one on the summit of the nose, like the others, but having a smaller close behind it. There are also 2 horns in Sir Hans Sloane's Museum, sticking to the same individual integuments, not much more than an inch from each other; which is an undeniable proof of the existence of this species. And, in fine, the brass medal of Domitian has, on one side, the figure of a rhinoceros with 2 horns on the nose, very plain. From all which I cannot but be inclined to believe, that this medal was struck from one of those of Africa; and that Martial had no more notion of a rhinoceros with one horn, than Bochart had of one with two.

Augustini also, in his Dialogue of Medals, has a figure of the rhinoceros, with 2 horns on the nose. So has also the figure in the Prænestan pavement, made by order of Sylla the dictator, on which he certainly designed to represent several animals, and other remarkable things, proper to Africa.

Explanation of the Plates of the Rhinoceros.—Plate 18, fig. 5, is a side view of the rhinoceros; fig. 6, a fore view of the rhinoceros, fore-shortened; fig. 7, a back view of the same, fore-shortened.

Pl. 19, fig. 1, two views of one of the feet; a, the upper part of the foot; b, the sole of the foot. 2, The tail of an old rhinoceros, in the museum of the Royal Society. 3, The penis in an erected state; a, the first theca or præputium, of a dark colour; b, the second theca, being flesh-coloured; c the tubular glans penis. 4, A horn of a rhinoceros, said to be 6 years old, being about 10 inches long. 5, The bottom or concave basis of the same, to show the cavity is very superficial. 6, A beautiful horn in Dr. Mead's museum, being about 37 inches long. 7, The horn of a rhinoceros, in the museum of Sir Hans Sloane, which (as those of oxen are sometimes liable to distortions in their growth) differs from the common form; it is 32 inches long. 8, The double horn mentioned above, belonging to Sir Hans Sloane; whether they crossed each other on the animal, is uncertain; it is most likely they did not, but that by drying they were crossed by the corrugation of the skin that joins them together. The straight horn is 25 inches long, the curved one somewhat shorter, and the two diameters of the bases 13 inches. 9, The concave bottoms of the above double horns, as they adhere to the same piece of skin.

An Account of a Comparison lately made by some Gentlemen of the Royal Society, of the Standard of a Yard, and the several Weights lately made for their use; with the original Standards of Measures and Weights in the Exchequer, and some others kept for public use, at Guildhall, Founders'-hall, the Tower, &c. By the Committee of the Royal Society. N° 470, p. 541.

When there were some time since prepared, by order of the Royal Society, to be kept in their archives here, and also in those of the Royal Academy of Sciences at Paris, standards of the yard measure, as also of the Troy and Avoirdupois weights; an account of which was published in these Transactions, N° 465; it was not the intention of the Society to determine what was the absolute legal length of the yard, or the real and legal weight of the said several pounds; but only to preserve, in those respective places, 2 measures, and 2 sets of those weights, sufficiently near to what were in common use, and agreeing with each other, for the purpose of comparing together, by a certain standard, to which recourse might be had in either kingdom, the success of such experiments made, either in England or in France, in which measure or weight might particularly be concerned.

The gentlemen also of the Royal Academy of Sciences, took care to have the length of their half toise set off on both the brass rods, on which the English yard had been already laid off, and provided 2 brass weights of 2 French marcs each; one of which was sent over hither, when one of the brass rods, just mentioned, was again returned over to the Society. And it was the proportion only between these several standards, that was proposed to be laid down in the said paper published in these Transactions; without intending to ascertain the just and legal proportions between the weights and measures of both nations. Though it is not to be doubted, but that this measure of the French half-toise, and the French 2 marc weight, are, like the English, sufficiently agreeable to what are there constantly used.

But as some gentlemen have since wished to know, how far those standards agreed with what are esteemed the original ones, in the Chamberlain's Office of his Majesty's Exchequer, as well as with those kept for public use, at Guildhall, at Founders'-hall, with the Watchmakers' company, and in the Tower of London, Mr. George Graham, F. R. S. was requested to take upon him the comparison of the said several standards; which he has accordingly done, and carefully viewed and examined the same at the Exchequer, in the presence of the president of the Society, the Earl of Macclesfield, Lord Charles Cavendish, John Hadley, Esq. William Jones, Esq. Peter Daval, Esq. and Cromwell Mortimer, M. D. and at Guildhall, Founders'-hall, and the Tower, in the presence of all the same persons, Mr. Daval only excepted, who happened to be otherwise engaged that day.

And as the council of the Society have now thought fit to direct an account to be here published of these trials and experiments: we shall first begin with the measure of the yard; and then proceed to what concerns the several weights of the Troy and Avoirdupois pounds.

The standards of length now used in the Exchequer, are 2 squared rods of brass, of the breadth and thickness of about half an inch; the one called the yard, and the other the ell. The ends of neither are exactly flat and parallel, or, if they were so once, they have since suffered some bruise or damage, and that possibly by the impressing near each end the seal of a crowned E; by which it appears, they were placed here during the reign of Queen Elizabeth, and probably at the same time when the several standard weights, hereafter mentioned, were lodged here also.

To these rods there belongs a substantial brass bar, of about the length of 49 inches, the breadth of an inch and a half, and the thickness of an inch: on one edge of this bar is a hollow bed or matrix, fitted to receive the square rod of a yard: and on another, a like bed fitted to receive that of an ell; and into these beds they usually fit the yard and ell measures brought to be examined and sealed at this office. The square yard and ell rods fit sufficiently well into these respective beds, so as neither to rub or shake very sensibly; yet as neither the ends of the rods, or of the hollow beds, are accurately flat and parallel, the greatest lengths of those beds must, of necessity, be somewhat greater than the greatest lengths of the rods intended to be placed in them: by which greatest lengths of those rods, and which were considered by all the gentlemen present, as the real and proper lengths of those rods, are meant the distances of 2 parallel planes or cheeks, so placed as to touch the rods respectively at both ends.

Besides all which, there also remains in this office an old 8-sided rod of brass, of the thickness of about half an inch, very coarsely made, and as rudely divided, into 3 feet, and one of those feet into 12 inches. This is marked near each end with an old English 𝔥 crowned; and is supposed to have been the old standard of a yard, lodged there in the time of King Henry the 7th, and used as such, till the other above-mentioned, and now accounted the standard, was made to supply its place.

Now, as the yard is from very old time mentioned in our acts of parliament, as containing 3 feet, or 36 inches; and the ell is not therein particularly described, though universally reputed equal to one yard and a quarter, or to 45 inches; we shall in the following comparison suppose, that the length of the square brass yard rod, here kept, and marked with a crowned E, by that length meaning, as above, its greatest length between two parallel planes, to be the true and genuine length of the English yard, or of 3 English feet; and with that length we shall compare the others here mentioned, expressing how much they respectively exceed, or fall short of, this supposed standard measure.

To examine all which, Mr. Graham was provided with very exact and curious beam-compasses of different sorts, and adapted to the several purposes they were to be used for. One of these was by parallel cheeks intended for taking the lengths of the standard rods above-mentioned to be kept in the Exchequer, another was by rounded ends, one of which was moveable, designed to take the lengths of such standards to consist of hollow beds or matrices, like those already spoken of at the Exchequer, and the others, to be presently mentioned, at Guildhall; and a third beam-compass was fitted in the common way, with fine points, for taking off, or laying down, such measures as are marked out by the distance of points or lines, on any plane flat superficies. All which compasses were severally so contrived, as to be lengthened by the turning of a fine screw, one of whose revolutions answered accurately to the 40th part of an inch, and to which there was applied an index, showing, on a small circular plate with 20 divisions, the fractional part of a revolution; and on which the place of the index might, by the eye, be estimated to about the 10th part of a division; by which the motion of the moveable cheek, end, or point, might consequently be judged of, to about the 8000th part of an inch.

But Mr. Graham, when he determined by these instruments, the following particulars, desired it might be observed, that though the alterations of the compasses were sensible to so small a quantity; it was not to be supposed that the measures here taken with them, could be estimated to the same exactness. The hand cannot judge with so much nicety, of the shake of a rod, when applied between the cheeks, or when let into one of the hollow beds or matrices above-mentioned; neither can the eye, though assisted with a magnifying-glass, pretend to see, with that accuracy, the place of the compass-points, when applied to the taking off a measure, set out by points or lines, on the plane surface of a rod or rule. All he therefore thinks possible, and that he has found he could several times together, under the same or like circumstances, be consistent in, is to take such measures to about the 1600th part of an inch.

We shall however, in what follows, give those measures as they actually did come out, in revolutions, divisions, and 10ths; all which are also, for the convenience of the reader, in a 2d column, reduced to the common decimals of an inch; and, in a 3d, to the vulgar fractions of the same.

It may further be noted, that the absolute quantity of all measures, anywise inscribed on standards of metal, must, from the nature of things, vary with the alterations in the heat or coldness of the weather; and, for that reason, the exact proportion between any two standards, taken at different times, cannot be expected to be found the same to the most perfect degree of exactness, unless the temperature of the air shall at those different times have been the same, or that a proper allowance has been made for the alteration of it. Yet, in the present case, as all the several measures referred to, are inscribed on the same metal, brass, as none of the differences we are concerned about are very great, and as the change of the weather was not very considerable between the days of trial; it has been thought this last consideration might safely be neglected, in setting down the following particulars. Which are, that

The greatest length of the matrix of the yard measure, at the Exchequer, exceeded the square standard yard by 0 rev. 8.2 div. = .0102 = $\frac{1}{97.56}$.—The yard inscribed on the Royal Society's rod, exceeded the same by 0 rev. 6.0 div. = .0075 = $\frac{1}{133.3}$.—The old brass standard at the Exchequer marked with the crowned 𝔥, fell short of the same by 0 rev. 5.7 div. = .0071 = $\frac{1}{140.3}$.—The standard ell rod, at the Exchequer, exceeded 45 inches, of such as the standard yard contains 36, by 1 rev. 19.5 div. = .0494 = $\frac{1}{20.25}$.

At Guildhall, the standards of long measure there used, are only two beds, or matrices, the one of a yard, and the other of an ell, cut out of two of the edges of a substantial brass bar, much like that at the Exchequer, but not altogether so thick; which bar is sealed with the Exchequer seal, and marked at both ends with C. R. crowned; and also, as it seems, with W. M. crowned in like manner. But there are here no rods fitted to these beds; so that all that seemed requisite and proper to be done, was to take both the greatest lengths of these beds, and also the least lengths of the same; the last being nearly the lengths of such square rods as might be so fitted into the beds, as to go in every way close, and without sensibly shaking; and, on taking the said measures, it appeared, that

The greatest length of the yard bed, at Guildhall, exceeded the standard yard, at the Exchequer, by 1 rev. 14.7 div. = .0434 = $\frac{1}{23.04}$.—The least length of the same bed, exceeded the said standard of a yard by 1 rev. 11.7 div. = .0396 = $\frac{1}{25.2}$.—The greatest length of the ell bed, at Guildhall, exceeded 45 Exchequer standard inches by 1 rev. 15.5 div. = .0444 = $\frac{1}{22.5}$.—The least length of the same bed exceeded the same number of like inches by 1 rev. 0.7 div. = .0258 = $\frac{1}{38.6}$.

The standard of a yard, in the Tower of London, belongs to the Office of Ordnance, and is kept in the drawing-room there; it is a solid brass rod, about $\frac{7}{10}$ of an inch square, and about 41 inches long; on one side of which is laid off the measure of a yard, divided into 3 feet, and each foot into 12 inches; the first foot has the inches divided into 10ths, the second into 12ths, and the third into 8ths of an inch, and the first inch of all is divided into 100 parts, by diagonal lines. This rod is said to have been provided by the late Mr. Rowley; it is sealed with the Exchequer seal, and two other seals of G. R. crowned, near one of the ends, together with his Majesty's mark commonly called the broad arrow. And the length of the yard, or of the 3 feet inscribed on it, exceed the forementioned Exchequer standard of a yard, by 0 rev. 8.9 div. = .0111 = $\frac{1}{90}$.

The standard yard, belonging to the Clockmaker's Company, was delivered to them from the Exchequer, by indenture, the 4th of September, 23 Car. II. A. D. 1671. It is a brass rod of eight sides, near half an inch in thickness, sealed with the Exchequer Seal, and C. R. crowned, near each end; and on which the length of the yard is expressed, by the distance between two upright pins, or small cheeks, filed away to their due quantity: this was procured by Mr. Graham, to be brought to the president's house of the Royal Society, on Saturday the 7th of May last, where all the above-named Company then met, to collate their respective notes of these several trials, all which were found to agree with each other: at which last meeting, Mr. John Machin, of Gresham College, the other Secretary of the Society, was present also: and the length of this last yard measure was then tried, and found to fall short of the Exchequer standard yard measure, now very carefully added on the middle line of the Royal Society's brass rod, 0 rev. 16.8 div. = .021 = $\frac{1}{47.62}$.

Now, as to the weights, those in the Chamberlain's Office in his Majesty's Exchequer, and which are esteemed the standards, are a pile, or box, of hollow brass Troy weights, from 256 ounces downwards, to the 16th part of one ounce, all severally marked with a crowned E: but they have no penny-weights, nor grain weights, that are any ways considered as standards.

The weight mentioned in all our old acts of parliament, from the time of King Edward the First, is universally allowed to be the Troy weight, its pound consisting of 12 ounces, each containing 20 penny-weights: and as the pound is the weight of the largest single denomination commonly mentioned in those acts, 12 ounces taken from the pile of Troy weights abovementioned, as there is no single Troy pound weight, must now be reputed the true standard of the Troy pound, used at this day in England.

Besides which Troy standards, there are also kept in the Exchequer the following standards for Avoirdupois weights: that is to say, a 14 pound bell weight of brass, marked with a crowned E, and inscribed

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ELIZABETH. REGINA.

1582.

As also a 7 pound, a 4 pound, a 2 pound, and a single pound, like Avoirdupois bell-weights, and severally marked as follows, excepting the variations for the number of pounds in each respective weight.

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With which are also kept a pile of flat Avoirdupois weights, from 14 pounds down to the 64th part of a pound.

When the Avoirdupois weight came first to be esteemed a lawful weight, does not appear; but by these standards it is plain it has been used as such ever since the reign of Queen Elizabeth. And as the weight of 15 pounds Avoirdupois has before been made use of, in determining the proportion between the weight of this pound and that of the pound Troy, we shall begin by giving the counterpoise of the said 15 pound Avoirdupois, as it was found in Troy weight: from whence we shall deduce the proportions of those pounds, and afterwards compare the same with the like proportions, deduced from the 7 pounds, and single pound bell-weights, and the single pound flat weight abovementioned: all which weights were taken in the presence of the above-named noblemen and gentlemen, by Mr. Samuel Read, scale and weight-maker near Aldersgate, who brought to the Exchequer a large balance of his own for that purpose, and which, when loaded with 15 pounds at each end, was very readily turned with 6 grains; as a less one, he brought also for examining the single pound weights, was with half a grain. He also brought with him what he called his own standard penny and grain weights, to supply what was necessary to make the counterpoise of the Exchequer weights: with all which the result was, that

The standard 14 pound, and single pound Avoirdupois weights, taken together, were, on a medium of 4 trials, alter counterchanging the weights in each basin, changing the basins, and then again counterchanging the weights, found to be counterpoised by 218 Troy ounces, 13 penny-weight, 23 grains and $\frac{1}{4}$th. from whence the Avoirdupois pound is deduced equal to 6998.35 of such grains as the Troy ounce is reputed to contain 480 of; and the Avoirdupois ounce, of which 16 are supposed to make a pound, is found equal to 437.4 like grains.

Again: the 7 pound bell Avoirdupois weight, with the same scales, and on a medium of 4 like experiments, counterchanging, as before, both weights and basins, was found to be counterpoised by 102 Troy ounces 1 penny-weight, and 21 grains. According to which, the Avoirdupois pound comes out equal to 7000.7, and the ounce to 437.54 Troy grains.

Again: the single bell Avoirdupois pound, with the less scales, on the medium of two experiments, counterchanging the weights, the basins not being moveable, was found to weigh 14 Troy ounces 11 penny-weight and 18 grains; or was equal in weight to 7002, and the ounce to 437.62 Troy grains.

The single Avoirdupois bell pound, against the flat Avoirdupois pound weight, was found, on a medium of two like experiments, to be heavier by two Troy grains and a half: whence the flat Avoirdupois single pound weight weighs only 6999.5, and the ounce 437.46 Troy grains.

The Royal Society's Avoirdupois pound was, in like manner, found to be lighter than the Exchequer single bell pound weight, by one grain.

And their Troy pound weight to be lighter than the 8 and 4 ounce Troy weights at the Exchequer, taken together, by half a grain.

The Founders' Company of London are, by their charter from King James the First, authorized and directed to have the sizing and marking of all manner of brass weights, to be made or wrought, or to be uttered, or kept for sale, within the city of London, or 3 miles from the same. And the weights delivered to them from his Majesty's Exchequer, and now kept in their hall, as their standards for the uses abovementioned, are a pile of flat brass Troy weights, from 256 ounces, down to the 16th part of an ounce, all sealed with the Exchequer seal, and marked with C. R. crowned 1684, and a stamp of the initial letters of the maker's name: as also a set of bell brass Avoirdupois weights, sealed and marked in like manner. And here the following trials were made, before the abovenamed gentlemen, by Mr. Read, but with a large balance, commonly used for trials at the hall, in their office for that purpose; and found to turn with about the same weight as the former; and also with a less one, turning in like manner under these circumstances, with about half a grain, which balance belonged likewise to the hall, as did also the penny and grain weights made use of, but which were not kept by them as standard weights.

And here it was found on a medium of 4 trials, made in like manner as before, at the Exchequer, that 15 pounds Avoirdupois, being their 14 pounds, and single pound standard weights, were counterpoised by 218 Troy ounces, 15 penny-weight and 23 grains: whence the Avoirdupois pound is deduced equal to 7001.53, and the ounce to 437.59 Troy grains.

Again: the single Avoirdupois standard pound weighed, on a medium of 2 experiments, counterchanging the weights, as before, 14 Troy ounces, 11 penny-weight, 16½ grains: or was equal to 7000.5, and the ounce to 437.53 Troy grains.

Again: this standard Avoirdupois pound, at a medium as before, outweighed the Royal Society's Avoirdupois pound, by 2 grains and ¼th: and the Troy standards of 8 and 4 ounces, taken together, outweighed the Royal Society's single Troy pound weight, by 2 grains and $\frac{1}{8}$th, at a like medium.

At the Mint in the Tower of London, their standard weights are only a pile of Troy hollow weights, from 256 ounces, down to the 16th part of one ounce, without any penny or grain weights. The larger of these weights, as low as the 8 ounce weight, are marked with A. R. crowned, and inscribed PRIMO MAII, A° DNI. 1707, A° REGNI VI°. The 4 and the 2 ounce weights are only marked A. R. crowned, without the date; and the less have only the Exchequer seal, and the rose and crown, being the mark of his Majesty's mint, as all the larger ones have also. And here it was found by Mr. Joseph Harris, one of the assay-masters of the mint, with a very curious balance of his own, fixed in a glass lantern, and which he was well assured might in such circumstances be depended on to less than half a grain; and with the addition of so many penny and grain weights belonging to his office as were necessary: that

The Royal Society's whole Troy pound weight weighed, at a medium, less than the 8 ounces and 4 ounces of these standards, taken together, by 2 grains and $\frac{3}{8}$ths.

That the Royal Society's Avoirdupois pound weighed in Troy weight, by these standards, 14 ounces 11 penny weight 16 grains and $\frac{7}{8}$ths; or 7000.87 grains.

That the Royal Society's pile of 16 ounces Troy, was lighter than 16 ounces of these standard weights, by 4 grains and $\frac{3}{4}$ths.

And lastly, That the Royal Society's 8 ounces and 4 ounces together, taken from their pile, weighed lighter than their single Troy pound weight, by $\frac{5}{8}$ths of a grain.

An Instrument for Reducing a Dislocated Shoulder; invented by Mr. John Freke, Surgeon of St. Bartholomew's Hospital, and F. R. S. N° 470, p. 556.

Mr. F.'s object in the present communication, is to show within how small a compass the whole power which can be made use of, in reducing a dislocated shoulder, may be contracted. If a machine for this purpose be not portable, it matters but little to an afflicted patient 10 miles off, how good an instrument is out of his reach.

This machine, fig. 2, pl. 20, which consists of 2 boxes A, joined at the ends by 2 hinges, contains, when folded together, every thing that can possibly be wanted in the operation beforementioned; and it may so easily be made use of, without the assistance of any other operator than the surgeon employed, that he ventures to affirm, a patient may be set down, the instrument applied, and the shoulder reduced, in 1 minute, ordinarily speaking.

The length of this instrument, when shut up, is 1 foot 8 inches, its breadth 9 inches and thickness 3 inches and a quarter. When it is opened, it is kept so by 2 hooks fixed on the backside of it; and when one end of it stands on the ground, the other stands high enough to become a fulcrum, or support of a lever BB, which is fixed on a roller b, by a large wood screw, which turning sideways as well as with the roller, it obtains a circumrotatory motion; so that it will serve to reduce a luxation either backward, forward, or downward.

The roller on which the lever is fixed, is just the diameter of the depth of one of the boxes, into which are driven 2 iron pins, the ends of which are received by the 2 sides of the box, which are 1 inch thick.

The lever is 2 feet 4 inches, and is cut off and joined again by 2 hinges c, to fold up so as to be contained in the boxes. On the backside of it is a hook, to keep it straight; the other end is to hang over the roller about 1½ inch, which is to be excavated and covered with buff leather, for the more easy reception of the head of the os humeri.

Two iron cheeks DD are screwed on each side of the lever, to receive through them an iron roller E, which has 2 holes through it, to receive 2 cords coming from a brace F, fixed on the lower head of the os humeri; for on no other part of the arm above the cubit can a bandage for this purpose be useful; for, if the surgeon applies it on the muscular part of the arm, it never fails slipping down to the joint, before you can extend the limb.

The iron roller has a square end, on which is fixed a wheel G, within the cheek, notched round, which works as a rotchet on a spring ketch under the lever, by which it is stopped, as you wind it with a winch; and may at pleasure be let loose, as there shall be occasion for it, by discharging the ketch.

He next describes the brace F, which, compared with common bandages, is of more consequence than can easily be imagined by unexperienced persons. It consists of a large piece of buff leather, sufficient to embrace the arm, sewed on 2 pieces of strong iron curved plates, riveted together, one of them having an eye at each end, to fasten 2 cords in; the other is bent at the ends into 2 hooks, which are to receive the cords, after they have crossed over the arm above.

In order to keep the patient steady in his chair from coming forward, or letting the scapula rise up, on depressing the lever, after the limb is drawn forward by the winch, there must be fixed over the shoulder a girth with 2 hooks at the ends of it, long enough to reach to the ground on the other side, where it must be hooked into a ring I, to be screwed into the floor, for that purpose.

Concerning a Person who made Bloody Urine in the Small-Pox, and recovered. By Pierce Dodd, M. D. N° 470, p. 559.

Making bloody water, Dr. D. observes, is universally esteemed as terrible a symptom as any that can happen in the small-pox; and all who have written concerning that distemper, unanimously agree that it is a certain forerunner of approaching death. Dr. Cade indeed says, in his letters to Dr. Freind, concerning purging in that distemper, that he has sometimes cured this symptom, by the help of camphire, and a copious quantity of acids; but then he adds, that this relief was only temporary; and that, to confess the truth, he never knew any body, that made that sort of urine, who ever survived the 16th day from the eruption: and there is nobody he knows that has been conversant with this distemper, but has constantly experienced, sooner or later, the like fatality in consequence of it. He means when this sort of urine has proceeded from a broken crasis and contexture, or, as it were, a thorough dissolution of the whole mass of blood: for he knows very well, that we now and then have streaks, and sometimes larger quantities of blood in the urine, from the acrimony of the Spanish flies, on the application of blisters, which are frequently used, and so frequently likewise absolutely necessary, in one or other of the stages of this distemper, and yet the patient does well. And Dr. Browne, physician in St. Bartholomew's Hospital, gives an account of a gentlewoman, who lived in Dean's-yard, Westminster, who made bloody urine in the small-pox, 4 or 5 days together; which made Dr. Needham, who attended her, to forsake her; and yet she recovered: but they found afterwards, that this bloody water was not occasioned by the malignancy of the distemper, but by a sharp stone, which was at that time descending from one of the kidneys through the ureters into the bladder, and which she afterwards voided.

The case here given is that of a young man, about 15 years of age, son to a gentleman of a very considerable fortune in Jamaica. He was taken with a fever, and great pain in his head, April 20th last, and had the small-pox come out upon him the day following, notwithstanding which the same symptoms still continued, and nothing almost would stay on his stomach, and his head likewise was very delirious: he was obliged therefore to be blooded, and to take a vomit, and to have blisters applied to his neck and his arms; and had testaceous and nitrous medicines given him.

The next day every thing was more quiet, and so again the 3d day from the eruption; but the small-pox were very numerous all over him, and of a small, rank, angry sort; as they generally are, he thinks on the West India constitutions: but this young gentleman had besides over-heated himself a little before, by performing a part at the Montem, near Eton, where he was a scholar.

Things continued in much the same state the 4th day, but towards the evening there were a few streaks of blood mixed with, and subsiding in his water; which did not much alarm Dr. D. because he did not know but it might be caused by the blisters: he had but one reason to doubt the contrary, and that was, he had had little or no strangury: but as certain persons do aver, there is sometimes such, or even a more bloody sort of waters, occasioned by the flies, even where there shall be no strangury at all, he was willing to hope the best, and so made no other alteration in his process, than to direct a very free use of sp. of vitriol.

What was ordered, happened to succeed: we had no more of that sort of water, either that night, or the next day, or the morning following: but he was sent for in a great hurry that day, viz. the 6th, in the afternoon, and found his friends in the most terrible consternation; not only because it returned, but began to increase upon them, and was pouring off in a free manner. It was necessary therefore to proceed in another method, and he accordingly ordered some gum arabic, olibanum, and pulvis amyli, and alum, together with a mixture of black cherry-water and small cinnamon, and treacle-water, with some tinctura antiphthisica and terra japonica in it, and the tincture of roses, strongly acidulated and sweetened with diacodium; on the use of which it began to abate, and the next day the urine returned to its usual state and colour. There was nothing further observable in the course of this case, except that the distemper was of the coherent kind, and accordingly attended with many other dubious symptoms likewise: for though it is generally thought, that the coherent sort is not so formidable as the confluent; yet, as Dr. Freind has judiciously observed, and Moreton before him, there is not so much difference between them, but they are almost always attended with much the same appearances, and the same fevers plainly at the time of maturation: for that the danger does not arise so much from the sort, as from the number of the pustules; which if it be great, there is the like reason to be fearful of the event, whether they flux, or whether they only cohere: all which notwithstanding, this young gentleman had the good fortune to escape.

Of the Bases of the Cells where the Bees deposite their Honey. By Mr. Maclaurin, F.R.S. N° 471, p. 565.

The sagacity of the bees in making their cells of an hexagonal form, has been admired of old; and that figure has been taken notice of, as the best they could have pitched on for their purposes: but a yet more surprising instance of the geometry of these little insects, is seen in the form of the bases of those cells, discovered in the late accurate observations of Mons. Maraldi and Mons.

de Reaumur, who have found those bases to be of that pyramidal figure, that requires the least wax for containing the same quantity of honey, and which has at the same time a very remarkable regularity and beauty, connected of necessity with its frugality.

These bases are formed from three equal rhombuses, the obtuse angles of which are found to be the doubles of an angle that often offers itself to mathematicians in questions relating to maxima and minima; that is, the angle, whose tangent is to the radius, as the diagonal is to to the side of the square. By this construction, of the 6 solid angles at the base, that correspond to the angles of the hexagon, 3 are equal as well to each other, as to the solid angle at the apex of the figure, each of which solid angles is respectively formed from 3 equal plane obtuse angles: and the other 3 solid angles are also equal to each other, but severally formed each from 4 equal plane acute angles, supplements to the former obtuse ones.

By this form the utmost improvement is made of their wax, of which they are on all occasions very saving, the greatest regularity is obtained in the construction, and with a particular facility in the execution; as there is one sort of angle only with its supplement, that is required in the structure of the whole figure.

M. Maraldi* had found by mensuration, that the obtuse angles of the rhombuses were of 110 degrees nearly; on which he observed, that if the 3 obtuse angles, which formed the solid angles abovementioned, were supposed equal to each other, they must each be of 109° 28′; whence it has been inferred, that this last was really the true and just measure of them: and lately M. de Reaumur† has informed us, that Mr. Koënig having, at his desire, sought what should be the quantity to be given to this angle, in order to employ the least wax possible in a cell of the same capacity, that gentleman had found, by a higher geometry than was known to the ancients, by the method of infinitesimals, that the angle in question ought in this case to be of 109° 26′. And we shall now make it appear, from the principles of common geometry, that the most advantageous angle for these rhombuses, is indeed, on that account also, the same which results from the supposed equality of the 3 plane angles that form the abovementioned solid ones.

Let GN and NM, fig. 1 and 2, pl. 21, represent any two adjoining sides of the hexagon, that is, the section of the cell perpendicular to its length. The sides of the cell are not complete parallelograms as CGNK, BMNK, but trapezia CGNE, BMNE, to which a rhombus CEBe, is fitted at E, and that has the opposite

* Memoires de l'Acad. Royale des Sciences, 1712.—Orig.

† Memoires sur les Insectes, tom. v.—Orig.

point e in the apex of the figure, so that 3 rhombuses of this kind, with 6 trapezia, may complete the figure of the cell. Let o be the centre of the hexagon, of which CK and KB are adjoining sides; join CB and KO, intersecting it in A; and, because COB is equal to CKB, and KE equal to oe, the solid EBCK is equal to the solid eBCO; from which it is obvious, that the solid content of the cell will be the same wherever the point E is taken in the right line KN, the points C, K, B, G, N, and M, being given. We are therefore to inquire where the point E is to be taken in KN, so that the area of the rhombus CEBe, together with that of the 2 trapezia CGNE, ENMB, may form the least superficies. Because Ee is perpendicular to BC in A, the area of the rhombus is $AE \times BC$, that of the trapezia CGNE, ENMB, is $\overline{CG + EN} \times KC$; these, added to the rhombus, amount to $AE \times BC + 2KN \times KC - KE \times KC$; and because $2KN \times KC$ is invariable, we are to inquire, when $AE \times BC - KE \times KC$ is a minimum?

Suppose the point L to be so taken on KN, that KL may be to AL as KC is to BC. From the centre A describe, in the plane AKB with the radius AE, an arc of a circle ER meeting AL, produced if necessary, in R; let EV be perpendicular to AR in V, and KH be perpendicular to the same in H; then the triangles LEV, LKH, LAK, being similar, we have $LV : LE :: LH : LK :: LK : LA ::$ (by the supposition last made) $KC : BC$. Hence, when E is between L and N, we have $LH + LV (= VH) : LK + LE (= KE) :: KC : BC$; and when E is between K and L, we have $LH - LV (= VH) : LK - LE (= KE) :: KC : BC$; that is, in both cases we have $KE \times KC = VH \times BC$; and consequently $AE \times BC - \overline{KE \times KC} = AE \times BC - VH \times BC = \overline{AE - VH} \times BC = \overline{AR - VH} \times BC = \overline{AH + VR} \times BC$; which, because AH and BC do not vary, is evidently least when VR vanishes, that is, when E is on L. Therefore CLBl is the rhombus of the most advantageous form in respect of frugality, when KL is to AL as KC is to BC. This is the same method by which we have elsewhere determined the maxima and minima, in the resolution of several problems that have usually been treated in a more abstruse manner. See Treatise of Fluxions, Art. 572, &c.

Now because OK is bisected in A, $KC^2 = OK^2 = 4AK^2$; and $AC^2 = 3AK^2$, or $BC = 2AC = 2\sqrt{3} \times AK$; consequently $KC : BC :: 2AK : 2\sqrt{3} \times AK :: 1 : \sqrt{3}$, and $KL : AL :: (KC : BC) :: 1 : \sqrt{3}$, or $AL : AK :: \sqrt{3} : \sqrt{2}$; and (because $AK : AC :: 1 : \sqrt{3}$) $AL : AC :: 1 : \sqrt{2}$; that is, the angle CLA is that, whose tangent is to the radius as $\sqrt{2}$ is to 1, or as 14142135 to 10000000; and therefore is of 54° 44′ 8″, and consequently the angle of the rhombus of the best form is that of 109° 28′ 16″.

By this solution it is further easy to estimate, what their savings may amount to on this article, in consequence of this construction. Had they made the base flat, and not of the pyramidal form described above, then, besides completing the parallelograms CGNK and BMNK, the surface of the base had been $3CB \times AK$; what they really do form amounts in surface to the same parallelograms, and $3CB \times AH$: the savings therefore amount to $3CB \times \overline{AK - AH} = 3CB \times AH \times \frac{\sqrt{3} - \sqrt{2}}{\sqrt{2}}$, which is almost a 4th part of the pains and expence of wax, they bestow above what was necessary for completing the parallelogram sides of the cells: and at the same time they seem also to have other advantages from this form, besides the saving of their wax; such as a greater strength of the work, and more convenience for moving in these larger solid angles.

It remains that we should show, that the plane angles CLB, CLN, and BLN, are equal to each other. We before found, that KL : AL :: KC : BC :: KA ($= \frac{1}{2}$KC): AC; consequently KL : KA :: AL : AC, and the triangles LKA, LAC, are similar: therefore LK : AL :: AL : LC :: KC : BC :: 1 : $\sqrt{3}$, and LC = 3LK. With the centre L and radius LC, describe, in the plane CGNK, fig. 3, the semicircle DCP, meeting the line KN, in D and P; join CP and CD, and let LQ be perpendicular to CP in Q, then will the angle CDK be equal to QLP, and we shall have PQ : LQ :: PC : DC :: $\sqrt{PK} : \sqrt{DK} :: \sqrt{LC + LK} : \sqrt{LC - LK} :: \sqrt{4} : \sqrt{2} :: \sqrt{2} : 1$ :: AC : AL. Consequently the angle QLP = ALC, and CLP = CLB, or the obtuse angle of the Rhombus CLBl is equal to CLP, the obtuse angle of the trapezium; and consequently, the 3 plane angles that form the solid angle at L, or the apex at l, are equal to each other: from which it is obvious, that the 4 acute plane angles, which form the solid angle at C or B, are likewise equal among themselves.

Though M. Maraldi had found, by his mensuration, these obtuse angles to be of about 110 degrees; the small difference between this and the 109° 28′ 16″, just found by calculation, seems to have been either accidental, or owing to the difficulty of measuring such angles with exactness: besides that he seems to admit the real equality of the several plane angles, that form as well the apex, as the other solid ones we have been treating of. And, as to the small difference between our angle and that determined by Mr. Koënig, who first considered this problem, but has not yet published his demonstration of it, that can only be owing to his not carrying on his computation so far, and would scarcely have been worth the mentioning, were it not yet in favour of the practice of these industrious little insects; and did it not therefore give us ground to conclude, that in general, and when the particular form and circumstance of the honey-comb does not require a variation from their rule, the bees do truly construct their cells of the best figure, and that not only nearly, but with exactness; and that their proceeding could not have been more perfect from the greatest knowledge in geometry. How they arrive at this, and how the wonderful instinct in animals is to be accounted for, is a question of a higher nature; but this is surely a remarkable example of this instinct, as it has suggested a problem that had been overlooked by mathematicians, though they have treated largely on the maxima and minima; and such a one, as has been thought to exceed the compass of the common geometry.

It may be worth while to add here, that if the cells had been of any other form than hexagonal, and the bases had still been pyramidal, these must have been terminated by trapezia, and not by rhombuses, and therefore had been less regular, because OA and AK would have been unequal: nor could there have been room for such an advantageous or frugal a construction as that we have described, because the solid content of the cell would have increased with the right line KE. The cells, by being hexagonal, are the most capacious, in proportion to their surface, of any regular figures that leave no interstices between them, and at the same time admit of the most perfect bases. Thus, by following what is best in one respect, unforeseen advantages are often obtained; and what is most beautiful and regular, is also found to be most useful and excellent.

On the Transit of Mercury over the Sun, April 27, 1740; *and of an Eclipse of the Moon, Dec.* 21, 1740. *By Mr. John Winthrop, Hollisian Professor of Mathematics and Astronomy at Cambridge in New England.* N° 471, p. 572.

Mr. W. being advertised, by the calculations of Dr. Halley, that the former part of this transit would be visible in his horizon, was resolved to observe it in the best manner he could, with the few instruments he was furnished with. He expected that the centre of the planet would enter on the sun at $5^h\ 2^m$; but being apprehensive that he might be earlier than the calculation, he for some time before that, with a 24-foot tube directed to the sun, kept his eye fixed on that part of his limb where the planet was to enter, as steadily as he could for the wind, which then blew fresh. This precaution was not needless; for at $4^h\ 54^m\ 59^s$, he perceived that Mercury had made an impression on the sun's limb; by the quantity of which he concluded, that almost one quarter of his diameter might be entered. Hence he continued to watch the planet's progress; but unfortunately, by a shaking of the tube, he missed the moment of is interior contact with the sun's limb, but was certain it could be but very little later than $5^h\ 0^m\ 40^s$; for he presently after saw him fairly within the sun.

From several observations made during the transit, Mr. W. infers, that Mercury's horary motion in longitude from the sun, was now $3'\ 58''$; according to which, if we suppose the central ingress to have been at $4^h\ 57^m$, we shall find the difference of longitude at that time $3'\ 20''$; and the semidiameter of the sun being $15'\ 57''$, the latitude of Mercury must be $15'\ 36''$. Now the angle of Mercury's visible way with the ecliptic being, by the theory of his motion, $10^\circ\ 23'$, we must conclude the former of the observed latitudes about $4''$ too small, and the latter as much too large;—an error very inconsiderable in this kind of observations. From these things we may gather by an obvious computation, that Mercury was in conjunction with the sun, in respect of longitude, at $5^h\ 47^m$, with $14'\ 59''$ north latitude; and that his nearest distance to the centre of the sun was $14'\ 44''$; and when he was at his nearest distance, the difference of his longitude from the sun's was $2'\ 39''$, which he passed over in 40^m of time, and consequently arrived at the middle of his course in the sun at $6^h\ 27^m$: whence the semiduration of the central transit was $1^h\ 30^m$, and the end at $7^h\ 57^m$, an hour after sun-set.

As to the lunar eclipse, Dec. 24, 1740, the sky was unfavourable from clouds. However the following observations may be depended on.

At 5^h	24^m	A plain penumbra, apparent time.
5	35	The true shadow seems to enter.
8	30	End of the eclipse.

Of the Transit of Mercury over the Sun, Oct. 25, 1743, in the Morning, observed at Mr. Geo. Graham's House in Fleet-street. N° 471, p. 578.

The beginning could not be seen for clouds; but about $8^h\ 45^m$, Mercury was seen (through a reflecting telescope 3-foot focus, magnifying about 50 times) about 4 or 5 of his diameters within the sun's limb.

At Mr. Short's house in Surrey-street, Mercury was seen just past the interior contact at $8^h\ 30^m\ 59^s$, through a reflecting telescope 2-foot focus, magnifying about 70 times; the person who observed it says, that the thread of light between Mercury and the sun's limb was so small, as scarcely to amount to the 20th or 30th part of Mercury's diameter.

Mr. Graham got an observation made by a person in his neighbourhood, by which it appears, that at $11^h\ 59^m\ 50^s$, Mercury preceded the sun's centre $42''$ in right ascension.

The sky clearing up towards 1 o'clock, the following times were observed at Mr. Graham's house with great accuracy.

Last interior contact at	1^h	0^m	42^s
End, or Mercury just leaving the sun's limb at	1	2	16

This last observation agrees to a second with the same observation made by Dr. Bevis at Mr. Sisson's house in the Strand.

Eclipse of the Moon, Oct. 22, 1743, *in the Morning, observed at Mr. Graham's House in Fleet-street.* N° 471, p. 580.

The sky was mostly overcast with clouds, so that the following observations are the only ones that could be made with any degree of certainty.

Beginning of the eclipse about	1^h	21^m	0^s
The shade touched Copernicus about	1	39	0
.... touched Plato about	1	45	0
.... touched Tycho about................	1	51	0
Total immersion about	2	17	0

Concerning the Remains of an ancient Temple in Ireland, of the same sort as the famous Stonehenge; and of a Stone-Hatchet of the ancient Irish. By Robert Lord Bishop of Corke, F. R. S. N° 471, p. 581.

These ancient remains were found in the county of Cork, in the parish of Kilgarriffe, about 10 miles from Bandon to the south-west. They consist of 9 large upright stones, in a circle, with a conical one in the centre, besides one at some distance quite out of the circle. It has been a very ancient heathen temple, and the burial-place of some person of great note, before the erecting of covered temples was used, in this, or perhaps in any other part of the world, except Judea. Which sort of places of devotion seem to be the most ancient of any that we have accounts of in history. For temples were originally all open, and thence received their name, according to Varro (lib. 6, de ling. lat.) a templando, which was an ancient word that signified to see or look out. The middle stone, which was the place where the priest stood, is lower than the rest, not being above 3 feet high, and was always dedicated to some deity or other; and was consecrated to that use by the pouring on of olive-oil: which custom was of very ancient date, and seems to have been borrowed from the practice of the ancient patriarchs, who called these stones Bethel, which word literally signifies in English, God's house: and, by a corrupt pronunciation of the word, they were in Greek called Βαιτύλια (vide Sanchoniatho). Which is the reason why that stone, which Rhea is supposed to have given Saturn to swallow instead of a child, is called Βαιτύλος; and not because it was covered with a woollen garment, which is called Βαίτη in Greek, as Hesychius pretends. Hesych. Etym.

It is remarkable, that some of these stones manifestly appear to have been reduced to their present form by art. There is no appearance of any mark of a tool; so that it is probable, that this was done with great labour, by the assistance only of sharp stones; which, before the invention of iron, or of that metal's being common, seems to have been the usual instrument of operation in other circumstances as well as this. For it is observed of Zipporah, the wife of Moses, when she was ordered to circumcise her son, that she took a sharp stone, and cut off the foreskin of her son. (Ex. iv. 25). And, when God orders Joshua to circumcise the Israelites, he says, make thee sharp knives, as we translate it; but in the original it is, knives of sharp stones. (Josh. v. 2, 3).

Herodotus and Diodorus Siculus both take notice, that it was the custom among the ancient Egyptians, at the time of embalming the dead, to cut open the body with an Ethiopic stone (Herod. Euterp. Diod. lib. i. c. 5): and Ovid, in describing the origin of the customs of the Corybantes, &c. says, that a Phrygian youth with whom the goddess Cybele was in love, and to whom he proved faithless, for a punishment to himself,* cut himself all over with a sharp stone; ille etiam saxo corpus laniavit acuto, &c. (Ovid, Fast. 4).

It is manifest, indeed, that the use of iron was found out in Egypt before the time of Joshua and Moses, both of whom mention it as made use of not only for cutting of soft things,† but also for chizeling of stones (Deut. xxvii. 5, Josh. viii. 31). But I apprehend it must have been very rare, and that the art of reducing iron to the hardness and consistency of steel, was not yet discovered; because, when God orders Joshua to write the words of the law upon stones, as soon as he had passed over Jordan, the way he is ordered to do it is this; to plaster the stones over with plaster first, and then to grave in this plaster the words of the law (Deut. xxvii. 2, 3). And yet this is called both by Moses and Joshua, writing upon the stones (Deut. xxvii. 8).

It is certain, that the art of polishing of jewels, and of cutting one hard stone with another that was harder, was invented and practised in Egypt before the time of Moses; for he speaks of graving the names of the children of Israel in 2 onyx stones, which, being harder than iron, even than steel, are not to be wrought upon with this; but must be cut by some stone which is harder than themselves. Wherefore Moses says, with the work of an engraver in stone, like the engravings of a signet, shalt thou grave the two stones (Ex. xxviii. 9, 11). And therefore the prophet Jeremiah mentions a pen of iron, as made use of for engraving (Jer. xvii. 1).

But the use of iron seems by no means to have been found out in these western parts of the world till much later; and therefore it is probable, that the inhabitants of these countries made use of stones, which were the original instruments used in cutting both for domestic and military service, in all

* Of the antiquity of this practice, see Lev. xix. 28.—Orig.

‡ Joseph, when he was sent for by Pharoah, shaved himself, Gen. xli. 14.—Orig.

countries of the known world; as appears of late years from the practice of the Americans. And it is also manifest, from the many instruments of war, that are made of stone, which have been dug up in these western parts of Europe, that the use of iron was not very common in these parts, till of late years. Montfaucon, in the 4th and 5th tome of his Antiquities, gives us an account of several tombs being opened near Paris, and in other places; where the hard and destructive part of the weapons found consisted of stone. He particularly gives the cut of a stone-hatchet in his own possession, which was made of touchstone, in the 4th tome of his Supplement, p. 30. But the bishop has in his possession a much more complete one, made of the same kind of stone, expressed by fig. 1. pl. 20, which is done with exactness, by a scale of a quarter of an inch to an inch, plainly made for doing execution both ways; and therefore answers the description given by Montfaucon of the Amazonian hatchet, or the sagaris of Xenophon (vide Montf. tom. iv. p. 69). The handle is made of yew, and the stone is not inserted into the handle at right angles, but makes an acute angle below towards the hand; the use of which appears at first sight.

Concerning a Zoophyton, somewhat resembling the Flower of the Marigold. By the Rev. Griffith Hughes,† Minister of St. Lucy's Parish in Barbadoes.* N° 471, p. 590.

However surprising the description of a flower, which is probably a real animal, may appear; yet we cannot, without the highest arrogance, presume to prescribe limits to the power of the Almighty, who, for wise ends, sometimes hides his works in such darkness, as to be concealed from the most exalted human knowledge. There are no ages past, in which fresh and numerous instances of his wonderful works have not discovered themselves. And what, in ours, seems most inexplicable, will, possibly, appear to futurity no more than the natural consequence of other discoveries then become familiar.

At the north end of the island of Barbadoes, in St. Lucy's parish, is a cave about 14 feet long, and 11 wide: its bottom is a basin always full of transparent salt-water, its greatest depth about 3 feet: in this basin there is a stone of about 4 feet long, and 3 in breadth, always covered with water. From small holes in the sides of this stone, at different depths under water, appear in full

* The animal here described is a species of Actinia, and is the Actinia Calendula of Solander and Ellis.

† Author of the Nat. History of Barbadoes, folio, 1750, with many plates.

bloom, at all times of the year, several seemingly fine radiated yellow flowers, with thick-set distinct petala:* these flowers, on the approach of a finger, or when disturbed by any thing else that came within 2 or 3 inches of them, would in an instant close all their leaves together, and the flower, stalk and all, would shrink back into the cavity of the stone: yet, if undisturbed for the space of a few minutes, they would again come in sight, and by degrees expand their leaves, and appear in their former beauty. From such an appearance at first he gave it the name of a sensitive flower; especially when he once saw several stamina, but without apices, rise up from the socket of the flower. Yet no sooner had these appeared to give the idea of a perfect flower, but that replete with animal life, if motion, and a capacity of self-preservation may be called such; these claws, or arms, darted from one side of the flower to another, and about its verge, with a quick motion, as if in search of prey. What further confirmed him in this opinion, was, that he observed these claws, when in motion, to be jointed, and that they would often close together, as so many forceps; though their appearance was but for a short time, soon retreating and disappearing again in the socket of the flower. As this seems, if it is allowed to be an animal, to be its manner of taking its prey, he queries whether, as these radiated leaves can in an instant close, with a strong elastic force, to avoid danger, they may not also, when the prey is brought within their circle, be of use to confine and secure it in their embrace, till it is conveyed to the mouth; which he supposes to be in the socket, of what he at first called a flower. The top of the stone, out of which these seeming flowers grow, is covered over with clusters of water-bottles, that resemble unripe grapes. Among these he found also several small blue flowers, resembling the yellow ones in their form and other qualities. See fig. 4, pl. 21.

Concerning the Seeds of Mushrooms. By the Rev. Roger Pickering, V. D. M.
N° 471, p. 593.

Mr. P. says he was always of opinion, that these plants had their seeds as well as others; and attributed the not discovering them hitherto, to the shortness of this plant's duration, and to its succulent and loose texture, by which it is liable to immediate putrefaction from the least alteration of weather. He could no otherwise account for the method used by the Italians, who make

* At first sight this species of animals greatly resembles the flower of the marigold, but is of a paler yellow. I take it to be a sort of urtica marina, of which Gesner has given descriptions and figures in his book de Aquatilibus; but a figure very nearly resembling this above described, is to be seen in Johnston, Hist. Nat. de Exanguibus Aquaticis, tab. 18. C. M.—Orig.

mushroom beds in their cellars, with a mixture of fine mould, and the parings of mushrooms laid upon dung; and that of our gardeners, who water their beds with water, in which such parings are soaked; but by supposing, that their success was owing to minute seeds lodged and retained in such parings, and washed off by such infusions. So also, as to the mouldiness of old dung and thatch, which the gardeners are very fond of in making their mushroom beds, he apprehended, that this mouldiness was not the nutritive juice or salt proper for the production of this plant, but the mushroom itself in its early and inceptive state. Some warm rains enabled him to reduce his conjectures to a certainty; by which he not only discovered, that this mouldiness is a collection of little mushrooms adhering to each other by minute fibres, or, as the gardeners in other cases call them, runners; but he discovered and preserved the seed of mushrooms.

He had prepared for his observations, by ordering the gardener to make a mushroom bed, in a well-sheltered place, after the usual manner; which was finished about 6 weeks without any appearance of shoots. But a few days after that, a great plenty appeared above-ground, among the asparagus, and on the grass walks, as indeed he expected, because on the night before there had fallen $\frac{1}{3}\frac{9}{6}$ of an inch of rain, which, together with an unusual height of the thermometer, for the season, made it the most suitable weather for mushrooms. He immediately chose out the most promising plants, which he covered with bell-glasses; where there were several together, and the single plants with little hand-glasses, which he had made for the preservation of wall-fruit.

Soon after he carefully gathered about a dozen mushrooms, of the esculent kind, from under the glasses; choosing such as gradually differed from each other in the colour of their gills, from a faint peach-bloom colour, to a deep purple; thinking that as he had got the mushroom, in its several states, secured by these glasses from the injury of the weather, he should be able to discover the seed.

With these he gathered several mushrooms of another kind, commonly known by the name of Champignons; which also he had secured under glasses. With these he began, and soon found, that the gills, as they are called, are no other than capsulæ, or pods for the seed; for with one of the lower magnifiers, and a fine penknife, he could easily divide them from adhering to each other. This encouraged him to apply directly to the larger sort of mushrooms; and accordingly he fixed on one of a deep flesh colour, which he thought was in its prime. He began with one of the gills carefully separated from the head, or stool, without bruising; but could discover nothing in it like seeds, except that here and there there were some globular dark spots, appearing, through the 5th magnifier, about the size of very small pin-heads: but when he endeavoured, with a fine brush, to wipe off any thing, to fix it on a talc, the lightest touch reduced it to water. On this, he had recourse to a thin, but tough filament, which was situated on the stalk or stem of the mushroom, in an exact distance from the head of the mushroom, and the mark, which the earth round about the stem had made. On this filament appeared a fine downy substance, of a lively brown, resembling the down on a moth's wing, but much finer. He could brush off some of this upon white paper, without reducing it to water; but, not having the new apparatus for opaque objects, there was nothing that appeared bold or sharp enough for him to depend on. He had then recourse to a fine talc in a slider, and brushed off some of this brown dust upon it; and after applying the 2d magnifier, he was gratified with the first sight of the seed of mushrooms; for he then discovered a multitude of round, regular, transparent bodies, bearing the same appearance as the farina of flowers. He then applied the highest magnifier, through which they appeared very bold, of the size of a moderate pin's head.

Fig. 5, pl. 21, shows a sketch of the mushroom, &c. in its just proportion. In which, a is the mushroom; b, the filament on which the seed was discovered, being probably a wise provision of nature, to prevent the wind's power over such minute bodies as the seeds are; for, by being placed at an exact distance between the head of the mushroom and the ground, it secures the seed before the wind's power can affect it, unless the wind be high; and, by another easy fall, enables it to lodge itself safely in the ground. This thin filament is that to which the edges of the head of the mushroom adhere, while it is, what is commonly called, a button, and from which it separates by expanding into a flap.

c, the part of the stem under-ground, from which the fibres shoot, on which the little mushrooms, marked d, grow, appearing at first but like a white mouldiness.

In fig. 6, a, b, are animalcules of the maggot, or fly-kind, found in the head and stems of mushrooms in a decaying state.

Fig. 7, the seed of the mushroom, as it appears through the first magnifier.

P.S. After writing the above, Mr. P. met with Sig. Micheli's Nova Genera Plantarum, where he found the observations above made on mushrooms, though entered on without any hint or direction from that, or any other writer, pretty near the same with his. Mr. P. thinks it therefore a piece of justice, due to him, and to the reading and judgment of Mr. Watson, candidly to allow the first discovery of the seeds of mushrooms to that Italian botanist.

Some Remarks occasioned by the preceding Paper, addressed to the R. S. by Mr. Wm. Watson, Apothecary, F. R. S. N° 471, p. 599.

Without derogating from Mr. Pickering's merit, Mr. Watson observes that it was to the late Sig. Micheli, professor of botany at Florence, that the world owes the first discovery of the seeds of mushrooms, as well as the flowers and seeds of the various species of lichen or liverwort: he not only saw with his glasses, but raised from their seeds, many kinds of mushrooms, as may be seen by his experiments in p. 135 of his excellent work, entitled Nova Plantarum Genera, printed at Florence in the year 1729. He constantly observed the seeds produce the same species, as in the more perfect plants.

A very worthy and learned member of this society, Dr. Haller, professor of physic, botany, &c. in the university of Gottingen, in his excellent work published last year, entitled Enumeratio Methodica Stirpium Helvetiæ, tells us, when treating of funguses, p. 34, that their seeds are produced in the laminæ of their concave side; as he has most evidently seen in the 35th, 50th, 73d, and 107th species mentioned in his work; which seeds are by nature, when ripe, shaken from the plants, and, being sown, propagate their species. He likewise mentions, that the seeds of different mushrooms vary in their colour, some being blue, others green, white, &c.

The late Mr. Ray indeed mentions a fungus, discovered by his friend Mr. Doody, which he calls, in his history of plants, vol. 3, p. 21, fungus seminifer externe striatus; and M. Tournefort, in his Institutiones Rei Herbariæ, p. 560, takes notice of another species of this tribe, which he calls fungoides infundi buli-forme semine fætum. M. Vaillant, in p. 57 of his Botanicum Parisiense, gives a description and figures of the seeds of these two kinds. His words are to this purport, when translated from the French: "Within the cavity," says he, "of these plants, towards the bottom, are contained many seeds heaped one upon another, cut on their superior surface somewhat like a triangle, broad underneath, where they are connected to a little tendon, and are whitish." Notwithstanding the high veneration he had for the opinions of these able botanists, he was satisfied the parts of these two plants, so imagined, are not their seeds, but rather their suckers, stolones; which, in most others of this tribe, are produced from the root; but from both these, as in many of the kinds of lichen, and in the dentaria bulbifera, are produced from other parts of the plant. Mr. W. observes, that in almost all plants, whose seeds are produced sparingly, or are difficult to be saved, Nature abundantly makes up that deficiency, by the great increase of their roots, by which their species may easily be propagated; as is manifest in mushrooms, potatoes, crocuses, golden-rods, starworts, and above all in the corona solis, flore parvo, tuberosa radice, of M. Tournefort, vulgarly called Jerusalem artichokes, the seeds of which, from the shortness of our summer, having never as yet ripened in England

He further adds, that though many species of mushrooms are eatable, and some of them better flavoured than the common sort, the gardeners only propagate that sort with red gills, called, by way of excellence, champignon, a name given by the French to all sorts of mushrooms; but some descriptive word is added to them, by which they may be distinguished from this. The method of propagating mushrooms, according to the usual practice, from their suckers, was first mentioned by La Brosse, in his treatise De la Nature des Plantes, and afterwards by M. Tournefort, in the Memoirs of the Academy of Sciences, Anno 1707, page 72.

Of the Disappearance of Saturn's Ring, in the Years 1743 *and* 1744. *By M. Godofred Heinsius, of Petersburg.* N° 471, p. 602.

This paper is of no use at present.

An Abstract of a Natural History of Greenland. by Hans Egede, entitled, Det gamle Gronlands Perlustrastion, eller Naturel Historie, af Hans Egede. Kiobenhabn, 1741, *4to. Communicated by John Green, M.D.* N° 471, p. 607.

Greenland lies about 160 English miles west from Iceland, beginning at 59° 40′ north latitude. Its east side stretches to Spitzbergen, in 78 to 80° latitude, and believed to be an island separate from Greenland.

Its west side is known to 70° latitude. If Greenland is an island, or joined to other countries, it is not known for a certainty, but probably joins to America on the north-west side: for between America and Greenland, stretches the fretum, or bay, called in the sea-charts Davis's Straits, which is navigated by them and other nations on account of the whale-fishery, but to the bottom of this sound no ship has ever been.

Greenland is a high rocky country, which is always covered with ice and snow, which never thaws except near the sea. The highest land can be seen 80 English miles from the sea. The whole coast is fortified with large and small islands. It has several firths or rivers, which run a long way within land; among which is Baal's River, where the first Danish colony was fixed in 1721, which runs 80 miles within land.

Greenland was first discovered by the Norwegians and Icelanders; and the brave Raude, who first discovered it in 982, praised it, and persuaded several of his countrymen to inhabit it; and at the instance of Oluf Tryggeson, first Christian king in Norway, carried a priest with him, who taught and baptized all the inhabitants; and from time to time Greenland multiplied into new colonies, many churches and abbeys were built, bishops and other teachers provided for: but the Norwegians were not the first inhabitants; for they found wild people on the west side, who doubtless were originally Americans. The present inhabitants probably are a race of the Schrellingers.

In 1721, a company of traders was set up in Bergen, with a royal privilege, when king Frederic resolved to begin a colony at 64°, with which Egede and his family went, and continued 15 years. Their design was to find the eastern district, as the best. A Hollander affirmed some of their ships had been there, and found the land free from ice in 62°. This Mr. E. found to be true in 1736.

In the Bay of Hope there are many good places for feeding cattle, with proper grounds for tillage, and good water: no trees, except within the rivers, only brush-wood; juniper-bushes abound here, whose berries are the size of the largest peas. There are divers plants here, as angelica, rosemary, scurvy-grass; and a grass with yellow flowers, whose root smells like roses in the Spring. In 60 and 65°, the country is best, and barley will ripen there: turnips and colewort grow well; especially the first, which are large, and of a sweet taste.

There are rocks which produce verdigrise, as also sulphur or brimstone, marcasite; and he found on an island one of a yellow brown sand, having cinnabarine red veins. There are whole mountains of the asbestos. There is found a grey stone, or bastard marble, of different colours. The sea produces several sorts of conchs and mussels, also divers sorts of corallines.

The summer here lasts from May to September. The cold at 64° is moderate, but at 68, &c. extreme, and will freeze brandy. The land is constantly covered with ice and snow, except near the sea, and in the rivers. Though the summer oft-times is warm in Greenland, it seldom or never thunders, &c. The aurora borealis is so strong here towards new moon in clear weather, as you may read by it.

Greenland produces bears, which live on the ice, and are dextrous at catching otters, seals, &c. Rain-deer are in great plenty. Hares are very large, good, and white all the year. There are plenty of foxes. They have dogs, none of which can bark, but only howl. Their birds are the ryper, or wood-partridge, ravens, eagles, falcons, sparrows, goldfinches, &c. The mosquitoes are very troublesome in July and August.

Besides whales, the seas produce the sword-fish, the whale's greatest enemy: and when he kills one, eats nothing but his tongue, leaving the rest to the shark, walross, and birds of prey. In these seas are cachelots or pot-fish, a sort of whales, their length from 50 to 70 feet. The white-fish are likewise in these seas, like a whale, but without fins on the back. There is likewise a small whale produced here called butts-kops; as also unicorns of the whale kind, which they call narval. The niser, or porpoise, is also in these seas; as also the walross, shaped like a seal, but much larger; his flesh is like fat pork; his irreconcible enemy is the white bear. There are several sizes of seals, but of the same shape, except the Klap-myss, which has a cartilaginous hood, that covers his eyes. There are other fish, as sharks, holly-butts, red-fish, trout, salmon, bull-heads, stone biters, smelts, whitings, herrings, and a fish like a bream, with pricks on its whole body. There are mussels, and some large ones that produce the pearl: here also are shrimps, crabs, &c.

Among the sea-birds, are the edder ducks, of three kinds; as likewise the alker, and the tornauviarsuk, which has beautiful feathers, and the size of a lark. There are also geese here. Greenland produces maws, redshanks, cormorants, lunders, parrots, sharvers, tersters, angle-tasters, snipes, &c.

The employments of the Greenlanders, on shore, are to shoot rain-deer; and at sea to catch whales, seals, birds, &c. Their bow is about 6 feet long, of tough fir, which they bind round with deer sinews: the arrow is pointed with iron or bone. All the sort of fish they catch, and cannot eat fresh, they dry against winter.

The boats are of two sorts; one used only by the men, about three fathoms in length, their breadth about 19 inches, with a hole in the middle, not larger than one man, close-laced, can thrust himself into: with these boats they are able to row 72 miles a day, using only one oar.

Their houses are of 2 sorts, winter and summer: the former are made of turf and stone, from 4 to 6 feet high, flat roofed; on one side are the windows, made of bleeched seal-guts, holly-butt maws, sewed together, and are sufficiently transparent; their doors are very low, by which they creep in on their hands and knees. Their summer-houses are made by raising poles, which they cover with young seal-skins.

Their language has no affinity with any known European one; few words are like the old Norwegian. It is difficult of pronunciation, as most of their words are gutturals. It wants the letters c, d, f, g, x.

Some Observations on a Polype Dried. By Mr. Henry Baker, F. R. S. N° 471, p. 616.

Mr. Baker apprehending that if a polype could be dried, and well extended before the microscope, some particulars in its structure might be distinguished better than when it is viewed alive, and in water, he applied himself to attempt it; and, after many trials, which were rendered fruitless by the minuteness and extreme tenderness of the arms and other parts of this animal, that contract as soon as taken out of water, and so cling together, as to become inseparable afterwards, without being torn to pieces, he happened, at last, to hit on a method of performing the operation perfectly; which method he here explains, as taken from his essay on this creature lately published, entitled the "Natural History of the Polype;" where the description may be seen at p. 84.

Fig. 8, pl. 21, represents the polype, as dried and viewed by the microscope; by which the following observations may be made: 1. As the body thus dried exhibits a reticulation of minute vessels, which appear every where most curiously interwoven, we may reasonably suppose they serve as veins and arteries, through which some kind of blood or juices circulates, as in other animals.

2. The anus of the polype may be discovered very plainly in this dried object; whereas in a living one it requires much attention to see it in a satisfactory manner.

3. The mouth, or opening between the arms, appears here like the mouth of a sack or bag, which indeed the body does not badly represent.

4. By observing the arms thus dried, we obtain a clear idea of the means by which this creature catches fast hold of its prey, the moment of its touching it, and before it can bring its arms to clasp about it; for we plainly see here, that the arms are thick beset with hairs, or rather sharp hooks, which possibly are moveable, and can strike easily into the body of a tender worm. But these hooks or hairs are not visible in the living animal; being then, perhaps, some how or other generally drawn in, or laid flat and close along the sides of the arms, as in some sorts of star-fish. Besides, the water wherein we are obliged to view a polype, when alive, will not permit so strict an examination as it can now be brought to.

A Catalogue of the fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1740, *pursuant to the Direction of Sir Hans Sloane, Bart. P. R. S.* N° 471, p. 620.

This is the 19th presentation of this kind, completing the number in all to 950 plants.

Of the Transit of Mercury over the Sun, Oct. 31, 1736, *and Oct.* 25, 1743. *By Dr. Bevis.* N° 471, p. 622. *From the Latin.*

In the following observations, Dr. Halley attended the clock, while Dr. Bevis observed the transit with the 24-foot telescope. He began about 8 o'clock to watch the commencement, but could see nothing in the sun besides spots.

Presently after the sun was covered with clouds. About 10 the clouds opened for a moment, when Mercury was seen on the sun. About noon it began to clear up, when Dr. B. renewed his application, and made the following observations.

Oct. 30^d	23^h	50^m	45^s	App. time, the centre of Mercury was 1′ 18″ distant from the sun's nearest limb, by the micrometer.
31	0	2	39	Mercury was about his own diameter distant from the sun's limb.
		7	4	the centre just emerged.
		8	33	the exterior contact, the sky being very clear.

The diameter of Mercury was so small, that with the microscope it hardly measured 10 seconds.

The transit of Oct. 25, 1743, he observed at Beaufort-buildings, situated about half a minute of time west from the Royal Observatory. Mr. Sisson counted the clock, while Dr. B. observed the sun.

At 8 in the morning nothing appeared on the sun, and he was soon after obscured by clouds. At $10\frac{1}{4}^h$ he first saw the planet on the sun, the transit being then nearly finished. About noon the sky cleared up again, and allowed the Dr. to make the following observations.

Oct. 25^d	0^h	58^m	34^s	app. time, the distance of their limbs was nearly equal to Mercury's diameter.
	1	0	33	the last interior contact.
		1	25	the centre just emerged.
		2	16	the last exterior contact.

The day before, a little after noon, he took the sun's diameter, equal to 32′ 27″.

Mr. Bird made a good observation, near the beginning of the transit, in Surrey-street, about $1\frac{1}{4}$ minute of time east of the Doctor's situation. He perceived a very small thread of light between the sun and Mercury, which had just entered, scarcely equalling a 10th of the planet's diameter, at $8^h\ 30^m\ 56^s$, that is, at $8^h\ 30^m\ 54\frac{1}{2}^s$ at Beaufort-buildings. Hence, Dr. B. infers the total ingress at his situation to $8^h\ 30^m\ 40^s$ as nearly as possible

He therefore sets down the whole transit, as seen at Beaufort-buildings, as follows:

Oct. 24^d	20^h	28^m	57^s	app. time, the first exterior contact.
		29	$48\frac{1}{2}$	the ingressus of the centre.
		30	40	the first interior contact.
Oct. 25	1	0	33	the last interior contact.
		1	$24\frac{1}{2}$	the egressus of the centre.
		2	16	the last exterior contact.

Concerning a Child of a Monstrous Size. By Mr. Geoffroy, F. R. S. and Member of the Royal Academy of Sciences at Paris. N° 471, p. 627.

Normandy furnished, some years since, a child, monstrous by its size, and a strength which its age could not naturally afford. It was born at Rouen, and is a prodigy of virility, of 3 years and 2 or 3 months of age, and is now in the hospital at Rouen. It has a very large neck, the breast very broad, and the belly larger than in its natural state. The upper part of the thighs is rather thickish, the rest is conformable to its age. He has hair only about the privy parts; the penis is 3 inches long when there is no erection, but of 6 when there is. It has been found that he has emissions. The fact is very true, and M. le Cat. F. R. S. a surgeon at Rouen, has fully traced it out.

Two remarkable Medical Cases, one of an extraordinary Hæmorrhage, the other an Ascites cured by Tapping. Communicated by Henry Banyer, M. D. N° 471, p. 628.

In January 1729, Daniel Goddard, a gardener, about 24 years of age, at Wisbech in the isle of Ely, received a slight puncture from a rusty nail in the sole of his right foot. And, notwithstanding there was not wounded any tendon, or blood-vessel, larger than small branches of veins, the whole foot was immediately swelled to a very unusual degree, without any fever, or other apparent cause for it. It was also attended with great pain, and an extraordinary pulsation on the part, as in wounds of arteries, and so distended as if the blood would burst out of its vessels.

After 2 days, on opening a superficial sinus, to enlarge the wound, there rushed out immediately such an obstinate flux of blood, as would not yield to any styptic means, longer than the bandage was held on by some strong hand. And though, by this incision, no vessels were wounded, but capillary veins; yet this hæmorrhage continued as violent as at first, for 6 days successively, whenever the necessary means were relaxed. On which, for the sake of revulsion, the patient had a vein opened on the arm of the opposite side; and it had such a sudden and surprising effect, that the flux of blood in the foot instantly ceased, and the wound healed very soon without any further trouble; but the flux of blood, consequent on venesection, became equally as difficult to restrain, as that in the foot for the space of 4 days; all which time it would have continued to flow most violently without the strictest bandage, or the same care of the hand, as before. Perhaps the period of this hæmorrhage might have been much longer, if Dr. B. had not suffered the ligature on the arm to be loosened now and then, as he judged the redundancy of blood required, for the sake of some evacuation, at each time. After the bleeding, he soon recovered his strength, so as to do his business in the gardens; and continued very well till the month of March 1730. About the middle of this month, he complained of sleepiness, and a particular heaviness all over his body; which was followed, in 3 days time, by a violent hæmorrhage from the nose. This flux, in spite of all means being tried, except venesection, continued 7 days, and could never be totally stopped, all this time, for 1 hour together. He recovered again in a very short time, and was able to work in the summer season, without any complaints, till October following. Then the hæmorrhage returned again at the nose, as before, with all the same circumstances, and in defiance of all endeavours, continued the period of 7 days. Thus it returned in like manner of bleeding, by stools, in the middle of March 1731, and continued to discharge this way great quantities of blood, in one motion, and sometimes two motions every day for 7 days together, in opposition to the most efficacious restringents. Also it made its regular return by vast profusions of blood from the intestines, in the beginning of October following, to the end of the first period of 7 days, without gripings, or any such uneasy sensations. Thus again it kept as orderly returns about the vernal and autumnal equinoxes of the years 1732, 1733, with vast profusions of blood by stool, for the usual term of 7 days, agreeing in all circumstances with the preceding years. Likewise at, or very near these two grand seasons, in the years 1734, 1735, this habitual hæmorrhage broke away by the kidneys and urinary passage; and still constantly, for these 2 years, kept its old stated time of 7 days, without any other variation.

This young man was seized in Dec. 1735, with the small-pox, of the distinct kind, which produced such a change in his constitution, that he escaped those periodical hæmorrhages, or any other spontaneous equivalent evacuations, for the two seasons of the year 1736; and remained in very good health till Christmas following, being above 13 months free from any symptoms of his old eruption. But, Dec. 27, without any previous notice of heaviness and sleepiness, the hæmorrhage returned by the urinary passages, but much more favourably, and continued only 3 days. Again, on May the 13th following, 1737, he then felt the previous warnings, and bled again by urine to the 20th of the same month; with this difference, that for 3 days the urine was only coffee-coloured, but afterwards, for 4 days longer, every discharge resembled an effusion of blood from a vein just opened. He presently recovered his strength, even though the air was exceedingly warm at this time; and the Doctor saw him 5 months after, very robust and healthy, and free from all kinds of tendency towards his old complaint. But he had always the appearance of too much fulness, though his constitution did not suffer so much as might reasonably be imagined, from such profuse hæmorrhages. He had no return of his bleeding, or any thing like it, the ensuing autumn; but remained perfectly well all the following winter season. Afterwards the Doctor had no opportunity of making further personal inquiries, but was informed by an intelligent man, that in March 1738 this unfortunate person got a slight wound again, somewhere on one of his legs, which proved equally as difficult, with respect to the flux of blood, as the first puncture in his foot. And, whether from too strict a restraint of the hæmorrhage, or for want of venesection, he fell into very violent convulsions for 4 or 5 days, and died in a manner like suffocation, from too much redundancy of blood.

As this hæmorrhage never once depended on any other distemper, or observed any regular concurrence with the revolutions of the moon, it appears to be a very extraordinary simple plethora. During the 4 years that this flux of blood came from the nose and intestines, the urine was never of a higher colour than amber; nor was there any symptom of a fever by the pulse, or otherwise, for the whole term of the disorder.

March the 26th, 1739, the wife of Mr. Matth. Wilkinson, of Long Sutton, in Lincolnshire, was tapped for an ascites, proceeding from frequent hæmorrhages, and a too liberal use of small liquors. She was between 30 and 40 years old, of a very low stature, and always of a weak constitution. The water was all taken away at one time, and measured 5 gallons. She was very faint immediately after the operation, and remained so for near 3 weeks after. But, by great abstinence from liquids, excepting Lower's bitter infusion, and sometimes a spoonful or two of cordial julep, she perfectly recovered her health again; and to a much better degree of it than she had enjoyed for many years before; without any appearance at all of a return of the abdominal tumour. The water was clear, and readily turned to a strong jelly on heating it; and Dr. B. was certain, there was unavoidably left in the abdomen a quantity sufficient to prove the existence of absorbent vessels. Perhaps those patients, in this distemper, whose water turns to a jelly, have a better chance to be cured by paracentesis, than others, whose discharge is more like urine, and will never curdle by heat. But time and repeated observation must confirm this opinion.

Concerning certain Chalky Tubulous Concretions, called Malm; with some Microscopical Observations on the Farina of the Red Lilly, and of Worms discovered in Smutty Corn. By Mr. Turbevil Needham. N° 471, p. 634.

The bed of malm lies in a valley, at the foot of a long ridge of chalky downs; extends from Winchester, where it begins, almost due south, about 4 miles the breadth not above a quarter of a mile; and depth, at a mean computation, about 5 feet. It is used in manure for the same purposes as chalk is, but answers the intent much better. It rises up in one continued bed, almost to the surface; where a thin layer of common earth but just hides it in places, where continual cultivation has not superinduced a new soil. The whole bed consists of separate detached pieces, and of several dimensions, mostly long and tubular; some few round, with a small cavity in the centre, others quite flat, and some, as it were, excavated on one side, as if the chalky laminæ had extended themselves round a piece of bark; but all of them hollowed within, agreeable to their exterior shape, very few excepted.

This valley formerly was probably over-run with wood, if not wholly, at least for some considerable length and breadth: wild boars tusks, which are known by their length; stags-horns, and a flint-knife, which have been found buried to some depth, in the malm, seem to evince as much. That trees of considerable dimensions have grown in it, is very evident; for, in a drain, lately made to convey the water from the main river to the adjacent meadows, trees of a vast size may be seen, at 2 or 3 feet depth, in no small number, retaining both shape and substance in some measure, though much decayed, and not so compact and solid in those parts, which have been exposed to the water; these lie out of the verge of this bed of malm, and are not consequently affected by it.

Now probably these trees, with the rest of the wood, might, by age, and some accident combining with it, have fallen; the uppermost might have served to bury the rest, and preserve them from a more immediate decay, by cutting off their communication with the exterior air. Rains, in process of time, must have washed off from the adjacent hills to some certain distance, and deposited in the neighbouring valley, but mixed with other heterogeneous substances, as decayed wood, earth, &c. a quantity of chalky particles, sufficient to involve, by a continual addition of new laminæ, roots, trunks, branches, twigs, and the broken extremities of twigs; and tending continually to form masses resembling the supposed particulars. He supposes, that the pieces of wood have been invested continually by additional laminæ; that the first laminæ must have adapted itself to, and assumed the exterior shape, whether smooth or knotty, of the inclosed wood; that the others have proceeded accordingly; that the extremities have gradually rounded themselves; and that in the interim, till they were wholly closed, the included wood has been insensibly attenuated by the passing moisture, and, particle by particle, either entirely, or in part only, wasted away. And though it may be objected against this supposition, that some pieces are entirely solid, and have the resemblance of white-thorn; yet these are but rarely found, and may very well be supposed to have been a species of wood of a more solid and durable contexture; which might consequently withstand any considerable attenuation by water, long enough to permit the chalky particles to penetrate, fix, and convert it into its own substance; while other woods, less tenacious, insensibly waste, and are carried off by the insinuating liquid, together with the chalky particles, which they not only could not arrest, but prevented effectually, by a blending and interposition of their own parts, from adhering to each other.

The reasons, why he apprehends the process of the whole to have been in the manner described above, are, first, the close vicinity, and almost contact of the chalky hills, on which this bed of malm attends throughout the whole line, and no farther. Secondly, That this malm is an alkalizate body, in a degree something inferior to chalk. Thirdly, The reasons for supposing that this valley formerly has been over-run with wood. Fourthly, The disposal of the several detached pieces of malm, which lie in all manner of directions. Fifthly, The resemblance which they bear to roots, trunks, branches, twigs, &c. Sixthly, In the hollow of some of the oblong tubular pieces, which were closed at both ends, on breaking them open, he found the remains of the included wood attenuated to a mere thread, which, though extremely tender, he could plainly discover to be wood, both by its exterior appearance, and by rubbing in his hand, to try if it would colour it, as decayed wood, that has imbibed moisture, will do. Within the laminæ of several, he found a fair impression of leaves, in no small number, and with little trouble. Some pieces he found quite flat, as if the chalky laminæ had involved a chip, and the cavity consequently went off insensibly less towards each extremity. Others he found, whose cavities at the extremities were irregularly shaped, agreeable to the jagged ends of broken sticks. Some, in short, he found excavated on one side, and convex on the other, as if the laminæ had surrounded a piece of bark.

As to the microscopical discoveries; on viewing an infusion of the farina fœcundans of the lilium rubrum flore reflexo, in common water, he thought he perceived some alteration in several of these minute bodies, as if the outward shell or husk had, at a small lateral orifice, shed a long train of globules adhering to each other, and enveloped in a filmy substance. Immediately he applied some fresh farina, adapted the microscope before-hand, with the tip of a brush dropped a small globule of water on the object, and in a few seconds, he plainly perceived a rope of exceedingly small globules to be ejaculated with some force from within, and contorting itself from one side to the other, throughout the whole line, during the time of action, which does not last above a second or two, and is to be expected from a few only of these farinaceous globules.

These emitted particles are very different from the small globules of oil, with which the farina of the lily abounds; for these diffuse themselves equally on all sides, while those, on the contrary, go off in one continued train, like the ejected pulp of a roasting apple; and are involved in a filmy substance, as the eggs of some aquatic insects are.

He has since chosen the farina of a pompion to repeat this experiment on, which is not of an oily nature; and, on account of its size, may be conveniently observed with the 2d magnifier, which has the advantage of a larger field. He viewed some few of these also out of the many farinaceous globules, which were within the area of the microscope, with the same success, and yet greater pleasure: for he could plainly perceive, during the time of action, by 2 or 3 lucid specks in the centre of the globule, which continually shifted their places, an intestine commotion within the farinaceous corpuscle, and a stronger ejaculation of the emitted particles.

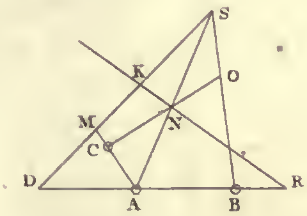
Upon opening lately the small black grains of smutty wheat, which is here distinguished from blighted corn, the latter affording nothing but a black dust, into which the whole substance of the ear is converted, he perceived a soft white fibrous substance, a small portion of which he placed on the object-plate: it seemed to consist wholly of longitudinal fibres bundled together, and without any the least sign of life or motion. He dropped a globule of water on it, to try if the parts, when separated, might be viewed more conveniently; when, to his great surprise, these imaginary fibres, as it were, instantly separated from each other, took life, moved irregularly, not with a progressive, but twisting motion; and so continued for the space of 9 or 10 hours, when he threw them away. He is satisfied they are a species of aquatic animals, and may be denominated worms, eels, or serpents, which they much resemble. He repeated the experiment several times, with the same success, and gratified others with a sight of it.

END OF THE FORTY-SECOND VOLUME OF THE ORIGINAL.

END OF VOLUME EIGHTH.

C. and R. Baldwin, Printers,
New Bridge-street, London.

Fig. 1.



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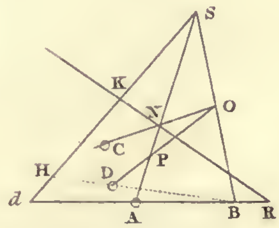

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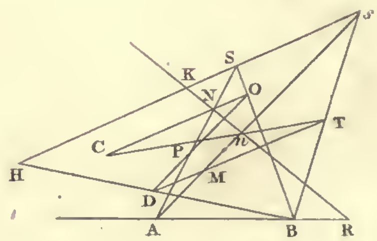

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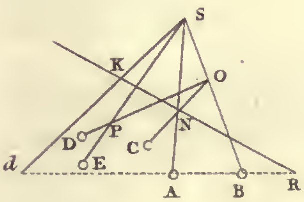

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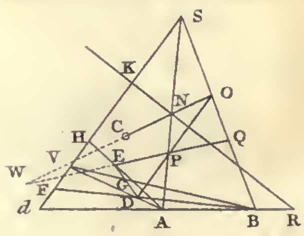

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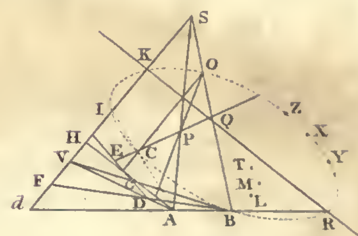

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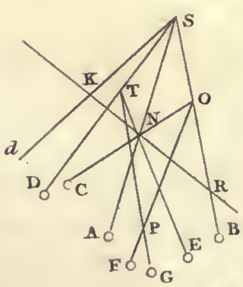

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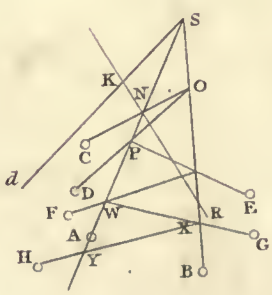

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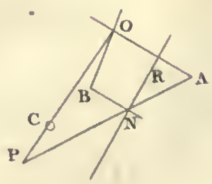

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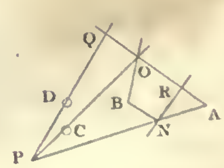


Published by C. & R. Baldwin, of New Bridge Street London, 1806.

Fig. 3.

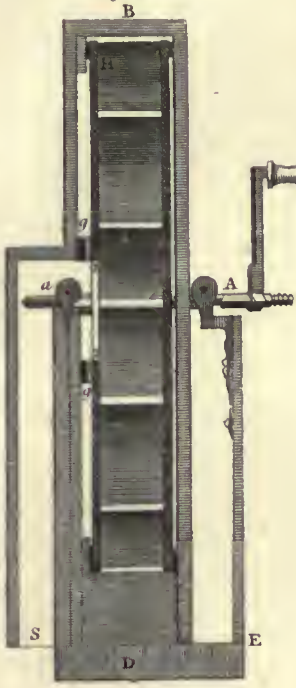
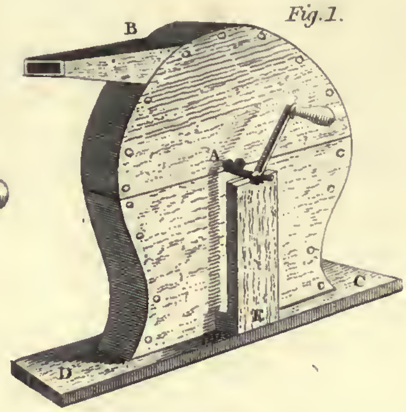


Fig. 1.



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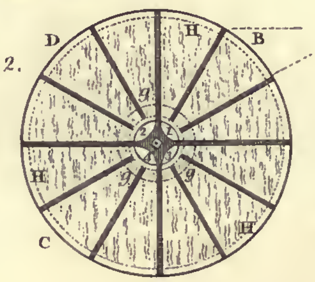

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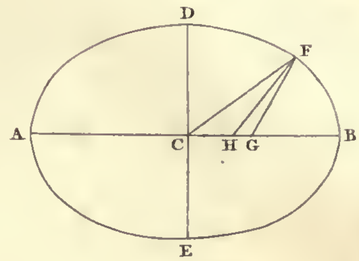
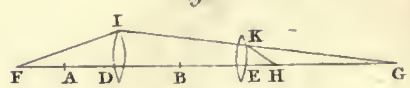


Fig 6



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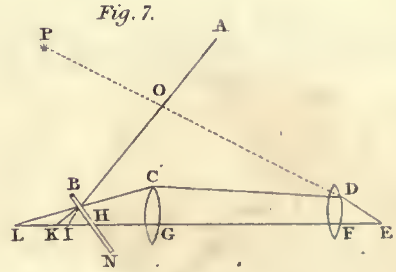

Fig. 4.

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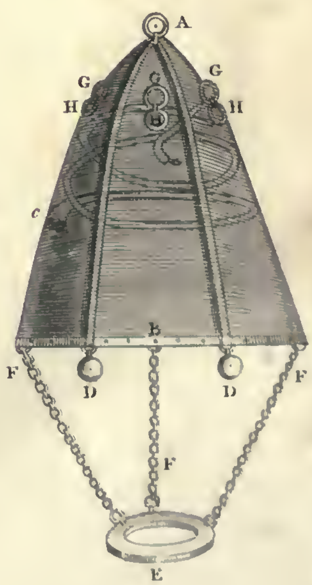

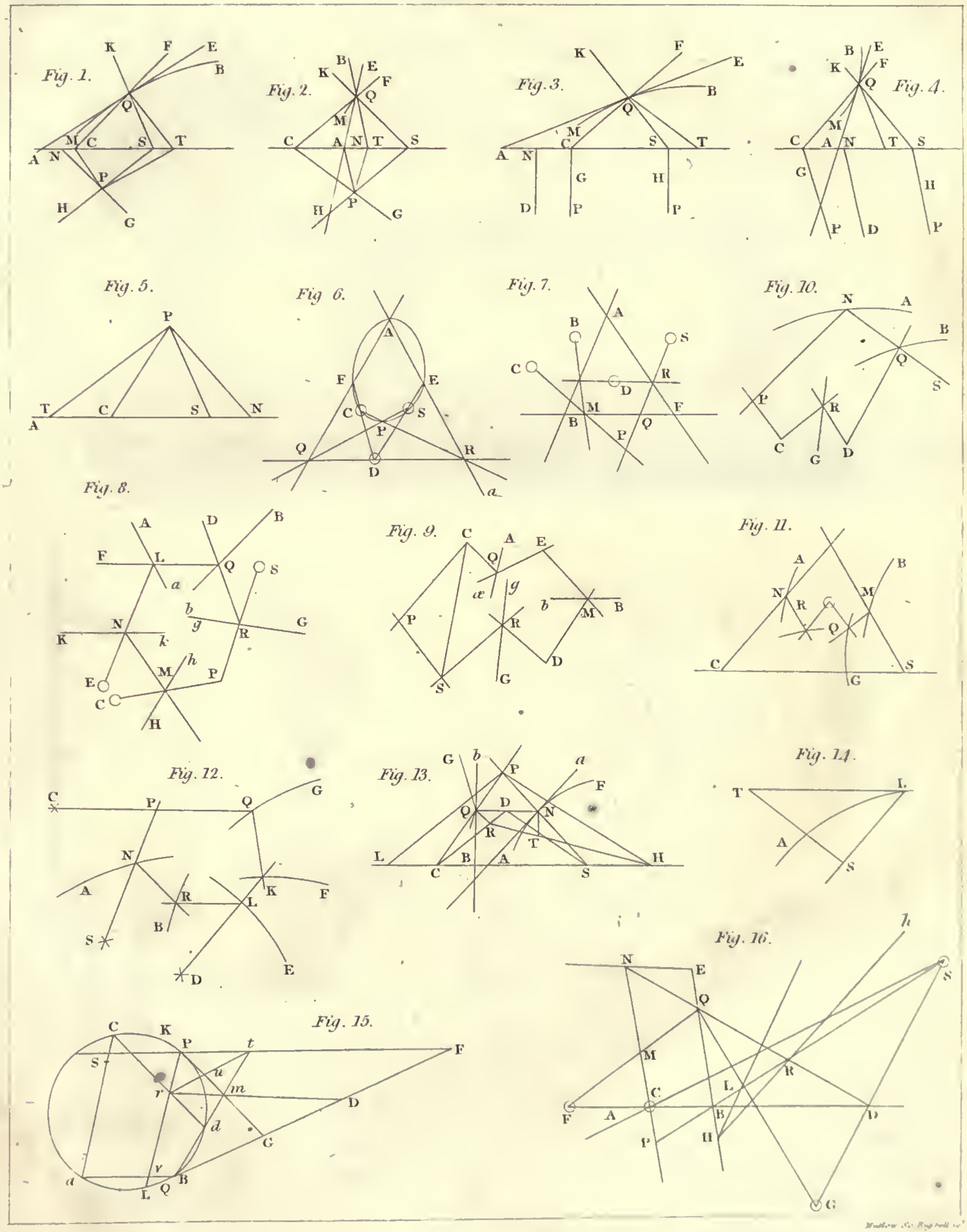

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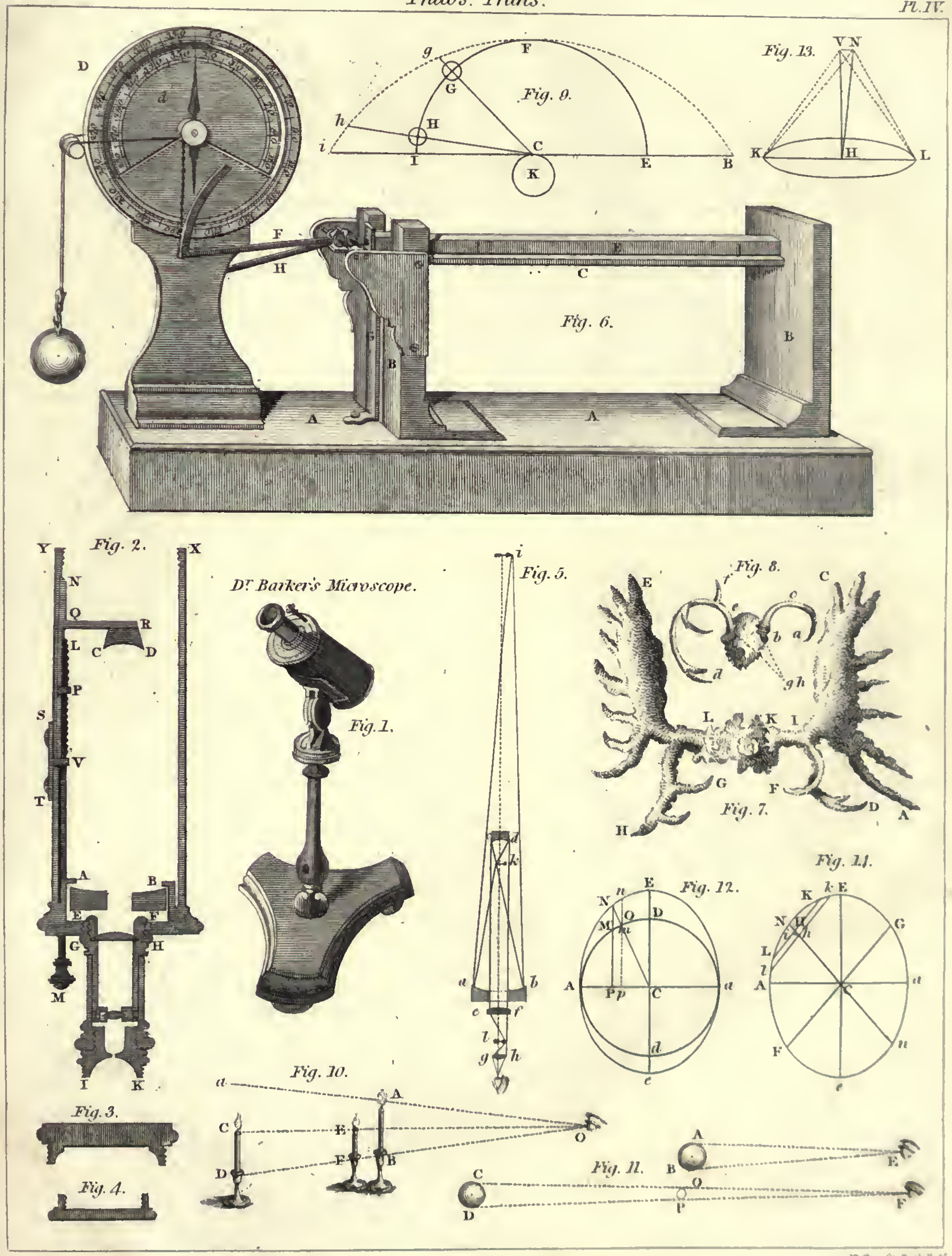

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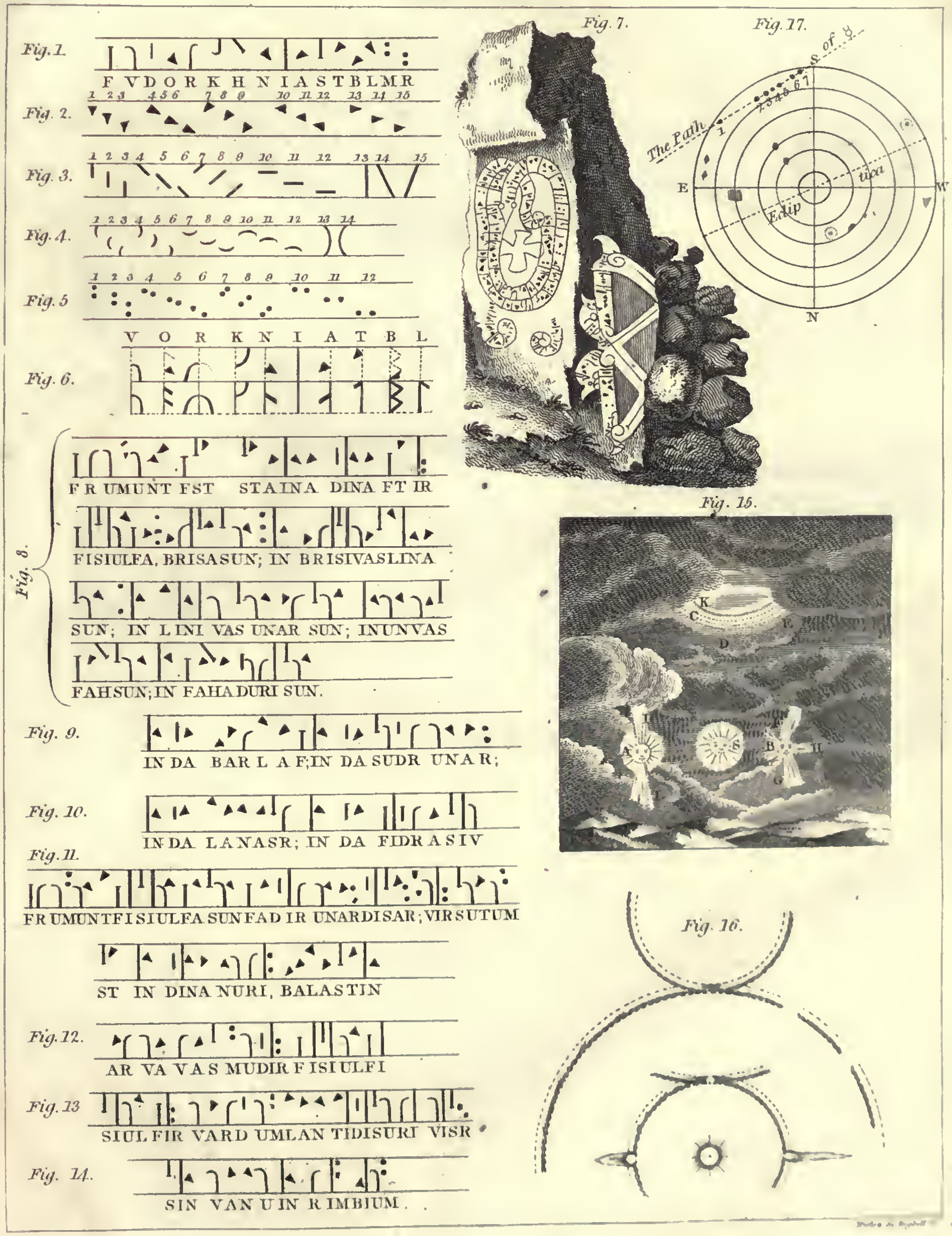

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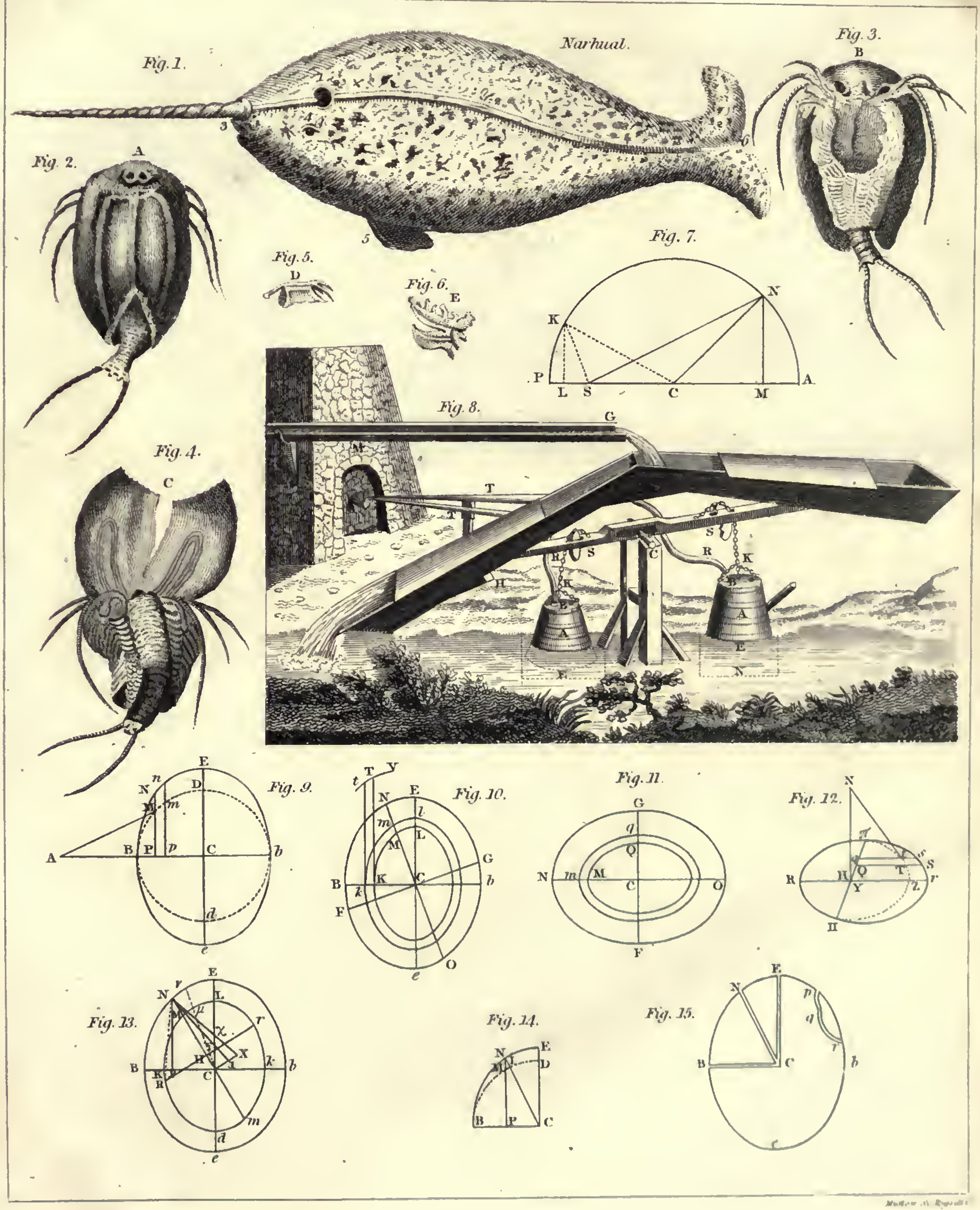

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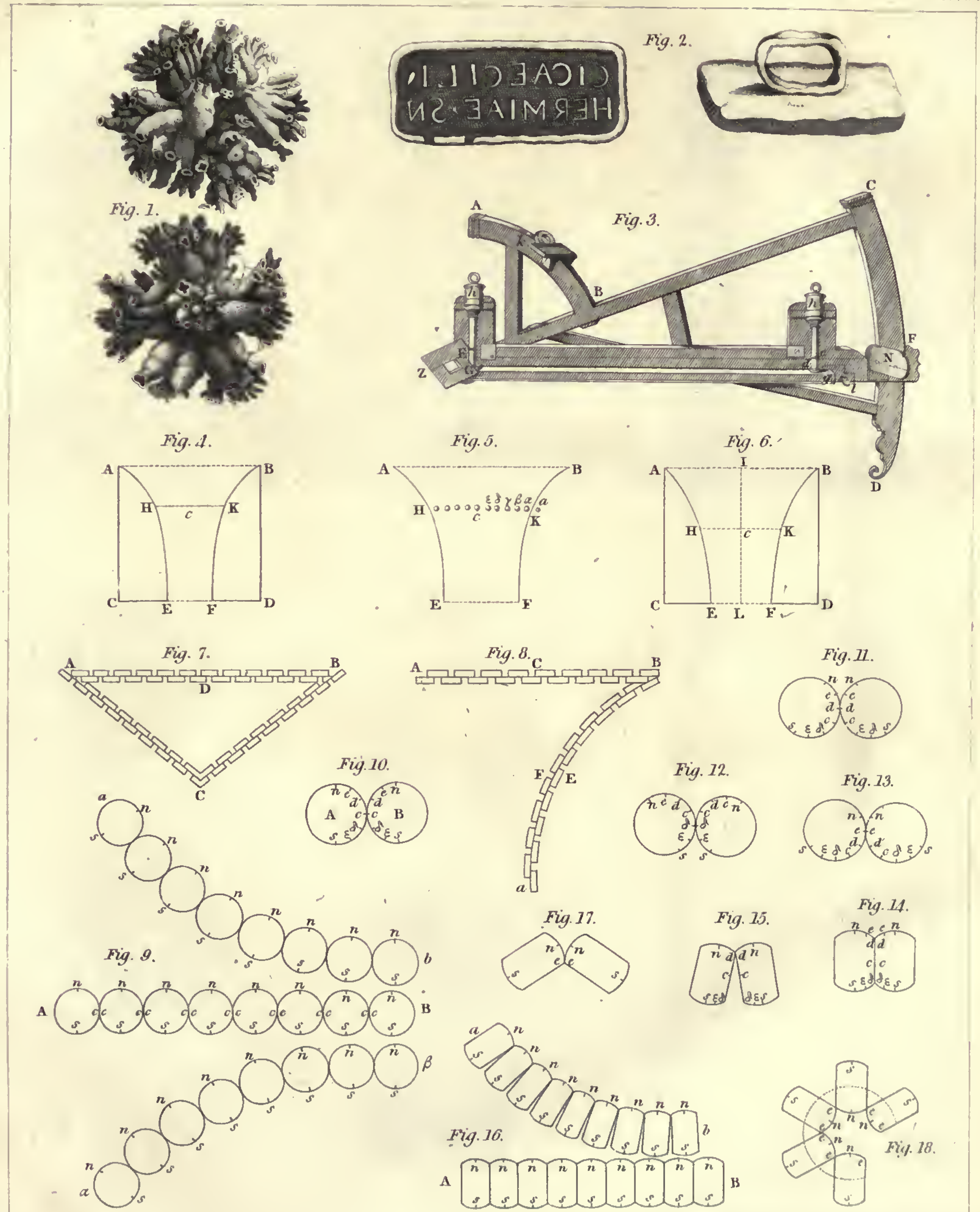

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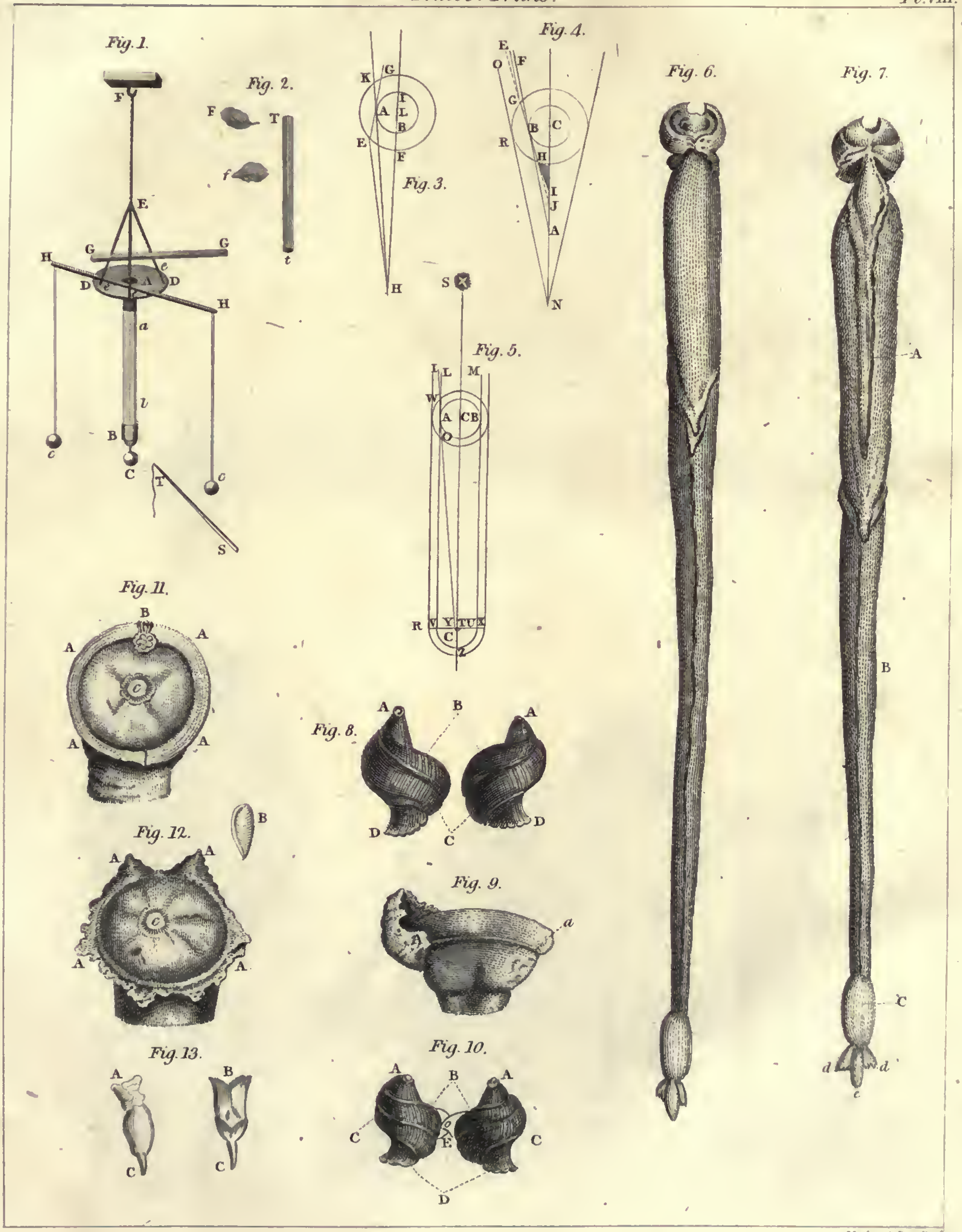

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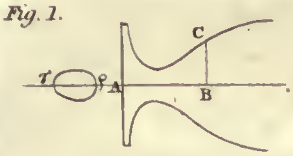

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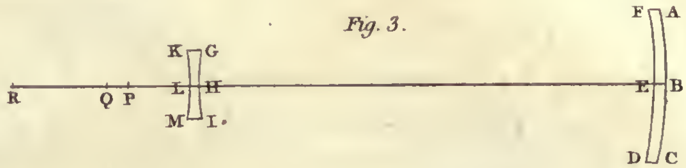

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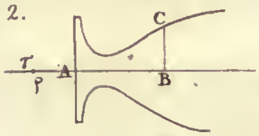

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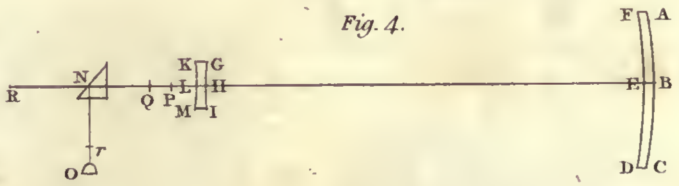

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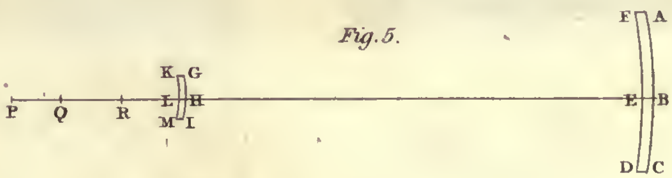
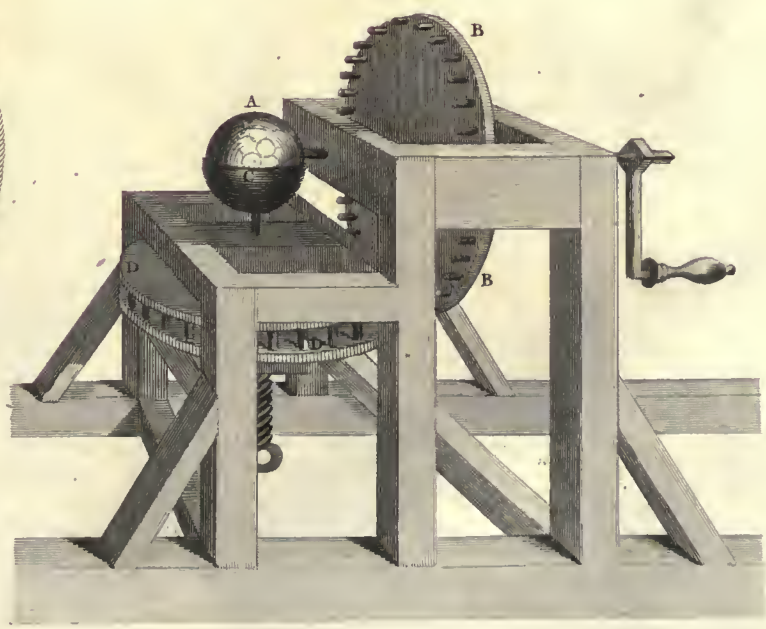


Fig. 7.

Fig. 8.



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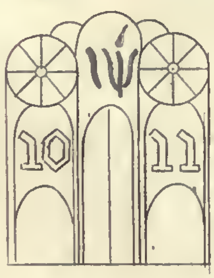

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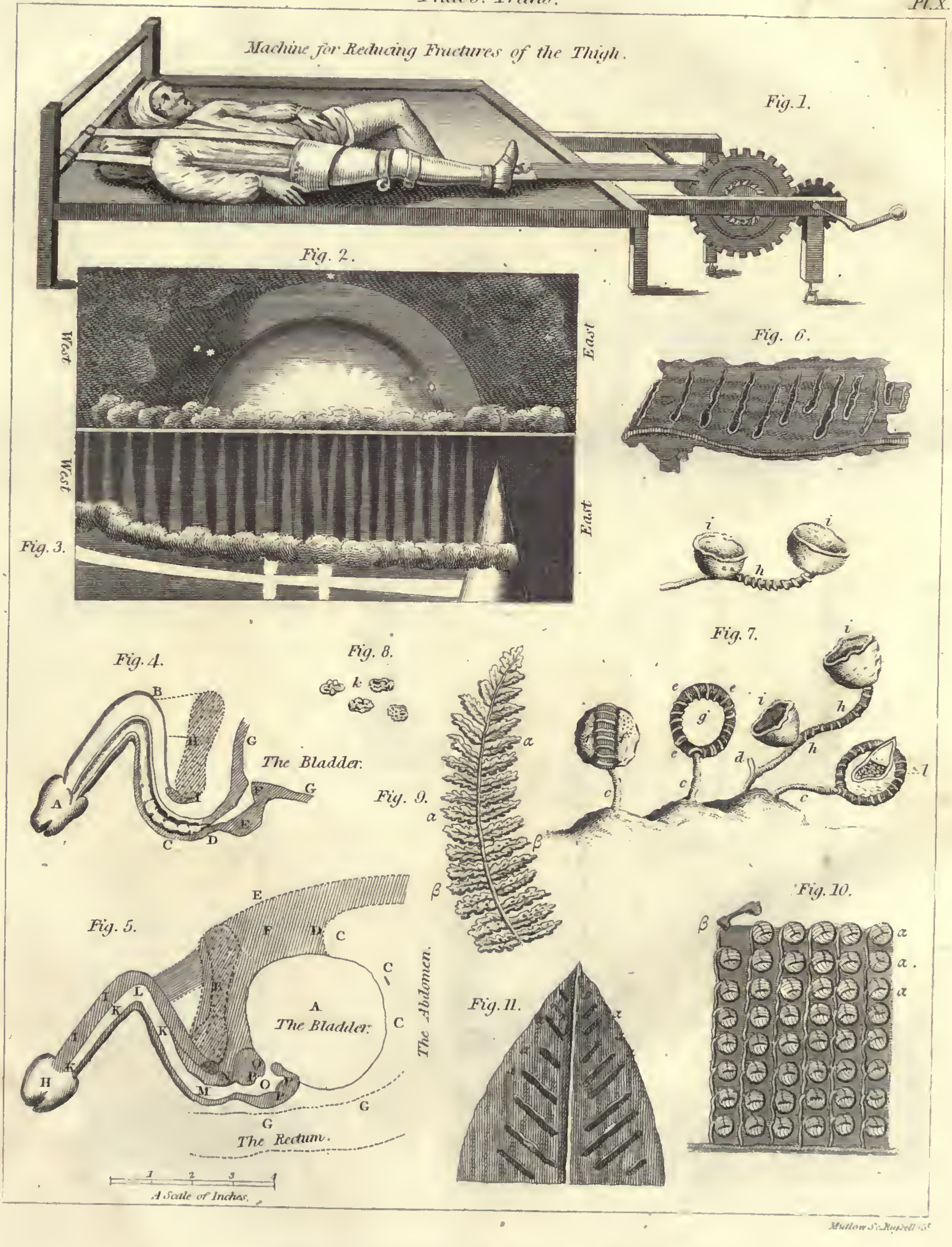

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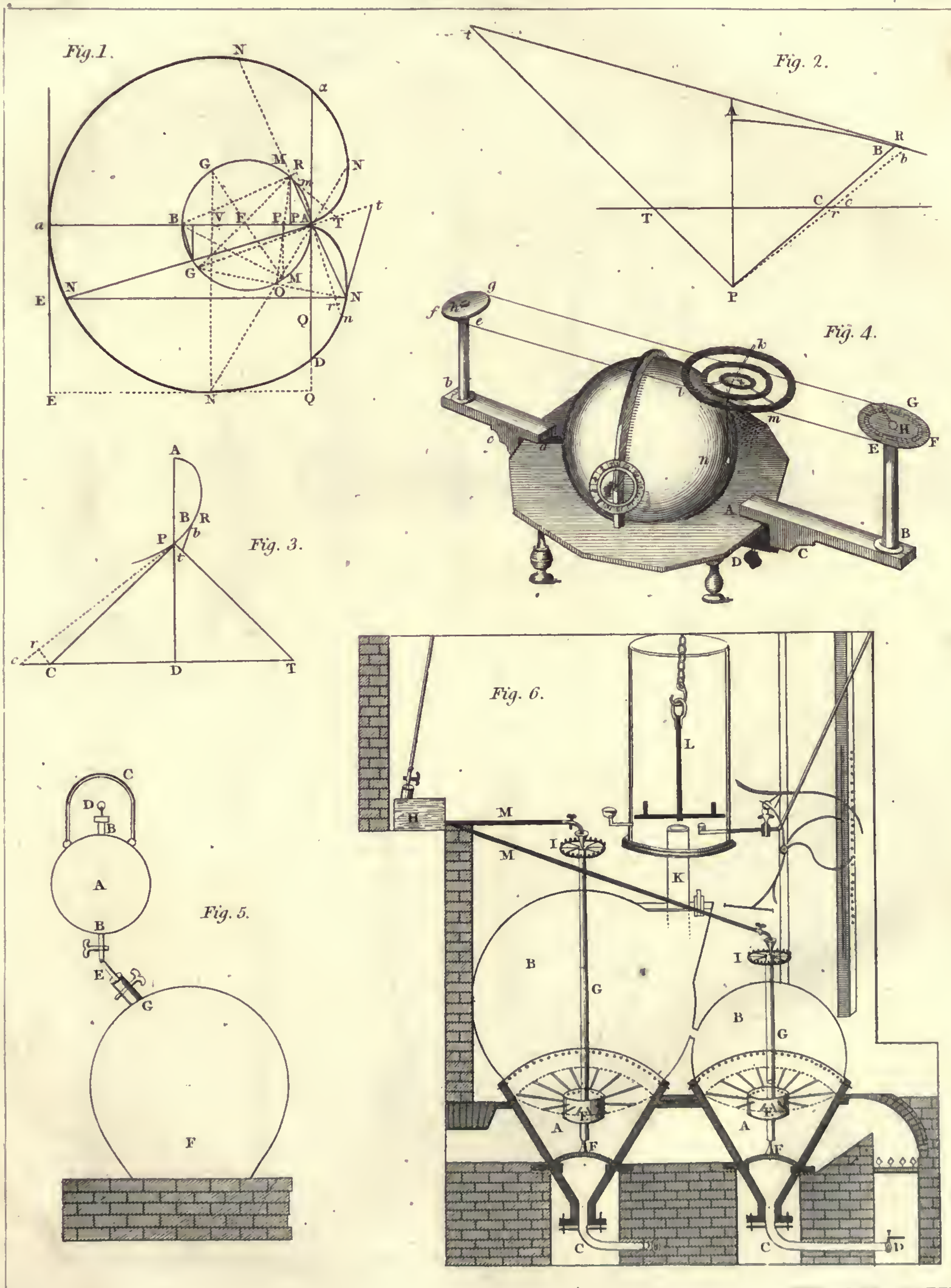

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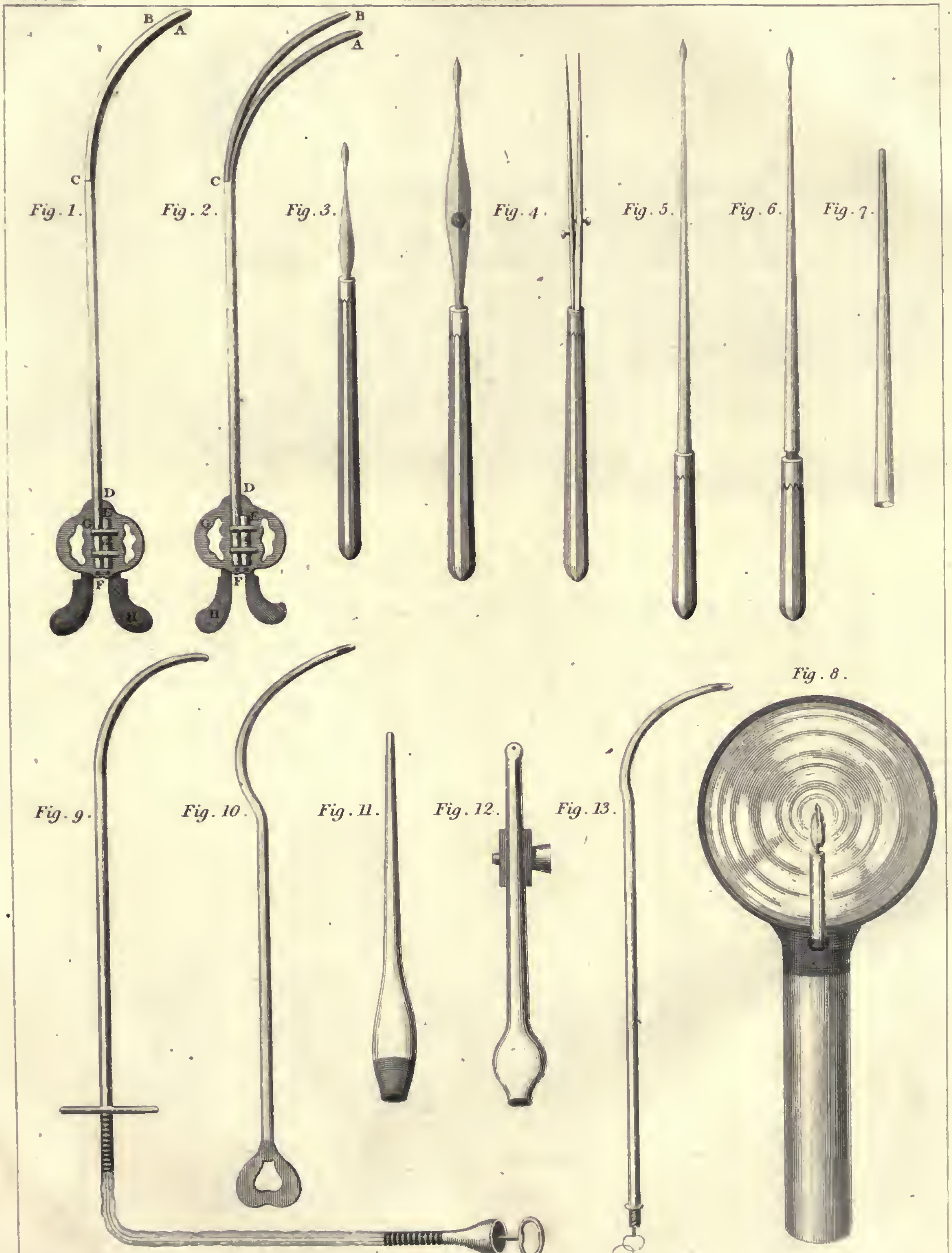

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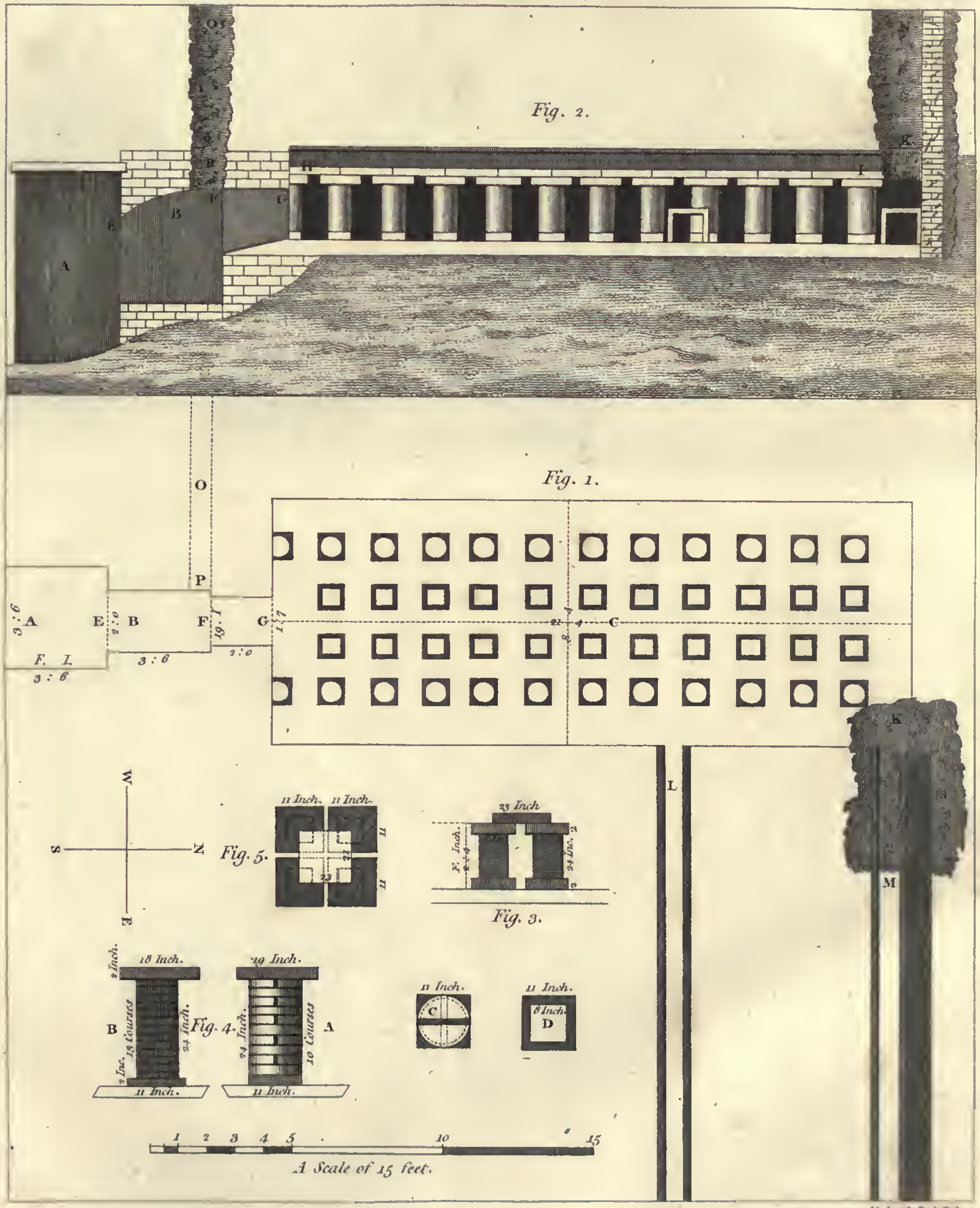

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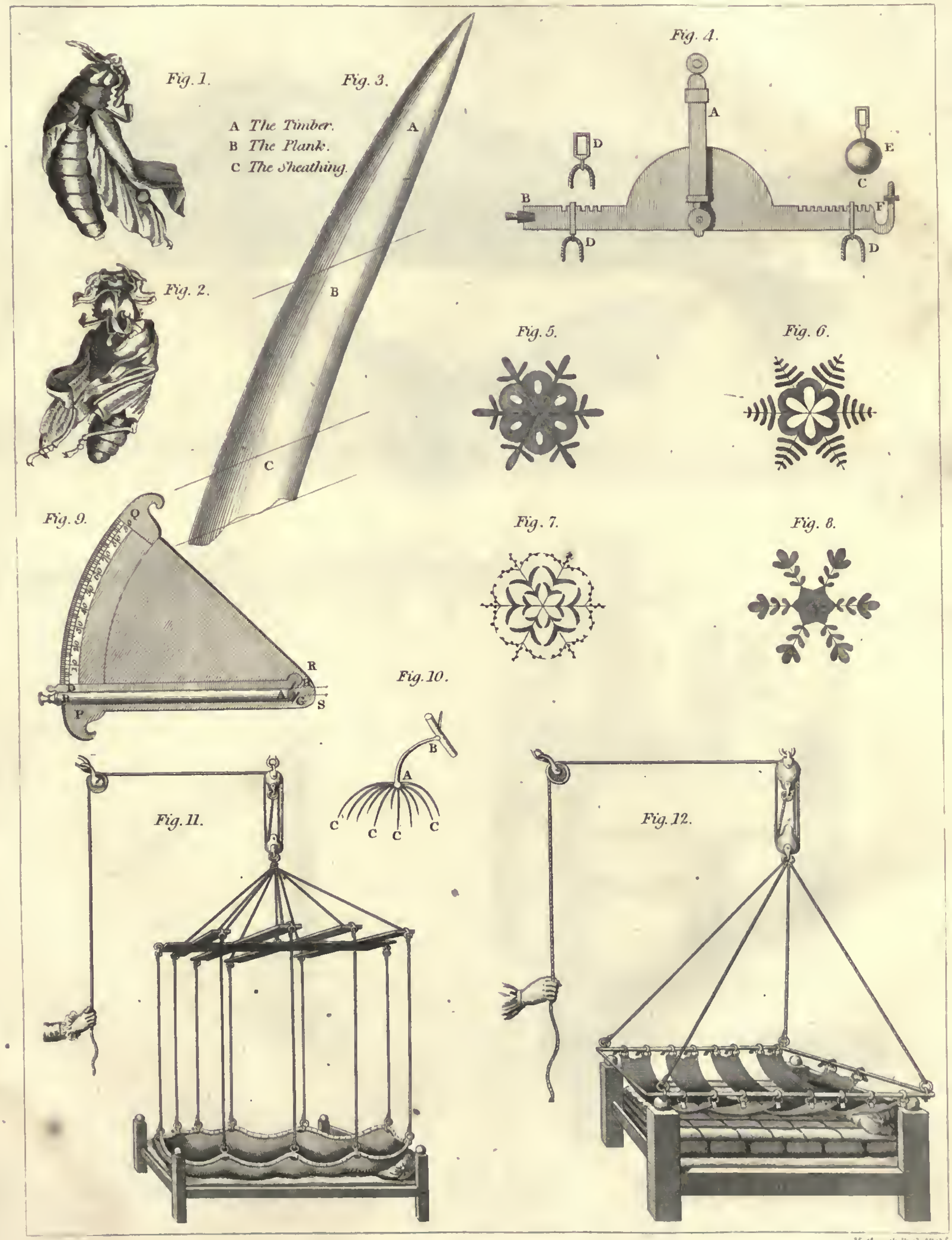

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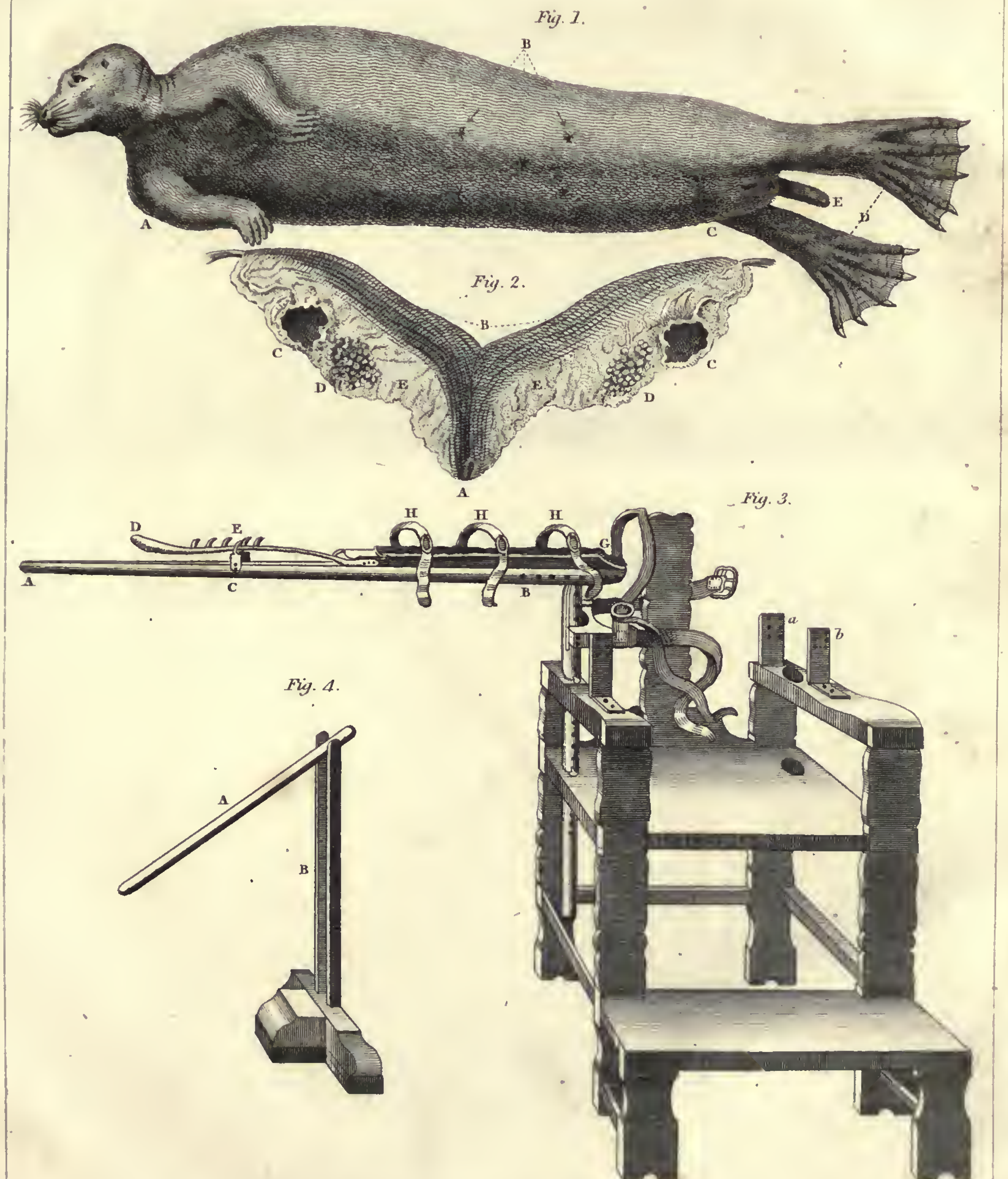

A Scale of Feet to be measured upon the fore part of the Figure.

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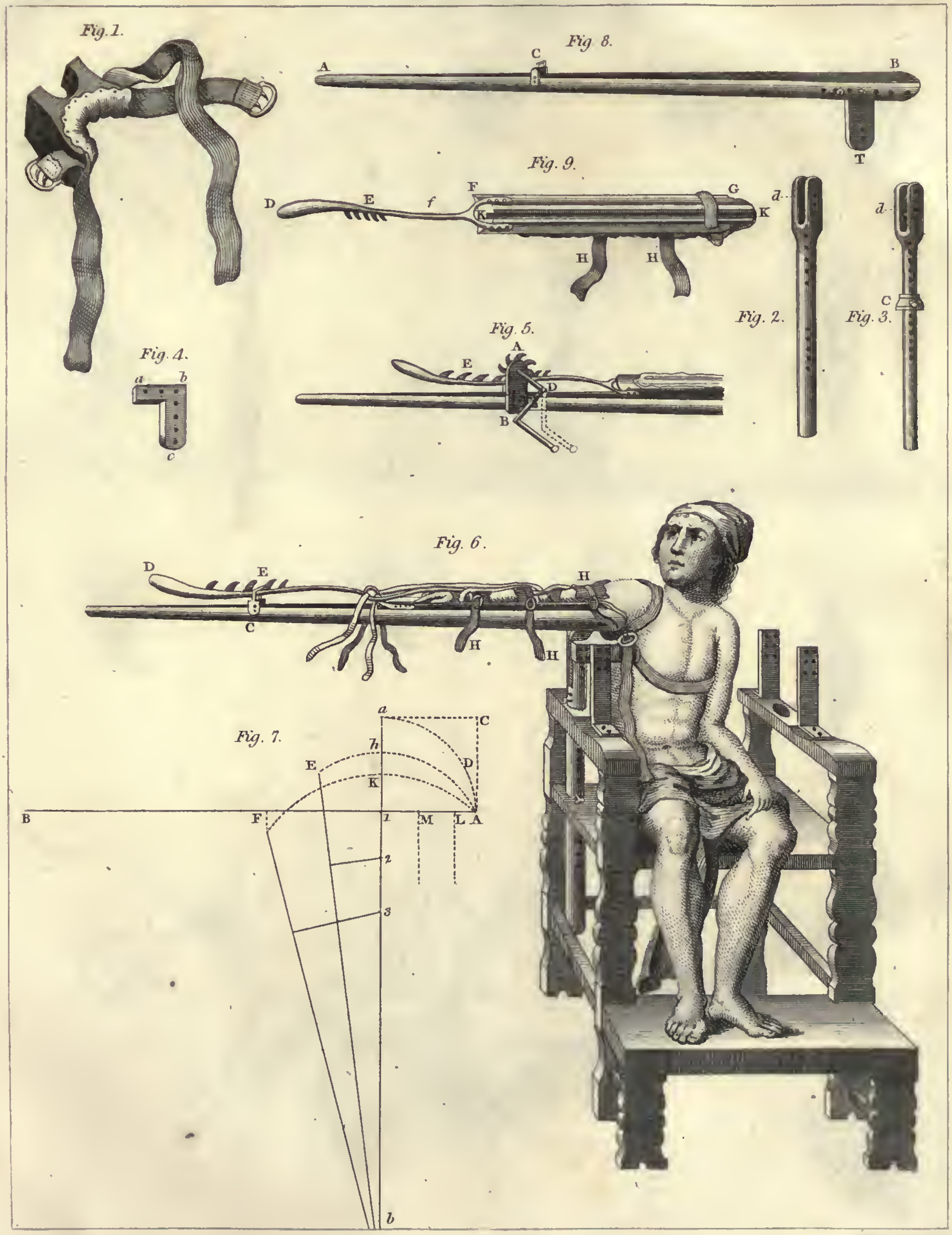

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Views of Polypi in their different States.

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Fig. 2.

Fig. 1.

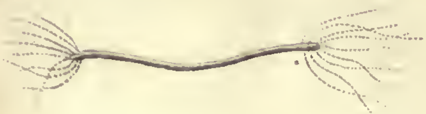

Fig. 3.

Fig. 4.

Fig. 5.

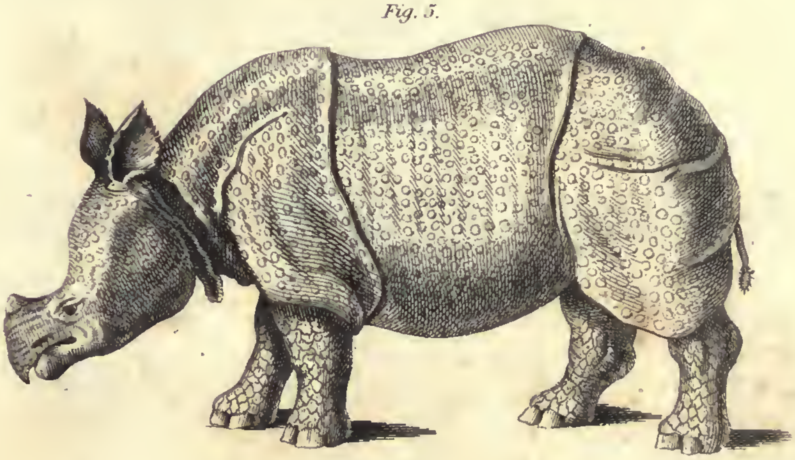

Fig. 6.

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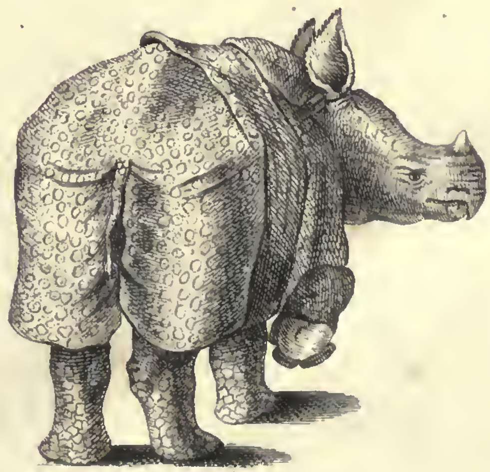

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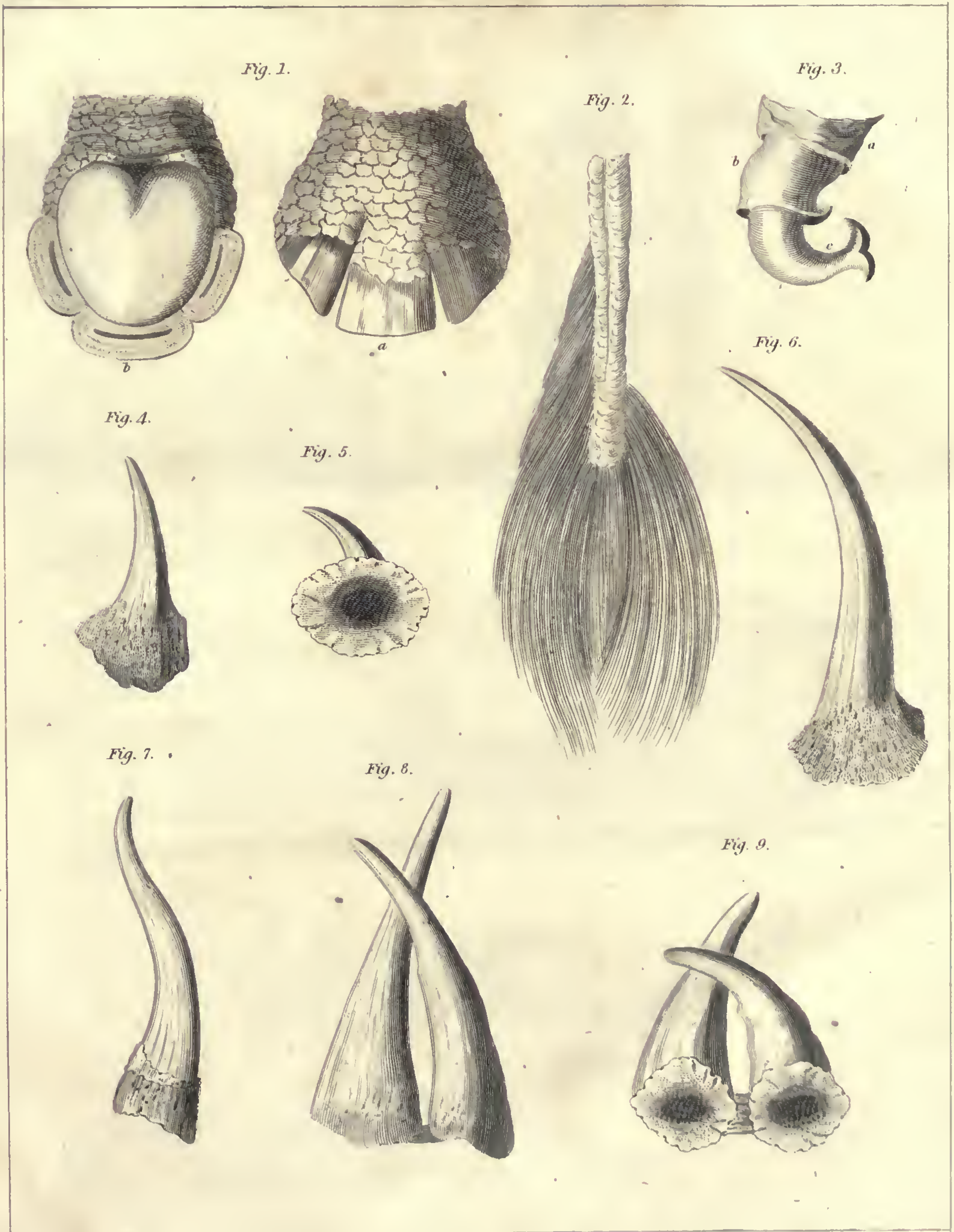

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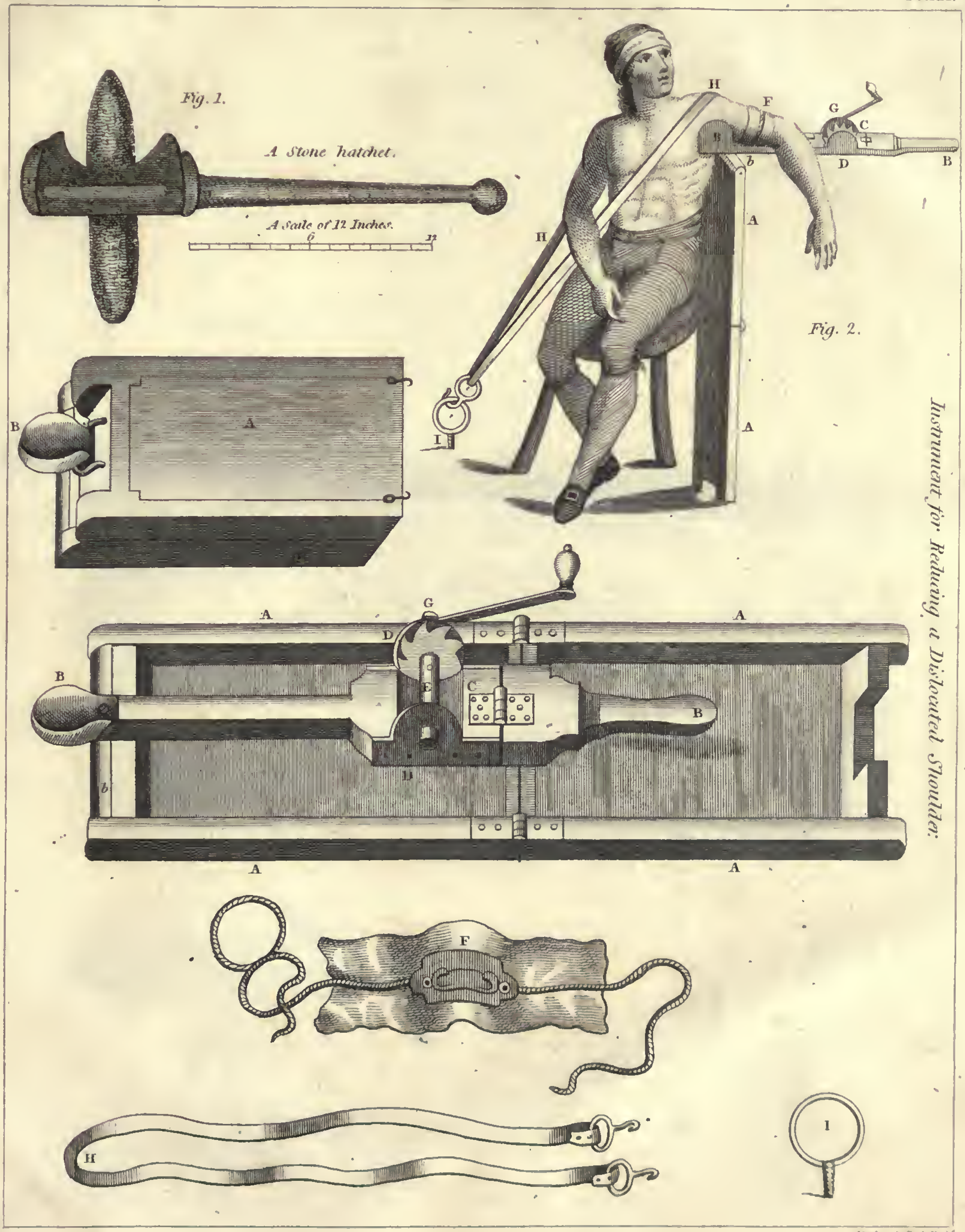

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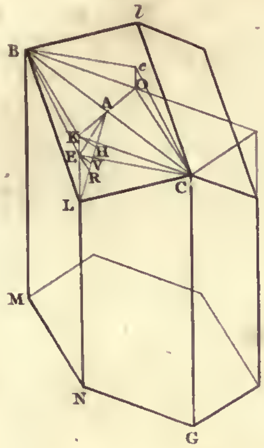
Fig. 1.



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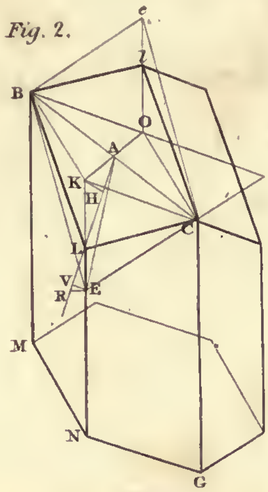

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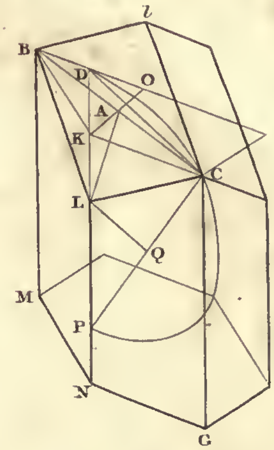

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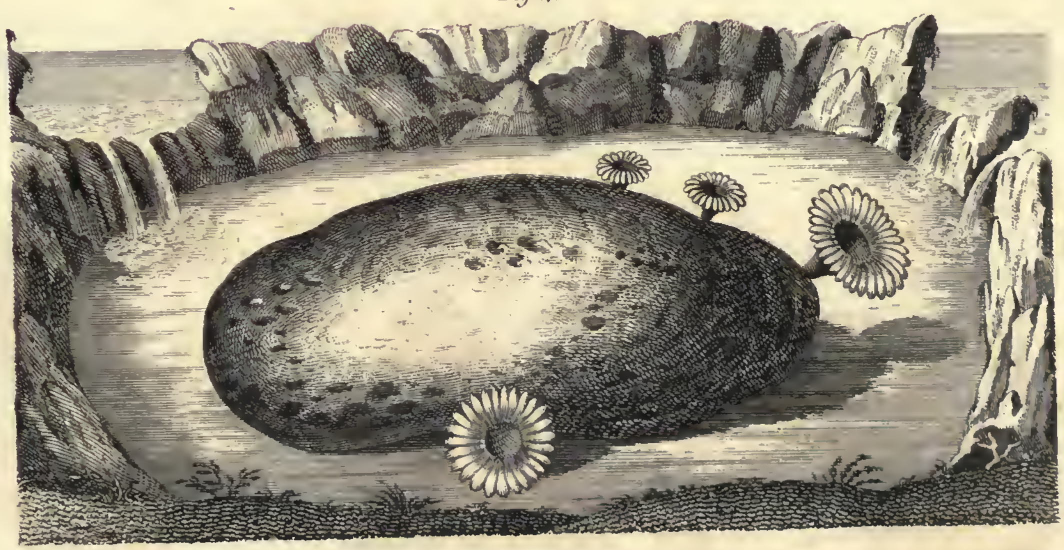

Fig. 5.

Fig. 7.

Fig. 6.

Fig. 8.

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