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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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* Erratum, p. 95, l. 10, for plate 5, read 4.

THE
PHILOSOPHICAL TRANSACTIONS
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ROYAL SOCIETY OF LONDON;
ABRIDGED.

Part of a Letter to Dr. Mead, concerning Secretions in an Animal Body. By Jos. Morland, M. D. N° 283, p. 1292.

An attempt to account for secretion upon mechanical principles.

Abstract of Part of a Letter from Dr. Bonomo to Signior Redi, containing some Observations concerning the Worms of Human Bodies. By Richard Mead, M. D.* N° 283, p. 1296.

Having frequently observed that poor women, when their children are trou-

* Richard Mead was born at Stepney near London in 1673, and after being privately educated in the house of his father, who was a nonconformist clergyman, he was sent to Holland, where he commenced his medical studies under the celebrated Pitcairn. He afterwards travelled into Italy, and took his degree of M. D. at Padua. On his return from the continent he settled in London, and soon got into practice. He was chosen physician to St. Thomas's Hospital in 1703, a Fellow of the Royal Society in 1704, and a Vice-President of that learned body in 1707; in which year also the university of Oxford honoured him with their diploma.

In 1716 he was admitted a Fellow of the College of Physicians. He attended Queen Anne in her last illness, and was appointed physician to George II. in 1727. He died in 1754, aged 81. He wrote the following works: 1. A Mechanical Account of Poisons, 1702; in which, though there are some good observations relative to the structure of the viper's teeth, the situation of the venomous bags, &c. and concerning the symptoms produced by various animal, vegetable, and mineral poisons; yet is this book too full of mechanical hypotheses concerning the spicular form and cutting action of the particles of various poisons; at the same time the curative directions are not the best that might be given, many of the proposed remedies (such for instance as the lichen caninus against hydrophobia) possessing little or no activity. 2. De Imperio Solis et Lunæ in Corp. Hum. 1704. In this work the periodical attacks and returns of certain diseases (such as mania, epilepsy, asthma, &c.) are referred to solar and lunar influences; a doctrine which is still maintained by some modern physicians. 3. A Discourse on Pestilential Contagion, 1720; written at the time when the plague raged at Marseilles, and when our government was anxious to have the best means of preventing the importation bled with the itch, with the point of a pin pull out of the scabby skin little bladders of water, and crack them like fleas upon their nails; and that the scabby slaves in the bagnio at Leghorn often practise this mutual kindness upon each other, it came into my mind to examine what these bladders might really be.

I soon found an itchy person, and asking him where he felt the greatest and most acute itching, he pointed to a great many little pustules not yet scabbed over of which picking out one with a very fine needle, and squeezing from it a thin water, I took out a very small white globule, scarcely discernible: observing this with a microscope, I found it to be a very minute animal, in shape resembling a tortoise, of a whitish colour, a little dark upon the back, with some thin and long hairs, of nimble motion, with 6 feet, a sharp head, with 2 little horns at the end of the snout, as is represented in fig. 1 and 2, pl. 1.

Not satisfied with the first discovery, I repeated the search in several itchy persons of different age, complexion, and sex, and at different seasons of the year, and in all found the same animals, but not in all the pustules.

And though, by reason of their minuteness, and colour the same with the skin, it be hard to discern these animals on the surface of the body, yet I have sometimes seen them on the joints of the fingers, in the little furrows of the cuticula, where with their sharp head they first begin to enter, and by thus

of the contagion fully pointed out. 4. De Variolis et Morbilis, (with an appendix containing a Latin translation, by three different translators, of Rhazes on the Small Pox) 1747; in which he showed himself a strenuous advocate for inoculation, then but just introduced into England. 5. Medicina Sacra, 1749; being an explanation of the diseases mentioned in the Old and New Testaments. 6. A Treatise on the Scurvy, 1749; annexed to Sutton's Account of a New Method of extracting Foul Air out of Ships; the adoption of which in his Majesty's navy was chiefly due to the exertions of Dr. Mead. 7. Monita et Præcepta Medica, 1757; a work which contains many excellent practical hints, relative to the treatment of various diseases both acute and chronical. This work was reedited in 1773, with notes, by Sir Clifton Wintringham, in 2 vols. 8vo. His collected works make a 4to. vol.

Although Dr. Mead did not make any important discoveries in anatomy or physiology, nor introduce new and improved method of treating diseases; yet is he entitled to the praise of having written many useful and valuable works, calculated to promote the knowledge of medicine and natural history. He was critically and profoundly conversant with the ancient languages, and his Latinity is distinguished for correctness, elegance, and perspicuity. His library was stored with the rarest and most esteemed works both ancient and modern, and was always accessible, (as well as his gallery of paintings and cabinet of antiquities) to men of letters and science, whether natives or foreigners. His liberality in this and other respects, (see the life of Friend, p. 423 of vol. iv. of these Abridgments) joined to the celebrity he had acquired by his various writings, procured him the most flattering attention from all quarters, and particularly from illustrious travellers who visited this country; and although a well-educated British physician had ever been regarded as a most respectable character, yet in the instance of Dr. Mead that character seemed to have attained to its highest degree of dignity and lustre.

gnawing and working in with their body they cause a most troublesome itching, till they are got quite under the cuticula, and then it is easy to see how they make their way from place to place by their biting and eating; a single one sometimes making several pustules, of which I have often found 2 or 3 together, and for the most part very near each other.

With great earnestness I examined whether these animalcules laid eggs, and after many inquiries, at last by good fortune, while I was drawing the figure of one of them by a microscope, from the hinder part I saw drop a very small and scarcely visible white egg, almost transparent and oblong, like the seed of a pine apple, as represented in fig. 3 and 4. I often fonnd these eggs afterwards, from which, no doubt, these creatures are generated, as all others are, that is from a male and female, though I have not yet been able by any difference of figure to distinguish their sex.*

From this discovery it may be no difficult matter to give a more rational account of the itch, than authors have hitherto delivered us. It being very probable, that this contagious disease owes its origin neither to the melancholy humour of Galen, nor the corrosive acid of Sylvius, nor the particular ferment of Van Helmont, nor the irritating salts in the serum or lympha of the moderns, bnt rather to the continual biting of these animalcules in the skin, by means of which some portion of the serum oozing out through the small apertures of the cutis, little watery bladders are made, within which the insects continuing to gnaw, the infected are forced to scratch, and by scratching increase the mischief, and thus renew the troublesome work, breaking not only the little pustules, but the skin too, and some little blood vessels, and so making scabs, crusty sores, and such like foul symptoms.

From hence we come to understand how the itch proves to be a distemper so very catching, since these animals by a simple contact can easily pass from one body to another, their motion being wonderfully swift, and crawling, as well on the surface of the body as under the cuticula, being very apt to stick to every thing that touches them, and a very few of them being once lodged, they multiply apace by the eggs which they lay. Nor is it any wonder that this infection is propagated by means of sheets, towels, handkerchiefs, gloves, &c. used by itchy persons, it being easy enough for some of these creepers to be lodged in such things as these; and indeed, I have observed that they will live out of the body for 2 or 3 days.

Nor in the last place shall we be at a loss to know the reason of the cure of this malady by lixivial washes, baths, and ointments made up with salts, sul-

* The animalcules here described belong to the Linnæan genus acarus.

phurs, vitriols, mercuries, simple precipitate or sublimate, and such sort of corrosive and penetrating medicines. These being infallibly powerful to kill the vermin lodged in the cavities of the skin, which scratching will never do, partly by reason of their hardness, and partly because they are so minute as scarcely to be found by the nails.

Neither do inward medicines perform any real service in this case, it being always necessary, after a tedious use of these, to have recourse to those external ones already mentioned. And if in practice we often experience that this disease, when we think it quite cured by unction, yet in a short time returns again; this is not surprising, since though the ointment may have killed all the living creatures, yet it may not probably have destroyed all their eggs, laid as it were in the nests of the skin, from which they may afterwards breed again and renew the distemper. And on this account, it is very advisable, after the cure is once performed, still to continue the anointing for a day or two more, which it is the easier to do, because these liniments may be made agreeable enough, and of a good smell, as particularly is that compounded of the ointment of orange flowers or roses, and a small quantity of red præcipitate.

An Account of the successful Excision of a Portion of a Dog's Intestine. By Mr. John Shipton, Surgeon. Abridged from the Latin. N° 283, p. 1299.

After remarking that wounds of the intestines have always been considered as mortal (in the human subject) by the best surgical writers, both ancient* and modern,† with the exception of Barbette;‡ Mr. Shipton proceeds to the relation of an experiment which shows that such wounds do not always prove fatal in the canine race. In these experiments he was assisted by Mr. Pleahill and Mr. Dobyns.

Having tied down a dog in the usual manner, and made a large incision into the animal's abdomen, they drew out the nearest portion of the ileum; and after securing the mesaraic vessels by ligatures, they snipped the intestine across in two places with scissars, and cut out a considerable portion (portionem duos circiter digitos longam); they then sewed up the wounded gut with the glover's suture, and the wound of the abdomen with the interrupted suture, after which they applied a plaster and bandage. When the dog was set at liberty he tottered and reeled about in attempting to walk, and appeared to be extremely weakened; the same night he vomited twice. After some days it became necessary to

* Hippocrat. Aphorism, 18 and 24 lib. 6. Cels. lib. 5. cap 26.
† Fabr. ab Aquapendente de Operat. Chirurg. cap. 55.
‡ Chirurg. part ii. lib. 2. cap. 11.

tighten the sutures, which had become loose. The dressing and bandages were afterwards laid aside, and in the course of three weeks the wound was healed merely by the dog's licking it.

After keeping the dog for some weeks, during which time he was perfectly healthy and lively, he was hanged and opened; when the part of the intestine where the suture had been made was found in the left hypochondrium (at a considerable distance from the external wound which was made in the right hypogastrium) firmly united to the peritoneum, see fig. f, and enlarged into the form of a sac, DDD. The omentum, G, also adhered to it, and there were moreover adhesions of the intestines, ** ee, in several places. In order to examine the cicatrix on the inside, they cut open the intestine longitudinally, when it appeared that the lips of the wound had formed adhesions on one side to the peritonæum, and on the other to the adjacent intestines; so that the external coats of these last constituted the internal surface or side of the gut that was wounded, forming a continuation of the intestinal tube, and thus affording a convenient passage for the food.

Mr. S. then proceeds to notice similar experiments made on brutes by other persons, viz. by Brunner, (see the preface to his Experimenta Nova circa Pancreas), who made a wound $1\frac{1}{2}$ inch long in the small intestine of a dog, after which the animal, though with difficulty, recovered; and by Mr. Cowper, see N° 208 of these Transactions: but in neither of these instances was a portion of the gut cut away. Lastly, Mr. S. refers, as connected with the subject, to Dr. Wallis's account of a horse, that staked himself in the stomach in leaping over a fence, inserted in N° 219 of the same Transactions.

Although there is a great difference (Mr. S. remarks) between the condition of the intestines in man and brutes, yet he infers that the above-mentioned successful experiments upon dogs, may serve as an encouragement to surgeons to sew up wounds of the intestines, especially of the larger intestines, whenever those parts become wounded in the human subject.

The figure which accompanies this account was drawn by Mr. Cowper.

Explanation of the Figure.—Pl. 1, fig. 5, AAA the upper part of the ileum, towards the stomach; BBB, the lower part; C the cicatrix of the wounded gut as it appeared internally; DDD the lips of the divided intestine; E the upper orifice of the intestine; F the lower orifice; ee the external coats or surfaces of the adjacent intestines, supplying the place of a portion of the gut which was here wanting; f a portion of the peritonæum united to the intestine; G the omentum likewise united to the intestine; *** marks showing where the wounded gut had adhered to the other; H the trunk of the aorta; I the cæliac artery; g the right gastric artery: h the right gastro-epiploic artery; i the hepatic artery; k the pyloric artery; l, the larger gastric artery; mm the splenic artery; K the upper mesenteric artery; L the phrenic arteries; o the trunk of the vena portæ; PP the arteries and veins distributed over the mesentery.

Concerning Green Weeds growing in Water, and some Animalcula found about them. By M. Lewenhoeck. N° 283, p. 1304.

I have often heard the common people say, that that green stuff or weeds, observed to drive upon the water, spring out of the ground from under the water. But as often as I have observed the said green weeds, I have always found that they are produced from the seeds of the same kind, as all other trees and plants are. I have always observed too that this green matter does not grow in deep waters, though they are small and still; but that it abounds in wide and shallow waters, and especially moats and ditches; and that the wind does it no harm. I have also found that the green weeds in those ditches which have but little water in them, are very small in comparison of those in larger waters, and near the sides of the banks, where they are shallow, and very little motion in them.

Delft-haven belongs to our town, and lies about 2 hours distance from it; from that town, by means of a sluice, the water of the river Maes, with the flood in summer time, is brought into our town, and it is as clear as if the river itself ran through the town. With this water comes in also a little of this green stuff, but so little, that we were half an hour a fishing up 30 of those weeds, and putting them into an earthen pot, together with a large quantity of their own water, for we took them up as deep as we could, that we might not hurt their roots.

I took out several of these weeds from the pot one by one, with a needle very nicely, and put them into a glass tube of a finger's breadth, filled with water, and also in a lesser tube, and caused the roots of the weeds to subside leisurely; then viewing them with my microscope, I observed a great many and different kinds of animalcula, of which two sorts had long tails, by which they seemed to be fastened to the roots of the weeds. These animalcula* were shaped like a bell, and they moved the round cavity of their bodies in a manner that they put the small parts of the water into such a motion, that I could not see the instruments they used to produce it.

And though I saw 20 of these animalcula together, gently moving their long tails and outstretched bodies, they contracted their bodies and tails in an instant,

* *Vorticella Convallaria.* Linn.

and then gently extended them again; and this kind of motion they continued a great while.

Pl. 1, fig. 6,* ABC represent one of the green weeds, of a common size, as it lay upon the water, and appeared to the naked eye. Fig. 7, DEFG show the same weed as it appeared in the glass tube filled with water, with its roots extended; fig. 8, HIKLMNOPQR represent a small part of the said root, as it appeared in the microscope, through the whole length of which were to be seen its vessels with their divisions; which roots I imagine were of no further use, and in a manner withered; they were also overgrown with a great many particular long particles, and mostly with little figures like flowers, as are represented in the fig. between K and L. The animalcula beforementioned, are to be seen like little bells, at LST and NVW; I saw above a hundred of these animalcula, with their tails fastened to the root, and living, between HIKLM, but other roots had none of them.

In several of these roots I observed one, and some few times two sheaths, or cases fastened in them, of several sizes, the largest is represented by RXZY. Out of the same sheath appeared a little animal,† the fore-part of whose body was roundish, as in XZY; and presently from the same rotundity proceeded 2 little wheels, that had a swift gyration, always one and the same way, as in abc; these small wheels were as thick set with teeth, as the wheel of a watch; and when these animalcula had for some time exerted their circular motion, they drew their wheels into their body, and their body wholly into their sheaths, and then soon after thrust them out again with the aforesaid motion; another while they remained as it were shut up in their shells; and though I observed the same wheels in other animalcula also, yet their bodies differed from each other, and their sheaths were of a darkish colour, so that I could not easily perceive the animalcula; and they seemed to be composed of globules.

I observed also other sheaths, that were several degrees smaller than those beforementioned, and as transparent as glass, so that one might plainly see the animalcula lying within them.

Fig. 8, Pdef represent the sheath with the little worm Pdf in it: in the same figure, ogh show a sheath with half the body of the same animalcule gh protruded out of it; and in which, by reason of its exceeding smallness, the wheels could only be seen now and then, and that only when the body was extended, which would soon be compressed or shrunk up; and about the middle of the body of one of these, which I conceived to be the lower part of its belly,

* This plant is the *lemna polyrhiza*. Lin. or greater duckweed. † A species of vorticella.

there was another of the same kind, but smaller, the tail of which seemed to be fastened to the other.*

Fig. 9. ABCDEFGH represent one of these, about double the natural size, whilst it was in the water, and fastened to the root of the green weeds; A is the tail with which it fastens itself; CDE represent 8 horns, though others that were smaller had but 6; it is drawn as stretched out at its whole length; but when contracted, it was not the 4th part so long. The horns had a very curious appearance; a small part of one is represented at KLM, fig. 10.

In fig. 9, BH show an animalculum coming out of the body of the larger, which phenomenon at first I thought might be a young animalculum fastened by chance to an old one; but observing it more narrowly, I saw it was a partus, for I could now see, that whereas this animalculum when I first discovered it, had only 4 very small horns, but 16 hours after it was grown much larger, both in horns and body, and 4 hours after that it forsook its mother's belly.

In the discovery of the said young animalculum, I had observed on the other side of the body of the largest animalculum, a small round knob of seed, which from time to time, and in a few hours, grew still larger, as in fig. 9, between G and I, and at last grew pointed; and in 13 or 14 hours it became so large, that you might see 2 horns upon it. In 24 hours it had acquired 4 horns, one of which was small, the 2d larger, the 2 others very large; and these 2 last were more strongly protruded and contracted than the smaller. Three hours after, this little animalculum was got clear of its mother.

I endeavoured to pursue my discovery of the generation of these creatures, and in order thereto wiped off the green weeds from the body, the better to make my observations; when the animalculum was found not only dead, but his horns and part of the body quite wasted.

Another animalculum, that had brought forth two young ones, had her body laden with another sort of animalcula, whose shape was flat below, and round above; which I have observed in most waters; and this last sort were above a thousand times less than the others, on which they crawled, and hindered their motion; but a much larger animalculum, whose body was almost round, tormented one of the aforesaid animalcula, not only by running upon its body, but by clinging so fast to one of its horns, that whatever effort the other made to get rid of it, she could not shake it off; and at last I found she had lost one of her horns in the scuffle.

* This appears to have been the *hydra viridis*, or common green polype, of which Leuwenhoeck was the first observer.

That which seemed very surprising was, that the said animalcula would sometimes extend their horns to so great a length, that through the microscope they seemed several fathoms long; as represented in fig. 11, NOP, where also are shown the knotty protuberances, as in fig. 10, KLM. These protuberances seem to be composed of 7 round globules placed together, viz. One in the middle, which was more prominent than the rest, and the others about it like a rose.

I observed in the green weeds abundance of strange animalcula, some of which feed upon the same green stuff, and to others it serves instead of skulking holes, to hide themselves from the fish, which would otherwise devour them.

To return to the generation of the abovementioned green weeds; I have several times observed, that most of the roots are thickest at the extremity, and provided with a great many parts that look like flowers to the eye, with long pipes in them, which I take to be a gummy excrementitious matter, which by overspreading the whole root, causes it to die or wither.

I have often dissected some of these small weeds, which had never yet produced any others, and have taken young weeds out of them, which were so very small, that they escaped my naked eye.

Let us suppose that ABC, fig. 6, which we call a green weed, is composed of three entire weeds, of which that part marked A, we will call the mother plant, from whence proceed several roots; first the plant B is immediately produced by the said mother-plant, and joined to it by a fibre, which, as it grows, conveys nourishment to it, till it be provided with roots of its own; before the weed B is arrived to its full growth, A has brought forth another, marked C, which also has no root: now if we should dissect one of those weeds, even though they were ever so small, we shall find young embryo weeds within them. I opened one of these immature weeds, and took out such an embryo as is before described; and placing it before the microscope, it was immediately drawn, before the moisture should exhale; for if that should happen, the vessels through which it receives its increase, could not be easily described.

Fig. 12 represents a weed which I had taken out of another of the size of C, in fig. 6: this weed received its nourishment by that part marked ST. Between ST inwards, you may observe five small particles, which I imagine to be also young weeds; and that part too that lies between ST outward, is a young weed, which had as round and as entire a shape as QRST itself. So that it hence appears, that all these green weeds proceed from a seminal matter, as well as other plants do.

That I might yet more fully satisfy myself, I caused some of these weeds to be taken out of a ditch, which was full of these weeds; but the largest of them were much smaller than the beforementioned, though their leaves were much thicker. Having examined them, I found, that although they were arrived to their full growth, they had but two roots, which were longer than the roots of the first weed; and when I viewed the smaller weeds, I could perceive they had a great many, but very short roots. While observing the last weed, I saw a great number of animalcula swimming in circles through the water, and so thick together, that even to the naked eye they seemed like a cloud; I never saw any of these animalcula in other waters; and the next day they were all gone. There was a great many sorts of these animalcula, and each had their different motion, and all so small as to escape the naked eyes. Among these, I observed some which were much larger than the rest, and were coupled together, in which action they lay very still on the sides of the glass, till a larger sort interrupted them; while they were quiet, I could easily see those parts which they use in motion, and also the motion of several parts of their body. These animalcula were so large, that the eye might see them in clear water, and in a glass tube; among several of these sorts I could perceive some as large as little sands, and as completely formed as garden spiders.

An Account of Books. 1. *Astronomiæ Physicæ et Geometricæ Elementa. Auctore Davide Gregorio, M. D. Astronomiæ Professore Saviliano et R. S. S. Oxoniæ,* 1702, *fol.** N° 283, p. 1312.

What the world has hitherto wanted, the learned Dr. Gregory has supplied, in a complete system of true and physical astronomy; and as the last ages have been sufficiently furnished by the ancients, with the elements of geometry, so doubtless the future will have recourse to this book, for those of physical and geometrical astronomy.

The physical explications are all built on the principles of the great Newton; and he has, throughout the whole work, so mixed these with the phænomena themselves, that they who are solicitous about the astronomical part only, may understand it, as it is there delivered, without inquiring into the physical causes. The geometrical parts are either all proved from the citations of standard authors, or demonstrated in lemmas, inserted in their respective places. And that the reader may not think physical inquisitions in astronomy to be only new, in his preface he proves from Diogenes Laertius, Plutarch, Stobæus, &c.

* This excellent account of Dr. Gregory's astronomy, and other curious astronomical matters, has much the appearance of the style and composition of the learned Dr. Halley.

that the ancient philosophers, besides their knowledge of the true system of the universe, were not unacquainted with the real and physical causes of the planets motions, and that universal law of nature from which they all spring, viz. that matter attracts matter in a reciprocal proportion of the square of the distance.

In the first book the author treats of the order, motions, and periods of the primary planets, and the chief appearances that arise from thence: as their directions, stations, retrogradations, &c. the different phases of the inferior, like to those of the moon, the eclipses of both sun and moon; and then he proves that the primary planets turn round the sun; and that the direction of their gravity is not towards the earth, but towards the sun, and that it is by that force of gravity, that they are kept from going out in right lines, and made to turn round in orbits; that this force of gravity decreases in each of them in the same proportion, that the square of the distance from the sun or centre increases; as likewise, that all the secondary planets, in moving round their primary, are kept in their orbits by the force of gravity, whose direction is towards the centres of their respective primaries; and that the accelerating force of this gravity is in a reciprocal proportion of the square of the distance; and that in the moon it is the very same with the gravity of bodies towards our earth. Now, because we know by observations, that the planets move not in circles, but in ellipses, in one of whose foci the sun is placed, there must necessarily be a particular law of gravity acting towards the sun, to make the planet move in such curves. This he inquires into, and finds it to be the very same with what he had before discovered, by comparing the distances of the planets from the sun with their periodical times.

Afterwards, in the 7th section, he proves, that this law of attraction is universal in all matter to matter, and that it is the very same with gravity; and that the sun and planets mutually gravitating towards one another, must necessarily turn round their common centre of gravity, which by reason of the vast magnitude of the sun, in respect of the rest, cannot be far from the sun's own centre. Now, since by this law of attraction, the moon must not only be drawn towards the earth, but also towards the sun, and that differently, according as its distance and position varies; he from thence, and the inclination of its orbit to the plane of the ecliptic, gives the reason why the nodes move backwards; and on the same account, there being as it were a redundant ring of matter about the earth's equator, he shows how the equinoctial points must necessarily constantly go back a little, and make a precession of the equinox.

Having thus settled the true causes of the celestial motions, he next inquires into the various opinions of philosophers on that subject, and examines the

Physica Cœlestis of Kepler, the Vortices of Cartesius, and those of the ingenious Mons. Leibnitz, whose several theories he sufficiently confutes; and then he gives an account of the other three celebrated systems of the universe, the Ptolemaic, Tychonic, and Semi-Tychonic, and the different forces that are necessary to make the planets move in the order, which those systems require.

In the 2d book are explained all those things that depend on the motion of the primum mobile, the doctrine of the sphere, the genesis of the circles, the use of the globes, spheres, and other instruments, that are contrived to represent the diurnal motions of the stars, together with the method of determining, by observations, the positions of the circles in respect of each other, and the places of the stars, their longitudes, latitudes, &c. the method of making catalogues of the fixed stars, with an historical account of all those catalogues that have been hitherto made. Here also he shows the way of solving all the common problems of the sphere by trigonometry, as also the foundation and method of making all sorts of dials by the same calculus: and lastly, the various ways of determining the parallax and refractions of the stars, with their demonstrations.

In the 3d book he descends more particularly to the theory of the primary planets. And, because each planet so moves in an ellipse round the sun, that it describes areas always proportional to the times, he gives the methods of dividing the area of an ellipse by a line drawn from its focus in a given proportion; which may be done either by the indirect method of Kepler, or directly by a series of his own, which he demonstrates; and then he shows Dr. Ward's Theory (wherein the planets are supposed to describe about the other focus angles always proportional to the times) to be only an approximation to the true one, wherein they describe areas at the other focus that are proportional to the times, and as such it is expressly mentioned by Kepler in his Epitome Astronomiæ Copernicanæ; but if the distance between the foci be great, this method will not give the exact place of the planet, and therefore it wants the approximation of Bulliald, which is only a nearer approach to truth. Here also he considers the oval figure, in which Mons. Cassini supposes the planets to move; and he proves, that if a planet moved in that curve according to M. Cassini's Theory, it would describe round the sun equal areas in equal times; and by consequence a centripetal force of a determined kind acting on the planet, might make it describe this curve; whereas Dr. Ward's Theory is physically impossible, there being no centripetal force towards the sun, that can make them move in an ellipse according to this law; but then he shows, that the Theory of Cassini does not answer observations, nor will give the true anomaly of the planet, and its distance from the sun. After this he comes particularly to show the methods of determining the orbits of the earth and planets, their excentricities, position of their apsides and nodes, and describes all the tables that are necessary to calculate their motions, together with the manner of making them; and he also shows the way of finding out the magnitudes, densities and figures of the sun and planets, and the distances of the fixed stars, where he takes notice, that the annual different distances of a fixed star from the pole (which Mr. Flamsteed in his letter to Dr. Wallis, printed in the third tome of the Doctor's works, says, he has observed in the polar star) does not necessarily arise from their parallax; but may proceed from other causes; so that from this observation, we cannot conclude the parallax of the fixed stars, nor does it directly prove the motion of the earth, which Mr. Flamsteed thinks he has done by it. And it is the more likely that this variation of the star's distance from the pole, observed by Mr. Flamsteed, proceeds from some nutation, since the young Mr. Cassini has demonstrated, that in the polar star it cannot arise from the parallax of the magnus orbis; and our author has given good arguments to prove the fixed stars to be at so great a distance, that the diameter of the magnus orbis is insensible in respect of it.

Having thus established the theory of the primary planets; in the 4th book he gives that of the secondary, whose motions are much more composed and intricate than any of the first, theirs being only compounded of a uniform progressive motion forward, and that of gravity, by which, as it were, they are drawn towards the sun; whereas the secondaries are not only attracted towards their respective primaries, but also towards the sun, and that with the same accelerating force of gravity as the primaries, when at the same distance, with a greater force when nearer, and a less when farther from the sun, than the primaries are. Which variety of the force of attraction, according to the various positions of the secondaries orbit, its inclination to the orbit of its primary, and the place of the secondary in this orbit, must necessarily produce various inequalities and irregularities in the motions of the secondaries; all which he considers at large, and from thence he explains the motions of the apsides forwards, that of their nodes backwards, the changeable inclination of the orbits to the planes of their primaries orbits, the different velocities of the secondaries, according as their position, in respect of the sun and their primaries, varies; and he applied the whole to the motions of the moon. All this, before the happy discoveries of that celebrated philosopher Mr. Newton, was not to be attempted by any physical explication, the greatest advancement that was made, being only to solve them by an astronomical hypothesis. He also gives an account of the tables necessary to calculate the moon's place seen out of the earth, and the manner of using them; and to this he joins Mr. Newton's own theory of the moon, which if used in practice will determine her place nicely enough, even in the quadratures, as has been found by comparing it with Mr. Flamsteed's observations; where when the error is greatest it exceeds not two minutes, but for the most part it is so small, that it may be justly enough imputed to the uncertainty of observations. After this he treats more particularly of the eclipses of both sun and moon, and the manner of calculating them; he gives an account of the moon's motion round her axis, and shows how all those various nutations and librations of the moon, that have hitherto so much puzzled the philosophers, do only arise from her menstruous uniform rotations round her axis. From thence he passes to the ways of determining the magnitudes, densities, and quantities of matter in the secondary planets, and the change of figure, both in the primaries and them; which must necessarily arise, if they are fluid, from their mutual gravitation towards each other; from which he explains the tides and the motion of the seas. He ends the book with a description of the ring of Saturn, and accounts for all its various appearances seen out of the earth.

In the 5th book the author treats of comets; where, after having given the several opinions of philosophers about their place, duration, origin, orbits and tails; he tells us, that certainly they are bodies that move round the sun, and differ from the other planets in that their orbits are very oblong ellipses, whereas the planets describe ellipses which come very near to circles; and from this hypothesis it is easy to give an account of the most of their phænomena; particulary he shows, that when a comet comes down from its aphelion, or when it ascends to it, its trajectory will seem to be a right line, and may be assumed as such, when its motions are to be determined without great niceness; and on this account it is, that Kepler, and several others of the astronomers, did really suppose that they went on always in straight lines, without ever returning. Their tails he takes to be vapours, which being heated by the sun, arise copiously from their bodies when they descend towards him, and are carried upwards by the air in which they swim; where he takes notice, that if this matter of the tail (which is spread at a great distance from the comet itself) should touch and mix with our atmosphere, it may infect it with qualities pernicious to the temperament of our vegatables and animals; and from thence may arise all those effects which, by long observation and common consent of mankind, have been attributed to comets, notwithstanding their being ridiculed by some modern philosophers. From this he passes to show the method by which the orbits of the comets are to be determined. The ways he takes are not only excellent in that case, but likewise of great use in solving other intricate and difficult problems in natural philosophy. And then he gives the way of finding both the heliocentric and geocentric places of a comet for any given time, and describes tables that are necessary for the more ready performance of it.

The 6th and last book is upon comparative astronomy; where he considers the phænomena that would appear to a spectator placed in each of the heavenly bodies, and compares them with those that are seen from the earth. The chief of those he takes notice of, are their vicissitude and duration of days and nights, the apparent size, intenseness of heat, and light of the sun, the length of their years, and variety of seasons, the phases of the moon, and the eclipses of the luminaries, the number, directions, stations and retrogradations of the planets that are to be seen, and the helps they may afford to discover the true system of the universe. After having given an account of all these appearances in each of the heavenly bodies, he shows that of all the primary planets, the earth is the fittest place where an observer may discover the true system of the world, and Mercury the most unfit for that purpose; and that therefore an inhabitant in any one of the planets may on better reasons suppose his own habitation immoveable, and that the whole heavens turn round about him, than these which our terrestial observers have for their immobility; and having fewer arguments, it will be harder to convince them of their own motion. We may reasonably then imagine, that an astronomer in any of the other planets, as for example in Venus, would suppose his own habitation immoveable, and would endeavour to make a system to satisfy all phænomena on that scheme. This, in his proposition on the astronomy in Venus, he says may be effected from the true system two ways: first, if Venus were brought from its place in the true system down to the centre, and the sun and the rest of the planets, together with their orbits, advanced as far the same way in parallel lines. Such a system in Venus would be perfectly Tychonic, in which the sun would be made to turn round Venus in an orbit, equal and like to that in which Venus does really turn round the sun, in the true system; and all the planets, together with their satellites, besides their being carried round Venus by the sun, would have all the same motions round the sun, as in the true system. The orbit of the earth in this system would cut the sphere of the sun after the same manner, as in the orbit of Mars it intersects the orbit of the sun in the Tychonic system, which is framed for our earth; whereas the orbit of Jupiter would not cut it, but surround it. The second way, by which an astronomer in Venus may contrive a system to satisfy appearances, and make himself unmoveable in the centre, is by bringing Venus to the centre, and advancing the sun and all the planets just as far in parallel lines, as was done before; but leaving their orbits immoveable, and the very same which the planets described round the sun in the true system. If this were supposed by an astronomer in Venus it would be the Ptolemaic system, in which the sun is carried round Venus, placed in the centre of all the planets orbits, and such of the planets move in an epicicle similar and equal, but in a contrary position, to the true orbit of Venus round the sun, the centres of the epicicles being moved in their deferents in the same times that the planets themselves perform their revolutions in the true system. And a Ptolemaic system made in this manner, would satisfy all the celestial phænomena, equally as well as the Tychonic or the true system; so that if the Ptolemaic system for the earth had been thus framed, there could have been no arguments from observations brought against it; nor indeed any other way left to convict it of falshood, but that only which is drawn from the principles of natural philosophy.

An Abstract of Dr. Mead's Mechanical Account of Poisons. Communicated by Samuel Morland. N° 283, p. 1320.

Every medical reader is so familiarly acquainted with Dr. Mead's writings, that the reprinting of this extract is judged to be wholly unnecessary. The facts relative to the structure of the viper's teeth, the situation of the bags containing the poison, and the manner of its infusion into the wound during the bite, have been already mentioned in the notes on Redi's observations on this subject, at p. 58, vol. i. of this Abridgment. It is only further necessary in this place to remark, that Dr. Mead's mechanical hypothesis of the manner in which he supposed the viperine and other poisons to act, is now exploded; a chemical explanation having been adopted in its stead.

Concerning a Water Spout, lately observed at Hatfield. By the Rev. Mr. Abr. De la Pryme, F. R. S. N° 284, p. 1331.

The weather in this part of the country has been exceedingly wet and cool, so that it seemed to be spring rather than midsummer, yet June 21, 1702, was pretty warm, on the afternoon of which day, about 2 o'clock, no wind stirring below, though it was somewhat great in the air, the clouds began to be much agitated and driven together; on which they became very black, and were very visibly hurried round, from whence there proceeded a most audible whirling noise, like that commonly heard in a mill. After a while a long tube or spout came down from the centre of the congregated clouds, in which was a swift spiral motion like that of a screw, or the cochlea Archimedis when it is in motion, by which spiral nature and swift turning, water ascends up into the one, as well as into the other. It proceeded slowly from west to north east, broke down a great oak-tree or two, frightened the weeders out of the field, and made others lie down flat on their bellies, to avoid being whirled about and killed, as they saw had happened to several jackdaws, which were suddenly snatched up, carried out of sight, and then thrown a great way off among the corn; at length it passed over the town of Hatfield, to the great terror of the inhabitants filling the whole air with the thatch it took of from some of the houses; then touching on a corner of the church, it tore up several sheets of lead, and rolled them together in a strange manner; soon after which, it dissolved and vanished, without doing any further mischief.

There was nothing more extraordinary in this, than in the other that I gave an account of in N° 281; and by all the observations that I could make of both, I found that had they been at sea, and joined to its surface, they would have carried a vast quantity of water up into the clouds, and the tubes would then have become much more strong and opaque than they were, and have continued much longer.

It is commonly said that at sea, the water collects and bubbles up a foot or two high under these spouts, before they are joined: but this is a mistake owing to the pellucidity and fineness of those tubes, which certainly touch the surface of the sea before any considerable motion can be produced in it, and that when the pipe begins to fill with water, it then becomes opaque and visible. As for the reason of their dissolving of themselves, after they have drawn up a great quantity of water, I suppose it is by and through the great quantity of the water they have carried up, which must needs thicken the clouds, impede their motion, and by that means dissolve the tubes.

*On the Tangents of Curves, deduced immediately from the theory of Maxima and Minima: with the Theorem, by which, with the Help of the same Calculus, certain Properties relating to the Conic Sections are investigated. By Mr. Humphry Ditton.** N° 284, p. 1333. *Translated from the Latin.*

I here propose a method of tangents, which is easy and sufficiently general, indeed quite general, as serving, by the same operation, for all curves whatever.

* Mr. Ditton was a very sound and respectable mathematician, as well as divine, and published many useful works on both these subjects, though he died but a young man, viz. Anno 1715, in the 40th year of his age. He was born at Salisbury, was bred among the dissenters, and in compliance with the desires of his father, against his own inclination, officiated for some time as minister to a congregation. But on his father's death he pursued his favourite subject, the mathematics, and through the interest of Sir Isaac Newton he became master of the new mathematical school in Christ's Hospital, London.

Besides the present, he has another paper in the Philos. Trans. for 1705, viz. on spherical catop-

Nor shall I scruple to call it a new one, since none of the celebrated geometricians, as far as I know, have ever published any thing of the kind. I shall here produce only a specimen in a few instances.

Let AGH (fig. 13, pl. 1) be a curve, having the vertex A, axis AK, ordinate FD, and centre (if it have one) the point K. Taking L a point in the axis, make $AL = n$, $AD = x$, $FD = y$, $FL = z$. Of these quantities the last three are flowing quantities, and n is a constant quantity; for this being always the same, answers to the others which are always variable. From the right-angled triangle FDL, we have this equation, $zz = yy + nn - 2nx + xx$; and determining z to be an extreme, there arises $2y\dot{y} - 2n\dot{x} + 2x\dot{x} = 0$; whence by interpreting $2y\dot{y}$ according to the particular nature of the curve, the quantity n will be left, expressed in terms that will also be proper to the curve.

And now by this means having z determined to its extreme value, that is, having the line FL either the greatest or least of all those which can be drawn to the curve from the point L, and therefore perpendicular to the curve in the point F; it is evident that DL is the subnormal, from whence the subtangent is easily derived.

For an example, let us first take the Apollonian parabola, which curve we will suppose to be here delineated. Therefore we have $2y\dot{y} = r\dot{x}$, supposing r to be the parameter; whence $r\dot{x} - 2n\dot{x} + 2x\dot{x} = 0$, and $n = \frac{1}{2}r + x$. Therefore the subnormal $DL = \frac{1}{2}r$. Now the meaning of this theorem is this: if beyond the limit D of the absciss AD, there is taken DL equal to the semiparameter, and from the point L be drawn LF straight to the point F; the right line so drawn will be perpendicular to the parabola in the point F, and the least of all the lines that can be drawn to the curve from the point L. I say it is the least; for to any one that considers the nature of the curve, it is evident it cannot be the greatest, (which I would have observed in what follows;) but it is necessarily either the greatest or least, and therefore the latter. And this is the first part of theor. 5, lib. 7, of De la Hire's Conics.

Let the ordinate EB be drawn, and join the points E, L, make the intercepted

trics which was copied into several foreign works. His other publications were, 1. General Laws of Nature and Motion, 1705; 2. An Institution of Fluctions, 1706; 3. Alexander's Synopsis of Algebra, with many additions and corrections, 1709; 4. Treatise on Perspective, 1712; 5. The New Law of Fluids, 1714; 6. The same year several theological pieces, particularly his discourse on the Resurrection of Jesus Christ: all which works met with very distinguished applause from the learned of several countries. Mr. Ditton was connected with the celebrated Mr. Whiston in a scheme for the longitude, viz. by the firing of great guns at a distance; but the plan being rejected by the Board of Longitude, it is said the grief at the disappointment, and the ridicule thrown upon it, in a ludicrous poem, by Dr. Swift, had the unhappy effect of occasioning his death. See a more particular account of Mr. Ditton, and his writings, in Dr. Hutton's Dictionary, vol. i. p. 388.

line BD $= f$, whence AB $= x - f$, and BL $= \frac{1}{2} r + f$. Now LE$^2 = \frac{1}{4}rr + rx + ff$, and FL$^2 = \frac{1}{4} rr + rx$. Therefore LE^2 — FL^2 = BD^2, which is the latter part of the same theorem.

The nearer the point F approaches to the point A, or to the vertex, in which the perpendicular cuts the curve, the nearer also the point L approaches to the same. Therefore when F coincides with A, and so the ordinate FD vanishes, then the minimum itself lies in the axis AK, and will be equal to the semi-parameter; that is, in this case $n = \frac{1}{2}r$ only; the absciss x belonging to the vanishing ordinate then also vanishing. If therefore AL $= n = \frac{1}{2}r$, taking the point D between A and L, make AD $= x$; then there arises FL$^2 = \frac{1}{4}rr + xx$, and therefore FL^2 — AL^2 $= xx$, that is, FL^2 — AL^2 = AD^2. As it is theor. 2, lib. 7, Conic. De la Hire.

Secondly, let there be a certain curve of a superior parabolic order, whose equation is

$$r^{p-q} x^q = y^p; \text{ then } yy = r^{\frac{2p-2q}{p}} \times x^{\frac{2q}{p}}; \text{ therefore } 2y\dot{y} = \frac{2q}{p} r^{\frac{2p-2q}{p}} x^{\frac{2q-p}{p}} \dot{x}.$$

Now if we substitute this value instead of $2y\dot{y}$ in the general equation, which determines z to be an extreme, we shall have from thence

$$n = \frac{q}{p} r^{\frac{2p-2q}{p}} x^{\frac{2q-p}{p}} + x, \text{ therefore the subnormal is DL} = \frac{q}{p} r^{\frac{2p-2q}{p}} x^{\frac{2q-p}{p}}.$$

Now this is easily applied to any of these curves, if the indices p and q are rightly expounded according to the nature and genius of each curve.

Thirdly, let it be supposed that the curve is an ellipsis, of which AK is half the greater axis. Now it follows, from its equation, that $2y\dot{y} = r\dot{x} - \frac{2rx\dot{x}}{q}$; whence $r\dot{x} - \frac{2rx\dot{x}}{q} - 2n\dot{x} + 2x\dot{x} = 0$, and $n = \frac{r}{2} + x - \frac{rx}{q}$; therefore $\frac{r - rx}{2q}$ = DL the subnormal. Now if, instead of the ellipsis, a circle were substituted, by proceeding with the equation in the same manner, we should find DL $= r - x$, making r the radius of the circle.

But let us return back to the ellipsis, another of whose properties may be derived from hence, as was done in the parabola. Make BD $= f$, whence AB $= x - f$. Then we shall have LE^2 = (LB^2 + EB^2 =) $\frac{rr}{4} - \frac{rrx}{q} + \frac{rrxx}{qq} + ff + rx - \frac{rxx}{q} - \frac{rff}{q}$; and FL^2 = (FD^2 + LD^2 =) $rx - \frac{rxx}{q} + \frac{rr}{4} - \frac{rrx}{q} + \frac{rrxx}{qq}$. Therefore LE^2 — LF^2 $= ff - \frac{rff}{q}$. Now this is theor. 6, lib. 7, Conic. De la Hire.

For that great geometrician requires, that it may be $q : r :: \frac{1}{2}q - x :$ LD, the value of which therefore is $\frac{r-rx}{2q}$, as found above: therefore it is a fourth proportional to the three quantities before exhibited. This being granted, he evidently demonstrates, that LF is the least of all the lines that can be drawn from the point L to the ellipsis. Moreover, because it is $q : q - r :: f : f - \frac{fr}{q}$. Therefore the rectangle $ff - \frac{rff}{q}$ or $\overline{f \times f - \frac{rf}{q}}$ is the same rectangle which De la Hire calls his specimen. But this specimen, according to his definition, is a rectangle like to the rectangle that constitutes the difference between the square of the transverse axis and the figure, that is, the rectangle $qq - qr$, being besides applied to the right line BD or f. Now that the rectangle $ff - \frac{rff}{q}$ has all these conditions, is very evident. It may be observed, that it follows, from the value of n before found, that $n > \frac{r}{2}$. For $n = \frac{r}{2} + x - \frac{rx}{q}$. Therefore $qn + rx = \frac{1}{2} qr + qx$. But because $q > r$, it is $qx > rx$, and therefore $qn > \frac{1}{2}qr$, and $n > \frac{1}{2}r$.

When the point F (as was just now observed in the parabola) falls on the vertex A, the minimum is determined in the axis: and because of x vanishing, we shall have $n = \frac{1}{2}r$. Then assuming any point D between A and L, if AD be equal to any x, by comparison there arises $FL^2 - AL^2 = rx - \frac{rxx}{q}$. And this is theor. 3, lib. 7, of De la Hire's Conics. For because it is $q : q - r :: x : x - \frac{rx}{q}$, it appears that the rectangle $xx - \frac{rxx}{q}$ is the exemplar, but applied to the absciss x: and therefore this is the adequate measure of the defect of the square of the least line, from the square of any other right line drawn from the same point to the curve. And this is what he demonstrates in the place above cited.

Now the theorems belonging to the lesser or conjugate axis of the ellipsis, (for hitherto we have insisted on the greater or transverse axis) are determined just in the same manner. For now let AK, or half the lesser axis, be $\frac{1}{2}c$, R the parameter; and the point L is now supposed to be placed beyond the centre, on the other side of GK. By proceeding as before, we shall find AL or $n = \frac{R}{c} + x - \frac{Rx}{2}$, and the subnormal, DL $= \frac{R}{c} - \frac{Rx}{2}$. That is, $c : R :: \frac{c}{2} - x : \frac{R}{2} - \frac{Rx}{c}$; and therefore, drawing FL, it will be the greatest of all the lines that can be drawn from the point L to the ellipsis; and $LF^2 - LE^2 = \frac{Rff}{c} - ff =$ to the rectangle, which is the exemplar, applied to BD or f. For it appears that this is the exemplar, since $c : R - c :: f : \frac{Rf}{c} - f$, and therefore, according to the definition, $\overline{\frac{Rf}{2} - f} \times f$ is equal to the exemplar. Now this is theor. 7, lib. 7, of De la Hire's Conics.

Again, when the point F coincides with A, because of x vanishing with the ordinate then vanishing, there is left $n = \frac{1}{2}R$, and AL is the greatest of all the lines that can be drawn from the point L to the ellipsis, and $AL^2 - FL^2 = \frac{Rxx}{c} - xx =$ to the exemplar applied to AD or x. And the same as theor. 4 of said lib. Conics.

But it ought to be observed at the foregoing case, (which should have been mentioned before) when we found $n = \frac{R}{2} + x - \frac{Rx}{c}$, that $n < \frac{1}{2}R$. For $cn + Rx = \frac{1}{2}Rc + cx$; and because $R > c$, therefore $Rx > cx$, and there will be left $cn < \frac{1}{2}Rc$, or $n > \frac{1}{2}R$.

Now as the matter is performed in the ellipsis, so in the same manner it might be performed in the hyperbola, and the least lines may also be determined in this curve. But there is such a connection between these two curves, and the transition from one to the other is so easy, that the labour may seem unnecessary even to novices. Therefore nothing more remains to determine the subnormal, than that the sign $-$ may be changed into $+$. For since in the hyperbola it is $2y\dot{y} = r\dot{x} + \frac{2rx\dot{x}}{q}$, and $n = \frac{r}{2} + x + \frac{rx}{q}$; (the general equation) there remains $DL = \frac{r}{2} + \frac{rx}{q}$.

Let it be conceived fourthly, that the curve MSN (drawn on the other side the figure) is one of the hyperboloids, whose asymptotes are AK, KH, and the right line SR an ordinate to the asymptote KH; make $SR = y$, $SP = z$, $KR = x$, $KP = n$, which here must needs be less than x, as will appear on consideration. The equation proper to the curve is $y^p x^q = r^q s^p$, instead of which, because of r and s being determinate quantities, may be written $y^p = x^{-q}$, and therefore $y^2 = x^{-\frac{2q}{p}}$, and $2y\dot{y} = -\frac{2q}{p}\dot{x}x^{-\frac{2q+p}{p}}$. Hence, since it is $zz = yy + xx - 2nx + nn$, for an extreme we have $2y\dot{y} + 2x\dot{x} - 2n\dot{x} = 0$; that is, $-\frac{2q}{p}\dot{x}x^{-\frac{2q+p}{p}} + 2x\dot{x} = 2n\dot{x}$, and $n = x - \frac{q}{p}x^{-\frac{2q+p}{p}}$; therefore $(x - n =)$ $\frac{q}{p}x^{-\frac{2q+p}{p}}$ is the subnormal PR.

Lastly, let us conceive the curve AFG to be a primary cycloid, and let the radius be r, the arch c, and the ordinate of the generating circle to be y, whose diameter may be represented by AK, and the centre posited between L and K. Then calling FD the ordinate of the cycloid, a, and the rest as before, the equation of the curve is $aa = yy + 2cy + cc$, and therefore $zz = (aa + nn - 2nx + xx =) yy + 2cy + cc + nn - 2nx + xx$, and z being determined for an extreme, $2y\dot{y} + 2c\dot{y} + 2y\dot{c} + 2c\dot{c} - 2n\dot{x} + 2x\dot{x} = 0$. But $\dot{y} = \frac{r\dot{x} - x\dot{x}}{y}$, and $\dot{c} = \frac{r\dot{x}}{y}$; then substituting these values, and duly reducing the equation, we shall have $2r - x + \frac{2rc - 2xc}{y} + 2r + \frac{2cr}{y} = 2n - 2x$, and therefore

$2r - x + \frac{2rc - xc}{y} = n - x =$ DL the subnormal.

The incomparable Dr. Barrow makes use of the subtangent as already known, to determine the maximum and minimum. And Mr. Newentiit, in his Analysis of Infinites, has done the same after him. But since the maxima and minima may be found by many other methods, in which nothing need be presupposed about the tangents of curves, it is plain that we may safely proceed from the maxima and minima to investigate the method of tangents.

Corol. 1. In going over again the foregoing examples, it will appear from each, that $2y\dot{y} - 2n\dot{x} + 2x\dot{x} = 0$, by putting instead of n in this equation its value derived from the nature of the curve. For example, in the hyperboloids $\frac{2q}{p}\dot{x}x^{-\frac{2q-p}{p}} - 2x\dot{x} + \frac{2q}{p}\dot{x}x^{-\frac{2q-p}{p}} + 2x\dot{x} = 0$, which appears by inspection. And the same will appear to be true in other examples, without any demonstration.

Corol. 2. From the invention of the subnormals we may easily determine the greatest and least ordinates of curves. In which matter I shall add, if the subnormal belonging to any point of the curve be put equal to nothing, we shall have the ordinate of that curve determined to be an extreme. And it will be the greatest, if it is on the concave side of the curve, but the least if it is on the convex side. For example, in the circle, making the subnormal $= l$, it will be $l = r - x$. Let $r - x = 0$, then $r = x$, and $y = r$; that is, the greatest ordinate is equal to the radius. In like manner, in the ellipsis $l = \frac{r}{2} + \frac{rx}{q}$; let $\frac{r}{2} - \frac{rx}{q} = 0$, then $rq = 2rx$, or $x = \frac{1}{2}q$. Therefore $yy = \frac{1}{4}rq$, equal to a fourth part of the figure, as they call it, or the square of the conjugate semiaxis, and therefore the greatest y is equal to that semiaxis. And the same method may be used in other curves. Let the subnormal be found from the given equation, and making that equal to nothing, we shall have the ordinate of the curve determined to a maximum or minimum, the first towards the concave part of the curve, and the other towards the convex part.

POSTSCRIPT. First, it will be easy by this method to determine the tangent, by operating at the convex side of the curve, as before on the concave side. For let AC be the vertical tangent, and C a point in it taken at pleasure. Make AC $= n$, CO $= z$, (by which symbol let all the lines be denoted, which are drawn from the point C to the convex curve AEG). Then drawing MO always perpendicular to AC, it will be CM $= n - y$. And since OM $= x$, it will be $zz = nn - 2ny + yy - xx$; and therefore, for an extreme value of z, $2y\dot{y} + 2x\dot{x} - 2n\dot{y} = 0$. In which equation, if $2x\dot{x}$ be expounded according to the nature of the curve, we shall have the line CZ determined, which in this place performs the office of a subnormal. This is too clear to need any illustration by examples.

Secondly, as in the foregoing method we have found the tangents of curves, by determining to extremes the lines LE or CO, drawn from a given point either in the axis or in the vertical tangent; thus by considering the lines QE, &c. drawn from a given point in the axis beyond the vertex, the same may be performed, and that universally. For all the lines QE are of a flowing and variable nature, but the tangent QF alone, (supposing QF to touch the curve) is constant and determined to one value. Therefore in this place we shall not insist on the hypothesis of an extreme, but shall only consider it as a permanent quantity. Let two points Q, L, be assumed, and thence to the same point of the curve E let two lines LE, QE, be always drawn. The angle QEL between the point of contact F and the vertex, will always be obtuse, but on the other side of the point F it will be acute; supposing, as said before, that QF touches the curve, and FL is at right angles to it. Make QA $= p$, AL $= n$, AB $= x$, BE $= y$, and QE $= z$. Also VE $= v$, which is intercepted between the points E and V, where QV falls perpendicularly from Q upon LE produced. Now because of the obtuse angled triangle QEL, we shall have this equation, $zz = pp + 2pn - yy - xx + \sqrt{yy + nn - 2nx + xx} \times 2v$; or instead of $\sqrt{yy + nn - 2nx + xx}$ writing f, it will be $zz = pp + 2pn - yy - xx + 2nx - 2fv$, and thence $2z\dot{z} = y\dot{y} - 2x\dot{x} + 2n\dot{x} - 2f\dot{v} - 2v\dot{f}$. Now if z be a constant quantity, in which case QE will coincide with the tangent QF, it will be then $- 2y\dot{y} - 2x\dot{x} + 2n\dot{x} = 0$, the rectangle $2fv$, and therefore its fluxion entirely vanishing. But this is the very general equation, that was determined by the foregoing method, which is deduced with the same ease from the supposition of a constant quantity, as before from the principle of an extreme quantity.

Specimen of a General Method of determining the Quadrature of Figures. By the Rev. John Craig. Addressed to Dr. Geo. Cheyne.*† N° 284, p. 1346. *Translated from the Latin.*

You will easily believe, sir, that I am not a little pleased that the method I use for determining the quadrature of figures, is so well approved of by M. Leibnitz and yourself: so that he acknowledges it somewhat resembles the method discovered by himself, and you conjecture it has some affinity with the method of Mr. Newton. And by your own success in such pursuits you have greatly improved the inverse method of fluxions, by your books on that subject, dedicated to Dr. Archibald Pitcairne, the ornament of our age and country. But many things still remain to be discovered for perfecting this inverse method; and I shall now give some reasons which show, that what remains, cannot be obtained by any methods yet in use.

And first, when from the given relation between z and y, the fluent of $z\dot{y}$ is required, all those methods demand that z should be expressed by y and given quantities; which however cannot be done when the equation defining that relation rises above a cubic or biquadratic; for here the common algebra stops, to the great reproach of that science. Secondly, although a general rule were known, for finding the roots of equations of any degree, yet it would be quite useless in this inverse method; for the root z would be involved in so many complicated surds, that by no art yet could we revert from the fluxion to the fluent. For these reasons I have tried another course, and with some success, a specimen of which is here given in what follows.

* The Rev. Mr. John Craig was a very respectable Scotch divine and mathematician, as appears by his writings, both those printed in these Transactions, and the books published by himself. The dates of his birth or death do not appear; but it would seem he was minister to acong regation at Gillingham, as his communications to the Royal Society are dated from that place. Besides his numerous papers in the Transactions, his own publications are chiefly these: 1. Methodus Figurarum Quadratus, &c. An. 1685; 2. De Quadraturis et Locis, 1693; 3. De Calculo Fluentium, 1718; 4. Theologiæ Christianæ Principia Mathematica, 1699. In this curious work, which was afterwards reprinted at Leipsic, the author maintains by mathematical calculation, that christianity will last only 1454 years from the date of his writing.

† Dr. George Cheyne, eminent both as a physician and mathematician, was born in Scotland 1671, and was educated at Edinburgh. Like his friend Mr. Craig, he applied himself to cultivate the new science of fluxions, lately discovered by Newton. At the age of 30, Dr. Cheyne came to London, where from his manner of living he became so corpulent, that his life was a burden to him. But by means of a vegetable diet he recovered his health and activity, and did not die until he was in the 72d year of his age. Besides his treatise on the inverse Method of Fluxions, he published also Philosophical Principles of Religion, natural and revealed. As also the English Malady, or a Treatise on Nervous Diseases.

SECTION I. Let $z^m + ay^n = bz^e y^r$ be an equation expressing the relation between the ordinate z and absciss y; in which the exponents m, n, e, r, denote any numbers, integer or fracted, affirmative or negative. Put $r - n = c$; then will the area $= \frac{m}{m+n} zy + \frac{mc + ne}{m . \overline{m+n} . \overline{c+1} + n . \overline{m+n} . \overline{e+1}} \times {}_a z^{e+1} y^{c+1}$ $+ \frac{\overline{m-e} . \overline{c+1} + r . \overline{e+1}}{m . \overline{2c+1} + n . \overline{2e+1}} \times \frac{bB}{a} z^{2e+1} y^{2c+1} + \frac{\overline{m-e} . \overline{2c+1} + r . \overline{2e+1}}{m . \overline{3c+1} + n . \overline{3e+1}} \times \frac{bC}{a} z^{3e+1} y^{3c+1}$ + &c.

Concerning this series the following things are to be noted: 1. That the capitals B, C, D, &c. denote the co-efficients of the terms immediately preceding; 2. That it exhibits the quadratures of all quadrible figures, whose curves are defined by an equation of three terms; 3. And that these are always quadrible when $\frac{m + r - e}{mn - mr - en}$ is an affirmative integer number, which we may call b; 4. Particularly that $l + 1$ gives the number of the terms of the series, counted from the beginning, that constitute the required area; 5. That if we suppose $e = 0$, this series will be changed into Newton's celebrated Binomial Theorem, which theorem is therefore a particular case of this series; 6. When application is made of this series to any particular figure, these following rules are to be observed; first, let the equation defining the given curve be reduced to the general form; then by comparing the particular equation with the general, let the co-efficients a and b be found, as also the exponents m, n, e, r. Secondly, if the exponents thus determined do not make l an affirmative integer number, according to the condition in note 3, then another term of the particular equation is to be freed from the quantity z; and if the exponents again determined do not give the condition of quadribility required, then the other term is to be freed from the quantity z; for all the three terms in the given equation cannot by any means be freed from the quantity z. Thirdly, if the said condition of quadribility does not belong to the equation, when treated according to the fore going rule, then by the series find the complement of the area, or fluent of $y\dot{z}$; which being found, the area required will become known: for it is well known that zy — flu. $y\dot{z}$ = flu. $z\dot{y}$. And that the complement may be obtained by the series without confusion, in the given equation, defining the particular curve, for z we may write Y, and for y write Z; and having made this change of the ordinate into the absciss, and the absciss into the ordinate, the equation may be treated according to the precepts of the second rule, till the condition of quadribility be obtained, or till it appear that no such condition can be had.

Example 1. Let $z^3 + y^3 = bzy$. Because here $m = 3$, $n = 3$, $e = 1$, $r = 1$, $a = 1$; therefore $l = 1$, and $l + 1 = 2$. Then, according to note 4, the first two terms of the series give the area $= \frac{1}{2} zy - \frac{1}{6} bz^2y^{-1}$.

Example 2. Let $z^7 + ay^3 = bzy^2$. Here $m = 7$, $n = 3$, $e = 1$, $r = 2$; which give $l = 2$; therefore, by note 4, the first three terms of the series give the required area $= \frac{7}{10}zy - \frac{b}{15a}z^2 - \frac{2b^2}{15a^2}z^3y^{-1}$.

Example 3. Let $z^3 + ky^5 = hz^{-2}y^{11}$. Here $m = 3, n = 5, e = 2, r = 11$; but because these do not make l an affirmative integer number; therefore, by the 2d rule, free the term $hz^{-2}y^{11}$ from the quantity z; then the equation becomes $z^5 - hy^{11} = -kz^2y^5$; where $a = -h$, $b = -k$, $m = 5$, $n = 11$, $e = 2$, $r = 5$; which give $l = 1$; hence the area $= \frac{5}{16}zy - \frac{k}{16h}z^3y^{-5}$.

Example 4. Let $z^2 - hy^2 = -kz^2y^2$. Here $m = 2$, $n = 2$, $e = 2$, $r = 2$, which do not make l an affirmative integer number; therefore freeing the term $-kz^2y^2$ from the quantity z, then $z^0 + ky^2 = hz^{-2}y^2$; where $a = k$, $b = h$, $m = 0$, $n = 2$, $e = -2$, $r = 2$, which make $l = 1$; therefore the area $= \frac{h}{k}z^{-1}y$.

Example 5. Let $z^2 - \frac{4g^2}{h}y^6 = -\frac{g}{h}z^2\ y^4$; where $m = 2$, $n = 6$, $e = 2$, $r = 4$; which do not make l an affirmative integer number; and the same thing happens when either of the other terms is freed from z; therefore, according to the third rule, I seek the complement: thus, as before directed, making $z = Y$, and $y = z$, the given equation becomes $Y^2 - \frac{4g^2}{h}Z^6 = -\frac{g}{h}Z^4\ Y^2$; which, by rule 1, reduced to the general form, will be $Z^6 - \frac{h}{4g^2}Y^2 = \frac{1}{4g}Z^4\ Y^2$; where $m = 6$, $n = 2$, $e = 4$, $r = 2$; which do not make l an affirmative integer number; therefore, by rule 2, freeing the last from z, it becomes $Z^2 - \frac{1}{4g}Y^2 = \frac{h}{4g^2}Z^{-4}\ Y^2$; where $m = 2$, $n = 2$, $e = -4$, $r = 2$; hence $l = 1$; and $a = -\frac{1}{4g}$, $b = \frac{h}{4g^2}$; then the complement of the required area is $\frac{1}{4}ZY - \frac{h}{2g}Z^{-3}Y$, or $\frac{1}{4}zy - \frac{h}{2g}zy^{-3}$; and theref. flu. $z\dot{y} - \frac{1}{4}zy + \frac{h}{2g}zy^{-3}$ is the area sought.

SECT. II. Let $z^m + ay^n = bz^{2e}y^{2c+n} + fz^e y^{c+n}$ be an equation expressing the relation between the ordinate z and absciss y. Then will the area be — $Azy + Bz^{e+1}y^{c+1} + Cz^{2e+1}y^{2c+1} + Dz^{3e+1}y^{3c+1} + Ez^{4e+1}y^{4c+1} +$ &c. Where, putting $2c + n = r$, and $c + n = s$, it will be

$$A = \frac{n}{m+n};\quad B = \frac{\overline{m-e+s}\,.\,A + e - m}{m\,.\,\overline{c+1} + n\,.\,\overline{e+1}} \times \frac{f}{a};$$

$$C = \frac{\overline{m-2e+r}\,.\,bA + \overline{m-e}\,.\,\overline{c+1} + r\,.\,\overline{e+1}\,.\,fB + 2eb - mb}{ma\,.\,\overline{2c+1} + na\,.\,\overline{2e+1}};$$

$$D = \frac{\overline{\overline{m - 2c} . \overline{c + 1} + r . \overline{e + 1}} . bB + \overline{\overline{m - e} . \overline{2c + 1} + s . \overline{2e + 1}} . fC}{ma . \overline{3c + 1} + na . \overline{3e + 1}};$$

$$E = \frac{\overline{\overline{m - 2e} . \overline{2c + 1} + r . \overline{2e + 1}} . bC + \overline{\overline{m - e} . \overline{3c + 1} + s . \overline{3e + 1}} . fD}{ma . \overline{4c + 1} + na . \overline{4e + 1}};$$

&c.

Concerning this series, the progression of which almost appears by inspection, it is to be observed, 1. That those figures are quadrible, which are defined by the foregoing equation, when the exponents m, n, e, c, and the co-efficients a, b, f, have the following relations, viz. when $\frac{2c + \overline{m . 2e - n}}{cm + en}$ is an affirmative integer number, which we may call l; and (l being greater than 2) when the relation of the co-efficients is as follows: viz.

$$\frac{\overline{m + 2c} . \overline{lc - c + 1} + r . \overline{le - e + 1}}{\overline{e - m} . \overline{lc + n} - s . \overline{lc + 1}} \times \frac{bV}{f} =$$

$$\frac{\overline{m - 2c} . \overline{lc - 2c + 1} + r . \overline{le - 2e + 1}}{m . \overline{lc + 1} + n . \overline{le + 1}} \times \frac{bP}{a} + \frac{\overline{m - e} . \overline{lc - c + 1} + r . \overline{le - e + 1}}{m . \overline{lc + 1} + n . \overline{le + 1}} \times \frac{fV}{a}.$$

Where V and P denote the co-efficients of two terms, which immediately precede the last term of the area required; viz. V the co-efficient of the term next to the last, and P the co-efficient of the second term before the last; as, if $Fz^{5e+1} y^{5c+1}$ be the last term of the required area, then V denotes E, and P denotes D. 2. That the last term of the required area is known from the value of the number l; for here also $l + 1$ gives the number of terms in the series, taken from the beginning, which constitute the area required. 3. If $l = 1$, then the relation of the co-efficients must be this, viz.

$$\frac{\overline{2e - m} . \overline{1 - A} + rA}{\overline{e - m} . \overline{c + 1} - s . \overline{e + 1}} \times \frac{b}{f} = \frac{\overline{e - m} . \overline{1 - A} + sA}{m . \overline{c + 1} + n . \overline{e + 1}} \times \frac{f}{a}.$$

But if $l = 2$, then the relation must be

$$\frac{\overline{m - 2e} . \overline{c + 1} + r . \overline{e + 1}}{\overline{e - m} . \overline{2c + 1} - s . \overline{2e + 1}} \times \frac{bB}{f} = \frac{\overline{2e - m} . \overline{1 - A} + rA}{m . \overline{2c + 1} + n . \overline{2e + 1}} + \frac{\overline{m - e} \; \overline{c + 1} + s . \overline{e + 1}}{m . \overline{2c + 1} + n . \overline{2e + 1}} \times \frac{fB}{a}.$$

SECT. III. Let $z^m = ay^n + bz^e y^{c+n} + fz^{2e} y^{2c+n} + gz^{3e} y^{3c+n} +$ &c. be the equation expressing the relation between the ordinate z and the absciss y, and consisting of as many terms as you please; then the area will be $Azy + Bz^{e+1} y^{c+1} + Cz^{2e+1} y^{2c+1} + Dz^{3e+1} y^{3c+1} +$ &c.

Which I believe is no contemptible theorem. The co-efficients A, B, C, D, &c. are found by a very easy calculation, as also the conditions of quadribility, and how many terms of the series the area requires. Now the number of these conditions increases with the number of the terms in the equation defining the relation between z and y: and particularly if that number of terms be called N, then N — 2 will be the number of the conditions of quadribility; one of which shows the relation of the exponents m, n, e, c, and is this, viz. that $\frac{Nc - 2c + 2e - Ne + m + n}{-cm - en}$ be an affirmative integer number, which call l; but the other conditions respect the co-efficients, a, b, f, g, h, &c; and lastly, $l + 1$ gives the number of the terms of the series, taken from the beginning, which constitute the required area.

Corol. From this general series may be deduced a series, exhibiting the quadratures of figures, whose curves are defined by an equation consisting of any terms, which constitute the general equation of the third section. For, to obtain this, there needs only to be computed a series for an equation consisting of as many terms of the general equation, counted from the beginning, as are the terms contained in the equation defining the curves. Then from the values of the quantities A, B, C, D, &c. the co-efficients b, f, g, &c. may be expelled, which do not belong to the equation proposed; the others will give the area required, as will appear by an example.

SECT. IV. Let $z^m = ay^n + bz^e y^{c+n} + gz^{3e} y^{3c+n}$ be an equation expressing the relation between z and y. Now because $z^m = ay^n + bz^e y^{c+n} + fz^{2e} y^{2c+n} + gz^{3e} y^{3c+n}$ is that part of an equation which, taking the terms in order from the beginning, include the given equation; which hereafter, for brevity's sake, may be called the complete equation; therefore the areas of the figures, whose curves are defined by the complete equation, will be = $Azy + Bz^{e+1} y^{c+1} + Cz^{2e+1} y^{2c+1} + Dz^{3e+1} y^{3c+1}$ &c: and the co-efficients enter into the values of the quantities B, C, D, &c. If therefore in these values there be put every where $f = 0$, because the term $fz^{2e} y^{2c+n}$ does not enter the given equation, we shall have the values of the quantities A, B, C, D, &c; which being substituted in the series, it will give the area sought. And calculating from the beginning, it is found that

$$A = \frac{m}{m+n}; \quad B = \frac{e - \overline{m - c - n} . A + m - e}{m . \overline{c+1} + n . \overline{e+1}} \times \frac{b}{a}; \quad C = \frac{\overline{c+n} . \overline{e+1} + \overline{m-e} . \overline{c+1}}{m . \overline{2c+1} + n . \overline{2e+1}} \times \frac{bB}{a};$$

$$D = \frac{\overline{\overline{m-3e} . \overline{1-A} + \overline{3c+n}} \times -gA + \overline{\overline{m-e} . \overline{2c+1} + \overline{c+n} . \overline{2e+1}} \times bC}{ma . \overline{3c+1} + na . \overline{3e+1}};$$

$$E = \frac{\overline{\overline{m-3e} . \overline{c+1} + \overline{3c+n} . \overline{e+1}} \times -gB + \overline{\overline{m-e} . \overline{3c+1} + \overline{c+n} . \overline{3e+1}} \times -bD}{ma . \overline{4c+1} + na . \overline{4e+1}};$$

$$F = \frac{\overline{\overline{m-3e} . \overline{2c+1} + \overline{3c+n} . \overline{2e+1}} \times -gC + \overline{\overline{m-e} . \overline{4c+1} + \overline{c+n} . \overline{4e+1}} \times -bE}{ma . \overline{5c+1} + na . \overline{5e+1}};$$

&c.

From hence appears the progression in the rest in infinitum. And thus is obtained a series exhibiting the quadratures of all the figures, whose curves are defined by this equation of 4 terms, viz. $z^m = ay^n + bz^e y^{c+n} + gz^3 y^{3e+n}$. And it may be noted that the conditions of quadribility, and the number of terms in the series, that constitute any required area, are the same as the conditions of quadribility, and the number of the terms, which agree with the figures whose curves are defined by complete equations.

Corol. Besides these two series in § 2 and 4, for figures of 4 terms, in the same manner may be computed infinite other series, for other cases of figures of 4 terms. Which is also to be understood of all other figures, whose curves are defined by equations consisting of any number of terms.

There is not time now to give a minute description of the method by which I obtain these series; yet it may not perhaps be amiss to give some short account of it. Thus, I assume a series composed alike of z and y, viz. $Azy + Bz^p y^q + Cz^s y^h + Dz^l y^k +$ &c. $=$ flu. of $z\dot{y}$: of which all the terms except the first have general exponents. Then I form an equation between two values of the quantity $\dot{z}$, one of which is derived from this series, and the other is easily found, by the direct method of fluxions, from the equation expressing the relation between z and y. From the terms of this equation, properly reduced, I determine the unknown exponents, p, q, g, h, l, k, &c; and then the co-efficients A, B, C, &c. And if there be more comparisons than what are sufficient for determining these co-efficients, then from the rest I determine the conditions of quadribility. By proceeding in the right way, the calculation will be very easy. I have also many other rules relating to this matter, which may perhaps appear another time; as also the use of this method in finding finite irrational quadratures, when rational ones cannot be obtained.

Abstract of Letters sent to Sir C. H. relating to some Microscopical Observations. Communicated by Sir C. H. to the Editor. N° 284, p. 1357.

I have made proof of my new set of microscopes, made by Mr. Wilson, and have found the way of applying them very readily. The contrivance of the ivory box and hollow screw for approach, with the illuminating convex at the end of it, is of great service both by day and candle-light; also the sliders with the plain and concave glass plates for objects are very convenient. But the brass tool wants improvement, viz. a fine threaded steel screw for a more steady approach, and some new turns and motions to the arm which carries the object, &c. also a brass arm to slide up and down on the square rod of the deep microscope, to which I would fix with a screw either the ivory box, or the handle of the brass tool, to be set by that means in a fixed position, to any height or inclination required, which will be very necessary when an illuminating glass is applied to either.

As for the glasses themselves, they are very good, and well wrought, and far superior to any of Mellins; particularly the greatest magnifiers augment more, and yet show an object more distinct than his do. The greatest shows a hair of the head considerably above an inch diameter, and some eyes see it at least two inches; but supposing it a bare inch, and that, as Mr. Hooke affirms, 640 hairs breadth makes one inch, the length and breadth of an object will by it be enlarged 640 times, the surface 409600, and the solidity 262144000. But the best of our glasses must needs fall short in power and goodness of M. Leuwenhoeck's; and though some persons question the truth of his relations, yet as far as I am able to follow him (and I have tried many of his experiments) I find him always faithful in matter of fact, and therefore question not his veracity in other things.

One of the first objects I tried my glasses by, was a living louse, in which I could plainly see the motion of the muscles, when he moved his legs, which are all joined in a longish dark spot in the middle of his breast, where the tendons seem all united. The like motion of muscles is also visible in the head when he moves his horns, and in the several articulations of his legs. I saw also clearly a multitude of various branchings of arteries and veins, and the pulse regularly beating in several arteries. But the most entertaining sight is the peristaltic motion of the intestines, which is continued from the stomach through all the guts to the anus. I have observed the like peristaltic motion in a flea, and in several sorts of small transparent maggots and caterpillars. But a louse is the most convenient, as it will bear rougher handling, and live confined between two concave plates, if not crushed, for 4 or 5 days.

I thought a mite would also prove a good subject for the microscope; but found them not so transparent as I expected. However I plainly saw, that all the bristles on the body of one of them (which to a common single glass, and to the greatest magnifier of my three glassed microscope look like plain smooth hairs) were, when viewed with a large magnifier, all spicated, or bearded like the ear on the seed head of some grasses, the appearance was like fig. A, pl. 1, and every bristle on the whole body and legs, both long and short, had the same formation. But all mites are not so; for of 7 or 8 inclosed together, I found only one whose bristles were of this make, in the rest the horns only were spicated. Whether they were of different kinds, or rather only of different sexes, I cannot determine; but they were all taken out of the same cheese at the same time, and were in other parts very like. Their mouths open horizontally, to the right and left, like that of a wasp, and hard headed maggot. After being some days shut up together, some were found dead, and the survivors preying on them, which gave me an opportunity of observing their manner of feeding, which was very remarkable, for they thrust one mandible forward, and bring the other backward at the same time, and this alternately, by which means they seem to grind their food.

Having pulled off a handful of muscles, which stuck to a piece of a rock that was covered by the sea every tide; I found that the organs by which they fix themselves so firmly to a stone, that even a storm will not wash them off, were threads which proceeded from that part called the beard of the muscle, and which had on their extremity a flat spongy substance, that adhered only by imposition, like the suckers or wet pieces of leather which boys fasten to stones, and they are very well described and represented by M. Leuwenhoeck. I also viewed and examined the inhabitants of those little white shells,* which stick like pustules on muscle-shells, as also on lobsters, oysters, stones, &c. These are also mentioned by M. Leuwenhoeck, who gives a picture of one of these little animals, taken out of its shell, which is very accurate, only that in the 12 long branches growing from the head, the bristles are there represented coming out quite round on each joint of every branch; whereas they grow only on the inside, (all the hind part being perfectly bare) and look not unlike a ruffled feather stripped on one side. I cannot guess at the use of these curious ramifications, unless they serve to draw in the food of the animal, which cannot move out of its place. For keeping them alive in sea-water, I saw them often put those branches out through the slit of the operculum, which closes the top of the shell, and draw them in again.

Some of the muscles which I brought were little more than a quarter of an inch long. I took one of these out of the shell, and exposed it to the microscope on a thin plate of Muscovy glass, and holding it to the light of a candle, saw in the thinner parts a vast number of veins and arteries, and the blood circulating in them more distinctly than I ever saw it in any other animal: for I had this advantage in the observation, that the object lay always quiet, without changing place, and my plate was so thin that I could bring to it what magnifiers I pleased, and look without disturbance as long as I pleased; for whereas other animals will not easily be brought to lie still any considerable time, and will not live long when exposed to a microscope, this lay always in the same position, and the motion of the blood continued with little alteration 6 or 7 hours, only by keeping the object moistened with sea-water, and might have lasted much longer, had I not thrown it away. I repeated the same experiment, for 2 or 3 days, with some of the remaining muscles, with little difference in the success.

Observing a small worm running among some fruit, which I could perceive

* *Lepas Balanus.* Linn.

to have a multitude of legs; it was not half an inch long, and the body not thicker than a hog's bristle. This insect I put alive into a small tube, and found it a perfect scolopendra, whose body was made up of 60 incisures, at every one of which was a pair of legs, one on each side, and each leg had five articulations. On his head were 2 horns, each of 16 joints, and under it a pair of terrible forceps, red, crooked, and pointed like the talons of a hawk, and I often saw him open and shut them, and wipe his horns through them. These forceps are not unlike, and probably for the same use as those on the head of a spider, but they are hardly seen (because generally kept close) in a living spider, but they are readily found opened, and in their perfect shape, in a spider's exuviæ, or cast coat.

I found a small black flat tick, sticking on my arm, and it had got its forepart so far into the skin, that I had much ado to separate it with the point of a needle, so as to preserve it entire and unhurt. I observed its snout, fig. B B, shaped not unlike the jagged proboscis of the serra piscis: the forepart a, being like the end of a broad pointed sword, is clear and transparent, and has 3 teeth on each edge, below which there comes out another serrated part bb, on each side, almost at right angles; but this is partly hid, when viewed on the back, by a thick horn c on the side of the head: I broke off one of the horns at d, and then it appeared as in the figure, which represents the fore part of this tick.

I afterwards examined the snouts or proboscides of dog ticks, to see if they had the like conformation, and found their appearance as in fig. B 2, the snout a being so covered by the two clumsy thick horns bb, that the serrated edges could not be perceived; but separating the horns, with some difficulty, they appeared in the position of fig. B 3, and then I could plainly see 8 teeth, or jaggs, on each side, as here expressed: but the snout of a dog tick has not the additional serrated part, which is in the wood tick. I could also perceive a tube or canal run through the snout, and see some bubbles move up and down in it, which I have also endeavoured to represent, as at a in this figure.

I have found some of those animalcula in pepper-water almost incredibly minute, which appear even to my greatest magnifiers not so large as a mite to the naked eye; and in the larger sort, I can plainly see the little feet by which they perform such brisk motions, which I never could find before. I have also discovered another sort of animalcula, which are very slender long worms, of which my pepper-water is exceedingly full: they are all of the same thickness, but their lengths various, and at a medium I judge the proportion of their length to their breadth at least as 50 to 1. Even to the greatest magnifiers they look like shreds of horse-hair to a naked eye, from a quarter to 3 quarters of an inch long; on a modest estimate, their thickness is not the 100th part of a hair's breadth, and consequently if you imagine a hair of your head split into above 7800 equal fibres, each fibre would be as thick as one of these animalcules. Their motion is equable and slow, and they wave their bodies but little, though sometimes they make greater undulations. They swim with the same facility both backward and forward; so that I cannot distinguish at which end the head is, and I have seen the same worm go forward with one end, and back again with the other end foremost, above 20 times together. Sometimes they will, like leeches, fix one end on the glass plate, on which the water is laid, and move the loose part of their body round about very oddly. These I take leave to call capillary eels, and they are represented at fig. c, in the several postures I have seen them swim.

I find the dust of the fungus pulverulentus, or puff-ball,* to be the minutest powder that I ever saw: to the naked eye, when crushed, it appears like a smoke or vapour, and with a common microscope the particles cannot be distinguished: but when viewed with the greatest magnifiers, each grain is visible, and exactly alike, appearing a perfect spherule, of an orange colour, something transparent, whose axis is not above the 50th part of the diameter of a hair: so that a cubical vessel of a hair's breadth of a side, would hold 125000 of them. This was the dust of that fungus which is larger than one's two hands put together; and I observed in another puff-ball,† of the size of a small crab, that all the globules were darker, and that every one had a little tail or stalk affixed to it.

I have met with great variety of very beautiful minute flies and insects on leaves and flowers, (for at this time of the year every thing is full of animals) especially one very pretty grub, which I found plentifully adhering to nettle-leaves: it is a wonderful thin animal, has a sort of a covering all over its back like a broad shield, which it lies under like a tortoise, and is all over beset and fringed round with spikes.

A buck from accident was killed the latter end of September, which being rutting time, I thought I might with the greatest advantage observe the semen masculum. Accordingly I took out one of the testicles, with the adjoining seminal vessels, and found the vasa deferentia very turgid, and full of a milky fluid. After various methods of viewing this liquor, I saw the animalcula in vast numbers, very perfectly in several postures, moving very briskly. The greatest task was to lay them thin enough before the microscope; for when the matter is too thick, nothing is seen distinctly, only a confused motion; and

* *Lycoperdon Bovista.* † *Lycoperdon pruniforme.*

when thin spread, it dries immediately; so that you must be very quick with it. I diluted some of the semen with warm water, just so much as would a little change the colour of the water, and by that means could see them more distinct and separate, even with smaller magnifiers, and they then kept their shapes long, even till next day when put in a small tube, but were without motion. To my best glasses they appeared about the size, and in the positions here represented, fig. D.

I have seen in some water, fishes as small as cheese-mites, of different sorts, very curiously made; they are of the crustaceous kind, shelled with many joints with very long horns, fringed tails, and have many legs like shrimps; some of these carry their eggs or spawn under their tails in one bag, another sort in two distinct bags, and some kinds on the fringes of their legs, like lobsters.

The animalcula in pepper-water, represented in fig. E are very common, and are described by Mr. Leuwenhoeck in some former Transactions. The tails of some of these are 9 or 10 times as long as their body, (which is about $\frac{1}{3}$ of a hair's breadth) but generally they are 4 or 5 times as long. As they move, they will often twitch up the tail in the posture as marked at b, and this spring is so strong, as when the tail is entangled, (as commonly it is) by the end, they bring back their whole body by the jerk and convolution of the tail, which then returns to its first straightness. To a good glass, the end of the tail seems to have a knob on it as in a, and the folding appears as in b; but examining it with one of the greatest magnifiers, I found the knob to be only a close spiral revolution, like the worm of a bottle-screw, and that the whole tail when twitched up was also a spiral: this appearance, to the great magnifier, is represented in c and d. I have also seen them sometimes as in e. I have farther observed that when these lie still, they thrust out a fringed or bearded mouth, which they can draw in again, and that a rapid stream runs constantly toward their forepart, as if they drew in water; but I rather believe this current is made by a nimble tremulous motion of some minute fins or legs, which my glasses will not show.

Those animalcula marked F also abound in all the waters, and are the largest of all, and I can see them in a good light and position with the bare eye, their length being about the breadth of a hair. These have a very quick motion, and are pepetually beating about like a spaniel in a field, and by their frequent turns and returns, sudden stops, and casting off, seem to be always hunting for prey. Their bodies are very thin, that which I take to be the back being much darker than the other side, and they frequently turn sometimes one side and sometimes the other towards the eye, and often part of each may be seen. Their edges are as it were fringed, with a multitude of very minute feet, which are most conspicuous about the head and hinder parts, where are also some bristles longer than the feet, which appear like a tail: a shows one of these with the back, and b one with the belly towards the eye; and in c and d it is represented as it often appears in other positions. I put some short shreds of my hair into their water, to compare their magnitudes by, and saw that they could use their feet in running as well as swimming, for they would often stand on a hair, and go on it forward and backward from end to end, often stooping down, and bending themselves in several postures.

Among these are commonly another sort, but not above $\frac{1}{3}$ of their size, whose feet are also very visible; some of them are shaped almost like a flounder, and others are rounder behind; for by their motions and actions I judge them the same animals. These also will stand and run on a hair, or any thing in the water; they are marked ab fig. G. I have also seen them double as at c, and so go forward, like flies in copulation. I was surprised at the fist view of this, thinking it a single animal of that shape, but have since often observed them both join and separate, and two of them following a 3d, sometimes the first, and sometimes the 2d laying hold of it, and driving off the other. The little feet of these animalcules are most distinguishable when the water is just all evaporated; for, being then stranded, they cannot change their place; and at that time you may see them move their feet very nimbly, and distinguish them some little time after the water is evaporated.

I thought those which I called capillary eels had been peculiar to pepper-water; but have since observed the same, though but few, in some stagnant water which drained from a horse dunghill. This liquor was mum-coloured, and the most pregnant of all that I had ever seen, and it would seem incredible to say what a prodigious number of all sorts I estimated to be in a quantity of it of the size of a pepper-corn; for they appeared as thick as bees in a swarm, or ants on a hillock; so that I was obliged to dilute the water, to observe the particular sorts. I found in this not only almost all the animalcula, seen in the other infusions, but many sorts which I never met with before. Among them were in great plenty those which are represented in H: their extreme parts appear bright, and the middle dark, which seems beset with bristles; their tail is pointed with a long sprig at its end, and their motion is slow and waddling.

But the prettiest object was a great number of a kind of eels, which appear most distinctly when the water is almost dried, which make brisk shoots, and have a pretty wriggling motion; they are of different lengths, and are about the thickness of what I call capillary eels. Some of them are shown at K, with some of the capillary eels among them, the better to judge of their proportions.

I preserved some of this dunghill-water by me 7 or 8 days, and found the number of these little eels decreasing every day, till I could hardly find one in it, though they were as plentiful as before in the water newly taken up. And, on the contrary, I observed great numbers in the kept water, which are very scarce in the fresh. Among these is one sort very singular in its shape and motions: its body is spherical, only a little pointed like a pear, and it seems very pliable, like a bladder filled with water, in which are a vast number of dark particles in confused agitation. Their most remarkable motion is a revolving one; they will turn sometimes above 100 times, sometimes not half so fast, in a minute, the same way, and then stop and turn the contrary way, and all this without moving a hair's breadth out of their place. They will also go forward, turn and return, and fetch a large compass with many deviations, and in their progression they always, even in the shortest turns, keep their pointed end foremost, the revolving motion still continuing: and when the water evaporates, their skin breaks, and the enclosed liquid diffuses. These are of different magnitudes, and their shapes appear at L.

There is another sort, represented at M, in great numbers, which are near as long as the largest kind formerly mentioned. These have brisk motions, are very active, and have many feet before, very visible. They will often contract, and again lengthen themselves as they swim, but especially when the water dries, they will shrink themselves up into a globular figure, and the feet then stand out, which may be seen to move very nimbly a considerable time after. These also are of different sizes: aa show them at their length, and bb represent them contracted.

Fig. N represents another animal, not uncommon among the rest, which is as large as the former, and in its motion, which is very nimble, keeps always the sharp end foremost. I have observed some variety in these, though I take them to be of the same species; some of them being clear, and curiously striated, from the point to the thick end; others only having a fore-part clear, and the bottle dark, as shown at a and b; but I cannot by any glass find the organs by which they move.

These now mentioned are the most remarkable for their size and motions; but there are a great variety of others, which I pretend not to describe, and cannot draw. In one I found a curious mechanism in a small diving insect, found in standing waters. It is like a small fly, with a head like a house cricket; but, instead of wings, it has 2 paddles on the shoulders, and on the end of the hinder legs, which are longer than the other 4, instead of feet and claws are perfect oars. I have also observed in 2 or 3 sorts of flies, that behind the eyes, on the top of the head, are placed three protuberances, with a black shining globe in each, like a ball in a socket, and are so disposed, as if made to look directly backwards. They are perfectly smooth, and without those hemispherical divisions, visible in the cornea of the eyes of the fly and beetle kind, but appear more like those of a spider.

I have tried several ways of killing the animalcula before-mentioned, by mixing salts, spirits and acids, &c. with their water, the least touch of which will immediately deprive them of motion and life. But I never yet succeeded in any trial of recovering or reviving them, after the water was evaporated, by the addition of fresh water. Many of those I have mentioned burst when the water evaporates, and though some keep their shapes a little while, yet they too alter in a few minutes, and I cannot imagine them recoverable.

An Account of a Book, viz.—Hippocratis Aphorismi, cum Commentariolo. Auctore Martino Lister, e Medicis Serenissimæ Majestatis Reginæ Annæ, Lond. 1703. N° 284, p. 1373.

Concerning the Cure of an Aposthumation of the Lungs. By Dr. John Wright. N° 285, p. 1378.

Mrs. Jane Terry was about 18 years of age, of a fresh complexion, and rather fleshy. Her relations apprehending she might have the small-pox, removed her to a nurse's house, where she had the distinct sort very kindly; her case proceeded so very well, as they conceived, that no physician was called to her, till they began to shell; only, that for some days before she had a little difficulty in her breathing, which gradually increased, till she began to throw up some blood, which was about the 7th day from their first appearing, and was accompanied with these circumstances; it had increased every day; for 3 days before I saw her, she coughed and brought up a viscous phlegm, such as patients vomit when their stomachs are very foul; only as mere phlegm is white, this was all of it as red as blood; it was not streaked with blood, or had a mixture of white phlegm with it, but was so deeply coloured, that it seemed to be all blood, only it would not flow as blood does while it is hot; nor did it coagulate as blood does when it is cold, but hung from the basins when it was poured out, as vomited phlegm does; and in this it differed from all the bloody expectorations I have seen, excepting one Mr. Jones at Kensington, who coughed the same bloody coloured pituita, but in much less quantity, for Mrs. Terry threw up above a pint in 24 hours, for some days, and though a less, yet a considerable quantity afterwards; Mrs. Terry's had a very strong smell, but Mr. Jones's had none at all. After some weeks she recovered, regained her flesh, which was wasted in her illness; the menses returned, and she continued very well from July till near Christmas.

In January I was called to see her, and she gave this account of herself. That about three weeks before Christmas she found herself a little short winded, which increased daily, with a fulness and weight in her left side; that she lay well on the left side, but when she turned to lie on her right, she felt as if a weight fell from the left to the right side, which gave her a shortness of breath and made her cough. Thus it continued increasing till Christmas, when she began to raise a considerable quantity of strong stinking pus; she said she ate her victuals well enough all this time, and was not feverish. At first she was under the care of another physician, who told her relations that it was an ulcer in her lungs, and he believed incurable, upon which they again committed her to my care. When I saw her, which was towards the middle of January 1701, she threw up a considerable quantity of stinking offensive pus, which was as fluid as the pus of other parts; her flesh was a little abated, but she was at no time feverish; she ate and slept pretty well, and had the catamenia duly. I prescribed such medicines, as abated that purulent expectoration several times, and she often gave me hopes of her recovery, continuing to have the menses regularly, and being still free from an hectic, but on every little cold, she again raised that fetid pus in a considerable quantity. She generally continued pretty free from coughing several hours together, till she perceived something of a fulness in her breast, which would oblige her to cough, and after she had once begun to raise, she could not cease, till she had brought up two spoonfuls or more of that fetid pus. This she did chiefly in the morning, afternoon, and at night. I apprehended she had an abscess in the left lobe of the lungs, and made her lie on the bed, with her head reaching to the chamber floor, leaning on her left arm. In this position she could at any time, after a little cough, set the pus a running out of her mouth till the whole was discharged: then she would get up, and seemed to be as well as another person, till it was almost filled again.

In the beginning of May last, Drs. Torlesse and Pitts favoured me with their kind assistance. They saw her lie in the position I have described, and saw more than 2 spoonfuls of stinking pus or corruption run out of her mouth, after a little coughing. This made it so apparent that there was an ulcer in her lungs, that they immediately approved of what I had before proposed, the making an aperture in her side, where we could apprehend the lungs grew to it, for that seemed unquestionable, from the posture of discharging the matter, and from some little pain she felt in her side. About a week before, the pus had begun again to increase, and she was taken with a chillness, after which her pulse became a little quicker, and she was rather feverish when the caustic was applied; this feverish state increased every day, and after some days a rash appeared, which lasted about 14 days before it was quite got off, and left her in a hectic, with redness in her cheeks towards evening, night sweats, continual looseness, extreme wasting of her flesh, and at length a swelling in the legs, though she kept her bed. We felt some little knots between the 7th and 8th rib, which, with other circumstances, made us conclude the adhesion was in that part, and would have laid the caustic there, but that it would certainly have spread to the glands of her left breast, which made us lay it between the 6th and 7th rib, reckoning upwards; as soon as could be it was taken off, and Mr. Cowper gently passed his knife through into the cavity of her breast, whence issued a bloody water, but no pus; by bending his probe he found the adhesion reached to the lower edge of the 7th rib; and before the eschar was separated, the pus began to flow at every dressing, and so continued, gradually abating, till the ulcer was cured; during which a part of the inside of that rib, above an inch long, exfoliated, and after that another lesser piece of the outside of the rib. Towards the latter end of the cure, she complained very much of a pain at the cartilago ensiformis, which was so great that she sometimes plucked out the hollow tent, which we conceived was occasioned by its pressing upon the nerve. During the first 7 or 8 days of her rash, she raised very little, if any of that pus, nor did it discharge itself then by the orifice, nor was there a collection of it in her breast, which made me apprehend, that the fever did so alter the state of her blood, as not to permit it to separate its impurities into the abscess. For 6 days before the fever began, she had the catamenia very orderly; by August she was cured, her side healed up, for she would not suffer it to be converted into an issue; by October she recovered her flesh, and the catamenia returned, which had been wanting ever since May; and now she is plump, fleshy, clear and fresh complexioned, has little or no cough, and no fœtid or tabid expectoration, and seems, and I believe is perfectly cured, having for many months taken no medicine.

There are several circumstances in this case, which I cannot forbear making some remarks on. That there was an ulcer in the lungs, and that it has admitted of a cure, contrary to the general opinion of physicians. That this ulcer contained at least two spoonfuls, and must have been as large as a hen's egg. That this abscess arose from a collection of purulent matter, with an indiscernible, if any, fever at all; and so continued from Christmas till about the 10th of May. The tender membranous or vesiculary composition of the lungs seems to justify this opinion, that it is almost impossible for them to heal, when there is a considerable diminution of them, the continual and indispensable necessity of their motion very much hindering the coalition of the vesiculæ.

Several parts of the body afford a proper cement to unite and repair them, when hurt or diminished. Carious and broken bones send forth a callus; when the skin is consumed by ulcers or burns, the parts afford a cicatrix, which pretty well supplies the defect of the skin. The lungs separate a viscid pituita, which will be expanded into flakes like a membrane; Mr. Stringer, Sarah Deeping, and some other patients, have brought up great quantities of them; and a little boy at Kensington coughed up several pipes, formed exactly like the bronchia, and its divarications, and at first view seemed to be the internal membrane. This child, two years before, had an ulcer in the right side of his lungs, and they adhered to his back; when I separated them, I found a cicatrix near 3 inches long, but very little, if any defect in his lungs. I am of opinion this pituita or mucus serves to re-unite the parts of the lungs, when there is a solution by an ulcer.

Consumptive persons generally flatter themselves, that they have no ulcer in their lungs, because they do not feel a soreness, as in the ulcers of other parts; and this opinion keeps them from making a timely application to physicians, while they might receive a speedy and easy cure. When Mr. Cowper touched the sound or ulcerated parts of the patient's lungs with the probe or finger, she discovered no sense of feeling, which may confirm the opinion of physicians and anatomists, that the lungs have little if any sensation. But when he touched her heart with his finger, though not for the 20th part of a minute, she grew very much disordered, pale and ready to faint; which shows that nature cannot suffer the least alteration in its pulsation, without great prejudice and inconvenience.

It is the opinion of some physicians, that the fever which attends consumptive patients, arises from some particles of the pus, which being received into the blood, and circulating with it, causes that effervescence which we call a hectic. This patient had no fever from Christmas to May, and then there came a continued fever, with a rash, which left her feverish every afternoon, with those symptoms which attend a hectic.

I have observed for many years, that if I could preserve my consumptive patients from that hectic fever, or relieved those who already laboured under it, I could cure them, though their expectoration was very plentiful and foul. I do not doubt but some part of her lungs adheres to her side, and it is probable that a small portion of them does not receive the air in inspiration; but I believe that defect is very inconsiderable, because she can run up stairs, and is no more disordered in her breath than most other people. The easy discharge of the pus, by her lying down in that posture, did undoubtedly very much preserve her lungs, and prevented its breaking through the abscess, into the cavity of her breast, and putrefying her lungs to a greater degree. Pus generat pus is a noted aphorism; and the air-bladders of the lungs are so very tender, that they must have yielded to the pressure of the pus had it lain long in the abscess, and been only discharged by violent coughing. By lying in a proper posture, Sir Tho. Proby, Sarah Deeping, and others, have prevented a greater solution in their lungs, and either prolonged their lives many years, or recovered their healths by proper medicines.

This and other instances, make me easily concur with some physicians in an opinion, that in some families the lungs have originally a more tender constitution than in others. Mrs. Terry's aunts are subject to great coughs and phthisic. Her aunt Fowke's little daughter, of about 7 years old, having a little fever, with some symptoms of the small-pox, but a great difficulty in breathing, I advised her bleeding at the arm, but she was so fat they could not find a vein that night; next day the small-pox appeared, which eased her lungs a little; on the 6th night after, she raised at several times about 7 or 8 oz. of clear blood, by violent coughing; I was sent for and drew away about 6 oz. of blood by leeches, which totally relieved her breath, and stopped the vomiting of blood. This instance, being so like Mrs. Terry's, confirms what I said before, that so great a loss of blood will not hinder the regular proceeding of the small-pox, which she went through very regularly, and recovered perfectly. I have observed the same effect in other patients, in whom bleeding after eruption was indispensable.

The continual motion of Mrs. Terry's breast in breathing, made the caustic spread farther than it was designed, or could be prevented; I have prevailed with some patients to yield to an apertion by lancet, which I take, in some cases, to be the better way; that little blood which may possibly get into the cavity of the breast is easily thrown out again by the lungs in inspiration, as she did the injection every time it was used.

After the pus began to flow at the orifice it lessened the discharge at her mouth, and in a moderate time the purulent expectoration totally ceased.

The same Case as the Foregoing, of the Cure of an Aposthumation of the Lungs. Addressed to Dr. Wright. By William Cowper, F. R. S. N° 285, p. 1386.

The matter or pus which first flowed from Mrs. Terry's side, was so offensive n its scent, as obliged the by-standers to quit the chamber; and the nurse, usually at the time of dressing, and afterwards, was wont to burn rosemary, &c.

to suppress the stench. So putrid was the pus, that it tarnished that end of the silver probe I passed into the cavity of the abscess, as it did the top of a silver syringe in making injections. There seems no room to doubt that the pus, which then flowed from her side, came from the same cavity as the pus which she before coughed up, when the liquor that was injected at her side came into her mouth; which she frequently complained of, and particularly of the bitterish taste of the tincture of myrrh which I sometimes used in the injections.

Among the many disorders that affect the parts of human bodies, those of the lungs have been considered as none of the least dangerous; and indeed, if observations did not assure us of the possibility of success, the commonly known structure of the lungs would afford us but mean arguments for the shift nature makes, in the instance you have given so exact a description of, as well as some other instances of the like nature I have met with.

About two or three years since, I saw a boy in the 9th or 10th year of his age, who, for some time after a continued fever, was attacked by an intermitting one; a cough ensued, in which he brought up at short intervals, no small quantity of thick purulent stinking pus, which discharge continued on him no less than 14 or 15 months before I saw him; his physicians ordering him issues in his back, I made them as usual; he had then a healthy aspect, his cheeks florid, and was very brisk and active; when he just came from play, he was bid to take a basin in his hand and cough as he was wont, which he did, wherein I saw him discharge at his mouth not less than 4 or 5 oz. of the sort of pus above mentioned; this his mother told me he had been wont to do twice every day; nor did he appear anywise disordered after, but returned to play immediately. His physicians sent him into the country, whence he came, where in about a twelvemonth I heard he died, but was not acquainted with his circumstances after; what success the operation we practised on Mrs. Terry would have had on this boy, I dare not determine; though I cannot but think it might have been safely performed on him, and on another patient I was since called to; but I could not obtain the consent of the physician that was consulted.

Another instance, in which a considerable part of the lungs was obstructed, and consequently became useless some time before the patient's death, was in a girl of 16, who had been scrophulous for 9 years; the glands about her neck and throat being very much indurated, as well as distended, her lips and nose were also swollen; about a year and a half before her death she coughed up 7 or 8 oz. of fetid pus, in less than 24 hours. On changing the air of this town for that of the country, together with the use of balsamic pectorals, she recovered a healthful look in her face, but continued somewhat asthmatic. On taking cold, her appetite as well as digestion failed her, she became feverish, and died after a few days indisposition. On opening the thorax, I found the lungs adhering to the pleura of the left side, in such manner that they could not be separated without laceration. A portion of one of the left lobes of the lungs, being cut off, sunk in water; from which part it was likely the matter came, which she formerly coughed up, though the ulcer was then closed, and no appearance of matter was to be seen in that or any other part of the lungs. The lymphatic glands, at the divarication of the windpipe, had by their intumescence so compressed the canal of the left side, that it wanted more than two-thirds of its proper passage for the air.

In these, and some other instances which I could produce, it is evident, that considerable parts of the lungs may be obstructed, and yet the person survive: but Mrs. Terry's case demonstrates the possibility of their recovery, when part of their lungs is totally obstructed, as must happen in such large abscesses. But how the remaining sound parts of such diseased lungs become capable of transmitting the whole mass of blood from the right ventricle of the heart to the left, in equal time and quantity with the blood that circulates in the rest of the parts, seems not easy to account for. Since I had often found water, injected by the arteria pulmonalis, return readily from the lungs again by the vena pulmonalis, I was tempted to try if melted wax, when very hot, would not do the like; which accordingly succeeded in the lungs of two young cats; for after injecting the wax, mixed with oil of turpentine, and tinged with vermillion, by the arteria pulmonalis, I found it had filled the pulmonic vein with the left auricle, insomuch that some of the wax had reached the left ventricle of the heart; some of the wax was extravasated, and came into the bronchia and windpipe at the same time.

In preparing a human heart, by filling its ventricles, auricles, and trunks of its large blood-vessels with wax, I found, on injecting the pulmonic arteries and veins with wax differently tinged, that the wax passed from the veins to the arteries without coming into the bronchia, or being extravasated, though the wax was not injected with near so much force as it might have been. I was never able to make wax pass from the arteries to the veins in human bodies, or quadrupeds, unless in their lungs as above noted, and the spleen and penis; nor did it happen in those parts except when the wax was impelled with great force, though I have constantly observed the communication of arteries and veins of the spleen and penis more open than in other parts, except the lungs. I wish Dr. Morland had told us* in what part of the human body Dr. Areskin had made wax pass from the arteries to the veins, so as to demonstrate their continuation

* Philosophical Transactions, N° 283.—Orig.

to the naked eye, because I have hitherto found the naked eye unable to discover the extremities of the arteries and veins when the blood itself was moving in them, in the transparent parts of the omentum or mesentery of quadrupeds, or in the lungs of live frogs or lizards; or after death, when the blood has been retained in their lungs in the following manner: on making an incision into the bodies of these animals their lungs will start out, and be distended with inspired air; on these as quick as you can pass a ligature, i. e. a waxed thread, and tie it firmly towards the upper part of the lobe, as near the heart as possible; when the lungs of frogs and lizards are dried, thus distended, you may examine them with the microscope, and they will appear as represented, plate 2, fig. 1st, 3d, 4th, 5th, and 6th. The 1st and 2d figures show the difference in the magnitude of the extremities of the veins and arteries of the lungs, and those of the foot of a frog viewed with the same microscope.

Hence it appears that the communications between the arteries and veins of the lungs are more open than those of other parts, at least in the feet of frogs; and till it can be shown that melted wax can be as easily injected from the arteries to the veins of other parts in a human body and quadrupeds, I shall be inclined to think the communications between the pulmonic arteries and veins in general are more open than the arteries and veins of other parts, excepting the spleen and penis. This patent communication of the arteries with the veins of the lungs, shows how those vessels transmit the blood in equal time and quantity, with the blood that moves in the rest of the blood-vessels of the whole body, in a healthful state. And hence it is, that when any of the blood-vessels of the lungs are straitened or totally compressed, the remaining unobstructed blood-vessels are forced to discharge more than they were wont, and in time those vessels become sufficiently dilated to supply the defect. The like happens in the communicant branches of the arteries of any part, when some considerable branch or trunk is tied up, as in the operation for curing the aneurism mentioned in N° 280.

Thus we find that the structure of the parts of animal bodies, is not only sufficient to perform the ordinary operations of nature, but their organs are so wonderfully contrived, that notwithstanding considerable parts of those organs are obstructed, yet their neighbouring parts, as the blood-vessels in the case before us, become capable of supplying the defect. And this indeed exacts our gratitude as well as admiration of the Divine Architect.

Explanation of the Figures.—Fig. 1 represents that part of the 5th figure at D. . . . . . done by a larger magnifying glass; A the arteries; B the veins of a frog's lungs, prepared as above mentioned; C their inosculations with each other; D. . . . . . the area of the microscope, as it appears to the naked eye.

Fig. 2 represents part of the hinder foot of the young frog, viewed with the same microscope when living; by which the different magnitude of the extremities of the arteries and veins of the lungs in the first figure, and in this expressed at C C, is very evident; the former being capable of admitting at least three globules of blood to pass abreast, whereas the extremities of the arteries and veins in the feet admit of one globule of the blood only to pass before the other; A A the trunks of the arteries; B B those of the veins lying by the side of the toes; C C their extremities continued with each other, in the transparent membrane between the frog's toes; a a two of the frog's toes.

Fig. 3 and 4 represent the extremities of the arteries and veins of a frog's lungs, viewed with the 4th glass of the same microscope; A A the arteries: B B the veins; C C their conjunctions with each other; D the area of the microscope.

Fig. 5 represents one of the hexagonal area of a frog's lungs, which were not so much distended by inflation as those parts of the lungs represented in the two former figures 3 and 4; by which the little areas or cells in the interstices of the extremities of the veins and arteries appear closer, and less than in the two foregoing figures, though viewed by the same microscope; A the arteries; B the veins; D the area, which is more magnified at fig. the 1st.

Fig. 6 represents the lower part of one of the lobes of a water lizard's lungs, as it appears by the microscope, when the blood is retained in the extremities of the vessels, as in the preceding figures; A A the trunk of the pulmonic artery; B B the vein; C C.... their branches, joining with each other; D D the transparent smooth membrane, which in this animal is not vesiculated, or full of cells, as in the lungs of frogs, on which the blood-vessels are expanded; nor does the internal surface of this membrane differ from the external, as in frogs and divers amphibious animals, the lungs of these water lizards being vesicated, and not vesiculated.

George Joseph Camelli's Observations on the Birds of the Philippines. Communicated by James Petiver, S.R.S. N° 285, p. 1394.

This list contains a description of 71 different kinds of birds of the Philippine isles.

Some Remarkable Curiosities in Denmark and Holland. By Dr. Wm. Oliver. N° 285, p. 1400.

The colleges and schools in Copenhagen, both as to their structure and foundations, are very ordinary; the best I saw there was the gift of the learned Borrichius, and consists of 12 apartments, for as many students, conveniently provided with stoves for their manner of living in that cold country. There is in it a little school for public exercises, and a small library belonging to their college; the rest of the students, except 100 maintained by the king in small colleges, live at lodgings in the city, as at Leyden and other universities abroad. There are in this university, erected about 200 years since, about 1000 students, and sometimes they have had near 1500.

The university library, over one of their churches, in one large room, consists of several libraries, the gift of particular men, which are kept apart, with the benefactors' names over them in capital letters. The royal library, belonging to the king, consists of a great variety of books, in good condition, and well chosen, in all languages: the books of each country being placed by themselves; the room is large and well built, and has a large gallery supported by pillars on each side. If Gudius's library at Gluckstadt be added to this, as I was told it will be, this library may be reckoned in the first rank of the European libraries.

The king's chambers of rarities are in all 8, large and well built, over the royal library; they are furnished with great variety of natural and artificial curiosities. The first contains coins and medals, gold and silver, modern and antique; Grecian, Roman, Danish, and Oldenburgh, besides those of other European nations, distinctly kept by themselves, making altogether a very valuable collection. Among the natural curiosities which are in the other chambers, the most remarkable are:

1. The petrified child: being the same that Bartholine mentions in his Hist. Anat. of which Paræus, Licetus de Monstris, &c. give the history at large. This child was cut out of its mother's belly at Sens in Champagne, Anno 1582, where it was supposed to have lain about 28 years. That it is a human fœtus, and not artificial, is visible to the eye; the upper part of it is of a gipseous nature, not so hard as the lower, the thighs and buttocks being hard and perfect stone as can be, of a red colour, and of a grain and superficies exactly like those taken out of the bladder. I had the curiosity to have it near me, and touched and felt it all over. It was first conveyed to Paris, where it was bought by a goldsmith of Venice, of whom Frederick the Third, King of Denmark, purchased it, and added it to his rarities. 2. Two elephants' teeth, weighing 150 lb. each. 3. Several heads of hares, with divers sorts of horns, brought out of Saxony. 4. An egg, said to be laid by a woman, of the ordinary size of a pullet's egg. This Ol. Wormius tells us was sent him by very good hands, and confirmed by people of credit; he further tells us, the woman brought forth two eggs, with the usual child-birth pains, the neighbours being called in to her assistance, they broke the first, and found a yolk and a white, as in that of a hen; the second was preserved and sent to him. Vid. Mus. Worm. p. M.

312. Jo. Rhod. Cent. 3. obs. 57. 5. The horn of a sea unicorn, or monoceros, so called by the learned, because supposed to have but one; but sometimes more are found to grow out of the upper jaw; and I was credibly informed at Copenhagen, that one Koens, a Hamburgher, brought home from Greenland the head of one of these with two horns on it. 6. Several pieces of gold ore, very rich, and some almost fine, dug out of the mines of Norway and Hungary. 7. Several large pieces of silver ore, also dug out of the mines of Norway, one of which weighed 560 lb. Anno 1666, 5 feet and 6 inches long, and 4 feet round, valued at 5000 crowns; another piece from Norway, also valued at 3272 crowns. I observed a great many silver threads or trees sprouting out of these two pieces, and fancy the whole mass had not above a fourth part of any baser metal, mineral, or earth, mixed with it. There are several of these silver ramifications or trees, all from Norway, which I take to be virgin silver, and pure metal. 8. A great variety of very large pieces of amber, some dug out of the island of Anaker near Copenhagen, and some when they were opening the ditches for the fortifications of Copenhagen; some of these weigh 40 or 50 oz. and were found every where sticking fast to pieces of black timber like ebony. 9. A numerous collection of very large branches of coral, white and red, and one black. 10. A large pair of stag's horns growing out of a piece of wood, after a very strange manner. 11. Another branch of a stag's horn, with a large piece of wood naturally adhering to the top of it, the top of the horn piercing the very body of the wood, and growing some inches beyond it. 12. A sheep, with a horn growing out of its side, about a foot long. 13. I measured the thigh-bone of a human body, that was 3 feet and 3 inches long; the head was 2 feet and 5 inches round, and the middle of it was $19\frac{1}{2}$ inches about. 14. Two very large scallop shells, that weigh 224 lb. each. 15. A piece of marble, with a natural representation of a crucifix on its outside, highly valued by the Lutherans.

There were besides these a great many more natural rarities, which my short stay there would not permit me to examine.

Among the artificial curiosities I observed, 1. A skeleton made of ivory, in imitation of a human skeleton, 2 feet 6 inches long, so artificially and curiously put together, that any one not well skilled in osteology may take it for a natural skeleton. 2. Two crucifixes of ivory, with the whole history of our Saviour's passion, extremely well carved and expressed. I saw there a great variety of toys made of ivory, curiously wrought on the outside; as, a small man of war in ivory, with silver guns, several small sloops and galleys of the same, and one made of the unicorn's horn, with a gold anchor. 3. A watch made of ivory, with all its wheels and motions. Several other utensils made of ivory and unicorn's horn, neatly carved and turned; besides some others of ebony, heart-oak, box, amber, silver, brass, &c. 4. In another chamber there is nothing but the garments, arms, and utensils, of Indians, Turks, Greenlanders, and other barbarous nations, in great number and variety. 5. A perspective of the late King of Denmark's family, the queen's face being in the middle, and eight princes and princesses round her, yet all conspire to form the king's face through a hole of a glass tube. 6. Six golden sepulchral urns, found in the island of Fuenen, Anno 1685, by a boor, as he was ploughing his land; when found they were full of ashes or a greyish earth. The largest weighs 2½ oz. the other five about 2 oz. and a drachm. This confirms the accounts given by Saxo Grammaticus, Olaus Wormius, and others, that it was a very ancient custom among the northern nations to burn their dead, and then bury their collected ashes in golden urns. They were very thin, and had three rings of gold round their necks, and several circles one within another, with one common centre, carved on the outside round the body of the urn. They held between 4 and 5 oz. of liquids. 7. Another sepulchral urn of crystal, of a conical figure, found near Bergen in Norway, about 30 years since, with a gold wire about it, that weighs 8 oz. 8. I saw in another chamber several urnæ lachrymales, in which were collected the tears of friends, which afterwards the old Romans mixed with the ashes of the dead; these were some of glass, and some of earth, and of several sizes. Brass lamps of several magnitudes and shapes, some of other metals, others of earth, in the shape of animals or idols, that were worshipped by the ancients. The stylus æneus of the Romans, the one end sharp, to write with, the other blunt, to rub out what was written. 9. The large Danish horn of pure gold, weighs 102¼ oz. is 2 feet 9 inches long, and holds about 2 quarts English measure. This horn was accidentally discovered by a country girl, Anno 1639, in the diocese of Ripen in Jutland; it is doubtless some Runic piece, of great antiquity by the figures carved on the outside, which seem to be hieroglyphics, monstrous shapes of devils, hobgoblins, &c. perhaps some of these might represent their gods, and probably this horn was used in their sacrifices, as of old among the Assyrians and other ancient nations, who were wont on solemn occasions to entertain the croud with mighty noises of horns and trumpets, or rather to drink out of at their solemn treats. 10. The Oldenburg horn of pure silver gilt with gold, and variously enamelled with green and purple colours, and weighs about 4 lb. The Danish antiquaries tell strange stories of this horn. I found in the same chamber a great many horns of this kind, some in metal, some of bullocks' horns, tipped with gold about the edges, others of ivory, unicorns' horns, &c.

In the tower of Copenhagen I saw Tycho Brahe's own celestial globe, which was 6¼ feet in diameter. This tower was built 1601, for making astronomical observations, near the Royal College in Copenhagen: it is above 150 feet high, and its area on the top is 60 feet diameter; the passage up to it is large enough for two coaches, and the ascent so easy and hardly perceivable, that it served for a place of parade for their gentry, when they had a mind to take the air in their coaches, riding up to the top, and so round the ring, as well as for an observatory. But M. Romer has converted the upper part of this tower now to other uses, where in a dark room he has his instruments for observation. Here I saw his machine for observing the stars by day; there is a pole 8 or 10 feet long, erected perpendicular in the centre of an equinoctial plain: on the top of this pole is fastened a telescope, not much above 3 feet long, which runs through the roof of the chamber, whose elevation is directed by an astronomical dial on the equinoctial plain, with an index fitted to it for that purpose, which determines it to the star he has a mind to observe at any time.

It is said he has invented two other machines of great artifice and use; by the one he will show at any time the station of any planet, according to the Copernican hypothesis; by the other he will demonstrate all the eclipses of the sun or moon, past or to come.

There is in the king's house, in the garden at Copenhagen, a royal throne, all of unicorn's horn, on which all the kings of Denmark are seated at their coronation.

An English gentleman showed me once in Holland, in the year 87, a cherry stone, with 124 heads on the outside of it, so that you might distinguish with the naked eye, popes, emperors, kings, and cardinals, by their crowns and mitres. It was purchased in Prussia, where it was made, for 300*l.* English, and is now in London, there having been a law-suit not long since commenced about it in chancery.

Gazophylaci Naturæ et Artis; *Decas* 1. *In qua Animalia, Quadrupeda, Aves, Pisces, Reptilia, Insecta, Vegetabilia; item Fossilia Corpora, Marina, et Stirpes Minerales e Terra eruta, Lapides Figura insignes, &c. Descriptionibus Brevibus et Iconibus illustrantur. Hisce annexa erit Supellex Antiquaria, Numismata, Gemmæ Excisæ et Sculpturæ, Opera Figulina, Lucernæ, Urnæ, Instrumenta varia, Inscriptiones, Busta, reliquaque ad Rem priscam spectantia; item Machinæ, Effigies Clarorum Virorum, omniaque Arte producta. A Jacobo Petiver, R.S.S. Lond.* 1702. N° 285, p. 1411.

The author of these decades in natural history procured at his own private expence many and great collections of plants, quadrupeds, birds, fishes, insects, shells, figured stones, and other fossils, from his correspondents in the East and West Indies, in Africa, and most parts of Europe. Some specimens of which he gave in his Ten Centuries, and here gives a publication of others with elegant and lively icons, besides explications, by way of decades, which will convey a most clear and lasting idea of the things themselves, and leave to posterity many noble materials towards raising the immense structure of nature, which is not to be carried on without the joint stock of all ages, and the generous contributions of such as think fit to promote the liberal arts and sciences.

Abstract of a Letter to Dr. Edward Tyson, from the Rev. Mr. Charles Ellis, giving an Account of a young Lady, born Deaf and Dumb, taught to Speak. That Coster first invented Printing, Anno 1430. *Of the Physic Garden at Amsterdam, and the Chamber of Rarities at Boln. Of a Monstrous Birth. Of the Quarry at Maestricht. Fr. Linus's Dials at Liege. The Cachot or Rooms cut in the Rock of the Castle in Namur. Sir Jo. Mandeville's Tomb at Liege. And the Friesland Boy with Letters in his Eye. Dated Bruxelles* $\frac{4}{14}$ *July,* 1699. N° 286, p. 1416.

At Haerlem I visited Van Dalen, the author De Oraculis; he is setting forth another piece De Sacerdotio Veterum, a learned and instructive work. He showed me besides his own, a curiosity of a young lady born deaf and dumb, yet taught by Dr. Amman to speak very intelligibly; she is about 17 years old; and I heard her read Dutch and Latin.

The first book printed by Coster, I find is not Donatus, as the Inscriptiones Hollandicæ say, nor Virgil nor Tully's Offices, as others have reported; but a Dutch piece of theology, printed only on one side of the paper; and after this is a single page of Latin, entitled Liber Vitæ Alexandri Magni, which made some believe it to be Q. Curtius, but it is a monkish Latin of that time. This and the theology were printed in 1430; whereas the Inscriptions and some other authors have told us from Coster's picture, that printing was invented by him in 1440. But a picture of Coster before another Dutch piece, bound up in the same volume, and printed 1432, bears the date of 1430, under which picture is the inscription mentioned by Mr. Ray, only the date 10 years sooner, and the Tetrastic, which is transcribed by the author of the Inscriptions from an effigy of Coster, which was then extant in the garden in this place, but is not now to be found.

I saw at Amsterdam a curious physic garden, admirably furnished, and in excellent order, which, as having more space, and more foreign plants, far exceeds that at Leyden; here are also series of lectures. At Boln is a hall well furnished with rarities, considering there is no university, schools, nor gardens, nor any professors. Here occurred a rarity, not then publicly exposed, two female children joined together from the neck to the navel; their picture was with arms embracing, and legs twisted, all parts and joints entire to both; the viscera also all double and perfect, the head only single, but appertaining to both, and looking over the right shoulder of one and the left of the other. They were born alive before 7 months.

At Maestricht I went into the quarry, of which you have an account in the Transactions, but it is more curious than there described, and larger, being 3 hours in length, and 1 in breath, and capable of sheltering 100,000 men. It cost me a fit of an ague, through its excessive chilness. The stone dug from hence is much like our kettering; the Jesuits have here a very fine chapel built of it.

Franciscus Linus's dials at Liege, the original of those formerly in our Privy Garden at London, are shamefully gone to decay, none remaining of use, but that which distinguishes the hours by feeling, and the globe which shows it in all other parts of the world.

At Namur there are no curiosities but such as are military, except only the cachot cut in the rock of the castle, with apartments for 600 men, and all rooms of use, as kitchens, &c. This was done by the order of Marshal Boufflers, to defend the garrison from the bombs, and was the labour of 4 years. At Liege is Sir John Mandeville's tomb, whose epitaph is also at St. Albans with us, which may be difficult to be reconciled.

At Brussels I saw a young Friesland boy, of about 5 years old, round the pupil of whose eye they pretend is naturally engraven Deus Meus, and the same in Hebrew. This is considered as a prodigious miracle in those parts; but on nicely surveying it I could perceive it was only the iris of the eye, not circularly joined, but lasked out into fimbriæ, which here and there might be thought to form some imaginary letters, as beginning at the lacrymal corner of the left eye, there is something like D, and I, and V, but not a trace for the strongest fancy to work out any more, nor any letter of Hebrew in the right eye, as they pretended. I do not doubt, but as the boy grows up, the others may conjoin again.

At the Hague there are very few natural curiosities, but there is a piece of art, which of its kind I believe never was paralleled, which is performed by one Elizabeth Pyberg, who cuts in paper not only towns, as Loo and Hounslerdyke, but faces to an extreme likeness; she has done King William and Queen Mary better than any limner I ever saw, and refuses 1000 gilders for the pieces; it is so curious that I could not believe the queen's drapery not to be point, till I had most carefully inquired into it.

A Description of some Corals and other curious Submarines, lately sent to James Petiver, F.R.S. from the Philippine Isles, by the Rev. George Joseph Camelli; Also an Account of some Plants from Chusan, an Island on the Coast of China. Collected by Mr. James Cuninghame, F.R.S. N° 286, p. 1419.

Among other vegetables described in this catalogue are the camphor-tree and tea-shrub.

Observations on some Animalcula in Water, the Solution of Silver, &c. By M. Ant. van Leuwenhoeck. N° 286, p. 1430.

About the latter end of July I saw something shining in the water of the canal here, at Delft, and being desirous to know what it was, I took a glass-tube of almost a foot length, and of a finger's breadth, and tying a string about it, I let it down into the water perpendicularly, till it was quite full. Viewing this water with a microscope, I observed several sorts of animalcula swimming in it, some of which I had never met with before. I viewed this water several times in one day, but could not find out the shining matter which I had seen in the canal. But on the 4th of August observing again, I could see in 8 or 10 places several small particles, sticking so fast to the sides of the tube, that although I put the water into a gentle motion, I could not remove them. This induced me to view the same with my microscope, and then I could plainly see those particles, representing a large bough or branch of a tree, with a great number of sprigs about it. Most of these branches were fastened with their stems to the sides of the tube, and seemed to arise out of a very small particle of matter that was likewise so fastened.

For more satisfaction, I took another tube, longer and broader than the first, well cleaned, and filled it with the same water. I viewed this water also several times, after it had stood 30 hours in the tube, and again discovered the same sort of boughs, as complete and perfect as if you were to see the same with your naked eye on a tree; and among others one in such a position, as if the thick branches from whence the rest proceeded, lay just against my eye. I placed a microscope before this last particle, with a design to observe from time to time whether it would increase in size. After 6 hours I could not find that it was grown any larger; and I observed that the ends of the twigs which were about 20, were laden with small transparent bubbles, whose diameters were thrice as large as the extremities.

Having now also observed about the twigs a sort of transparent animalcula, of the same size with those bubbles, I supposed that the before-mentioned round bubbles were the same with those animalcula that had taken their station on the ends of the twigs; and I was confirmed in that opinion when I perceived the next day in the morning, after the water had been 48 hours in the glass-tube, that most of these round bubbles were off the twigs, and that a very few of the animalcula were got upon the twigs, and remained there immoveable while I made my observations.

Twelve hours after this I could not see that the branches were grown larger; but I then observed that several of the small twigs were again invested with round bubbles, and an animalculum that was about 50 times as large as one of the bubbles, was running on one of the branches; and as the end of one of the twigs, where that animalculum had been, was again laden with the said bubbles, I concluded, though I had no ocular proof of it, that the said animalculum had dropped her young ones there.

On the 6th of August I took another glass-tube, of above a foot long, and an inch in diameter, one end of which was hermetically sealed, that there might be no cause of suspicion that these branches or twigs proceeded from the cork; and having rinsed it two or three times, I set it on my desk, and viewed it several times, but could perceive no branches, till after the water had stood above 40 hours in the tube; after which I took a microscope, and fastened it to the tube, to see whether the branches grew any larger after the first discovery of them; but I could not perceive any alteration. Now I could see, on the extreme parts of the twigs, 3 or 4, or sometimes 5 round bubbles, like roses, by one another; and when one of these branches, which was only fastened to the glass by its thicker end, came to be removed by that small motion into which I put the water, all the 5 small twigs were likewise put into motion, and the same motion was likewise imparted to the bubbles, though some of their stalks were so exceedingly small, that I could not see how they were joined to the said twigs.

I poured out the water very gently from the glass-tube, that the branches, which were fastened by their stems only to the sides of the glass, while their twigs were kept in a continual motion by the water, might lie along on the glass, to have them better drawn and represented. In fig. 7, pl. 2, ABCDEF represent the particle as it lies on the side of the tube; A shows the stem or root of the branch by which it was joined to the sides of the glass.

Here it is seen what a vast number of branches and twigs there are in this single figure, which indeed appears irregular upon the paper; but in the water, where they were unravelled, and could play freely, it was a very agreeable sight, being in colour like oak, and in several places beset with round bubbles, just as if they were composed of such particles. G and D represent some of the particles of the figure of a rose, which seem to consist of round bubbles, as also H, though their stalks are so fine, that I could not perceive them to be united to the main branch; however when that moved, they moved also.

As soon as the water was poured out of the glass tube, I immediately viewed the branch, and between two of the sprigs, BD and BE, I saw two of the small animalcula swimming in the little water which remained unexhaled among the twigs, and their figure was like the bubbles that were described by H. During these observations I discovered an exceedingly small particle, breaking off from one of the particles or bubbles under the said letter H, and presently it swam off with a small motion; but the whole space of its progress was not above a hair's breadth; the said small particle was certainly an animalculum, for I could plainly see it turn and wind itself about; I observed a little motion also in another of these particles or bubbles, that I have said before were like a rose, but that particle remained still entire. On the sides of the same tube lay several other small branches, but not so perfect as the first figure, and when the water was poured out they appeared like fig. 8, IKL, and fig. 9, MNO.

What shall we say concerning these boughs or little trees? we cannot imagine that they proceed from a seminal matter in the water, but rather, that they are composed of that matter which, ranging in small particles through the water, does by a kind of natural inclination cement or coagulate into one body; and this will not seem strange to any one who has seen the like experiment in filing of iron; for if you apply that part of the iron that is filed, and still remains warm, to the filings, it will immediately attract the said filings, and they will hang down like a chain from the iron, nay, if you apply these filings to a magnet, the attraction will be much stronger.

Now I am speaking of inclinations, I cannot forbear acquainting you, that I have several times, for my diversion, dissolved silver in aquafortis, and as soon as I put the silver into the menstruum, have endeavoured to observe the operations, and have always found a great number of air-bubbles arising from the silver, which, before they were separated from the metal, were as large as a common pin's head, or as a grain of sand. These air-bubbles as soon as they left the silver, and in proportion to their mounting toward the superficies of the water, grew less and less, insomuch that they almost escaped the sight, and presently after they vanished entirely, and this change happened usually in the space of three pulses; some of these air-bubbles lasted a little longer, others again, though as large as a pin's head, burst in pieces before my eyes, without leaving any other smaller bubbles behind them.

For further satisfaction, I prepared a glass bottle, whose axis was an inch and half wide, of the shape as seen, fig. 10, ABCDEFG, where ABCD represent the great bowl or globe, and ADEG the less, and F the bore or orifice of the glass. I placed this glass sloping, as in the figure, after having first filled it, a little above EG, with aqua fortis, while it stood perpendicularly; and then I put into it 2 grains of fine silver, which lay at the bottom of the glass, in such proportion as is expressed in the figure at B; by which all the bubbles proceeding from the silver must ascend perpendicularly to C; being persuaded, that when the quantity of air extracted from the silver should arrive at C, it would remain there, and that in proportion to the space that the said bubbles should possess there, so much of the aqua fortis would be protruded from the greater bowl to the less. Now when I saw that but very few more bubbles arose from the silver, I could perceive very plainly that most of those air-bubbles that had been extracted, not only grew less before they arrived at C, but were quite vanished. Now that the silver was visibly united to the aqua fortis and had remained so 24 hours, there was as much air in the glass at C, as would fill the space of 3 or 4 pins heads; whereas there arose such a vast number of air-bubbles from the silver during its solution, that I was amazed what was become of them, seeing they were in a manner vanished, without leaving any traces behind them.

I caused the air in C to dislodge out of the orifice F, by raising the glass perpendicularly; and then threw in three times as much silver; on which a vast number of air-bubbles continually ascended towards C, but were dissipated before they could arrive at the top; and such of them as were larger, and held out till they came to C, then burst in pieces, without leaving any vestige behind them, except always as much air as would fill the space of three or four pins heads. Further, I put 30 grains of silver into the said water, without observing any other remarkable difference, except that there remained at C as much air as the size of an ordinary pea.

In these observations I have likewise taken notice, that the larger the air-bubbles are, which arise from the silver, the faster they mount towards the top of the glass; and this takes place also I imagine in that air which the learned call aer subtilis, viz. how much the larger the particles of air are that perspire from our bodies, the faster they ascend.

Having weakened some of this aqua fortis, thus impregnated with silver, I put a little of it in a clean glass tube, and now and then threw into it a little copper of the size of a grain of sand; and then viewing the silver water with a microscope, I observed that the silver in this clear water was coagulated into such bodies as are represented by the abovementioned trees; which coagulation we may call an inclination; and that I may give a better idea of this kind of coagulation, I caused to be represented a small particle of the silver, no larger than a large sand, in fig. 11, PQRST. I was surprised to see, in a few seconds of a minute, such a coagulation of branched particles (which even through a good microscope were invisible) arising from a seeming clear diaphanous and liquid matter.

From these coagulated small silver particles, I was led to consider the rich silver stone, brought in small fragments from the West Indies; which has its particles coagulated in just such branches as appeared in the water by the microscope; from whence I am the more fully confirmed in my hypothesis, that when the parts of that stone were a fluid substance, the silver particles were also mixed with them; and when the stone consolidated, the silver also was coagulated in branches, just as we see it now lying in the stone, with which it is surrounded, and as it were imprisoned; from whence several have imagined that in process of time the silver grew in the said stone.

I took a piece of gold, and threw it into a proportionable quantity of aqua regia; in the dissolving of which I saw air-bubbles ascending in like manner as I said before of the silver. A little time after I took some of the aqua regia thus impregnated, and laid it on a clean glass, to observe, after the mingled salts were coagulated, what gold particles I could discover in them. When it was fair warm weather, the salts were for the most part coagulated, but in such a variety of figures, that they were hardly to be accounted for; sometimes representing an exact hexangular figure, by and by the very same figures were no less irregular; the occasion of which was, as I conceive, because the coalescence of new particles was not equal on every side; but that which was the most remarkable was, that upon these salt particles, that were as transparent as yellow crystal, there lay other salt particles of a fine gold colour, and sometimes they appeared as if these fine gold coloured salts were inclosed in the other salt particles, which caused a very agreeable phenomenon; but when the weather grew damp and moist, all the crystal salts were dissolved, and the gold-coloured salts were mixed with the common particles.

I took another little piece of gold, that had been wrought by a goldsmith, and having beaten it very thin, I put it into the same glass, where my other gold had been, with aqua regia; and observed that a white substance was separated from it, and subsided in the bottom of the glass; for as aqua fortis dissolves silver, while the gold escapes untouched, so aqua regia dissolves gold, without affecting the silver at all. Hence I infer, that the gold was alloyed with some silver, which was the white substance, separated as before said. The piece of gold that lay in the aqua regia would not unite with it; whence I concluded that there was too much gold, and too little water, for I had made the glass so hot that I could not bear it with my hand.

I took also a small piece of copper, and put it into aqua regia, where a little gold had been already dissolved, and presently observed that the aqua regia began to operate on the copper, as if it had lain in aqua fortis; the gold also cemented with the copper, but in much smaller branches than silver in aqua fortis; and I saw that the gold coagulated so strongly with the copper, that the water about the copper was quite divested of its gold colour; and after an hour's time, the water was so impregnated with the particles of the copper, that it assumed a green colour, and then one might perceive with the naked eye the gold that lay about the copper.

Again, I took other aqua regia, that was impregnated with as much gold as it could dissolve, and observed that the coagulated salt particles were of an exceedingly fine gold colour, being composed of extremely small particles, and the corners or angles were very regular. I placed these salts before my microscope, and they seemed of such a figure and texture, as if it had been a piece of gold newly taken out of the mines.

Now, in order to expose to view the coagulated salt particles in their first coalition, I put the said impregnated water on a clean glass, and then caused part of it to run off the glass again, that the remainder might lie as thin as possibly in the glass without touching it, imagining that the said water lying thus thin, the salt particles might coagulate the more regularly; and I observed afterwards that most of those salt particles were hexangular, though so exceedingly small that they almost escaped the sight, and many of them were regular hexangles, as represented; see fig. 12, ABC; fig. 13, DEF; fig. 14, GHI; fig. 15, KLM; and fig. 16, NOP. Most of these figures were as transparent as glass, and all of them were surrounded with a liquid matter, somewhat of a gold colour, which did not exhale, though the weather was very warm and dry.

I saw also a vast number of particles so exceedingly small, that their figure was not distinguishable, though I believed them to be salt particles likewise; the rather, because I could not observe any of these small salts lying near the aforesaid large ones, which, through the sympathetic inclination of homogeneous bodies, I concluded to be composed of a congeries of such small ones. I saw also other particles that were not pellucid at all, which I suppose were surrounded with so many gold particles, that they were not only of an irregular figure, but also opaque. Many of these salt figures were more protuberant in the circumference than in the middle, and had also several rings, which still the more confirms me in the opinion of those secret inclinations of like bodies; and thus in their increase they preserve the same shape which the single salts had put on at their first coagulation.

Into such water I also put a little piece of copper, and observed that immediately the gold had partly united itself with the copper, but the aqua regia had not so much power over the copper, as to protrude out of its substance so many air-bubbles, as it might otherwise have done, if there had not been so much gold in it; the water thereon assumed a greenish colour, and all the yellow colour was turned into coagulated gold, insomuch that one might with the naked eye distinguish the gold from the copper; particularly the extreme parts of the little boughs formed by the coagulated gold; though the said gold consisted of particles so very minute, that they did not appear larger with a good microscope, than the smallest grains of sand to the naked eye.

When I weakened with rain water the aqua regia, which had been impregnated with a great deal of gold, and then put into it a bit of copper, I observed that the gold in a short time was coagulated, or united with the copper, and that the water was turned to a light green. I have also observed that the small twigs of these coagulated particles were not of a fine gold colour; from whence I inferred, that there was copper blended together in them. The said aqua regia, which had been impregnated with gold, having remained some days in the glass, I perceived that where it was thickest, it was covered with a film or skin, and that most of the salts, when the weather was not warm, nor the sun shined, were dissolved, and much more in rainy weather.

Moreover, I exposed the glass with the said water to such a heat of the fire, that the salts evaporated in smoke, and that part of the glass where there lay least of the aqua regia, assumed a colour that was somewhat reddish, but where the said water lay thicker together we could not see the fine shining gold with the naked eye; but when we raised that glass, where the gold lay higher to the light than the microscope, I saw that the gold particles were not united with the other, but now and then coagulated into branches; and where the gold particles were not thus coagulated, they were so very small, that how nicely soever I viewed them, they escaped my sight; and though there lay together more than a thousand millions of these small particles, all of them did not compose a body that was larger than one single grain of a large sand. Where this matter lay thin and dispersed, I observed that its small particles were such solid and compact bodies, as did not admit the least ray of light; and where they lay single, I saw other particles still smaller, whose bodies were more loose; but I imagined these last to be some particles of that matter which the aqua regia had left behind it.

Now after the said aqua regia had remained on some of the glasses several days, and the salt particles for the most part were changed into a fluid substance, I observed among several of the coagulated branches small distinct particles, which I fancy to be gold, though I could not for the smallness perceive the colour of them. To satisfy myself herein, I took a small glass bubble, with as much aqua regia impregnated with gold, as was equal to something more than a large pin's head; and set it over such a strong fire, that both the glass and gold were near melting; then viewing the glass, I saw very plainly that a small portion of the said gold was coagulated into branches, and that these coagulated particles were round knobs or bubbles, which were dispersed in great numbers over the whole glass; in some places close together, in others thinner, of different magnitudes, and many of them so small, that although I viewed them through a good microscope, they in a manner escaped my view; insomuch that it is almost impossible to conceive, unless one has had ocular proof of it, that such vast numbers of gold globules could proceed from so small a quantity of matter; and who knows how many more gold particles must coalesce in melting, to compose one of these smallest globules. Now though the minute gold particles could not be discovered, when surrounded with aqua regia, yet we could easily see the shining gold colour of them all, now (excepting only the very smallest globules) appearing like so many complete round pellets.

In my former letter I affirmed that the green stuff found on still waters, which we also call duck weed, did not spring from the ground, but from seed within it. But now in winter, when those ditches, in which I had seen much of this green weed, were covered with ice, I saw but a very little of it, and what was very small of leaf. On this I thought it might be possible in warm weather for those green weeds to spring from the earth, though it did not seed upon the ground, nor increase in size there. For supposing, as it really happens, that these weeds in warm weather, by the rarefaction of those particles of air in their vessels, are driven up to the superficies of the water, the same weeds, by the contraction of the air in cold weather, will grow heavier than the water, and consequently sink to the bottom, and according to the change of the weather, will have their vicissitudes of emerging or subsiding. To be convinced of this rising and falling of the green weeds, take a small glass-bubble, such as is represented by fig. 17, QRST, leaving the small orifice at T open, for the water to run in and out; and let this bubble be put into a vessel with water; and it will be found in summer that, by the dilation of the particles of air within the said bubble, it will rise to the surface of the water; whereas the same air, through its elasticity, being contracted in cold weather, will make room for the admission of a mass of water so much heavier than itself, into the bubble, and then it will subside.

Abstract of a Letter from the Rev. Mr. William Derham, F.R.S. to Mr. John Haughton, F.R.S. containing his Observations on the Weather, &c. for some Years last past. Dated Upminster, April 5, 1703. N° 286, p. 1443.

As to the most remarkable weather, especially rain, of last year, and its effects, Mr. Townley tells me, that it is a general complaint in the north of England, that there were but small crops of hay; which calamity befel the southern parts also; owing to the growing months of March and April having been dry months in Lancashire, and May no wet month, considering the quantities of the other months, and of other years. Here at Upminster, April was fortunately a wet month, till the 23d, otherwise we should doubtless have suffered more than we did in the want of hay; for the growing month of March was a dry one, and May was a very dry month; for very little rain fell from April 23 till May 29, and then it fell in great showers, the greatest quantity of that month's rain. Thus much for the weather in the spring months of the year 1702, and the effects it had on hay; which effects I have some reason to think extended to many parts of this kingdom, besides those already spoken of.

As to the other months, there is little remarkable, besides the vast disproportions of rain between Lancashire and Essex, which I should scarce take notice of, if it was not what happens almost every year; the cause of this I cannot judge of, unless it be that Lancashire having a western situation near the sea; from which quarter the winds in England blow more than from the eastward.

I have added two tables more of the stations of the mercury in the barometer at Townley in Lancashire, and at Upminster in Essex, with their differences; and this observed at 3 times of the day, viz. in the morning, and about 3 in the afternoon at Townley, but at noon at Upminster, and at 9 o'clock at night. One table to the first day of every month; the other the most remarkably low, high, and more settled stations of the mercury the last year 1702. By these barometrical tables it may be seen whether at all, or how far consentaneous to truth, that opinion is of some learned curious men, both in England and abroad, that the mercury ascends and descends in all places at the same time, and in the same proportion. It is manifest that the mercury commonly rises and falls in one far distant place, when it does so in another, but not alike: also when any considerable variation is in one place, it is so in another; when remarkably high, remarkably high; when low, low; when a great ascent or descent, generally the same elsewhere; but only the differences of all these are not in equal proportion in all places; all which seems reasonable to be expected, by reason of the difference of weather in different places, especially as to wet and dry.

There is one thing more in the third table, which I think deserves especial remark, because I believe it to be the most considerable alteration of the mercury that has ever happened since the invention of the baroscope; and that was the descent on Feb. 3d and 4th last: concerning which Mr. Townley gives me this account: "That on Feb. 3d the mercury was, at 3 in the morning, at 29.15; at 3h. 28.50; and at 10 at night, at 27.5. The next day it fell yet lower, and about 12 was at the lowest, viz. 27.39; but for an hour before, and as much after, it varied only so much as to make it sensible that it was fallen, and began to rise again; the lowest he had ever seen it before was on Nov. 18, 1674, when it fell to 27.63. That Mr. Flamsteed at the observatory observed as remarkable a descent of his mercury; and that it happened about the same time of the day, viz. two o'clock in the afternoon at both places." And lastly he tells me, "That the descent in February last was the greatest that has been, since the filling his tube, which was in March 1665." The particulars which I observed here at Upminster about that descent were, that on Feb. 2 the mercury was high, viz. 29.80; the next morning 29.50; at noon 29.16; at night 28.43; the next morning (viz. Feb. 4.) at 7 o'clock, it was at 28.5, and was globose, as if it had risen, or was inclined to rise; but it continued in the same station till afternoon, and then began to rise about 2 o'clock, and rose hastily. The weather accompanying was fair on Feb. 3d in the morning; hazy at noon, and rain at night; and a violent tempest in the night, and all the next morning, of Feb. 4th.

The medium depth of the last 4 years rain, was, at Upminster $18\frac{3}{10}$ inches and in Lancashire $43\frac{8}{10}$ inches.

The Eighth Book of East India Plants, sent from Fort St. George to Mr. James Petiver, F.R.S. with his remarks on them. N° 287, p. 1450.

An enumeration of 51 East India plants.

On the Seeds of Oranges, &c. By Mr. Anthony Van Leuwenhoeck. N° 287, p. 1461.

I have several times opened the kernels of oranges and lemons, and often found that what we call a stone, kernel, or seed, is improperly so called, and that when we have stripped off its skin or membrane, we shall frequently find that two seeds are inclosed in it, viz. under the skin, and without side of the kernel, there is a small seed; a circumstance which has not occurred to me in other sorts of seeds. It is true we often find in hazel-nuts, almonds, peach and apricot kernels, a double seed or kernel; but then each of them is inclosed in a separate membrane, that have no communication with each other, but in a bare simple contact; each having a distinct stalk or string, by which consequently they receive their proper nourishment.

In November I received a present of some Surinam oranges; and in the first I opened were 38 complete kernels; but in neither of two others could I meet with 4 good seeds. I opened several kernels of the first orange; one of which, surrounded with its membrane, is represented ABCD, fig. 18, pl. 2. After stripping one of these kernels of the outer membrane, I discovered that there lay a string under it, that caused a little protuberance in the first skin, represented in the said figure by ABC; from which string, not only the seed, but the plant within it receives its increase and nourishment. EF, fig, 19, shows the same seed separated from its first and hard skin, which is only designed, I conceive, for the defence of the inward parts; whereas nuts, almonds, peach-stones, &c. are armed with strong and thick shells; FG represents the aforesaid string, parted a little from the seed itself, to render it the more visible; and it is not only joined to the seed at F, but extends its small vessels also through the second membrane from F towards E, the seat of the plant, in order to its nourishment; but those vessels are so exceedingly fine and small, that they escape the sight before you can trace them to their origin. Now we may certainly conclude, that the said string does actually comprehend in itself as many distinct vessels as are to be found in the orange-tree when arrived to full maturity; for if all these vessels were not in the young plant, while it lies involved in its mother, the kernel's matrix, whence can they afterwards proceed? Though the said string was very small, yet I was resolved to try if I could have a sight of the vessels within it, and I succeeded several times, but not without a great deal of trouble before I could place it in such a position as my limner might be able to draw it exactly. Fig. 20. HIKLMN represent a part of the same string cut across, which FG, fig. 19, show entire, and in its whole length. The said particle of the string at HIMN has abundance of exceedingly small vessels, but very difficult to be seen; about IKLM they grow larger, and consequently are more visible; and thus you may see a great many, though not all the vessels. After this observation, I queried whether these great vessels were not the origin of contexture of that membrane which covers the seed; and on the other side, whether the former, represented by HIMN, be not the feeders of the seed and plant.

OPQ, fig. 21, represent a piece of another string cut across; RSTV that part of the hard membrane that surrounds it; TVQW faintly shows that vast number of fibres, that nature uses in the contexture of such a small string; whence I was led to think that those long fibres, represented by QW, were designed to add a stiffness and solidity to the outer membrane, for the better defence of the internal vessels. I have often observed that the outmost membrane RSTV compressed the string so closely, that there remained little space between them. In the said figure XYZ represent that membrane which surrounds the kernel or seed and the plant; and all the membranes are very thin, particularly the inmost of them. Upon cutting such a seed or kernel across, you would think the particles of which it is composed to be nothing but irregular globules squeezed or compressed close together.

AB, fig. 22, represent such a seed divested of its membranes, and which seemed to have but one plant within it; whereas I have often seen under the membranes another small plant, which lay as if it were squeezed into the substance of the seed, and so is not to be seen till the seed be stripped of its membranes, contrary to those of nuts, peaches, almonds, &c. which where there is two of them, lie entire and distinct in their proper membranes, as I hinted before. I have likewise observed, under the membrane of an orange-kernel, three distinct seeds, with their plants, but some of them indeed much larger than the others. These seeds, with their inclosed plants, are easily divided into two lobes, or distinct parts, insomuch that they seem not to have been united, but only where the small plant lies; so that the whole kernel, AB, was framed by nature for no other use, in my opinion, but to nourish the tender plant within, till it be able to stand alone, and draw its subsistence from the earth about it.

Having split the seed AB into two parts, one of which is represented by CDEF, fig. 23, together with part of the plant, which would have become a tree sticking close to it, the plant itself may be seen at C, no larger than a grain of sand to the naked eye. The counterpart of the said kernel is represented by GHIK, fig. 24, and G the little pit or bed of the plant; and in the said figure between H and K, the cavity in which the second seed also lay, as likewise at DF in fig. 23.

This sight was wholly new to me, though I had dissected many sorts of seeds before; viz. to see in the middle of one seed or kernel, another complete and perfect seed; especially when I considered that from the vessels arising out of the string, and dispersing themselves through the membranes, both the kernel and plant are produced; and yet these same vessels must insinuate themselves into the very heart of the first seed, before they can produce a second seed and plant within it.

Placing such a small plant as is represented at c in fig. 23, before a microscope, I had it delineated, as seen in fig. 25, LMNOPQ; where QLM is partly that which nature intends for the body and root of the tree; NOP the leaves with which the young plant is already provided; and OP that part of the leaf which is next the sight, and somewhat protuberant, by reason of the inclosed smaller leaves. MN and PQ show the two sides of the plant torn off from the kernel, to which it was united, and from which it received its nourishment. I also turned the young plant a little about, as it stood before the microscope, to show the two largest leaves; whereas in the former position, I could see only one of them.

STV, fig. 26, represent the two great leaves in this position, between which, according to all appearance, a great many small ones are shut up; but when I came to cut the leaves across, as they lay involved in the bed of the plant, I imagined that I saw the said small leaves; and when I cut, after the same manner, that part of the plant which is to be the body and root of the tree, I discovered within the small particle that which was designed for the pith, and even the wood itself, and all as plainly as if I had been observing with my naked eye a young plant of an inch thick.

WXYZ, fig. 27, represent the texture of the pith, as it appeared; where may be observed a great many small particles, which at first sight one would be apt to take for irregular globules, but placed in a right line, and all of them of greater length than breadth; these I take to be nothing else but small tubes or vessels, by which the plant receives its nourishment, and perhaps every one of them is covered with a distinct membrane. These said parts, which compose the pith of the plant, are not to be discovered, unless with a sharp knife you cut off a piece from the seed, and place it immediately before the microscope; for all the moisture is so soon exhaled after it is cut, that one cannot possibly make any observation.

Moreover, for further satisfaction, on Nov. 19, I took a little copper-box, and put into it some white scouring sand, very dry, but somewhat moistened by the seeds I mingled with it, being newly taken out of an orange. Afterwards I carried the box in the day-time in the fob, where I used to put my watch, and all night I placed it within a large tin bottle, filled with warm water, and put into my bed, by which means it was also kept warm till the morning; and after repeating this for three days following, I opened the box, and took out one of the seeds, but could not discover any change in it. But Nov. 25, about 6 o'clock in the evening, after the sand and the seeds had been 6 days in a continual warmth, I opened the box again, and observed that the root was shot forth a small matter out of the membranes of the seed, as shown in fig. 28; AB being that part of the plant which was to become the root, and BCD the rest of the seed, involved in its membranes.

Moreover, I took another seed out of the said box, which having cleansed from the sand, and separated from its membranes, I took the two lobes of the kernel that inclose the plant, as seen in fig. 23 and 24, and with fine pins parted them a little asunder, that I might show the top of the plant represented in fig. 25, by OPQ.

Fig. 29, EFGHI represent the seed, which, as beforesaid, had lain 6 days in the sand, and was divested of its membranes. EF show that part which is destined for the root, FG and IF are those parts from whence the young plant receives its nourishment; and which by two pins, K and L, are divided from each other, in order to discover that part of the inclosed plant which will become the body of the tree, which is represented by H, and may be seen with the naked eye.

Fig. 30, NOPQR show how far the seed can shoot out its root in 12 days, which is also divested of its membranes, and placed sidewise before the glass, that the strings uniting the two parts of the seed to the plant, may be the more easily discovered. NOR show the root; OP and QR the seed or kernel, divided into two equal parts; OR the strings united to the plant, and from which it draws its first nourishment. On cutting those strings across, and close to the plant, I found in each string three vessels, through which I concluded that most of the nourishment was derived from the seed to the plant. I separated the two parts of the seed with two pins, that they might be the better viewed by the naked eye, as appears in fig. 31; STX show the root; TV and WX the two halves of the kernel; and Y that part which is to become the body of the tree, much larger than H in fig. 29.

After having proceeded with my copper-box about 18 days, in the manner above-mentioned, I opened it again, when the appearance was as in fig. 1, pl. 3; where ABCDE represent the copper-box; and C the seeds, as they, with their plants and roots, were risen above the sand; also fig. 2, FGH show the cover of the said box. Having poured out all the sand, I observed that all the moisture was evaporated from it, which doubtless was drawn off by the seeds, of which I had put 16 into the said box; all of which had shot out their roots and produced plants, some indeed much larger than the rest; and two or three of the seeds produced double plants, and one that yielded three plants. Fig. 17, AAA represent the said three plants, and BBB the three roots; all confusedly huddled together, by the pressure and want of room.

Observing how dry the sand was, when poured out of the box, I fancied if a little more water had been put into the sand, or fewer seeds, there would have been a greater increase both of the plants and roots. For which reason I took a glass tube, that was large enough to hold the same conveniently, and hermetically sealed at one end, and about 4 inches long, as here represented in fig. 4, IKLMNO; in this tube I put 5 or 6 seeds, and laid between each of them a little wet sand, which was very fine and white, and of that sort which is used for making glass, and had always kept dry in my closet for 12 years. I proceeded with this tube as I had done before with my copper-box, having stopped it with a piece of cork, as LPQM.

On the 10th day I observed that the seed was come to such maturity, that the part which nature intends for the body of the tree, was grown up as high as the cork; and on the 12th day it appeared as in the said figure at RST. I then took the plant out of the glass-tube, when it appeared as in fig. 5, ABCDEFGH; only this plant had but one root ACDH; but others have more, as you have seen at BC. Here FG shows that part which is to be the tree; DE the seed or kernel, which being surrounded with its membranes I took them off, the better to expose to view those parts that serve for the nourishment, not only of the root, but of the upper parts of the plant likewise, as also the short string D.

Fig. 6, KLMN shows a small part of the great root; which appeared as thick and large as the space between K and N, and the small roots appeared growing out of its sides, as is represented between M and N. Thus we may see with the naked eye, how a small particle, no larger than a coarse sand (as the plant is represented at C, in fig. 23, pl. 2,) is increased in bulk, within the space of 11 days, as has been already shown in fig. 5, AHGF; and all this is effected by heat and moisture in a close vessel; a plain demonstration that the plant, and all that belonged to it was actually in the seed; that is, not only the young plant, its body, root and fruit, but even its seeds also, to perpetuate the species.

In December I took another glass tube, longer and larger than the first, as fig. 7, ABCDEFGH, and stopped both ends with a piece of cork, boring a small hole in the upper cork, and filling the tube a little higher than BG with dry scouring sand, first moistened with a little rain-water. In the upper part of this sand, thus disposed, I set two kernels of an orange brought from Curacao, but some days after they had been out of the orange; so that being drier than the others, they consequently required a longer time before they could sprout out; yet in the space of 3 weeks, by the warmth of my body only, they germinated, as shown in the said fig. BCFG. And pouring out some of the sand, with which the seeds were covered, they appeared as BCG. Between ABGH are represented the long roots, with their small twigs and branches against the sides of the tube.

Afterwards I observed but very little change in the plant for some days; the reason of which I suppose to be that all the moisture was exhausted by the plant; wherefore, pouring a little more rain-water on the top of the sand, the plant grew larger, insomuch that in 5 weeks time it was got almost to the uppermost cork DE; and the roots also were spread into more branches, and had not only extended themselves to the lower cork, but one little root had insinuated itself between the cork and the glass, and had there shot forth another branch.

As the external membranes of these seeds are very thick and hard, and that part of the tender plant, which nature has designed for a tree, is not able to bore through it, or burst it asunder, as happens in the plants of nuts, almonds, peaches, &c. therefore this plant does not spring up in a right line through the seed or kernel, but out of its sides, as seen in fig. 5, between D and F, and fig. 8. between N and O.

After one of these seeds had lain near 6 weeks shut up in the glass-tube, and grown in proportion to that time, I observed that one of its leaves was withered or corrupted; on which I opened both the corks, and poured out the sand, which being very dry came away easily, but a small branch of the root had so insinuated itself into the cork, that it could not be separated without violence.

Fig. 8, IKLMN represents the said whole plant, of which LMN shows the body, and M the 3 leaves at the top, it had put forth; IKL is the root, with its twigs and branches; LNO the seed, or kernel, still surrounded with its membranes; and lastly, IP shows the cork that stopped the bottom of the tube, with the root sticking to it.

Now if we renew the comparison, which I have formerly made, between the animalcula in semine masculino, and these plants; though these animalcula are a million times smaller than a plant in an orange-kernel; and though we cannot make our observations of the growth and increase from time to time, of the said animalcula in their mother's matrix; yet we may certainly conclude, that the laws which the wise Creator of all things has followed in the production of both animate and inanimate creatures, are homogeneous and uniform; and that as the earth is the common matrix of plants, so is the tuba fallopiana in most of those animals that are formed ex semine masculino; for, as the animals in the matrix receive their nourishment and increase by a string, till they come into the air and world, so are all seeds, at least as far as we know, supported and nourished by a little string; and the seeds thrown into the earth do again, by the same string, convey nourishment to the seed or kernel.

We have discovered that there are some animalcula which have no males among them, and the same is observable among a few species of fishes also. These animalcula and fishes may be compared to some seeds, which have no other substance in them, besides the plant itself, and the membranes that invelope it; such are the seed of the beach tree, as also those of cresses. I have also discovered the strings of the walnut and chesnut seeds, by which they are nourished.

Some new Observations upon the Parts and Use of the Flower in Plants. By Mr. Sam. Morland. N° 287, p. 1474.

It has been long since observed, that there is in every particular seed, a seminal plant, conveniently lodged between the two lobes, which constitute the bulk of the seed, and are designed for the first nourishment of this tender plant. But Dr. Grew is the only author who has observed that the farina, or fine powder which is at its proper season shed out of those thecæ or apices seminiformes, which grow at the top of the stamina, does some way perform the office of male sperm. But I think him wrong, in supposing them only to drop upon the outside, or the uterus, or vasculum seminale, and to impregnate the inclosed seed by some spirituous emanations, or energetical impress. What is now proposed as the subject of inquiry, is, whether it be not more proper to suppose, that the seeds which come up in their proper involucra, are at first, like unimpregnated ova of animals; that this farina is a congeries of seminal plants, one of which must be conveyed into every ovum, before it can become prolific; that the stylus, in Mr. Ray's language, or the upper part of the pistillum in Mr. Tournefort's is a tube designed to convey these seminal plants into their nests in the ova; and that there is so vast a provision made, because of the odds there are, whether one of so many shall ever find its way into, and through so narrow a conveyance.*

To make this supposition the more credible, I shall lay down the observations I have made on the situation of these stamina, and the stylus in some few species of plants. First, in the corona imperialis, where the uterus, or vasculum seminale of the plant, stands on the centre of the flower, from the top of which arises the stylus; the vasculum seminale and stylus together representing a pistillum. Round this are planted six stamina; on the extremities of each of these are apices, so artfully fixed that they turn every way with the least wind, being in height almost exactly equal to the stylus, about which they play, and which in this plant is manifestly open at the top, as it is hollow all the way. To which we may add, that upon the top of the stylus, there is a sort of tuft, consisting of pinguid villi, which I imagine to be placed there, to

* On this distinction of the stamina and styles (pistilla) into male and female organs of generation, is founded the celebrated sexual system of Linnæus.

catch and detain the farina, as it flies out of its thecæ. From hence I suppose, that the rain either washes it, or the wind shakes it down the tube, till it reach the vasculum seminale.

2. In the caprifolium, or honey suckle, there rises a stylus from the rudiments of a berry, into which it is inserted, to the top of the monopetalous flower; from the middle of which flower are sent forth several stamina, that shed their farina out of the cases upon the orifice of the stylus, which in this plant is villous or tufted, on the same account as the former is.

3. In allium, or common garlic, there arises a tricoccous uterus, or seed vessel, in the centre of which is inserted a short stylus, not reaching so high as the apices, which thus overtopping it, have the opportunity of shedding their globules into its orifice more easily. For which reason I can discern no tuft on this, as on the former, to ensure their entrance, that being provided for by its situation just under them.

Now, nothing can be more natural than to conclude, that where a fine powder is curiously prepared, carefully reposited, and shed abroad at a peculiar season; where there is a tube so planted as to be fit to receive it, and such care in disposing this tube, that where it does not lie directly under the cases that shed the powder, it has a particular apparatus at the extremity to insure its entrance; nothing can be more genuinely deduced from any premises, than it may from these, that this powder, or some of it, was designed to enter this tube. If these stamina had been only excretory ducts, as has been hitherto supposed, to separate the grosser parts, and leave the juice designed for the nourishment of the seed the more reserved, what occasion was there to lodge these excrements in such curious repositories? they would have been conveyed any where, rather than where there was so much danger of their dropping into the seed vessel again, as they are here.

Again, the tube, over the mouth of which they are shed, and into which they enter, leads always directly into the seed vessel. To which we may add, that the tube always begins to die, when these thecæ are emptied of their contents; if they last any longer, it is only while the globules, which enter at their orifice, may be supposed to have finished their passage. Now can we well expect a more convincing proof of these tubes being designed to convey these globules, than that they wither, when there are not more globules to convey.

If I could now show that the ova, or unimpregnated seed, are always to be observed without this seminal plant, the proof would arise to a demonstration; but having not been able to discern this, I shall recommend the inquiry to those gentleman who are masters of the best microscopes, and address in using them. Though, in the mean time, I have made some steps towards a proof of this sort, and have met with some such hints as make me not despair of being able, in a short time, to give the world even this satisfaction. For, not to insist upon this, that the seminal plant always lies in that part of the seed which is nearest to the insertion of this stylus, or some propagation of it into the seed vessel; I have discovered in beans and peas and phaseoli, just under one extremity of what is called the eye, a manifest perforation, discernible by the larger magnifying glasses, which leads directly to the seminal plant, and at which I suppose the seminal plant entered; and I am apt to think, that the beans or peas which do not thrive, will be found destitute of it.

But I must now proceed to describe some other plants, by which it will appear, that there is always a particular care exercised to convey this powder, so often mentioned, into a tube, which may convey it to the ova. Now in leguminose plants, if we carefully take off the petala of the flower, we shall discover the siliqua or pod, closely covered with an involving membrane; which about the top separates into many stamina, each fraught with its quantity of farina; and these stamina are close bound upon the brush, which is observable at the extremity of that tube, which here also leads directly to the pod: it stands not upright indeed, but so bended, as to make near a right angle with it.

In roses there stands a column, consisting of many tubes closely clung together, though easily separable, each leading to its particular cell, having the stamina in great numbers planted all round. In tithymalus, or spurge, there arises a tricoccous vessel, that, while it is small and not easily discernible, lies at the bottom, till it is impregnated; but afterwards it grows up, and stands so high on a tall pedicle of its own, as would incline one to think, that there were to be no communion between this and the apices, which he sees dying below.

In strawberries and raspberries, the hairs which grow upon the ripe fruit are so many tubes, each leading to its particular seed; and therefore we may observe, that in the first opening of the flower, there stands a ring of stamina within the petala; and the whole inward area appears like a little wood of these hairs or pulp, which, when they have received and conveyed their globules, the seeds swell and rise in a carnous pulp.

From this theory I shall content myself at present with suggesting, that hence one would conclude, that the petala* of the flower were rather designed to sever superfluous juices from what was left to ascend in the stamina, than the stamina to perform this office, either for them, or the unimpregnated semina. And observe the analogy between animal and vegetable generation, as far as was necessary there should be an agreement between them.

* In the Linnæan sexual system of botany, the petals are supposed to serve merely as a covering or protection to the stamina and pistilla.

Plate 3, fig. 9, represents a yellow lily: A the top of the pistillum or tube, at which the seminal plants are supposed to enter, and through which they are conveyed to the unimpregnated seed in the seed vessel; bbb the apices seminiformes, which, when open, shed that powder which enters the tube at A; c the place of the seed vessel at the bottom of the tube, the seed vessel itself being concealed under the leaf in this figure.

In fig. 10, D represents the siliqua in a flower of the pea-kind: E the tube which arises from the siliqua, and conveys the plants to it; F the membranous coat that involves the siliqua, laid open; gggg the apices, which, before the membranous tegument is laid open, appear to rise from its edges, and by the petala of the flower are kept close upon the orifice of the tube, that they may conveniently shed their farina into it.

Fig. 11 is a French bean represented sidewise. Fig. 12 the same opened: h the seminal plant; i a perforation, at which it is supposed the seminal plant first entered.

An Experiment to discover the true Cause of the Motion of the Dura Mater. By H. Ridley, M. D. Translated and abridged from the Latin. N° 287, p. 1480.

Baglivy having advanced in his 4th book* de Fibra Motrice an opinion† concerning the alternate movement of the dura mater, different from the explanation given by Dr. Ridley in the 6th chapter of his Anatomy of the Brain; the last mentioned physician was induced to subject this matter to the test of experiment.

Having tied down a dog and removed a part of the cranium, neither Dr. R. nor his assistants could perceive any pulsation either of the dura mater or of its longitudinal sinus; but a little while afterwards the aforesaid sinus being wounded by the contact of a red hot iron introduced with a different intention, a profuse hæmorrhage ensued; by which means the vessels were considerably unloaded; after which the pulsation both of the sinus and of the whole dura mater was very conspicuous. An incision was made into this sinus in its whole length, in order to trace by the spirting out of the blood the insertion of arteries into it, but in vain; although Vieussens and Wepfer mention that they detected several such insertions. Hence it is evident (Dr. R. infers) that these sinuses have no other motion but that which is communicated to them, as well as to

* Lib. i. Cap. v. of the Venetian edition in 4to. 1752.

† Baglivy supposed that the dura mater had, in consequence of its fibrous structure, a systaltic action similar to that of the heart.

the dura mater, by the arteries distributed through the substance of the brain; the arteries which are ramified over the dura mater, at the same time contributing in some degree to the said motion.

Such is the experiment related by Dr. R. in his Treatise on the Anatomy of the Brain; but for further satisfaction he made the following experiment. Having removed the upper part of a dog's skull he laid bare the dura mater; and, after wiping away the blood, he had a good view of that membrane, and of the longitudinal sinus; whose systaltic action (which was quicker than usual) exactly corresponded with the pulsations of the heart. This motion was observed by himself and those who were present at the experiment, for the space of a quarter of an hour; after which he thought of raising up the dura mater from the brain by means of a tenaculum, and dividing it with a knife, in order to examine the motion of the brain underneath. Accordingly the dura mater was punctured by a tenaculum (which the dog appeared to feel sensibly, but without being thrown into convulsions); and after the blood, which flowed from this puncture, was wiped away, the systaltic motion of the cerebrum was seen at the puncture, and a small clot of blood which had stuck in it was thereby forced out. The point of a pair of scissars being then introduced into the aperture of the dura mater, that membrane was cut transversely in the part which was farthest from the longitudinal sinus; whereupon the brain, covered with the pia mater, protruded itself through this opening; the motion of the dura mater being still very perceivable by the touch, and in a slight degree by the sight also; notwithstanding that the elastic or contractile power of its fibres had been impaired by the incision. After some hours had been spent in making these observations, during which the dog had been tortured in various ways and had lost much blood; at length in order to destroy all the pulsific power which may be supposed to be inherent in the dura mater, or to be communicated to it from some other source; it was moistened with some drops of oil of vitriol, which turned it black; no contraction, however, or if any a very obscure one indeed, took place; but although the dura mater was no longer capable of yielding to the impulse given by the movement of the brain, yet the pulsation of the brain itself could be distinctly felt on applying the finger.

All this time the dog was far from being exhausted; but on plunging the point of the knife an inch deep into the substance of the brain, he was seized with violent convulsions. At this time on introducing the finger into the beforementioned opening made in the dura mater, the movement of the brain was perceived to be stronger than before. After this a probe was pushed deeper into the brain; whereupon the dog exhibited strong symptoms of pain; lastly, on thrusting the whole knife quite to the opposite side of the brain, the most horrible convulsions were excited; yet even at this time both Dr. R. and his attendants could feel very distinctly the alternate elevation and depression of the brain, on introducing their fingers.

Hence Dr. R. infers that no doubt can remain respecting the proximate cause of the aforesaid motion.*

Account of a Book, intituled, Consilium Ætiologicum De Casu quodam Epileptico: quo respondetur Epistolæ Doctiss. Viri Thomæ Hobart, M. D. Annexa Disquisitione de Perspirationis Insensibilis Materia, et Peragendæ Ratione. Authore Gulielmo Cole, M. D. N° 287, p. 1485.

Dr. Cole supposes the seat of epilepsy to be "in the cortical part of the brain, close to where the medullary part begins; at which place he thinks the nutritious juice, newly separated from the blood, but happening to be somewhat more gross than usual, meets with a check in its progress, on account of the natural coarctation of the vessels here at their passing into the medullary part; so that the protrusion being continual, it must undergo some congestion, and in some measure distend this repository; whence it gradually becomes maturated into a ferment, which acquiring a certain degree of acrimony, breaks forth into the continued channels of the medullary part, and by vellicating them, produces the first symptoms of the disorder; and as this offending matter is further propagated along the tracts of the nerves and fibres, the other symptoms take place.

* The alternate elevation and depression of the brain and dura mater, is a subject in the explanation of which physiologists have widely differed. See Haller's Element. Physiologiæ, tom. iv. lib. x. § xxxviii, xxxix, xli, et seq. where the principal authorities on both sides are referred to. Baglivy's hypothesis of an original systaltic power belonging to the dura mater itself, and resembling that of the heart, has been long since abandoned; and the only question has been whether the cerebral movements correspond with the systole and diastole of the heart, or with the dilatation and contraction of the lungs? This last circumstance, although it was overlooked by Dr. Ridley, is now regarded as the principal cause of this phenomenon. For it has been found that the brain and dura mater are distended or elevated in the act of expiration (i. e. when, from the collapsed state of the lungs, an impediment is opposed to the free transmission of the blood through them, and the blood is therefore accumulated in the right cavities of the heart, in the superior vena cava, and consequently in the vessels of the brain); and on the other hand, that those parts, (viz. the brain and its investing membranes,) are depressed or subside during the act of inspiration, i. e. when a free passage being restored to the blood through the lungs, the previous accumulation in the right cavities of the heart, in the vena cava, and consequently in the blood vessels of the head, is removed. This alternate elevation and depression of the brain, and its membranes, corresponding with the times of expiration and inspiration, must not be confounded with the pulsatory action which the encephalic arteries possess, in common with the arteries distributed to other parts of the body.

In regard to the cure of the epilepsy after noticing the inefficacy of specifics (the misletoe excepted) he considers the use of evacuants in this disorder, viz. 1. phlebotomy; 2. fontanels, and particularly setons; 3. purgatives; and 4. insensible perspiration; concerning which last he subjoins a discourse at the end of the treatise.

Remarks on M. Leuwenhoeck's Observations on Green-Weeds and Animalcula. N° 288, p. 1494.

I observed the same animalculum* Mr. Leuwenhoeck speaks of in N° 283 of the Transactions. I found it the beginning of this month (June) in some clear water, which I took up in a ditch, in which, with my utmost attention, I could discover no more than this single one of the same kind; fig. 13, pl. 3, represents it in one of the postures it appeared the first day (for it varies every moment) and the knob at a, which looked like the gut cœcum, was sometimes a little more hastened: two days after, I could perceive two or three white fibres at the extremity of it; and on the fourth day, the animal lying stretched at its full length, appeared as in fig. 14; and I plainly saw, that what I thought an excrescence, was a young one, with 6 horns coming out of the side of the old one; and the next day I found it in the water entirely separated from the body, being about one third of the length of the parent. The formation of the horns are well represented by Mr. L. and they issue like radii, not from the extremity, but quite round a small knob, which I take to be the head. The horns have a vermicular motion, and are extended or shortened both together and severally. The other extremity is flat, which it often fixed, like a leech, to the bottom or side of the glass in which I kept it. It also contracts and dilates its body at pleasure, and especially, when touched or disturbed, will bring both body and horns into a small compass, and has then the appearance of fig. 15 and 16. The horns are perfectly white, and the body yellowish, and to a naked eye not easily discernible in the water, it being when extended no thicker than a horse-hair.

The small plant mentioned in the same Transactions, is the lens palustris, or duck-weed, which floats plentifully on our ponds or ditches. But I must dissent from Mr. L. where he says it does not come originally from the bottom; for many years since, the late W. Ch. Esq. showed me the manner of its springing out of the mud; and we often observed, that when the leaves were grown to a competent size, the force of the water easily drew the minute

* Hydra grisea. Linn.

single fibred root, and raised the plant to the surface. I agree that the leaves, when floating, continue to grow, and may be increased in the manner he mentions: and I have often taken out of them the young plant which he represents; so that they may be called seeds more properly than leaves; and my opinion is, that toward the end of the year, on their corrupting, they sink to the bottom, and there take root, so as to continue the succession.

The animalcula which Mr. L. describes sticking to the root of the plant, I have often observed, not only in water plants, but adhering to the bodies of many sorts of water insects, which I have seen covered almost all over with tufts of them, each tuft consisting of many animalcula, which appear not much unlike the flowers of a lily or fox-glove.

This congeries of animalcula will lengthen and contract themselves both altogether and severally, and I have observed, when they lie at length, that they extend some extremely minute organs, like small feet (not easily discerned even with my best glasses) which by their quick agitation bring a current of water from all sides toward them. But I never saw that motion in them which Mr. L. says is like that of a mill wheel, nor indeed can I perceive the possibility of such a rotation of any member in an animal mechanism. But I think I can easily account for this mistake of Mr. L. or rather of his painter, for in the same water wherein I have seen these plants and animalcula I have observed a small round animal, whose numerous legs stand like radii all round its body. This has a swift progressive motion, but will very often lie still (when you can only perceive those radii) and then turn very swiftly round like a wheel, sometimes one way, and then stop and turn the other way, without stirring a hair's breadth forward. Now it is very probable that one of these might show its motions so very near to, or among a tuft of the other fixed animalcula, that it might be very easily mistaken for part of the same, and I am very confident this is matter of fact.

These animalcula are sometimes seen loose, but generally they are fixed in clusters by their tails to other bodies, and perhaps cannot separate themselves, and I think it no mean instance of wisdom, that many kinds of water insects, which are so fixed, and even some of which have but slow and irregular motions, are furnished with such organs about their heads, the vibration of which brings a constant current towards their mouths, and with that food for their support; otherwise they would be starved for want of nourishment.

The insects on whose bodies I have seen these animalcula are of divers sorts, and I have observed no small variety in the water of our ditches, not only of reptiles and the caterpillar kind, but of eels and perfect shell-fish, both crustaceous and testaceous. Fig. 17* and 18 show them as they swim with their back upwards; but the members and legs on the other side are so various, and so much more curiously formed, than those of lobsters and shrimps, that I despaired of giving any tolerable representation of them in any other position. These are about the same size, the largest being rather less than a very small flea, and the least a little larger than a mite; but all are breeders, and carry their spawn at their tails; that represented fig. 18, in two bags, one on each side, which are fastened about the 5th joint; and the other in a single bag or film under the tail; and I have often seen these bags broken, and the spawn, which is globular, and large in the proportion to the fish, scattered through the water. There is also among these a third sort of the same kind, not less elegant, though far less in bulk, which is shaped more like a shrimp, and carries its spawn like that. All these three species, as also some other water insects, are certainly monoculous, and have their eye exactly in the middle of their head; and I could never, with my utmost attention, find so much as a dividing line in it. Some of them, especially in some waters, are dark and cloudy; but generally they are so transparent, that through the shell I can see the peristaltic motion quite through their whole length, and a constant pulsation of a part, which I guess is the heart; but I could never discover any course of blood in them (nor even in shrimps themselves which are as large as some thousands of these) though I have seen it plainly in animals a little larger, viz. the smallest new hatched spiders, and in that water insect which is described and figured, though not accurately, by Swammerdam, under the improper name of pulex aquaticus. But this is of the testaceous kind, of which I have seen a greater variety, and not less curious than those of the crustaceous.

I have further observed the lens palustris, and am fully satisfied of the truth of its first springing from the bottom. I lately took up some on the shallow side of a pond, and found the ends of the stalks (most of which were at least 5 inches long, and as thick as a strong horse-hair) manifestly rooted in the bottom; so that I could not take them up without raising the mud with them, which also adhered very visibly to them. These stalks or roots are of a curious texture, and almost transparent, and I have seen their outside very prettily covered with a regular sort of net-work. Mr. L's figure represents them but badly.

In my observations of these stalks, I often saw adhering to them, and sometimes separate in the water, many pretty branches, composed of rectangular

* Monoculus quadricornis. Linn.

oblongs and exact squares, which were joined together, as in fig. 19, which I drew as exactly as I could from one of them. There are often 20 or more of these figures in one branch, which generally adheres at one end to the stalks of the plant, and I think it remarkable that these rectangular parallelograms are all of the same size, the longest side not exceeding one-third of a hair's breadth, and that the length is just double the breadth, the squares being visibly made up of two parallelograms joined longwise. They seem very thin, and the texture of every one is nearly the same.

In some water which I took out of a pit I found a small water-newt, not an inch long, which I suppose was of this year's hatch, and the legs being so small as not readily to be discerned at first view, and the body very clear, I took it at first sight for a fish. This I kept by me, instead of tadpoles, to show the circulation of the blood in its tail. But that was not the only entertainment it gave me, for I found the course of the blood in every part of its body, and particularly in every toe of the feet, it was a curious sight to observe the stream come to the extremity of the toe in one channel, and return by another. In this newt, just below the setting on of the head on each side, are three little rugged fleshy branches, which it spreads like fins, and which help to poise its body; observing these with the microscope, I found each of them divided, something like a leaf of polypody, into a great many pointed branchings, in each of which, as in toes, I can see the blood come to the extreme point on one side, and return on the other; and 30 or 40 of these branchings will sometimes appear at one view, and the blood seen be distinctly circulating in all. For, as Mr. Cowper rightly observes, the globules of the blood of these animals are very large, so that I can see the circulation in them very well, even with the smallest magnifiers, which take in a large area. And, from what has been said of this course of blood, I am persuaded, that these organs in the newt are not only designed to be serviceable in their swimming, but (though they have lungs like a frog) may be also analogous to the gills in fishes.

In my examination of the waters of our ditches (in which I daily find new varieties of animalcula) I met with great numbers of those round bodies, mentioned by Mr. Leuwenhoeck in the Transactions, N° 261; while I was observing them, I saw with surprise, that each of these spherical bodies, which are smaller than a mustard seed, have a constant progressive motion, and at the same time a slow revolution about their own axis, and contain within them other small globules, some more, some less; but I never found above 10 in any one; and these I have seen move and change their position within the others, which Mr. L. says he never observed. While I had one of these bodies on a glass plate before my microscope, I saw (as Mr. L. describes it) one of the contained globules slip out of it; and while the large one lay still, for want of sufficient depth of water to float in, this small one that came out had immediately a very quick rotation on its axis; and what was most surprising, at the same time it kept an equable revolution about the larger globe, as the centre of its orbit, always very nearly at the same distance, though I could not perceive any vortex in the water which bore it; and what is still more remarkable, I saw it stop, and then make its revolution round the central body the contrary way, the rotation on its own axis always continuing. And when the water was so far evaporated, that all lay at rest, by the addition of new water the same motions were renewed. This I thought a very pretty representation of the planetary motions about the sun. I think it not easy to account for the motions of these globules, nor will I, to solve the difficulty, say in contradiction to Mr. L. that I believe them animate, though I have formerly seen some not very unlike them both in shape and motion, which I am satisfied are animals.

I find all the earwigs, which I have examined by a microscope, infested with great numbers of minute insects, which stick like lice on many parts of their bodies, and especially just under the setting on of their head. They are alike on all, and I never found the same on any other animal; they are white and shining like mites, but much smaller; are round backed, flat bellied, and have long legs, especially the two foremost.

Concerning some Spots on the Sun, observed in June last. By Mr. Stephen Gray. N° 288, p. 1502.

June the 15th, 1703, between 4 and 5 o'clock in the afternoon, I saw a spot in the sun, by placing a white paper so far behind the 6-foot telescope as to give the image of the sun 9 inches diameter; the spot was in the lower right hand quadrant of the sun's disk; its form was almost round, inclining to an ellipsis; it was distant from the sun's limb about 6 or 7 minutes, and its diameter I judged to be about 10 or 12 seconds. A little before the sun set I saw the spot with a 16-foot telescope, and could perceive that it was environed with a mistiness. The 16th I saw the spot again, about 2 in the afternoon, and found it advanced nearer to the sun's western limb. But the 17th was cloudy, and so was the night, which hindered me from observing the eclipse of the moon. The 18th in the afternoon it cleared up, and a little before 5 I saw the spot with the 16-foot glass, through thin clouds, and found it was now very near the sun's limb, little more than half a minute from it; it was much contracted in its breadth, so as to be 4 or 5 times longer than broad. The 19th, in the morning, I looked for it again, but could not see it, so I concluded it was then either gone off the sun's disk, or if it adhered to the limb, the great tremulation of the atmosphere hindered me from seeing it.

Astronomers, by these spots, have found, that the sun revolves on its axis, so as that in 27 days the same point in the sun's disk returns to the same place, seen from the earth; hence its semirevolution in 13½ days, and consequently the spot going off the sun's disk the 19th of June, may be expected to return the 2d of July next to the eastern limb of the sun's visible hemisphere, if it be not dissolved before that time.

June the 26th, 1703, in the evening, I looked to see whether there were generated any new spots in the sun, but found none; but on the 27th, about half an hour after 8 in the morning, by receiving the sun's image on white paper from the 6-foot telescope, I saw a spot near the sun's vertical, towards the lower limb. Between 9 and 10 I elevated the 16-foot tube, the clouds being now of a convenient thickness to let me view the sun without prejudice to my eyes, and found that this spot was of a triangular form, and that it was accompanied with two other smaller ones; the sides of the great spot were curvilinear, this with two smaller ones made an equicrural triangle. At 4 in the afternoon the triangular spot had a small fragment separated from it, and itself was now become elliptical. At half an hour after 5, the fragment from the great spot was itself divided into two, and at half past 6 o'clock, there was a small fragment separated from the lower end of the great spot. The observations made this afternoon with the 16-foot glass, were when the air was clear, and to secure my eye, the eye-glass was smoked with a wax candle.

The 28th, about 7 in the morning, I saw that the great spot was much augmented, but the smaller ones, that yesterday attended it, were vanished, and that there were two new ones generated at about 1½ minute's distance from the great one below, and towards the left hand of it; the great one was a parallelogram, with a very black diagonal crossing it. At 10 o'clock, there was another diagonal crossing the former, and the two smaller spots, which before were longish, had now taken a round form.

Some Observations on the Spots of the Sun. By the Rev. Mr. William Derham, F. R. S. N° 288, p. 1504.

Mr. Derham saw the same spots as Mr. Gray. Since the last appearance of the solar spots, says he, I have invented and provided myself with a very nice micrometer, and a watch that beats half seconds, hoping to have been able to have seen another revolution of them. My micrometer is not, as usually, to be put into a tube; but is to measure the species of the sun on paper, of any radius, or to measure any part of it, which I am inclined to think is more exact than the common way. By this means I can easily and very exactly, with the help of a fine thread, take the declination of a spot at any time of the day; and by my half second watch, and a fine cross hair, which latter way I learnt from my friend Mr. Flamsteed, I can measure the distance of the spot from the sun's eastern or western limb.

This cross fine hair, I advise, from my own experience, should be set, not at the exact focal distance from the eye-glass, as usually, but a little out of that distance, nearer towards the object-glass; because the shadow of the hair, will be thereby much narrower, and more strongly appear across the species of the sun, received on the paper, which I take this occasion to note, not only because I believe it has scarcely ever been before observed, but because it may be of good use in taking the sun's altitude, measuring his diameter, &c. this being a more easy, and perhaps a more exact way, than by looking through the tube.

One of the spots was first round and strong, afterwards long, and with a nucleus. The very same spot I saw again on the sun's eastern side on July 5, but very faint, small and long, so as to be but just discernible. On July 6 it quite disappeared, both through my tubes, and on paper, which is better. The other spots had these remarkable appearances and variations. On June 28, viewing the sun towards evening, I espied a large strong dark spot, with two or more glaring nubeculæ behind it. These the next day were become four strong dark spots, the foremost with a tail to it, conjoining the little spot next it. On June 30 I saw spots, but it being a cloudy morning, and I absent from my tubes in the afternoon, they were not exactly taken. July 4, between two long spots appeared something like a round nubecula.

Observations on the Invention and Progress of Printing, to the Year 1465. N° 288, p. 1507.

What Mr. Ellis says, Phil. Trans. N° 286, about the books printed at Haerlem by Laurence Koster, agreeing so well with the account given by Theodore Schrevel and others, leaves us little or no room to doubt whether the honour of the invention be due to this or the other cities, whose writers have so eagerly contended for it; since none of them have pretended to show any book printed so early as A. D. 1430 or 1432, or near that time. But the difficulty lies either in showing why the practice of this art should be at a stand from A. D. 1432 to the noted reviving of it at Mentz by John Fust and Peter Schoeffer, who (as it has been commonly, but erroneously said) printed the first printed book there, namely Tully's Offices, A. D. 1465; or else, in giving any tolerable account of the progress of this invention during an interval of more than 30 years.

Boxhorn, as well as Schrevel and other authours, expressly say, that Koster could not advance this invention so far as to print so large a work as the Speculum Salutis, without gradual improvements; and that his first essays were on loose and small leaves of paper, before he attempted whole books. These being loose and single are supposed to be all lost; but I once observed a loose leaf of paper in octavo, lying in an old MS. breviary in her majesty's library at St. James's, which I thought was one of Koster's first pieces, done when he had attained to some experience in the art, and to get money. It is a small rude wooden cut, of the five wounds of our blessed Saviour, and the instruments of his passion, with a Latin inscription at the bottom, to this purport, that those who should say so many Ave Marias before it, should have so many thousand years of pardon. The ink used in this cut or print was writing ink, and it was all black, without those other colours with which Koster seems afterwards to have adorned his books.

In the above-mentioned Boxhorn's book De Origine Artis Typographicæ, it is said, that Hadrian Junius had a book printed by Koster, and like that kept in the chest at Haerlem; now among those bequeathed to the Bodleian library at Oxford, by Mr. Francis Junius, a kinsman of Hadrian's, there is a thin small folio, numbered 31, which may probably be the same; and which Mr. Foss, a learned and curious Danish gentleman, assured me was very like to that at Haerlem. This contains the sum of the history of the Old Testament, all represented in rude wooden cuts, coloured with divers colours, without shadows, resembling our cards, which, with sheet-ballads, are remains of the old manner of printing, and stamped on one side only; the white sides of two leaves being pasted together, the black both in the pictures and in the inscriptions, which show their meaning, being writing ink,* as the aforesaid leaf, inartificially spread upon the wooden block, here thick and there thin, spreading and yellowish; the letters extremely rude, and altogether manifestly showing that the art was yet in its infancy. The stamping of this book on one side only, was not because the printer did not know how to dispose the pages in such manner as might be proper and easy for the book-binder's use; for it has its signatures all along in minuscule letters, set in the middle of the page; but because it was thought that the paper would not bear a second impression on

* It is to be wished that Mr. Ellis, when he had Koster's books in his hands, had observed whether the black ink was printing ink or not; whether Koster's picture was ancient, and coloured or not; or if there were more in either of the books; whether the whole was cut upon wood, or composed with printing letter; whether there were signatures for the book-binders, &c.—Orig.

the backside, just as the book writers of those times, when paper began to be cheap, and to be made up into books, would yet have the first and last leaf of each quaternio, senio, &c. to be of parchment, for strength's sake. This book is imperfect, and has no date now appearing, and perhaps never had any; neither has such another book as this, which contains the history of St. John and the Apocalypse, in such like wooden coloured pictures and inscriptions. This is inscribed LAVD. E. 65. in the same Bodleian library, and has also its signatures in majuscule letters, as indeed I have observed signatures in many MSS. of different ages, as high as 1000 years ago and upwards, expressed either by letters or numbers. This book, though printed on one side, and pasted as the former, is yet more elegant, and shows that the art was much improved. And in the same library, Arch. B. Bodl. 88, there is an ancient MS. with the same figures and inscriptions, though the habits of the figures are different, those of the MS. being of the older fashion; and it is very likely that there is another copy of this body in the emperor's library at Vienna; for Lambecius, Comment. de Biblioth. Cæs. lib. II. p. 772, reckons among those which he brought away from the archiducal library at Inspruck, a book, of which he gives this account, Apocalypsis S. Joannis Apostoli et Evangelistæ Latino Germanica, chartacea in folio, una cum Vita ipsius, et multis figuris ligno incisis, quæ propter vetustatem suam spectatu sunt dignissimæ; and in this book at Oxford, besides the printed cuts, there is also a Commentary on the Apocalypse in High Dutch. Besides these two very ancient printed books, Mr. Bagford told me that in the MS. library of Corpus Christi, or Bennet College in Cambridge, he saw a third, containing the History of our Saviour, printed on one side only of the paper, with such like wooden cuts, but yet more neatly than either of the former, which I had before shown him at Oxford. And these three books being, as is before said, stamped only on one side of the leaf, the whole wrought or cut upon wood, not set or composed with printing letter, and printed with writing ink, sufficiently demonstrate that the art was as yet in its infancy, and may, though they bear no workman's name, be very reasonably ascribed to Koster, not only because no other person lays claim to them, but because in divers circumstances they agree with the history of the man, and with what remains of his workmanship. If it be asked, why Koster did not set his name and the year to these books, as well as to that at Haerlem, mentioned by Mr. Ellis? it may be answered, that Schrevel tells us, that Koster bound Fust, above-mentioned, by oath to secrecy, and not to betray the art to any person whatever. In which it is likely that his design was not so much to let the world think that he had a new way of multiplying the copy of a book, much quicker than the quickest penman; but that he designed to impose upon the world, by selling his printed books for new-written copies, by which the book writer and illuminator must, as he might well pretend, be so paid for their work, as to maintain themselves and families. This trick might be long undiscovered in and about Haerlem, because there was no other printing by which this might be contradicted; but at length, as Boxhorn and Schrevel write, Fust ran away with all his master's tools and materials, and in process of time set up a printer's shop at Mentz, being assisted by his servant Peter Schoeffer, a young man of a good genius, who afterwards married his daughter, and became his partner in the business. The story goes that this John Fust went to Paris (but whether before or after his settling at Mentz, I cannot tell) and that he there offered a great number of printed Bibles to sale, as if they were manuscripts. But the French were not to be so caught. They considered the number of these books, and their exact conformity to one another throughout the whole, to a line, a word, a letter, a point, and that the best of book writers could not be thus exact, and therefore by indicting him of diabolical magic, or threatening him with it, they at once gave birth to the story of Doctor Faustus, and obliged him to discover the art. And I doubt not but about this time, many books were printed and sold for manuscripts; I having several such books without dates, which looked rather older than any I have seen with them. I speak now of those that are set or composed of letter, which with printing ink of lamp-black and oil, and the printing press, is said to be the improvement of Schoeffer above-mentioned, though Schrevel with less reason ascribes the two former to his countryman Koster.

When Fust and Schoeffer began first to work at Mentz, is uncertain, but the first mention I find of him as a printer at Mentz, is in Schrevel's Haerlem, pag. 272, where he says that this Fust (or Faustus, as he calls him) published Alexandri Doctrinale cum Petri Hispani Tractatibus, A. D. 1442; but this, and some other books mentioned by writers on this subject, are never said to be extant in any particular place, in order to be consulted upon occasion; and therefore their titles and dates are not so much to be relied on. But another date, which, though not so old, is more authentic, may be found in the above-cited book of Lambec, p. 989, where he says he brought away from Inspruck, among other choice volumes, and placed in the Imperial library at Vienna, a Psalter, printed upon parchment, with this inscription at the end: Præsens Psalmorum codex venustate capitalium decoratus rubricationibusque sufficienter distinctus, adinventione artificiosa imprimendi ac characterizandi, absque calami ulla exaratione sic effigiatus, et ad eusebiam dei industrie est consummatus per Johannem Fust, Civem Moguntinum, et Petrum Scoeffer de Gernszheim, Anno Domini millesimo CCCCLVII. in Vigilia Assumptionis. From this time there are constant remains of the industry of these men; and I can mention more books printed by them than the Durandus, in the library of Basil in Switzerland, printed, as a gentleman who saw it told me, A.D. 1458. Joannes Januensis's Catholicon, in her Majesty's and the Lord Bishop of Norwich's libraries, printed 1460. The Latin Bible of 1462, yet extant in the French King's library, and in divers monasteries beyond the sea, and perhaps in England. The Tully's Offices, printed both in 1465 and 1466, if both these be not the same edition, the last sheet or leaf being composed afresh: and so on till Schoeffer worked for himself after the death of Fust, and Schoeffer's posterity after him. But I willingly forbear the catalogue, in hopes that this, with that of the other old printers throughout Europe, and especially of our English workmen, with their devices, the effigies of most of them, and a multitude of uncommon remarks relating to writing, printing, parchment, paper, binding, &c. will be communicated to the world, when Mr. Bagford's papers shall be digested.

On the first discovery of the art by Fust at Paris, or at his first settlement or public profession of it at Mentz, it quickly spread over the best part of Europe,* and was commonly used in other countries before it was known in England, notwithstanding what some writers affirm to the contrary; the first book that we pretend to have been printed here, being Hierome, or rather Rufinus, on the Creed, printed at Oxford, A. D. 1468.

To prove this in some measure, not to mention the progress of printing in other countries, I instance in Italy, and particularly Rome; here, not to insist on the large catalogue of printed books described in an epistle to Pope Xystus IV, published at the beginning of the 5th tome of the Bible printed with Lyra's Commentaries at Rome, A. D. 1472, and transcribed by Boxhorn; I shall only relate the sum of what I meet with in Bernard Mountfaucon's Diarium Italicum, tome 1, p. 255, 256, viz. that Joannes Aleriensis, in a flattering epistle to Pope Paul II, who was elected A. D. 1464, congratulates him, because printing was first used at Rome under his pontificat. Which if spoken of the very first practice of the art at Rome, and not of an established imprimery, seems to be false; because this learned monk, in the same place, says he saw a Lactantius in the musæum, or study, of Monsieur de la Thuilliere, which has these words at the end, Lactantii Firmiani institutiones cusæ in venerabili monasterio Sublacensi Anno 1461, antepenultima Octobris. Now, unless a man will suppose printing to be invented in this monastery, he must believe it to be brought hither from Rome, which is about 20 miles distant from it. And the same

* From these places, and from books, or parts of books, where, or wherein such sorts of letter was used, the printers still call their letters Italic, Roman, English, &c. austin, canon, pica, primer, brevier, &c.—Orig.

author says, that Floraventes Martinellus, in this Roma Sacra, affirms that printing was practised at Rome in the palace of the Maximi, A.D. 1455, under Pope Nicolaus V. by Conrad Sweynheim and Arnold Pannartz, who were both Germans, and continued printers there for many years after.

The custom of putting the dates of printed books at the end of them, was taken up in imitation of several manuscripts of the middle and later ages (for I never saw or heard of any ancient manuscript in capital letters, either Greek or Latin, which has a professed date written in the first hand;) but here the inspector ought to be cautious, lest he be led into an error: for several manuscripts at the end have a date, which may be by some understood of the time when those individual copies were written, whereas they only denote the time when the author finished his work. And some of these dates being printed from the manuscripts, have deceived many curious men. For example, the first edition of Lyndwood, Paulus a Sancta Maria, and others which I could name. Besides some dates in ancient printed books, being not corrected, are false; such as a book printed in the beginning of the 16th century, in the library belonging to the Ashmolean Museum at Oxford, which thus pretends to 400 or 500 years of age. A Julius Hyginus once shown me by Mr. Millington the bookseller, printed at Paris, as there set down, An. Dom. MCCCCXII, instead of MCCCCCXII: for the printer is mentioned as then living in l'Origine de l'Imprimerie de Paris. I have indeed a book wherein, among other tracts, is one of an old print, at the end of which there seems to be such a mistake, though not so easily rectified as the former. The words are these, Explicit opusculum Enee Sylvii de duobus amantibus in Civitate Leydensi Anno Domini Millesimo CCCC quadragesimo tertio LEIEN. Now though Leyden seems to be the place where it was printed, yet 1443 cannot be the time; for, just before, Sylvius says himself, Vale, ex Vienna quinto Nonas Julias MCCCC quadragesimo quarto. Sylvius was elected Pope by the name of Pius I. A. D. 1458, and died A. D. 1464. Now it may seem probable, that if this Tract was printed after his election, as suppose A. D. 1463, or even after his decease, his papal dignity might have been remembered; if it be judged to have been printed before his election, I know none that will allow of printing at Leyden, or even in Lyons, so very early.

One objection may be urged against what is said of Koster's, or the old printed books above-mentioned, being printed on paper about A. D. 1430, or soon after: for some authors are of opinion, that paper made of linen rags was first made at Basil, by some Greeks who fled out of their country after the sackage of Constantinople, A. D. 1452, in imitation of the cotton paper, commonly used in the Levant. But this can have no force, our paper being much older: for I have a piece, the writing upon which seems to be about 350 years old, and agrees very well with a charter which I have seen of Thos. Beauchamp Earl of Warwick, bearing date A. D. 1358, and. 32 Edward III. In the archives of the library belonging to the R. R. Dean and Chapter of Canterbury, I saw an inventory of the goods of Henry Prior of Christ's Church there, taken on his decease, the 20th year of King Edward III, and this is written upon paper. In the Cottonian library, though searching after other matters, I could not but observe several writings upon our paper, in the time of most of our kings and queens, as high as the 15th of King Edward III, and I doubt not but there are others more ancient in the same place. But in the east, the use of cotton paper is much more ancient; and I have in the Bodleian library seen an Arabic manuscript, among those which the university bought of Dr. Huntingdon, written in the 427th of the Hegira, i. e. A. D. 1049; and others in the same place, without dates, seem older.

The proximity of the subject induces me here to take notice, that though the invention of the rolling press is commonly ascribed to Lipsius; yet it seems older than his time, from a printed book in the Bodleian library, placed LAVD. D. 138. This is a missale secundum usum Ecclesiæ Herbipolensis, i. e. Wurtzburg in Germany. Rodolfus, archbishop of that church, sets forth in an instrument at the beginning of the book, the reasons why he caused this missal to be published, which instrument bears date the 8th of November 1481, by which time he orders all the copies to be finished by Jorius Ryser his printer, who seems to have done so, since his name, and this year 1481, is written at the end of the book. Instead of a seal to this instrument, is an engraven print being the arms of the see, supported by 2 angels, and St. Kilian, its first bishop and protector, behind; as also this prelate's own arms, with those of the see in another escutcheon, and a very fine mantling. This is extremely well engraven for the time, and equals the performances of some of our best workmen at present. The evident marks of pressure by the plate, with some touches of ink at the edges, the roughness of the print, and other circumstances concurring, I thought this must needs be wrought off at the rolling-press. But being unwilling to rely too far upon my own judgment, I showed it to several very skilful and curious gentleman, to several printers, engravers, and others, working constantly at the rolling-press, who all concurred, though at different times, one not knowing what another had said, that it was not only excellently well engraven, and this before Albert Durer's time, but that it was certainly pulled from the rolling-press, and could be done no other way. And that this print was not done after that time, appears from several notes written here and there in the book. One of them specifies that William Kewsth, vicar of St.

Bartholomew's church in Wurtzburg, bought this book the same year, 1481, paying 18 florins for the parchment, printing, rubrication, illumination, and binding. By another it appears, that he gave it to his church for ever. And by some others it appears that it remained there during the times of his several successors, till the last age, when, as I suppose, the Swedes, under Gustavus Adolphus, plundered the church, and carried the book away.

END OF VOLUME TWENTY-THIRD OF THE ORIGINAL.

Concerning Worms observed in Sheep's Livers and Pasture Grounds. By M. Leuwenhoeck. N° 289, p. 1522. *Vol. XXIV.*

In the summer of 1702, we had not rain enough to cover even the lowest parts of the meadows adjacent to our city; so that none of the sheep of that year drank of the waters that used to stagnate on those meadows, which when it happens, according to the opinion of our butchers, produces a certain sort of worms, called bottiens, in their livers. Notwithstanding which, I was informed that some of the sheep of those pastures had their livers infected with those worms: this made me conjecture that the above-mentioned distemper, in the livers of the sheep, must proceed from some other cause, than their drinking the said waters.

Therefore I caused a butcher, who was owner of one of these meadows, and who had also informed me that the sheep which he turned into that ground were greatly pestered with worms in their livers, to cut me two pieces of green-sod from thence, to the end that I might try whether I could find any such worms in them. This land he told me was so high, that it was never wholly under water in winter time; but the ditches about it were so full, that they were in a manner level with the land; and some of the lowermost parts of the same when it rained much, were for a short time covered.

I narrowly sifted those two pieces of the earth; but could find no animalcula in them, that any way resembled the worms in the sheep's livers. From hence I infer, that the animalcula that are found, not only in sheep's, but in cows' livers also, must not be sought for in those waters that stagnate on the land; but that we must seek them in the land itself; which being thoroughly wet or soaked, they ascend to its superficies, because the common water not being natural to them, they cannot live in it; and thus lurking in the grass, they are swallowed by the cows and sheep, and such as escape their teeth are conveyed into their stomachs and bowels, and insinuate themselves even into the liver.

I have been often told that the cattle which feed in Siltagtig grounds, are free from this disease of worms; but being informed that this kind of ground is very low, and lies under water the most part of the winter, I gave the butcher these reasons: why kine and sheep that feed in high clay grounds are troubled with worms in their livers, and those in low grounds are free, is, only because the low grounds lie all the winter under water; for though such like worms may be found in some of the low lands, yet as soon as they are overflowed with water, those worms, abhorring the water, die immediately. To confirm my reasoning, I took a glass tube, which at the upper end was about an inch wide and above a foot in length; I put into it a little piece of the above-mentioned earth, near 5 inches long, but so narrow, and the grass about it clipt so close that it would easily go into the tube without pressing, and then poured upon it boiled water, which had stood till it was cold: presently after I perceived that several very small and long white worms came out of the earth, which reaching and incurvating their bodies, subsided leisurely to the bottom of the tube, none of them being able to emerge to the superficies: whence I concluded that they could not live in the water; and in effect, after they had lain 24 hours in the bottom of the glass, I found they were all dead. It seemed to me also that these white worms consisted of several sizes or magnitudes, and that they could not be the offspring of our common worms, because they were much longer, in proportion to their thickness. I saw likewise a common worm creeping out of the abovesaid earth, which leisurely subsided and remained at the bottom of the tube, with little or no motion, and the next day it was dead.

As for those small animalcula that came out of the earth, and swam about the water, they were of so many several sorts and sizes, and were so exceedingly small, that I could not perceive what figure they were of, though I viewed them very carefully and frequently, and though I shifted the earth and water three times.

Now that these animalcula may be called water-worms, though they are found in the driest parts of the earth, appears from their living so well in the tube filled with water, in which, though I observed them day after day, I found no difference in them, save that they were increased in number, and besides I have met with several of them in common water.

At another time I went into one of the meadows near this town, which consisted of a good clay soil, and lay as high as any about town; I dug out a little bit of the said earth, about the size of a crown piece, which was covered with clover-grass, short and fine; imagining I should find in the top of it some animalcula, because I had formerly found in the rotten wood of a willow tree, and in another rotten plank that had lain in the open air, some of those animalcula which are usually found in the water. Having clipped away the grass from the clay, and put the top of it in a clean glass tube, about as thick as a child's finger, and poured upon it boiled rain-water, after it was cold, and having shaken the earth and water well together, the water was so thick and troubled, that I could perceive none of the animalcula, though there were a great many in it. But after it had stood about half an hour in the tube, I could perceive several animalcula creeping up its sides, and others swimming about the water. This water having stood several hours, and acquired a little more clearness, I saw two particular animalcula that came very near in figure to those that produce little wheels out of their bodies; only, instead of such wheels, they protruded a horny part out of their body, which they sometimes drew in, and then thrust out again; there was also one animalculum that put out two wheels, and near it I perceived two other sorts of animalcula, but immediately lost sight of them again; from whence I concluded, that so much water was not natural to them, and therefore they were dead; and after the water had stood three days on the clay, I saw several animalcula, that were four times as long and as thick, clinging to the sides of the glass, without any motion, though they stirred about briskly at the first.

I took another glass tube, and put into it a little of the same clay, which I handled very tenderly, pouring upon it some of the boiled water, as before, without shaking it at all, that the animalcula might emerge the better; and after an hour's time I saw above 20 animalcula swimming, whereas in the first I could perceive none; and one of them protruded wheels out of his body. Now it will appear strange to some, that these animalcula which usually and naturally swim in the water, should be found in earth that has not been moistened by rain, or otherwise, in several weeks: but they must be informed as I have often found by experience, that several sorts of very small animalcula are to be met with in rain-water, and especially in the gutters on the tops of houses; for I have taken some of the slime or dirt of those gutters, after they have been dry above a year, and diluted the same in boiled water after it was cold; on which I saw several animalcula swimming, and some of them, being folded up almost in a globular figure, extended their body leisurely, and then swam about in the water.

If it be objected, how comes it to pass that these animalcula, that are merely aquatic, should be found on the land several rods distant from any ditch; I answer, I conceive this to happen after the following manner. We often find that in a storm, the water has been so furiously driven again the sides of the ditches, and the parts of such water so minutely divided, that not only several of its smaller particles have been carried a great way into the land by the strong wind, but some of them also thrown up or exhaled into the clouds. In the said small particles of water are conveyed the above-mentioned small animalcula, far up into the land; and when the ground becomes dry, they contract themselves into an oval figure, and the pores of their skin are so well closed that they do not perspire at all, by which they preserve themselves till it rains, when they open their bodies, and enjoy the moisture.

The Solution of a Problem, proposed by Mr. John Bernouilli, in a French Journal, Feb. 1703. *By the Rev. Mr. John Craig.* N° 289, p. 1527. *Translated from the Latin.*

Problem. To find innumerable curves, that shall be of equal length to a proposed geometrical curve.

Solution. Let w, s denote the co-ordinates of the given curve, and x, y those of the curve sought: then from the condition in the problem it will be, $\dot{w}^2 + \dot{s}^2 = \dot{x}^2 + \dot{y}^2$. Put $\dot{x} = \dot{w} - m\dot{z}$; then will $\dot{y} = \sqrt{\dot{s}^2 + 2m\dot{w}\dot{z} - m^2\dot{z}^2}$: in this equation, for s let its value be substituted as expressed by $w, \dot{w}$, and determinate quantities; and for $\dot{z}$ let such a value be assumed, composed of w, $\dot{w}$, and determinate quantities, as that the fluents of $\dot{x}$ and $\dot{y}$ may be found. And thus will be determined x and y, the co-ordinates of the curve sought.

Example 1. To find a curve equal to a parabolic line. Let $2a$ be the latus-rectum of the parabola: then is $2as = w^2$, and $\dot{s}^2 = a^{-2}w\dot{w}^2$, and therefore $\dot{y} = \sqrt{a^{-2}w^2\dot{w}^2 + 2m\dot{w}\dot{z} - m^2\dot{z}^2}$. To find the fluent of this, assume $m\dot{z} = a^{-2}w^2\dot{w}$; hence $\dot{x} = \dot{w} - a^{-2}w^2\dot{w}$, and $\dot{y} = \dot{w}\sqrt{3a^{-2}w^2 - a^{-4}w^4}$; the fluents of which, by the methods already known, are

$x = w - \frac{w^3}{3a^2}$, and $y = \frac{w^2 - 3a^2}{3a^2}\sqrt{3a^2 - w^2}$.

Example 2. To find a curve equal to the circular arch. Let a be the radius of the circle; then is $s = \sqrt{a^2 - w^2}$; hence $\dot{s}^2 = \frac{w^2\dot{w}^2}{a^2 - w^2}$, and therefore

$\dot{y} = \sqrt{\frac{w^2\dot{w}^2}{a^2 - w^2} + 2m\dot{w}\dot{z} - m^2\dot{z}^2}$. To find the fluent of this, assume $m\dot{z} = \frac{4w^2\dot{w}}{a^2}$; then $\dot{x} = \dot{w} - \frac{4w^2\dot{w}}{a^2}$, and $\dot{y} = \frac{-3a^2w + 4w^3}{a^2\sqrt{a^2 - w^2}}\dot{w}$; the fluents of which, by the common methods, are found $x = w - \frac{4w^3}{3a^2}$, and $y = \frac{a^2 - 4w^2}{3a^2}\sqrt{a^2 - w^2}$.

Example 3. To find a curve equal to that of an ellipse. Let $2r$ be the latus-rectum, and $2a$ the transverse axis: then is

$s = \frac{r\sqrt{a^2 - w^2}}{a}$; hence $\dot{s}^2 = \frac{r^2w^2\dot{w}^2}{a^4 - a^2w^2}$, and $\dot{y} = \sqrt{\frac{r^2w^2\dot{w}^2}{a^4 - a^2w^2} + 2m\dot{w}\dot{z} - m^3\dot{z}^2}$.

To find the fluent, assume $m\dot{z} = \frac{2a + 2r}{a^3} w^2\dot{w}$;

hence $\dot{x} = \dot{w} - \frac{2a + 2r}{a^3} w^2\dot{w}$, and $\dot{y} = \dot{w}\sqrt{\frac{r^2w^2}{a^4 - a^2w^2} + \frac{4a + 4r}{a^3} w^2 - (\frac{2a + 2r}{a^3} w^2)^2}$;

the fluents of which, found by the known methods, are

$x = w - \frac{2a + 2r}{3a^3} w^3$, and $y = \frac{2a^3 - ra^2 - 2aw^2 - rw^2}{3a^2} \sqrt{a^2 - w^2}$.

Example 4. To find a curve equal to the cubical parabola, whose equation is $3a^2s = w^3$. Hence $\dot{s}^2 = \frac{w^4}{a^4} \dot{w}^2$, and therefore $\dot{y} = \sqrt{\frac{w^4}{a^4}\dot{w}^2 + 2m\dot{w}\dot{z} - m^2\dot{z}^2}$. To find the fluent, assume

$m\dot{z} = \frac{w^2}{2a^2}\dot{w}$: hence $\dot{x} = \dot{w} - \frac{w^2}{2a^2}\dot{w}$, and $\dot{y} = \frac{w}{2a}\dot{z}\sqrt{3w^2 + 4a^2}$; and the fluents are $x = a - \frac{w^3}{6a^2}$, and $y = \frac{(3w^2 + 4a^2)^{\frac{3}{2}}}{18a} = \frac{3w^2 + 4a^2}{18a}\sqrt{3w^2 + 4a^2}$.

From infinite other values of the quantity $m\dot{z}$ rightly assumed, may be found infinite curves equal to the given one. It may be further observed, that this problem has some kind of affinity with a certain Diophantine problem. His problem is, to divide the sum of two squares into two other squares, having their sides rational; and Bernouilli's problem is, to divide the sum of two squares into two other squares, the fluents of whose sides may be found. And as the solution of Diophantus's problem depends only on the common algebra, so the solution of Bernouilli's problem requires only the common inverse method of fluxions. The artifice of each consists in a due assumption of the sides required; that of Diophantus that the sides may be rational, that of Bernouilli that the fluents of the sides may be found.

A strange Effect of the late great Storm in Sussex. By John Fuller, Esq. in a Letter of Dec. 6, 1703. N° 289, p. 1530.

We live 10 miles from the sea in a direct line, and yet cannot persuade the country people but that the sea water was blown thus far, or that during the tempest the rain was salt; for all the twigs of the trees the day after were white and tasted very salt, as I am informed almost by every body, though I did not taste them time enough myself, nor observe it; and that not only upon this hill where we live facing the sea, but in all other places within 14 or 15 miles of the sea, as well in the valleys, between which and the sea are several very high hills, as on the hills themselves.

Observations on the late great Storm. By the Rev. Wm. Derham, F. R. S. N° 289, p. 1530.

Of the preceding parts of this year, the months of April, May, June and July, were wet in the southern parts of England; particularly in May, when more fell than in any month of any year since 1696; June also was very wet; and though July had considerable intermissions, yet on the 28th and 29th there fell violent showers of rain. And the newspapers gave accounts of great rains that month from divers places of Europe; but the north of England (which also escaped the violence of the late storm) was not so remarkably wet in any of those months; at least not in that great proportion, more than in the southern parts, as usually there are; particularly July was a dry month with them. September with us was a wet month, especially the latter part of it. October and November, though not remarkably wet, yet have been open warm months for the most part. My thermometer, the freezing point of which is about 84, has been very seldom below 100 all this winter, and especially in November.

Thus I have given a short account of the preceding disposition of the year, particularly as to wet and warmth; because I am of opinion that these had a great influence on the late storm; not only in causing a repletion of vapours in the atmosphere, but also in raising such nitro-sulphureous or other heterogeneous matter, which when mixed together might make a sort of explosion, like fired gunpowder, in the atmosphere: from which explosion I judge those coruscations or flashes in the storm proceeded, which most people, as well as myself, observed, and which some took for lightning.

On Thursday, Nov. 25, the day before the tempest, in the morning there was a little rain, the winds high in the afternoon, at S. b. E. and S. In the evening there was lightning, and between 9 and 10 o'clock at night, a violent but short storm of wind, and much rain, at Upminster, and of hail in some other places, which did some damage. Next morning, Nov. 26, the wind was S. S. W. and high all day, and so continued till I was in bed and asleep. About 12 that night the storm awakened me, which gradually increased till near 3 that morning. And from thence till near 7 it continued with the greatest violence; then it began to abate slowly, and the mercury to rise swiftly. The barometer I found at 12h. ½ P.M. at 28.72, where it continued till about 6 the next morning, and then hastily rose; so that it was gotten to 82 about 8 o'clock.

The degrees of the wind's strength not being measurable, but by guess, I thus determined with respect to other storms: on Feb. 7, 1699, was a terrible storm, that did much damage: this I number 10 degrees; the wind then

W. N. W. vid. Phil. Trans. N° 262. Another remarkable storm was Feb. 3, 1702, at which time was the greatest descent of the mercury ever known: this I number 9 degrees. But this last of November, I number at least 15 degrees.

I have accounts of the violence of the storm at Norwich, Beccles, Sudbury, Colchester, Rochford, and several other intermediate places.

I have just received an account from a clergyman, an intelligent person, at Lewes in Sussex, not only that the storm made great desolations thereabouts, but also an odd circumstance was occasioned by it, viz. "That a physician travelling soon after the storm to Tisehyrst, about 20 miles from Lewes, and as far from the sea, as he rode he plucked some tops of hedges, and chewing them he found them salt. Some ladies of Lewes hearing this, tasted some grapes that were still on the vines, and they also had the same relish. The grass on the downs in his parish was so salt, that the sheep in the morning would not feed, till hunger compelled them, and afterwards drank copiously, as the shepherds report. This he attributes to saline particles driven from the sea.—He hears also, that people about Portsmouth were much annoyed with sulphureous fumes, complaining they were almost suffocated with them."

Observations on the same Storm. By M. Leuwenhoeck. N° 289, p. 1535.

Upon the 8th of December, 1703, N. S. we had a dreadful storm from the south west, insomuch that the water, mingled with small parts of chalk and stone, was so dashed against the windows, that many of them were darkened with it; and the lower windows of my house were not opened till 8 o'clock that morning, notwithstanding that they look to the north east, and consequently stood from the wind, and though guarded from the rain by a kind of shelf or pent-house over them, were yet so covered with the particles of the water which the whirl-wind cast against them, that in less than half an hour they were deprived of most of their transparency. Supposing this might be sea-water which the storm had not only dashed against our windows, but spread also over the whole country, I viewed the particles with my microscope, and found they had the figure of our comman salt, but very small, because the water was little from whence those small particles proceeded; and where the water had lain very thin upon the glass, there were indeed a great number of salt particles, but so exceedingly fine that they almost escaped the sight through a very good microscope.

But as to the upper windows, where the rain had beat against them, and washed them, there was little or no salt to be found sticking upon them.

During the said storm, and about 8 o'clock in the morning, casting my eye on my barometer, I observed that I had never seen the quicksilver so low; but half an hour after, the quicksilver began to rise, though the storm was not at all abated, at least to any appearance; from whence I concluded that the storm would not last long; and so it happened accordingly. Some persons feared that the scattering of this salt water by the storm will do a great deal of harm to the fruits of the earth; but, for my part, I am of a quite different opinion; for I believe that a little salt spread over the surface of the earth, especially where it is heavy clay-ground, renders it very fruitful; and so it would be if the sea sand were made use of to the same purpose.

Concerning the Figures of Sand. By M. Leuwenhoeck, F. R. S.
N° 289, p. 1537.

I have formerly affirmed of sand, that you cannot find in any quantity whatever, two particles that are entirely alike; and though perhaps in their first configuration they might be alike, yet at present they are exceedingly different; the sand, especially what we make use of, is liable to so great alterations, that it would be a wonder, if even in its smallest particles, of which there may be a thousand in one small grain, there should be an exact similitude. For let us conclude, that our common sand, of which one grain differs from another in transparency, was at first formed with smooth sides and sharp points; but these are so rubbed against one another, that several small holes may be perceived in them, by which they lose their first figure; and who can conceive what changes those particles of sand undergo, that lie next the superficies of the terrestrial globe; especially such as lie at some depth under a stony ground, where laden waggons often pass; for those particles of sand by frequent compressions and collisions, indent holes in their sides, and break off the sharp points or angles; not to mention what alterations are made in these small bodies by storms, thunder, earthquakes, &c.

I got some shining sand, which, though very thin, was not transparent, its lustre being wholly occasioned by the reflection of the light from its polished sides; several particles of this sand, which were much larger than the rest, reflected no light, though they seemed smooth to the naked eye; from whence I concluded that they had lost their lustre by the frequent rubbing of their sides against others. When I viewed several grains of the sand with my microscope, I was surprised to see that many of them were hexangular, and the more when I had sifted the finest from the coarsest; neither could I observe that any of the sands were like each other. I viewed the said sand with great attention several times, imagining that by some earthquake or otherwise it might be thrown up, from the place where it had lain at rest, to the superficies of the earth; and many grains as I fancied had preserved their original shape and figure; for they had received little or no damage, especially the small ones, and many of those had such points and sides that no polished diamond could equal their beauty.

I infused some of this sand into aquafortis, to try whether it would dissolve them, or deprive them of their shining quality; and though they lay in it several days, I could not perceive any alteration in them. I tried the sand also, with a fire brisk enough to melt silver, and yet it did not affect either the figure or lustre. When viewed with the microscope, several appearances of them were as follow.

Plate IV. fig. 1, ABCDEF represent a hexangular grain of sand, that was as bright and shining as any polished metal; and the triangular figures, which appeared on it, were as bright as the rest of the body, which occasioned a very agreeable sight.

Before one of my glasses I placed another grain of sand, less than the former, but it was flat, and not the 16th part so large as a coarse grain picked out of our common white sand. This was a surprising sight, and is represented at fig. 2, GHIKL, where you may see not only as it were a ruined temple, but in the corner of it GHI appear two images of human shape, kneeling and extending their arms towards an altar, that seems to stand at a small distance from them; this was yet the more agreeable that it was as bright as any polished steel.

Fig. 3, MNOP represent as near as could be traced, another hexangular small sand, with two sharp points like pyramids, and each side that composed them very smooth and shining: I have seen several such sands, that on each side had a smooth, shining and oblique superficies, sometimes on one single grain to the number of 24 such polished sides or faces.

I have also observed several small sands, which, instead of terminating their six sides in a sharp point, ended sometimes in a triangle, quadrangle, and even in a pentagonal or hexangular shining flatness. I remarked several three sided sands, of which some were regular triangles, which were very thin and shining, others were thicker; as QRS, fig. 4.

There were other sands, that were complete hexangles, the flat sides of which appeared like a steel looking glass in a frame; and in some of them were little holes, which seemed to be likewise hexangular; whence I concluded that such a hole was made by the pressure of another sand of the like figure. See fig. 5, TVW. When I viewed any of these sands sidewise, each of the six sides, which in the figure appear as a frame or border, seemed to be a polished looking-glass.

Fig. 6, XYZ also represents a hexangular sand opposed to the sight a little sidewise, by which the reflection is not so full and large, as if the flat side were placed directly before the eye; but then it came from the two sides which represent a part of the frame, and are shown in this figure by the letter x. Now when the hinder part of such a sand is brought before the sight, I observe that it has the same figure, and that then that part of it, which I should have described as a dark circle of the shining sand, was composed of 12 bright, oval, flat superficies.

ABC, fig. 7, represent another hexangular sand, whose bright superficies or area was of a different make from its circumference; for there appeared in the middle of them several triangular figures, which, though they were something raised or imbossed, were very bright, and very pleasant to behold; and though the circumference in this position appeared very dark, yet the sides when opposed to view were no less bright and shining.

Fig. 8, DEFGH represents another grain of sand, with its protuberant parts, and their respective sides; but it is impossible to describe with the pen the beauty and variety of the figures in the said sand, neither can any one conceive it, but those that see it. I turned its opposite side to my glass, and I discovered its several shining angles as in fig. 9, IKLM. I placed another small shining sand before my glass, which appeared as in fig. 10, OPQR, several of whose sides were unblemished. In short, should I undertake to give you a view of 1000 others, and should enter on a strict examination of every one of them, I doubt not but we should discover every one of them to be of a different size and figure, besides several other particularities which might be peculiar to each one, as the great rent or breach in fig. 8 is described by aa. I have also observed that this shining sand weighed twice as heavy as our common scouring sand. Now on taking some of the pellucid or transparent sand (which did not shine, because it reflected no light) I observed that the sides and angles of each grain were freer from scars and blemishes, than most others I had yet considered; from whence I concluded that such sand had not lain long near the surface of the earth.

I placed one of these sands before a microscope, so as to have a full view of three of the four oblique sides, as fig. 11, ABCDEF; one of the flat sides being represented by BCDG; another by DEAG, which had a streak or scratch in it, supposed to be done by the pressure of another sand upon it; and the third flat side by BGab.

Having represented this transparent sand in its shortest position, I put it in another sight, so as to see it in its full length, as in fig. 12, HIKLM, with the streaks or little holes, which I firmly concluded to be occasioned by the collision of other sands. I likewise observed some few sands, that were long and slender, and which did not appear thicker through a common microscope, than a single hair of one's beard to the naked eye.

I have taken several of the shining sands and broke them to pieces; and then viewing the broken pieces through a microscope, I observed that many of those small particles, though they were a thousand times less than a grain of common sand, had a glance or lustre when the light fell upon them: and that several such particles, and some that were larger, if viewed against the light, presented a fine blooming red; but some of them were so only in part; from whence I concluded that part which was not red, was thicker than the rest, and did not admit light through it. Among these small broken pieces of sand, I observed some that had six sides, others that were triangular; all which are to be considered as common sand. I took one of the above-mentioned long slender sands, which seemed to be one of the thickest, and placing it before a microscope, it appeared as in fig. 13, NOPQ, in which may distinctly be perceived four flat sides. I have observed in many sands, that their bodies consisted of unspeakably thin scales or scaly particles.

Among these shining sands, I discovered others that had no lustre at all, neither had any of their particles, after broken to pieces, but it appeared to be a dark red matter; and in other sands, so broken, there was not only a red matter, but even 100 shining particles, all proceeding from one sand. I have also seen some sands, which, in the middle of their shining sides represented small figures without lustre; but on viewing them more narrowly, I found it was a red matter, incorporated as it were in the sand. Of several sands of the coarsest sort, placed before a microscope, one seemed to represent an agreeable rock of stone, another a deep cavern, &c.

These last sands were not, I suppose, so shaped from the beginning; but those cavities and protuberances which were seen, came from various chances and accidents; as by the collision of other particles of sand, which were larger than these, which in their turn were again broken by other bodies of a larger size, such as pebble stones, &c.

I have split some grains of sand, and discovered figures of triangles in them.

Soon after the cathedral church at Utrecht was almost ruined by the dreadful storm, my curiosity led me to view the fallen pillars, and other parts of that building that lay about; when I observed that several of the pedestals were composed of red stone, as also some tomb-stones; and that in another church there was a whole pillar of such stone; on viewing some of this with the microscope, it appeared to consist for the most part of large and transparent grains of sand; and that the redness of the stone was only caused by a certain red matter, which had produced such a continuity or adhesion in the mass of the sand, as to be able to consolidate it into a red stone. Being of this opinion, I gently separated several sand-particles from it; and because the red matter which adhered to some of the sands more than to others, hindered me from taking a distinct and clear view of each particular grain, I put some of them into aquafortis, to try whether that would dissolve the red matter, and restore its lustre to the sand; but in vain, for that menstruum could not separate them. I then took several of those particles of sand, that were least surrounded with the red matter, and placing them before the microscope, observed that some of them had 10 or more sharp points or protuberances, so neatly and regularly fashioned, that no polished diamond could outdo them. I set one of the largest grains of sand before my glass, and could perceive in it, and that in a very small compass, 7 neat prominent points, exceeding each other in length and size, and issuing out of very smooth sides; and I judged that one such sand was furnished with above 100 flat sides, which were very smooth, and consequently very shining, though with the naked eye no brightness could be discovered; the reason of which I conceive to be, that this sand being partly transparent, admitted the light through its pores, and did not reflect it back like the shining sands.

These sands, though I viewed ever so many of them, I found to be each of a different figure; and when I picked out any that were somewhat oblong, I seldom failed to discover at the ends a four or five sided obtuse point, so neat and regular, as if it had been polished. I broke to pieces several of these grains of sand on a clean glass, and found that they were not near so strong as the shining sand; and having placed the small particles of such a broken sand before my microscope, some of them appeared like whole sand, with its entire sides and angles; all which formed a very pleasing sight.

Fig. 14, ABCDE represent, through a microscope, one of the above-mentioned sands, of which many together compose the red stone; ABCD show the points, which are much more plainly seen than several others that cannot be distinguished, because of their different positions.

Fig. 15 shows another sand, which likewise has several points, though not so obvious as the former, because they do not stand so far outwards.

Fig. 16, MNOP represent the last mentioned sand in another position.

Fig. 17, QRST show also a sand of the aforesaid stone, in which the diamond cut is better than the former.

Fig. 18, VWXYZ represent two grains of sand, that were still joined to each other; the one is shown by VWX, wherein may be observed several points, and particularly between W and X; and the other is shown by XYZ, and the most of its points lying between Y and Z.

Now as some of the smooth sides of these sands were large, and others small, and as all of them were not so smooth as polished glass, and had several small scratches or slits in them, I suppose that might happen by the breaking or dividing of these sandy particles from each other.

That piece of the red stone still remaining entire, was about the size of a pea; and when I beat it in pieces, a spark of fire flew out of it. I made a small bit of it glowing hot, and so let it drop into water, supposing that not only the particles of sand would thus be separated, but that the red matter also which consolidated the sand would be divided from it; but I found that the sands only were separated from each other, and each particle of sand was as strong as if it had never been in the fire, and was also surrounded with the red matter; but in some of them, which had assumed a greater transparency than before, I could plainly discover that each particular grain consisted, or rather was a congeries of several small particles, of which could be seen in some sands, 50 such projecting like pointed pyramids, all transparent, and some of them had the same figure as the grain of sand itself had. By these observations I was fully satisfied, that the sands of which the above-mentioned red stone was composed, had for the most part preserved their original figure, and that they were so hard and solid, that their falling one upon another could not produce any adhesion, otherwise than by the intervention of that red matter, which was interspersed and mixed with them.

From these observations I was naturally led to consider diamonds; and my hypothesis is, that all the diamonds that have been, or shall be discovered, do not grow, nor are made in any series of time, but were formed like other sands, in the beginning of the world; for how is it possible that such a pellucid body can be produced in the bowels of the earth, by a congealing or coagulated succus or moisture; and if it were so, why do we not meet with very large diamonds? for when a small diamond is once formed, there would be a more than ordinary conflux of the same plastic matter to it, as we find in other things, that they have always a strong tendency or inclination to substances of a homogeneous nature with themselves. I have been assured that in some places, water filtrates through the rocks into the subterraneous caverns, and coagulates at the top of those vaults into icicles, like nine-pins, and at last are really petrified. And yet I believed that these petrifactions, viewed by the microscope, would be found to differ from the rock itself. To be satisfied in my observations, I took a piece of white marble brought from Italy, which was of two sorts, the one strong, the other light and very brittle. I broke the brittle marble gently, that the configuration of the small particles might not be much altered; and having viewed several of them with my glass, I saw abundance of surprising particles, which may justly be stiled sands, with their regular sides and angles, and many of them of the same figure as the shining sand.

But not being satisfied with this hypothesis, viz. that the abovementioned particles were originally nothing else but sand particles, with their angles and smooth sides, and that at the time of their coalition or falling upon one another, they were all soft and flexible, and thereby were so rivetted and joined together as to become one solid body, which we call marble; and consequently that the said particles did for the most part change their figure, and assume another form, proportionable to the solidity of their coalition, and that yet some of them had preserved those smooth sides and angles, which they had at the time of their conjunction; which sides and angles represent the points that are found in diamonds; and that in one particle of sand may be discovered 3, and in another 7 neat and regular points.

From these observations, I imagined that almost all the sand of the whole earth has preserved the figure given at the creation, and that their particles, before they happened to collide or fall upon others, were composed of such hard bodies, that they could not be joined to others, and so have remained what they originally were; only that by their collisions with other bodies, their first figure may be somewhat impaired; and the nearer they lie to the surface of the earth, the more subject they are to such alterations.

When I opposed such a diamond-like sand with its point to the sight, I could see its point; but the sides from whence the points arise, are more obvious; and in such a position the sand would appear 4, 5, or 6 sided; and not only so, but where two sides joined, I could also perceive such a point of a diamond-cut projecting out.

I have caused three of the sand particles of the abovementioned white marble, as they were separated from each other, to be drawn, as follows. Fig. 19, ABCDEF represent one of those grains of sand, of which a great number compose the white Italian marble, where at each of these capital letters may be seen its points or angles. Fig. 20, GHIKLM shows the second sand, and those letters the several points. Fig. 21, NOPQ is the third sand, wherein there is such a flatness NOP, as is observable in few sands.

I took a little piece of the said marble, the parts of which were very close and compact, and making it red hot, I dropped it in the water; in doing which, I observed not only that the sand particles were separated from one another, but some of them broke off with such violence, that they were thrown out of the water.

I presently took some of this water, to see what salts it had received from the stone; and I soon perceived a sort of film, or scum over the water, without being above to discover any particles in it, which I conceived to consist of coagulated salts; and that there were some such under the scum, but they were so obscure that I could not well perceive them; and the more, as they were hid by the scum.

I took then a larger piece of the abovementioned stone, and heating it red hot, I let it fall into the water also; on which there came a thicker scum over the water, which I let lie upon it 24 hours, and then breaking it, and taking a little out of the water, I found it hard and petrified; and placing it before the microscope, to discover, if possible, the figure of those particles, I could not however accomplish my intentions, though I often repeated the trial, by reason of the smallness of the particles, and their strict union with each other; only a few of them appeared to be 4 and 6 sided, and to reflect the light from so many parts; others, that were larger, were composed some of 3, some 4 smaller particles, which we may thus reckon to be salt ones. Several thousands of these small particles lay in a very small space; which being separated and broken in pieces, I judged that each particle consisted of many more, and much smaller ones; and indeed they were so minute, that they almost escaped my sight, through the best of my microscopes. I tried some of the same particles in a pretty strong fire, and found that they lost some of their transparency, and were divided into particles, that were quite dark and obscure.

After this, I took a little of that water which lay under the surface of the supposed coagulated salts, imagining I should find some salt particles in it, and putting it upon 3 clean glasses, I observed that after the space of 4 hours, the water was quite exhaled, and that there remained many thousands of salt particles out of one single drop of water; which salts were all separated from one another, many of their surfaces being very bright, but their sides dark; and I could clearly perceive that some of their surfaces were shining, and their figure quadrilateral: the darkness of their sides I judged to be, because those salts were squares, in shape like dice; between these lay other particles, that were much smaller, and I believe of the same figure too, but because of their smallness I could make no certain judgment of them.

After the water, which lay very thin, was suddenly exhaled, I saw several salts that were larger than the rest, and more irregular in their figures, but coagulated together, some of which were squares, but not regular. I took about ten times as much rain water, and mixed it with the salt water, and then observed that the salts were not dissolved, but emerged to the top of the water; but when I pressed the same salt particles under water, so as to wet them all over, they subsided to the bottom, and remained there, without being dissolved, or uniting themselves to the water.

Now seeing that the great number of salts extracted from the stone are of so obstinate and tenacious a figure, that nothing but fire can divide them, and that they are such inflexible bodies, that they cannot be dissolved in water; we may well conclude, that the grains of sand which compose such stones, were not only soft at the time of their coalition or union with each other, but that at the same time there intervened a very inflexible fixed salt, instead of mortar, between the particles of sand; unless you chuse to say, that each particle of salt in some degree consists of such fixed salts.

After this I took a piece of hearth stone, called Benthemer stone, because found in quarries in the county of Benthem: this stone was so soft, that I could easily crumble it between my fingers; and afterwards viewing it with one of my glasses, could perceive nothing but particles of sand, without the least smooth side, or regular angles; and it seemed to me, that this sand had acquired a sort of conglutination, or was grown into a solid substance, which we call stone, a long time after it had been nothing but sand, and its particles had been worn and collided against each other. My reason is, because this sand, that had been lately stone, was as full of small holes and breaches as any sand I ever saw; and in viewing them, they seemed to be composed of thousands of smaller particles; and that some of them were of a triangular, others of an exact flat quadrangular figure; and when I observed these sands at rest, I judged that the original shape of many of them were hexangular, and many were pointed like diamonds, which points issued out from smooth flat sides.

I took a piece of alabaster stone, and having viewed it several times, I observed, after breaking or beating it very small, that the little particles were very thin and pellucid, and their figure a long and flat square, with two acute angles and two obtuse ones; and though I saw among them some others that were not quite so regular, yet I supposed that the exactness and regularity of their figures had been lost in the violent separating them from one another. The particles were for the most part so exceedingly small, that they could hardly be seen through my best microscope; but some of them of a larger figure, appeared to be composed of very thin particles lying upon each other. Now, on viewing those particles of the stone that were as large as grains of sand, I found that each of them was composed of several thousands of smaller particles, whose shape I could not discover; and when I made a little bit of the stone red hot, and dropped it into the water, it dissolved into a white substance as fine as meal or flour, losing all its transparency; and each particle, though its figure had been an oblong square, was now composed of such small particles, that it was impossible to perceive any shape it had.

After this, I took a small piece of mineral stone, brought from Sumatra, which was so rich, that 100 cwt. of it contained near 50 gilders of silver, and 30 in gold; the piece was about the size of a common bean, and putting it over a pretty smart fire, the sulphur, of which there was a great deal in the mineral, stood in bubbles, and remained on the stone in the figure of round, black, burnt globules; I then dropped it red hot into water, where it remained whole, only with this difference, that whereas before it was very hard, now it became very brittle; and having broken it, I perceived it to consist chiefly of irregular particles, though some few were of an exact diamond-cut; and I could see, much more plainly than before, the globules of gold and silver, lying separately from one another, some of the former of which were so exceedingly small, that they almost escaped the sight in the microscope; and as gold is not near so easy to be melted as silver, I saw some that the fire had not force enough to reduce to globules, and upon it lying a small globule of silver, which the fire had brought into that figure; and though the gold and silver touched each other, yet they were not united, because the fire where that gold lay, was not strong enough to reduce it to a fluid body,

I have often observed in these mineral stones, that one part of them was white, and another, a little lower in the same stone, a dark grey colour; and that they are brought to us in little pieces, the largest not exceeding a joint of one's finger; and that in some few pieces there were little cavities or breaches, without any metal, either of gold or silver, (for in that mineral there is no other) but oftentimes with very small crystals; which I call so, because of the analogy of their figures with those of rock crystal, being, like them, transparent, hexangular, and ending into a point or spire. These crystals were in the white part of the stone; for those that were in the grey were not pellucid. RSTV, fig. 22, represent a small particle of such crystal, clear and transparent.

Some believe that these crystals are formed by the exhaling damps, or moistures, deep in those mines, from whence the mineral stone is dug; but this is contrary to my opinion; for upon that hypothesis, the whole cavities should be filled with the particles; whereas we find the contrary; for I have observed that not one fourth part of the cavities are filled with them; and I have found some of the said crystals in such small cavities, that a pin's head would have filled them, and in breaches of the mineral stone that were not so broad as the thickness of a small pin; whereas I could meet with none of these crystals in other cavities that were much larger. I rather suppose that most of the stony matter where those cavities are found, was of the same configuration as the said crystals; and at the time of the coalition or union of the particles of stone, the intercepted air occasioned some cavities, in which the crystals were inclosed, and in which they acquired the figure which they now retain; in the mean time the other parts about these cavities were so firmly united, as to become a hard and solid stone.

Account of an Earthquake in the North of England. By Mr. Thoresby. N° 289, p. 1555.

This earthquake was felt at Hull, on Tuesday, the 28th of December 1703, about 3 or 4 minutes after 5 in the evening. It lifted up chairs and tables, and made the pewter-dishes and windows rattle, shook whole houses, and threw down part of a chimney. The shock came and went suddenly, and was attended with a noise like wind, though there was then a perfect calm. Much in the same manner was it felt at Beverley and several other places, particularly at South-Dalton. It was still more violent at Lincoln, where also it raised up the chairs people sat on, &c. It was felt pretty much at Selby, as also near Navenby; where the sudden noise seemed to be like the rumbling of two or three coaches driving furiously, and shaking the chairs on which people sat; and even the very stones were seen to move. A little before there was a violent storm; and at Leeds there was a much greater storm the preceding night, and next day, than was lately in the south parts of England, of which there is an account in a former part of this N° of the Transactions.

An Account of a Book, viz. Euclidis quæ supersunt omnia Gr. Lat. ex Recensione Davidis Gregorii. M.D. Astronomiæ Professoris Saviliani, et R.S.S. Oxon. 1703. *Folio.* N° 289, p. 1558.

In this edition is published whatever is believed to be Euclid's, by any mathematician of eminence. But as many things have been attributed to Euclid that are not his, Dr. Gregory, in the preface, after a short summary of Euclid's life, gives not only a description of every particular book, but also his opinion whether it be truly Euclid's or not, with his reasons. Then come the elements, which make two thirds of the whole volume. The first 13 books are certainly Euclid's. The 14th and 15th are by most thought to be by Hypsicles of Alexandria. There are no scholia, no explications added to the elements, (being thought needless to a book of elements, written with so much judgment as this is) nor any notes, except in some very few places, where there are various readings that are material, or where the text is manifestly corrupted.

Next come the Data, which are undoubted Euclid's, even more undoubtedly than the elements themselves. For it has been said that Theon quite changed the elements, and supplied their demonstrations, but no one has ever questioned whether the Data are Euclid's. Dr. Gregory, in the preface, compares this book with Pappus's description of it; restores some places in it that have been corrupted; and shows the use that the ancients made of these data.

The two Musical Tracts follow, which the editor thinks are not both Euclid's, perhaps neither, as is shown in the preface. Next are Euclid's Phænomena, which were never before published in Greek. This book is not doubted to be Euclid's, as agreeing with Pappus's description of it. Dr. Gregory has restored its own original figures, which Josephus Auria, in his translation, had changed for others, far less convenient and intelligible.

After this come the Optics and Catoptrics, which, if not spurious (for Proclus indeed mentions books of Euclid concerning these subjects) are very much corrupted, as in the preface is fully shown. To these are added the notes of Sir Henry Savile, which he wrote on the margin of his own book, and which show that he was as great a master of mathematics, as he was a patron of them.

Next is the book De Divisionibus. This commonly goes under the name Machometes Bagdedinus. But because there is no other book extant of Euclid's with this title, although it is clear from Proclus that he wrote such a one; and because Mr. John Dee, who translated it, thinks that this is Euclid's, it was thought proper to publish it here. To this also are added some notes of Sir Henry Savile, which much elucidate the matter. Lastly, is a Fragment de Levi et Ponderoso, published by Hervagius in Latin, and by Tatraglia in Italian, which commonly passes for Euclid's. These last two are not to be found in Greek, being lost, if ever they were in that language. There are several other of Euclid's works, mentioned by Pappus and Proclus, that are quite lost. These Dr. Gregory describes at length in the preface.

A Letter from Dr. William Oliver, F.R.S. concerning a Calenture. N° 290, p. 1562.

In the month of August 1693, (then in the Bay of Biscay) in lat. 47, a sailor on board the Albemarle man of war, between 30 and 40 years of age, was seized with phrenitis, or as Dr. O. terms it, a calenture. He was raving and so violent, that 3 or 4 men could scarcely hold him. He was recovered by copious bleeding, 50 ounces of blood (as near as could be guessed) being drawn from him by 3 orifices, in the space of $\frac{1}{2}$ an hour.

A New Division of Terrestrial Brute Animals, particularly of those that have their Feet formed like Hands; with Observations on the Opossum. By Edward Tyson, M. D. F. R. S. N° 290, p. 1565.

Having formerly given the anatomy of a female opossum, (Phil. Trans. N° 239*)

* See page 248, vol. iv. of this abridgment.

I shall now add that of the male. But referring to Mr. Cowper's dissection, next following this paper, I shall only here observe a few things, further illustrative of the history of this animal.

With respect to the species of this animal, having upon dissection observed the penis to be fleshy, and to have no bone in it, I find it cannot be referred to the dog or weazel kind, as some have thought; and what Piso means by his glires majores montani, to which he refers it, I shall not here inquire. I must confess we cannot be at a certainty in this matter, unless we had a more perfect enumeration and description of the several sorts of animals that are in the world; and by a strict inquiry into their inward as well as outward parts, observed, how gradually they differ from one another; by easy and gentle steps the intermediate species linking the whole together. However till this can be attained, every little help will contribute somewhat.

To give therefore my thoughts on this subject, I shall here propose a division of such terrestrial animals, as have several divided claws and nails, into

Animalia Χειρο-δάκτυλα, seu manu digitata, i. e. whose feet resemble hands, and have fingers rather than toes.—Ποδο-δάκτυλα, seu pede digitata, i. e. whose digiti, from the order of their position and shortness, as also uses, more resemble toes than fingers.

For though our language makes a sufficient distinction between them, by calling the one fingers, the other toes; yet the Greek and Latin do not; for δάκτυλοι and digiti signify those on the hand, and feet likewise. To discriminate them therefore, they are obliged to add another word, as digitus manus, or digitus pedis; which sufficiently justifies our distinction of Χειρο-δάκτυλα and Ποδο-δάκτυλα.

Now we may observe these differences between the fingers and toes, viz. that the fingers are much longer, having usually a thumb set at a distance from the range of the other fingers; and are so contrived, for the better holding what they have a mind to, and especially in these animals, to assist them in climbing trees, &c. for catching their prey. Whereas the toes are shorter, and are set in a more even range together, and better contrived for swift running; by which way this other sort of animals take their prey.

This latter sort we shall not insist on here, but rather give a subdivision of the former, viz. those animals which have their feet formed like hands. Now where there is a thumb, though we may esteem the hand there more perfect, yet I find it is not always necessary; for in several animals it is wanting, as will appear by the following scheme; which I propose here only as an essay or hint, to be enlarged and amended by farther observations.

Animalia χειρο-δάκτυλα, quorum pedes omnes sunt manu-formes, seu animalia quadru-mana.

Cum pollice in omni pede, sc. simia (caudata, non caudata,) romack, coati Brasil. &c.

Sine pollice in pedibus.—Anterioribus, vantrevan, sciurus, mus, &c.—Posterioribus, genus felinum.—Duo tantùm sunt manu-formes, sc. vel

Anteriores, cum pollice; mantegar, &c.—sine pollice; cuandu Brasil. Margravii, tlaquatzin spinosum Hernandez, hystrix, tamandua Brasil. &c.

Posteriores, cum pollice, carigueya, s. opossum, &c.—Sine pollice.

Under the first member of this division I include the ape and monkey kind, which, as I have shown in my discourse on the ourang outang, ought rather to be reckoned a four-handed than a four-footed animal. And considering how large a species of animals may be reduced under this quadrumanous kind, agreeing in this particular, though in others different, I think it but just to assign them a general class, afterwards to be subdivided according to the gradual differences they have from one another.

The romack therefore, though differing much from the monkey kind in the head and face, yet being quadrumanous, and on each hand having a thumb, I reduce under this head. This animal was brought alive from Fort St. George. Whether it is described by any, or what other names it is called by, I know not. And because in its face and head it so much resembles a fox, and in the rest of its body a monkey, I shall call it ἀλωπη-πίθηκος, vulpi-simia, or the fox monkey. But the next I have mentioned in this class, the coati of Brasil and Virginia, or the rackoon or rattoon, though it does not resemble the monkey kind in its body, yet because it has hands like a monkey, as Margrave tells us, I place it likewise here; as may be all others whose feet are all formed like hands, and have a thumb on each.

For there are some that have not a thumb on their fore-feet, and others that want one on the hinder. In the number of the former may be reckoned the vantrevan, the squirrel kind and mouse kind, or any others that may be observed to have all their feet formed like hands, only that their fore feet want a thumb. The vantrevan, as it was called by the person who showed it here in London, altogether resembles a monkey: on the fore feet it had only four long fingers, but no thumb. It is a beautiful animal, very brisk and nimble in motion, and is loving; it has a very long tail, by which it suspends its body as the opossum does.

The squirrel kind has on the fore feet four long fingers, on the hind feet five, and one like a thumb. It uses its fore feet like hands in holding up its food to its mouth, and lives on trees, as monkeys do. But the affinity between the monkey and squirrel kinds appears better by some monkeys I have seen, which on the belly have a large thick fur, and a thick brushy tail like the squirrel; whereas usually the ape and monkey are thinner of hair on the belly, and that on their tail is shorter. This sort of monkey I call therefore the squirrel monkey, or sciuro-pithecus. But its face more resembled a man's or an ape's, as likewise its teeth, and in these respects it differs much from the squirrel kind.

Nearer to the squirrel comes the mouse kind, which in the shape of its head, the long teeth before, and the large and prominent eyes, it more resembles; and it uses its fore feet as hands in feeding itself, where it has only four fingers without a thumb, but on its hinder it has five, of which the innermost and outermost are placed at a distance from the range of the three middle fingers, like two thumbs, as may be observed in some of the lizard kind.

Some may question why we should include the cat kind in the number of the animalia χειρο-δάκτυλα, since their feet seem rounder, and to have rather toes than fingers, But we may observe that it uses its fore feet like hands in climbing and catching its prey; and when it does so, it exerts its claws and lengthens them; but when it uses its feet in going and running it shortens them, as being most convenient for that purpose; so that it is well provided for both, and its digiti are of a middle nature between fingers and toes, as they are lengthened or shortened. And we may observe that on each fore foot there is a pollex or thumb, set at a distance from the range of the other claws, by which they more resemble hands, and on the hinder feet there are only four digiti, without a thumb.

We come next to those animals that have only two feet formed like hands, and those are either the fore feet or the hinder. Those whose fore feet only are formed like hands, have either a thumb there, as the mantegar, &c. or have only four fingers without a thumb, as the cuandu, &c.

The mantegar* is not described by any author, and the strangest that I have seen. It is about the size of a mastiff dog; it measured from the extremity of its nose to the anus 3 feet 2 inches; the girth of the body was 2 feet 2 inches; the head 14 inches long; the forehead 5 inches broad; the head somewhat resembling that of a horse; the nostrils large; the nose of a deep cinnabar colour, and the bones of the nose depressed lower than those of the upper jaw, where the skin was of an azure blue colour; there is a large tuft of hair on the forehead, as also under the chin; the forepart of the body, and the inside of the arms and legs, are almost bare of hair; the hair on the outside of them of a mottled brown and olive colour; on the back it is blackish; there were mammæ on the breast; it has an umbilicus; and the præputium is without a frænum, as

* The animal meant in this description is the *simia mormon*. Linn.

in the ape kind, and is of a vermillion colour; the scrotum of an azure colour; it has no tail: it is very fierce, having two long tusks in the upper jaw, and is very lascivious; the fore feet perfectly resemble hands, having long and thick fingers and a thumb, and all the nails of those fingers flat; the nails on the hinder toes and fingers are imbricated, not flat; and though the claws were pretty long, and somewhat imitating fingers, yet the thumb is not so perfect, and the whole differs from the fore feet. When sitting and supporting itself by a stick in one hand, being thus erect, and holding a cup in the other, it would drink out of it, and not lap; its food was chiefly fruits.

Among those animals whose fore feet are like hands, and have no thumb, I reckon the porcupine kind: as the cuandu of Brasil, a sort of porcupine described by Margrave and Jo. Nieuhoff, Voyages p. 18, which on the fore feet has only four fingers, on the hinder five. Therefore, as Margrave observes, for want of a thumb, it is but slow in climbing trees; but the better to help itself it twists its tail about a bough to save itself from falling. And much alike, if not the same, is the tlaquatzin spinosum of Hernandez. Also the common porcupine, before has four fingers, behind five. So the tamandua of Brasil, or ant-bear, before has only four fingers, where the want of length in the fingers is supplied by that of the nails, and behind it has five toes. But I must confess there must be some allowance made for ranging this anomalous animal, as Mr. Ray calls it, here; but because he climbs trees, and in doing this makes use of his tail, as some others here mentioned do, I was willing to include him with the rest. And we may likewise bring in here the ai, the ignavus, or sloth, because it climbs and lives on trees, and has a head not unlike an ape's; and, as Margrave assures us, two teats on the breast, but on each foot only three claws, with very long nails, like the tamandua, and its feet being very narrow and thus defective in toes, it is very slow in motion.

Now to conclude this scheme, among the animals whose hinder feet only are like hands, is to be reckoned the cariguеya or opossum, which having described at large in the anatomy of the female opossum, I shall not insist farther on it here; and if there be any other animals that have their hinder feet formed like hands, either with or without a thumb, intending to include all those animals that are observed to climb or live on trees into a class together: and they being observed to have their claws, either all or several of them, formed like fingers, I place them therefore under this general title of animalia χειρο-δάκτυλα.

Now, to return from this digression, we shall proceed in our observations on the male opossum, omitted in the account given of the female. In describing the ears, I had not an opportunity of observing that white rim that encircles them, which is very beautiful: for when the animal is in health there runs, for the breadth of two lines or more, an edging round the verge of the ear, of a perfect milk white colour. But the ear here being so very thin and tender, it is easily affected by cold or illness, and then this white part becomes jagged and crumpled, as if burnt up, and the whiteness disappears. It is on this account that Margrave, in his description of the tai-ibi of Brasil, which now I take to be the male opossum, says, it has aures subrotundas, molles, graciles, albas, teneras ut charta molles, not that the whole ear was white, but only the edges.

But what I was most desirous to know was, whether the male had that marsupium or pouch, for receiving the young, as is affirmed by some. Mr. Cowper, in the subject he dissected, neither observed the pouch nor the muscles belonging to it, as has been described in the female, nor indeed did I in that I dissected. Only this I observed, when first I had it, that the skin there seemed to be looser; so that with my finger I could easily thrust it in, and by turning it round, could form for the present a pouch; but this would easily turn out again, on withdrawing my fingers. Whether therefore it is capable of being formed into a pouch or marsupium on occasion, I shall leave as a query to be resolved by those that live where they breed, whether they ever observed the male to receive the young ones as the female does? However, in the male there were those bones I call marsupialia; and I observed muscles running from them to the hinder legs, which doubtless are very serviceable to them in drawing up their bodies, as I find Mr. Cowper has likewise remarked.

I shall further add, to confirm what Oppian and others write concerning fishes receiving their young ones into their bellies, that Mr. Herbert, in his Travels, lib. 1, p. 23, says, that in their voyage they took a shark 9½ feet long, and found in her paunch 55 young ones, each a geometrical foot in length; all which, he adds, go out and in at pleasure.

As to the brain of the opossum, I observed that being taken out of the cranium it weighed 2 drams 2 scruples. I did not find either in the cerebrum those anfractus, or in the cerebellum those circilli which we usually meet with in other brains. The whole was of an oblong figure, and seemed to be divided into three parts, viz. the cerebellum, the cerebrum, and that part of the cerebrum which was projected into the rostrum. For by the pinching in of the cranium here, the fore part of the cerebrum, from whence issued the processus mamillores and olfactory nerves, was by this constriction remarkably distinguished from the cerebrum; like an anterior brain. In the vermin kind, and those that have a long rostrum, I have observed the like. For nature here seems to give them more particularly the advantage of the sense of smelling, for finding out their prey, or avoiding the danger they would shun.

I observed the optic nerve and the eye to be large, the better to look out for either. And when I have mentioned the auditory nerves to be large likewise for the same reason, to give them a quick sense of hearing any sudden noise, and so to avoid the danger, these were the greatest remarks I made upon the nerves. It was observed that it saw best in the twilight, and not so well in the bright sun, which I was easily brought to believe, because it was then to seek out for its prey. In the eye I observed the membrana nictitans: the glandula lachrymalis was large and oblong; there was the musculus septimus suspensorius; and the crystalline humour was large, very transparent, and almost of a globular figure; the eye or iris black.

The Anatomy of those Parts of a Male Opossum that differ from the Female. By William Cowper, F.R.S. N° 290, p. 1576.

The singular contrivances of the sexual organs of the opossum render the anatomy of them very desirable, I may say entertaining, to those who have a taste for such inquiries. Comparative anatomy, instructive as it is, does not escape the censure of the vulgar; though the greatest illustration of the use of parts are not only to be had from thence, but the very existence of several organs in human bodies have been made known to us by discoveries first made in the bodies of quadrupeds. The circulation of the blood, and the passages for the chyle and lympha would have been as little known to us as our predecessors were it not for dissections made on the bodies of several animals.

This male opossum, as well as the female, dissected by Dr. Tyson, was brought from Virginia, and presented to the Royal Society; and was also kept alive in their repository; but falling from its meat, it languished and died: the cause of its death appeared to be from a mortification of the duodenum, immediately below the pylorus, which seemed to arise from a quantity of hay, that had been collected in the stomach, and matted together in the shape before described, and figured the hairy tophus the Dr. found in the stomach, but I could not find any hair in this; this wad of hay slipping out of the stomach, it stuck in the duodenum, which together with the viscid matter that involved it, completely obstructed the passage in that gut, as well as that of the gall into the gut, which appeared from the distention of the liver as well as the fullness of the gall bladder. The omentum, which in this animal is only fastened to the bottom of the stomach, had also suffered a gangrene, as had almost the whole intestinal canal.

Besides the organs employed in generation, the male opossum differs externally from the female, there being no marsupium or pouch to receive the young ones; nor are there any muscles inserted into the skin of the abdomen springing from the ossa marsupialia, as Dr. Tyson calls the bones, which may deserve the name of hyoides, from the figure they make with the ossa pubis of this animal; which bones seem not to differ in the male, from those of the female he has described and figured in the Transactions referred to.

There is no external appearance of genitals in the male opossum, but the scrotum; which is but just large enough to contain the testes; nor could I readily discover any other foramen outwardly in these parts besides the anus, A, (pl. 5, fig. 1) which leads to the rectum; but on withdrawing its sides, I found another foramen, B, which on dissection appeared to be the præputium or out-let of the penis. On compressing the parts on each side this cloaca, A, B, I observed two drops of yellowish-coloured liquor, resembling pus, start out on each side the anus, CC, which on further examination I found come from two glandulous bodies, or bags, placed on the sphincter muscle of this part. This sort of liquor it seems Dr. Tyson found in the pouch of the female, which, like this, had more of the peculiar fœtor of this animal, than any other part besides; for on removing these parts with the skin about the cloaca, I was freed from the ungrateful smell. On separating the skin from the muscles of the abdomen, the two bones above-mentioned (peculiar, I believe, to this animal) appeared, from whence some muscles sprang, and were inserted into the ossa femorum, which performed the office of the psoas muscles in other animals, which last named muscles were much smaller in this, than in other animals.

The abdominal muscles were also fastened to the last mentioned bones, particularly the recti, which enabled this animal to project or spring its body, especially in drawing its hind legs forward, with more advantage or force than other animals. Immediately under the skin about the cloaca I found a thin fleshy muscle, inclosing the præputium, and the lower parts of the rectum and odoriferous bags, together with the four mucous glands, MMNN, fig. 2, 3, at the root of the penis, and body of the penis itself A; all which parts were liable to be compressed by the action of this muscle, especially when the penis is erected, by which its erection is sustained by compressing the two external veins on the dorsum penis. On removing this thin broad sphincter muscle, I was obliged to clear away two lumps of hard fat, before the body of the penis could be discovered.

The scrotum being removed, each testicle appeared as represented on the left side QTV, fig. 2, the vasa præparantia and deferentia QQ being inclosed in the cremaster muscles PP. These muscles were proportionably very large in this animal, as I have always observed them in those that have no vesiculæ seminales, which is the case of this animal; and this provision of nature seems not only necessary to suspend the testes, but these inclosing cremaster muscles also compress the epididymides and vasa deferentia, and oblige them to dispatch their contents (the semen) into the urethra in the time of the coition, which otherwise would have a slow progress; but this contrivance appears more peculiarly requisite to this animal, because the defect of the vesiculæ seminales here seem to be supplied by the largeness of the epididymides of the testes ww, fig. 2, 3, which are the excretory ducts of the testes, and appear in this animal to have a larger bore than ordinary: for this reason, the tunicæ vaginales are very straight in this animal, as appears in the figure TVRR, fig. 2.

On discovering the originations of the spermatic arteries, I was surprised to meet with an appearance I never heard of, nor observed before; and in this I should not have had any satisfaction, if I had not first injected wax into the trunks of the great artery iii, fig. 2, and vena cava h below the diaphragm. It seems, the descending trunk of the great artery, below the emulgent arteries, in this animal, is placed directly under the trunk of the vena cava; nor does the iliac branches of the arteries here twine about those of the veins, as in human bodies and some quadrupeds, which is done perhaps to compress the channels of the veins, by means of the pulsation of these arteries, to drive up the blood in the veins towards the heart; but that contrivance seems no way necessary in this animal, because the contrary position of its body is more customary, in hanging by its tail with its head downwards: it is not unlikely, if the veins of this animal were examined below the heart, but we should meet with some contrivance to prevent the precipitate flux of the blood in that pendulous position, as I have observed in the trunk of the cava immediately above the liver in dogs.

The spermatic arteries, aa, (fig. 2) arise from the forepart of the descending trunk of the great artery, and pass through a very small perforation, made on purpose in the vena cava, and descend straight to the testes, as in human bodies, and are not contorted in their progress, as we find them in most, if not all quadrupeds. Perhaps this perforation of the cava was not only made for transmitting the spermatic arteries, but may also frame an anulus, that may check the velocity which the blood would otherwise have in those arteries, which rapid motion of the blood we find nature studiously avoids in the testes of all animals: for in men we see these spermatic arteries (contrary to all other trunks of arteries) are less at their originations from the great artery; and in quadrupeds (except in this) the spermatic arteries are contorted before they reach the testes. The spermatic veins, after leaving the testes of this animal (like those of human bodies) have several divisions and inosculations, which are all reduced to one trunk on each side, and empty themselves into the cava immediately above the perforation bb.

Had the known structure of the testes, in relation to their excretory ducts, been left undiscovered till now, the bare inspection of those parts in this animal would instruct us: for on dividing the tunica vaginalis, RR, fig. 2, 3, I found the inclosed testicle and its epididymis lying loose, insomuch that they parted from each other as expressed WXYZ, and with the assistance of a pretty large convex glass I could see the excretory duct Z, arising from one end of the testicle, where the spermatic artery and vein Y may be seen: after that duct has gone a little way, it may be seen folded up into the body called epididymis WW, and at length making the vas deferens SS. In men, and most, if not all quadrupeds, the epididymides and testes adhere so close to each other, that, without some dexterity in dissection, their rise from the testes is not to be discovered. The vasa deferentia, SS, fig. 1, after they leave the præparantia ab, as in men and other animals, become somewhat larger; but on crossing the ureters, ee, become less again, at their entrance into the urethra, immediately below the neck of the bladder; where their orifices could be perceived on each side a caruncle: nor are there any vesiculæ seminales near the vasa deferentia of this animal, as in boars, bulls, horses, &c. which nevertheless cannot be allowed to communicate with each other as in men; for though the vasa deferentia and vesiculæ seminales, of those last named animals, empty themselves into the urethra at the same orifices with the vesiculæ seminales, yet their communicant ducts are so very short, that whatever comes by the vasa deferentia will sooner escape into the urethra, than be received by the vesiculæ, as in men.

The length of the urethra, between the bladder and the penis, exceeded 4 inches, of which more than 3 inches and a half was inclosed with a glandulous body, analogous to the prostatæ in men and other animals; the orifices of the secretory ducts of this glandulous body are very numerous, and open into the urethra on all sides, as appeared on opening the urethra; and compressing this glandulous body, or the prostatæ, its secreted liquor started out.

This part of the urethra IKKL, fig. 2, 3, thus inclosed with the prostatæ, being very much contorted or folded, in its natural situation between the bladder and the penis, when there is no erection, must necessarily be drawn out, and become straight, when the penis is extruded on an erection; by which means this glandulous body is necessarily compressed, and the succus prostatarum forced into the urethra. The prostatæ of divers animals are compressed by muscles framed on purpose, that inclose them, as in boars, rams, &c. but in men they are compressed by the musculi levatores ani.

At the root of the penis of the opossum we meet with 4 glandulous vesiculæ MMNN, two on each side, which empty themselves into the urethra, and contain a mucous matter, like that I find in the glands I lately discovered in this part in men. These vesiculæ are not only compressed by the thin broad sphincter muscle above mentioned, but the bulbs of the cavernous bodies of the penis CC, and urethra EE, when distended in the erection of the penis, also compress these mucous bags. This compression in men is effected by the intumescence of the bulb of the cavernous body of the urethra, Phil. Trans. N° 258. In boars, rams, cats, &c. nature is so solicitous to discharge the contents of the excretory ducts of these glands, that, like the gizzard of birds, each mucous gland is inclosed with a proper muscle to compress it.

The penis fell next under my examination, the fabric of which appears not less surprising, than what is met with in the uterus of the female; and in many circumstances differed from what I have found in all the animals that I have hitherto dissected: besides the forked glans of its penis, BB, its cavernous bodies DD, fig. 2, 3 had no connection with the ossa pubis, nor did the muscles called erectores, or directores, CC, adhere to any bone, as in men and quadrupeds but all those parts lay loose under the ossa pubis. The other extremities of the two corpora cavernosa penis are received into the glans. Nor did the corpus cavernosum urethræ e, or its muscles EE, fig. 3, adhere to the sphincter ani, as in most other animals, but the whole body of the penis lay loose between the bones of the pubis and the rectum; so that on the intumescence or erection of the penis, it is at liberty to be extruded from its præputium, wherein it is secured from outward injuries when not erected. To favour this extrusion of the penis in this animal, the urethra IKL, fig. 2, is not only very long between it and the bladder OO, but is much more contorted or folded in acuter angles, than is expressed in the figures, otherwise the penis could not be extruded, but the bladder OO must follow it. Besides, it appears that nature designed this extrusion of the penis of this animal in its erection, because we meet with instruments to draw it back again into the præputium. ffG show a pair of muscles, elegantly framed for that purpose, on the forepart of the penis; they arise fleshy from the corpora cavernosa penis DD, and becoming tendinous ff, as they pass through two ligaments or pulleys on the ossa pubis, and are afterwards united into one tendon G, which is inserted at the upper part or dorsum penis. Besides this pair of muscles, which is peculiar perhaps to this animal, I found another pair of muscles HH, fig. 2, 3, that also withdraw the penis, arising from the rectum, and are inserted into the extremities of the corpora cavernosa penis: in cats, male porpoises, bulls, rams, and boars, we meet with two ligaments springing from the os sacrum or ilium on each side, and inserted into the corpora cavernosa penis of those animals, which like these muscles serve to draw in the penis of those animals into the præputium.

The corpora cavernosa penis of the opossum differ in their figure, from what we find in other animals; their upper parts DD are bulbous, and covered with muscles CC, like the bulb of the cavernous body of the urethra in men: in other animals, those parts of the corpora cavernosa penis are of a conical figure. The muscles of the cavernous bodies of the penis in this animal having no connection with the os pubis, cannot apply the dorsum penis to the last named bone, and compress the vein of the penis, so as to retard the refluent blood, and cause an erection, as we have observed in other animals; but some large veins of the penis here take a different course, and pass through the middle parts of the bulb KKC, fig. 4, and are only liable to the compression made by the intumescence of these muscles CC, fig. 2, that inclose them.

But the chief agent in continuing the erection of the penis in this animal, is the sphincter muscle of its anus, or rather cloaca, to which the broad sphincter muscle above-mentioned is continued, and does somewhat contribute. When the penis is extruded from the cloaca, which must happen when it is erected, the sphincter of that part necessarily embraces it; the like must be done by the sphincter muscle of the cloaca of the female in coition: on these accounts I am apt to think that these animals are not very quick in that act. Besides, the shape of the penis, (fig. 4) shows an unfitness for its retraction, till there is a detumescence of its glans AB, which perhaps does not happen in these animals till both male and female are satiated, as in dogs and other animals that have bones in their penis, and have a bulbous intumescence of the glans in coition, and no vesiculæ seminales as in this animal, and also impregnate the female with more than 2 or 3 at a time, as the opossum does.

As the bulb of the cavernous body of the urethra in man is framed for the use of the glans, to keep it sufficiently distended when required; so it seems it is necessary to have two of those bulbs inclosed with their particular muscles EE, fig. 2, in this animal, to maintain the turgescence of its doubled or forked glans AB, fig. 4, when the penis is erected: in this distention of this glans penis of this animal, the middle part of the orifice of the urethra, where the probe is seen passing out of fig. 3, is necessarily compressed, as represented by D, fig. 4, and two distinct apertures CC are left open, as appears by AB on each side its forked glans.

Those who suppose an aura seminalis of the male passes by the way of the blood of the female to their ovaria, to fecundate the ova, will here meet with an instance I must leave them to solve. For to what end has nature been at the trouble of making double emissaries for the semen of the male opossum, as though she designed the impregnation of a double uterus of the female? Certainly one passage in the glans penis would have been sufficient to convey the semen masculinum to the mass of blood of the female in the manner they conceive. Nature would never have been so careful in this animal, in making a double glans, and contriving two distinct apertures in the glans, when its penis is erected, if the propagation of the species had not depended on it: doubtless it was for that end chiefly, that the penis of this animal differs so much from what we meet with in others. Nor could its penis in these circumstances be exposed in a præpuce, as in other quadrupeds, by reason of the numerous accidents that would certainly occur in this animal's way of living: nor could its penis have been thus retracted when not erected and sufficiently extruded, when it is, if (as in other animals that are also retromingent) the penis here had been fastened to the ossa pubis.

Thus we see nature in these instances, as frequently occurs in many others, accomplishes the same ends by different methods. Although there are no vesiculæ seminales in this animal as in dogs, weasels, &c. yet we find its penis without a bone in it, as in those; but then we meet here with additional contrivances to maintain its erection: not only the sphincter muscle of the cloaca, fig. 3, of the male opossum, but that of the female also closely embraces its penis in coition, and effectually retard the refluent blood from its corpora cavernosa, by compressing the veins of the penis E, fig. 4. Nor could the penis of this animal be framed like that in boars, rams, bulls, &c. in whom the corpora cavernosa are too large, when not erected, to be secured within the cloaca of this animal.

The Explanation of the Figures.—Fig. 1, pl. 5, shows the external appearance of the genitals of the male opossum. ABCC the anus or cloaca; A its lower part, which leads to the rectum; B its upper part, or the orifice of the præputium, whence the urine and the penis is extruded; CC two small apertures, whence the yellowish coloured liquor, that had the peculiar fœtor of the animal, had its exit; D the scrotum, just large enough to contain the testes; E that part of the abdomen, where the marsupium is seen in the female, which here appears a little more depressed than in other animals, but cannot retain the young ones, as the pouch of the female does; FF the two thumbs of the hind feet or hands.

Fig. 2 represents the foreparts of the organs of generation dissected from the male opossum; AA the body of the penis; AB the forked glans; CC the muscles analogous to the directores penis in men and other animals, which here inclose the bulbi of the cavernous bodies of the penis; DD the two corpora cavernosa penis, before they join and make the body of the penis; EE parts of the two bulbs of the cavernous body of the urethra; Gff a pair of muscles, whose two tendons ff pass through two ligaments or pulleys on the ossa pubis, and are afterwards united into one tendon G, inserted into the dorsum penis, and serve to draw the penis within the cloaca after an erection; HH two other muscles which serve for the same use, and arise from the rectum, but are fixed to the opposite part of the corpora cavernosa penis; I the urethra where it has no glandulous body inclosing it; KK the prostatæ or corpus glandosum, inclosing the urethra, which lies contorted between the penis and the bladder of urine, in the pelvis of the abdomen of this animal; MN two mucous bags on each side, at the root of the penis, which empty themselves into the urethra; OO the bladder of urine; PP the musculi cremasteres; QQ the left cremaster muscle, inclosing the tunica vaginalis; RR the tunica vaginalis of the right side, opened to show the inclosed vasa præparantia and vas deferens; SS the vas deferens; TV the tunica vaginalis inclosing the left testicle, with its epididymis V; WXYZ the right testicle, as it appeared on opening the tunica vaginalis; W its epididymis; X the body of the testicle; Y the spermatic vein and artery, as they pass to and from the testicle; Z the excretory duct of the testicle, which could be distinctly seen arising from the testes, and proceeding to the epididymis W, where it is folded up and constitutes that body, whence it is continued to the bladder of urine, and called vas deferens SS; aa the spermatic arteries, arising from the forepart of the descending trunk of the arteria magna, where they have a common duct, which is divided as it passes through an aperture made on purpose in the trunk of the vena cava; bb the spermatic veins at their entrance into the cava; dd the kidneys; ee the ureters; gg the emulgent veins; Ψ part of the left emulgent artery; h the vena cava below the liver; ii the descending trunk of the great artery; kk the mesenteric arteries; l the lower mesenteric artery, which in this animal does not arise from the great trunk; m the left glandula renalis, that of the right side being placed behind the trunk of the vena cava n; o a common trunk of an artery, from whence springs the gastric, the superior and inferior mesenteric, and the emulgent arteries of this animal. The design of nature in confining all those arteries to one trunk in this animal, might be perhaps in favour of its usual posture in hanging by its tail, with its head downwards. This trunk of the arteries of the viscera of the lower belly, having so many united forces, is the less liable to any compression, that might be made by the contained parts of the lower belly in that posture.

Fig. 3 represents the backside of the genitals of the male opossum; A the body of the penis; B its glans; CC the bulbi of the corpora cavernosa penis, covered with their muscles; DD the corpora cavernosa penis; EE the two distinct bulbs of the cavernous body of the urethra, inclosed with their particular muscles; FFG parts of the muscles expressed on the forepart of the penis in the preceding figure; HH the other pair of muscles springing from the rectum, and inserted into the sides of the corpora cavernosa penis; IKL the urethra covered with the prostatæ KLK; MN the two mucous bags on each side; O the bladder of urine; P the musculus cremaster; Q the tunica vaginalis opened; R the vasa præparantia cut from the great trunks; SS the vas deferens on each side; WXYZ the left testicle, as in the preceding figure, with the opposite side turned here; ee parts of the ureters; ** a probe inserted into part of the urethra.

Fig. 4 shows the forepart of the penis, as it appears when its corpora cavernosa are filled with mercury and dried; AB its forked glans; CC the two distinct apertures that appear in this distension or erection of its corpora cavernosa; D the middle part of the orifice of the urethra, which is occluded on the intumescence or erection of the penis; E the two veins of the glans, which are compressed by the two sphincter muscles of the male and female in coition; F the bulbs of one of the cavernous bodies of the penis distended; G one of the bulbs of the cavernous body of the urethra also distended: these bulbi were opened on the other side, Ψ, to fill the cavernous bodies with quicksilver, but are all expressed as they ought to appear on both sides in the following figure: H the urethra; I the muscles dried, expressed by FFffG in fig. 2 and 3; KK the veins tied up to keep in the mercury, as they pass the muscles of the bulbi.

Fig. 5 represents the backpart of the penis expressed in the preceding figure. AB its forked glans; EE parts of the veins arising from the glans; FF the bulbs of the cavernous bodies of the penis; GG the two bulbs of the cavernous body of the urethra; H the urethra; KKkk the veins tied up, as they pass out of the bulbi to keep in the mercury.

Tractatulus de Ambaro, a Reverendo D. D. G. J. Camello, communicatus D. Jacobo Petiverio Societatis Regiæ Socio. N° 290, p. 1591.

Some observations on ambarum, i. e. ambergris; concerning the nature and origin of which see note at p. 94, vol. ii. of this Abridgment, where references are given to more ample and accurate accounts of this substance.

Concerning the Jesuits' Bark. By Dr. Wm. Oliver, F. R. S. N° 290, p. 1596.

Peru bark comes from a tree about the size of a plum-tree, with leaves like ivy, but not quite so large, which are always green. The Indians call it querango. It is gathered in autumn, and the rind taken off all round, as well from the trunk as boughs, which grows again in 4 months, as cork does. The trunk is about the size of a man's thigh. It bears a fruit not unlike a chesnut, excepting its outer rind or shell, which is properly called china china, and is esteemed by the natives beyond the bark taken from the trunk or boughs.*

Account of a new Baroscope. By Mr. Caswell, of Oxford, F. R. S. N° 290, p. 1597.

Suppose ABCD, pl. 3, fig. 20, to be a bucket of water, in which is the baroscope *xrezyosm*, consisting of a body *xrsm*, and a tube *ezyo*, both which are concave cylinders communicating with each other, and made of tin, or rather of glass; the bottom of the tube *zy* has a lead-weight to sink it, so that the top of the body may just swim even with the surface of the water, by the addition of some grain-weights on the top. When the instrument is forced with its mouth downwards, the water gets up into the tube to the height *yu*. On the top there is added a small concave cylinder, which I call the pipe, to distinguish it from the bottom small cylinder, which I call the tube; this pipe is to sustain the instrument from sinking to the bottom; *md* is a wire; *ms*, *de* are two threads oblique to the surface of the water, which threads perform the office of diagonals; for while the instrument sinks more or less, by the alteration of the gravity of the air, there, where the surface of the water cuts the thread, is formed a small bubble, which ascends up the thread, while the mercury of the common baroscope ascends.

The circumference of the body is 21 inches, therefore its area $= 35$: the altitude $ms = 4$; therefore the solid content $= 140$; each base *xm*, *rs*, has a convexity, whose altitude is .65, therefore the conoid on each base is nearly $= 11\frac{1}{2}$; hence d the whole body is $= 140 + 11\frac{1}{2} + 11\frac{1}{2} = 163$, and b the entire altitude of the body $= 4 + .65 + .65 = 5.3$. The inner circumference of the tube is 5.014, therefore its area $n = 2$; the length of the tube $= 4.5$, therefore the tube's capacity $= 9$; hence c, the content of the body and tube $= 163 + 9 = 172$ cubic inches, that is almost $2\frac{1}{2}$ quarts.

Suppose the air's pressure when greatest $= 30.5$ inches of mercury $= 30.5 \times 14 = 427$ of water, and $f = 427$; therefore $fc = 73444$. Put a for the depth *ou*, of the air in the tube when the body is just all immersed; the air in the instrument on immersion contracts somewhat by the cold of the water; this contraction I find is nearly as much as would be produced by an addition of 1 inch to the atmosphere's altitude 427; this in cold weather; but in warm weather, it is probably twice as much; but we will now suppose it $= 1$; therefore

* A more ample account of the Jesuits' or Peruvian bark occurs in the 40th vol. of these Transactions. It is the cinchona officinalis, Linn. See Lambert's Description, with plates, of the genus cinchona, published in 1797. Also Ruiz Flora Peruviana.

the depth of the surface of the water in the tube, below the surface of the outer water, is $= b + a$; therefore the pressure on that inner surface is as the altitude of the atmosphere above it $= f + b + 1 + a = F + a$, putting $F = f + b + 1$. Then, since the spaces into which the air is contracted, are reciprocal to their respective pressures; and that while the instrument is out of the water, the pressure f answers to the space c, therefore $F + a : f :: c : \frac{fc}{F + a} =$ space which the air takes up in the instrument under water; therefore, $\frac{fc}{F + a} - d =$ that part of the tube which is possessed by air $= an$, supposing the tube's area $2 = n$: therefore $fc - Fd - ad = Fan + aan$; and hence $aa + (F + \frac{d}{n}) \times a = \frac{fc - Fd}{n}$. Put $F + \frac{d}{n} = 2g$, then $aa + 2ga = \frac{fc - Fd}{n}$; therefore $a = \surd : \frac{fc - Fd}{n} + g^2 - g$.

Then suppose the atmosphere's gravity so much less as to sink the mercury $\frac{1}{10}$ inch $= 1.4$ of water; therefore putting $\varphi = F - 1.4$, and in the last equation α instead of a, and γ instead of g, we have $\alpha = \surd : \frac{fc - \phi d}{n} + \gamma\gamma : - \gamma$. Thus I find $a = 2.72$, and $\alpha = 2.94$, therefore $\alpha - a = .22$; which $.22 \times n$ gives .44 cubic inches, and (supposing a cubic inch $= 253$ grains) $.44 \times 253 = 111$ grains-weight of water, that was raised up into the tube in the first case more than in the second, and therefore the baroscope requires an addition of 111 grains on its top to sink it to the level of the water in the second case more than in the first, and this upon the sinking of the mercury in the common baroscope only $\frac{1}{10}$ of an inch; now 1 grain in this new baroscope, is nearly as discernible as $\frac{1}{10}$ inch in the common, and therefore this new baroscope is 111 times more exact than the common one.

Put $f = 247$, $c = 172$, $d = 163$, $n = 2$ as above, only change F, putting it $= 437.3$, that is, suppose the body sunk in water 4 inches lower; in this case $\alpha = 208$, therefore $a - \alpha = .64$, which multiplied into $\varphi n = 1.28$ cubic inches, which $\times$ 253, gives 324 grains; and so much (the body's top *xm* being sunk 4 inches under water) the body becomes heavier than while *xm* was at the surface of the water. Therefore this 1.28 divided by the aforesaid depth 4, gives .32, the area of the top pipe, such as would balance or buoy up the body at any depth. Strictly speaking, the pipe should be gradually wider upward, in order to sustain the instrument at any depth, but as to sense it is cylindrical, and its circumference $= 2.005$. But as the least alteration of the air would make the body's top *xm* in that case pass through the 4 inches, which 4 inches I suppose all the variety of depth that the instrument has room given it in the bucket to ascend or descend, therefore the pipe is made a small matter larger, viz. its circumference is 2.14, by which the pipe, according as the body sinks more, gives more resistance to the descending body. The pipe's area is .3643; therefore the capacity of the pipe in 4 inches altitude is = 1.457. But as abovesaid to give justly no resistance, its capacity should be 1.28. Therefore this 1.28 taken from 1.457, leaves .177 the actual resistance in 4 inches depth, viz. .177 × 253 = 44 grains.

But this resistance will not be the same in all weathers. In order therefore to calculate what it will be, when the mercury of the common baroscope is very low; for example, only 28 inches high = 392 of water, f must be supposed = 392, therefore $F = f + b + 1 = 398.3$, and the rest as before; viz. $d = 163$, $fc = 67424$, $Fd = 649229$. Thence by the aforesaid equation $a = 2.59$, $\alpha = 2.84$; therefore $\alpha - a = .25$, which $\times\, n$, gives .50 cubic inches, which × 253 = 126 grains. So that this baroscope, when the mercury is lowest, is 126 times more exact than the common one, supposing the body immersed afresh when the mercury is so low.

Next, while the mercury is so very low, suppose the top of the body depressed 4 inches under water: therefore $\varphi = F + 4 = 402.3$; the rest are as before, viz. $fc = 67424$; then α will be 1.9; but before, while the top of the body was at the surface, α was 2.59; therefore the difference .69 × tube's area 2, gives 1.38 cubic inches, which × 253 gives 349 grains; and so much the baroscope is heavier when the top xm is 4 inches under water; or, which comes to the same, supposing that mercury at 28, and xm at the surface, this baroscope by the mercury's ascending $\frac{4}{14}$ inch, will become 349 grains heavier. The pipe's capacity in 4 inches altitude, was 1.457, from which take the aforesaid 1.38, the residue = .077, which × 253, gives 19 grains in 4 inches; so that the pipe will sustain the baroscope, and also 44, when the mercury is $30\frac{1}{2}$ high, and only 19 grains when the mercury is 28 high. The fewer grains difference there are in its sinking through 4 inches, the more nice the baroscope will be.

Where the thread cuts the surface of the water, there is formed a bubble; therefore, while the instrument sinks in water 4 inches, which is all the room that I give it, the bubble moves on the two diagonal threads 20 inches; it follows therefore, that 120 grains difference would make the bubble pass over 120 inches, if the threads were so long; but, as it has been above calculated, about 120 grains difference of weight of the instrument, is produced by so much of the alteration of the air, as would make the mercury of the common baroscope $\frac{1}{10}$ inch; therefore when the mercury ascends $\frac{1}{10}$ inch, the bubble of this new baroscope ascends 420 inches: therefore this new baroscope is about 1200 times more exact than the common one.

The Observations made with this new Baroscope are as follow.—1. While the mercury of the common baroscope is often known to be stationary 24 hours together, the bubble of the new baroscope is rarely found to stand still one minute. 2. Suppose the air's gravity increasing, and accordingly the bubble ascending; during the time that it ascends 20 inches, it will have many short descents of the quantity of $\frac{1}{8}$ inch, 1, 2, 3, or more inches, each of which being over it will ascend again: these retrocessions are frequent, and of all varieties in quantity and duration, so that there is no judging of the general course of the bubble by bare inspection, though you see it moving, but by waiting a little time. 3. A small blast of wind will make the bubble descend; a blast that cannot be heard in a chamber of the town, will sensibly force the bubble downward. The blasts of wind sensible abroad cause many of the abovesaid retrocessions, or accelerations, in the general course; as I found by carrying my baroscope to a place where the wind was perceptible. 4. Clouds make the bubble descend. A small cloud approaching to the zenith effects more than a great cloud near the horizon. In cloudy weather, the bubble descending, a break of the clouds, or clear place, approaching to the zenith, has made the bubble to ascend; and after that break had passed beyond the zenith a considerable space, the bubble again descended. 5. All clouds, except one, hitherto observed by me, have made the bubble to descend. But the other day the wind being north, and the course of the bubble descending, I saw to the windward a large thick cloud near the horizon, and the bubble still descended; but as this cloud drew near the zenith, it turned the way of the bubble, making it to ascend, and the bubble continued ascending till the cloud was all passed, after which it resumed its former descent. It was a cloud that yielded a cold shower of small hail.

Concerning some Fossils. By Mr. Edward Lhwyd, Keeper of the Ashmolean Repository in Oxford. N° 291, p. 1566.

The state of fossils is quite different in Essex, from what it is in Wales and Ireland. In these latter, the shells are generally crystalline; but in Essex, and sometimes about Oxford, they are testaceous; which difference is doubtless to be attributed to the soil, and particularly to the chalk and flint of Essex, which those other countries want, excepting a small part got in the north part of Ireland. But there it is remarkable that their chalk is absolutely petrified; I mean, whereas the flints are in England embodied in chalk, they are there in a chalk-white lime stone. And as chalky countries alone afford those echinitæ, which I have stiled pileatus, galeatus, and cordatus; so I could never find them in all my travels except at that place; from whence in the time of paganism the druids procured them, and sold them among our northern Britons for stones of miraculous efficacy, against perils by fire and water; persuading the vulgar that they were generated in cocks'-knees; as thousands in the Highlands believe at this day.

I was surprised that so many fossils, found in Essex, were scarcely distinguishable from sea shells: for the case is otherwise in those places I searched. We have indeed about Oxford, one or two fossil shells of a testaceous substance; but in colour they differ farther from those of the sea, than the Essex fossils do, where they are sometimes imbedded in solid stone: which takes off any objection some might offer, of their being an accidental scattering of gulls, crows, &c. on the Harwich cliffs.

Concerning Harwich Cliffs, and the Fossil Shells found there. By Mr. Sam. Dale. N° 291, p. 1568.

Harwich Cliff is a sort of promontory, which divides Orwel Haven from the æstuarium contained between that and Walton Nase: it is situated near a quarter of a mile distant to the south of the town, and contains many acres of land: its greatest height, from the strand or beach to the top, is 40 or 50 feet. At the bottom of this cliff is a stratum of clay, which is succeeded by another of stone, each about a foot thick; in this stratum of stone are imbedded divers shells (though but thinly) as well of the turbinate as bivalve kind, and also pieces of wood and sticks. Over this are divers strata of bluish clay, about the height of 20 feet: this clay has pyrites or copperas stones sticking in it, but I could observe no shells. Above this are likewise divers strata, which reach to within about 2 feet of the surface; some of which are only of fine sand, others small stones and gravel, mixed with fragments of shells, and in others small pebbles are mixed; and it is in some of these last mentioned strata, that the fossil bivalve and turbinate shells are imbedded, which lie promiscuously together: the strata with the shells observe no order in their lying, being sometimes higher and sometimes lower in the cliff; and sometimes 2 or 3 one above another, with other strata of sand, fragments and gravel between. Above all these is a covering of common sandy earth, which is about 2 feet thick; in some places of which are veins of a species of osteocolla, though more tender than osteocolla officinarum, which is brought from Germany: this I have adventured to call osteocolla Anglicana; which incrusts about small strings like the fibres of the roots of trees; it is of various sizes, and sends out branches here and there; but it is so tender, as not to be got out of the earth in any large pieces. Whether it appears above the earth like the German, I never could discover.

The shore before this cliff, as far as the ebbing of the sea permits observation, is rudely paved with stones, several of which are veined with that sort of substance, which by Helmont and other later naturalists, is called ludus Paracelsi: of these stones, the inhabitants have a tradition, that they are formed by the clay, which tumbling down from the cliff, and being washed by the flowing of the sea, are in a short time converted into stone; and the ingenious Mr. Silas Tayler, in his MS. collections of Harwich and Dovercourt, writes thus concerning it: "The washing of these cliffs discovers a bluish clay, which tumbling down upon the shore, although washed by the sea at high-water, within a short time turns into stone: some may be seen, which are new fallen, as soft as the clay in the cliff; and others, that have lain there longer, crusted over and hard; but if opened or broke, the clay is still soft in the middle; others, that have lain longest, petrified to the very heart; and with these the walls of the town are for the most part built, and the streets generally are pitched." How far this is matter of fact, I will not determine; and though I must at the same time own that many of the stones are washed out from the stratum at the bottom of the cliff, yet I have sometimes been inclined to Mr. Tayler's opinion, because he lived long upon the spot, being store-keeper of the king's building yard for many years, and by his collections, &c. seems to be a person of probity and learning; and also because several of the stones have cracks or chops in them, as clay and earth have by being exposed to the sun; and there is yet [anno 1702] lying upon that shore a stone, in which a large pile (perhaps of oak) such as was formerly used there to preserve the cliff from the injuries of the sea, evidently appears to be imbedded; which can owe its situation to no other cause, than by being pressed into the superficies of the clay while soft, and petrifying with it; which being square, takes off an objection which some might make, had it been round, of its being lodged there in the general deluge.

I am aware that this manner of petrification is not only different from the common methods nature uses in that operation, but also thwarts the opinions of several learned and ingenuous men; and it was strenuously opposed by Mr. John Morton of Oxendon in Northamptonshire, when we, with Mr. John Luffkin, were upon the spot; the substance of his discourse imports that he thought, from attentive observations, that those stones are not petrifactions from the stratum of clay.

As to petrifactions, he adds, "I have only observed these three sorts: 1. A stony incrustation upon sticks, and any thing that lies in the way, in the petrifying springs; the soil in those waters is usually intermixed with particles of stone, that trickle down into it with the water; and are there detained. Of this first sort you have doubtless many instances in Essex, and I think there is one at Harwich cliff; though this is not so properly called a petrifaction. 2. The second sort is that which is performed by the permeation or insinuation of the finer sorts of stony particles; as is the case of some of our petrifying waters, particularly that at Knaresborough; the stony particles however of the Knaresborough spring are very fine. And many of the fossil-shells have undergone the same fate. 3. The third, which indeed is a petrification, properly so called, is often met with on the sides of caves and grottos, as at Pooly-hole in the Peak, and in the fissures and clefts of mines and quarries. Of this kind are the several sorts of fluors, the lap. stillatitii stalagmitæ, &c. that we meet with in the fissures and hiatuses of the earth. These are continually receiving an additional increase of real and solid stone, as is observed in several caves in the peak, &c. This I take to be performed in such a manner as the incrustations are, viz. the particles of stone are brought along with the water as their vehicle, and at length are deposited on the sides of the cave or fissure: but here the particles of stone are extremely minute and fine, and do thereby naturally concrete and join very close together; whereas, in our incrustations, the particles of stone being grosser, the stone is rough, coarse, and friable. And this I leave to your judgment, if it be not a more reasonable Hypothesis than that of Dr. Plot, in p. 33, of his History of Oxfordshire, viz. That the very body of the water is turned into stone as it drops down from the rocks. As to that hypothesis of the transmutation of a stratum, e. g. of chalk to clay, of coal to common stone, or the like, I must confess I never met with any thing in nature which would countenance it, that is, such a transmutation in the bowels of the earth. Nor is there any thing that proves it, that ever I have met with in any natural observations."

A late author is of opinion, that this bed of stones was the foundation of the loamy cliff, where the cliff has been washed away or cut; and that they are the production of a vitrioline juice, in conjunction with the loam; as the common copperas stones are by the same juice in a gravel; and that the latter were only to be found where the cliff was gravelly, and not where the loam is. How far these stones are the effect of a vitrioline juice, I will not determine; but this I can affirm, that I have now by me some of the pyrites, or common copperas stones, which I picked out of the clayey stratum of this cliff, in which they may be frequently met with. Nor do I remember at any time to have observed these stones to be invested with either gypsum or trochitis, as the same author affirms, but often with the aforesaid ludis Paracelsi, and some other sorts of lapides stalagmitæ.

How those shells or marine bodies came to be deposited here, is a subject which has employed the heads and pens of several learned and ingenious men. I shall therefore only make some remarks on the positive assertion of the aforesaid author, concerning the imbedding of these fossil shells in this cliff, and the alteration of the channel, viz. "That this bed of shells, which covers the cliff, was carried thither at the making of the harbour or clearing of it. For the harbour or channel there is artificial, and of no old date, the current having been formerly on the other side of Languard Fort, which then stood in Essex." Against the first part of which, although many reasons might be given to prove the contrary, I shall only observe, that as our author begs the question, How else could the shells lie at the top of this cliff? I shall also ask him, why the same strata of sand, and fragments of shells, with the same fossils imbedded, are to be found at Walton Ness on the other side of the æstuarium, which is 5 or 6 miles broad from Harwich, as also at Bawdfey cliff in Suffolk, which is 8 or 9 miles distant, and in other cliffs on that shore, where I have met with them. A second question may here be asked; how it comes to pass, that none of those buccina heterostropha, (whose exuviæ are in such plenty in all the cliffs hereabouts) are not now to be found in this channel, nor the adjacent seas? For I cannot think the clearing this harbour could have destroyed all that species of shell-fish, of which there was then such plenty; and therefore some other origin must be allowed them, than what this author has assigned. Nor can I allow the harbour here to be artificial, because so great a work as this is, viz. the making a channel 2 miles wide, as it is in this place, would not have been without some record in history; and besides, the earth, &c. which must arise from this work, must consequently have made a much greater hill than the cliff ever was. And another doubt will from hence arise; why the workmen should bring all the earth, &c. to this side the channel, and not lay some on the other, as it is plain they did not. The ground on which Languard Fort stands, as far as Walton Coleness, which is about 3 miles, is only a sandy level or beach, which I believe has in time subsided there, as may be observed at the mouths of other large rivers. And as to the argument our author alleges, of Languard Fort being accounted to stand in Essex, to confirm his hypothesis of the change of this channel, it will be of no force with any one who observes, that not only parts of parishes, but likewise of counties, are often divided from those parishes and counties to which they belong, and included in others; of which many instances could be given; e. g. a part of Kent is on the Essex side the Thames; and in Oxfordshire, the parishes of Shilton belongs to Berkshire, Daylesford to Warwickshire, Compton to Gloucestershire, and Stratton-Audley to Buckinghamshire, though all are included in the other: and there is a farm belonging to the parish of Braintree, which is separated from it at least 2 miles. And to me a probable reason of this Fort being accounted in Essex, is the sands here subsiding, made at first an island, which being nearest to Essex was accounted of that country; or 2dly, the island so made belonging to none but the crown, it was at the pleasure of the king's officers to call it of which county they pleased. Nor was it Mr. Tayler's ignorance (as this author says) that made him mention these stones as petrifactions made by the sea. For, in his aforesaid collections, he did not omit the tradition the inhabitants of this town have, about the alteration of the mouth of this Haven, as appears from his own words: "It is generally believed, that Stoure did formerly, in a straighter current, discharge itself into the sea about Hoasley-Bay, under the Highland of Walton-Colness and Felix-Stow, in the county of Suffolk, between which and Languard Fort are (it is said) certain remains of the old channel, which the neighbouring inhabitants still call fleets, retaining at this day the tradition of the course of the water, and the entrance into this Haven to have heretofore been by and through them."

And I am of opinion that this tradition is matter of fact, having before hinted what mutations the mouths of great rivers daily undergo by the lodgment of sands, &c. which may be assigned as a better reason for this alteration, than that of our author, i. e. that it was artificial; and the yearly washing of the cliff on the Harwich side likewise adds to its probability; it being a constant observation, that where the sea gains on one side, it loses on the other. And that this level was so made, I am confirmed by the modern removal of the Fort more toward the point, and that more sand was added after the old Fort was built: this alteration is noticed by Mr. Tayler in these words, "And although several now living pretend to remember the building of Languard Fort, yet we find there was an older Fort thereabouts, Anno 1553, and called by the same name, which was not far distant from this modern one, a little north of it, where are still to be seen two faces and flanks of a bastion, the rest of it being worn away by the sea, but in its stead it has left upon the shore a long row of sand banks."

The spring mentioned by Mr. Edmund Gibson, in his English edition of Cambden, from the aforesaid manuscript of Mr. Silas Tayler, is a very small inconsiderable thing; nor could I observe that it did petrify or incrustate either pieces of wood or sticks; but I have a piece, which I broke off from a large pile on that shore, which was petrified so far as it was driven into the earth, and the sea water came; and I suspect there yet remains some others of the same.

I have already noticed, that the fossil shells are imbedded in a loose stratum of sand, gravel, &c. which may serve to demonstrate, that their matrix is not a clay-bed on the top of the cliff; as also, that they could not be scattered there by crows, gulls, and other sea-fowl, as well as that some of them are likewise bedded in stone at the bottom of the cliff; and although some few of them may be met with on the top of the cliff, yet it is only where the earth has been broken by the digging of ditches, &c.

The fossils I have found at this cliff, are the following. 1. Buccinum fossile heterostrophum rostratum lævem maximum Listeri referens. 2. Buccinum fossile rostratum maximum Listeri referens. 3. Buccinum fossile minus ventricosum mucrone obtuso. 4. Buccinum fossile tenue minus ponderosum, striatum et undatum. 5. Buccinum fossile tenue confragosum. 6. Buccinum fossile striis prominulis marginalibus insignitum. 7. Buccino-turben fossile reticulatum minus. 8. Buccino-turben fossile sulcatum. 9. Buccino-turben fossile rostratum. 10. Buccino-turben maximum rostratum fossile spiris intùs striis elatis insignitis. 11. Cochlea fossilis maxima umbilicata quinque spirarum. 12. Cochlea fossilis umbilicata mucrone obtuso. 13. Nerita parva fossilis. 14. Turbo fossilis spiris duabus striis eminentibus insignitis. 15. Pecten minor fossilis unica aurita. 16. Auricularia maxima. 17. Pectunculus fossilis fere circinatus striis tenuibus, valvis per ginglymon connexis. 18. Pectunculus fossilis crassus rostro acuto striis majoribus. 19. Pectunculus fossilis fasciis transversis undatis notatis. 20. Pectunculus vulgaris fossilis. 21. Pectunculus fossilis striis majoribus et elatioribus. 22. Pectunculus maximus fossilis Listerianum maximum referens. 23. Pectunculites maximus striis latis. 24. Concha parva fossilis fasciis tranversis insignis. 25. Concha longa fossilis fasciata. 26. Conchites lævis maxima. 27. Conchites parva fasciata. 28. Trigonella minor sive vulgatior Anglica Lithoph. Brit. 816.

An Instrument, for seeing the Sun, Moon, or Stars, pass the Meridian of any Place: useful for setting Watches in all Parts of the World with the greatest Exactness; to correct Sun-Dials; to assist in the Discovery of the Longitudes of Places, &c. By the Rev. Mr. William Derham, F. R. S. N° 291, p. 1578.

Of all the methods for finding the meridian of any place, the most commodious for common use is an instrument of Sir Christopher Wren's, or two of Mr. Gray's, or one published in the Appendix of a little book, called The Artificial Clockmaker.

Sir Chr. Wren's contrivance I am informed is thus: at one end of a ruler erect a sight, through which to see the pole-star, &c. At the other end set up two circles of small wire, one within the other; the diameter of the innermost, equal to double the tangent of the distance of the pole-star from the pole, the distance of the sight being radius; and the diameter of the outermost circle, equal to double the tangent of the distance of the next star to the pole-star, from the pole. The instrument thus prepared, if you look through the sight, and bring the two circles to the two stars, whose distances from the pole they represent; a line passing through a sight and centre of the circles, is the elevation of the pole: and two plumb-lines hung up, one over the sight, the other over the centre of the two circles, will lie exactly in the meridian of the place. Mr. Gray's ingenious Contrivances are in the Phil. Trans. N° 268 and 270.

The last instrument is what I have made use of for several years, and which I can recommend on my own experience, for a ready way to find the meridian of any place, and to see the transits of the celestial bodies over it, either northward or southward. The instrument is thus made of wood, or rather iron, or brass, to indure the weather, without swelling or contracting, viz. prepare a small flat iron bar, as cc in fig. 21, pl. 3. At each end of which rivet on two upright sights, to turn stiffly, at the joints II. Let one of the sights, cd, have a perforation, large enough to see the pole-star through it; the other sight, ab, a very small perforation, to see the sun through. Just behind the joints fix two upright arms CD and CD, but to bend off, so as to be out of the way of the sights, when you look through them. These arms ought to be long enough for the plumb-lines to reach the pole-star, on one side; and the sun at his greatest height, on the other side, when you look through either of the sights. The plumb-lines therefore are tangents to their opposite sights, and their lengths may be found by a table of natural tangents, making the distance of the two sights radius. Thus, in the latitude of London, if the instrument be two feet from sight to sight, the southern plumb-line should be near 4 feet, and the northern one about 2 feet 10 inches. On the tops of these two arms, place two small cross pieces DE and DE, to turn with a joint at D; which cross pieces are to hold the plumb-lines EF and EF, and to turn off and on, so as to bring the plumb-lines exactly to the sights. Place this instrument on a pedestal H, to turn round on it stiffly at the pin G.

The instrument being thus prepared, the way to set and use it is thus: plant it in a convenient place, where the pole star may be seen by night, and the sun by day. When that star is on the meridian, is the time to set the instrument, which is thus to be done; viz. through the sight with the larger hole, cd, look at the pole-star, and turn the whole instrument about, till you see the opposite plumb-line nicely to intersect the pole-star. Or when you have brought the plumb-line near the star, you may more easily bring the plumb-line to intersect, by moving the sight cd backward or forward, at the joint I, instead of moving the whole instrument. And that you may more easily see the pole-star through the sight, let the plumb-line be a very fine cat-gut string, or horse hair, &c. And if it be white, or some such light colour, it will be the better seen, with the help of a candle shining on it by night; which is necessary.

The sight cd, and opposite plumb-line being thus set in a direct line with the pole-star on the meridian, it is manifest, that the instrument lies exactly in the meridian, so as to see any star on the meridian to the north. And that you may see the same southerly; the next day, or when you please, you may hang up the plumb-line EF, on the southern arm CD, so as that the plumb-line may exactly intersect the perforation cd. This may be easily done by moving the top joint, with the plumb-line on its cross-piece, backward and forward, till the plumb-line hangs to your mind. If the sight, with the lesser perforation ab, be not exactly under the northern plumb-line, it must be brought to be so by turning the sight, by help of its joint at I. And then all the instrument is set right; so as to see the sun, moon, or stars come on the meridian towards the south.

But to see the sun transit the meridian, it is necessary to guard the eye with a coloured glass, or a glass darkened with the smoke of a lamp or candle; which is done in the following manner: Chuse two pieces of glass, cut into the same size and figure; taking care that they do not refract vitiously; which may be known by moving the glass before the eye. If the objects you look on seem to dance about, the glasses are false and refract; but true, if all seems steady. Smoke one of these glasses over the flame of a lamp or candle, till it be obscured enough to take off the sun-rays, but not so as to darken it too much: which may be seen by looking at the sun or candle with it. One of the glasses being thus darkened, lodge them both together, and fasten them in a little case fit for the purpose, with the smoked side innermost, and an edging of card between to keep them asunder, so as that the soot may not be rubbed off or disordered. It is is proper to have two glasses thus prepared; one for a strong sun; the other less darkened, for the sun behind a thin cloud, mist, &c. With one of these glasses, held behind or before the sight ab, you may plainly view the sun pass.

To imitate the aforesaid Instrument on a Journey, or wherever you come.—Instead of an entire instrument, only prepare two sights, as in fig. 22, with perforations as before. Let these sights be nailed or screwed down on the tops of two stakes at II, so as to turn stiffly upon them. The plumb-lines, at least one of them, may be hung up at the end of a house, as at K, or on the bough of a tree, if the wind do not shake it, or any where you see fit. And the sights must be stuck up, so as to bring the pole-star to intersect, and all be-performed as before directed. This, although in a manner the same with the instrument before, yet is more convenient in some respects: chiefly because the plumb lines may be made longer, and the sights set farther asunder, than in the instrument before can conveniently be done; which is some little advantage for seeing the transits. These sights may also be made so light, as to be easily carried about; or they may be readily made or imitated in any place wherever you come.

To know when the Polar Star comes on the Meridian.—The way is this: subtract the right ascension of the sun from the right ascension of the pole-star, the remainder gives the degrees, minutes, and seconds when the pole-star transits the meridian above the pole. Divide these degrees by 15, it gives the hours, and every degree under 15 multiplied by 4, gives the minutes; and every minute multiplied by 4, gives the seconds, of apparent time of the pole-star's southing. I scarcely need to say that it comes under the pole at 12 hours distance, only making some small allowance for the alteration of the sun's right ascension in that 12 hours time. But you may shorten the labour, by using tables of the sun's right ascension in time, instead of his right ascension in degrees, &c. which may be found in Sir John Moor's Math. Compend. and in several other books. If the sun's right ascension exceed the pole star's, add 360 degrees, or 24 hours, and then subtract.

The right ascension of the pole-star is determined by Mr. Flamsteed, to be 33m. 4s. of time, Anno 1690; and the increase of its right ascension in 10 years is 1m. 16s. of time. Therefore this present year 1703, the right ascension of the pole-star is 35m. 22s. of time.

Or you may see when the pole-star comes to the meridian, by hanging up a plumb-line, and observing when the thill-horse in Charles's Wain, called Alioth, comes near the line, together with the pole-star, on one side the pole; or the bright star of the 3d magnitude in Cassiopeia's thigh on the other side.

The foregoing instruments may be set by any other star, as well as the pole-star. But the pole-star, in our northern hemisphere, is most convenient, because it makes but a small circle round the pole, and therefore moves slower, and consequently is longer in transiting the meridian. And therefore a small error in calculation, or a little expence of time in setting the instrument, may be disregarded.

The Uses of these Meridian Instruments.—1. You may see with all imaginable exactness, when it is noon, even to 1, 2, or at most 3 seconds of time. For you may see when the very limb of the sun touches the meridian, and while all his disk is passing it. So that it far exceeds all sun-dials. And besides another great conveniency is, that it will fit most latitudes. So that there is no need of having a strict regard to the elevation of the pole, nor any danger of error in making and setting, as is in most other instruments; but all is with ease and certainty performed. Therefore, 2dly, Into whatever place you come, you may easily see the errors of the sun dials there; and which go truest, and which false.

3dly, As the sun, so also the fixed stars may be seen to transit the meridian; by which the hour of the night may as exactly be known, as of the day by the sun, knowing the right ascension of the star that transits. For, as above, subtract the right ascension of the sun from the right ascension of the star, the remainder converted into time, is the time of that star's culmination or southing. And if 12 hours be added or subtracted, making due allowance for the alteration of the sun's right ascension in that time, it shows the exact time of that star's coming to the meridian northward.

4thly, The hour of the day and night being thus, to 1, 2, or 3 seconds discoverable, by the aforesaid instruments, I doubt not but that they may be useful in finding the exact differences of meridians, either by the eclipses of Jupiter's satellites, or the occultation of the fixed stars by the moon.

I do not pretend that these instruments are any otherwise useful in finding the longitude, than by showing the exact time of the day or night; which is one thing absolutely necessary in this matter. Neither will they serve without a well adjusted pendulum-watch, or pocket-watch, that will keep time exactly from one observation by the meridian-instrument to another. Nor are they useful on shipboard; but only on land, where they may remain fixed. Though on head-lands, or any where on shore, they may be useful to the seaman. And indeed, till better discoveries are made, they may be of service wherever long telescopes can be used, for seeing the appulses of the moon to the fixed stars, or the eclipses of Jupiter's satellites; which is only on land. Unless a convenient standing for a man, and a telescope might be hung pendulously in a ship, which, especially in a calm sea, may be as little subject to disturbance, as the pendulums of watches are, which will retain their motion at sea.

5thly, You may with all exactness continue a meridian line for many miles, by looking through either sight, and observing what objects are intersected by the plumb-lines.

6thly, These instruments are prepared with little cost or trouble, and easily carried about, or imitable in any place, the latter especially.

A further account of the Pediculus Pulsatorius, or Death-Watch, continued from* N° 271. *By the Rev. Mr. Wm. Derham, F. R. S.* N° 291, p. 1586.

Plate 3, fig. 23 shows the death-watch, as it appears to the naked eye, and

* The insect here described, is the termes pulsatorium. Linn.

fig. 24, as magnified with a microscope. This insect very much resembles a louse in shape and colour, but it runs more nimbly: it is common in every house, in the warm months; but in the cold season of the year, it hides itself in dry obscure places, and is seldom seen.

Some time after their copulation, they lay their eggs in dry, dusty places, where they meet with least disturbance; for in such only I have found them. These eggs are very minute, much smaller than the nits of lice; though lice are not much larger than this insect is. These eggs are white, and shaped like nits, but more transparent; and, like the eggs of all insects that I have observed, are hatched by the warmth of the approaching spring, which to them is the same as an incubation. At their first leaving the egg-shell, they are exceedingly small, so as scarcely to be discerned by the sharpest eye, without the help of a convex glass. With a microscope I have seen them crawling about, but could scarcely perceive any hairs, feet, &c. they rather looked like moving eggs. At the first leaving their shells they are less than their eggs, though the eggs are scarcely visible without a microscope.

These young death-watches are perfectly like the mites in cheese; I could not perceive any difference between them, when much magnified with a microscope, but only that mites have more bristles about their breech. In this shape they continue 6 or 8 weeks, feeding on divers things they can meet with. Indeed they are a great annoyance to me, in devouring or defacing my specimens of insects. And there are scarcely any sorts escape these voracious, though minute animals. From this mite state, they grow gradually to their more perfect one: when they become like the old ones, they are at first very small, and then run about more swiftly than when mites, in which mite state they creep but slowly.

An Eclipse of the Moon, observed near the Royal Exchange, in London, on Sunday Morning December 12, *Anno* 1703. *By Mr. J. Hodgson, F. R. S.** N° 291, p. 1594. *Translated from the Latin.*

Concerning this eclipse, Mr. Hodgson saw the moon more than 20 times, from the beginning to the end of her emersion; yet by reason of the intervening clouds, and the short time he had to view her in, he could not pretend to determine any thing with exactness.

* Mr. James Hodgson was a respectable mathematician, and sometime master of the Royal Mathematical School in Christ's Hospital, London. Besides a multitude of communications in the Philos. Trans. from vol. xxiv. to vol. xlix. inclusive, he was also author of several mathematical publications: as, 1. Treatise on Navigation, in 4to. 1706; 2. System of the Mathematics, in 2 vols. 4to. 1723; 3. The Theory of Jupiter's Satellites, 4to. 1750; 4. The Doctrine of Fluxions, 4to. 1758; 5. The Valuation of Annuities upon Lives, in 4to.; 6. An Introduction to Chronology.

Concerning Balls voided by Stool. By Mr. R. Thoresby, F.R.S. N° 291, p. 1595.

A poor girl at Rawden, near Leeds in Yorkshire, about 14 years of age, having been tormented with colical, and, as was supposed, nephritic pains for some time; at length voided a roundish ball, per anum, as hard as a stone. After a while, the pains returning with greater violence, so as to make her roll on the ground, she voided another as hard, and much larger. Upon which, a neighbouring gentlewoman, who had been much afflicted with the gravel, gave her some of those medicines which she used to take herself; after which, the girl voided a third ball, also per anum, with less pain, though the largest of the three.

The first of these balls is smooth and glossy, of the colour of a hazel nut, 3 inches round, and somewhat compressed. The other two were rough and gritty, and in like manner a little compressed into a kind of obtusely triangular figure. The 2d is 4 inches and a half round; the last 5 inches and a half. Considering their bulk, all 3 are very light, especially the 2 latter and greater ones, of which the last weighs but 5 drachms 36 grains; and both of them float in water. This lightness proceeds from the matter they consist of; which, in some places is purely downy or fuzzy; in others, mixed with a gritty substance, yet not confusedly, but regularly mixed. The fuzzy parts possessed the central part of the ball, with a small particle of blackish glass or other vitrified substance in the very centre itself. Over which are several coats, gritty and fuzzy, alternately ending in the circumference with a grit, much resembling the ground work and superstructure of the oriental bezoar-stone.

The powder of one of these balls scraped off with a knife, is no way moved or affected with any sort either of alcaline or acid liquor dropped on it. Nor does it stink when burned; it consists therefore of no animal substance; but the girl being of the age usually attended with the green-sickness, the gritty parts (with the glassy particle in the centre, as the most ponderous and least moveable) seem to be broken off from tobacco-pipes, and ground small between her teeth; the downy or fuzzy, to be licked or scraped off the lean of mutton, or the rind of peaches, or some other part or plant; the girl's stomach kneading and concreting the matter into a coat, as her changeable appetite supplied it alternately with one or the other sort.

An Account of Books—I. Epistola D. Guilhelmi Musgrave, S.R.S. ad Editorem missa, in qua Ratio redditur Libri nuper editi, cui Titulus, De Arthritide Symptomatica Dissertatio. Auctore Guilhelmo Musgrave, M.D. Inclyti Medicorum Londinensium Collegii, et R.S.S. 8vo. N° 291, p. 1597.

In this treatise Dr. Musgrave distinguishes arthritis into arthritis primigenia and arthritis symptomatica, conformably to the following scheme:

- **Arthritis.**
  - Primigenia
  - Symptomatica
    - Falsely so called, which succeeds to Rheumatism,
    - Improperly so called, which succeeds to Chlorosis, Dropsy,
    - Properly so called, which succeeds to Melancholy, Scurvy, Lues venerea, Asthma, Fever, Colic, Some cutaneous diseases.

He then enters into a description of the diagnostic symptoms of each species, and lays down the method of treatment.

II. Specimen Lithographiæ Helveticæ curiosæ, quo Lapides ex Figuratis Helveticis Selectissimi Æri incisi sistuntur et describuntur, a Johanne Jacobo Scheuchzero, M.D. Tiguri,* 1732, *8vo.* N° 291, p. 1604.

Dr. Scheuchzer of Zurich, the author of this specimen, has shown much diligence in his mineralogical inquiries. With indefatigable industry, and great expense and danger, he has carried his researches to the tops of the highest mountains of Switzerland; and even there found a variety of sea-shells, and other marine productions. Besides his descriptions, the author has caused icons to be engraven of all of them. The chief of the figured native fossils he found in this country, are the belemnites, p. 25, 44, the selenita rhomboidalis, p. 49, and the fluor crystallinus trigonus, p. 29. He gives a remarkable variety of the fossil corolloid bodies; ex. gr. corallium fossile cortice reticulato, p. 14, retepora seu eschara maxima Imperati lapidea, p. 13, alcyonium tuberosum forma ficus vel quintum Dioscor. p. 17, fungulus pyriformis lapideus,

* Dr. John James Scheuchzer was Professor of Mathematics at Zuric, where he was born in 1672. He wrote many large and valuable works on natural history, and died in 1733. Besides the treatise above mentioned he was author of the following publications, most of which are enriched with a vast number of plates: (1) Historiæ Nat. Helvetiæ Prolegomena, 4to. 1700. (2) Itinera Alpina, 4 vols. 4to. 1702-1711. (3) The Nat. History of Switzerland in the German tongue, 3 vols. 4to. 1706-1708. (4) Herbarium Diluvianum, fol. 1709. (5) Museum Diluvianum, fol. 1716. (6) Bibliotheca Scriptor. Hist. Nat. 8vo. 1716. (7) Hydrographia Helvetica, 4to. 1717. (8) Biblia ex Physicis illustrata, forming several folio volumes, and containing descriptions and figures of the various natural productions mentioned in the Bible. In addition to the above, he wrote two or three medical tracts, and communicatated several papers relating to astronomy, meteorology, anatomy, and natural history, to the Royal Society, inserted in the Transactions of that learned body between the years 1707 and 1726. He had a brother (John Scheuchzer) who was likewise distinguished by his knowledge and writings in natural history.

p. 6; astroites, p. 36, 39. Of the sea-shells that he gathered on these mountains, the most considerable are the nerita, p. 26; auris marina fossilis, p. 58; umbilicus marinus, p. 24; concha tellinoides, p. 21; conchæ margaritiferæ fragmenta, p. 55; conchula echinata, p. 49; pectunculus parvus capillaribus striis notatus, p. 23; pecten dense striatus, ibid; chamafossilis, p. 55. In this shell he observes there were discernible the vestigia of the muscles by means of which the animal adhered to the shell. Nor ought we to omit the echinus spatagus, p. 61; the asteriæ, p. 2; the entrochi, p. 4, &c.; two joints of the claw of a lobster struck out of a piece of stone, p. 27; and a piece of a shell of a crab, of that sort that is called the molucco-crab, lodged in a very hard sort of stone, p. 65.

III. De Locis solidis secunda Divinatio Geometrica, in quinque Libros Injuria Temporum amissos, Aristæi Senioris Geometræ. Autore Vincentio Viviani, Magni Ducis Etruriæ Mathematico Primario, et Regalis Societatis Londini Sodali. Opus Conicum in Lucem prolatum, Ann. 1701, *Fol.* N° 291, p. 1607.

Vincentio Viviani, who was 80 years old when this book was published, and the only surviving scholar of the famous Galileo, was ever since the year 1642 employed by the great Dukes of Tuscany in directing the fortifications of their dominions, superintending their buildings, the banks of the rivers, and other public works; so that he complained that he had not leisure to prosecute his inventions in geometry, which he made in his younger days, of which this book is one.

His other works are these. In the year 1659, he published in folio, De Maximis et Minimis Divinatio Geometrica in Quintum Apollonii Librum. In the year 1692 he published in Italian, Formazione e Misura di Tutti i Cieli, in consequence of an ænigma architectonicum which under a fictitious name he had proposed in the beginning of that year, in which divers learned men concerned themselves.* These are all he has published in mathematics besides this book, and two small pieces, containing the solutions of some problems proposed by French mathematicians. He died at Florence, An. 1703, and left many other works, though in an unfinished state.

In the preface to this book, he gives an account of Aristæus Senior Geometra, as far as can be gathered from the ancients, and of his writings. This Aristæus wrote five books of Conic Sections, which Euclid himself valued so much, as to imitate and add to them, as Pappus says. He wrote other five

* For solutions of this enigmatical problem, see pp. 479 and 609 of vol. iii. of this Abridgment; where also is some account of the life of Viviani.

books De Locis Solidis, which are these that Vincentio Viviani pretends to restore. Pappus also seems to say that he wrote a history of what had been done in geometry until his time. And Campanus, in an annotation on Prop. 1. Lib. XIV. of the Elements, mentions a book of Aristæus, entitled, Expositio Scientiæ quinque Corporum, from whence it may be inferred that he was of the Platonic sect.

Though the authour intended five books, as Aristæus had written; yet he has published only three, and seems to despair of ever publishing the other two.

LIBER I. *In quo de Locis Ordinationum Conicarum Limitibus pertractatur.*— This book is divided into five parts. Part 1, are 34 lemmatical propositions, with considerable improvements in demonstrating the properties of the conic sections from the regulatrix; moduli ex semirecto, ex verticali, ex laterali; and in the hyperbola, from the modulus ex asymptoto triangulum circumactum a symptotale, &c. all which he there defines.

Part 2, prop. 35, shows that the altitudines normalium (or the subnormals) in all the conic sections, erected from the points of the axis, where the ordinates are erected, are ad locum planum; and prop. p. 36 and 37, that the normals to a right line and a circle erected as above (which is always understood) are ad locum planum; but in the 38 to the 42, that the normals of the conic sections are ad locos solidos, which he there determines. Part 3, prop. 43, shows that in all the conic sections and the circle, the altitudines normalium super ramos ex vertice are ad locum planum; but from thence to the 49, that the normales super ramos ex vertice are ad locos solidos, which he there determines.

Part 4, in the first three propositions, from the 50 to the 52 inclusive, he determines the locus solidus of the rami from the vertex of a circle, or from an origin between the vertex and the centre, or without the circle. Prop 53, he shows that the rami from the focus of any conic section, erected to the axis, are ad locum planum of a right line, there determined. In the following prop. to the 58, he determines the loci solidi, made by the ordination of the rami of a parabola, drawn from the principal vertex, and from an origin in the axis between the vertex and the focus, and below the focus, and above or without the vertex. In the next four, to the 62, he determines the loci solidi made by the ordination of the rami drawn from the origin in the lesser axis of an ellipsis; viz. either the vertex, the centre, between the vertex and centre, or without the vertex. From the 63 to the 68, he determines the loci solidi made by the like ordination of the rami on the greater axis of an ellipsis. From the 69 to the 77 the like is done in regard to the hyperbola, where there occurs a greater variety, as it is here managed. The next two propositions are the like in the opposite sections, where they superadd any thing to what was before said of one hyperbola. And because a rectilinear angle may be considered as an infinitely narrow hyperbola, viz. whose transverse axis is a point, in the 80 and last proposition of this book, he determines the locus solidus, made by the ordination of the rami to this angle, from an origin in its axis, either within or without the angle. To this book he subjoins an epilogue, containing some general corollaries useful, as he says, toward some things which he intended to publish: as, that in a circle the loci solidi, made by the ordination of the rami from an origin in the vertex, or within, are parabolas, which are all similar lines; and that the loci plani, arising by the ordination of the rami from the focus of a conic section or circle, are straight lines, which are also similar lines; and so in other cases, that like loci arise from such applications.

Part 5, the first two propositions determine the loci solidi, arising when the tangents of the parabola, intercepted between the section, and either the axis or the tangent at the principal vertex, are made ordinates to the principal axis. And the next two determine the loci solidi, arising when the normals, either to the section, or to the rami, proceeding from the principal vertex, are made ordinates to the tangent in the said vertex.

Liber II. *In quo Loci Ordinatarum Potentium Limites indicantur.*—In this he treats at large, in 71 propositions, of the loci, both plane and solid, arising from ordinates on a straight line, whose squares are equal to the sums or differences of the rectangles and squares of a line, and its segments, and other assumed lines, in all the variety and combinations of them.

Liber III. *In quo Loci Variarum Dispositionum Limites assignantur.*—Here is the determination of the loci plani and solidi that arise from several ways different from the former. For example, if from two given points there be drawn several pairs of straight lines, whose squares together are equal to a given square, the concourse of each pair is in the locus planus of a circle there determined. And, the other conditions remaining, if of each pair of straight lines, one be drawn from a given point, and the other be perpendicular to a given straight line, the concourse is in the locus solidus of an ellipsis, there determined. Afterwards are several problems concerning arithmetical, geometrical, and harmonical mean proportionals between two extremes, and divers methods for describing the conic sections by points. There are also subjoined several addenda to all the preceding three books.

At the end of the books are prints of the orthography and gate of a stately house built by the author Vincentio Viviani at Florence, with the inscriptions on its front, in honour of the French King Louis XIV. from whom he had an annual pension for many years: and of the family of the Great Duke of Tuscany; and of his preceptor the famous Galilæo Galilæi, with a print of Galileo's busto in brass, which is set over the gate.

An Account of Cochineal. By Mr. Ant. Van Leuwenhoeck, F.R.S. N° 292, p. 1614.

A merchant of Amsterdam writes, that it is impossible, and altogether incredible, that the drug called cochineal should be, as I have asserted, flies, or any sort of animal endued with wings, head or feet; not only if we consider the vast number of them that are brought in every fleet from America; for you will find that two of the largest of these particles, 8 of the middling sort, and 20 of the smallest, scarcely weigh a gold grain; so that in a pound of them, at a medium of large and small, one may count 102400 particles; now in a fleet that brings 200000 pounds of this drug, what a vast number of animals must there be? Besides, says he, where can you find men enough, who at the proper time of the year shall catch these insects, and dismember every individual by pulling off its head, legs and wings, &c. so that he concludes that cochineal must needs be a fruit, or the excrescence of some kind of plant.

Though I am convinced that cochineal is nothing else but the trunk or hinder part of a living creature, and was persuaded also that the cochineal animals, like other insects from worms, are changed into flies; yet for further satisfaction, I have renewed my inquiries upon this subject, and in so doing I find reason to reject some of my former positions, being now fully convinced that the cochineal animals are not produced from worms, but at once bring forth their own likenesses.

Mr. L. then premises a short abstract of what he before said in N° 193, viz. that there is a certain plant called the prickle pear, or Indian fig; the leaves of which are round and thick, and sharp pointed; that upon the leaves or twigs of the said plant are small knobs or protuberances, from whence are produced by the heat of the sun little worms; that these worms in process of time become flies, in likeness to cow-ladies or lady-birds, as some call them, which when they are arrived to their full growth, are taken in this manner; to windward of the plant, on which these animals are found, they kindle a fire of any combustible matter, having first spread cloths under and round about the said plant, with the smoke of which they are presently suffocated; then shaking the tree, they receive them upon those cloths in great numbers, and with very little trouble; after which they spread them abroad in a like cloth on a sandy place, or a stone-floor, where they are exposed to the heat of the sun till they are dried, that is, till their small bodies are shrivelled up together, and rubbed between the hands till their wings, legs, &c. fall off, which are garbled out, and then the remaining trunks of the animals are put into shallow copper boxes, till they become quite dry. The aforesaid plant has no flowers or blossoms on it, and its fruit is of a fleshy substance and red, and when ripe, by handling it, the fingers will look as if they were stained with mulberries. Some say, that the cochineal worms feed on the blossoms and fruit of this plant, which causes their bodies to be of that red colour. And that if you take the seed of the plant or the dead worms, and dry them after the above-mentioned manner, that cochineal is not so good as when those animals have got wings, and are then smothered."

Now for further satisfaction, I took several particles of this same cochineal, both of the largest and smallest, and having dissected them, I found that they had all eggs in their bellies, excepting only one that was exceedingly small. Having opened some of the largest trunks, and separated the eggs, which I took out of their bodies, and counted them, I judged that there were above 200; and having observed several of them with my microscope, I could perceive not only a membrane or shell on most of them, but also an animalculum of an oval shape included in the said shell, and almost as large as the shell that contained it, which seemed at first very surprising, and almost incredible in so small a species of fly as the cochineal, till by a very nice and long inquiry I was fully satisfied, that it was really an animalculum that lay within it. I pursued this operation with so good success, that I not only separated the egg-shell from the animalculum, but in some of them I could perceive their legs also orderly folded up against their body, and could separate them from it, especially in such as were full grown; nay, in some I even discovered the several joints of the legs, and thus in the space of two days I saw the legs of 100 animalcula, many of which in my handling were broken off, and lay by themselves.

We must not imagine, that these animalcula have such short legs as the caterpillars or silk-worms; but the unborn ones have, in proportion to their size, legs as long as those that are full grown; and as the legs stand close to the head, in that part which one may call the breast, so when the said animalculum lay stretched out at length, its little legs could be just seen peeping out of the body.

Thus are those persons mistaken who give the name of worms to these animalcula, and the reason of their error proceeds from hence, that through the exceeding smallness of the object, they are not able to discover with their naked eye, whether the new born insect be a worm or any other kind of animal; 200 or more eggs of these animals can lie in so small a particle of matter, as a single grain of cochineal; to which if you add the consideration of the great number of blood-vessels lying in so narrow a compass, and that each egg receives its nourishment and increase, as it certainly must, by a string or artery; and that probably there are veins in every string for carrying on the circulation of the blood; how then can we sufficiently admire the depth of Almighty wisdom, in the structure of such animals. The shape of the eggs of the cochineal fly is very like that of our hen eggs.

On viewing some of these embryos, after having divested them of the membrane or shell in which they were shut up, I observed on their head, a kind of a tool or instrument, about a fifth part as long as the whole body of the animalculum, and at the extremity a very slender point, something like that instrument which those animalcula have that are found on currant bushes, &c. and by which they get their food; and when they have so done, they clap it to their breasts till they have occasion for it again. From whence I infer that the cochineal flies also acquire their food after the same manner, viz. that they have no teeth to gnaw the leaves of the plant, as silk worms do, but that they only insinuate their said instrument into the leaves, and after that manner get their nourishment. And this notion seems to be supported by what an old Spaniard said, viz. that these animalcula feed on the blossoms and fruits of the plant, and that by those means they became red. From hence we may conclude, that the insects do not hurt the leaves, fruits, nor even the blossoms of trees, as far as we can discover; which may also the better satisfy us, that the cochineal flies, with the abovementioned instrument, by boring into the leaves, acquire both their food and increase.

We see, that in all small flies, that are produced from worms or maggots, the smallest are always the males; and this rule holds good also in flies and lice, among which also the hinder parts of their female bodies are always larger, by reason of their being so often impregnated with eggs; but when I had soaked their trunks thoroughly in water, in order to some further inquiries, I then imagined, that all the cochineal flies are females, and that hardly one fourth part of them was arrived to their full growth, before their bodies are filled with young. This position of mine, that all the cochineal flies are females, may seem very surprising, and perhaps not meet with credit by those that maintain there can be no animal generated without a copulation of male and female; but they would be of another opinion if they had seen the unspeakable number of animalcula which last summer, 1703, infested the leaves of the lime-trees, or those others that were found upon currant-trees, cherry-trees, or hazel-nut-trees; all which animalcula bring forth live young; and these young ones being very small, have their bodies full of other young, and are all females, and consequently there is no copulation among them; these, when they are full grown, get wings; so that there is no other change in them, than increasing in bulk, and the sprouting out of wings.

Now if this be true in these animalcula, though they are fifty times smaller than the cochineal flies, we may easily believe the same of these also, especially since the hinder parts of all of them are much alike; in confirmation of which we may add, that eels, prawns, or shrimps, have also no males among them. The old Spaniard said, that when the cochineal flies are dry, they rub them between their hands, and so the wings, &c. are separated from the remaining trunk; but if he had been more exact in his observations, he might have found that not only the wings were thus separated from the hinder part of the body, but also the upper part, with which go also the legs, the wings, and the head. It is easily perceived that the lower part is divided from the upper by nothing but a kind of a short string, no thicker than a hog's bristle; so that one part may be easily separated from the other, especially when the animalcula are dried.

The old Spaniard further affirms, that the cochineal is not so good till the animalcula have got wings; from whence we might be apt to conclude, that the cochineal animalcula become flying insects altogether, like silk worms, which from reptiles are all changed, and that in a very short time, into butterflies. But the case is quite otherwise with these cochineal flies, for they do not lay their eggs all together, or in one day, but I rather suppose, that one of these animalcula at once brings forth 20 eggs or young ones; and so they require above 10 days before they can be delivered of all their eggs, for after I had taken 200 eggs out of some of the cochineal flies, I saw exceedingly small ones still remaining in the ovarium or egg-nest.

In my observations upon the lime-trees, I saw not only several animalcula that had wings, but others much smaller, and that in a gradual descent, so far that many of them were hatched that very day; and these observations I did not make at one certain season of the year only, but found that they continued to hatch as long as the day were moderately warm. So I suppose it is also with that animalculum whose trunk or lower parts compose what we call cochineal, at least it was my opinion, after I had sufficiently observed several grains of cochineal given me by three different persons; for when I compared some of the largest grains of cochineal with the smallest, I found that 15 of the small ones were scarcely equal to one of the larger; and when I viewed the powder or dust of the cochineal, which I took out of the bottom of a box, I met with some trunks of those flies so very small, that I judged 100 of them not equal to one large grain.

Plate 5, fig. 6, AB represent a grain of cochineal. CDEFG, fig. 7, is another grain, as it appeared through the microscope; the extreme parts by C, and by EF, a seeming orifice, which is the part where the string was broken off, and by which both parts of the body were joined together. The concave bows or circles that appear in the grain DEFG, are not natural, but adventitious to it, proceeding only from the drying or shrinking up of the great number of eggs that lie within the animalculum; for if the grain were well soaked in water, those concave parts would become convex, and be also more obvious to the sight, as well as the stem of that string abovementioned.

But on taking one of the largest grains, which is somewhat flat, that part of the trunk will not shrink inwards; the reason of which probably is, that the animalculum, before it was killed, had discharged most of its eggs.

Fig. 8, HIK show an egg with its shell or membrane, as it was taken out of a grain of cochineal, in which egg might be seen the young one within, and the shell surrounding it.

Fig. 9, LMN represent an unborn cochineal animalculum, which I had separated from the egg-shell with a great deal of pains; it lay with its back to my sight, and in such a position as to show three of its legs.

By fig. 10, OPQRS show a small particle of the vessels belonging to an ovarium or egg-nest; where may be seen divers broken filaments or strings, to which the eggs were fastened, except the great vessel R, through which probably several other vessels received their matter for the nourishment and increase of their eggs. ST shows a string to which the egg TV was fastened, as other eggs were to the other strings before I broke them off. These strings OPQRS were almost transparent, and I could see other small particles in them when I took them out of the cochineal grain, and separated the eggs from them; but as they began to dry, they assumed a reddish hue, and when they were quite dry, they became of a light red colour.

Fig. 11, WXYZ represent another animalculum, which I also took out of its egg-shell, in which the legs are to be seen very plainly, between W and X, but I could not see that leg which lies upon the body.

Fig. 12, ABCD is another animalculum, cleared of the egg-shell, in which may also be perceived the legs between A and B.

Fig. 13, EFGHI represents an animalculum lying upon a glass, as I had taken it two days before out of a cochineal grain; it was not much altered by drying; it had but two legs left, the other being broken off. In this animalculum there appeared, at the extremity of the head, a crooked part GH, which I take to be the instrument with which it extracts its nourishment out of the leaves of the plant.

Fig. 14, KLM represent a small part of the blood-vessels, which partly covered another animalculum, NOP, with six legs, taken out of the belly of its dam. I saw also among the eggs that I had separated from the ovarium, as also among those fastened to the vessels of the ovarium, such exceedingly small eggs, that 100 of them did not equal one large egg.

In all the cochineal grains I ever examined, I found eggs in their bellies, and young ones in those eggs; but in some many more than in others, and in those whose bellies were much shrivelled, I found but few eggs: from whence I concluded that such as had but few eggs in them had already brought forth a great many young, and would not have lived long, according to the age of all small flies, which die soon after laying their eggs.

Now, forasmuch as those animalcula that are found upon lime-trees, &c. have young ones in their bellies, even before they come to half their growth; therefore in order to see whether it be so likewise with the cochineal flies, I took eight of the small ones, which I judged did not altogether make more than one large grain, such as is represented by AB in fig. 6, and steeping them over night in rain-water, next morning I found only five that had subsided, the other three being so light as to swim on the water, which consequently had not penetrated into them. Out of the first of these small grains that I dissected, I took eleven eggs, in some of which the animalcula were so completely formed, that I could easily see their legs, besides several exceedingly small eggs. In the second I could meet with no eggs that were come to their full growth. Out of the third I took three perfect eggs. In the fourth there were none perfect. In the fifth two perfect eggs, but always less. From these observations I conclude, that the production of the cochineal flies takes place in the same manner as that of the animalcula on the lime, currant, plum, and hazel-nut-trees.

As for what the old Spaniard said further, that they stifle the cochineal flies with smoke as soon as they have got wings, because then the cochineal is better; this is not at all surprising, because, when the flies are arrived at their full growth, their bodies are then most full of eggs; from hence probably is chiefly derived that noble colour of scarlet, though it seems that most of these cochineal flies are killed or smothered before they come to their full growth.

The cochineal flies in all appearance dwell on the back or underside of the leaves, which defend them from the great heat of the sun in those climates; and as the smoke cannot destroy all those flies, the few that remain must multiply very much in a short time. I had got about a spoonful of powder or dust, together with some sands, out of the cochineal box, and found that what appeared to be nothing but dust, was abundance of very small cochineal flies, and some of them so minute, as if they had been just hatched, and some of them gradually larger than others; there were also other small particles, which I judged to be the excrements of the animalcula; I saw also abundance of legs with three joints, and some also that had but two joints, and a few one joint only; among these legs, some had claws on, which were either white, or dark coloured, or of a light red.

I could perceive, though with great difficulty, 6 legs in some of the animalcula I had extracted out of the abovementioned eggs, disposed in such exact order, as may be observed in the aurelia of a silk-worm. At the same time I discovered that the unborn animalcula had two horns, in which, at one time, I counted five joints, and another time I thought I saw more.

After these observations, viz. that the cochineal animalcula are not changed from worms to flies, I reject my former positions, viz. that they have no shields with which they cover their wings; having found among the cochineal grains, little black shields or vaginæ, with a small round red spot on each shield.

The animalcula, whose wings are covered with shields, seem all of them produced either in the earth or in wood, from whence they receive their nourishment and growth; and if nature had not made this provision for them, being shut up in the earth or wood after that they are changed into flying insects, they could not dig out their way, without hurting their tender wings. For having found among the cochineal grain, one of the aforesaid shields on the hinder part of an animalculum, and viewed the same more narrowly, I saw plainly, that that trunk or hinder part had no similitude with any of the other grains.

Now seeing that all animals from the beginning are made to bring forth their like, if the young cochineal flies had been endowed with wings, it would have been in vain, for the reasons above-mentioned; though indeed it is necessary they should have wings as soon as they are full grown.

I sent my Amsterdam friend a duplicate of what I have here related, as also a copy of the figures, who returned me an answer, stating, that he has also taken 200 particles out of a large cochineal grain; but that he could not, after the nicest observations, discover any animalcula in the eggs, &c. wherefore he finally concludes, that what I called blood-vessels, are analogous to the same parts which we find in cherries, grapes, &c. and that what I take for the shell or membrane of the egg, are only the skins that cover the seed. Now, though I was entirely satisfied with the account I have given, and with the figures taken of the cochineal grain; yet I dissected several others of the largest sort, and took the animalcula out of the egg-shell, and placed them before divers glasses in such order, that I could not only distinctly see the body of the animalculum, with its parts, divided into several circles, but the two horns also, with the joints with which nature has provided all these unborn animalcula, were as plainly visible.

Fig. 15, ABCDE show the body of the said animalculum; BH, DI and DK the 4 legs, the 2 other being hid from the sight; EF represent one of the horns, of which we had a fair view; the second horn AG was not placed in so convenient a light, and consequently not so well delineated; at the extremity of the horns there were three small hairs, which are also seen at F and G.

*An Experiment, to show the Cause of the Descent of the Mercury in the Barometer in a Storm. By Mr. Francis Hauksbee.** N° 292, p. 1629.

In the late hurricane of wind, Nov. 1703, it was observable, that the Mercury in the barometer did not only considerably subside, but on extraordinary gusts a visible vibration of the quicksilver appeared. And to prove that high winds can lessen the pressure of the atmosphere, an experiment has lately been made at a meeting of the Royal Society at Gresham College, April the 12th, 1704, by Mr. Francis Hauksbee, giving a demonstration of this phenomenon. Plate 5, fig. 16, the recipient A containing about 5 quarts, having about 3 or 4 times its natural quantity of air compressed in it by the syphon BB, which for that purpose is screwed on at the bottom, within the side of the bason C. The stop-cock D being turned, and the syphon taken off, a small swan-neck pipe, E, is screwed on in its place, which fits into a brass socket, which is fixed in a cubical piece of wood F, right against the horizontal pipe G. From the same cubical piece F arises a naked barometer HH, whose cistern lies open to the passage, which leads from the swan-neck pipe to the horizontal tube aforesaid. Likewise from the same piece F, proceeds another pipe or tube I, parallel to the horizon, leading to another cubical piece of wood K, 3 feet distant from the former: out of which likewise arises another barometer LL, whose cistern is also open to the horizontal tube I, and by that means has a communication with the open cistern of the other. The parts thus disposed, and the stop-cock being turned, the condensed air proceeds strongly through the swan-neck pipe, which discharges it into the horizontal tube G, whose current so diminishes the pressure of the atmosphere on the cisterns of the respective barometers, as to cause the mercury to descend 2 inches at least. And it is observable, that that barometer, which is 3 feet distant from the current air, is equally affected, and subsides parallel with the other. It is also to be

* Sometime Curator of experiments to the R. S. and author of Physico-Mechanical Experiments, published 1709. He trod in the footsteps of Boyle; and by his experiments on electricity, he made some important additions to the then comparatively small stock of facts in that branch of natural philosophy.

noted, that as the current air is weakened in its force, the weight of the atmosphere again increases, and the mercury in the barometers gradually ascends.

An Account of some Eclipses of the Sun and Moon, observed at Cambridge, in New-England. By Mr. Thomas Brattle. N° 292, p. 1630.

On the 12th of June 1694, in the morning, I went to the College at Cambridge, about 4 miles from Boston, and observed an eclipse of the sun, with the brass quadrant there, having telescopic sights, the rays of the sun being transmitted through one of the said sights, on a clean paper, pasted on a plain piece of board, and fastened at right angles about a foot distant from the said sight; on which paper I had drawn a circle, between 2 and 3 inches diameter, equal to the sun's disk, and within that several concentric circles, dividing the diameter into 24 equal parts, by which I could observe to $\frac{1}{4}$ a digit. The room in which the observation was made, was darkened with blankets; and to render the observation the more exact, and to rectify the watch, I took the altitude of the sun with the quadrant.

The eclipse began at $9^h\ 14^m$, and continued increasing till $10^h\ 55^m$, when it was at the greatest, the quantity being rather more than $10\frac{1}{4}$ digits. After which it gradually decreased to the end, at 38^m afternoon.

The second is of a lunar eclipse, that happened Feb. 11, 1700, in the evening, as follows:

The moon rose eclipsed, and the horizon was so overcast, that I despaired of having any observation; but at $\frac{1}{2}$ an hour past 6, she came from under the cloud, and at $6^h\ 25^m$ I had just a sight of her, and judge her eclipsed about 5 digits. The eclipse ended at $7^h\ 43^m$.

The observation of the eclipse of the sun on the 27th of Nov. 1703, was as follows: The eclipse, which was a very small one, began at $10^h\ 6^m$, and ended at $10^h\ 44\frac{1}{2}^m$.

I judged when the eclipse was at the height, that the chord of the eclipsed part was nearest equal to the side of an inscribed decagon, or subtended about $\frac{1}{10}$ of the periphery of the sun's disk.

The last is an observation of the eclipse of the moon on Dec. 12, 1703, in the morning. The eclipse began at $11^h\ 45^m$; the total immersion was at $12^h\ 54\frac{1}{2}^m$; the moon began to emerge at $14^h\ 39^m$; and the eclipse ended at $15^h\ 45^m$.

On these eclipses Mr. Hodgson, in London, remarks as follows: I had the good fortune to make some few observations on that of Dec. 11, 1704. The heavens being cloudy most part of the night, it was 35_m after 4 in the morning following, before I could perceive that the moon was eclipsed; and then, as near as I could judge, she had been so about 3 or 4 minutes at most;

from whence we may conclude, the eclipse began at London about 31 or 32 minutes after 4 the same morning. Mr. Brattle found, that at 44 minutes after 11 at night, part of the moon's disk looked somewhat duskish, and that at 52 minutes, the shadow was well entered; so that from hence, as well as from a comparison of the ingress and egress of the principal spots, it probably began there about 49 minutes after 11; whence it follows, that Cambridge in New England lies $4^h\ 42\frac{1}{2}^m$, or 70° 37′ to the westward of the meridian of London.

I happened to see the moon the same morning at 35 minutes after 5, when she wanted at most but 3 minutes of being totally eclipsed; so that at London she immerged at 38 minutes past 5. Mr. Brattle saw her immerge exactly at 54 minutes after 12; whence it follows, that the difference of the meridians, found by comparing these observations, is $4^h\ 43\frac{1}{2}^m$, or 70° 52′, agreeing very well with the former; so that by taking a mean between them, the difference of longitude of the 2 places is 4^h 43, or 70°, 45′.

Observations of the Weather, made in a Voyage to China. An. Dom. 1700. *By Mr. James Cunningham, F.R.S.* N° 292, p. 1639.

This journal contains nothing but a seaman's ordinary account of winds and weather, latitudes and longitudes, with the barometer and thermometer, and the magnetic needles.

A Register of the Winds and Weather, with the Observations of the Mercurial Barometer, at Chusan, an Island in 30° 25′ *N. Latitude, on the Coast of China. By Mr. James Cunningham. F.R.S.* N° 292, p. 1648.

An Account of a Book, viz. Lexicon Technicum: or, An Universal English Dictionary of Arts and Sciences, explaining not only the Terms of Art, but the Arts themselves. In Folio. By J. Harris, M.A. and F.R.S.* N° 292, p. 1699.

The design of this Dictionary is to explain not only the terms which are

* We find no particulars relative to the life of Dr. Harris, the author of this first real Dictionary of the Arts and Sciences, as to the state in which they then were. Some years after the above, a second volume of the same was published, in a new alphabet, in the title of which he is stiled "late Secretary of the Royal Society." A third volume was afterwards added, but it is supposed a compilation by the booksellers. He seems to have been a patron of the mathematical Wm. Jones, Esq. the friend of Sir Isaac Newton, vice-president of the Royal Society, and father of the late Sir Wm. Jones, the celebrated Indian Judge: as Mr. Jones dedicated to the Rev. Mr. John Harris his Treatise on Navigation, published in 1702, in whose house Mr. Jones says he composed his said book.

used in every art and science, but likewise the arts and sciences themselves. The author has been very full and particular in all the parts of the mathematics. In Geometry, under the name of each figure, he demonstrates its essential properties, and shows its construction and use. Under such words as Parabola, Ellipsis, Hyperbola, the author is very large, and not only defines those figures, but demonstrates their most remarkable and primary properties; as likewise those of the Conchoid, Cycloid, Logarithmic Line, Cissoid, Quadratrix, and Spiral Lines; and is very full in the useful arts of trigonometry, both plane and spherical, with their uses and applications; in spherical geometry, or the art of projecting the sphere in plano, in surveying, dialling, &c. in all which he has not so strictly confined himself to other authors, but that the reader may meet with something that is new.

Under Algebra and the terms belonging it, such as equation, construction, &c. he has given a clear and distinct account of the nature of that science; giving all the rules, with their reasons and demonstrations, the resolution of affected equations, the constructions of cubics and biquadratics, with the investigation of Mr. Baker's central rule, &c.

Under the word Asymptote, he has several considerations concerning asymptotical curves; where he shows that curves which admit of no rectilineal asymptote, may yet be asymptomatical to one another, with several other considerations of the like nature. Nor has he been less full in what we call the new methods; under the word Fluxions, he has given the first principles of that science, viz. the nature and algorithm of them; and their use and application he has every where given under proper heads, and by these is shown a universal method of drawing tangents to all curves; of determining the points of inflexion and retrogression; of resolving questions de maximis et minimis; of finding the centres of gravity, oscillation, &c.

All the parts of Arithmetic are here explained, with its application to anatocism, compound interest, and Annuities, together with the doctrine of surds, the method of extracting roots by converging series, logarithms, and fractions, both vulgar, decimal, and sexagesimal.

He has likewise given the description and use of both the celestial and terestrial globe; the different hypotheses of astronomers, with an explication of the terms belonging to each; the parallaxes, magnitudes, motion, and distances of the planets; with several curious observations relating to the heavenly bodies; Mr. Newton's Theory of the Moon, with a large account of comets, from the same author; the nature and use of optics, catoptrics, dioptrics, with several methods for finding the foci of spherical glasses, the doctrine of the acceleration of heavy bodies; the composition of motion, geography, music, &c.

In Anatomy he has been very particular, giving a large account of all the parts of a human body, both internal and external, with the descriptions of the muscles and bones, in a fair plate, and under such general words as blood, circulation, heart, ear, eye, arteries, veins, bile, lympha, chylification, &c. he has always consulted the best authors.

In Architecture he has, in a plate, given an explanation of the five orders of pillars, with a full and clear account of the nature and rules of that art, which is to be met with under the several terms thereunto belonging. In Fortification, besides an explication of the whole art under proper terms, from the best authors in that kind, he has also a new plate with a description annexed, wherein all the parts of a fortified place are clearly seen at one view. He has described the several parts of a ship, both as in the dock when building, and when rigged and under sail at sea: in which he has been very accurate and particular; for besides the helps of the best dictionaries in that kind, as well as draughts, sections and models, he has often gone aboard himself, to get a more ready and sure knowledge in this matter.

He has given the laws of motion, both with respect to uniform and accelerated motions; he has determined the laws of the shock of bodies perfectly hard, and those that are elastic, &c. In Gunnery he has given demonstrative rules and methods for shooting in great guns and mortars, from the theorems of the learned Captain Halley, and from Mr. Anderson's tables. He has given us also the doctrine of mechanics and statics; has determined the laws of projectiles; and is very large in hydrostatics, giving an account of the nature and properties of fluids, determining them both experimentally and mathematically, and has annexed a very accurate table of the specific gravities of different bodies, from Mr. Boyle, Mr. Collins, and his own experiments.

The grand phænomena of nature he has explained from the best authors; as the law of gravitation from Mr. Newton; the theory of the tides from Captain Halley, on his principles, &c. And under such general terms as magnetism, light, colours, elasticity, solidity, divisibility, volatility, firmness, heat, cold, wind, &c. we have what is discovered concerning the nature and properties of those qualities from experiments and observation.

The phænomena of the rainbow or iris, he has accounted for, from the learned and ingenious Captain Halley. He has given an account of snow from Dr. Grew, and one of ice from the French.

As to sound, he has collected all he could meet with; but he hints, that that quality is not sufficiently understood, and wishes it were a little better considered. He is very full in his explication of the phænomena and properties of the air and atmosphere, as its gravity, spring or elasticity, &c. and likewise in his descriptions of the use of such instruments as have been invented, to enable us to judge of them; as the barometer, thermometer, hygrometer, &c. most of which is from the Hon. Mr. Boyle and the Philosophical Transactions. He has given an account of springs and fountains, from Capt. Halley and Dr. Woodward. In the art of Botany he has been very large, giving an account of the several kinds of subalternate species of plants, and their specific differences; in which he chiefly follows our excellent botanist Mr. Ray; but has consulted Mr. Tournefort, also Morison, and other writers on this subject.

He has explained the method of calculating of automata, or clock and watch-work, from Mr. Derham, as also the terms of art used in painting and sculpture. He has given a table of fossils from the learned Dr. Woodward; a scheme of metals and stones from Bishop Wilkins's real character; and a table of animals from Mr. Ray. He has also given, from Dr. Woodward, a very large account of vegetation, confirmed by accurate experiments and observations; from all which that matter is set in a better light than it has ever yet appeared in. In chemistry he has been very large and particular; explaining the chemical principles, vessels and degrees of fire; and has omitted no process or operation of use, that he could either meet with in books, procure from his friends, or had an opportunity of trying himself.

In Heraldry he has given the entire art of blazoning and marshalling a coat of arms; and explained all the ordinaries, charges, bearings, &c. by figures. In Logic, Metaphysics, Ethics, Grammar, Rhetoric, &c. he is designedly very short; giving usually the bare explication of the words and terms of those arts. In History and Chronology he has given what properly belongs to them as arts; as an account of the civil computation of time, the origin, and the reduction one to another, of the several æras, epochas, periods, &c. As to the Law, he has consulted the best authors and dictionaries in that kind he could meet with, and has from thence transcribed abridgely all that seemed necessary, and then had it examined and corrected by a person of known ability in that profession.

Of Cassini's Orbit of the Planets. By Dr. Da. Gregory. N° 293, p. 1704.
Translated from the Latin.

Since the time when the celebrated M. Cassini, in his treatise on the Origin and Progress of Astronomy, proposed to the astronomers a certain curve for the orbit of a planet, there have been many debates among the learned, concerning the nature of this curve, and the law of gravitation requisite for describing it. Having myself reconsidered this subject, the different species of this curve, with some of their properties, not before sufficiently examined, have occurred in my inquiries, as follows.

It is well known that the nature of the curve is this: If from two given points, F and G, fig. 1, pl. 6, to any point in the curve H, the two right lines FH, GH be drawn; the rectangle under FH and GH is equal to a given space. The right line FG produced both ways till it meet the curve, shows the two vertices A and B; and AB is the principal axis; also the middle point C between the vertices is the centre of the figure; and DE drawn through C perpendicular to AB, is the less axis; and the points F and G are the two foci.

In this figure, if the less axis be greater than the distance between the foci, the curve terminating the figure is every where concave towards the centre, such as the figure is commonly described. But if the distance of the foci be lessened, while the principal axis continues the same, the less axis will be increased, which yet remains less than the axis of an ellipsis, described with the same principal axis and the same foci; till at last, when the foci unite, it becomes equal to the greater axis, and the figure changes into a circle. But, on the contrary, if the distance of the foci increase, the less axis will be diminished, and will become equal to the said distance, when this is to the principal axis, as unity is to a mean proportional between 1 and 3.

If the distance of the foci be further increased, the less axis will be still diminished, and the curve at its extremities will no longer be concave towards the centre, but convex, as in fig. 2, till the distance of the foci be so far increased, as to be to the greater axis, as the side of a square is to its diagonal; then the less axis will become nothing, and the curve touch the centre on each side.

If the distance of the foci be greater than in the aforesaid ratio, the less axis becomes impossible, and the figure changes into two conjugate figures, as in fig. 3, which will be diminished as the distance of the foci increases, till at last the figures vanish in two conjugate points only.

The distance of the foci still increasing, the two conjugate figures emerge again, which increase in the same manner as they before decreased, being different from the former in the order of the foci and vertices, and so proceed increasing to infinity. And afterwards this system will again approach to the circle gradually as before it receded from it.

Hence it appears at first view, that this figure cannot at all be proper to constitute the orbit of a planet. For, not to mention the case in which it becomes two figures, and forsakes the nature of an orbit, viz. whenever its eccentricity is so great as the comets require (if they move round the sun like planets, as is most probable) to describe their respective courses: to pass over these cases I say, yet even in those cases in which it returns into itself, and completes its orbit, some of its eccentricities are so great, that about D and E (fig. 2) the curve becomes convex towards the sun; and therefore the planet would require a centrifugal force from the sun, in order to describe this part of its orbit; while at the same time both in places that are nearer to and farther from A and B, there ought to be a centripetal force towards the sun. That is, it must be granted, that the circumsolar bodies must move by such a law, that at equal distances from the sun, in the one a centripetal, and in the other a centrifugal force must take place; which it is easy to perceive is very different from the known laws of nature. And though none of the planets has so great an eccentricity; yet since it is known to geometricians, that if all the species of a figure, beyond a certain limit, are unfit for performing a natural effect, then the remaining species of that figure within the limit cannot be admitted as proper to perform the same effect; it therefore follows, that this curve of Cassini's, must necessarily be rejected out of astronomy; not only for the reasons alleged in Prop. 8. lib. 3, of the Elements of Astronomy, viz. that it neither agrees with celestial observations because of the shortness of the less axis, nor do physical reasons correspond with it, since for the description of this there is required a centripetal force towards the sun, very different from that which nature employs; but also because of its utterly impossibility. For it is impossible that any species of this figure can be described by a planet, so that the angles at a focus different from the sun may be proportional to the times; for thus the area described by the radius vector could not be proportional to the times. For the angle at one focus being increased by equal increments, the cotemporary increments of the area at the other cannot become equal; as of late I had hastily imagined.

In the last two figures (2 and 3) the greatest breadth of the figure is found, if with the centre C a circle be described through the foci; for this will cut the curve in L, L, the points required. And the greatest ordinate KL is a third proportional to the right lines GF and FD in the first of these, or a fourth proportional to GF, GA, AF in both of them.

DE remaining, the ordinate FP from the focus, is equal to the less semiaxis CD, when the less axis is to the distance of the foci, as the side of a square to its diagonal. If the distance of the foci be greater than in this ratio, then FP will exceed CD.

R. P. Georg. Jos. Camelli Tractatus de Plantis Philippensibus scandentibus, ad Jacobum Petiver, S. R. S. missus. N° 293, p. 1707.

An enumeration of certain climbing plants which grow in the Philippine Islands. The descriptions are too incomplete to answer any botanical purposes.

Concerning the Flesh of Whales, and the Crystalline humour of their Eye. By Mr. Leuwenhoeck. N° 293, p. 1723.

I have often concluded that the globules of blood, which are the cause of its redness, are of the same magnitude both in large and small animals, and consequently that the particles of blood in a whale are no larger than in the smallest fish. But not being able to procure any whale's blood, I was obliged to acquiesce in bare speculations. From the particles of blood I was led to consider those of the flesh of a whale, imagining that the flesh particles of that fish were no larger than those of a horse or cow, and that the bulk consisted only in the number of its particles. Having cut across a piece of whale's flesh, in order to separate the particles lengthwise; I could perceive no difference in size between them and those of a large ox.

On examining the crystalline humour of the eye, it appeared to me, that the fibrous matter it is composed of, is of thinner particles than that of a young perch. The reason why this appears to be the case is, that we see the former on one side, where the threads or fibres are thickest; but if we view them where the fibrous particles meet together, they are exceedingly fine and slender; whereas, on the contrary, the fibrous matter of the whale not meeting together, but often crossing, is of the same thickness throughout.

I have discovered that the crystalline humour in a whale is of a quite different texture, or circumvolution, from the fibrous matter in many other animals; for as in others it consists of three particular conjoined branches, all arising out of one point, the same humour in the eye of a whale is composed of five circumvolutions, all which unite together in one point, and make one scale.

And here again the amazing order and wisdom of the Creator is obvious; for what man could wind about a globe or ball with a single thread of equal size throughout, and not cross itself in any point; and yet such is the contexture of the filaments or fibrous matter, of which the scales of the crystalline humour of a whale are composed.

Having carefully observed the eyes of several fishes,* and particularly the tunica cornea, I found that the said tunic, or rather the pupil or apple of the

* On the crystalline humour of the eye of fishes, see vol. iii. p. 91, of this Abridgment.

eye was very flat, like those in the human eye, and other animals; whence I concluded, that though the crystalline humour in fishes was spherical, yet the same was made good by the flatness of the apple of the eye in the same fishes; whence one might probably conclude, that the eyes of fishes are of the same contexture with other land-creatures, and consequently the effects in both are the same.

Having taken the eye out of the head of a living cod-fish, and put the tunica cornea into several copper globes, or internal circles, it appeared that the protuberant roundness of the said tunic was equal to the segment of a circle, whose diameter was 2 inches. The said eye was a little prominent out of the head, like those of other animals, and though the tunics or apples make a larger circle, yet they are not larger, and the axis of the crystalline humour was a little longer than half an inch. Now if the crystalline humour (which I have sometimes called the crystalline muscle) in our eyes, and in many other animals, consists of a flattish roundness, and not perfectly spherical; and if the diameter of the circle made by the tunic of the said eyes be an inch long, the crystalline humour in fishes being spherical, and their tunic describing a circle, whose diameter is 2 inches, all these eyes, as before said, may have the same effect.

After this I took a whiting, which weighed about 9 ounces, and examining its eye, found it described a circle of $1\frac{1}{2}$ inch in diameter, and the diameter of its crystalline humour was very nearly $\frac{1}{3}$ of an inch. When I dissected the crystalline humour of a small fish, and found the innermost part no larger than a large grain of sand, I observed that the fibrous particles, of which those exceedingly small scales were composed, consisted of as many parts as the uppermost scales of the same humour.

Being once asked why nature has given us eye-lids, seeing that fishes have none; I answered, that it was absolutely necessary for us, and all land-animals to have eye-lids; for if it were not so, and that the apple of our eyes was not often moistened in the space of an hour, and all the foulness that might fall on it washed away, our sight, or the tunica cornea, would be so clogged with filth, that we should not be able to use our eyes; besides, the tunic would otherwise be parched up or shrunk with heat, and consequently we should become blind; whereas, on the contrary, fishes living always in water, they want no eye-lids, because the same water keeps their eyes always moist and clean.

But I have since found that I was not entirely correct, for flounders, plaice, soles, and I believe all flat fishes, can cover their eyes; and if they could not, I fancy they would lose their sight, because that sort of fish is not so nimble as others in swimming, being only able to move their tails, the chief instruments of speed, upwards and downwards; wherefore these fishes in a storm do not betake themselves to the bottom of the sea, as I am informed, but dig themselves holes in the sand, which secures them from being cast upon the beach or strand: now if they had not eye-lids, the sharp points of sand, while they are making their nest, would wound the tunics of their eyes, by which their transparency would be destroyed, and the fishes become blind: which is a farther proof how perfect every creature is in its own species.

Concerning the Tubes or Canals that convey the yellow Sap in Aloes-leaves, &c. By Mr. Leuwenhoeck. N°. 293, p. 1730.

I observed the external skin or membrane of the leaves of aloes, to discover, if possible, of what tubes or pipes they were composed; but was not able to find out the conjunction of the parts; because that membrane was so weak and tender, that it always broke without any remarkable discovery. I observed that in the said membrane there lay as it were pressed in, roundish particles that looked like little bladders, and in those little bladders, green particles, that had a sap in them; and they lay as it were in a right line, and so interwoven with each other, that they seemed to serve for tubes or canals. The impressions of these round particles were in several places so regular, that each consisted of six sides, disposed in the exactest order imaginable; and in each particle might be discovered a protuberance; and they were separated from each other by rings or circles, which I supposed to be the canals.

I caused a small part of the forementioned hexangular particles to be drawn, just as it appeared through the microscope; as pl. 6, fig. 4, where some of the canals, in order to distinguish them the better, are represented outwards, as at ABCD.

On further examining into the aloes leaf, I discovered another sort of canals or vessels, in which the sap appeared somewhat reddish. Taking this sap out of the said vessels, to try whether it contained any salt particles, and what figure they were of; I let it stand a little, that some part of it might evaporate, and the salts coagulate; then placing some of it before the microscope, I observed a great number of long, slender particles, that lay in the sap, and were sharp or pointed at both ends; and as I imagined that these long particles could not be coagulated in so short a time as the sap was pressed out of the canals, but rather that they were there before, I endeavoured to bring them out of the vessels, so that there should come very little sap with them, that they might be seen and drawn more distinctly; a few of which (out of several hundreds that in a short time I had heaped together) are represented in fig. 5, EFGH.

These slender particles appeared through the microscope very clear and transparent; I laid them upon a clean glass plate, and viewing them about 3 weeks after, was surprised to see that some of them had assumed a peach colour, and especially such as lay close upon each other. After several observations, by breaking and cutting in pieces the aloes leaves, I not only placed them so nicely before the glasses, that they could be seen lying in those rings or circles, which I imagined to serve for canals, as in fig. 6, IKLM, in the middle of five rows between I and K; but also separated a small piece of leaf in such a manner, that the ends or points of those sharp particles stretched themselves beyond the canals, as is here represented in N and O. These sharp particles lay within a small compass, like little bundles, as is shown in the figure; but they do not all lie in such order, nor always so close to each other, but at a greater distance in several places, and sometimes indeed closer. It is to be observed, that fig. 6, IKLM, as it appears to the naked eye, did not cover more space than a large grain of sand; from whence we may guess what a vast number of such sharp particles are included in one aloes leaf; in each of these particles in this figure; are a great many parts to be seen which seem to be canals; and I suppose that each circle, or the membranes in which the sap and other parts are shut up, are also full canals.

Next proceeding to the vessels or canals, which lie something deeper in the leaf; here, fig. 7, ABCDEFGHI, represents one of these, which is in a manner surrounded and involved in a sort of viscous or slimy matter, and which I endeavoured to separate from it; from these vessels or canals, in four distinct places, and all in a very little space, there proceeded a kind of branches; as two between DE and FG, and two between H and I, which run across from the leaf. These twigs or branches unite again in one vein or canal that lies just by them, which is not near so large as the first mentioned canal; and this union or conjunction is represented by KLMNOPQR, and there are three such small canals to a large one. The said vein or canal that runs across, is not only joined to a second ascending vein, which extends the length of the leaf, but it goes still farther, and falls into a third canal, which also runs the length of the said leaf, as may be seen in the same figure by STVWXYZa; and perhaps this order runs through the whole leaf.

I fancied that some of these veins or canals were composed of long particles, that extended parallel and very near to each other, but in a winding and serpentine manner; to the end that the said veins should never fall in with each other, but always remain open and distinct; and agreeably to this opinion, I have sometimes, but very seldom, taken those parts out of the canals. In the said fig. 7, WBCDEFX represent the last mentioned particle, which composes a canal, but of a very small length, and it seems to be of a flattish shape, as between W and B; but viewing it more narrowly, we found that the seeming flatness was occasioned by the canals lying so close to each other; for we saw clearly that B was two distinct canals, when they were stretched out, as in BCF; and that one of those canals was again subdivided into two more, as from C to D; and perhaps these slender particles D and E were still further subdivided.

After this, I took that particle of the aloes-leaf which I had before examined lengthwise, and cut it across, to view it in that position, that I might discover the orifices or openings of those canals which in fig. 7, are represented in their whole length by ABCD. In fig. 8, HIKL are represented (as they appeared through a microscope that did not enlarge the object so much as that through which I viewed fig. 4 and 5) six large tubes, which show themselves in the last mentioned vessel or canal, in which also there are a great many other less tubes. These small vessels, which are to be seen about H, have other cross canals that issue from them, from whence proceed the large internal membranes, with the sap in them.

Fig. 9, MNO represent other canals, that were also cut across; and by PQR in fig. 10 is also shown, how out of the canals so divided, the membranes proceed, in which most of the sap is included, and of which the greatest part of the aloes-leaf is composed; and these membranes, with the sap that lie in them, are so clear and transparent, that you can see nothing but their circles or circumferences, as in RQSTVWXP. Between these membranes there are canals though but very few, with their branches, that run the length of the leaf, from whence also the membranes proceed; and when I took one of these vessels surrounded with membranes, and the matter shut up in them, and separated a little of it from those other fore-mentioned parts, so that it was exposed naked to the air, it assumed presently after a red colour: whereas, those parts that were involved in their membranes, retained their colour, which was a little greenish. This change, from green to red, reminds me of what I have observed several times in the eggs of a living crab, which I have taken out of her, and breaking them in pieces, viewed them, and perceived that the matter which lay in the egg-shells was green; but when I let it dry a little, it presently turned from green to red.

I cut across the large canals of another aloes-leaf, which appeared yellow, as did also the sap which came out of the internal membranes; and when I cut a slip or slice of such a leaf, which was as thick as the back of a knife, and laid it upon a clean glass plate, it turned presently to a peach colour; and so did a small drop of the sap upon the same glass; which being almost dried up, there appeared as many salt particles in that little matter, as if the fourth part of it had been mixed with shining sand.

I took a little of the said sap, and put it upon my thumb-nail, letting it dry there, and observed that it left a yellowish colour behind it; that the particles of salt had coagulated on my nail after the same manner as a foggy moisture is congealed in winter on glass-windows; and the next day I perceived a reddish colour where the sap had lain; and where it had been thickest, the red was deeper, which we call a peach colour. I tried the same experiment upon two other nails of my hand, and the success was the same, and the colour lasted several days.

I cut a small splinter of the wood, in which there had been some of the aloes' sap, but in which at that time there was but little colour to be seen; and placing it before a microscope, I saw that it gradually assumed a peach colour, which in some places was as bright and as fine as I ever saw. Cutting off a slice of the aloes-leaf from the thickest part of it, which appeared as ABC, fig. 11, which letters represent the side of the leaf, that, as I imagined, was next the plant; like as CDA the other side, which one would take to be the back of the said leaf. That part of the leaf which may be considered its skin or rind, and in which the parts represented in fig. 4, 5, 6, and 7, are for the most part shut up, is the space which is represented in fig. 11, between BF, or DH; and between EFGH lies the forementioned slimy or viscous substance; in which I could not discover any vessels that run through the middle of it, like those which proceeded out of the large canals, and spread themselves to the innermost parts of the leaf, and were exceedingly small and numerous.

I placed another slice of the aloes-leaf (which was also about the thickness of the back of a knife) upon a clean glass, and viewed it several times, for the sake of the fine peach colours that were to be seen in it; and I observed in it a kind of oval figure, that lay in exact order, with its sap shut up in it, after it had been dry about 3 weeks. Fig. 12, DEFGH shows the said oval membrane; and FIG that part which I call the canal, and by which I suppose it receives its increase and inward matter. In this figure are seen a great many fibres, which I concluded it had borrowed from other membranes, as well from those that lay upon it, as from those that lay under it. Now if we consider that these membranes cannot be coagulated by the air, since they are not formed by every evaporation or exhalation of the moisture; but that such membranes must doubtless be composed of an infinite number of fibres, so small as to escape our sight, we have fresh cause to be surprised, at the inconceivable wonders shut up in such a leaf.

Fig. 10, RPABCSQR shows a small part of the rind or bark of the leaf, as it was cut off across between D and H, in fig. 11, and dried up irregularly; which part, with SQRPA, lay inwards, as is represented by H in fig. 11; and in fig. 10 lay on the outside of the leaf, as is shown by BC. Now these parts, which in fig. 10 are cut across between AB and CS, are the very same parts which in fig. 6, IKLM, are represented lengthwise, with this difference only, that fig. 10 is drawn from a less microscope.

Having squeezed the sap out of the aloes-leaf, in order to discover what salt particles would coagulate in the same; I placed it on my escritoire, and always observed, that when the water was mostly evaporated, there remained abundance of salt particles behind, which were almost all of them quadrangular figures, and of a cubical thickness, as is represented at IK, fig. 13; and again some smaller at LM, fig. 14; and others much smaller, as in fig. 15, 16; nay, some of them were so exceedingly small, that they almost escaped the sight, even through the microscope; insomuch that one could not judge whether those salts were round or square.

I observed also some figures of salt that were composed of several coagulated after such a manner, that their shape could not be described; and among others, there was one salt particle that was composed of four others, joined together cubically, as represented at NO, fig. 17. I saw another salt likewise, that was made up of 20 quadrilateral salt particles coagulated together, viz. 5 in length and 4 in breadth; and another salt, in which I counted 30 particles joined after the same manner. When I took the sap out of an aloes-leaf, whose vessels contained a yellow sap, I saw the salt particles lying so clear, as if they were little pieces of glass; and as they were surrounded with a fine peach colour, it was a very agreeable sight.

From my observations on the salts, I was led to consider the aloes itself, as it is used in the apothecary's shops. Having taken a piece of aloes, and beaten it small, I wrapped it up in a paper, and put it into a clean glass, pouring rain-water upon it, in which I let it infuse 2 or 3 days; then pouring off the top of it, I caused it to evaporate, and then discovered a great number of salt particles, of the same shape and proportion as those represented in fig. 14, 15, 16, and 17. There were also coagulated a great many salt particles, of the figure represented at EFGH, fig. 5, only with this difference, that they were something shorter and thicker in the middle, and some of them not so even or smooth as in the said figure.

I had sent me also a small plant of dragon's blood; in which I viewed the stalk of the leaf after I had cut it across, and discovered at the same time several particular colours of a light and of a deeper red; and I observed in the said stalk little places in which I could perceive no colour; but on cutting the same stalk lengthwise, I could then see that those places were canals, through which

I concluded the red sap passed, and that those many colours, which lay in those canals, were a sort of bags, that contained the sap; and that the several colours were wholly produced by the sap that oozed through the sides of the canals, and so made the whole stalk red.

Concerning Tobacco Ashes. By Mr. Antony Van Leuwenhoeck, F. R. S. N° 293, p. 1740.

The alkaline salt obtained from tobacco by incineration, is the same with that extracted from the ashes of other vegetables which do not grow on the sea-shore or under the sea; and as the figures of these saline particles (as extracted from other vegetables) have been already described in some of Mr. Leuwenhoeck's former papers, the reprinting of this letter is deemed wholly unnecessary.

Cuculus Lævis cæruleo flavescens, cui in supremo Capite Bronchiarum Opercula; or, the Yellow Gurnard. By Edward Tyson, M. D. F. R. S.* N° 293, p. 1749.

If we may justly infer an identity of species in fishes from the likeness of their fins, we have then some ground to conclude, that this fish ought to be referred to the gurnard kind. Not but that in many remarkable particulars it differed from it. However, not finding any other species it agreed with better, and the fishmonger who sent it to the Royal Society not knowing its usual name at Hastings, where it was taken, I shall call it the yellow gurnard, and in support of that appellation shall compare this fish with the red gurnard, and show wherein they agree or differ.

In the general shape of their bodies I found sufficient similarity. In both, the head was the largest part, the body thence gradually still lessening and growing taperer as it approached the tail, where it was very small in both. The yellow gurnard measured 11 inches in length, whereof the tail was 2; the girth of the head was 4½ inches.

The fins as to number and situation were the same in both, I shall therefore omit their description, and only notice wherein they differed in other circumstances. In the fore fin on the back of the yellow gurnard there were four or five radii or spines, of which the first was 6 inches long, the next about 2, the others shorter; but in the same fin of the red gurnard there were six strong bony spines, sharp pointed; whereof the second from the head, which was the longest, was only a little more than an inch, and the rest not much shorter. Note, my red gurnard was a small one, and rather less than the yellow one.

* Callionymus Lyra. Linn.

In the hinder fin of the back of the yellow gurnard there were nine radii, in the red gurnard fourteen; in both, the radii near the tail were the longest; those in the yellow gurnard being $2\frac{1}{2}$ inches long. The membrane that joined these radii, as to colour, differed much in these fishes. For, in both the back fins of the red gurnard, this membrane was all of a white transparent colour; in the fore fin of the yellow gurnard it was yellowish with blue spots, edged with black, and the membrane of the hinder fin was of a faint bluish colour, with four yellow streaks, about a line broad, running the whole length.

The pinnæ bronchiales (whereof there were two on each side, and their situation in both the same, the uppermost being inserted perpendicular, the lowermost horizontal to the body) differed likewise in colour. For in the yellow gurnard the upper fins were white, the lower of a blackish colour, with several beautiful long spots of an azure blue. In the red gurnard the upper fins were of a dark reddish colour, the lower ones white; but here between these two fins I observed three naked cartilaginous radii, which are not in the yellow gurnard, and are well expressed in Salvianus's figure of the red gurnard.

The fin on the belly was placed exactly alike on both. The yellow gurnard here had nine radii, and its membrane of a darkish blue colour. The red gurnard had seventeen or eighteen radii here, and its membrane transparent white. The tail in both was much the same. Over the anus in the yellow gurnard was a slender pendulous body, which was not observed in the red gurnard.

The colour of the body of these two fishes also differed very much; and I know not but that it may be a property in this species to vary in colours, more than other fishes do. The belly of the red gurnard was of a silver colour, as also some part of the sides near the belly; the rest, and the back and the head, were of a reddish colour; in the head there were some small whitish spots; the belly of the yellow gurnard was white, but under the lower jaw was black; the sides and back were yellowish, but between the belly and sides there ran a blue streak or list, about a line and a half broad, from the head to the tail; and a little higher on the sides there was a chain of blue spots the length of the fish; for on the sides of the head I observed these blue spots, only, from the eyes to the end of the rostrum, the spots were of a deep yellow colour. There being therefore so much of blue and yellow over the greater part of the body of this fish, I have given it the epithet of cæruleo flavescens; for, where the ground is blue the spots are yellow, and where yellow the spots are blue.

Though hitherto there seems a tolerable agreement between these two fishes, yet in the remarks I shall now add, the disagreement will appear greater. For the yellow gurnard was without scales, I therefore call it lævis. The red gurnard had not only scales on the back, but likewise a ridge of spiny scales all along the sides, as also on each side of the back fins were placed the like spiny ridges or scales. But the belly seemed almost smooth, and had but few scales, and those very fine; and indeed those on the back were much smaller than those in most other fishes. If Mr. Leuwenhoeck's observations be true, that even the anguillous kind are scaly, then the difference will not be so great, the one having membranulous scales, the other bony. Or it may be our subject is an intermediate species between the gurnard kind and some other.

And this I am the more apt to believe, because, though it has gills on each side, yet it had not those apertures at the sides of the head that the red gurnard had, and is common to most fishes, except the cetaceous kind; but, like them, the yellow gurnard had two large apertures on the hinder part of the head, an inch beyond the eyes, at which it spouts out the water. By blowing into these holes I extended the cavities where the gills lay, and observed that over these cavities were placed a flat bone, which by the contraction of its muscles might serve to force the water out, and perhaps is assisted in this action by another loose bone that lies over it, whose edges are jagged or indented. At which place in the red gurnard I observed a strong sharp spine.

These foramina in the head of this fish is a thing so very remarkable, that it may be considered as a characteristic; nor do I know at present what other fish to parallel it with; for the cetaceous kind that have spouts in their heads, have not bronchiæ, but lungs. The better therefore to distinguish this fish, I have added this particular to its name.

But to conclude, I observed the eyes in the yellow gurnard were placed more on the top of the head, and the skin here covered almost half of them, like an eye-lid, which I did not observe in the red gurnard, whose eyes were placed more at the sides of the head. The head likewise of the red gurnard was more protuberant, in the yellow flatter. The end of the rostrum, also the teeth and tongue in both, were exactly alike, only in the palate of the yellow gurnard I observed two cartilaginous bones, whose edges were bent downwards from the palate, and served, I suppose, for the hooking in and staying the cartilage of the tongue, when it makes a compression for the forcing out the water by the foramina of the head; which contrivance I did not find in the red gurnard, not having the like occasion for them. This fish being stale, I had not an opportunity of dissecting it, and observing the viscera; and shall only further add, that the gills had four osseous radii on each side.

A Question proposed for Solution to Medical Men. By Wm. Cockburn, M. D. F. R. S. N° 293, p. 1753.

The question here proposed is, what is the rule or method to be followed in apportioning the doses of medicines, and especially of vomits and purges, to the ages and constitutions of different patients?

Concerning Cobalt, and the Preparations of Smalt and Arsenic. By Dr. David Krieg, F. R. S. N° 293, p. 1753.

The cadmia, or cobalt, is a massy, heavy, grey shining stone, found in great quantities in the mines about Shneeberg, and some other places of Hermanduria. It is often mixed with marcasite, sometimes with silver and copper ore, in the figure of hair. After the cobalt is picked out, and separated from the common stone, it is beaten to powder by an engine or machine, commonly used in mines, called a poolwork. By that operation the water carries away the light stuff and sand, leaving the heaviest behind.

This powder is afterwards put into a low and broad furnace, made on purpose to separate the sulphur and arsenic, where the powder is spread all over, and the fire, which is beneath and behind it, is forced to pass its flame along over the powder, and so carry with it the arsenic in form of a smoke, which is afterwards received by a low chimney, and out of that carried into a close channel made of brickwork, of about 50 or more paces, where the arsenic by the way sticks to the wall, in form of a yellowish white powder, which is taken out every 6 months, and melted into whole pieces. The cobalt being thus roasted, and smoking little more, is taken out when red-hot, cooled again, and gathered for melting. Its colour, by that way of roasting, is turned a little more whitish.

When they intend to melt it, the powder of the cobalt is mixed with pot-ashes and powder of white flint-stones; the proportion of which is according to the goodness of the cobalt, or as the smalt is to be made of a deeper or paler colour; for instance, they take one part of pot-ashes, two parts of cobalt, and three or four parts of flint. This mixture is put into large strong pots, standing in a hot furnace, six or eight pots in one furnace, where it stands melting for five or six hours, changing into a blue glass, which is afterwards taken out with a large iron ladle, and put into a vessel full of cold water, where it cracks and becomes more tender, to be more easily powdered again; but the empty pot in the furnace is filled up again with the aforesaid mixture. And thus they continue night and day, still maintaining the fire in the furnace. The blue glass taken out of the water is powdered again by the common engine; the finest part being separated by a sieve, is put into a mill, and ground in water to the finest powder, which by washing is further separated from the coarser parts. The same is afterwards dried in small warm chambers, then put into barrels, and thus sent away to several countries.

If one of the melting pots happen to break, or to be very much burnt, so that it must be taken out, they always find on the bottom two cakes of different matter, not mixed with each other. The undermost is a sort of æs caldarium, or gleiken spisse, and the uppermost a marcasite. The grass and fruits growing about the place where such a work-house stands, are commonly poisoned by the arsenical smoke, so that neither cattle nor men can feed upon them with safety.

Plate 6, fig. 18, 19, the furnace where the cobalt is roasted, and the arsenic separated; a the furnace to roast the powdered cobalt; b the chimney receiving the arsenical smoke; ccc the channel of stones to collect the arsenic.

Fig. 20, the furnace for melting the cobalt into a glass; aaaa the holes where the melting pots stand. The large holes where the pots are put in are shut up with brick, and a small one, bbbb, left open, where the glass is taken out with the iron ladle.

Fig. 21, represents two grinding-stones to grind in water.

Observations of the spots that appeared on the Body of the Sun in June and July, 1703. By Capt. Stannyan. N° 294, p. 1756.

On Saturday, May 15, 1703, at sun-set, there appeared two suns, the mock sun above the real one, which was then only 5 degrees above the horizon. I took a good 7-foot telescope, with a small aperture, and soon discovered a solar spot near the sun's centre, which I designed to observe more exactly the day following, but it proved cloudy. Monday, May the 17th, at 6 o'clock in the morning, the spot was advanced considerably towards the sun's western limb; it seemed of a strong consistence, very compact, resembling a face, and was distant by noon from the anterior limb of the sun's disk 61 seconds of time. May 18, at noon, the spot was 46 seconds of time distant from the preceding limb. May 19, the solar spot was within 33 seconds of time of his western limb. May 20, at noon, the spot was arrived within 21 seconds of time of the preceding limb, moving nearly in a straight line, and intersecting the parallel of declination passing through the sun's centre. May 21, we had no sun-shine. May 22, at 7 o'clock in the morning, the solar spot was advanced very near the limb of the sun's disk. May 23, at 6 in the morning, the spot was got to the very edge of the sun's disk, resembling a barley corn, lean and slender, and of a duskish colour, wanting only its own shortest diameter of the sun's limb. At 8 o'clock I observed it again, also at 10 and at 12. At 2 it was slid into the very circumference, and hardly visible, had I not kept an eye upon it all the day long; about 3 in the afternoon it totally disappeared.

On Monday, June the 7th, 1703, at 3 o'clock in the afternoon, I discovered the same spot, as I supposed, which I had seen go off the sun's disk on May the 23d, re-entering the sun's face just at the time and place that I expected it. At 4 o'clock the spot appeared distinct, but slender, with an eliptical speckly mist about it, and five or six light coloured streaks. June 8, at 6 in the morning, the spot was very visible, and traced again its former path, coming in exactly where expected; it kept its shape, but those lemon coloured streaks disappeared, though itself and the mist about it grew bolder and broader visibly, as it re-entered the sun's disk. June 9, at 5 this evening, the spot had not altered its shape, but advanced gradually over the sun's disk, as it had formerly done. June 10, at noon, the sun shining very bright, I had an opportunity of being assured it was the same spot; I plainly saw it move over its former path, and it was then distant from its nearest limb 29 seconds of time. At 5 in the evening its shape was altered, appearing larger and blacker. June 11 was a bad day for observation, but I had a sight of it: it continued black and bold as before. June 12, at 7 in the morning, the sun's body being very clear, I saw the spot through the 18-foot glass, retaining its former shape. June 13, by this day noon the spot was arrived at the same point of the sun's disk that I found it in on Monday at noon, May the 17th; which makes me inclinable to believe it was the very same spot. June 14, the distance of the spot from the next limb of the sun's disk was 45 seconds of time from the anterior edge of the sun's body; and on Tuesday, May the 18th, it was observed to be in the very same place of its path, within a single second of time. At 4 it had altered its shape, and it was distant from the preceding limb 612 such parts as the sun's semidiameter is 900. June 15, at noon, the solar spot was distant 32 seconds of time from the leading limb of the sun's disk, and covered the very place where the same spot had been observed on Wednesday the 19th of May. June 16, no sun-shine. June 17, no sun-shine. June 18, at noon I observed the solar spot very slender, but black and bold to appearance, the mistiness about it on the right hand perceivable, and that on the left grown slender, in proportion with the spot itself, and found it distant 5 seconds of time. June 19, all this morning, it being clear weather, I saw the spot distinctly; at 12 I perceived that all the cloud, or misty matter, that used to surround the spot, was invisible, and the spot itself reduced to little or no breadth, in comparison to what it had been towards the sun's centre, and so close to the limb of the disk, that I could only perceive a small streak of the sun's light between it and the limb of the sun's body; at 2 o'clock I could just perceive it, being extremely slender.

The first revolution I saw the spot half in the circumference of the sun's limb at 2 o'clock on Sunday, May the 23d; and the second revolution I just perceived it at half an hour after 2 o'clock on Saturday the 19th of June.

On Sunday, June 27, about 6 in the evening, I observed several spots in the sun's disk, but had not the conveniency to use my longest telescope, because of some trees that were in my way to westward, so that I made no observation till the Tuesday following. June 29, about 7 in the morning, I counted sixteen remarkable spots in the sun's body, in several parts; the foremost centre of six was distant from the sun's anterior limb 81 seconds of time, and the last cluster 87. June 30, at 8 this morning the solar spots had greatly increased in number, and strangely changed their places. The cluster of seven spots seemed to me to move gradually, as the single spot did in May, but the cluster of four went too fast forward, the twelve spots without a mist about them straggled all manner of ways, and the nine spots and the five black small ones went backward, and unbent itself, at the same time, as it were into a straight line. I am apt to believe it went backward, as that the other went too fast, or faster than ordinary forward, for in 24 hours the foremost cluster advanced 21 seconds of time, which is more by 6 seconds than ever the single spot moved in that time, even when nearest the sun's centre, and the distance in time between the first and the last cluster this day was greater by 3 seconds than the day before. The foremost cluster of four spots was distant from the advancing limb of the sun 60 seconds of time. At half past 4, the advancing cluster passed the intersection in 55 seconds of time, after the sun's foremost limb had passed conformably to the spot's path; and the last spot passed in 63 seconds of time, the last limb passing the intersection, according to the path of the spot, in 126 seconds of time, the sun's largest diameter passing in 136 seconds: the spots by this time appeared quite black, and of very odd shapes. July 1, at 8 in the morning, the leading and largest spot was distant from the anterior limb 44 seconds of time, the last cluster lying a little oblique, passed in 53 seconds. July 2 and 3, no sun-shine. July 4, this morning at 8 o'clock, the leading spot was distant from the advancing limb 10 seconds of time, the spots and clusters retaining nearly the same shape, but beginning to contract themselves. July 5, at 7 o'clock, I found the spots had quite altered their shape, appearing dull and slender. July 6, at 10 o'clock the sun's disk was found clear of all spots. July 17, about 4 o'clock in the afternoon, I observed some spots in the sun's body, resembling those I saw the 3d of June, only with this difference, that these appeared as if they had been heated red-hot; they seemed to be in the same part of the sun's disk. I observed them above an hour together that day, but could never afterwards see them, nor discover whether they were coming in, or going off his visible disk. I continued to observe the sun as often as was possible, with my 18-foot glass, till the end of the month, but without further success.

Georg. Joseph. Camelli De Plantis Philippensibus scandentibus; Pars 2da. Ad Jacobum Petiver, S. R. S. nuper transmissa. N° 294, p. 1763.

A continuation of the catalogue of plantæ scandentes (climbing plants) growing in the Philippine isles.

Concerning some Fossils of Switzerland, &c. By Mr. Leuwenhoeck, F.R.S. N° 294, p. 1774.

Having imparted my thoughts and observations to M. Valkenier, about mountain-crystal, and particularly that it was not produced from ice, nor grew in the mountains, where it has its increase, as many are of opinion; but that it is coagulated of the air, where it is fixed in the mountain; that air and a little moisture is shut up in crystals; the parts of which being separated, are again resolved into long particles, of which a great many are hexangular, that gentleman gave me four small snail-shells, which he had gathered in the mountains, where through length of time they were changed into metal, for they were uncommonly heavy. For further satisfaction, I took one of the snail-shells, that I judged to be metal, and beat it to pieces with a hammer; but finding that it broke very easily, I separated the parts with the less trouble, and broke the fore-part with my fingers, a small part of which, represented by fig. 22, pl. 6, I placed before a microscope, of which BCD was what was joined to the fore-part of the snail-shell, and in which I observed 6 prominent particles, and 5 others at AE, which were fitted or insinuated into the 6 particles, and as often as I broke the shell in pieces, I still observed in the fore-part 6, and in the hind-part 5 particles, in the same order and disposition; so that I fancy it must have been a limb or part of the inclosed snail, and consequently that it had been once a living animal.

Having placed the broken part of one of these little pieces of snail-shell before the microscope, I observed that the internal matter was composed of very small shining particles, which one would take to be broken pieces of metal. As I have seen particles of metal apparently similar, but which were really nothing but brimstone, I took some of the small pieces of the shell, and put them into a glass, and then upon the fire, when I observed, that in proportion to the smallness of the parts, a great deal of water and sulphur was extracted from them, without discovering the least sign of metal in the remaining black matter.

I examined also that small piece of matter, which I judged to have been the living animal within the shell; by its uncommon weight it seemed to be also turned into metal, and the more when I had broken it to pieces, for then the manifold small shining particles appeared to be wholly metalline. The said matter was very hard, insomuch that when I struck it against a steel, it yielded sparks of fire; I broke off a piece of it, and placing the remainder before a microscope, it appeared as in fig. 23; the break of which is represented by GH. On which side, the more I viewed it, the more I was persuaded that the many small shining particles were certainly metalline; but when I brought it to the fire, and endeavoured as much as I could to keep the smoke together, it appeared plainly to be nothing but sulphur; which was manifest, both by the smell, and because what was drawn off was of a yellow colour, just as sulphur appears to the naked eye.

That fig. 22 is a part of a real snail is clear to me; but how, and after what manner, the sulphur gets into the shell, we can only guess. My opinion of the matter is this: the snails, and other things that are found upon the mountains of Switzerland, and which are supposed to be changed into metals, have lain, and do still lie, where a great quantity of sulphur is shut up in the bowels of the earth; and that sulphur, by a subterraneous warmth being rendered volatile, mounts upwards in exceedingly fine particles like fire, and so insinuates itself not only into the inmost parts of the snail-shell, where, according to all appearance, the snail itself is almost consumed, but also into the pores of the shell, and is there fixed and coagulated, so that the whole snail is converted into a sulphureous substance.

Now, in order to show the exceedingly small particles, into which sulphur is divided by fire, I prepared a small glass globe, represented by fig. 24, and placed in it at B, a small particle of sulphur, about the size of a single corn of sand; and when the glass and the air within it was of the temperature of the air we commonly breath in, I sealed hermetically that part of the glass which was open at E; and then blowing the flame of a candle upon the other part of the glass where the sulphur lay, till it evaporated into smoke, which for a short time was carried about the glass in a circular motion; and after the sulphureous smoke subsided, and settled itself on the glass, I examined it with a microscope, and observed so many exceedingly fine particles on the sides of the glass, that no man could believe that such a small grain of sulphur could possibly be divided into so many parts; insomuch, that the smallest globules of this divided sulphur could hardly be seen with one of my best glasses, and the largest globules lay round about the sulphureous matter that was not quite dissolved. After letting the glass sphere rest about half an hour, I observed with surprise that some of the small globules of sulphur, that lay a little remote from the rest, had formed themselves into a right line, and that 4 or 5 were placed lengthwise; and some globules were so extended in length, that they touched one another; but some hours after they were all united, and composed one small body just like salt-petre, when being joined to water, it begins to coagulate.

I had a snail-shell, fig. 25, which was very heavy in proportion to its size, and which seemed to be not quite whole, some part of it being broken off at LM. On breaking it to pieces, it seemed to be full, excepting the slenderest part of the tail, of a pale yellowish substance, that was visible enough to the naked eye. To satisfy myself about the said seeming metalline particles, I put about a fourth part of the whole into a glass, and brought it to such a strong fire, that the glass was ready to melt; in this operation, I separated a great deal of sulphur from it; and on taking the remaining matter out of the glass, I saw that all the sulphur was not extracted from it, for I discovered several sulphureous particles lying in the remaining matter, which seemed like a black burnt earth; and they were divided from each other in oblong particles, each having distinct figures, according to the places they were to possess.

I sealed up hermetically the glass where the extracted sulphur was coagulated, that no moisture or vapours might either get in or out of it; and then viewing those sulphureous particles that had lain farthest from the fire, I saw that the glass (though nothing thereof was perceptible to the naked eye) was covered with globules, just as if one had spread a table all over with sand; and a little while after, I saw the small particles were coagulated together in irregular figures; and as I before imagined, that the small sulphureous particles were fastened to the glass, now I saw an oleaginous and watery substance, which one might call the spirit of sulphur, drawn off together with the sulphur; and that several places in the glass had no globules, and nothing to be seen but some of this oleaginous matter.

I had also a third snail-shell, the outside of which was as smooth as if it had been polished; and when viewed through a microscope, I observed that the shell was adorned with surprising figures; from which I concluded that its smoothness was occasioned by its long and frequent attrition against other hard bodies. This shell I could break very easily with my fingers, and with my naked eye discover that it was mostly composed of the abovementioned shining matter, as it was also for the most part full of sulphur. Another shell was also of a similar nature.

M. Valkenier gave me, among other things, two little dice, of the size as represented fig. 26, adding, that some people said they were stone, others bone; and that these dice were found in Switzerland, deep under ground, and in great numbers, in a place where there had formerly been a Roman camp; and that the Swiss were of opinion, that the Roman soldiers had made use of them in play, while they were encamped there. I struck several blows with a hammer on one of them, which indeed made holes in it, but without breaking it; whereas, had it been stone, it could not have withstood so many blows. At last, having cut off some small bits, and made them glowing hot in the fire, I observed that it was black as bone is, when it has not lain long in the fire, and white when it lay longer. In this last viewing of it, I was still the more confirmed that it was bone, as I could then discover the pores of a bone in it; yet in my opinion it could not be made of ox-bone, as the parts were smaller.

Experiments on the Motion of Pendulums in Vacuo. By the Rev. Mr. W. Derham, F.R.S. N° 294, p. 1785.

At my request, Mr. Hauksbee made an experiment to discover the difference between the vibrations of pendulums in vacuo, and in air. He provided a proper receiver, and all other things necessary, such as, an eight day clock, vibrating seconds, and a half seconds movement of mine. The issue of the experiment was, that my pendulum vibrated two-tenths of an inch on each side farther in vacuo, than it did in the free air; and went 7 seconds slower in 20 minutes, than the other movement. But in the open air, my pendulum in 20 minutes, went only $3\frac{1}{2}$ seconds slower than the other pendulum.

This experiment I was desirous to try over again myself, which I did with the following instruments; viz. an air-pump of Mr. Hauksbee's; the next instrument was the aforesaid small movement, with a pendulum of about 10 inches long, that vibrates half-seconds, and is driven by the power of a spring: this instrument was commodious, not only for being easily fitted with a receiver, but also for vibrating half-seconds very nicely, and also because its vibrations are all of equal extent. The last instrument was a very well regulated month-piece, vibrating seconds all the year, with tolerable exactness.

Being thus provided, the result of many repeated experiments was, that in vacuo, as before, the vibrations were always larger, than in the unexhausted receiver. At the first, when my little movement was newly cleaned, the vibrations were above $\frac{8}{10}$ of an inch larger than in the free air. But afterwards (I suppose from some of the fouled oil of the pump spirtled on the wheels, in letting in the air, by which the force of the spring on the pallets was weakened) as the vibrations in the unexhausted receiver were a little contracted, so in the exhausted receiver they were more contracted, and only about a quarter of an inch larger than in the free air. The alteration in time, which this difference of the vibrations produced, was constant, only about 2 seconds in an hour slower in the exhausted, than in the unexhausted receiver. For if in 4, 5 or more hours going, the two pendulums did not vary a quarter of a second in the open air, or when the unexhausted receiver was put over the little movement; yet when the receiver was exhausted, the half seconds movement would lose, at the rate of two seconds in every hour, in every experiment, in many hours going.

In order to see what alterations would arise from varying the vibrations, by opening and shutting the paliets, I caused the vibrations in some experiments to be as large as the receiver would bear; in others, to be as short as possible; always adjusting the pendulum to vibrate half seconds nicely in the air. Yet still the success was much the same, or the difference was scarcely perceptible. Only I imagined, when the pendulum vibrated but a little way from the perpendicular, that the vibrations in vacuo were not so much enlarged, as when it vibrated in a larger arch.

In all these experiments, repeated many times with the same success, I had no reason to think, but that the vibrations were enlarged in vacuo by the vast rarefaction of the medium; only, that perhaps the different state of the air might alter the force of the spring, which drove the movement. For the trial of this, I put a well adjusted pocket-watch, with Hook's regulator, i. e. the common small spiral spring to the balance, into the vacuum. And after several trials, at the same pitch of the spring, I found not the least alteration in the watch's going in many hours; neither the springs, nor any other part of the watch seeming to be in the least affected by the vacuum; the balance circumvolving, or keeping the same turns, as in the open air.

But to be still more sure, if possible, I tried what would be the success by putting the half-seconds pendulum again into the receiver, and only pumping out a part of the air. And accordingly I left no more air in, than what kept the included mercurial gauge at about 6 inches height. The event of which was, that the vibrations were then not above $\frac{1}{10}$ inch larger on each side, than in the unexhausted receiver; and the time lost, but about half a second in an hour, or $\frac{3}{4}$ at most. And so, according as the mercurial gauge was more or less high, I always found the vibrations greater or less; always gradually decreasing, according to the quantity of air re-admitted.

From these experiments the following remarks have occurred. 1. That what Mr. Boyle long since observed (from a cocked pistol going down as fiercely in his vacuum, as in the air) may be hereby further confirmed, viz. that the air is not the cause of the motion of restitution in solid bodies, as springs. For if it was, it would certainly have been discovered in so tender an instrument as a well adjusted pocket-watch, lying under the perpetual influence of two springs.

2. As in vacuo (where the pressure of the atmosphere is taken off) heavy bodies descend quicker than they do in the open air; so it may be observed, that pendulums move swifter in the exhausted receiver, than in it unexhausted. That heavy bodies descend quicker in vacuo, is evident from the swift descent of the less heavy bodies, as cork, the down of sowthistles, the lightest feather, &c. which do all precipitantly descend, like a stone, in a tall exhausted receiver. And that the pendulum, in our experiment, moved faster in vacuo, is manifest from its vibrating but 2 seconds in an hour slower, when the vibrations were $\frac{1}{5}$ of an inch on a side, enlarged by the highest rarefaction of the air. Whereas I find by experiment, that near the same increase of the vibrations, in the open air, makes the pendulum go 6 or more seconds slower in an hour. I say nearly the same increase, because it is scarcely possible to manage the pallets so, as nicely to make the same vibrations as were in vacuo.

3. I query, Whether the variations of pendulums observed under the equinoctial, and between the tropics, do not arise as much, or more, from the rarity of the medium, and the increase of the vibrations consequent thereupon? It is scarcely, I think, to be doubted, but that the air is much thinner, and finer near the line, than it is without the tropics. And it is evident from the barometer, that on the tops of high mountains the atmosphere gravitates less than nearer the centre. And therefore, though I like the notion of the decrease of gravity from the increase of the distance from the earth's centre, yet I am apt to think that this is not the only reason of the phenomenon.

I wish therefore that Capt Halley, when he observed his clock go slower at St. Helena than in England, had at the same time observed whether the vibrations were not enlarged. It might be therefore very well worth the while for such as have opportunity, to notice, whether their pendulums between the tropics do not make larger arches, than in higher latitudes? Also in what latitude they begin to alter? Whether the vibrations be greater nearer the line, than in any other part between the tropics? Or whether the greatest increase be not always in those places where the sun passes their zenith? If the vibrations be found larger under the line, or in any other part of the torrid zone, then it may be observed how much larger they are, and in what proportion they increase or decrease, by approaching nearer to, or receding from the place of their greatest increase?

Also it may be worth observing, whether pendulums do not vary on the tops of high places, or in different states of the atmosphere, according as the mercury is high or low in the barometer? But then in this, and indeed in the former cases, it is necessary, or at least very expedient, that the movement be so exactly well made, that the power, whether weight or spring, do at all times exert the very same force upon the pads or pallets; which is very rarely met with; for most clocks are apt to vibrate sometimes larger, sometimes smaller arches in the 24 hours, according as the weight or spring more or less exerts its force on the work.

An Account of a Book. Apicii Cœlii de Opsoniis et Condimentis sive Arte Coquinaria Libri x. *Cum Annotationibus Martini Lister, et variis Lectionibus integris Humelbergii, Barthii, et variorum. Lond.* 1705, *8vo.* N° 294, p. 1782.

The learned world has been already obliged to the editor for his notes upon Sanctorius, and Hippocrates's Aphorisms; and in this edition of Apicius he gives further proofs of his generosity in serving the republick of letters.

His preface may be justly called a critical introduction to the history of Apicius, as to his country, his age, his design, and the various editions of his work, at Venice, Basil, Lyons, and Zuric; among which many doubts are cleared up. And the annotations of Dr. Lister will appear as valuable as those of the most professed critics; he being intent upon things, and they too frequently upon words.

After the Prolegomena come the 10 books of Apicius under their distinct titles, with the scholia and notes of G. Humelbergius, C. Barthius, Dr. Lister, and others; among which are interspersed several remarks of J. Casaubon, Salmasius, Nonnius, &c. all distinguished in the several columns of this beautiful edition. Among these, the reader will meet with many passages of the ancients illustrated, and set in a true light; especially Horace, Celsus, Scribonius Largus, Dioscorides, Pliny, Athenæus, Cœlius Aurelianus, Martial, Petronius, the Rei Rusticæ Scriptores, the Geoponici, and many others. At the end there is a very particular and complete index.

It was thought proper to print only 120 copies of this piece, to satisfy the curious, at the expence of some noble and liberal subscribers, whose names are affixed on the back of the title page; after the manner of publishing Mr. Lhwyd's Lithologia Britannica, in the year 1699.

Concerning Animalcula on the Roots of Duck-weed. By M. Leuwenhoeck, F.R.S. N° 295, p. 1784.

In the month of July last, I caused several of these weeds, as they were carried by the stream through Delft, to be taken out of the water in an earthen pot; and taking several times a full view of the animalcula that were fastened to the small roots of that green stuff, they appeared as in fig. 27, pl. 6; where

ABG represents the case or sheath, into which the animalculum withdraws, on the least shaking or stirring of the weed, and then thrusts out again, as far as BCFG, and at the same time appear the two little wheels represented by CDEF; in observing the motion of which, one would be apt to think they were two real revolving wheels. But soon as we could place the animalculum* before our eyes, in such a position as to view the little wheels, not partially, as is here represented, but completely, and all at once, we were convinced of our mistake in our former assertions; for now we saw clearly, that what appeared before to be two distinct little wheels, was really one and the same circumvolution.

Fig. 28 represents the said wheels, larger than they really appeared, in which the rotation proceeds from H to I, and so on from I to K, and from K to L and M. I was at last fully satisfied about this surprising and regular phenomenon; for having thrown away the weeds, to whose roots the animalcula adhered, and made my observations on several others, I found the same appearance every time, only with this difference, that the internal arches in the said fig. at K and M, were not near so much indented.

During those observations, one animalculum appeared as in fig. 29, N being that part of it by which it was fastened to the weed. This animalculum had his receptacle or little house, represented by NOT, composed of round bubbles, which figure had not occurred to me before. When this animalculum had thrust that part of its body, from O to P, out of its case or shell, it extruded at the same time that surprising wheel-work which consisted of four round parts, represented by PQRS, three of which were to be seen very plain, but the fourth at T was almost hid from the sight. This wheel-work moved from P to Q, and from R to S.

Fig. 30 represents that instrument, which in fig. 28, is for the most part described by PQRS, much larger than it really appeared; in which we saw the said teeth and wheel-work move from ABC to D, and from D to EFG, and in that manner perform its whole revolution; and at the least sudden touch of the animalculum, it presently drew the wheel-work into its body, and then its whole body into its case or shell, so that we could see nothing but a kind of horn, as NOT, fig. 29, in which it hid itself; but it quickly put forth that part of its body represented by OPQRST, and then the rotation or wheel-work proceeded as before.

If any one should be inclined to make this experiment, he must take care to choose out duck-weeds that have long roots; for I never observed any of these animalcula in the short roots of young weeds, nor indeed in the great roots,

* These animalcules belong to the Linnæan genus *vorticella*.

when once they are covered with a rough matter, and tend to decay, as I have seen a great many in the beginning of August; and he ought not to leave off his inquiry, though he should meet with 10 or 20 roots that have none of the abovementioned animalcula in them; for I have sometimes examined more than 25 roots, without meeting with any of these animalcula on them; as also, on the contrary, I have sometimes found three of them on one weed.

I was the more easily induced to assert what I formerly did, about the animalcula that are fastened to the roots of duck weed; viz. that they had two distinct revolving wheels; because I had discovered several animalcula that protruded two wheels out of the forepart of their body, as they swim or go on the sides of the glass; one of which is represented by fig. 31. This sort of animalcula I found in great numbers in the gutter-water in summer, which had stagnated some days in the small pits or cavities of the lead. These animalcula are gradually less and less, so that 50 small ones are not so large as one great one, or full grown animalculum, big with young.

In October 1702, I caused the dirt of the gutters, when quite dry, to be gathered together, and taking a small quantity of it, I put it into a paper on my desk; since which time, I have often taken a little of it, and poured on it boiled water, after it had stood till it was cold, that I might obviate any objection that should be made, as if there were living creatures in that water. These animalcula, when the water runs off, or dries away, contract their bodies into a globular or oval figure. After the said dry substance had lain near 21 months in the paper, I put into a glass tube of an inch diameter the remainder of what I had by me, and poured on it boiled rain water, after it was almost cold; and then immediately viewed the smallest parts of it, particularly that which subsided leisurely to the bottom, and observed a great many round particles, most of which were reddish, and were certainly animalcula; some hours after I discovered a few that had opened or unfolded their bodies, swimming through the water, and a great many others that had not unfolded themselves were sunk to the bottom, some of which had little holes in their bodies; from whence I concluded, that the little creature called the mite had been in the paper, and preyed on the said animalcula. Next day I saw three particular animalcula swimming through the water, the smallest of which was 100 times smaller than the above said animalcula. So that it is surprising that these small insects can lie 21 months dry, and yet live; and as soon as ever they are put into water begin to swim, or fasten the hinder parts of their bodies to the glass, and then protrude their wheels, as in the figure, just as if they had never wanted water.

In the month of September, I put a great many of the last mentioned animalcula into a wide glass tube, which presently placed themselves on the sides of the glass; whereupon pouring off the water, I then observed that several animalcula, to the number of 18 or 19, lay by each other in the space of a coarse sand; all which, when there remained no more water, closed themselves up in a globular figure. Some of the bodies of these animalcula were so strongly dried up, that one could see the wrinkles in them, and they were of a reddish colour; a few others were so transparent, as if they had been little glass balls, that if you held them up between your eye and the light, you might move your fingers behind them, and see the motion through their bodies.

After these animalcula had lain thus dried up a day or two, in an oval or globular form, I poured some water into the glass tube, whereupon they presently sunk to the bottom; and after the space of about half an hour they began to open and extend their bodies, and getting clear of the glass, to swim about the water; excepting only two of the largest, that staid longer on the sides of the glass, before they stretched out their bodies and swam away.

After I had poured the water from them, in order to see how they brought their bodies into that orbicular form; viewing two of the largest of them, I observed that they stretched out their bodies in the space of a minute, several times, to an extraordinary length, and thrice opened the hinder part of their bodies, and discharged some excrements, which, in the little water that remained about them, were dissolved into small pellets, before they assumed their round figure.

In the month of October, before the dirt of the leaden gutter was quite dried up, I took a handful of it, and laid it on a glazen earthen dish, in order to preserve it. This foul stuff when dry, is as hard as clay; so that the mites cannot come at the animalcula, that are thus doubly shut up; upwards of 21 months after I took some of this dry stuff, and infused it both in cold water that had been boiled, and in rain-water newly fallen; whereupon the animalcula began to show themselves, and that in great numbers; and soon after there appeared two sorts of much smaller animalcula.

Observations on staining the Fingers with a Solution of Silver in Aquafortis, &c. By Mr. Anthony Van Leuwenhoeck, F.R.S. N° 295, p. 1794.

It is well known that the skin and other parts of the animal body, are turned black by a solution of silver in aquafortis, or nitrous acid.

Concerning Hydatides voided by Stool. By Dr. W. Musgrave, F.R.S.
N° 295, p. 1797.

In April last [1704], I was called to see one Mrs. Pear of Tiverton. She is about 30 years of age, of a tender constitution, had a bad habit of body, and had about Candlemas last a fever which continued near three weeks, and was at length overcome by testaceous powders, alexipharmacs, but chiefly by the cortex. In this fever she had sour vomitings, and a pain in her stomach, which remained a long time; and after the fever, it was accompanied with a plentiful salivation; with wind, and pains in her side, to an extraordinary degree; under all which she laboured to the time of my seeing her.

About three weeks before my visit, she was seized with a jaundice, and while taking medicines, viz. pilulas et decoctum ictericum Fulleri, for that illness, she discharged several bladders by stool; and continued so to do, sometimes every day, at other times once in two or three days, ever since the first discharge of this kind, to the time of my visit. These bladders were of various sizes; the least was of the size of a large pin's head; the largest, equal to a pullet's egg: they were also of different colours; some white, others more yellow, from the liquor contained in them, which was a sort of gelly, like that of hartshorn, tinged more or less with saffron.

Before the discharge of these bladders, there was, besides the symptoms already mentioned, a coldness, and a sickness at her stomach, almost perpetual; with frequent inclinations to vomit, and hysteric suffocations: since that discharge, these symptoms have disappeared, and are succeeded by a soreness of the same part, as if something had been torn there. The bladders came off without pain; many of them whole and entire; one of which I saw, about the size of a large gall, or marble stone: others were broken, and appeared not unlike the empty skins of currants, gooseberries and plums. One bladder only, and that a broken one, came away by vomiting; which to all appearance had been almost as large as a goose egg. The gelly thrown up with this bladder and which in all likelihood had been contained in it, before it broke in coming up, was thicker and more fœtid, than was found in any of the other bladders. The number of those discharged by stool, amounted to several scores. During the whole course of this illness, the patient was rather loose than costive; had no manner of appetite, and seldom slept without an opiate.

I found her much wasted in flesh, with a dead pale look; such as argued her being very low: she had stools of an unusual smell, no way natural, and had vomited a great deal of cold phlegm. She was very willing to think these bladders came from her stomach, and urged the following reasons for her opinion; first, had they been originally in the bowels, in all likelihood the purges, of which she took many in the jaundice, would, as she said, have carried them off much sooner. Secondly, from the almost constant pain of her stomach, and frequent inclination to vomit, ever since her fever, to the time of discharging the bladders. Thirdly, from the rawness and soreness of her stomach after the bladders came off. This was her opinion, and these her reasons for it; and indeed I think they may be esteemed so far of force, as to prove, that some, and perhaps a great number of these bladders came from her stomach.

There was no appearance in any one of these bladders of such an order of parts, or organs, as showed them to be insects; nor upon examination was there any animal discerned in the liquor contained in them. Indeed, these observations were made only by the naked eye.

The medicines given, after I was called in, were chiefly of the vulnerary and digestive kinds; that which did her most service, but it was after the bladders were come off, was a tincture of myrrh and gentian, in large and frequent doses, and with a proper vehicle; under the use of this slight but advantageous medicine, from a very weak condition, she recovered an appetite, &c. and is now perfectly well.

Abstract of a Letter from Mr. John Thorpe, M.A. to Dr. Hans Sloane, S.R.S. concerning Worms in the Heads of Sheep, &c. Dated Oxford, July 24, 1704. N° 295, p. 1800.

With this you receive the delineation of a worm,* fig. 1, pl. 7, found in dissecting the head of a sheep, in the cells formed between the laminæ of the os frontis; it is an apode, and seems to be a species of the eulæ, though much different from the common sort breeding in putrid flesh. It is every where of a fair pale colour, excepting its tail, which ends a little obliquely in a plane, on which are impressed two remarkable black spots. Besides two small white corniculæ, its head is armed with a pair of black, sharp, and crooked forcipes, which, in contracting and extending its body, it draws in, and puts out at pleasure; with these, in creeping, it takes hold of the surface of the body on which it moves, and draws itself forward on pretty large, protuberant, and somewhat flattish toruli, fewer in number than those on its back, as in fig. 2; which alternately swelled and relaxed, seem instrumental to its motion, and supply the place

* Seemingly the larva of some insect, perhaps the larva of the œstrus ovis? The vertigo or giddiness of sheep is ascribed to the presence of a different animal breeding in the substance of the brain, viz. to the hydatis multiceps.

of feet. It looks of a clear, crystalline substance, and almost transparent. The membrane, that invested the cavity of the cells containing it, was very fat, and in most places separated from the bone; the blood-vessels appeared turgid and inflamed; whereas the membrane of the opposite cells, which have no communication with these, was thin, pellucid, adherent, and nowise preternaturally affected.

Mr. Bobart informs me, "that some gentlemen of the university, lately observed three worms lodged in the same parts of the head of a sheep, but in two distinct cells; the largest apart by itself, the other two in the cell adjoining, one of which was considerably less than the other, agreeing in form with the forementioned, of a whitish colour in general, with the two notable spots on the flat of the posterior part, but a shadow of brownness from the back down the sides, especially of the riper one; roundly turgid on the back, and flat underneath, divided with several annuli, as these animals generally are; at the extremity of which protuberances, serving instead of feet, there appears a little darkish brown spot on each side or edge, as they faded and withered they changed to a light red or phœnicious colour, and afterwards brown."

Moufet * mentions worms yearly breeding in the brain of stags, goats, and sheep, especially when fat; but supposing them generally known, he gives no description of them; unless from † Benedictus and ‡ Mathiolus; that they are eulis maximis æquales, et ejusdem cum illis formæ; though the worms meant by those two authors are not found in the brain, but (as they both agree) sub lingua in concavo circiter vertebram, qua cervici innectitur caput. § Avicenna asserting the possibility of worms breeding in the head, says expressly multoties nascuntur in anteriore parte capitis super locum qui est in strictura narium; to which || Foubertus adds, ubi oves, et capræ dicuntur perpetuo vermes habere, ut cervi in gutture. This plainly shows the distinct places of their generating in the heads of stags and sheep, as well as describes the particular cells in which they are found in the os frontis of the latter.

From hence perhaps we may be allowed to account for those various instances given by medicinal authours, of worms ejected at the nose by men and women. Benivenius, Fernelius, Zacutus Lusitanus, &c. relate several cases of this kind, all agreeing that they were attended with extreme pains in the head, manias, lypothymies, &c. which immediately ceased on their ejection.

It must be granted, ¶ Ægineta, ** Gabucinus, †‡ Forestus, †§ Borellus, and

* Theatr. Insect, lib. 2, cap. 30. † Lib. 21, in procem. ‡ Comment. in Dioscor. lib. 2, cap. 52. § Lib. 3, sen. 1, can. 3, tract. 2. cap. 3. || Cap. 9, de Cephalalgiâ. ¶ De re med. lib. 4, cap. 57. ** De Lumbric. cap. 8. †‡ Observat. medicinal. lib. 21, obs. 28. †§ Hist. et Observat. medic. cent. 2, obs. 70.—Orig.

particularly * Fulvius Angelinus, who wrote an entire discourse De Verme admirando per Nares egresso, very rationally conclude, that the worms they treated of came from the stomach, or lungs, not being attended with those symptoms, nor affecting the head, as in the cases related by the authors we have mentioned, particularly in that remarkable instance of † Hollerius of a worm evacuated at the nose nullâ tussi, nullo vomitu antecedente, which by the acute pain it produced, plainly discovered where it lay concealed.

Whether worms may generate in the brain or its teguments, I shall not determine; it is sufficient, supposing it possible, that there is no passage for them thence to the nose; where they are often found, as we are assured by many credible writers. Angelinus and Alsarius à Cruce place them prope nasi colatoria in stricturâ narium; both seeming to imply that sinus more exactly described by ‡ Avicenna; which is to be understood of human heads, not those of brutes, but only as applied by Joubertus.

Account of very large Stones voided per Urethram. In a Letter from Mr. Edw. Lhwyd, Keeper of the Ashmolean Museum, Oxford, to Dr. Hans Sloane, S.R.S. Dated Oxford, Sept. 22, 1704. N° 295, p. 1804.

Thomas Olton, upwards of 78 years of age, being afflicted with stone in the bladder, applied to Dr. Bullen and other physicians in the neighbourhood for assistance, and took various medicines according to their direction, but without relief. At length, in one of the paroxysms, which was more violent than any preceding attack, he voided per urethram two stones, each of which weighed above 2 dr. The expulsion of the first stone was attended with excruciating pain, but the second occasioned very little uneasiness, in consequence of the dilatation and disruption of the passage by the first calculus. It is probable that these two calculi were once united into one mass in the bladder, as they fitted each other exactly at the broken places.

On Firing Gunpowder on a red-hot Iron in Vacuo Boyliano. By Mr. Fr. Hauksbee. N° 295, p. 1806.

The red-hot iron being included in a recipient proper for that purpose, and the air withdrawn, which was in about 2 minutes of time, the mercury then in the gauge standing at $29\frac{1}{4}$ inches, a quantity of gunpowder was dropped upon the red-hot iron, which continued upon the surface of it some small time before it inflamed, and then was observed not to fire all at once; and

* Apud Alsar. Crucium de Quæsit. per Epist. cent. 3. † De Morb. intern. lib. 1, cap. 54, in Schol. ‡ Loc. supra citat.—Orig.

the last of the quantity that did so, seemed to give the greatest flash, upon which, the mercurial gauge descended something more than an inch, but rose again $\frac{3}{10}$ of the same. And on several repetitions of the like quantity of powder (the factitious air being always first withdrawn) the phenomena were very similar. Again, on purging the recipient of the factitious air, and the mercury elevated in the gauge as at first, three quantities were dropped on the iron, whose explosion, as well as the air produced from them, seemed in proportion to the quantity of powder, the mercury then in the gauge subsiding to 26. But upon dropping 6 quantities (the recipient being purged as before) which quantities not descending at once, but successively as fast as might be, the quantities that first reached the still ignited iron taking fire, by their flame making an explosion of the whole, at once blew up the recipient, although the weight of air incumbent on it was equal to 144$\frac{1}{2}$ lb. accounting the receiver at 3$\frac{1}{2}$ inches diameter, though it was something more, which sufficiently allows for the want of height of mercury. The gauge then standing at 29$\frac{1}{2}$ instead of 30, from which the calculation is made. The gunpowder used was the common glased sort, and the weight of the six quantities, which removed the receiver, with so great a pressure incumbent on it, was but 7 grains, each quantity weighing something more than one. I did not observe the recipient to be broken before it reached the floor. It was thickly lined with sulphureous and nitrous steams, so that the flashes of fire through the cloudiness of the glass seemed very much to resemble faint lightnings. The content of the receiver was equal to about 25$\frac{1}{2}$ oz. of water, allowing for the bulk of the iron and pedestal.

An Experiment, made to try the Quality of Air produced from Gunpowder, fired in Vacuo Boyliano. By Mr. Fr. Hauksbee. N° 295, p. 1807.

On making the late experiment of firing gunpowder in vacuo, it was hinted as worthy of trial, whether the factitious air of fired gunpowder was endued with any quality differing from common air: To satisfy the query, on Dec. 26, about noon, I inclosed a red-hot iron in vacuo, the mercury in the gauge then standing at 29$\frac{1}{2}$ inches; upon dropping the first quantity of powder, by which is to be understood something more than a grain weight, its explosion made the mercury in the gauge descend about an inch, undulating but very little. The second quantity being let fall, the mercury subsided about $\frac{3}{4}$ of an inch; and so for several quantities following, it descended by pretty equal stages, till it had fallen about 6 or 7 inches; and it was observed, that upon every quantity fired, the undulations of the mercury increased. But after it had subsided 6 or 7 inches from 29$\frac{1}{2}$, its several descents became less, little, if at all, exceeding $\frac{1}{2}$ an inch, although the quantities fired were equal; but still the undulations increased, and the explosions manifestly did so too: till at last the receiver seemed to be in great danger of being blown up by a single quantity; the undulations of the mercury being then augmented to 6 or 7 inches. Now 26 quantities or 32 grains having been fired upon the iron, and the mercury in the gauge fallen to $12\frac{3}{4}$, I carefully observed the gauge, which in 7 minutes had ascended $2\frac{3}{4}$ inches, the next 5 minutes it rose but $1\frac{1}{4}$ inch; and so less successively every 5 minutes, so that in an hour and 17 minutes it had attained only to 21 inches, the iron not being quite cold. At 9 the same night I observed the gauge, and found the mercury elevated to $22\frac{1}{4}$ inches, precisely: next morning at 9 it had attained to $22\frac{1}{2}$; and so continued all that day, the iron then being reduced to the temperature of the outward air. So that from $12\frac{3}{4}$ to $22\frac{1}{4}$ seems to be the weight or spring of heat equal to about $\frac{1}{3}$ of an atmosphere of air, which would press the mercury upon the upper part of the gauge, but equal to such a degree of heat as was then contained in the receiver, when the gauge was fallen to $12\frac{3}{4}$: the remaining space, from $22\frac{1}{4}$ to $29\frac{1}{4}$, is supposed to be supplied with factitious air, and answers to about $\frac{1}{4}$ part of the whole contents of the receiver, which was equal to $25\frac{1}{2}$ ounces of common water, allowing for the iron and pedestal. This air, produced from gunpowder, I find to be actuated by heat and cold as common air: for, holding my warm hands upon the receiver, the mercury in the gauge would immediately descend, and rise again when reduced to the temperature of the outward air. This I repeated several times, with the like success. What further occurs in this experiment is, why the explosions of the like quantities of gunpowder should be greater when resisted by air, than in vacuo, where nothing seems to hinder the extension of their flame.

Georg. Joseph. Camelli, De Plantis Philippensibus scandentibus; Pars Tertia. Ad Jacobum Petiver, S. R. S. nuper transmissa. N° 295, p. 1809.

A continuation of the catalogue of climbing plants, growing in the Philippine Isles.

A Universal Spherico Catoptric Theorem. By Mr. Humphry Ditton. N° 295, p. 1810. *Translated from the Latin.*

The finding the foci, both in dioptrics and catoptrics, easily follows from the calculation for the caustic curves. For nothing more is requisite, than to know the locus in which the radius (perpendicular to the curve, either reflecting or refracting) is a tangent to the diacaustic or catacaustic curve. Concerning which method, there may be consulted Mr. Hays' book on Fluxions, lately published.

I shall here attempt the same thing on other principles, as far as catoptrics are concerned.

Let DEF be the portion of a concave spherical speculum, fig. 3, pl. 7, its centre being B, and radius BE or BD: also let A be a radiating point placed in the axis, from whence proceeds the ray AD, which at the point D is reflected into DC. Now the distance of the focus C, from the vertex of the speculum E, is to be investigated.

It is to be noted, that the point D is supposed to be very near to E. For the remoter rays go beside the eye, placed in the axis AE; and so contribute nothing to vision. And because the arc DE is indefinitely small, the angles DAB, ADB, as also their sum DBC, are very small, and therefore they will have to each other the same ratio as their opposite sides. By reasoning on this principle it was that Dr. Halley, professor of geometry at Oxford, arrived at his dioptrical theorem.

These being premised, put AB $= b$, BD $=$ BE $= r$, BC $= z$, CE $= r - z = f$, suppose, for brevity sake. Then b and r are known quantities, because the radius of the speculum and the distance of the lucid point from the vertex are given; but z and f are unknown and required quantities. Now in the triangle DAB, it will be, as the angle DAB : the angle ADB :: $r : b$; and in the triangle DBC, the angle BDC $=$ the angle ADB from the nature of reflection; also the angle DBC $=$ DAB $+$ ADB by Euclid's Elem. Therefore since the angle DBC is as $r + b$, and the angle BDC as b; it will be also, as the angle DBC : BDC :: $r + b : b$; and from the principle above-mentioned, as DC : BC :: $r + b : b$. But because the point D is very near the point E, DC may be accounted equal to CE; therefore it will be CE : BC :: $r + b : b$, that is, $f : z :: r + b : b$, and hence, by compounding, $f + z : f :: r + 2b : r + b$; but $f + z = r$, therefore $r : f :: r + 2b : r + b$, and hence $f = \frac{rr + br}{r + 2b}$. Q. E. I.

If we put $r + b =$ AE $= d$, the theorem will contract to this form, $f = \frac{rd}{2d - r}$. But in either way the theorem will serve for finding the focus, whatever be the form of the speculum, or the condition of the rays.

Corol. 1.—It will be $dz = df - rf$, by substituting z for $r - f$, or AE $\times$ BC $=$ AB $\times$ CE, or, which is the same thing, the line AE is harmonically divided in the points A, B, C, E: for that equality of rectangles is the property of a line so divided. So that in every spherical speculum, the lines DA, DB, DC, DE are harmonicals; and the radiating point, the centre, the focus, and the vertex, are points that constitute an harmonical division.

Corol. 2.—First, if d be greater than r; then, by the calculation, f, or $\frac{dr}{2d-r}$, is always greater than $\frac{1}{2}r$. That is, if the distance of the radiating point be greater than the radius of the speculum, then the focal distance will always be greater than half that radius. Also $\frac{dr}{2d-r}$ will always be less than r; that is, the focal distance always less than the said radius of the speculum.

Secondly, if d be $= r$, then will $f = \frac{dr}{2d-r} = r$: that is, if the radiating point be placed in the centre of the speculum, its image will there be united with it.

Thirdly, if d be less than r, then the expression for f will be either positive, or negative, or infinite, according as the quantity $2d$ is either greater than, less than, or equal to the quantity r. If $2d$ be greater than r, that is d greater than $\frac{1}{2}r$; then the focus and the radiating point lie on the same side of the speculum. But if $2d$ be less than r, or d less than $\frac{1}{2}r$; then the image will be in the axis of the speculum produced beyond the vertex. And if $2d$ be $= r$, or $d = \frac{1}{2}r$; then the image is at an infinite distance, or the reflected ray becomes parallel to the axis.

Corol. 3.—By means of this calculation it may be readily determined, how the motion of the image corresponds with the motion of the radiating object, in respect of the speculum. Let the distance of the image from the speculum be $\frac{dr}{2d-r}$ as before, when the distance of the object is d. Now let the distance of the object be any how changed, and from d let it become nd, making n any number, integer or fraction: then instead of the former equation $f = \frac{dr}{2d-r}$, we shall have $F = \frac{ndr}{2nd-r}$, another equation to a new focus. Where, when n is greater than 1, the second distance of the object is greater than the former; but less when n is less than 1.

This being premised; if d be greater than r, and n greater than 1; then will F be less than f, that is $\frac{ndr}{2nd-r}$ less than $\frac{dr}{2d-r}$, or $2nddr - ndrr$ less than $2nddr - drr$, as is manifest. That is, in a concave speculum, if the distance of the object be greater than the radius, then while the object recedes from the speculum, the image approaches towards the same. But if it be less than 1, then $2nddr - ndrr$ will be greater than $2nddr - drr$, or F greater than f. That is, as the object approaches toward the speculum, the image recedes from it.

Suppose now that d is less than $\frac{1}{2}r$; and let nd be any other distance of the object still less than $\frac{1}{2}r$: then will $2nddr - ndrr$ and $2nddr - drr$ be negative quantities; or $ndrr - 2nddr$ and $drr - 2nddr$ will be positive quantities. And here, if n be greater than 1, then will $ndrr - 2nddr$ be greater than $drr - 2nddr$, or F greater than f. But if n be less than 1, then will $ndrr - 2nddr$ be less than $drr - 2nddr$, or F less than f. That is, in the concave speculum, if the distance of the object be less than half the radius of the speculum, then while the object recedes from the speculum, the image recedes from it also; or while the object approaches towards the speculum, the image approaches to it also.

And all these conclusions, which we have deduced from the steps of the calculation, are contained in one scholium in Dr. Gregory's Catoptrics.

Corol. 4.—In the equation $f = \frac{dr}{2d - r}$, if d be supposed infinite, it will be $f = \frac{1}{2}r$: which is a rule for parallel rays, or for a radiating object placed at an infinite distance. The same thing will follow, if b be made infinite in the equation $f = \frac{rr + rb}{r + 2b}$.

Corol. 5.—In the equation $f = \frac{dr}{2d - r}$, changing the negative sign of the quantity r into positive, it will be $f = \frac{dr}{2d + r}$; or in the equation $f = \frac{rr + br}{r + 2b}$ changing the positive sign into negative, it will be $f = \frac{br - rr}{2b - r}$; which gives a rule for a speculum convex towards the radiating object. The change of the sign is very plain; for as in the concave speculum it is $d = b + r$, so in the convex it will be $d = b - r$.

Corol. 6.—In a convex speculum, (those things continuing which are stated in corol. 3, concerning the concave speculum) it will appear, that if n be greater than 1, then $2rndd + ndrr$ will be greater than $2rndd + drr$; and if n be less than 1, then $2rndd + ndrr$ will be less than $2rndd + drr$. That is, both the object and the image at the same time either approach towards the speculum, or both recede from it.

In a convex speculum it also appears, that if the object recede to an immense distance, yet its image will not recede from the vertex more than half the radius, but that it will there stop, in the middle point between the centre and the vertex: for supposing either d or b to be infinite, it will be $f = \frac{dr}{2d}$ or $\frac{br}{2b}$, that is, in either case $f = \frac{1}{2}r$.

To these may also be added the solution of a catoptric problem, viz. To find such a position of the radiating point, in respect of a given speculum, that the radiating object may have a given ratio to its image made by the speculum. Let r to q be the given ratio; also let o denote the object, i the image, d the distance of the object from the speculum, and f that of the image. Now, as Dr. Gregory has demonstrated, it will be $o : i :: d : f$, that is, the object and the image are directly proportional to the distances from the vertex of the speculum; and because it is required that it may be $o : i :: r : q$, it must also be $d : f :: r : q$; or (instead of f taking its value) $d : \frac{dr}{2d - r} :: r : q$; hence $2dq - rq = rr$, and $d = \frac{rr + rq}{2q}$. Now because $dr = \frac{r^3 + r^2q}{2q}$, and $2d - r = \frac{rr}{q}$, it will be f, or $\frac{dr}{2d - r} = \frac{r^3 + r^2q}{2q} \div \frac{r^2}{q} = \frac{r + q}{2}$, which is the distance f of the image from the speculum, answering to the distance of the object. Therefore if the object be set at the distance $\frac{rr + rq}{2q}$, its image made at the distance $\frac{r + q}{2}$, being compared to it, will have the ratio of q to r: or it will be $o : i :: r : q$; for it is $o : i :: d : f :: \frac{rr + rq}{2q} : \frac{r + q}{2} :: r : q$.

We have here considered the radiating object and the image as if they were lines: but, if they be surfaces, then it will be $o : i :: d^2 : f^2 :: r : q$; hence we shall come to this equation $4dd - 4qdr = r^3 - qr^2$, from whence the value of d may be very easily found by the common methods.

R. P. Georg. Jos. Camelli, De Plantis Philippensibus scandentibus, Pars Quarta. Ad D. Jacobum Petiver, S. R. S. nuper transmissa. N° 296, p. 1816.

A continuation of the catalogue of climbing plants growing in the Philippine Isles.

Concerning the Barks of Trees. By M. Leuwenhoeck, F. R. S. N° 296, p. 1843.

Although I have been many years fully convinced myself, that the bark of trees was produced from the wood, and not the wood from the bark, as many have affirmed; yet I find that some, and those persons of good learning, still maintain the contrary. This induced me to make a nicer inquiry into the barks of trees, in order, if possible, more fully to convince the world that the bark of trees always proceeds from the wood. Mr. L. first tried a small piece of dry cinnamon wood; but found it unfit for his purpose, partly by the fibres running lengthwise in the bark, and partly because he could not cut the piece across without the bark separating from the wood. For which reason he turned his thoughts to the bark of cherry, plum, beach, &c. which have the vessels of their bark not extended lengthwise, but circularly around the wood. And hence it is that the bark of such wood cannot be stripped off longwise, but only circularly.

I have asserted formerly, (says Mr. L.) that in all countries where there is any winter, so far as to put a stop to the growth of trees, at all times as long as the growth lasts the bark grows thicker, and that the new bark protrudes that which was made before further and further from the wood; insomuch that in the bark of old trees, one may cut a finger's breadth in depth, before coming at any thing like greenness or sap: and by considering those barks carefully, it may be seen what part of the bark from time to time is deprived of its nourishment, and consequently what part of it is quite dead. By these observations I have discovered in a twig of a cherry-tree, of one year's growth, that the bark consists of at least 6 thin membranes, whose exceedingly thin vessels or fibres extended themselves circularly about the wood, and were very closely united to each other. Fig. 4, pl. 7, represents one of these membranes, separated from the rest, as viewed by the microscope; where it may be observed that the vessels or canals do not run longwise, but circularly about the wood; on which account the vessels cannot long remain whole; but must from time to time be broken in pieces.

I next turned my thoughts to the beach-wood, because the greatest part of that wood is covered with a red bark, which sticks close to the wood, and grows yearly thicker; and on the outside of that bark there is produced a whitish sort of bark several times in a year, which falls off from the wood as if it was peeled; but this only happens in beach-wood of an ordinary thickness; for in the thickest wood this peeling or scaly sort of bark is not produced, and then the bark grows very thick; but the most part of such bark is pushed away, and remains as it were without nourishment; and in such there is no outer scaly sort of bark produced. Fig. 5 represents a small particle of this wood, as it was cut across; in which the ascending vessels or canals, both great and small, are easily seen, and between which run the horizontal vessels, which receive their juices from the ascending ones. Here QVTSR represents a particle of the bark, in which the horizontal vessels, as they lie in the wood, and are continued on to the bark, and from whence the bark is produced, are shown by MNOP, of which N and O do not go quite throughout into the bark, by reason of some hard matter contained in it, as shown at X. But the horizontal vessels, that are represented by PRS, and MQVT, go throughout the wood into the bark, so far as to preserve the bark from mortifying.

Now as the horizontal vessels are continued from the wood into the bark, so there sprouted out from every side of those vessels exceedingly small canals, which run circularly about the wood, and so for the most part produced the bark of that wood. In the same figure, as MQVT shows one of those horizontal vessels, continued from the wood, and carried on into part of the bark, which is represented by QVT, so between Q and V are shown a few of those vessels which sprout out of the said horizontal ones, and run circularly about the bark; and how carefully soever I observed them, I could not discover one ascending canal; which must needs run lengthwise up the bark, in case the bark had its rise from the root of the tree. Fig. 6 shows also a thin scaly particle of the bark of the said wood before a microscope; the vessels or canals of which run also horizontally from A to B, or from C to D. Fig. 7 shows also a small scale of the bark of the twig of a tree, in which the vessels represented by WX or ZY run circularly about the wood; but I have forgotten to what tree it belonged.

In another piece of cinnamon wood, says Mr. L. I have inquired into that part of the bark that lies next, and is as it were joined to the wood, and in cutting it to pieces have satisfied myself more than ever, that the cinnamon, otherwise called the bark, receives both its nourishment and increase from the wood alone, and not from the root; for when I divided this bark into small parts, I could discover no ascending vessels in it; but on the contrary, so many horizontal vessels coming out of the wood, and those too so large, that I do not know that ever I have discovered so many vessels in the barks of any other trees. Fig. 8 shows a very small particle of the bark of a cinnamon tree, in which the horizontal vessels lie by each other in such order, as is here represented between LMNRS, or between NOQR, in which they are all cut across. We also see that about the horizontal vessels there lie oblong parts, that run into a point at both ends, which parts, represented by LMN, lie together; we may observe also how the said parts surround the horizontal vessels.

All the said long particles, which in a great measure compose the cinnamon bark, are not incurvated, as in fig. 8; but many of them are extended in right lines, as in fig. 9, which represents a very small particle of the abovementioned long parts, which likewise incloses some horizontal vessels; and wherein may be seen at A how regularly the sharp points are ranged by each other, as also between BF and CE, between which the horizontal vessels are to be seen in that order in which they always lie. That sharp and pointed particle represented by FH, seems to be out of its place; and I fancied that in dividing it from the other parts, I might have broke it off at F.

I also placed 3 other long sharp pointed particles before a microscope, as in fig. 10; where we also see in how regular an order the pointed parts appear, as in KM for instance; from whence we may conclude, that all the other parts of the like nature are disposed in the same manner. Fig. 11 shows another pointed particle, that was exceedingly incurvated, which I suppose might be occasioned by its having surrounded two several divisions of the horizontal vessels. We may pretty easily conceive how one canal is produced, or issues out from another; but how the said long sharp pointed particles, represented by fig. 8, 9, 10, 11, are produced, is, as well as a great many other phænomena, past my understanding.

I have also examined into the nature of the bark of a thick lime tree; the rather because I know no other barks of trees whose parts are so easily separated from each other, either in length or breadth; insomuch that in Muscovy they make of it mats for packing, and rope-work, which is very strong, and if I am rightly informed, is not easily subject to rot, though it should lie long wet. This bark I also cut across, to discover the bent or run of the horizontal vessels that issue out of the wood. Fig. 12 represents a small particle of the bark of a lime-tree, as it was cut across; where ABC show the horizontal vessels that issue out of the wood, and consequently they are cut lengthwise. These vessels although at their first issuing out of the wood they lie close to each other, as from A to B, and from B to C; yet they do not remain always so close; but as the tree grows thicker and larger, the horizontal vessels are more divided from each other; as for instance, that which at B is but one bundle or collection of vessels, with the increase of the tree divides itself into two, and the separation grows larger and larger, as in BMK and BML. Now, that there may remain no interstice or vacuity between the said horizontal vessels, there are other vessels produced from those, as seen between MID, which new vessels produce a matter that fills the place of MLK. These parts are roundish, but so interlinked with each other, that they serve for canals. They do not run through the thickness of the old bark; for in some places the bark dies sooner than in others, for want of nourishment; insomuch that you may perceive in the bark of a lime-tree, of an ordinary thickness, three distinct crusts, lying one upon another; the outermost of which being destitute of nourishment, by little and little become dry and withered. Fig. 13 represents a very small particle of the bark of a lime-tree; wherein are shown partly the mouths of the canals that lie lengthwise in the bark, and are here cut across; but chiefly to give a view of the horizontal canals, as they are cut in their length, as at NS or PQ; which same horizontal vessels are represented in fig. 12, by AIH, BMLG, BMKF and CDE. These canals or vessels, described by NS or PQ are not of a continued hollowness throughout, but rather seem to consist of oval particles linked to each other.

I placed before a microscope a little bit of a lime-tree, which was cut off the wood lengthwise, and the horizontal vessels cut across; to see how those horizontal vessels or canals lie in the wood; which vessels are also continued into the bark, so far as it is alive, and serve for the feeding and increasing of it.

Fig. 14 represents a little slice of the lime-tree wood, in which we may count, in nine several places, the horizontal vessels or canals that are cut across, and which canals are situated between the small ascending vessels, which for the most part nourish the wood. Now between the horizontal vessels and canals in the wood and in the bark there is no difference, but in the ascending vessels and the bark there is, for they are of such a disposition as the horizontal vessels which are in the wood and the bark; and thus they agree with those vessels represented in fig. 13, by NS or PQ. Now if we find that the horizontal vessels or canals, as well in the wood as in the bark, are of one contexture; and that the ascending vessels in the bark of a lime-tree are also of the same; we may more firmly conclude, that the bark is produced from the wood, and not from the root.

I have turned my thoughts again upon the consideration of cork, which is said to grow as the bark of a tree, on a kind of oak* in Spain; which if so, I imagine that the burning which we perceive in the flakes of cork, is done by two hot iron plates, in order to make it flat and straight. I took then one of those pieces of cork which are cut into stoppers for bottles, as is represented in fig. 15. In which we must suppose that BG is the part that lay next the tree, and that E was the outside of the same.

Between GHIKE are five distinct divisions, running across from F to D, which is the part that surrounded the tree, and from whence I conclude, that the cork was arrived to such a thickness in 5 years time, for each streak denotes the growth of that year. There are also four distinct dark streaks, of which GI is the middlemost, which I supposed were large canals. We must also conclude that the length of all corks (in order to prevent either moisture or air from passing through them) must be according to the length of the cork as it grows on the tree, and so that part of the cork represented by ABC was the lowermost part, and DEFG was the uppermost or near the uppermost, according to its situation upon the tree. For further satisfaction, I cut a little piece of a cork, as from G, where we may suppose that it was joined to the tree, cutting it after such a manner, that the cut of the knife went from G to H, and having placed it before the microscope, I perceived all the canals so situated, as if they came out of the wood, without discovering in the least any ascending vessels, though I cut it ever so often; from whence I must again conclude, that the growth of the cork proceeds from the wood.

Fig. 16 shows a small particle of a cork, as it was cut off between G and H, of which we must suppose LMN to be the part next the tree, and so the vessels or canals, by which it receives its increase, run horizontally, as from L to Q,

* Cork is the bark of the quercus suber. Linn.

from M to P, and from N to O; but I could not find one single canal that was perpendicular, or can be said to proceed from the root. These forementioned canals have no thorough passage, and it seems that in each canal there are so many valves as there are horizontal vessels in them. At a b is represented a line running quite across, and something incurvated, which is that part of the cork where, the season of the year being over, a stop was put to its growth.

For further satisfaction, I cut another piece of a cork after the same manner, so that whereas in the foregoing figure, the horizontal canals were represented in the length, now the same canals were cut across. Fig. 17 represents a small piece of cork, as it appeared through a microscope that was more magnifying than the former in fig. 13, from which this piece of cork was cut off, between B and G, and was that part which was next, or united to the tree, and from whence it received its increase; and consequently those canals which in fig. 16 were cut longwise, were now across. Here we perceive that almost all those parts that were cut across did not consist of round canals but of hexangular ones, which is agreeable to the most perfect order, because it prevents any vacuities taking place between them.

Concerning the Vitrified Salts of Calcined Hay. By Mr. Leuwenhoeck, F.R.S. N° 296, p. 1856.

A large hay-rick in Salisbury Plain was reduced to ashes by spontaneous combustion. These ashes were found to consist of saline particles,* forming "a light glassy stuff and very brittle, which when walked over, cracked and broke under the feet; and when put into the mouth had a salt taste."

On Insects in the Bark of decaying Elms and Ashes. By Sir Matthew Dudley, F.R.S. N° 296, p. 1859.

About 5 or 6 years since I transplanted several elms more than 6 inches diameter, which for the first 2 or 3 years all thrived very well; but 2 or 3 years ago, there happening a very dry time in July or August, I observed one of those elms, which stood very shallow, and on pretty high ground, looked very sickly; the leaves turned yellow, and began to fall off; which made me with a knife examine the bark, when I found the inside not so green, but of a redder colour than the rest; and between it and the tree not so moist, and the bark sticking very close to the wood; but what was most remarkable, I discerned a great many small black flies† of the beetle kind, viz. having a hard case, under

* Potashes or vegetable alkali, with carbonaceous matter and other impurities.

† Dermestes polygraphus. Linn.

which their thin long wings were contracted and covered, between the bark and the tree; and viewing it more carefully, I observed that these flies had made their way thither by piercing the bark in innumerable places, easily discernible on the outside, being about the size of a large pin-hole, or rather such as a large pin's head would enter. Some of the flies I found just entering, having got quite through the bark; others had made some progress between the tree and the bark, which appeared as a channel. I despaired of recovering the tree; however my servants being watering others, I caused them to bestow about 2 hogsheads on this tree, then stirring the earth about the roots, and laying some half rotten litter about it, to defend it from the scorching rays of the sun; upon this the tree in some degree recovered its verdure again that year, and the next year made very good shoots, and so continued until this year.

But this being a very dry summer, I perceived several of my trees in the beginning of August look sickly, and particularly the tree formerly mentioned. I made the same trial on them all, and found the bark sticking close to the tree, with but little moisture between, and vast numbers of those small flies, which had pierced the bark in numberless places. I ordered the earth about the roots to be loosened as before, and about 2 hogsheads of water to be poured on each; then viewing them the next morning, I found the outside of the tree almost covered with bees and wasps, and large black flesh flies, which were all busy in sucking the juice or sap, which ran plentifully out at every hole, made by the little flies in the bark, which was very glutinous, and sweet as honey. I again examined the bark, and found it very moist between it and the wood, and all those small flies either gone, or drowned in their new habitation by the sudden rise of the sap: this tree recovered.

On examining several trees, which looked sicklier than the rest, I found that almost all greenness had left the bark, and there remained no moisture between it and the wood; but the bark stuck so close to the wood that it was hardly to be parted; and throughout the whole tree the bark was pierced by the small flies, which, from the hole at which they entered, had made each of them a straight perpendicular channel from their entrance upwards, about 2 inches long, or something more, very little, if at all, wider than just to move themselves straight forwards in it; for I observed that, when disturbed, they all came out backwards. All along on each side this channel, as close to each other as they well could, so as yet to be distinct, there were small channels running horizontally from it, in every one of which, at the extremity, there was a maggot, in size just the width of the small channel, very lively, whitish, and almost transparent. These trees, though well watered, received no benefit by it, but died. It is to be observed, that in those trees having their leaves green and healthful, there was none of these flies to be found.

About the middle of October I found that those small white maggots, and consequently their channels, which they exactly filled, were grown much larger, and they had made their progress from the place where they were first hatched, which was close to, or upon the very wood of the tree, almost to the very outside of the bark of the elm, which is usually pretty thick; and in every one of the perpendicular channels before-mentioned, I found the mother fly lying dead, for the most part towards the entrance of the said channel.

These observations put me upon examining the wood which lay in my yard, for timber or fuel, when in all the elm which was felled last spring, I found the bark as much pierced; the same mother channel, and the same small channel, proceeding from the mother channels, full of maggots, which had also made their way almost to the outside of the bark.

Observing some elm which had lain much longer in the yard, and taking off the bark, I found the same tracks, both of mother fly and maggots, and that at the extremities of almost all the horizontal channels, made by the maggots, where they had subsisted long enough to come to any perfection, the bark was pierced quite through, by a hole just the size of the channel, and nothing left remaining but a kind of whitish tough skin, exactly the colour and size of the maggot, at the mouth of the hole, and the rest of the forsaken channel perfectly filled with dust or the excrement of the maggot.

I then examined the ash wood, which had lain some time in the yard, and at first sight, it being young, and its bark pretty smooth, I perceived it full of small holes; and on separating it from the tree, I found just the same sort of work as in the elm, and by the same sort of fly, having found several of the mother flies dead in their channels, and the same empty skins at the extremities of the other channels; only with this difference, that whereas in the elm all the mother channels were perpendicular, and the maggot channels horizontal, here in the ash it was just contrary, all the mother channels were horizontal, and the maggot channels perpendicular; this I at first thought might be accidental, and peculiar to that piece of wood, but on examination of above 100 pieces of wood of different trees, and felled at different times, I found it exactly to hold true in them all.

I observed several oak and maple trees, which had been felled, some in winter and some in summer, and the bark remaining on them, but could find no such thing in either of them.

Fig. 18. pl. 7, shows the bark of ash: fig. 19, the bark of the elm; fig. 20, the worm as large as the life lying on its back; fig. 21, the mother fly, with its

belly upwards, as large as the life; fig. 22 and 23, the worm and the fly with their backs upward; fig. 24 and 25, the worm magnified; fig. 26, the mother fly magnified.

Concerning a Leaden Coffin, &c. taken out of a Roman Burying-place, near York. By Mr. Ralph Thoresby, F. R. S. N° 296, p. 1864.

There has been lately found in the Roman burying-place a remarkable lead coffin, which by the circumstances seems to have been for a person of quality; it was 9 feet deep in the ground, 6 of which were clay, and 3 a black earth; the coffin, which was about 7 feet long, was inclosed in a prodigious strong one, made of oak planks, about 2¼ inches thick, which besides the rivettings were tacked together with brags, or large iron nails; these are 4 inches long, the heads not diewise, as the large nails now are, but perfectly flat, and an inch broad; I have one somewhat different, the nail itself is half an inch broad, and thin, somewhat in the form of a wedge, and the head not round, as the others, but somewhat like the modern draw-nails, but these old ones are generally square, the four sides of an equal breadth; many of them are almost consumed with the rust, and the outsides of the planks, but the heart of the oak is firm, and the lead very fresh and pliable; whereas one found about a year ago is brittle, and almost wholly consumed, having no planks to guard it: but what I was most surprised at was, that the bones should be entire, though probably interred 1500 years ago; for it is above so many centuries since their custom of burning gave place to that more natural of burying their dead, which, according to M. Muret, was re-introduced by the Antonines. I have a thigh-bone, which is exceedingly light, and the lower jaw, which was furnished with all the teeth, but some of them are since stolen out.

Experiments on the Production and Propagation of Light from the Phosphorus in Vacuo; made before the Royal Society, by Mr. Fra. Hauksbee. N° 296, p. 1865.

Exper. I.—Having provided a dark room, the first experiment was by drawing some lines on a piece of blue paper with the phosphorus, which became immediately luminous in the open air, having a continual undulating motion. This being placed under a receiver, after some few exsuctions, the undulation ceased, but the luminous quality appeared to be in a great measure increased; the receiver being farther exhausted, it became manifestly brighter; and so continued, till on the admission of air, which was done gradually, the light sensibly diminishing all the while. But on the repetition of the experiment, it was the opinion of the gentlemen then present, that it did not appear quite so brisk, or so vivid, as at first.

Exper. II.—To 2 or 3 small pieces of phosphorus, in a glass dish, was added a small portion of oil of vitriol, tartar per deliquium, and oil of cloves, which mixture fired the phosphorus in the open air: but adding a little common water, the flame was extinguished. This preparation being included in a receiver, very little light appeared: but on exhausting the air, it became very apparent, and sent forth vivid steams. At the same time the ingredients in the dish very much resembled a boiling flame, exhibiting a strong light; so that several objects near it were thence distinguishable; and so continued till the air was admitted, at which time it became opaque. Nor would shaking the engine (by which means the mixture in the dish was put in motion) produce any sensible recovery of light.

Exper. III.—A small quantity of the said ingredients being put into a bottle with a narrow neck, and included under a receiver, appeared then with very little light; but on withdrawing the air, the phosphorus began to be luminous, and continued to increase, in proportion to the rarefaction of the air, issuing out of the bottle in a pyramidical form. At last, although the receiver was well exhausted, the vivid steams that were then emitted, were so fine and subtle, as to ascend in so thin a medium, reaching the upper parts of the receiver (which was not tall), and descending again by its sides. On letting in the air, the light quite vanished; and it would have been in vain, as I have often tried, to have waited in expectation of its recovery in the open air.

Observations on the Seed-Vessels and Seeds of Polypodium. By Mr. Anthony Van Leuwenhoeck, F.R.S. N° 297, p. 1868.

Being at a place where polypodium or fern grew, I viewed the seed vessels, which were in great numbers on its leaves; and when I placed some pieces of a leaf before the microscope, I observed the seed vessels appearing like so many round globules; and that they burst one after another before my eyes. And lately, in the middle of the last winter, there fell into my hands two leaves of fern, that grew upon the stump of an old willow tree, and were almost withered; which kind we call oak fern. This leaf had a great deal of seed on it, which was much larger than the seeds of the fern that grows in the country. Of these seed vessels, sometimes 13 of them lie in a row by one another.

Fig. 27, pl. 7, represents the largest part of an oak fern plant; which I call a leaf, because it has no root, but only a stalk; and as part of the leaf was divided into 17 branches, of which ABG is one, we may conclude that the whole leaf consisted of a great many more. I have often counted in one branch only of such a piece of a leaf, and in one row only of such a branch as expressed by AB, 13 of those little particles, which to the naked eye appear like roses; and consequently in the whole branch ABG, 26 of the same in two rows, which I shall call seed vessels; and when I separated one of these seed vessels from the rest, I observed that all of them had very short kind of stalks, by which they were fastened to the leaf, and by which they received their increase, but to the eye appeared no larger than a point, and the leaf seemed clearer or more transparent there, in which place I judge there was a canal or vein of the leaf.

Having separated one of these rose-like parts from the leaf, and the seed vessels of which it was composed, from one another, I found that the number of those seed vessels amounted to 149. Some have fewer. Almost all the seed vessels were open, and in some of them not the least appearance of any seed; being shed after it was ripe, and the vessel opened.

Fig. 28 represents one of those seed vessels, that was opened more than the rest, and by which the seeds, as I imagine, were thrown out, as it always happens in those seed vessels that open of themselves; for I could not perceive at the orifice the least rent or breach: the orifice is represented by LM. Between H and O, may be observed 8 screw-like parts; and of this configuration are all the seed vessels.*

I have met with some of the said fern leaves, in which the seed vessels had not been yet opened, and consequently had not shed their seeds. I have opened several of these seed vessels, and viewed the seeds with the microscope, it being impossible to see them with the naked eye, or even with common spectacles: I have counted above 50 seeds in one of those seed vessels, which seeds appeared through the microscope like those 3, PQR, in fig. 29.

Fig. 30 and 31, are the said seeds more magnified. In all of them one might perceive the spotted particles with which the seeds were adorned.

Fig. 32 represents another seed vessel, which was not open as the former: and whereas in fig. 28 the screw-like parts of the seed vessel are placed opposite to the sight, in this last the same parts appear sidewise. ABG represent the stalk; BCDEFG the seed vessel itself; and EF the opening of the vessel, by which it discharges its seed.

Fig. 33 is another seed vessel, that had not yet opened itself, and consequently in which the seeds are still shut up. HIM show the stalk, and IKLM the seed vessel, in which the screw-like parts are intercepted from the sight; and it is so placed that it cannot be perceived whereabouts the opening shall happen.

* The capsules or seed vessels of the fern-plants are provided with an elastic ring, produced from the pedicle on which they are supported. When ripe they open with a sudden jerk and scatter their seeds.—Withering from Hedwig.

The shell or skin of these seeds is composed of so exceedingly thin a membrane, that the wind, or even the seeds themselves, that are inclosed by it, would easily break it, were it not fortified or stiffened by those parts that I call its ribs or seams, which are much thicker than the rest of the membrane, and by which both the seed and the vessels themselves are preserved. To find what substance might be contained in these seeds, I contrived to break one of them, and found a great deal of oil coming out of it; and as each of these seeds was of a yellowish colour, so was the oil also, where it was a little thick; but where it was thinner, it was as clear as water; the other particles, that lay in and about the oil, were exceedingly minute. I made but one little hole or breach in the seed, at which to squeeze out the oil, and the other particles it contained; which having done, I observed that the skin or membrane of the seed had no colour at all in it; and then I could see a great many more figures in the said membrane, than were distinguishable in it before. Now when we consider the exceeding smallness of the abovementioned seeds, it is easy to conceive how, after being full ripe, they may be scattered abroad with a brisk wind, and some of them consequently may fall upon old rotten trees, and from thence receive their nourishment and increase.

Concerning Pewter Money, coined in Ireland, by the late King James. By Mr. Ralph Thoresby, F. R. S. N° 297, p. 1875.

I had a crown piece of pewter, inscribed melioris tessera fati, which was sent me by the gentleman who found a quantity of them in the Treasury at Dublin, and whose account of it is as follows: viz. "That King James, having turned all the brass guns of Ireland, and all the brass and copper vessels of protestants that he could seize, into coin, viz. half-crowns, somewhat larger than an English half-penny; shillings, broader, but not so thick as a farthing; and sixpences in proportion; it was ordered to pass current in all payments, even in bonds, and discharge of judgments aud statutes. But these stocks of metal being all spent, which he began to coin in June 1689, and no circulation to bring them back into his treasury, he called in all that he had coined, and the half-crowns, which before were stamped with a face, were re-stamped with his effigies on horseback, and then paid out to those who brought them in, as crowns; and the smaller coins were melted down, and re-coined again under the same denominations, but with less metal. After the turn was served by this stratagem, he had not wherewithal to import copper and brass; but, for want of it, fell foul on the pewter dishes, &c.—And the piece I sent you of that metal was coined for five shillings; and the proclamation to make it passable, was as ready as the stamps, for it was prepared: but King William passing the Boyne, prevented their proclaiming it. There was very little of it coined;

for our government could meet with none of it, until one day, rummaging all their tinkerly treasure, that they had left behind them in Dublin, when they were routed, by accident I met with one bag of 150 of those pieces. So that the piece I sent you, although it is of no intrinsic value, it is a rarity; and had I thought it would have been acceptable, I would have sent you a specimen of every sort that he had coined and re-coined here.

I am, Sir, your very humble servant,

Dublin, Nov. 27, 1696. Tho. Putland."

This valuable curiosity has the same inscription on both sides, as the common brass crowns; but there is this legend added upon the rim of it, Melioris Tessera Fati Anno Regni Sexto.

A Prospect of the Weather, Winds, and Height of the Barometer, on the First Day of every Month; and of the whole Rain in every Month, in the Year 1703, *and the beginning of* 1704; *observed at Towneley in Lancashire, by Rd. Towneley, Esq. and at Upminster in Essex. By the Rev. W. Derham, F.R.S.* N° 297, p. 1877.

The registers of the winds and weather are omitted as useless; on which Mr. Derham adds the following observations.

At Lisle, one year with another, the depth of the rain amounts to 22 inches 3 lines, Paris measure, or 23 inches 3 l. which makes about 23¼ inches English or 24½. At Paris, one year with another, they amount to 20 inches 3¼ lines, Paris measure, which is near 22 inches English. But at Towneley, one year with another, according to Mr. Towneley's computation formerly, the rains amount to above 41 inches depth. And by taking eight other years, in which the rain was observed both at Towneley and Upminster (viz. from 1696 to 1704) I find that all the 8 years rain at Towneley amounts to above 1700 l. Troy, at Upminster 823 l. only. Which said sums being divided by 8, give 212 l. ½, one year with another, at Towneley, and near 103 l. at Upminster. Each of which sums being doubled, and making a decimal fraction of the last figure, gives nearly the number of inches, which all the rain would have risen to, if the earth had stagnated, viz. 42½ inches at Towneley, and about 20¼ inches at Upminster. Wherefore the rain at Upminster is less than at Paris, at Paris less than at Lisle, and at every one of the places much less than at Towneley.

Notwithstanding the great disproportion of rain between one place and another, yet there is a great agreement between our barometers; one rising or falling when the other does, and that much or little, as the other does, though not always exactly in the same proportion.

Another thing shown by the foregoing table is, that there is some agreement between the winds at Towneley and Upminster. Which, though not always exactly in the same point, yet they often tend the same way, blowing within a point or two perhaps of the same course; especially when the wind is somewhat strong. Or if the winds have differed, yet the scud, as the seamen call the current of the clouds, has commonly shown the motion of the upper air to agree.

Astronomiæ Cometicæ Synopsis, Autore Edmundo Halleio, apud Oxonienses Geometriæ Professore Saviliano, et Reg. Soc. S. N° 297, p. 1882.*

Concerning Horn-like Excrescences growing on the Fingers, &c. By Dr. Rd. Wroe, Warden of Manchester College. N° 297, p. 1899.

Nathaniel Hulme of Bolton, 8 miles from Manchester, aged about 17, had the small pox at about 8 years old: soon afterwards, he had an itch, almost to the degree of a leprosy, whereby his fingers and thumb-nails began to grow thick, and by degrees hardened into horns; which grew in 7 or 8 months, some an inch in length, and some almost 2 inches, and others much longer. It began in the fore finger of his left hand, and so to all the rest of that hand. All which horns, at about the end of twelve months, fell off one after another, without any pain, unless when cut off, as they were at first, there appearing great quicks or roots under the nails. By degrees they came on the thumbs, and then on the fingers, of the right hand; which grew to the same length in about a year's time, and then fell off, he having shed them 5 or 6 several times. They are now at present all come off his left hand, but growing again: that on his little finger is 2 inches long. This account I took of him above two years since, in 1702; and have seen him frequently since, and lately, and the horns still grow, and fall off, as usual.

In a 2d letter dated Oct. 2, 1704, wherewith 2 of the horns which grew upon the boy's fingers were sent, Dr. Wroe mentions, that he had then all the fingers of both hands armed with the like, and some as long as those he sent. He had them on every toe also, but kept them cut, that he might be able to wear shoes. He adds that he saw him a few days before, and thought he could not live long, being miserably overspread with his leprosy.

* Dr. Halley's Astronomy of Comets is here omitted, as it has been elsewhere published in a much fuller and more complete state, and that both in the English and Latin languages: as in the Miscellanea Curiosa, of Dr. Halley, in 3 volumes 8vo. in English; also translated and published in English, by G. T. Gent. in 8vo. 1757; and still more complete and perfect in Dr Halley's Astronomical Tables, in 4to. both in Latin and English.

Concerning some Swedish Coins; and a Calculation for finding Easter. By Mr. Ralph Thoresby, F.R.S. N° 297, p. 1901.

I have a Swedish coin, or rather square copper plate, 9 inches broad, and 9 and a half long, which is much like the Roman Æs grave, and was current there so lately as An. 1679, though now they are not to be met with. It has at each corner the impression of a crown, under which is the year, and round it this inscription, Carolus XI. D. G. Sve. Got. Wan Rex. and in the middle of the plate in a circle 2 Daler. Söih. Mijt. The other side of the plate has no inscription.

The learned Richard Thornton, Esq. on reading Dr. Wallis's letter to Sir John Blencowe, concerning the observation of Easter, in Phil. Trans. N° 240, made this calculation,

Rightly to understand the Rule in our Common-Prayer Books for finding Easter.

Note, 1. That the 21st of March, in all but Leap-years, and in Leap-years the 20th of March, was, at the time of the council of Nice, when this rule was made, the vernal equinox: consequently, 2. That the 20th of March in Leap-years is the same as the 21st of March in common years. 3. That the full moon meant in this rule is not to be found in our almanacs, but by the calendar of our common prayer books, where, in the first column, the golden number of every year is placed over against the day of the new moon in every month of the year. 4. That the 14th day, including the 1st day of the moon, is the full moon, and not the 15th, as Dr. Wallis would have it in his letter.

An Experiment, made at Gresham College, on the Propagation of Sound in condensed Air: with a Repetition of the same in the open Field. By Mr. F. Hauksbee. N° 297, p. 1902.

A bell being included in a brass recipient, and placed at one end of a room about 50 yards long, at the other end of which stood some gentlemen to observe the sound; before any air was intruded, the bell on shaking was heard at that distance, though not without diligent attention. On the intrusion of one atmosphere of air, the bell being shaken as before, the sound was very sensibly augmented; two atmospheres being impelled, and the bell made to ring, a considerable improvement of the sound was then manifest. But on the intrusion of the 3d, 4th, and 5th atmospheres, the bell being made to strike, the sound seemed not to be propagated proportionably to the first and second; which might happen by means of some escapes of air, or the valve, which should have hindered the return of the injected air, not holding so tight as it ought; but that some of the intruded air would repass into the syphon, by which means the quantities supposed injected were deficient. Another reason why the latter atmospheres did not propagate the sound as the two first, is this; though 25 compressions of the syphon are equal to the natural content of the recipient, yet when the air becomes pretty strongly condensed, as by the intrusion of 4 or 5 atmospheres, the remaining air at every stroke, which will lie between the bottom of the embolus and the valve (though but little) is at the same density at the same time as the air in the recipient; which, on drawing up the forcer, will extend itself, to supply such a space of the cylinder as so much common air; and is so much short of what should be injected at every stroke, 25 of which become equal to the natural content of the receiver. Hence the deficiencies of the real atmospheres, or quantities, by a certain number of strokes, may be very considerable, and to account for them, very difficult. Notwithstanding the deficiencies are not known, yet at the end of the 5 (though imperfect) atmospheres, the bell being shaken as at first, it was the opinion of a gentleman then 50 yards distant, that the sound seemed to be almost as sensible as when it came to be made and exposed in the open air.

June the 9th, about 5 in the morning, I repeated this experiment in an open field, called the White Conduit, with much the same success as the former. On shaking the bell before any air was intruded, it was but just audible at 30 yards distance. On the injection of one atmosphere, it became then as audible at 60 yards, as it was before at 30. A second being intruded, the bell, on shaking, might then be heard at 90 yards distance. But after that, though near 100 strokes of the forcers were repeated, yet could it hardly be heard 20 yards farther. Which I attribute in a great measure to the reasons before given.

An Experiment made at a Meeting of the Royal Society, on the Diminution of Sound in rarefied Air. By Mr. Fr. Hauksbee. N° 297, p. 1904.

A bell being included under a receiver, which being shaken to make the clapper strike, it was very observable that the interposition of the glass between the bell and the ear, was a great obstruction to its sound, yet it was audible at some good distance from it: but gradually withdrawing the air, and making several stops to shake the bell at different degrees of rarefaction, the diminution of the sound at every stop was very distinguishable. Till at last, when the receiver was well exhausted of air, the remains of sound was then so little, that the best ears could but just distinguish it: it appearing to them like a small shrill sound as at a great distance. On suffering the air gradually to re-enter, it was easy to perceive the increase of sound at the different times the bell was made to ring: the recipient being again replete with air, the sound then seemed something more clear and audible than at its first inclusion.

Concerning the Figures of the Salts of Crystal. By M. Leuwenhoeck, F. R. S. N° 298, p. 1906.

M. Valkenier, from Switzerland, showed me some crystals that were very remarkable; one as large as my fist, which looked like several small flint-stones joined together; they seemed to be united by very transparent particles, which might be called wild crystal; the largest prominent points of which seemed no larger through the microscope than a large grain of sand; many of them appeared to be hexangular, like the mountain crystal. The same gentleman further told me, that in some of the highest mountains in Switzerland, the large and small stones were almost mixed or united with the wild crystal; which mountains are therefore called by the inhabitants nagelfelsen, that is, the nail mountains or rocks, by reason of the great number of prominent points in them.

I struck off 4 or 5 small flint stones from the great piece; in doing which, several of the wild crystal particles came off together; to which I placed a microscope, and observed that almost each of them was of a different figure, though most of them ran into a hexangular point, of several sizes; in breaking them from one another, the sides assumed particular figures, which looked as if they were composed of nothing but long particles, all of them as bright and transparent as any crystal, excepting only in those parts where they were joined to the stones. The crystal particles lay very thick upon one another; and they were so small, that 100 and more were not equal to one of our crystal particles; among others, I observed some no larger than a coarse grain of sand, which were composed of 100 other smaller particles, all very transparent, and appearing through the glass like a little mountain of crystal, forming a very agreeable spectacle.

I saw likewise several crystal particles, which, instead of ending in one hexangular point, like others, consisted of several sharp ones, some of which were very different both in length and thickness from the rest; which confirmed my opinion, that they were coagulated out of long particles.

I observed, that where the little flint stones had not lain very close to one another, the void space, as it appeared to the naked eye, had also been filled with the crystal matter; but when I came to view them more narrowly, I could perceive that those crystal parts were indeed united to the little flint stones, but that there were several small cavities between them; from whence I concluded, that the flint stones lying so upon one another, as we now find them, a fluid crystalline matter, out of which all the crystals were coagulated, had insinuated itself between the flints; and where those stones were not so closely joined, there did not happen to be enough of the crystalline matter to fill up the vacant space.

Fig. 1, pl. 8, shows one of the crystal particles; where may be observed at ABC, and partly also between D and F, how it had been joined, and broken off from other crystal particles; between DEF is that part from whence proceeds several prominent points.

Fig. 2, represents a larger particle of crystal, which had been joined to another particle lying by it; viz. GHIKO show that part which had not been united, and in which the several prominent points are represented by LMNGO.

From the softness of these crystals, I imagined, that what we call wild crystal is mostly salt, if perhaps it be not all so. And when burnt in a strong fire, and thrown into water, it immediately fell down into a fine white powder, like lime. And by evaporating some of the water, there remained innumerable multitudes of salt particles, of all different forms, which coagulated, and shot in crystals again. And the same happened on dissolving the crystals, by boiling them in water over the fire, and then evaporating the water again.

Fig. 3 shows a small figure of a salt, in which such a salt particle lay, that according to all appearance was at first coagulated, and afterwards arrived at that size when the water was quite exhaled. Fig. 4 is another salt particle, having likewise within it a coagulated salt particle, which at R has 2 right angles, and at S an acute one, both its long sides running parallel, and at equal distance from each other; there was a great many of this shape. Fig. 5 shows a hexangular figure, in which also lay a smaller of the same shape; which inclosed figure was very thick. Fig. 6 is a salt particle, whose superficies made an exact square: of these figures I met with very few, especially where the water had lain thick. Fig. 7 shows a form, of which I discovered an exceedingly great number, though it was very thin. Fig. 8 shows also another salt particle, of which there was an unspeakable number coagulated in the water.

Having made a great many experiments on the last mentioned water, till it was quite exhaled; and having also poured new water twice over the wild crystal, I saw with great surprise, that each time there was a coagulation of new salts on the surface of the water; but most of them so very small, that they appeared through the microscope no larger than a grain of sand to the naked eye; and yet I could perceive the shapes of them very clear, and the more, because they were all very transparent; and whereas a great many salt particles are so soft, that in moist weather they are dissolved into a watery vapour, these were so hard and so dry likewise, as if they had been the salts of chalk; and if one shook the glass in which they were, they came off from it, especially those salt particles that were pretty large.

After these observations I took another view, by the help of a microscope, of several pieces of my mountain or rock crystal, just as I had broken it off from the stone, in which I had formerly discovered, amongst others, some such figures as are represented in fig. 5; and now observing again a hexangular piece of crystal, which ended also in a point of the like shape, I discovered in it several pointed small blue crystals, as in fig. 9. These were of several sizes, and some of them a finer blue than the rest.

I also discovered in another piece of crystal much the like figures, but none of them were blue, some of which were as transparent as crystal, and others again as dark as if they had been nothing but a blackish earth; some of them lay much deeper in the crystal than the others.

I had one piece of crystal, in which I counted above 30 little blue figures, such as in fig. 9, and some of them so very small, that they appeared no larger through a microscope than a grain of coarse sand to the naked eye; and as far as I could judge of them, they were most of them hexangular; but I observed, that some of them were not so perfect and regular as the rest; one point of them being larger than the others, as in fig. 10.

A Register of the Weather for the Year 1692, *kept at Oates in Essex. By Mr. John Locke.** N° 298, p. 1917.

This is a register of the state of the barometer, thermometer, and hygrometer,

* From the place (Oates) and date of this register, as well as from the style of the few lines of introduction, it appears to have been drawn up by the great Mr. Locke, during his retirement at Lady Masham's seat at Oates in Essex. This celebrated philosopher and politician was born at Wrington, near Bristol, in 1632, and educated at Westminster school; from whence he removed to Christ Church, Oxford, of which he became student. After taking his degree in arts, he pursued the study of physic, in which he made great proficiency, and had some practice. But his tender constitution but ill suiting with the fatigues of that profession, he accepted the offer of going abroad as secretary to Sir Wm. Swan, envoy to the elector of Brandenburg.

This employment continuing only for a year, he returned to Oxford, where he was prosecuting his medical studies, when, in 1666, an accident brought him acquainted with lord Ashley, afterwards earl of Shaftesbury. That nobleman conceived so high an opinion of his abilities, that he took him into his confidence, and persuaded him to turn his attention to politics; an advice which he so well profited by, that he acquired great celebrity, and was consulted by the most distinguished persons of his time. About 1669 Mr. Locke attended the earl and countess of Northumberland into France; but that nobleman dying at Turin, Mr. Locke, who was left in France to for every day, from the 9th of Dec. 1691, to the end of the year 1692. It is neatly and orderly kept, in 7 columns; of which the 1st column is the day of the month; column 2 the hour of the day; column 3 the degree of the thermometer; column 4 the barometer; column 5 the hygrometer; column 6 the wind; and column 7 the weather: the particulars of which are now no longer of any use.

An Account of the Lake Wetter, in Sweden. By Dr. Urban Hearne, F.R.S. N° 298, p. 1938.

The lake Wetter, extends north and south about 80 English miles in length, and from 10 to 20 in breadth. It divides Gothland into two parts; the one, situated to the east, is called Ostrogothland, and the other, to the west, Westrogothland. But there is nothing in this account of the lake Wetter, beyond what may be gleaned from gazetteers and other books of geography and travels.

attend the countess, returned the year following, with that lady, to England. On his return, he lived, as before, at lord Ashley's; who having, jointly with some other lords, obtained a grant of Carolina, engaged Mr. Locke to draw up the fundamental constitutions of that province.

From a disgust at the Aristotelian philosophy, and not satisfied with that of Descartes, Mr. Locke began to form the plan of his celebrated "Essay on Human Understanding," in 1671; but was prevented at that time from making any great progress in it, by an appointment to the office of secretary to the presentations, by his patron, now become the Lord Chancellor. But the year following lord Shaftesbury being displaced, Mr. Locke followed him. The office of secretary to the commissioners of trade however, he enjoyed till the board was dissolved in 1674. He then went to Montpelier for his health, where he remained a considerable time. After his return, he accompanied his patron to Holland, whither his lordship was obliged to fly, to avoid a prosecution for high treason. In 1684, Charles the 2d ordered Mr. Locke to be removed from his student's situation at Christ Church: and the year following, the English envoy at Holland demanded him of the States-General, on suspicion of his being concerned in Monmouth's rebellion. On this he kept retired for several months, during which he was employed in preparing for the press his Essay on Human Understanding, which was completed in 1687, though it did not appear in public till after the Revolution, an event which restored him to his native country, he having accompanied the princess of Orange to England. Soon after his arrival he was made commissioner of appeals; and in 1695 he obtained a seat at the board of trade. He might have had other preferment, but the state of his health induced him to decline all the advantageous offers that were made him, and to prefer spending the latter years of his life in a calm philosophical retirement at the seat of lady Masham, at Oates in Essex, where he composed several of his later works, and where he died in 1704, in the 73d year of his age. His remains were interred in the church of that place, where a monument was erected to his memory. But the highest eulogium on his character was performed by queen Caroline, consort to George the 2d, who erected a pavilion, in Richmond-Park, in honour of philosophy, where she placed our author's bust on a level with those of Bacon, Newton, and Clarke, as the four principal English philosophers. His works, which are very numerous, solid, and profound, in philosophy, politics, theology, &c. have been often printed, both separately and collectively; and for depth and originality of thought, closeness of reasoning, and a style peculiarly adapted to the various subjects of which this philosopher treated, they ever have been and ever must be held in the highest estimation.

Experiments on the Resilition of Bodies in common Air, also in Vacuo and in condensed Air; made at a Meeting of the Royal Society at Gresham College. By Mr. Francis Hauksbee. N° 298, p. 1946.

Having provided a tall glass receiver, in the upper part of which I had a contrivance to lodge 4 marbles, and from whence I could at pleasure drop them successively on a plane. The distance descended by each marble to the plane, was about 13½ inches; the weight of two marbles 59 grains, of the other two 63 grains; being all of the sort usually sold at toy shops. The plane on which they fell, was a round flat piece of solid glass, about an inch thick, and 3½ over the upper surface being well ground and polished. It was fixed in a tin frame, contrived on purpose to keep its lower surface from being contiguous to the plate or leather, on which the recipient was placed. On dropping the marbles on the said glass plane, their resilition then was something more in vacuo than in common air; and those dropped in common air had likewise some small advantage in their rebound, above those let fall in condensed air; the condensation being but one atmosphere; not daring to venture more, the breaking of the recipient being very hazardous. The resilition of the marbles from the plane in vacuo was about 10½ inches, which was something more than ¾ of their descent: in condensed air their rebound was about 10 inches: so that we must account their resilition in common air to be the medium of the other two, it being difficult to judge to a nicety in so sudden a motion. But there was a sensible difference between the rebound of those dropped in vacuo, and those in condensed air. I could not observe that the small difference in the weight of the marbles made any discernible alteration in their resilitions.

An Experiment on the Descent of Malt Dust in the evacuated Receiver, at Gresham College. By Mr. Fr. Hauksbee. N° 298, p. 1948.

I took some malt dust, and having dried it well, put a quantity of it into a fine muslin bag, where being loosely inclosed, it would upon shaking discover itself plentifully in the open air, undulating and floating a considerable time before it would descend; but being included within a receiver, from which the air was well exhausted, and then shaken, the dust decended like a ponderous body, precipitating in straight lines from the top to the bottom of a tall receiver.

An Account of Animals and Shells sent from Carolina to Mr. James Petiver, F. R. S. N° 299, p. 1950.

Among the crustaceous animals Mr. P. gives (Sect. I.) a description of the 5-holed Carolina, sea-urchin or fritter: echinus compressus Carol. &c. Musei Nost. 125 fig.

This is a very singular animal, having 5 oblong perforations through the body of the shell, viz. 4 against as many points of a 5-rayed star, lying in the midst of the shell; the other is placed between two of its beams: these are of different magnitudes, having seen them from less than 2 inches to almost 4 diameter, and this last, in the midst, where its thickness exceeds not half an inch, and gradually to its edges, which are thinner than a shilling.

He afterwards gives an account, in Sect. II. of several species of muscles, cockles and other bivalves; in Sect. III. of some patellæ, cochleæ, buccina and other single shells; and lastly, (Sect. IV.) of two glossopetræ; viz. glossopetra Carol. maxima nigrescens ponderosa. This is one of the largest he ever yet saw, being above three inches from the tip to the hollow or middle of the root; this last part is very large and protuberant: the teeth black, and smooth on the edges, but in the middle much furrowed; the whole very weighty.

Glossopetra Carolin. leviter serrata é nigro rubroque eleganter variegata. This is almost a straight tooth, i. e. very little waving to either side, as most of them more or less do; its edges are rather pounced than notched, from the tip to the middle of the root near an inch and half; its toothy part is finely variegated with red and black.

The Doctrine of Combinations and Alternations, Improved and Completed. By Major Edward Thornycroft. N° 299, p. 1961.

In order to understand what follows, it must be observed, 1st. That as in the notation of powers, *aaaabbbcc* is designed by $a^4b^3c^2$, and universally p times the position of a, q times the position of b, r times the position of c, by $a^pb^qc^r$, so in things exposed likewise: (unless where it is proposed they should be all different) which indices, as they have here no relation to powers, but express only the occurrences of those things to which they respectively belong, I therefore call indices of occurrences.

2dly, That as often as I shall hereafter mention the combinations or alternations of the p^s, q^s, r^s, or s^s, which considered by themselves are capable of no variation, I mean of those things whose indices they are.

3dly, That m is generally put for the whole number of things exposed, whether all different or not, i. e. equal to the sum of their indices; and n, for such a number of them, as each combination and alternation must consist of: unless presupposed equal; which explains what is hereafter meant by the combinations and alternations of m things taken n and n; or of m things taken m and m; and the like expression, by whatever symbols the number of things out of which the combinations and alternations are to be made, or of which they are to consist, may be designed.

Lemma 1.—If in a right line, at any distances, be placed any number of things, *abcd*, &c. the number of the intervals *ab*, *bc*, *cd*, &c. terminated each by two adjacent things, is one less than the number of things. For, whereas every interval is terminated by two adjacent things, if to any number of things, be added one thing more, one interval only is thereby added. Q. E. D.

Lemma 2.—The number of the alternations of m things *abcd*, &c. different from each other, taken m and m, is m times the number of the alternations of $m - 1$ things *abc*, taken $m - 1$ and $m - 1$. For, by lem. 1st, the last letter d, besides the position it has, may have $m - 2$ positions, viz. in the intervals which are between $m - 1$ things *abc*; but it may also have one more, for it may be put first of all, it may therefore have m positions; and those in all the different orders, whereof $m - 1$ things are capable; which being all the possible positions of d, in all the varieties of *abc*, is all the variety whereof the whole number of things exposed *abcd*, &c. is capable. Q. E. D.

Lemma 3.—The number of the alternations of m things *abcd*, &c. different from each other, taken m and m, is equal to $m \times m - 1 \times m - 2 \times m \times 3 \times m - 4$, &c. continued to m places. For let mo, express the number of the alternations of m things different each from other; $\overline{m - 1}o$, of $m - 1$ things, and the like. It is evident that if $m = 1$, it will be $mo = m$; for there can be but one order of one thing. And if m be greater than unity, then will it be, by lem. 2, $mo = m \times \overline{m - 1}o = m \times m - 1 \times \overline{m - 2}o = m \times m - 1 \times m - 2 \times \overline{m - 3}o =$, &c. till we have an equation consisting of m places; i. e. $= m \times m - 1 \times m - 2 \times m - 3 \times$ &c. continued to m places. Q. E D.

Lemma 4.—If $m\omega$ express the number of the alternations of m things $a^p b^p c^p d^q e^q f^r$ &c. taken m and m; and α the number of p^s, β the number of q^s, γ the number of r^s; it will be $m\omega =$

$$\frac{m \times m - 1 \times m - 2 \times m - 3 \times m - 4 \times m - 5 \times \text{\&c. continued to } m \text{ places}}{\overline{p \times p - 1 \times p - 2 \times \text{\&c.}}^{\alpha} \times \overline{q \times q - 1 \times \text{\&c.}}^{\beta} \times \overline{r \times r - 1 \times \text{\&c.}}^{\gamma} \text{ each series continued to}}$$

p, q, r, &c. places respectively.

For the number of the alternations of any number of things, however divided into parts, is produced by a continual multiplication of the alternations of those things among themselves respectively, which compose each part, into the number of their alternations one among the other; i. e. in the present case (the several occurrences being supposed to compose the several parts, and consequently the number of the alternations of the things composing each part equal to unity) $m\omega =$ to the number of the alternations of the things composing the parts one among the other; but the number of their alternations one among the other, is the same in this case, as if the things exposed, being all different, were divided into the same parts; for the things which compose each part in both cases, are different from the rest of the things exposed; i. e. by lem. 3d,

$$m\omega = \frac{m \times m-1 \times m-2 \times m-3 \times m-4 \times m-5 \times \text{\&c. continued to } m \text{ places}}{\overline{p \times p-1 \times p-2 \times \text{\&c.}}|^{\alpha} \times \overline{q \times q-1 \times \text{\&c.}}|^{\beta} \times \overline{r \times r-1 \times \text{\&c}}|^{\gamma}}$$ each series continued to p, q, r places respectively. Q. E. D.

Lemma 5.—The number of the combinations of m things *abcd*, &c. different from each other, taken n and n; is equal to $\frac{m \times m-1 \times m-2 \times m-3 \times \text{\&c.}}{n \times n-1 \times n-2 \times n-3 \times \text{\&c.}}$, each series continued to n places. For if the things exposed be divided into two parts, viz. in the ratio of n and $m-n$; it is evident that their different combinations taken n and n, are produced by the alternations of the things composing the parts one among the other: and therefore the number of those $=$ to the number of these $=$ to the number of the alternations of m things taken m and m, the indices of whose occurrences are n and $m-n =$

$$\frac{m \times m-1 \times m-2 \times m-3 \times \text{\&c. continued to } m \text{ places}}{n \times n-1 \times \text{\&c.} \times m-n \times m-n-1 \times \text{\&c. each series continued to } n \text{ and } m-n \text{ places}}$$

respectively by lem. 4th, i. e. because $n + m - n = m$, $=$

$$\frac{m \times m-1 \times m-2 \times m-3 \text{ \&c.}}{n \times n-1 \times n-2 \times n-3 \text{ \&c.}}$$ each series continued to n places. Q. E. D.

But the number of the alternations in every combination, is $= n \times n-1 \times n-2 \times n-3 \times$, &c. continued to n places, by lem. 3; therefore,

Lemma 6.—The number of the alternations of m things *abcd*, &c. different from each other, taken n and n, is $= m \times m-1 \times m-2 \times m-3 \times$, &c. continued to n places. Q. E. D.

Scholium.—Since, in the things exposed, the same things may occur more than once, and also n be less than m, the indices of the occurrences which are in some of the combinations of m things, taken n and n, may differ from those which are in others; but those combinations, the indices of whose occurrences are the same, are said to be in the same form: therefore, whereas n is equal to the sum of the indices which are in each combination taken n and, n if n be expressed by all the different combinations of such indices only (being integer numbers) whereof no one may exceed the highest index of the things exposed, and being more than one in a combination, are each of them, which are in the same combination, comprehended in a distinct index thereof; these expressions of n will necessarily be the several forms of the combinations taken n and n, whereof m things are capable; whence is derived a general theorem for finding the combinations and alternations of m things taken n and n universally: i. e. whether m consist of things all different or not, and whether n be equal to, or less than m.

Theorem.—If n be expressed, according to all the different forms of combination which the things exposed are capable of,

$$\text{and}\left\{\begin{array}{l} p = \text{the highest index} \\ q = \text{the next highest} \\ r = \text{the next highest} \\ s = \text{the next highest} \\ \text{\&c.} \end{array}\right. \left\{\begin{array}{l} \alpha = \text{the number of } p^s \\ \beta = \text{the number of } q^s \\ \gamma = \text{the number of } r^s \\ \delta = \text{the number of } s^s \end{array}\right\} \begin{array}{l} \text{in every form of} \\ \text{combination;} \end{array}$$

$$\left\{\begin{array}{l} \text{A} = \text{the number of all the indices not less than } p \\ \text{B} = \text{the number of all the indices not less than } q \\ \text{C} = \text{the number of all the indices not less than } r \\ \text{D} = \text{the number of all the indices not less than } s \\ \text{\&c.} \end{array}\right\} \begin{array}{l} \text{which are in the things} \\ \text{exposed;} \end{array}$$

and $b = \alpha + \beta$, $c = b + \gamma$, $d = c + \delta$, &c.

Then the number of the combinations of m things taken n and n, in any one form of combination, shall be $\frac{\text{A} \times \text{A} - 1 \times \text{A} - 2}{\alpha \times \alpha - 1 \times \alpha - 2}$ &c. $\times \frac{\text{B} - \alpha \times \text{B} - \alpha - 1}{\beta \times \beta - 1}$ &c. $\times \frac{\text{C} - b \times \text{C} - b - 1}{\gamma \times \gamma - 1}$ &c. $\times \frac{\text{D} - c \times \text{D} - c - 1}{\delta \times \delta - 1}$ &c. continued to so many terms as there are different indices in the form of combination, and each term to α, β, γ, δ, &c. places respectively; and this number multiplied into

$$\frac{n \times n - 1 \times n - 2 \times n - 3 \times n - 4 \times n - 5 \times n - 6 \text{ \&c. continued to } n \text{ places}}{\overline{p \times p - 1 \times p - 2 \times \text{\&c.}}|^{\alpha} \times \overline{q \times q - 1 \times \text{\&c.}}|^{\beta} \times \overline{r \times r - 1 \times \text{\&c.}}|^{\gamma} \times \text{\&c.}}$$

each series continued to p, q, r, &c. places respectively, shall be the number of their alternations.

But the sum of all the combinations and alternations which are in every form of n, shall be the whole number of combinations and alternations of m things taken n and n.

Demonstration.—1st, Then it is evident, that those combinations, which are in different forms, differ from each other. Again, it is evident that the combinations of m things, as $a^p\, b^p\, c^p\, d^p\, e^q\, f^q\, g^q\, h^r\, i^r$, &c. (the indices simply considered) taken n and n, in a form wherein are p^s, q^s, and r^s, shall be equal to the number of the combinations of the p^s, which are in the things exposed, taken

α and α, multiplied into the number of the combinations of the q^s taken β and β multiplied into the number of the combinations of the r^s taken γ and γ.

But because p and all lesser indices are comprehended in every index which is greater than themselves; therefore is A = to the number of p^s which are in the things exposed; and for the same reason, would B = the number of the q^s, and C the number of r^s: but the number of the p^s, which are in every form of combination, is $= \alpha$; therefore is $\text{B} - \alpha =$ to the number of q^s; also because the number of p^s and q^s together, which are in every form of combination, wherein there are q^s, is $= \alpha + \beta = b$; therefore is $\text{C} - b =$ to the number of r^s; and so on, how many soever may be the different indices in any form of combination.

But, by lem. 5th, the number of the combinations of the p^s, which are in the things exposed, whose number is A, taken α and α, is $= \frac{\text{A} \times \overline{\text{A} - 1} \times \overline{\text{A} - 2}}{\alpha \times \overline{\alpha - 1} \times \overline{\alpha - 2}}$ &c. continued to α places; and the number of the combinations of the q^s, whose number is $\text{B} - \alpha$, taken β and β, is $= \frac{\overline{\text{B} - \alpha} \times \overline{\text{B} - \alpha - 1} \times \overline{\text{B} - \alpha - 2}}{\beta \times \overline{\beta - 1} \times \overline{\beta - 2}}$ &c. continued to β places; and the number of the combinations of the r^s, whose number is $\text{C} - b$, taken γ and γ, is $= \frac{\overline{\text{C} - b} \times \overline{\text{C} - b - 1}}{\gamma \times \overline{\gamma - 1}}$ &c. continued to γ places. Q. E. D.

But every combination, in one and the same form, affords the same number of alternations; therefore the number of alternations, in any one form, is so many times the number of combinations, as is the number of alternations in any one of these combinations. But, by lem. 4th, the number of alternations in any of those combinations, shall be

$$\frac{n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3} \times \overline{n-4} \times \overline{n-5} \times \overline{n-6} \times \text{\&c. continued to } n \text{ places}}{\overline{p \times \overline{p-1} \times \overline{p-2} \times \text{\&c.}}|^{\alpha} \times \overline{q \times \overline{q-1} \times \text{\&c.}}|^{\beta} \times \overline{r \times \overline{r-1} \times \text{\&c.}}|^{\gamma} \times}$$, each series continued to pqr, &c. places respectively. Q. E. D.

Now to make an application of this general rule to those particular cases which have already been considered by others, and which are contained in our 3d, 4th, 5th and 6th lemmas, and by us more generally demonstrated; I say if $n = m$, there can be but one form of combination, and but one combination in that form; and therefore the number of alternations =

$$\frac{m \times \overline{m-1} \times \overline{m-2} \times \overline{m-3} \times \overline{m-4} \times \text{\&c. continued to } m \text{ places}}{\overline{p \times \overline{p-1} \times \text{\&c.}}|^{\alpha} \times \overline{q \times \overline{q-1} \times \text{\&c.}}|^{\beta} \times \overline{r \times \text{\&c.}}|^{\gamma} \times}$$ &c. each series to p, q, r, &c. places respectively, i. e. (if $p = 1$) $= m \times \overline{m-1} = \overline{m-2} \times \overline{m-3} \times \overline{m-4} \times$ &c. continued to m places, which are the cases of the 4th and 3d lemmas.

But if the things exposed be all different, and n be less than m, which is the case of the 5th and 6th lemmas; then also can there be but one form of combination, and it will be $A = m$, and $\alpha = n$, and the whole number of combinations $= \frac{A \times A - 1 \times A - 2 \times \&c.}{\alpha \times \alpha - 1 \times \alpha - 2 \times \&c.}$ i. e. $= \frac{m \times m - 1 \times m - 2 \times \&c.}{n \times n - 1 \times n - 2 \times \&c.}$ each series continued to n places, and therefore the number of alternations $= m \times m - 1 \times m - 2 \times$ &c. continued to n places.

But fully to illustrate this theorem, which, as delivered in general, may seem somewhat too abstracted, to be commonly understood; I shall therefore subjoin one short example.

Example.—Let the things exposed be *aaabbbcc*, or according to our way of notation $a^3\ b^3\ c^2$: it is required to find the number of their combinations and alternations, taken 4 and 4.

Then (because in the things exposed, there is no one thing occurs more than thrice, nor more than three things different from each other) will all the forms of combination, which the things exposed are capable of, be these,

$$\text{viz.} \left\{ \begin{array}{c} 3\ .\ 1 \\ 2\ .\ 2 \\ 2\ .\ 1\ .\ 1 \end{array} \right\} \text{Then}$$

In the 1st form will $p = 3$, $q = 1$, $\alpha = 1$, $\beta = 1$, $A = 2$, $B = 3$.
In the 2d form will $p = 2$, ——, $\alpha = 2$, ——, $A = 3$, ——.
In the 3d form will $p = 2$, $q = 1$, $\alpha = 1$, $\beta = 2$, $A = 3$, $B = 3$.

The number of combinations in the 1st form $= \frac{A}{\alpha} \times \frac{B - \alpha}{\beta} = \frac{2}{1} \times \frac{2}{1} = 4$.

The number of combinations in the 2d form $= \frac{A \times A - 1}{\alpha \times \alpha - 1} = \frac{3 \times 2}{2 \times 1} = 3$.

The number of comb. in 3d form $= \frac{A}{\alpha} \times \frac{B - \alpha \times B - \alpha - 1}{\beta \times \beta - 1} = \frac{3 \times 2 \times 1}{1 \times 2 \times 1} = 3$.

And the whole number of combinations $= 10$.

Also the number of alternations in the

1st form $= 4 \times \frac{n \times n - 1 \times n - 2 \times n - 3}{p \times p - 1 \times p - 2|^{\alpha} \times q^{\beta}} = 4 \times \frac{4 \times 3 \times 2 \times 1}{3 \times 2 \times 1^{1}} = 4 \times 4 = 16$.

2d form $= 3 \times \frac{n \times n - 1 \times n - 2 \times n - 3}{p \times p - 1|^{\alpha}} = 3 \times \frac{4 \times 3 \times 2 \times 1}{2 \times 1|^{2}} = 3 \times 6 = 18$.

3d form $= 3 \times \frac{n \times n - 1 \times n - 2 \times n - 3}{p \times p - 1|^{\alpha} \times q^{\beta}} = 3 \times \frac{4 \times 3 \times 2 \times 1}{2 \times 1|^{1} \times 1^{2}} = 3 \times 12 = 36$.

And the whole number of alternations $= 70$.

Many are the properties of this theorem in common with others. As, to find the unciæ of a multinomial raised to any integer power. To raise an infinite series to an integer power, though of an interrupted order, without introducing any thing immaterial, or which must afterwards be expunged: and many others. But then so many terms of the series must be taken in at first, as shall serve to the purposes of the intended approximation, otherwise as often as it shall fall short of that, the operation must be begun de novo. Many likewise are the properties peculiar to this theorem, and great variety of problems might be framed; and I scruple not to say, many may occur in practice, which are solvable by this, and no other method whatever.

Hence may be found the number of words whereof the 24 letters are capable, from one letter in each word, to any number of letters given.

Hence may be found the number of all numbers, to any given number of places, which may be produced from any number of figures given.

Hence also the compass of a musical instrument being given, the time and number of the bars, whereof each tune shall consist, the number of tunes may be found which that instrument is capable of.

To give an instance of the prodigious variety that there is in music, I have calculated the number of tunes in common time, consisting of eight bars each, which may be played on an instrument of one note compass only, and it is this, viz. 27584.270157.013570.368586.999728.299176; whereas the changes on 24 bells are only 620448.401733.239439.360000, which is but the $\frac{1}{444588.604583}$ part of the number of tunes; and yet Dr. Wallis, in his algebra, demonstrates, they could not be dispatched in 31557.600000.000000 years. If then the instrument were of as many notes compass as any instrument now in use, how prodigiously must the number of tunes be increased; the calculation of which (though much more intricate and operose) would be equally attainable by our theorem.

Of Ossifications or Petrifactions in the Coats of Arteries, particularly in the Valves of the Great Artery. By William Cowper, Surgeon, and F. R. S. N° 299, p. 1970.

How far anatomical inquiries inform us respecting the true seats and causes of diseases, which have been ascribed to the want of spirits in some, and of radical moisture in aged people, &c. may be in some measure seen by two observations, among others, published in the Trans. N° 280: the first there mentioned is of a young gentlewoman, in whom the parietes, or membranes, that compose the trunks of the arteries of the arm near the axilla, being very much thickened, so that the diameter of its bore was lessened to more than a third part of its natural size; insomuch that a part of the trunk of the artery cut transversely, very much resembled a bit of the stem of a tobacco-pipe, its sides were so thick, and its bore consequently so much lessened: the other was of the trunks of the arteries of the leg, that were obstructed by petrifactions or ossifications, in a person about the age of 67. Since which, I have met with several of the like instances in aged people, particularly in the legs of an old gentleman; whose toes and foot were sphacelated, in which the ossifications diminishing their channels in some places, and totally obstructing them in others, is made very evident.

The dissections of morbid bodies not only instruct us in the seats and causes of diseases, but very often inform us in the true use of parts, as will appear by the following instances. The ossification or petrifaction in the great artery, at its rise from the heart, has been so commonly found, that some think it is constant; how it may be in some animals I cannot be certain, but in human bodies I am well assured that whenever it happens, it is a disease, and in some measure incommodes those parts in the due execution of their office, as the following cases will evince: but that this paper may be of some use, I shall set down the symptoms before death, which may help our conjectures when the like offers again. A thin man about 30, who languished with an ulcer in the thigh, attended with a caries, or rottenness of that bone, at its articulation with the tibia and patella, called the knee, where all those bones were affected, at length fell into a true phthisis, and coughed up no small quantity of pus; some months before his death I frequently saw him, when he would often offer me his wrist, to feel his unequal pulse, which was wont to amuse him; the artery there missing sometimes one, sometimes too strokes in six or seven; at first he told me he observed it missed but one in ten, but at length those stops became more frequent, especially on any agitation of the body or mind, though a polypus in any of the great vessels about the heart may induce that symptom, yet its continuance so long before death, shows it owing to some other cause, as appeared, on opening the heart and great artery of this person.

Pl. 8, fig. 11, represents the trunk of the great artery opened and displayed. aaa the three semilunar valves of the aorta, which hinder the blood from returning to the heart after it is expelled thence by its systole or contraction; these valves in this case were somewhat thicker, and not so pliable as naturally, and did not so adequately apply to each other; as is expressed by aaa, fig. 14. Whence it happened sometimes that the blood in the great artery (AAA fig. 11) would recoil, and interrupt the heart in its systole. But this stubbornness of these valves was owing to a bony or stony substance, marked in the said figure, which appeared much plainer when the valves were dry, as represented in the figure beneath, marked with an * ; aa the two valves pinned out and dried; b the petrifaction or stony body at their junction. In this instance I observed the left ventricle of the heart, expressed at GG, DD, ee, ff, to be a little dilated from its natural size, but was not, by two parts in three, so large as the left ventricle of the heart of a subject I have dissected. The symptoms, some years before the death of this person, who was about 40 years of age, were extraordinary shortness of breath, especially on any fatigue, with an intermission of one stroke in three of the pulse; the posture of sitting up was more eligible than any other; he complained of great faintness, and now and then pain about the heart, the extreme parts often cold, which towards his death increased more and more on him; his legs and arms being mortified some hours before. On opening the chest, the heart, particularly its left ventricle, was found larger than that of an ordinary ox, and filled with coagulated blood; the valves of the great artery AA, fig. 11, were petrified, insomuch that they could not approach each other, as expressed fig. 12 and 14; but an orifice, represented at fig. 15, remained always open by the petrifactions bb, fig. 13, and aa, fig. 15, which had clogged these valves, and hindered their application to each other, as in a natural state is represented in fig. 12 and 14, aaa.

The explication of the symptoms in both these cases is obvious enough: for though the person in the first instance did not die of the same disease as the other, yet the symptoms in his illness plainly showed what must follow, from the disorders of these valves, as they are rendered more or less useless; for as their office is to prevent the return of the blood into the heart, in its diastole, by exactly shutting up the passage of the aorta, like the valves in water engines, so if by any accident they are hindered from doing their duty, as they were by the petrifactions mentioned, the consequences must be, not only a regurgitation of blood into the heart, but they baulk its impulsive force, when the muscular fibres in these valves cannot contract, to prepare the passage for the blood of the left ventricle, when it is to be expelled into the aorta. Hence the intermissions of the pulse in the first instance may be accounted for. In the latter instance, these valves were wholly useless, and the circulation became more difficult, as appeared by the refrigeration of the extreme parts, the mortifications, &c. In both these cases, the left ventricle of the heart was dilated proportionably to the bad constitution of these valves, which plainly shows these valves give such assistance to the heart, as it cannot be without, and that it gradually suffers according to their indisposition.

Before these papers were sent to the press, I had an opportunity of observing a like instance of that first mentioned, in an elderly gentleman, about 72, who sometimes had intermissions in his pulse several years before his death, in whom I found divers petrifactions in the mitral and semilunar valves of the left ventricle of the heart.

The Explanation of the Figures.—Fig. 11 represents the left ventricle of the heart opened, &c. AAA the inside of the aorta slit open to the left ventricle; BB the bulbous trunk of the vena pulmonalis divided through, and pinned aside; to show aaa the three semilunar valves of the aorta, which hinder the blood from returning to the heart; b a small stony substance at the conjunction of two of the semilunar valves, expressed at the * below this figure: aa parts of the two valves dried; b the petrifaction, as it appears in the dried valves; c part of the lower trunk of the vena cava, cut off immediately above the liver; ccc the left auricle opened and pinned out; DD the sides of the left ventricle divided and drawn aside, to show its inside dd ee ff GG; dd the mitral valves of the left ventricle of the heart, or arteria pulmonica, divided and turned aside; ee the carneæ columnæ, whence spring the tendons fastened to the valves, dd, expressed by df in fig. 13; ff a transverse cord or tendon, by which the columnæ carneæ are drawn nearer each other in the systole, or contraction of the heart, when the blood is expelled into the aorta; by which the tendons expressed ff, fig. 13 and 15, draw the mitral valve laterally; by which means its orifice gc, in the said figure, is not only closed to prevent the return of the blood by the vena pulmonalis, but at the same time it opens a passage for the blood of the arteria magna, by withdrawing the mitral valve d, fig. 12, from the orifice of the aorta aaag; GG the internal surface of the left ventricle, where it is somewhat smoother as it leads to the aorta; gg the trunk of the coronary vein divided when filled with wax; hh the coronary artery in like manner divided; i one of the trunks of the vena pulmonalis; kkk the three orifices of the trunks of the vena pulmonalis, as they open into the bulbous trunk; expressed at BB; H the cone of the heart.

In fig. 12, A is part of the aorta next the heart; aaa the three semilunar valves, as they appear next the heart in a natural state, when the heart is in diastole, and the blood hindered by these valves from returning to its left ventricle; bb part of the basis of the heart cut off; ee the two columnæ carnæ of the left ventricle; d the mitral valve; ff the tendons springing from the carneæ columnæ, and inserted into the upper and middle parts of the valve, as well as to its lower margin; which is better expressed in the following figure; g the orifice of the aorta completely closed by the application of these three valves to each other.

Fig. 13 shows the same parts as in the preceding figure, as they appeared when the valves of the aorta were petrified, excepting a, which represents a part of one of the valves that was not covered with the petrifaction; bbb the petrifactions on the rest of the valves; † a small petrifaction on the mitral valve; hhh some of the transverse tendons, which draw the carneæ columnæ to each other, when the heart is in systole, for the more effectual closing the orifice of the mitral valve, expressed here at g.

Fig. 14 and 15 show the same parts represented in the two preceding figures, as they appear viewed towards the heart, when dried and displayed; AA the trunk of the aorta; aaa, fig. 14, the semilunar valves in a natural state, when the blood in the arteries presses them close to each other; bbbb the trunks of the two coronary arteries cut off; aa, fig. 15, the semilunar valves petrified; c the orifice of the mitral valve next the vena pulmonalis; ddd the internal surface of the mitral valve leading into the left ventricle; eee the columnæ carneæ ff their tendons; gg the transverse tendons, which draw the fleshy columns to each other, when the heart is in systole.

Account of a dropsical Body dissected. By Mr. John Lafage. N° 299, p. 1977.

A maiden lady 52 years of age, complained, about 6 weeks before Mr. L. saw her, of a circumscribed hard swelling on the right hypograstic region; from that time her belly grew by degrees to an exorbitant size, and at last suffocated the lady. The body was greatly emaciated, and the legs swelled a few days before her death.

I expected water, but there was only a viscous darkish humour, to the quantity of 18 gallons; after the evacuation of that matter, I was no less surprised to perceive a large heap of vesicles arising from a thick membrane covering the guts, it being the peritonæum separated from the muscles: I took it out, to examine the better those vesicular bodies* disposed on the outer surface of that membrane, as also those on its inside, towards the guts. The vesicles were of different magnitudes; some of the largest had been broken and sunk, others were broken and almost empty, and the others very much distended and full; the matter of all of them was of the same nature with the extravasated humours. What was contained in the lesser ones proved to be of a different colour and consistence, not unlike gelly, white of eggs, gall, and honey; in some it was much like the humour of a true meliceris. There was but little matter extravasated in the cavity of the abdomen; most part was contained between the peritonæum and the muscles. The right kidney was affected with a particular dropsy; all the viscera besides were in a natural state; 2 polypuses were found in the heart, and 2 pretty large stones in the gall bladder.

* Hydatides.

Account of a Book. By James Douglas, M. D. viz. De Aure Humana Tractatus; in quo integra Auris Fabrica, multis novis Inventis, et Iconismis illustrata, describitur; omniumque ejus Partium usus indagantur. Auctore Antonio Maria Valsalva Imolensi. M. D. &c. Bononiæ* 1704. *4to.* N° 299, p. 1978.

The authour divides the human ear into 3 cavities, viz. the external, which contains the auricle, and the auditory passage; the middle, which comprehends the tympanum, or cavity of the barrel, in which are the four little bones, &c; and the internal, which contains the labyrinth, which he farther divides into the vestibulum, the semicircular canals, and the cochlea. The prominence called helix ends in the lobe of the ear, which it constitutes, and that called anthelix terminates in the antitragus. He gives the common names to the cavities that lie between the eminences of the auricle, and divides the concha into two cavities, viz. the superior and inferior.

Under the skin of the auricle he takes notice of a great number of glands, which, from the likeness of the humour they separate to that of tallow or sebum, he calls glandulæ sebaceæ: which liquor being carried to the surface of the skin, he alleges hardens there, and turns into a scaly greasy substance, much like to that of bran. That there are abundance of such glands under the skin of the head, he thinks that the greasiness of the hair, and the dandriff combed from the head, may be a sufficient proof. The lobe of the ear, and the lower part of the helix, are made up of a duplicature of the common integuments, without any cartilage; in no part of the auricle, except in these two, is the membrana adiposa conspicuous. Besides the commonly described processes or eminences of the auricle, formed by the windings of its cartilage, he takes notice of another that is small and acute, situate near the beginning of the auditory passage.

He has discovered some little glands, of the conglobate or lymphatic kind, which, with respect to their situation upon the tragus, he calls glandulæ tragi: these are sometimes 3 in number, sometimes 2, but for the most part there is only one of them to be found in each auricle. To the 4 external muscles of

* This Italian physician was a very expert anatomist, and afforded great assistance to Morgagni in the dissection of morbid bodies. Besides the abovementioned treatise on the ear, he read several dissertations before the Academy of Bologna, wherein he presented many new observations on the structure and diseases of the eye What he took for excretory ducts of the renes succenturiati, were afterwards shown by Ranby (Phil. Trans. Vol. xxxiii.) to be blood-vessels. Valsalva died in 1723, aged 57.

the auricle, described by Casserius, he adds a new one of his own discovery, and names it, musculus auriculæ anterior: it springs from the investing membrane of the temporal muscle, above that part of the zygoma, which proceeds from the os temporis: thence running straight down, it splits into two parts, one being inserted into the fore part of the superior cavity of the concha; the other a little higher, into the fore part of the cavity of the scapha.

He says the posterior muscles of the auricula vary as to their number in different subjects; there being sometimes 4, and sometimes but 2 of them; yet for the most part there are 3 in each auricle. Besides these, he describes 2 internal muscles belonging to this part; which he says none has hitherto taken notice of: one he calls musculus tragi, the other musculus anti-tragius, according to their situation; but in lean and emaciated bodies, he owns they are not to be seen.

He has discovered a new ligament, which fastens the auricle to the processus zygomaticus of the temple bone. On filling the meatus auditorius with wax, he observes that in the beginning it ascends a little; then about its middle it is crooked downwards; again it run upwards, and then downwards to the membrana tympani, by which it is obliquely shut. He describes the incisuræ, or slits, in the cartilaginous part of this passage, more accurately than Mons. Duverney has done. When the membrana adiposa comes to the beginning of this meatus, its fleshy fibres are spread upon it in a reticular manner, and in the areas or spaces between, the glands which separate the cerumen are placed. The cavity of this auditory tube, in a fœtus, is very much contracted, and filled with a whitish stuff, which in process of time dries, falls off, and comes out with the cerumen; yet sometimes it hardens into a membrane, which sticking close to the membrana tympani, hinders the free access of the air, and so causes a deafness, till it is removed by art.

Our author observes in the back part of the auricle a vein, which he says none has hitherto taken notice of, and calls it occipitalis, because it receives several twigs from other parts about the occiput; from all which it brings back the refluent blood into the lateral sinuses, piercing the cranium at a hole behind the processus mamillaris. None of the branchings of the hard portion of the auditory nerve are spread upon the backside of the auricle, as some write and delineate; for the nerves that supply that part, come out beween the first and second vertebra colli: a twig of this nerve, running upon the back of the antitragus, is sometimes successfully cauterized in the tooth-ach.

It is very probable, that there are lymphatics both in the auricle and auditory passage. That the membrana tympani is made up of two membranes, is very apparent in a fœtus; the innermost of which is from the dura mater, and the outermost is only an expansion or continuation of that fine skin that invests the meatus auditorius. He thinks the passing of the smoke of tobacco from the mouth by the ears, the evacuating of pus, &c. from the barrel the same way, seem to evince the necessity of a perforation, or hiatus, in the membrana tympani; though none of his repeated experiments were so successful as to discover it hitherto.

He reckons the sinuses of the processus mamillares, which are divided into several cavernous cells, as part of the cavity of the barrel, because they communicate with it: and in some other animals, where these sinuses are wanting, the cavitas tympani is considerably larger. The head of the malleus lies hid in the beginning of the sinus mastoideus, but is no ways connected to it. In its manubrium, or handle, he demonstrates three processes, which he names major, minor and minimus: to the three muscles of the malleus, he gives the same names of its processes to which they are inserted. Musculus processus majoris, first discovered by Eustachius, rises from the cartilaginous part of the tuba Eustachiana, and not from that bony canal which runs laterally upon the osseous part of the same; then running along that canal, it enters the barrel, where its tendon, being inflected a little downwards, is inserted in the processus malleoli major. He says, that none have taken notice of the true origin of this muscle before him, notwithstanding its rising from the tube does very much conduce to hearing. Musculus processus minoris, is the laxator auris externus; he reprehends some modern anatomists for omitting the description of this muscle, though fairly described by J. Casserius long ago. Musculus processus minimi begins at the side of the barrel next the face, and running along the same, it comes to its insertion, being inflected under the chorda tympani, in the smallest process of this bone.

The incus is joined by small ligaments to the malleus, whence these bones have either no motion at all, or but a very obscure one between themselves. The long leg of the incus runs parallel with the handle of the malleus, the extremity of which is crooked a little downwards: its shorter leg is connected to the side of the sinus mamillaris by a ligament, which yet allows it a small motion. The bone that Sylvius discovered, should be called ovale, from its figure, and not orbiculare, since it is not round.

The figure of the basis of the stapes is nearly elliptical; yet its margin is a little defective on one side. It is convex towards the labyrinthi vestibulum, and concave towards its head. Its bony substance is so thin, that it is almost transparent, and not pierced with holes, as a certain modern describes it. The sides or branches of the stapes are furrowed on the inside; the space between being sometimes shut with a membrane, and often but half shut; but for the most part he observes no membrane at all between them. The stapes stands on the foramen ovale in a middle position, between vertical and horizontal; it shuts this hole exactly, being fastened to its margin by a thin membrane, but yet so loosely, that it has the freedom of moving up and down; which motion he thinks contributes much to hearing; for upon opening the ear of one that had been very long deaf, he observed, that the ossification of this very membrane was the only cause of his deafness. The fleshy belly of the musculus stapes, first discovered in a horse by Casserius Placentinus, is contained in a bony channel, excavated about the middle of the true Fallopian aqueduct laterally, from which its tendon is obliquely carried to the head of the stirrup.

Although those four little bones have no periosteum, yet several blood vessels run upon them, and enter their substance, which is very compact and hard; the stapes indeed is something brittle, not because it is more porous than the rest, but because it consists only of one bony lamella. The fenestra rotunda he sometimes observes to be of an oval figure: the membrane that shuts it is fastened lower down than its margin.

He describes several small holes that pierce the cranium, and open into the tympanum just above the articulation of the malleus and incus; by these, extravasated blood or purulent matter, contained within the skull, may be carried into the cavity of the barrel; from whence they may either pass through the hiatus in the membrana tympani, or else by the tuba Eustachii, and so be evacuated by the mouth. He proves the existence of such holes by injection, and two practical observations.

The duct that goes from the ear to the palate he calls, from its figure and first observer, Bartholomæus Eustachius, tuba Eustachiana: it consists of a bony, cartilaginous, membranous, and fleshy part. The membrane that lines it is full of glands. To dilate and keep this tube open, he has found out a new muscle, which novus tubæ musculus, as he calls it, rises fleshy from that portion of the tube between the beginning of its cartilaginous part and its extremity; hence descending obliquely by the lower part of the internal ala of the processus pterigoides, it becomes tendinous, which growing broader again, is so inserted into the inferior margin of the membrane, that covers the foramina narium; where it joins with its fellow on the other side.

The uvula, which he considers as part of the pharynx, is moved by three pair of muscles, one known long ago, but ill described, and two new ones discovered by himself. 1. Salpingo-staphylinus; which rises from the inferior bony part of the Eustachian tube, whence it descends obliquely to its insertion into the basis of the uvula, where it joins its fibres with its partner-muscle on the other side. 2. Glosso-staphylinus; which comes from the lower part of the tongue, and ends near the middle of the uvula. 3. Pharyngo-staphylinus; which has a large and broad beginning from the lower part of the pharynx, whence ascending and passing under the tonsillæ, it terminates at the side of the uvula laterally. When we inspect the mouth of a living person, the two arches or risings we observe at the sides of the uvula, proceed from the swelling of the two last-described muscles.

In the pharynx he observes three orifices, one that leads to the mouth, another to the nose, and a third to the œsophagus; all which are dilated and contracted by the following muscles, whose descriptions agree very well with the life, as I also observed in the same subject. 1. Pharyngo-staphylinus, or staphylo-pharyngæus; which is the same with the third muscle of the uvula, and serves the motions of both in common, being by some falsely called cephalo-pharyngæus. 2. Glosso-pharyngæus, falsely called by some sphœno-pharyngæus: its origin is in common with the glosso-staphylinus, whence it goes round the upper part of the pharynx, uniting with its fellow on the other side by a tendinous line. 3. Stylo-pharyngæus; to the known description of which, he adds nothing. 4. Hyopharyngæus: this has a double origin, one from the horns of the os hyoides, the other from the cartilaginous appendages near the basis of that bone; from whence it surrounds part of the pharynx, and joins with its partner by a middle line. He adds, that a violent contraction of this pair of muscles may cause a luxation of the foresaid appendages, which hinders deglutition till reduced. 5. Thyropharyngæus; which rises from the sides of the cartilago thyroidea, and like the former, goes round the pharynx, uniting with its fellow in its middle and back part. 6. Cricopharyngæus; which rises from the cartilage of that name, and like a sphincter surrounds the beginning of the œsophagus.

Although I designed not at this time to have made any remarks on what our author advances in this treatise, referring that to another opportunity, yet I cannot refrain from one reflection en passant; which is this: had the accurate Valsalva read and examined what Mr. Cowper has written sometime since, on the muscular structure of the fauces, in his excellent book of Reformed Myotomy, an abstract of which our author might have seen in the Acta Eruditorum, published in 1696, Suppl. Tom. ii. p. 508, he had certainly obliged us with a better and more complete account of its muscles; for he has wholly omitted the most considerable part of Mr. Cowper's Musculus Pterigopharyngæus, which rises from the processus pterigoides, being satisfied with describing only the lower part of it, which springs from the tongue and the os hyoides, which he makes to be two pair of distinct muscles. I wonder how he came to overlook this, which I always observed to appear in dissection, as Mr. Cowper has described it, since his happy industry has led him to the discovery of several parts in the ear, &c. which are not to be found in any book extant.

He says the musculus chondroglossus, described by a certain modern, is not always to be found. The artery that furnishes this cavity with blood, goes off from the carotid, while in its oblique canal in the os petrosum: and the vein that carries back the refluent blood, opens into the diverticulum of the jugular vein: he thinks it may have lymphatics as well as the external cavity of the ear.

He reckons the chorda tympani to be a twig of the portio dura. He says, that there are 12 orifices that open into the vestibulum, viz. the fenestra ovalis, the 5 orifices of the semicircular canals, one of the canals of the cochlea, and 5 holes that admit so many twigs of the portio mollis nervi auditorii.

He distinguishes the semicircular canals into the major, minor or minimus. He is very nice in adjusting the different lengths of these canals, and the proportions they bear to one another in their diameters; which are different in different subjects, but always alike in both ears of the same subject.

The cochlea consists of a modiolus or cone, and a septum, which divides it into two canals, which he calls scalæ; that which respects the fenestra rotunda, is the scala tympani or superior; the other, which communicates with the vestibulum, he calls the vestibuli scala: he is also very curious in ascertaining the difference between the two scalæ: he remarks that modern anatomists have erred in the position of these scalæ or turnings; for what they call the superior he rightly names the inferior, et è contrà. Its septum is made up of two substances, one hard, but very friable, called lamina spiralis; the other soft, thin and pellucid, which he calls by a new name, zona cochleæ.

The canal, by which the auditory nerves enter, he divides into the common, which admits both the soft and hard pair together, and the particular, which contains only the portio dura: he observes that it is this particular canal in which the hard portion lies, that Fallopius first discovered, and named it ob similitudinem aquæductus: the tuba Eustachii is very falsely, though now commonly so called.

He observes, that as the portio dura turns aside from the common channel into its own, it detaches one branch, which, going out at a hole in the inside of the os petrosum, spreads itself on the dura mater and trunk of the 5th pair of nerves, in several small twigs.

In the bottom of the common canal, he takes notice of 3 small sinuosities or cavities: one descends towards the centre of the cochlea, in which are several holes for the entry of part of the portio mollis, where it is dilated into a very fine membrane, which makes the zona cochleæ; the second goes towards the vestibulum, through which the portio mollis enters by 5 holes, where its twigs or branchings are presently expanded into a very fine membrane, which lines all its surface, being further continued through all the semicircular ducts: this nervous expansion, from its resemblance to a very thin and narrow ribbon or fillet, he calls zona, and from its use, sonora, of which he reckons 3, according to the number of canals: he says, these zonæ sonoræ are very conspicuous in several quadrupeds, and in volatiles especially.

The labyrinth has both veins and arteries, though its cavity is not invested with any periosteum to support them; but whether they proceed from those diffused through the os petrosum, or that they enter together with the auditory nerves, he cannot positively determine. He doubts not but these vessels are also accompanied with lymphatics, as well as those of the retina, as he has observed in the eye of an ox. For the use of the parts, on which our authour is exceedingly large, I refer the reader to the book itself, which is enriched with a number of curious plates, especially of the parts relating to the ear, drawn from the life, and well engraven.

Concerning an Improvement of the Hessian Bellows, &c. By Mr. D. Papin. N° 300, p. 1990.

I am busy at present for a coal-mine, which has been left off, because of the impurity of the air: I have therefore improved the Hessian bellows, an account of which was printed at Leipsic, in Actis Eruditorum, anno 1699, with this title, Rotatilis Suctor et Pressor Hassiacus; which may be applied for wind as well as for water. The shape of the tympanum was cylindrical, as represented fig. 16, pl. 8; where DAFC is the circumference; CP, DP, AP, are the radii, which bear the wings cm, DN, AO; BE is the aperture through which the wind must be driven, in the direction of the tangent CB: and it may be observed that when the engine is working, every wing's from the end of the aperture e, till it comes to the beginning of the same aperture c, drive always the same air, with the same swiftness, and at the same distance from the centre: so that in going over all that circumference, the air finds resistance by friction, and gains nothing at all. I therefore now make the circumference of the tympanum in a spiral shape, as fig. 17, where the spiral circumference is AFGB; the radii are AP, CP, DP, &c. the wings are AM, CN, DO, &c. and the aperture is AB. And it is to be observed, that every wing, in going round, drives new air: because the air which is first in motion finds room to recede from the centre, towards the spiral circumference, and so gives room for new air to come to the wing: and when the wings come near to the aperture, they drive their new air into the aperture without any friction; also the air, which has been first driven and removed from the wing, cannot lose its swiftness, because the wings, which continually follow, always drive new air, keeping that which is before always in the same velocity. This new shape of the Hessian bellows affords also another advantage; because the air in going round follows the spiral line, which is nearer to the straight line than a circular circumference; and when the air comes to the aperture, it gets into it without any loss of its substance; but in the cylindrical machine, fig. 16, the air always goes round in a circular circumference; and when it comes to the aperture, the wind is driven exactly in the direction of the tangent, except just in the beginning at C; and afterwards the impulse is oblique; which obliquity is always increasing till the wing comes to the point A, and must occasion a great dimunition of its strength. I have made such bellows where the radius AP is but $10\frac{1}{2}$ inches, the wing AM 2 inches broad, and 9 inches high; because the tympanum is also so high, or little more: the aperture AB was also 9 inches, or a little more; so that it makes a square hole. When I work this engine with my foot, it makes such a wind as to raise up 2 pounds weight; and a stronger man could do much more: but this is more than sufficient for our purpose, since we need only drive air enough for the respiration of such men as can work in the mine; and we can easily make wooden pipes with boards, to conduct the wind to the very bottom: so that the air within will be continually renewed, as well as without.

I have made some trial of the Hessian bellows in a very strong fire in a furnace, to melt glass, iron, or any other hard metal; and though I could open the furnace above the matter to be wrought upon; yet no flame would get out through the aperture, nor could cold air from without get into the furnace: so that it is very like this will be a great conveniency for several sorts of work, since men may work the matters when they are most softened in the fire; and they may be drawn up perpendicularly, so as not to be bent, as they are when drawn horizontally. I believe this would be a good way, especially to make glass pipes and looking-glasses of an extraordinary size. And I think that the Hessian bellows may be applied to several good uses, and so deserve very much to be improved.

How to judge of the Age of Manuscripts, the Style of Learned Authors, Painters, Musicians, &c. By Mr. Humfrey Wanley. N° 300, p. 1993.

It is evident that a man may judge of some manuscripts by the hand-writing; and of the genuine and spurious works of some authors, and the time in which they lived, by their style, but can scarcely be infallible: suppose for instance, a man should bring to any antiquary a good MS. copy of the Hebrew Bible, Pentateuch, or Psalter, written in a small common letter, without points, fine knots, flourishes, pictures, and great letters, or any thing that should look pompous: suppose that the ink, parchment, &c. should carry a seeming face of antiquity with them, and that a man should say his MS. was 1000, 1200, or 1300 years old, when as really it was written within a very few years; could the antiquary from the hand alone soon find out the cheat?

All the Hebrew MSS. that I have seen, are written either in Samaritan or Chaldee letters. As to the Samaritan, I own they bear a good resemblance to each other, and that they differ very much from those Samaritan characters which we find stamped on some truly ancient and genuine coins. But then there seems to be such a resemblance, as to the character, between those coins struck in ages far distant from each other, that it is hard (from the consideration of the metal, its fabric, weight, or from the shapes of the letters in the inscription, &c.) to say, which coin was made in the time of David, or Solomon, and which no older than the time of the Maccabees; this being rather to be gathered from the words and meaning of their inscriptions, than from the figure of the characters which compose them. The same may be said, in a great measure, of the old Greek, Punic, Roman, British and other coins.

The Chaldee character has indeed varied in tract of time, according to the different fancies and humours of men. The even plain letter, I think, is the most ancient. This they altered into a more neat way of making it, as in R. Stephens' Hebrew Bibles. There is a third fashion, of waving the perpendicular strokes like rays, as in some of the Hebrew coins exhibited in the prolegomena to the Polyglot Bibles. Then 4thly, there is a large fat letter in the MS. Rituals and Liturgies, besides the Rabbinical letters of Italy and Germany, with their offspring, the Litteræ Coronatæ, and perhaps others that I never saw: not to mention here the Jewish custom of writing the vulgar language of the country wherein they live, with Hebrew letters. It seems a hard matter to trace the original and progress of all these ways of writing, so as on the bare sight of a MS. written in the Hebrew language or character, to say, by the shape of the letters of this book it appears to be so old: and it seems much more difficult to assign the particular province or country where each Hebrew book was written; as for instance, in Italy, France, Spain, Portugal, England, Holland, Germany, Poland, Barbary, Persia, India, in the several provinces of Turkey, &c.

The same nearly may be said of the Greek manuscripts, in which language there has been a great diversity of writing, according to the different humours of the scribes, the fashion then in use, or the manner of that particular province, in which such a book was written. Nor is it easy (though one would be apt to take such differences for so many land-marks,) to tell the age of a Greek MS. without the date; and I never yet saw such a date so high as the year 6400, according to the Greek computation. And it is still much harder, from any remarks about the character, illumination, ink, parchment, paper, binding, &c. to find out what country, province, or island, such a Greek book should be written in, or what countryman the scribe should be. What farther adds to the difficulty is, that it is known that the shapes of the majuscule letters found in Greek MSS. have been retained for above 600 years together, with little variation; and also, that some MSS. written with minuscules, and with accents, are older than some others which want them: and also, that the present Greek copistes or librarij have three or four different hands commonly used by them, one being their own common hand, the others an imitation of old MSS. which are more beautiful, but troublesome in writing, than their ordinary running hands: it being customary, I am told, when a man wants a copy of such a book to be written, for the copiste to ask in what hand it must be written, one hand being more costly than another; and according as they agree, the book is written. And thus I have seen some very new things written in the same hand with books which are certainly 400 years old.

What methods learned men have taken, in order to inform themselves of the different ages of MSS. I know not, but my own has been this, viz. I have been careful to get all the dates I could, when it was said that such an individual MS. was written, at such a time, or by such a particular person, every book with a date; being as a standard by which to know the age of those books of the same or a like hand, and of those that are not very much older or newer. Where dates have been wanting in some books, perhaps they have had some succession of emperors, kings, popes, bishops, or other officers; and setting down the continuance of their predecessors for so many years, months, and days, if there be only the naked name of him who is the last in order, all other circumstances concurring, I then judge the book to have been written during the life or reign of such a person: especially if that succession be afterwards continued by a more recent hand, or that there be two such successions, as of kings and bishops, and the last of each happen to be cotemporaries. I have made other observations from historical notes and ecclesiastical tables, in some books. At other times I light upon some authentic charter or original writing, in the same hand with such a book as I have remembered to have formerly seen, but without any guess at its age. The age of the charter being known, that of the book is then known also: for I never entertained any notion, or relied on any observation, but as I found it confirmed by the suffrage of concurring circumstances, and sufficient authority.

But even in dates, I have found that a man ought to be very cautious; for some have been altered by later hands, for corrupt and base ends. Some are so worded, as when one thinks that the time they mention, is the time when the MS. was finished by the copiste, or book-writer; it is meant only as to the time when the author finished his composition. Other books are post-dated, that they might be accounted new. Of this last kind, is a Greek MS. I saw in the university library at Cambridge, which, as appears by an annotation written in it, was bought such a year at Rome, for so much; and yet the date pretends that the book was written at Rome in such a year, which happens to be two years after it was bought and paid for. The reason of these post-dates was, because, before printing came up, a book was so much the more valuable as it was newer. An old book might be bought for an old song, as we say; but he that transcribed a fresh copy, must be paid for his pains. And therefore, I have found in some catalogues of the MSS. formerly extant in our abbey-libraries, that when they said such a book was liber vetus, they would often add, et inutilis; but liber novus was nitidus, eleganter scriptus, lectu facilis, &c. which mean opinion of the ancient copies, by the bye, may have been the occasion of the loss of many a good author.

The librarii or book-writers were from the time of the Romans a particular company of men, and their business a trade: but though book-writing was their profession, yet they afterwards had but a third part of the business. Learning, after the erection of monasteries, was chiefly in the hands of the clergy; and they were for the most part regulars, and lived in monasteries: among these were always many industrious men, who wrote continually new copies of old books, for their own use, or for the monastery, or for both; which seems to have swallowed up above half the business. Then, if an extraordinary book was to be written, for the standing and more particular use of the church or monastery, the antiquarius must be sent for, to write it in large characters, after the old manner, and such a copy they knew would last for many ages, without renovation. Between these two sorts of people, the writing-monks and antiquarii, the poor librarii, or common scriptores, who had families to maintain, could hardly earn their bread. This put them upon a quicker way of dispatch, that so they might undersell each other: and in order to this dispatch, they would employ several persons at one time, in writing the same book, each person, except him who wrote the first skin, beginning where his fellow was to leave off: or else they would form the letters smaller and leaner, and make use of more jugations and abreviations than usually others did. And this is the only account that I can give, for that variety of hands which in former ages, being learned of, or borrowed from the Romans, was commonly used, and in fashion at the same time, and in the same country, throughout these western parts of Europe, and for their growing less and less for one age after another. An instance of this may be given from the hands of England, which about the year of our Lord 730 were of three sorts.

1. The Roman capitals, still retained, and kept up by the antiquarii, in some books and charters. 2. The more set Saxon letters (which have a near affinity with the more ancient Irish characters, as being with them derived from the Roman,) which were used as the common hand of the age, by the monks in their books, and some charters of their dictating and writing. 3. The running Saxon letters, fuller of abbreviations, and something of kin to the Longbardic and Franco-Gallic, (both which, with this third sort, were also of Roman origin,) and was used by these librarii in their books and in the charters; as also by some authors who wrote much, as Bede, &c.

There was another sort of book-writers still in use, namely, the notarii, whose business it was to take trials and pleadings at courts of Judicature; to write as amanuenses from the mouth of an author; and to take homilies and sermons at church, from the mouth of the preacher. These notarii made use of notæ or marks instead of letters: but when, in process of time, letters were usually written small and quick, and abbreviations grew common, the notarii were turned off, unless they would write books in long-hand, as other librarii did, and their notæ grew out of use; and most of their performances in notes or marks have been since destroyed.

Suppose then that a man had one Latin book of each of the four sorts above-mentioned laid before him, written all at a time, and without any date or note of the age: would not he be ready to say that the first three were older than each other? As that in capitals was older than that in the middling hand; and this again older than that in the running and smaller hand? and that such a book written in the notæ being all full of marks, was not Latin, but of some other unknown language? But to come down later; suppose that a person should have some more recent books or charters laid before him in the pipe, text, exchequer, chancery, court, and common hands, all written at the same time, would he not be apt to say, that one seemed to him to be older than another, and that they were the hands of several nations?

If it be difficult then for an inquisitive person to be a perfect master in all the successions of hands, that have been used in his own country, so far as he may be guided by the monuments extant in it, (and I never heard of any man that was such a master) surely, it must be more difficult to pronounce the age of those books, from the hand, which were written in other countries, in an unknown language. And what may make a man yet more liable to mistakes, besides the want of dates in the most ancient Greek, Latin and other MSS. was the practice of many writers, still to use the very same hand when in years, as they learnt when they were young; like as many ancient people, who yet continue to write the Roman and secretary hands, which were more fashionable 50 or 60 years since, than now.

As to the great facility of finding out an author, and the time he lived in, by his style and phrase, people have learned the knack of changing their style, upon occasion, so artificially, as not to be discovered, but when they themselves wish to be known. Who would have thought that Erasmus wrote the Epistolæ obscurorum Virorum? Or that some of the nicer, nay, the most eminent modern critics, could have been imposed on by their familiar and near acquaintance, who trumped upon them their own recent performances, for invaluable fragments of the ancients, whose other works these very critics had lying before them? It has been a frequent practice, in all ages, for poor scribblers to father their wretched offspring upon illustrious persons: and the disparity between the genuine works of the one, and the spurious pieces of the other, being evident enough, it has been easy to distinguish between the gold and the brass. But I would humbly ask this question, is all that is even now by learned men ascribed to some ancient voluminous Greek and Latin authors, undoubtedly theirs? May not there still some supposititious pieces lurk among them, which have the luck to be received, only because they have been more ingeniously counterfeited? Nay, may not the same person in the course of his life, even alter and vary his style and phrase unwittingly, and without any design to do so? I think Mr. Richardson, somewhere in his answer to Amyntor, on occasion of the difference in point of style between the Revelation of St. John and his other works, between the Prophecy of Jeremiah and his Lamentations, tells us from Dr. Cave, that the consideration of the times when a man writes, or of the persons to whom, or the subjects about which, or the temper of body, or the humour he is in when he writes, or the care and pains that he takes in writing, may occasion such alterations in his style, as that no certain rule can be inferred from thence. And if it was really possible to find out the time when an author lived, only by diligently reading his works; surely the world would have been long since agreed as to the time when Homer lived, though they could not tell where he was born. And I believe even in the list of ecclesiastical writers there are some, and those not of the least consideration, who, notwithstanding their works have been read over and over, are still reckoned to be of uncertain age.

As for pictures, though I have much less experience in them, than I had once in MSS. yet I will not deny but that the works of a hundred masters may be known by the hands, though they may be almost as different as their several hands in writing: but that one painter cannot copy from another, so exactly, as that in tract of time it shall not be known which picture is the original, is what I dare not assert. It has been frequently practised by painters to borrow pictures of those who are lovers and judges of such things, to copy them, and to return their copies for the originals, without any discovery made by the discerning owners. And I believe it possible, though exceedingly difficult, for a great master to copy a picture so, that when they both stand together, a good judge shall not dare positively to say which is the copy and which not: nor he that drew the original, dare to own, that he could imitate his own handywork better than a stranger has done. There are a great many stories common among painters, to this purpose. And one would not think it much more difficult, for a man to imitate a drawing or picture, than to counterfeit another man's hand-writing, which some people can do most exactly. And others with pen and ink will copy after any thing that is printed so nicely, as that one would affirm their writing to be printed off at the press.

As to the notion of discerning the age, as well as the hand of the painter, by his picture, it is very curious, and I doubt not but there is a great deal in it. I only want the whole works of some great painter, with an account of the time when he wrought each piece, to fit me for the making the experiment. And why might not this notion be advanced a little farther, and the painter's complexion be known by his pictures, as well as his age? As supposing that the sanguine naturally run upon portraits, poetical histories, nudities, &c. The choleric upon battle-pieces, sea-fights, fire-pieces by land or sea, tempests, &c. The phlegmatic upon the still-life, flower-pieces, birds, beasts, fishes, &c. and the melancholic upon landscapes, architecture, pieces of perspective, &c. Not but that the different genius of a country, or the desires of a good customer, may oblige a painter to work upon a subject, which he had no great fancy for.

As to the difference in the works of painters grown old, in respect of what they did when young, I doubt no certain rules can be established, as to their performances in that kind. I know that painters generally live faster than other men, which may at length occasion a failure in their sight and memory, a trepidation in their hands, &c.; yet I never heard that Michael Angelo, Alb. Durer, Titian, and others, painted worse at the latter end of their long lives, than they did before. Nay, I hear that Signior Verrio, though grown old, paints now far better than ever, and is grown almost ashamed of some of his own works which he painted at Windsor-Castle in the time of King Charles II. There may be this in it, that aged persons having attained, through long practice, to a greater experience, to a more solid and mature judgment, than they had when younger, are more cautious of that which they let go out of their hands; and correct those flashy touches of their pencil, and other superfluous irregularities, which they and others were formerly very fond of.

As for the flame and motion of the eyes in a picture, or the breath in its mouth, I can say but little, having as yet never had the happiness to see such rarities, though I have been admitted to the sight of some of the best pieces of the most celebrated masters. As to the painters painting a living or moving thing, so that one shall almost discern the motion, and see the bird flying, the horse or hound running, &c. that is more easy, especially when assisted with the friendly and pregnant fancy of the charmed spectator. In the still-life indeed, the eye is quickly deceived, and though there are, I believe, several masters now living more excellent at it than ever Zeuxis and Parrhasius were; yet still, with all their art, it is very difficult to impose on a man so, as to make him believe it is not a picture, but the very life that he sees before him.

Musicians seem to be under the same predicament with painters, since they are observed to live fast, as also the poets. It is by the practice of many years that they attain to a just knowledge and mastery in their respective arts; and as their first compositions are little and light, suitable to the mercurial temper of heedless and inconstant youth; so, in time this wears off, and as their experience and judgment increases, their compositions grow more solid and sound. A young man may make a better minuet or jig, but the elder a more sound service or anthem. The music of the former, with other accomplishments, may go a great way towards inticing a foolish girl to love; but that of the latter excites the devotion, moves the affections, and raises the passions of those truly religious souls, who take pleasure in singing praises to the honour and glory of his name, who lives for ever and ever.

If it should be said, that the very best painters, musicians and poets, died young, or at least before they attained to an advanced age, when they would have failed or grown dull, as others did: I must beg leave to say that old men are of two sorts, either those who are much affected with their age and weakened, or those who are not. If a man be born of unsound parents, or has lived all along in an air disagreeable to his constitution, or is always unhealthful, or has lived an intemperate or debauched life, or has been crushed by any heavy misfortunes, or always lived in poverty or discontent; it is no wonder, if, in spight of all this, he attains to old age: but then, he will probably lose the clearness of his head, the fixed attention of his mind, the brightness of his parts, which he might be formerly noted for. If a man has never had any of these disadvantages to struggle with, but has all along been blessed with the contrary; then, he being bred up to a profession, and always following it, his judgment in it still increases, and his hand one would think should be more nimble and ready, and the man a better painter, musician or orator than ever: and why not a better poet too? For if Mr. Dryden (though he was said to be unhealthy at last) would have taken as much pains, or had been allowed time to his mind for revising his latter poems, as in some of his former, they might have been as well, if not better received. I do not see that it is old age that does a man this diskindness, but rather, that it is the accidents that too often attend it, which yet many are free from to the very last.

Supposing then, that Raphael, or Vandyke, or the late H. Purcell, or Alessandro Stradella, should have continued their practice of painting and music, till they grew old, from the accidents attending which, suppose them, as a great many other people, to be very free; might we not then have justly expected from them, even greater wonders than they had ever before performed. I will not say that an old general is fitter to be trusted than a young one; or that the late Mareschal Schomberg, at his death, was a better soldier, notwithstanding his age, than the present kings of Sweden and Poland: but rather, that the study of divinity, or of the laws, seems as nice and copious as those of painting and music. Now the old and sage men of those professions are every where most regarded, they are found to have the ripest judgments, and they are deservedly employed in the most weighty affairs appertaining to their professions. And it has been seen, as before said, that some painters and musicians have not at all failed as they grew old, but kept that great reputation to the last, which they had before acquired.

On the whole, it seems to me, that there is a gradual and sensible alteration in the appearance of things, and especially in the scripture or hand-writing of MSS. Now these ought to be considered with respect to the particular places where they were written. Every country is supposed to have remaining in it, the greatest variety and most considerable monuments of its own characters; unless they are known to have been carried away to other places. And therefore, if any man be desirous of considering the letters of any language that has been confined to any one particular region or province; it is but going thither, and it is ten to one, if he be diligent, but he may satisfy his curiosity very well.

For example, suppose I should be willing to consider the nature of the Irish letters, their orgin, progress, and variations, with their relation to the Roman, Franco et Anglo-Saxon: this might be done by travelling in Ireland, principally by taking a trip into the Scotch Highlands, and perhaps into the Isle of Man, and by consulting some English and other libraries, whither some Irish MSS. have been carried.

If I would consider the French, Italian, Spanish, or English hands, each country affords sufficient helps. But if a man would consider the letters of a dead or living language, which spread far, and has been, or is used in several countries: he cannot be supposed a perfect master in all the ways of writing that language, till he has considered the whole state and succession of its letters in each of those countries. Among those languages I reckon the Hebrew, Arabic, Turkish, Armenian, Persian, Greek, Latin, Teutonic, Sclavonian, &c. And though Latin is common among us, and every one is pronouncing the age of a Latin MS.; yet I think they would do well to inquire where, as well as when, a book was written. And if they are certain that such a Latin book was written in such a particular country, or province, it is then more easy, by considering the succession of letters used in that province, or by comparing it with other books written there, to say how old it is. For want of this consideration, many learned persons have been almost always out in their calculations, and have pronounced at random. If then this method appears rational, and even necessary, in order to attain a sufficient measure of this sort of knowledge; it follows, that it is no easy matter to assign the age even of a Latin MS. no not even in England, where yet I suppose there may be as great a variety of Latin hands, as in most other countries.

As for painting and music, I know very well that each painter's hand, and each musician's manner, differs from another; but whether there is a gradual and remarkable variation from themselves, in the course of their lives, is what I never heard asserted. This is certain, that they can change their way of painting and composing at pleasure; and therefore, Mr. H. Purcell's Dulcibella is said not to be like his other music; and Mr. Fuller the painter could pass one of his pieces upon Sir Peter Lely for a most incomparable picture of Mich. Angelo. But then these changes and variations, from their usual manners, are very seldom made; and a man generally pursues and practises that which is most agreeable to his own genius. For this reason, when a painter's hand is fixed, his manner is then limited; and so, when a curious person comes into a gallery, he knows that this picture was done by Ryley, Kneller, Vandyke, Dobson, Tintoret, &c. and that to be a copy after Rubens, Georgeon, Salv. Rosa, Han. Carraccio, Pietro di Cortona, &c. When he comes to an opera, to a concert, or to church, not knowing before-hand what music is to be performed, yet he may soon discern that it was composed by Corelli, Baptist, Bassini, Charissimi, Blow, Purcell, &c. And so upon reading an ancient author, a sagacious and learned person may find, that he writes according to the manner of such an age, that the style imitates such another, or that the book, though it bears such a man's name, yet might, perhaps, be more truly ascribed to another, with whose style it more exactly agrees. As for instance, that piece of S. Cyril's, published from the Escurial MS. by Barthasar Corderius, is thought (by reason of the analogy in point of style) to be Origen's: but then, whether all this can be always done, done easily and without errors, is the doubt. And it seems yet a greater difficulty, certainly to discover how old the painter, musician, poet, orator, or other author was, when he finished any one piece of his works, unless a man is plainly told so: this being a sort of knowledge, that those who have been otherwise sufficiently experienced in their several arts and professions, have not as yet pretended to.

Of a Person who died of a Scirrhous Tumor in his Breast. By Mr. Thomas Greenhill, Surgeon. N° 300, p. 2009.

Mr. J. D. was supposed to have died of a consumption, because 14 months before he had been violently seized with an inflammation of his lungs, accompanied with a sharp fever, difficulty of breathing, cough, acute stitches, and pleuritic pains, with a spitting of blood, &c. He was bled largely in the beginning; which was often repeated during his sickness; and he continually took such proper medicines as were prescribed him. But about Easter, there appeared a tumor on the breast-bone, pap, and pectoral muscle, of the left side, with a fulness under the axilla: from whence there was conjectured to be a collection of purulent matter in the cavity of the thorax, and that the sternum was foul: the first, from the aforesaid tumors, and his spitting a bloody and purulent matter; and the latter, from the rising and inequality of that part. But opening him, I found his case very different, and surprising; for soon removing the common teguments of the thorax, I found, instead of a rising of the bone with cariosity, only an oblong tumor, about 4 fingers in length, and 2 in breadth, and a proportionate thickness, weighing about 3 ounces; it extended itself perpendicularly, on the surface of that part of the sternum, which joins with the cartilago ensiformis. I separated it easily with a knife, from the breast-bone, and found it to be of that sort of wens, or encysted tumors, called atheroma, containing a pappy substance, like sodden barley. Next appeared a very large tumor on the left side of the thorax, covering the whole pap and pectoral muscles forwards, with a fulness under the axilla of the same arm. Then opening the thorax, I found the same tumor comprehending the intercostals, deltoides, subclavian, and subscapulary muscles, and the whole axillary and mammillary glands, which being obstructed, and its vessels replete with a creamy pappy matter, thicker and whiter than the former; there was produced such an induration of the aforesaid glands and muscles, which compose the upper part of the breast, that it may more properly be esteemed a scirrhous tumor.

The same tumor on the outside of the breast was somewhat larger than one's hand, extending itself from the clavicle, to the lower part of the pap; and laterally from the basis of the muscle, quite under the arm-pit. Internally it possessed a third part of the cavity of the breast, crouding the left lobe of the lungs to the right side, and in its upper part firmly growing to it; which it likewise did every way to the intercostal muscles. It was about the size of a penny loaf; and the whole tumor, being considered together, might reasonably be allowed to weigh between 3 and 4 pounds; which being cut into, there oozed out of it, like an expressed sponge, a great quantity of thick, white and pappy matter: and what is more particularly remarkable, there was formed a large sink or pelvis, in the middle of the axillary gland, which contained a thinner and discoloured matter, and had a free communication to the vessels of the lungs in the upper part of it, where it was united; and from hence it was that he generally found ease, when he had somewhat emptied it by large expectorations; and that he could so exactly perceive, when any thin rheums or matter flowed to the part; and it was here only that the lungs were black, and replete with stagnated blood, and some globules of the aforesaid matter in its vesiculæ. The rest of the lungs was pretty clear from any ulcers or matter, but they were of a sublivid colour, and strictly adhered on both sides to the pleura, but particularly on the left side, all about the scirrhous tumor. The vesica fellis, or gall-bladder, was full of stones, of the size of a runcival pea, and consisted most of odd angles, and were formed of a thick viscous sediment of gall (which we found in it) from an obstruction of its vessels, or jaundice, which he had some years before: they were 22 in number, some triangular, quadrangular, quincuncial, &c.

There was nothing else remarkable, besides a marasmus of the external parts, the wasting of the caul (omentum), and an emptiness of all the viscera and blood vessels in general.

Abstract of a Book, entituled, Olai Rudbeckii, Atlanticæ sive Manheimii pars secunda, &c. &c. Upsalæ; in folio. N° 300, p. 2012.

The learned author in this second part of his Atlantica, or Manheim,* has given a further elaborate illustration of the northern history and antiquities. He divides this great work into 11 chapters. In the first he states, that the island Atlantica was not feigned by Plato, neither was it America, nor Africa, nor the Canary islands, nor was it drowned in the sea, as many have thought, but that it is Sweden itself. In the second chapter, he speaks of the elegies of the ancient poets, of their abstruseness, and of the genuine way of explaining them. Particularly in the third and fourth chapters, he treats of the fables of the Scalds, or gothic poets, from whom he pretends the same was delivered to the Egyptians, the Greeks, and the Romans, &c. Chapter five treats of the heliobatria, or sun worship, among the Atlantics, and of its origin, with its propagation from thence through Europe, Asia, and Africa. In like manner, chapter six and seven treat on northern geolatria, or earth worship; which likewise, he says, in process of time extended through the same three quarters of the earth. Also chapter eight treats on selanolatria, or moon worship of the north. Chapter nine on the Runic fasts, the inventor of which, he says, was Atlas, king of the north. Chapter ten, on the tympana laponica, or Lapland drums, also of Lapland witches, incantations, &c. Lastly, chapter eleven, treats of the mensa Isiaca, or the tables of Isis, which the author expounds by the motions of the sun, moon, and earth. In short, the author labours to prove that every thing relating to religion, customs, manners, languages, &c. was derived from the ancient inhabitants of the north, particularly Sweden, Norway, Lapland, and even Iceland.

Catalogus Concharum Fossilium, Metallorum, Mineralium, &c. quæ a Cl. D. Johanne Jacobo Scheuchzero, M. D. Tiguri, et Societat. Reg. Angl. Soc. nuper accepit D. Jacobus Petiver, S. R. S. N° 301, p. 2042.

This catalogue contains a description of forty specimens of fossil-shells, metals, minerals, &c.

Epistola D. Joannis Philippi Breynii, M. D. et Societat. Reg. Soc. De Plantis et Insectis quibusdam rarioribus in Hispania observatis. N° 301, p. 2045.

There is nothing in this letter concerning certain vegetables and insects, which at that time passed for rare, that is of sufficient importance to be reprinted. The

* For an account of the first part, see p. 525, vol. ii. of this Abridgment.

locust and the mantis (of which an account is here given with plates) are described and figured in numerous books of natural history.

Concerning a Pin found in the Gizzard of a Fowl. By Mr. N. Regnart. N° 301, p. 2055.

On cutting the stomach or gizzard of a fowl Mr. R. found something resist his knife, which proved to be a pin that the pullet had swallowed, and which in all probability had lain there some time, for it had pierced through the membrane on the inside, and made a passage into the thick part, where it had formed itself a bed. The head of the pin had passed through the first shrivelled membrane, but stopped at the second, which seemed more thick and sinewy, so that the head remained inclosed between the two membranes, the body of it having made its way into the fleshy muscular part. At the point there was formed a callus, of the size of a small pea, which seemed a defence that nature had made to oppose it, as it was working itself farther.

It is not at all strange that a pin should lie in the fleshy parts, since we see a musket ball will lie there a considerable time, without much injuring the part; but how it should pass through the membranes of the stomach, without obstructing its functions, and the pullet thrive well after it, he leaves to others to determine.

Olai Rudbeckii Atlanticæ, seu Manheimii, pars Tertia, &c. &c. Upsalæ; in folio. N° 301, p. 2057.*

The author divides this third part of his Atlantica into thirteen chapters. The first treats of the most ancient writings of the hyperboreans, and the practice of the Greeks and other nations, of taking some things from them. From Schroder, he says, that the Runes, or Runic letters, were invented by Magog the Scythian, and communicated to Tuisco, chief governor of the Germans, Anno Mundi 1799. And he thinks it remarkable, that Magog is there mentioned as the inventor of the Runes, at that particular time that he himself has shown in his former volume, from Pliny, Wormius, and their own writings; that Atlas was one of the first inventors of the Runic calendars, from whom they are called Atlas's Calendars, or Runstaff's; whom he makes also inventor of the true golden number, between the year of the world 1800 and 1900, which number stands an undoubted argument for the true age of the Runic calendars, and the sixteen Runic letters, used by their ancestors in writing, and being more ancient than the letters of most other nations, especially European, as many as

* See p. 239 of this volume.

have been seen to this day; and these letters, he says, were fomerly rightly called by the name of golden apples, kept by Atlas till he communicated them to Hercules. He concludes that the Greeks and Phœnicians received their letters from them: ancient writers testifying that Ops, in former ages, carried the Runic letters, cut in brass, to the Greeks; which the author, in his first volume, has shown was before the time of Moses. He afterwards makes Cadmus a Scythian, who carried letters to the Phœnicians. In the second chapter an account is given of the golden number, and of its force in predicting various things, and of its invention, which he ascribes to his Hyperboreans. Chapter three treats of the celestial signs, most or all of which, he says, owe their invention and rise to Sweden; and every one of which he explains, by circumstances peculiar to that country. In chapters four, five, six, seven, eight, nine, the author treats of the six ages of the world, viz. the golden, the silver, the rocky or stony, the brazen or ashen age, the age of the heroes, and the iron age, which, he says, ended with the Trojan war.

The tenth chapter treats of the first form of government among the northern nations. Here the author gives an account of the most ancient of their ancestors that settled themselves in Sweden; and this chiefly from the scripture, and shows they were the sons of Japheth, among whom the chief was Gog, which name, he says, among them was a title of honour given to kings, heroes, and giants, and therefore the first of their ancestors, being a giant, was honoured with that title; and from the posterity of this Gog, many places of Sweden have drawn their names; of which he gives many instances: so the name of Magog or mangog, in the Swedish tongue, signifies a valiant and stout man; from whence many other places in Sweden have their names. So he says, Meschec, another of the sons of Japheth, was the progenitor of the Finlanders, who are most northerly, whence they are still called Mesar. As for the name of Finlanders, they have it from the Swedes, and he mentions several places in Finland that have their names from Meshec; and so from other sons of Japheth other places in Sweden have their names. He tells us from Scroder, that Magog was the inventor of the runes; and says, there is no room for doubting but Atin, Atlas, and Magog, were one and the same person. Chapter eleven treats of the form of government of the Atlantics, under Saturn, and his expedition. Here the author, after a long search into the origin of the word Chetim, finds that as some called the Goths, Gothi, Gythæ, Getæ, so they called their land Gutheim, Gythiam, Chetim, whence he thinks he has made it plain that Chitim, the son of Javan, grandson of Japheth, and great grandson of Noah, chose that country for his seat, and gave it his name; he had a son whose name was Cœlius, or Uranus, who had Saturn or Boreas, under whom was the golden age. This Saturn had many children and grand children, whom he made petty kings, and carried some with him in his expeditions, and made them kings abroad over the nations he conquered: hence they were called Tiodanar, Titans, that is, kings of people; which celebrated name passed to the Greeks, Pherecydes calling them Hyperborean Titans, and the poet subterraneous gods, or who possessed the seat of the Inferi, in the farthest parts north. And Plato, the author says, owns that all the tradition concerning the Elysian Fields and Hell, owes its rise to their north; which being cut on a table of brass, was carried by Ops from the Hyperboreans to Delos, belonging to the Greeks.

The twelfth chapter treats of Jupiter, his form of government and his expeditions at several times, about the year of the world 2100; the author having proved, he thinks, that Jupiter was the son of Saturn in Sweden, and held his empire there; he also undertakes to show, that he went thence to other countries and subdued them; which has been the occasion that various nations and cities, claim his place of birth and sepulchre to themselves, among whom he first numbers those of Crete. The thirteenth chapter treats of the migration of the Atlantics under Jupiter, Bacchus, Inachus and others, to divers parts of the world, as Thracia, Bactra, Egypt, but especially into Phœnicia.

From the Phœnician language he conceives we may draw a strong argument that they are a colony of theirs (the Swedes); what he has said before concerning the race of the Phœnicians, and the greatness and colour of their bodies confirming the same. And therefore, lest any people in the north, or neighbouring to the Phœnicians, should claim this glory to themselves, he adds here for a close a table of some words in the chief and mother tongues of the rest; that these being compared with the Runic or Phœnician words, it may clearly appear how close an alliance there is between the Scythian or Swedish, and the Phœnician languages. Now those most ancient tongues, to which in a manner all the rest owe their birth, are these: the Scythian or Swedish, the Runic or Phœnician, the German, Hebrew, Greek, Latin, Sclavonian, and Finnic. Many words of each of which he has given in a table, which being compared together, the Phœnician language appears to be in a manner the same with the Scythian or Swedish. Moreover, by considering the divine worship, customs, and letters, of the Phœnicians, he finds they owe their rise to the Scythians. As for the time of this expedition of Thor and Bacchus, whom, as well as his chief deputies, Mercurius, Inachus, and the rest, the poets call by the names of Hercules, Fenesius, Dionysius, and Titan, he concludes it to have fallen in with the times of Abraham, and his great grandfather Saruck, with which he finds the scriptures to agree.

Mineralia quædam, Conchylia petrifacta, et alia Fossilia è Berolina a Clariss. Christian. Maximiliano Spenero, Doct. Med. &c. N° 302, p. 2082.

A mere catalogue of 32 specimens of petrified shells and other fossils.

De Piscibus, Moluscis et Crustaceis Philippensibus, Ex MSS. R. P. Geo. Jos. Camelli ad D. Jacobum Petiver, S.R.S. transmissis. N° 302, p. 2085.*

Little more than a mere catalogue of certain fishes, molusca and crustacea, found on the shores of the Philippine isles.

Epistola Viri Reverendi D. Georgii Hickes,† *S.T.P. ad D. Hans Sloane, M.D. et S.R.S. De varia lectione Inscriptionis, quæ in Statua Tagis exaratur, per quatuor Alphabeta Hetrusca.* N° 302, p. 2076.*

A letter on the various reading of an inscription found on the statue of Tages, by four Hetruscan alphabets.

The Theory of Music reduced to Arithmetical and Geometrical Proportions. By the Rev. Mr. Thomas Salmon. N° 302, p. 2072.*

Having had the honour last week of making a musical experiment before the society at Gresham College, it may be necessary to give a further account of it; that the theory of music, which is but little known in this age, and the practice of it, which is arrived at a very great excellency, may be fixed on the sure foundations of mathematical certainty. The propositions, on which the experiment was admitted, were: that music consisted in proportions, and the more exact the proportions, the better the music; that the proportions offered were the same that the ancient Grecians used; that the series of notes and half notes were the same as our modern music aimed at, which was there exhibited on finger-boards calculated in mathematical proportion. This was demonstrated upon a viol, because the strings were of the greatest length, and the proportions more easily discerned; but may be accomodated to any instrument, by such mechanical contrivances as shall yield those sounds, which the music requires.

To prove the foregoing propositions, two viols were mathematically set out, with a particular fret for each string, that every stop might be in a perfect exactness: on these, a sonata was performed by those eminent violists, Mr. Fre-

* A strange irregularity in the pages of the original of this part appears, as in the titles of the papers in this page, and in several other places. But it was thought best not to correct them, otherwise the facility of referring to the Original would be obviated.

† A learned divine and antiquary of the 18th century. He died in 1715, aged 73. Besides his theological works, he wrote Institutiones Grammaticæ Anglo-Saxonicæ, et Antiqua Litteratura Septentrionalis.

deric and Mr. Christian Stefkins; when it appeared, that the theory was certain, since all the stops were owned by them to be perfect. And, that they might be proved agreeable to what the best ear and the best hand performs in modern practice, Signor Gasperini played another sonata on the violin in concert with them, in which the most complete harmony was heard.

The full knowledge and proof of this experiment may be found in the two following schemes, in which music is exhibited, first arithmetically, and then geometrically; the mathematician may, by casting up the proportions, be satisfied that the five sorts of half notes, here set down, exactly constitute all those intervals of which our music consists. And afterwards he may see them exhibited on a monochord, where the measure of all the notes and half notes comes exactly to the middle of the string. The learned will find that these are the very proportions which the old Greek authors have left us in their writings; and the practical musician will testify, that these are the best notes he ever heard.

Figure the 1st, containing the proportions set out arithmetically.

An eighth $\frac{1}{2}$						
A seventh $\frac{8}{15}$						
A sixth $\frac{3}{5}$						
A fifth $\frac{2}{3}$						
A fourth $\frac{3}{4}$						
A greater third $\frac{4}{5}$						
$\frac{8}{9}$	$\frac{9}{10}$	$\frac{15}{16}$	$\frac{8}{9}$	$\frac{9}{10}$	$\frac{8}{9}$	$\frac{15}{16}$
tone major :	tone minor	hemitone	tone major	tone minor	tone major	hemitone

An octave with a greater third. C $\frac{17}{18}$ *C $\frac{16}{17}$ D $\frac{19}{20}$ *D $\frac{18}{19}$ E $\frac{15}{16}$ F $\frac{17}{18}$ *F $\frac{16}{17}$ G $\frac{19}{20}$ *G $\frac{18}{19}$ A $\frac{17}{18}$ *A $\frac{16}{17}$ B $\frac{15}{16}$ C.

A $\frac{17}{18}$ *A $\frac{16}{17}$ B $\frac{15}{16}$ C $\frac{17}{18}$ *C $\frac{16}{17}$ D $\frac{19}{20}$ *D $\frac{18}{19}$ E $\frac{15}{16}$ F $\frac{17}{18}$ *F $\frac{16}{17}$ G $\frac{19}{20}$ *G $\frac{18}{19}$ A. An octave with a lesser third $\frac{5}{6}$.

The Explication of the first Figure.—Between the two lowest lines, you have the series of all the 12 half notes in an octave, from A-re to A-la-mi-re; which added together make an octave, or exact duple proportion: the several parts also added together make all those intervals of which it is constituted. As for example, the two half notes from A to A * $\frac{17}{18}$, and from A * to B $\frac{16}{17}$ make a major tone $\frac{8}{9}$; to which if a hemitone, from B to C $\frac{15}{16}$, be added, you have a lesser third $\frac{5}{6}$.

In like manner between the two next lines, you have the series of all the 12 half notes, in an octave from C fa-ut to C sol-fa-ut: the first two tones added together make a greater third: and so you may add a tone or hemitone, till you arrive at every interval in the octave, which is so called because, eight sounds are required for expressing those seven gradual steps, by which we commonly ascend to it. It may be also observed, that the proportions falling upon the same notes in two keys, one finger-board will be sufficient for both.

It is acknowledged by all who are acquainted either with speculative or practical music, that every interval is divided into two parts, one of which is greater than the other: an eighth $\frac{1}{2}$, into a fifth $\frac{2}{3}$, and a fourth $\frac{3}{4}$. Again, a fifth $\frac{2}{3}$, into a greater third $\frac{4}{5}$, and a lesser third $\frac{5}{6}$. Thus also a greater third $\frac{4}{5}$ must be divided into a tone major $\frac{8}{9}$, and a tone minor $\frac{9}{10}$. The lesser third (to comply with the practice of music) is rather compounded of, than divided into a tone major $\frac{8}{9}$, and a hemitone, which is its complement, $\frac{15}{16}$.

Three tones major, two tones minor, and two of the foresaid hemitones, placed in the order found in the scheme, exactly constitute the practical octave; which is so called, because it consists of eight sounds, that contain the seven gradual intervals. But it is also necessary to set down the divisions of the whole tones, which are the true chromatic half notes, because there is great use of them in practical music.

To make all our whole notes, and all our half notes of an equal size, by falsifying the proportions, and bearing with their imperfections, as the common practice is, may be allowed by such ears as are vitiated by long custom: but it certainly deprives us of that satisfactory pleasure which arises from the exactness of sonorous numbers; which we should enjoy, if all the notes were truly given according to the proportions here assigned.

It is very easy to satisfy ourselves in the arithmetical scheme, by those operations which Gassendus has set down in his Manuduction to the theory of music, tom. v, p. 635. As for example, his rule for addition is, that two proportions being given, if the greater number of one be multiplied by the greater number of the other, and the lesser by the lesser, the two numbers produced exhibit the compounded proportions. Thus, take a practical fifth $\frac{2}{3}$, and a practical fourth $\frac{3}{4}$, for the two proportions given; multiply 3 by 4, and you have 12; then multiply 2 by 3, and you have 6: which compounded proportion of 12 to 6, makes the practical octave $\frac{1}{2}$. Thus, according to his arithmetical operations of addition, subtraction, multiplication or continuation, and division, is our whole system proved; which, for the more easy application to practical music, may be exhibited geometrically on 6 parallel lines, representing the 6 strings of a viol.

This mathematical fixing of the frets enables every practitioner, who stops close to them, to give the proportions of the notes in a greater exactness, than can be done on the bass-violin or violin itself; since they may be set forth more perfectly by a pair of compasses dividing a line, than the nice ear can direct.

Though the frets for the several strings do not stand in a straight line, and the places are also shifted in different keys, yet the ear naturally directs the fingers to them: insomuch that those persons, who have always been accustomed to stop upon frets that go quite across the finger-boards of their instruments, do with very little practice fall rightly upon these. Such is the power of a musical genius, as may be undeniably proved by those that play upon the violin; who, when they change the key, fall upon the right stops, though they have no visible direction where to stop, nor time to alter, by the ear, the note they first pitched upon.

By this standard of regular proportions may the voice be formed to sing the purest notes; they are all the same in vocal and instrumental music; if then the instrument, which governs the voice, be perfect, the ear will of necessity bring it to perfection. It is a great pity that a good natural voice should be taught to sing out of tune, as it must do, if it be guided by an imperfect instrument; and this may be the reason why so few attain to that melody, which is so much valued; but since we now know wherein perfection lies, a constant practice will come to the attainment of it. The dividing wholes into chromatic hemitones is very necessary, but very difficult for the voice to be broken to: if it learns from an instrument whose whole notes and half notes are supposed to be equal, the sound must needs be very uncertain and unharmonical; whereas the proportions truly fixed, would bring it to a perfection in the nicest and most charming part of music.

The chromatic hemitones are the smallest intervals our modern music aims at; though the ancients had their inharmonic quarter notes, which they esteemed their greatest excellency: these may also in time be recovered, since we know their proportions; for as the diatonic tone is divided into chromatic hemitones, so after the same manner may the chromatic hemitones be divided into those least inharmonic intervals, which were always made use of. But if we go no further, yet this experiment demonstrates the true theory of music, and brings the practice of it to the greatest perfection.

Concerning the Bones of a human Fœtus, voided through an Imposthume in the Groin. By Sir Philip Shipton. N° 302, p. 2100.

I visited a woman, 66 years old, in Drury-lane, who had a child consumed in her uterus for about 28 years: she bore two children after this, one lived 11 years, and the other 6. About 8 years ago an imposthume broke out in the right inguen, and then several bones of a dead child were expelled. The woman has a great swelling now in that groin, where she feels something very hard, which she suspects are bones.

An Abstract of a Book, entituled, Νεκροκηδεια: *or, The Art of Embalming, &c. Part I. with a Map and fourteen Sculptures. By Tho. Greenhill, Surgeon, 4to. London,* 1705. N° 302, p. 2101.

An extraordinary Case of a Costive Person. By Mr. B. Sherman. With a Note on the same, by Mr. William Cowper, F.R.S. N° 302, p. 2110.

Thomas Phillips of Easthorp, in Essex, was in perfect health till he was a year and a quarter old, when a strange and almost continual rumbling in his intestines seized him; the consequence was a violent looseness, for which all the physicians near the place could find no remedy: but at last, when they despaired of the child's life, the looseness terminated in such an unusual obstruction, that he did not go to stool for 2 or 3 weeks together, and from 3 weeks it proceeded gradually to the intervals of 17 or 18 weeks, and so continued till he came to be about the age of 15, when his body resumed its natural temper, which lasted 4 or 5 years; but then the obstruction returned, and continued, or rather increased till he died; for it was customary with him, in the last years of his life, not to evacuate any manner of excrement under the interval of 19 or 20 weeks, and sometimes (twice at least) he had no discharge for 21 or 22 weeks together. He lived to be near 23 years of age, and walked about almost to the hour of his death; for he was suddenly seized with very sick fits, but could not vomit, two or three of which fits carried him off in a few hours; and when he died it was 9 weeks after he had any stool. The patient never vomited, nor ever felt any excrementitious taste in his mouth; neither did he sweat much, nor make more urine than in proportion to his drinking. When he did go to stool, he evacuated many times in a day, and that several days together, until he had emptied himself; and throughout his whole life he never discharged any other than very thin fæces.

Before his time of evacuation came about, he was of an extraordinary size for many weeks, unless he could break wind, which he often endeavoured to do, by laying his body on the edge of a table or stool, but often to no purpose. He declined the use of all medicines for many years before he died, contenting himself with going to stool once in 3 or 4 months, or 19 or 20 weeks, as abovementioned. But what was surprising, was, that he generally had a pretty good appetite, and eat and drank as the rest of the family did;

nay, till the time that his body came to be very full, he could work at the plough, or such like husband labour.

Mr. Cowper's Note on the foregoing Letter.—It is not improbable, if the abdomen of this person had been opened, but some of its contents would have been found not unlike those I have mentioned, in my explication of the 34th table of prints published by Dr. Bidloo, where I take notice of a young gentlewoman I dissected, in whom I observed the omentum so lessened, that at first it appeared doubtful if that part had ever been existent in the subject; but, on strict examination, the little remains of it resembled a congeries of small glandules, stuffed with a suet like matter. The whole canal of the intestines, even from the pylorus to the anus, was distended with fæces, and the surfaces of all the small guts adhered so strictly to each other, that they could not be parted without tearing their external membrane, to which the omentum contributed by its adhesion: the whole compages of the intestines very much resembled that of the external surface of the brain covered with the pia mater; so that the mesentery in that subject could not be seen, till this external inclosure was divided. By this disorder, it is certain, that the peristaltic motion of the guts must needs be very much lessened, if not quite hindered. The peritonæum also in that case was very much thickened, and had several preternatural white bodies set at various distances on its internal surface; the like appeared on the stomach, which very much resembled in figure the miliary glands on the back part of the aspera arteria.

A Letter from A. Mesaporiti of Genoa to A. Vallisneri, Professor of Physic at Padua, concerning a Hæmorrhage from almost every Part of the Body; also concerning a Pain of the Bowels occasioned by a Cartilaginous Thickening and Coalescence of the Intestines. Extracted from the Latin.* N° 303, p. 2114.

A young lady, aged 18, who had enjoyed good health before, was troubled with hæmorrhages from various parts of the body, and at length with transudations of blood through the pores of the skin. She was first seized on the 9th of April with a spitting of blood, accompanied with pleuritic symptoms, for

* Dr. Antony Vallisneri or Vallisnieri, celebrated for his observations in Natural History, was born 1661, at Tresilico, in the duchy of Modena, and studied at Bologna under Malpighi. Some years after taking his degree of M. D. he was appointed professor of physic at Padua: he was elected a member of the R. S. of London, and of various other learned and scientific associations. He died in 1730, aged 69. He bestowed considerable attention on the generation of insects and worms, and other subjects of natural history and physiology. His works amount to 3 vols. folio.

which she was blooded in the arm; afterwards she had a hæmorrhage from the nose, attended with head ach, on which occasion a vein was opened in the foot; notwithstanding which, blood still continued to be discharged from the mouth and nose alternately, accompanied with pain at the stomach, vomiting, head ach, &c. In the beginning of May the catamenia appeared in sufficient quantity, after which, however, the hæmorrhage and febrile symptoms returned, as before.

At a consultation of physicians, it was recommended that small quantities of blood should be occasionally drawn from the salvatella vein, and that anodyne and astringent medicines should be given. But these measures proved ineffectual; for in the beginning of June she had an eruption of blood from the ears, and what is more remarkable, a short time afterwards she had a hæmorrhage from the ends of the fingers and toes. Afterwards the blood burst forth from the navel, from the corner of the eye, and lastly, it transuded through the skin in various parts of the body; viz. from the middle of the thorax; from the foot, from the palms and back of the hands, from the chin, and from the tip of the tongue. Nevertheless the patient was not much debilitated; but related with a cheerful countenance all that had happened. On examining the palm of the left hand, a small cicatrix was discernible at the point where the eruption of blood had taken place the day before. This eruption was accompanied with a considerable degree of pain; but when the blood transuded through the skin, no cicatrix could be traced, and it was only known to have happened by the stain made upon the linen.

On the 14th of June she had a copious flow of the menses; after which the symptoms abovementioned left her for 20 days, whence some were induced to believe that the disorder was removed. After this, however, the perspirations returned, and the patient found herself getting weaker, in consequence of which she tried a change of air; but on the 5th of August she returned to town in all respects worse than before.

In this manner she continued until the middle of September, when she had a more profuse sweating of blood than ever; after this, she had none that was of any consequence. It should be mentioned, however, that from a slight excoriation above the ankle there flowed a large quantity of serum, which was with difficulty stopped by styptics, compresses and bandages.

On this case the author remarks, that several instances of hæmorrhages taking place from various parts of the body, in women labouring under a suppression of the menses, are recorded by Benivenius, Hollerius, and others; but that the present case is very different from those, and the more extraordinary, as there was no obstruction or deficiency of the menstrual evacuation.

Subjoined to the above history is a short notice of a labouring man, who died after having suffered for a great length of time excruciating pains and spasms in the bowels. On opening the body, the intestines were found compacted together by strong adhesions, and their coats thickened and indurated to such a degree, that the cavity or passage was nearly obliterated.

An Answer by Dr. Cockburn, to the Question proposed in a former Number of the Transactions, respecting a Method of proportioning the Doses of Emetics and Purgatives to every Age and Constitution in all Parts of the World. N° 303, p. 2119.

Dr. Cockburn supposes, 1. That emetics and purgatives produce no effect till they get into the mass of blood; and 2. That their general effect consists in a change produced in the crasis of the blood and other circulating fluids. From these two postulata he deduces the conclusion, that where the crasis of the blood is the same, the doses of medicines, in order to produce the required effect, must be proportional to the quantity of blood. Thus, for example, if a certain dose be required for altering the crasis of lbj. of blood to any given degree, a double dose will be required to alter the crasis of lbij. of blood in the same degree; a triple dose to produce the same alteration in lbiij. and so on. And universally, if the quantity of blood b require the dose d; the quantity of blood mb will require the dose md; that is, b : d :: mb : md. To enter into a further detail of this paper would be no ways instructive, as the postulata from which the conclusions are drawn are by no means admissible.

An Explanation of the Rule for finding Easter. By the Rev. J. Jackman. N° 303, p. 2123.

Having met with several explanations of the rule for Easter in our Common Prayer Book, and two published among the Transactions of the Royal Society, but none right and sufficient; and having undoubtedly collected the true sense of the same, by comparing the said rule and the table for Easter in the Common Prayer Book together, I have been persuaded to communicate it to you, in order to be inserted (if you think fit) in the next Philosophical Transactions, as here follows:

The Rule for finding Easter in the Common Prayer Book, is thus worded, viz

Easter-day is always the first Sunday after the first full moon, which happens next after the one-and-twentieth day of March. And if the full moon happens upon a Sunday, Easter-day is the Sunday after. For the right understanding of which, it is sufficient to observe, 1. That the full moon meant is the 14th day of the moon, according to the calendar in the Common Prayer Book, which may be called the church calendar, counting that day of the month for the first of the moon, which has the golden number of the year collateral to it in the first column of the said calendar. 2. And that these words, "next after March 21," are meant inclusively, as if it had been said, next after the commencement of March 21; so that if the full moon happens on March 21, the same must be the paschal full moon.

Now, in order to prove that these observations are both right, and sufficient for understanding the rule, I shall only suppose, that if they are necessary and sufficient to reconcile the rule with the authentic table to find Easter, (from which practice never varies) then are they right and sufficient. Which being premised:

1. I prove that the first observation is necessary to that end: because, if the paschal full moon be any day before or after the 14th of the moon by the church calendar, then the rule and the table will clash. For (1), if it be any day before, then as often as the said 14th of the paschal moon is a Sunday, that very day, at latest, must be Easter-day by the rule, as being a Sunday after the full moon therein meant: whereas by the table and practice it is not till the Sunday after that. Thus, Sunday, April 1, this year (1705) was the 14th day of the moon by the church calendar, and therefore must have been Easter-day, or after, by the rule, if the full moon therein meant had been any day before the said 14th of the moon; whereas Easter-day was April 8, by the table, and was observed accordingly. And this obliges us not to understand the true full moon, by the full moon in the rule, because that happens about 4 days before the 14th of the moon by the church calendar. (2) If the full moon meant in the rule, be any day after the 14th of the paschal moon by the church calendar, then as often as the said 14th happens to be Saturday, and consequently the full moon meant in the rule to be the Sunday following at soonest (that being the very next day) that Sunday cannot be Easter-day by the rule; whereas by the table and practice it is. Thus, Saturday, April 4, 1702, was the 14th day, of the moon by the church calendar; and therefore if the full moon meant in the rule were any day after that, it must have been on Sunday, April 5, at soonest; consequently April 12 at soonest must have been Easter-day by the rule; whereas April 5 was Easter-day by the table and practice: and this evinces the mistake of those, who make the 15th day of the moon to be the full in the sense of the rule; as Dr. Wallis, Philos. Trans. 240, Mr. Wright, in his Postscript to his Short View of Mr. Whiston's Chronology, &c. and the Introductio ad Chronologiam (reprinted at Oxford, A. D. 1704.) p. 37.

2. I prove the second observation necessary to the same end; because a full moon in the sense of the rule, (viz. the 14th day of a moon by the church

calendar, often happens on March 21; and in that case the Sunday following is always Easter-day by the table and practice; whereas it must be a month after by the rule, unless we understand these words, [next after March 21] as I explain them. And this will be the case next year (1706); nor does the proof of this point need the supposition of the foregoing, (though that may now be fairly supposed, as being already proved) for, count the full moon how you will, March 22 can never be Easter-day by the rule, unless March 21 may be the paschal full moon by the same; and yet March 22 is Easter-day by the paschal full moon by the table and practice, as often as the golden number is 16, and the dominical letter D.

I am aware that this 2d observation may seem forced and unnatural; and that perhaps might induce some to count the 15th day of the moon for the full in the rule; and Mr. Thornton, Philos. Trans. 297, to substitute March 20, in Leap-years, for March 21; neither of which Hypotheses, however, do any service, all things considered. The former indeed would vacate my second observation, March 21 never being the 15th day of the moon by the church calendar; but then it would make the rule notoriously irreconcilable with the table and practice, as has been already seen. And as to Mr. Thornton's Hypothesis, 1. The only colour for it (viz. that at the time of the council of Nice the vernal equinox was March 20 in Leap-years, and not March 21, as in common years) is, for any thing that I know, more likely to be false than true, and does by no means follow from the intercalation. 2. If this pretence were true, it was probably too great a nicety to have been regarded by the church. 3. This hypothesis forces more the words of the rule than mine. And lastly, if it were admitted, it would solve the difficulty only in Leap-years, and my second observation would still remain necessary, because the case happens as well in Common as in Leap-years; whereof we have an example in the next year (1706). Nor will my second observation be much questioned by such as know and consider the inclusive way of reckoning used by the Romans, and from them derived to all the Latin churches, and particularly that of England; for it is as proper to say, next after March 21, with the meaning I contend for, as to say, tertio (ante) calendas, nonas, vel idus, in the sense of the Roman calendar; or as to say (as our church does a little after this rule for Easter) that Ascension-day is 40 days after Easter, intending Easter-day itself to be one of those 40. And it is observable in this very rule, that after it had been said, that Easter-day is always the first Sunday after the full moon, &c. it is added, that if the full moon falls on a Sunday, Easter-day is the Sunday after; which had been a gross tautology, if by the first Sunday after the full moon, might not be understood the day of the full moon itself, when happening to be

Sunday. And if the Sunday of the full moon may be signified by the first Sunday after the full moon, then the full moon of March 21, may be signified by the full moon next after March 21. 3. I prove that my two observations are sufficient to reconcile the rule and the table; because I have drawn up a table to find Easter for ever, by the rule understood according to those observations, and in the plain and obvious sense in all other respects; and on comparing, have found it to agree in every particular with the table for the same purpose in the Common Prayer Book; and any body else may make the same trial.

Concerning some Norman Coins found at York. By Mr. Ralph Thoresby. N° 303, p. 2127.

A gentleman designing to build on a piece of ground, he had bought in High Ousegate, York, had labourers to remove the rubbish of a former house; which, with about 30 more, were burnt down April 3, 1694. In digging below the foundations of the former house, they discovered at a considerable depth the foundations of an older fabric, very probably unknown to the builders of the later house. These lower foundations were very well supported at several angles with good oak piles, some of which were still sound; besides these piles, there were several large timber trees, that lay athwart, to make the stronger foundation: between the head of two piles in this lower foundation, the workmen found a small decayed oaken box, in which had been hoarded about 200 or 250 Norman coins; but age and the moisture of the place had so defaced them, that little more than 100 of them could be preserved. I had the perusal of about half that number, which proved the noblest stock that ever I saw, or indeed heard of, of William the Conqueror's coins; only 2 or 3 being of any other prince: those, though later in time, are more rare in value than many of the Roman and Saxon coins: these lower foundations also very well answer the account we have of the timber buildings in those ages.

The coins are very much alike; the king is represented full-faced, with a crown and lables, but neither sceptre, cross, nor star, as in other monies of his that I had before; most of them are inscribed WILLEMV REX, which some have mistaken for William the 2d, but by the declining of the strokes it appears to be designed for V, as I have one with the S after the V and before REX. By this accident there appears greater variety than ever was known before of the Conqueror's money; I have of these sorts, WILLEMV REX. WILLEMVS REX. WILLEMV REX. I. (which is not to be reckoned a numeral letter, it being improper to pretend a distinction when there was none of the name before, but for part of the letter A) WILEM REX A. (Angliæ). And for the reverse I have some that were coined at LVNDRE (London), EOFER wick (York), WINC (Winchester), EXETE (Exeter), LIN (Lincoln, I presume, Lyn-Regis not being old enough), LINCOL (Lincoln), DEOTFORND (Thetford), and LOYNC (which I take for Loin or Lancaster).

Several Experiments on the Mercurial Phosphorus, made before the Royal Society, at Gresham-College. By Mr. Fra. Hauksbee, F.R.S. N° 303, p. 2129.

EXPER. I. *Showing that Light is producible from Mercury, by passing common Air through the body of it, after the Receiver is well exhausted.*—I took a glass recipient, open and ground at both ends, its content equal to about 30 ounces of water. The upper orifice was closed with a brass plate, by means of a wet leather laid on its edge. In the middle of this plate was screwed a stopcock; and at its lower orifice was inserted by cement a small glass tube, reaching from thence to near the bottom of a glass, included in the receiver. In this glass was put as much quicksilver as would cover the bottom of the tube about a quarter of an inch. Being thus prepared, (see fig. 18, pl. 8) and placed on the pump, the stopcock was turned, to hinder the air's passage that way, till the receiver was sufficiently exhausted. Which being done, and the stopcock returned, the air then rushed strongly through the body of the mercury, by passing the tube, and blowing it up with violence against the sides of the glass, that contained it, appearing all round like a body of fire, consisting of abundance of glowing globules, but descending again into itself; the phenomenon continuing till the receiver was half full of air.

EXPER. II. *Showing that Mercury will appear as a Shower of Fire, whilst descending in Vacuo, from the Top to the Bottom of a tall Receiver.*—A tall receiver being provided, about 21 inches high, to the upper orifice of which was screwed a glass, resembling those now commonly used for cupping, having an open passage through its neck; in which passage was cemented a piece of small tube, drawn taper to one end by the flame of a candle, which, with the cup, made an entire funnel; the small aperture of which was stopped with a round piece of stick as a plug, to prevent the mercury entering the receiver before its time. Within this tall recipient was included a glass, about the height of 17 inches, with a round crown like a shade. In this manner (see fig. 19) being placed on the pump, and about a pound and a half of quicksilver put into the funnel, the air was begun to be exhausted, and in 2 minutes it was sufficient to exhibit the phenomenon. Then loosening the plug that stopped the funnel, the mercury, by the pressure of the air, was driven violently into the receiver, striking strongly on the crown of the included glass, which broke the quicksilver into small particles, descending all around the exposed sides of both the glasses, and appearing like a shower of fire in a very surprising manner. The form of the receiver, as well as the included glass, was very distinguishable by its light, and continued so till all the mercury had entered through the funnel. What more occurred to my observation was, that the descending mercury resembled more the falling of snow, by the slowness of its motion, than that of rain: that none of it appeared luminous but what descended contiguous to the sides of the glasses: that some of the globules of mercury descended quicker than others, according to their different magnitudes: that the descending globules of light did not slide down the sides of the glasses, but were carried round by their own weight, as if they revolved on an axis: and, what was remarkable, that the descending globules had a double motion, the one perpendicular, the other as a rotation on an axis; and, in that motion, the adhereing parts of quicksilver were continually tearing from the sides of the glasses, producing an apt form, which in such a medium, from such a body, exhibits light: that the smaller globules, whose weight were not sufficient to cause their descent, remained opaque, there being (in this, as well as all other mercurial experiments) no light to be obtained without motion: that the same motion given to the like globules of quicksilver in common air, produce not the same effect, as I have lately tried, by forcing mercury through leather, by condensing air strongly on its surface. From all which it seems to appear very plain, that there is required the concurrence of a proper figure, medium, and motion, to produce the mercurial phosphorus.

Exper. III. *Showing, that it requires not so thin a Medium, as is made by the Weight of the Mercury in the Torricellian Experiment, to produce the Mercurial Phosphorus.*—To try whether so thin a medium as a vacuum, or the nearest approach to it, was absolutely necessary in the production of such a light as is discoverable in the barometer, by putting the mercury in motion, I made use of the gauge belonging to my air-pump. Upon the plate of the pump I placed a small receiver, the air from which being exhausted, the quicksilver in the gauge was elevated to 29 inches and a half. Then permitting some air to re-enter the receiver by the cock, the mercury in the gauge descended, and made several vibrations before it became stagnant: in all which it appeared luminous only while descending, till the quicksilver was purposely broken by a violent agitation of it: then the separate parts appeared light on their under surfaces, which became concave during their ascending, as the other were when they exhibited their light in descending: the convex surfaces being always opaque. These appearances continued on every admission of air, till near the half was admitted. But after that, though the mercury had the same motion given it as before, yet no light ensued. Hence it readily appears, that notwithstanding the mercurial phosphorus in the Torricellian experiment is not producible in so dense a medium as common air; so on the other hand, it requires a medium not so thin by much as the nearest approach to a vacuum to effect it.

EXPER. IV. *Showing that a considerable Light may be produced from Mercury in a Glass, by giving it Motion before the Receiver is quite exhausted.*—Having provided some quicksilver, very fine, and free from the least appearance of soil on its surface, the glass which held it being likewise very clean and dry; the glass, with the contained mercury, was included within a receiver on the plate of the pump, in form of fig. 20, from which I presently began to exhaust the air. But before it was quite exhausted (the mercury in the gauge not exceeding 28 inches, the barometer at the same time standing at 29 inches and a half), the pump was shaken, by which means the mercury in the included glass, being put in motion, exhibited such a light, that not only the receiver and included glass were distinguishable by it, but hands and fingers on the outside were so too. It is to be observed, that though the quicksilver would give a light on a small motion, yet the light was increased by the increasing that motion. What farther occurred was, that when a pretty brisk agitation was given to the mercury, it would resemble waves of light breaking on the sides of the glass, scattering some species of the same appearance towards the upper part of it: that on repeating the experiment three or four times, the phosphorus seemed every time to be more vivid than before, till at last, by often shaking the quicksilver, its surface became something solid, rendering the light less then, than it had been before: that in this, as well as in all other mercurial experiments, the light exhibited is of a very pale colour: that the first appearance of this light, is when about half the air contained in the recipient is exhausted, which still increases with the rarefaction; and confirms, with all the rest, the necessity of air rarefied to such a degree as to produce the mercurial phosphorus, concurring with that made by the motion of the quicksilver in the weather-glass.

EXPER. V. *Showing very odd Flashes of Light, on the repetition of the Experiment, resembling a Shower of Fire.*—The account already given of this experiment, so far as occurred at the first time of making, leaves but little room to add more. However, although but little, it is deserving of further notice: its appearance being not only very surprising, but distinguishably the clearest and most vivid light in all the mercurial phosphori, produced in rarefied air. On repeating the 2d experiment, the mercury not only appeared like a shower of fire, but from the crown of the included glass were frequently darted flashes resembling lightning, of a very pale colour, very distinguishable from the rest of the produced light. The flashes seemed then to be darted horizontally, though at other times I have observed them to incline upwards, and sometimes downwards, sometimes from divers parts, as well from the outward receiver, as included glass. Sometimes they would be thrown in a figure so odd, that I have no idea of any comparison. I have likewise observed them to proceed directly from the stream of quicksilver, as it descended from the funnel, before it reached the included glass. They generally fly to the side of the outer receiver, (unless their origin begins there, as sometimes I have observed) where the light breaks, and spreads in a very odd form. Something farther observable was, that the crown of the included glass appeared sensibly more luminous than any other part of it; which light was constant, without any alteration, during the descent of the mercury through the funnel, and that was at least 2 minutes of time. The quantity of mercury used for this experiment was about 3 pounds.

Exper. VI. *Showing that Abundance of Particles of Light are discoverable, by shaking Quicksilver in a Glass even in the open Air.*—In some of the foregoing experiments, where I assert that the phosphorus of quicksilver is not producible but in such a medium, I would there be understood, a light of the same nature and quality of that discovered in the barometer, by putting the mercury in motion; which light is very different from what is made by shaking mercury in the open air, as was very sensible in this experiment. For, having put into a clean glass globe, whose content was equal to about 30 ounces of water, about half a pound of pure quicksilver, then closing its mouth with a brass cap; in the middle of which was inserted a cock, by which means there was a free communication with the outward air; thus prepared, the globe was shaken, and the particles of light appeared plentifully, about the size of small pins' heads, very vivid, resembling bright twinkling stars, exhibiting some small faint light, like the whitish appearance of the via lactea. Their numbers increased according to the rapidity of the motion given. I then took the same glass, with the same mercury, and applied it to the pump, by means of a hollow brass pipe, which screwed on, both to the cock and pump, in manner of fig. 21. The air from within which being exhausted, and the cock turned, to prevents its getting in again, it was taken off, and moderately shaken; the mercury then appeared luminous all round, not as before, like little bright sparks, but a continued circle of light during that motion: but when that motion was checked with another of greater violence, it then appeared luminous almost all over the globe. On suffering the air to return, that mode of light vanished;

nor could any thing be recovered by shaking, but only the bright sparks, as at first.

Account of some Magnetical Experiments and Observations. By W. Derham, Rector of Upminster in Essex, and F. R. S. N° 303, p. 2136.

Having lately invented an azimuth-compass, as I was preparing it for observing the magnetic variation, I took occasion to try several magnetic experiments; and by that means met with this odd phenomenon. Having touched a piece of wire, so that it tended strongly N. and S., I had a mind to see whether it would have any inclination to either of the poles of the world, when turned round like a ring, so as that the two ends of the wire met. And having again straightened it, I was surprised to find it had quite lost its verticity. The cause of which I presently concluded to be the contact of the northern and southern ends of the wire, which I thought might so influence each other, as to confuse its poles; although I confess I had never observed any such confusion to arise from the bare contact of the northern and southern ends of two other touched pieces of wire.

On this I touched strongly the same, and other pieces of fresh iron wire, and having found them all to turn eagerly N. and S. I coiled them round so as that the ends should not come near each other, and again speedily opened them straight; and found, as before, that every piece had utterly lost its verticity: nay, the magnetic virtue was so absolutely destroyed by bending the wire, that it had not only lost its inclination to either pole, but the two ends of each wire seemed indifferent to the poles of the loadstone, viz. whereas, before the bending, the adverse poles of the loadstone would repel, and the similar poles attract the adverse, or similar ends of the wire; now the repulsive virtue was quite extinguished, and either end would indifferently be attracted by either pole of the magnet; just as if the wire had been heated red hot (which is well known to destroy the virtue) or never had been touched at all.

This I repeatedly experimented on wires of different lengths, with the same success. Only this must be observed, if you only bend the wire round in such a manner that it shall spring back into its place, or recoil, so as to be near the same straightness, that then no such, or but little of such effect, will ensue. But to produce this effect, the wire must be sharply bent, so as that violence may be exerted on it. If it be coiled two or three times round a small round stick, it will succeed best. It is also necessary that every part of the wire be bent, to evacuate the magnetic virtue: for if the ends, or any other part happen not to suffer the violence of bending, that part will retain its magnetism. As for instance, if the wire be all coiled, except half an inch, or indeed half a tenth of an inch at each end, every part so coiled will both lose its verticity, and will incline indifferently to either pole of the magnet; but the two ends, (although not able to turn the whole wire N. and S.) will fly from, or tend to the respective pole of the magnet: or if every part of the wire be coiled, except a small bit at one end only, all that coiled part, when extended, will be utterly deprived of its magnetism as before; and only that uncoiled bit retain its aversion, or inclination to the magnetic poles.

From the consideration of all which particulars, it is very manifest, that the violence exerted on the wire by bending, utterly extirpates the magnetic virtue, or at least makes such a confusion in it, that it is in a manner wholly destroyed: which seems a case very odd, aud never I think noticed before.

Further Observations and Remarks on the same Subject. By Mr. Derham. N° 303, p. 2138.

By consulting more accurately what others have written on magnetics, I find that Grimaldi, de Lumine and Colore, and M. de la Hire had both hit upon the same discovery before me. However, they have not prosecuted their discovery so far as I did, and my account contains divers things not taken notice of by them.

However some trials that I made did not succeed. I touched and coiled several iron wires, but the effect that ensued was not such as before-mentioned. The verticity was indeed much weakened, but not totally destroyed, and the ends of the wires would be attracted or repelled by the poles of the magnet; whereas I said they used only to be attracted. The next morning I tried again: and then the magnetism of the wires was totally destroyed, as I related. This experiment I repeated several times, and on various wires this winter, and commonly find, that, all the day, coiling will destroy the magnetism; but that it will not absolutely do it in the evenings. But whether it will have the same effect in all seasons, or whether it succeeds thus only in different times of the day, I must leave to further trials. I well know that the sphere of the activity of magnets, is more or less, at different times. That magnet in the Society's repository, found in Devonshire by Dr. Cotton, is known in some weathers, or at some times, to keep a key, or other piece of iron, suspended to another iron at 8, 9, or 10 feet distance.

Wishing to try the event of twisting iron wire from end to end, after it had been well touched; the success was, the verticity was always weakened, and sometimes inverted. And when it was so, the loadstone accordingly repelled or attracted, just as if the twisting the wire had given a new touch the contrary way. But in some wires so twisted, the verticity was wholly destroyed, or rather much confused. For I found by drawing one of the poles of the load-stone along near the sides of the wire, that in some places it would attract, in others repel; and so attract and repel all along the wire. Nay, I fancied that in some places, one side of the wire would be attracted, the other repelled, by one and the same pole of the loadstone. These changes sufficiently show that the magnetic virtue is put into great confusion by the violence exerted on the wire by twisting; which not only separates the fibres of the iron (as may be seen with the eye, but especially when assisted with a microscope) but also changes their situation from lengthwise to screw-wise.

I next tried what would be the issue of splitting or cleaving touched wires; particularly, whether they would exert the same effects that magnets are said to do, when sawn in two meridionally; concerning which, Dr. Ridley, in his Treatise of Magnetical Bodies and Motions, ch. 9, says, "Cut a piece from a magnet-stone meridionally, and that end which was placed south, when it was whole, being severed, will turn north, though naturally at first it was the south point." But Mr. Barlow is of a contrary mind, and says, that the poles of such a piece of magnet, when severed, will repel the same poles, to which it adhered in the whole magnet. But he subjoins, in his Magnet. Advertisements, ch. 2, "But here you must beware of an error, which some have fallen into, who observing the before-mentioned discord, erroneously supposed, that, if both these magnets, the greater and the less, i. e. the piece cut off, were conveniently placed to swim in water, the small one would not with its end point to the south pole of the earth, as it did in the magnet when entire, and when it was a part of the true north-end, but would point contrarily: there is, says he, no such alteration; but that both the great and small one, and all the like, that are cut meridionally one from another, will absolutely point the very same way, which the entire one did: only, the meridian will be somewhat removed, &c."

Dr. Gilbert is as express as Mr. Barlow. For, (lib. 2, c. 5) speaking of a magnet divided, and showing how that the parts which in the whole stone coalesced, do by separation repel each other, he says, "That what was the north and south pole before, is such still: for, says he, the verticity is not changed, as B. Porta erroneously asserts; and though the poles when divided do not incline to each other, yet both are directed towards the same point of the horizon. How the truth lies between Dr. Ridley and the two latter authors, I cannot determine, having never cut a magnet in that manner: but by the magnetic laws, as well as from the authority of Dr. Gilbert and Mr. Barlow, I doubt not but the latter is the truer opinion.

But in cleft wires the case is very various. Oftentimes the poles are quite changed; so that what was the north, becomes the south pole of the wire in all respects; I mean not only turning, but also embracing, or avoiding the poles of the loadstone, as if it had received a new and contrary touch. Sometimes one half of the wire will retain its magnetism, which it had before splitting, and the other half have it quite changed. Sometimes no change at all will ensue, only the magnetism be much weakened; as indeed it always is in all the experiments where the wire is split. But generally, where one of the halves has suffered change, and the other not, I have observed, that it is the thinner and weaker that has been changed, and the thicker has retained its virtue. Sometimes, where one of the split halves receives an inverted verticity, or seems to have no verticity at all, one of its ends will incline to one of the poles of the magnet, not according to its touch, but in an inverted order, and the other end be attracted indifferently by both the poles of the loadstone. And in some cases, that end shall be attracted by one pole, but be neither attracted nor repelled by the other; but stand as it were hesitating whether it had best fly to, or from that pole of the loadstone. Only, if that pole of the magnet be too near, then that end of the wire will constantly fly to it: as indeed it is the nature of all magnets and magnetic bodies to do, when they touch, or approach very near each other, though they repelled before.

The cause of these great changes in touched wire, produced by splitting, I have sometimes imagined to arise from the violence exerted on it by bending. But in some wires that I split, or cleft with very little bending, one half has been utterly changed, but the other not. In others that I cleft, by suffering the halves to bend as much as they would, no change has taken place; and some have suffered a total change. Sometimes I have imagined that the splitting the wires in a north or south position, or that the beginning to split at the north or south end of the wire first, might be the cause of this contraversion of the poles: but trials showed there was little in any of this.

There is one thing more that is very surprising, in split wire, viz. that the laying the one or the other side of the half uppermost, will cause a great alteration in its tendency, or aversion to the poles of the magnet. But if you lay the contrary side of that half uppermost, the same end shall be attracted by one, and repelled by the other pole of the magnet. In other pieces, where the ends are regularly attracted or repelled, only in an inverted order, as if new touched, if it lay with the round side uppermost at that time, and be then turned upside down, viz. the flat cleft side uppermost, it is ten to one if one of the ends be not either attracted by both the poles, or repelled by both; or else attracted or repelled by one, and hesitate as to the other: for so it often happens.

The cause of this lubricity of the magnetism, I imagined might be, because the sides or edges of the wire had received contrary poles by splitting: and consequently were turned topsy-turvy, and that what was the north might then be the south edge of the half. But I could never discover, but that the sides of each end, or of any other part, were the same, when I held the loadstone to one or the other side; which indeed I always did, in every experiment, for the greater certainty.

I tried also the old experiment of touching wires, by rubbing them backwards and forwards with one of the poles of the loadstone. Mr. Barlow discovered the error of this way of touching; viz. that it weakens much or hurts the touch. On the trial, I found what is said, not only to be true, but also that the reason of it is, because the poles of the wire, or needle, so touched, are not at the ends, but in, or near the middle of the wire or needle. Sometimes one is near the centre, the other at one or both ends. For in some wires so touched, both the ends of the wire would be attracted by one pole of the loadstone, and repelled by the other. And in such case the repelling pole always found a sympathetic part near the centre of the wire. In others (especially where a verticity succeeded, as sometimes it will do, and that pretty strongly too) the verticity would be inverted, and the ends of the wire be attracted and repelled in a direct contrary manner to the natural form. And the reason of all this will be manifest from these following experiments.

I touched a wire from end to end with only one pole of the magnet. This gave so strong a virtue, that I am almost of opinion, it is the best way of touching. The consequence was, the end where I began, always turned contrary to the pole that touched it. I again touched the same wire, and others too, with the other pole of the magnet, from the same end, and then that end turned the contrary way. Ex. gr. Mark one end of a wire for the north-end, and touch that wire, by drawing the north pole of the magnet several times along the wire, from the north to the south end: this wire so touched shall have a strong verticity; but the north end shall stand south. But if you touch that, or another wire, (for it is all one, because the latter destroys the former touch) by drawing the north pole of the magnet from the south to the north end of the wire, then this north end will turn north. And so it will do the same, if you touch with the southern pole from the north to the south.

Lastly, there is one experiment more that gives further light into the premises, viz. I touched an iron wire exactly in the middle with only one pole of the loadstone, without drawing it backwards or forwards. The event was, that in that place that pole of the wire was, and the two ends were the contrary pole of the wire; and were accordingly repelled or attracted by the poles of the loadstone; and the middle, and an inch or more on each side, were attracted by the pole only that touched it.

Concerning a Roman Inscription lately found at York, &c. By Mr. Ralph Thoresby, F. R. S. N° 303, p. 2145.

The Roman monument lately discovered at York was found in digging a cellar in Coning-street, not far from the Roman wall and multangular tower, described by Dr. Lister, in N° 149. This monument, dedicated to the genius or tutelar deity of the place, is not of that coarse rag, as most of the Roman monuments are, but of the finer grit, like the altar at the Lord Fairfax's house in York; it is 21 inches long, and 11 broad, and is inscribed GENIO LOCI FELICITER. There was a larger stone found near it, but without any inscription; nor is there upon either of them the representation of a serpent, or a young visage, by both which the ancients sometimes described these dij topici.

The author of this votive monument seems to have had the same superstitious veneration for the genius of York as those at Rome had for theirs, whose name they were prohibited to utter, or inquire after; hence it is that upon their coins the name of this deity is never expressed, but in a more general manner by genius P. R. or Pop. Rom. I have such a one of Constantius, minted at London, as appears by the exerg. LON. under the effigies of that deity, with a patera in the right hand, and cornucopia in the left, inscribed GENIO POPULI ROMANI. I rather instance in that of this emperor, the father of Constantine the Great, because he made York his imperial seat, and was here deifyed; the medal of whose apotheosis I also have, minted at the same place, and inscribed MEMORIA FELIX. As the genius of the city of Rome was expressed by G. P. R. so was the genius of the commonwealth, in a most sordid flattery, by that of the emperor, who they pretended to be their happy genius. I have one even of Nero, and not only after his quinquennium, but also the year after he had laid most of the city in ashes; yet by the express order of the senate, inscribed GENIO AUGUSTI. S. C. Some of the fathers have therefore justly reproached the Romans for paying a greater veneration to the genii of their emperors than to Jupiter their supreme god. This custom of deifying the genii, and that of assigning gods for defence of particular cities is very ancient, as appears by what the prophet Jeremiah saith of revolting Judah, "According to the number of thy cities are thy gods."

An Account of some Roman Coins found at Clifton, near Edlington, in Yorkshire. By Mr. Ralph Thoresby, F. R. S. N° 303, p. 2149.

These coins were dug up at the east entrance of Clifton, 3 miles from Doncaster, the Roman Danum, where the Præfectus Equitum Crispianorum resided, and one from Cunsbrough, or Coningsburgh, an ancient seat of the kings during the Saxon heptarchy. They were found so near the highway that the cart tracks had worn the earth off the top of the urn containing them. Upon further digging, there was found another theca nummaria, full of copper coins, as the former was. Both the urns were large and entire, the larger one might contain 2 gallons; by the fragments, the urns having been accidentally broken, they appear to have been of a finer clay than those found at South Holland, in Lincolnshire, mentioned in the Phil. Trans. N° 279, and the coins also much better preserved by being in a drier soil. Of 60 I bought of those found at Fleet, in that county, there was not one before Gallienus, nor after Quintillus; and of near 150 of these from Clifton, there is not one of an older date: so that both those in Holland and these in Yorkshire seem to have been hid in some common calamity that followed the death of that short-lived emperor Quintillus, who reigned only 17 days, Anno Dom. 271. And within 6 years after, we find that Probus the emperor, on some commotions in Britain, sent over certain Vandals and Burgundians, who had invaded Gaul, to inhabit Britain; upon whose arrival, probably such as had made the insurrection, might conceal their treasure, and being slain in the conflict, it lay hid till now. This I consider as a more probable conjecture than the persecution of Dioclesian, which was not till 34 years after. These, though they have not added one emperor to my collection, yet have they made a very considerable addition to the variety of reverses: so that I have above 30 of Gallienus.

An Account of a Book, entitled, Joh. Conradi Beckeri, Phil. et Med. D. Tractus Alsfeld. Medici Ordin. Paradoxum Medico-Legale de submersorum Morte sine pota Aqua, aliquot Cadaverum sectionibus detectum, et e principiis mechanicis illustratum. Cui adjicitur Dodecas Observationum circumstantiis curaq; rarissimarum. Giessæ-Hassorum, 1704. N° 303, p. 2152.

This author having made several experiments about drowning, and being dissatisfied with the various opinions that were held concerning it, was induced to write this present treatise, in which he relates these following cases as matters of fact.

On dissecting a drowned dog, that had been one hour under water, the abdomen appeared to be a little distended, as did likewise the stomach and intestines in a small degree; but no water was found in either of them. The lungs were likewise distended, but not more than the stomach or intestines, and appeared exactly as those in dogs that are hanged. An incision was made into the trachea, on which they sank down immediately. This at first made a great noise, the physicians were amazed at it, and every one much wondered how a dog could be drowned without water in him. This the author says was the first occasion of his dissenting from the ancients in cases of drowning.

The next observation is of a countryman, who having been missing for some weeks, was afterwards found drowned in a very shallow place. The body being cleaned from mud, some livid spots appeared externally, though we could not perceive any violence had been used. On opening the abdomen, the intestines appeared to be full. After we had tied up the gula and duodenum, that we might more easily judge of the contents of the stomach, we took it out, and by the weight of it judged there must needs be a very considerable quantity of water in it, neither were we mistaken in the quantity ; but upon cutting into it, it appeared to be the liquor he had been too freely drinking of; we thought there might be much about the same quantity in the lungs, by reason of their great distension ; but this was a mistake, it happened we were out in our guessing, for on cutting into the trachea the wind rushed out, and the lungs sunk down immediately. We understood by some persons present that they saw the deceased at a fair very much in liquor, and fancied that in passing over that river, in his way home, he unfortunately fell in, and was suffocated.

The next subject was a woman, who unfortunately fell into a well ; after the body was taken out, there appeared several large contusions : there were two on each arm, about the breadth of two fingers, but longer, which appeared as if made with a stick ; the blood was extravasated between the os bregmatis and panniculus carnosus on the left side ; there were two fissures on the os frontis, and one on each orbit of the eye ; under the pia mater of the left side was some black grumous blood ; the stomach and intestines were distended, which upon incision sunk down ; the lungs were cutinton severa l places, but not the least drop of water was found in them. Now if this woman had fallen into the water alive, it is plain she died for want of respiration ; but if the fall occasioned her death, it proves those to be mistaken, who think that water can get into the passages after death.

A man, after 5 days search, was found standing upright in the bottom of a fish pond ; he was carefully taken out and viewed, and not the least mark of any violence appeared. I myself, with the assistance of a surgeon, opened the thorax and abdomen : the intestines were very much distended, and contained in them a whitish liquor like chyle. The stomach was so swelled as induced the spectators to think it was full of water ; we tied up both the orifices, and took it out, that we might more easily measure the contents : it was so very light, that none but those that handled it could ever have imagined it ; on opening it, a great blast of wind rushed out, and it sunk down again immediately ; neither was there more than 6 ounces of a whitish liquor in it. The lungs were so distended with wind as to fill up the whole cavity of the thorax; we pricked and cut into them, but not a drop of water came from them. Hence we have all the reason in the world to believe that this person was stifled alive under water, and may see how fallacious that standing rule of the ancients is, which allowed no person to be drowned without considerable quantities of water in the lungs, stomach, and intestines.*

After these experiments, the author proceeds to answer the objection that was made, of persons recovering by being set upon their heads; and afterwards to show why it happens, that in drowned persons the water is seldom found either in the lungs or stomach.

Several Microscopical Observations on the Pumice-stone, Coral, Sponges, &c. By Mr. Anthony Van Leuwenhoeck. F. R. S. N° 304, p. 2158.

I have often heard it said, that the pumice-stone is found driving in the sea, and that the occasion of its lightness is, that it is calcined by the fire before it is thrown out of the burning mountains, after such a manner as to fit it for swimming on the top of the water. I have often observed this stone, but could never conceive that the cavities which are found between its parts, excepting those exceedingly small tubes or pipes, some of which appear to be hollow, could be occasioned by fire. For if it were true that such stones were thrown out of the volcanos very high into the air, they would probably be red-hot, and in that state falling, from whence it would follow, that they must necessarily sink; for it is certain that the heat cannot come into the cavities of the small pipes without driving the air out of them; now there being no common air in those pipes while the stones are glowing hot, and the stones falling immediately into the sea, the heat is expelled by the water, which insinuates itself presently into those cavities, and consequently the pumice-stone, having its tubes filled with it, will sink down to the bottom, and not rise to the surface of the water. That this is so we may be satisfied, if we take a piece of charcoal that has been prepared as usual, and throw it into the water, we shall see how high it will rise, by reason of its lightness, above the surface. Then this piece of coal being thrown into the fire till it becomes red-hot, all its tubes or pipes, instead of air, are filled with a subtile matter, which I shall call fire; now if you throw it immediately into water, the fire will be expelled from the pipes, and since

* See note at p. 323, vol. ii. of this Abridgment. In cases of submersion a small quantity of water does generally get into the trachea and bronchia; but not enough to destroy life, the extinction of which is, in such cases, to be ascribed to other causes, as shown by Mr. Goodwyn in his treatise on this subject.

there can be no vacuum, the water will immediately succeed, and the piece of coal must necessarily sink to the bottom.

Now, that we may the better conceive the configuration of the pumice-stone, I have placed several very small particles of it before a microscope. Pl. 9, fig. 1, represents a small particle of the fore-mentioned stone, in which we may perceive the very small tubes, though by reason of their exceeding smallness those cavities can be seldom seen; some of the largest of them are represented between G and H. I discovered also such exceedingly fine vessels, that several hundreds of them are not equal to a single hair in the wool of sheep. The stone is of a surprising and inconceivable figure; for whereas the canals or vessels in trees and plants almost all appear very regular, some running perpendicular, others horizontally, but the canals of this stone run so strangely, that one can hardly form any idea of them, especially when viewing its particles as they lie in and about those small cavities.

Fig. 2 represents a small particle of the said stone, of which QIKLMN lay near a cavity that was in the stone, and wherein are described, as well as it is possible, those small pipes of which that part is composed. Now as such a stone is continually growing, or, to speak more philosophically, the juices are always carried up through those pipes, I suppose there happened some obstructions in bringing those saps about LM, by which means those little branches, NO and PQ, shot out into the cavity of the stone.

I placed before another microscope a much smaller and thinner particle of the stone, as appears in fig. 3; of which RSTY lay near the cavity in the stone; and there happening some obstructions in the pipes or vessels about STV, by which the ascending juices were forced to alter their course, it occasioned an excrescence, which bent or protruded itself into the cavity of the stone, as at SVWXY.

On talking with the man where this stone is bought, he told me that it was true the pumice-stone is found floating on the sea, but that there are whole rocks of those stones, and that sponges grow upon them. Now, if what he affirmed were true, I imagined that some common or sea-salt would be found in some of the pores of this stone; to satisfy myself in this, I took a piece of stone about the size of the tip of my finger, and laid it on some charcoal till it was glowing hot, and then threw it into clean rain water, imagining it would sink to the bottom, but I was deceived in my expectation, for most of the water had insinuated itself into the pores or pipes of the stone. After this trial, I took the particles of the stone that were broken to pieces in the water, and laid them on a clean piece of paper, and then put those pieces into water again, in order to impregnate it with the salt particles that were in the stone; I then placed some of the water upon four distinct glass-plates, and when it was almost evaporated, I could perceive a great number of salt particles, whose basis was an exact square, all the four sides of which ascended pyramidically; the superficies of others, which could be but just perceived by the help of a good microscope, was a very small square, resembling the coagulations of common salt; and as this in moist weather becomes liquid, so also did these; but as soon as it was fair and dry weather again, the watery parts exhaled, and the salts resumed their former figure; I saw likewise among them some particles that were common salt.

Now, I doubt not that many of these pumice-stones are found in the sea; for having viewed several of them, they appeared not to have any sharp angles; and, among the rest, there was one of an oval figure, and as large as a man's fist, which when thrown into the water I saw it float with the half of its bulk above the surface of the water; from whence I concluded, that by its long driving in the water, the angles of such a stone were so worn off, that instead of having sharp and uneven points, they were grown to be blunt and smooth. The same stone being laid on the fire, I could perceive a smoky matter evaporating from it.

This last experiment induced me to take some of the little pieces of the stone, in order to keep together, as much as it was possible, the matter which was exhaled from them; and then I perceived that the first matter which was driven off, was as a very bright vapour; but the next, which was forced out by a yet stronger fire, was not near so bright, and was charged with immense numbers of exceedingly small particles, which, by reason of their weight, sunk to the bottom, after which that vapour became clear: there was also drawn off from the same stone a little matter, which I took to be sulphur.

I have before now heard, that the sponges which are found in the sea did grow upon the rocks; which however I could hardly conceive, believing that the solid rocks could not produce such a substance; but when I came to be informed that there are whole rocks in the sea composed of pumice-stone, it is easy to imagine that they may produce such a matter as we call sponge.

Having one of these sponges, in which I found a little hardness, from which I supposed the sponge received its first growth. Having opened that part of the sponge, I took out of it a little shell, and some very small stones: the shell was of a particular figure, and such as is not to be found on the sea shores. Then examining several other sponges, I took various small shells, horns, and stones out of them, and among the rest, one shell larger than any of those that I had seen before; from whence I concluded, that the storms, by putting the sea into an extraordinary motion, in or about those places where sponges grow upon the rocks, had raised from the ground these small shells and stones, and thrown them into the sponges while growing; the rather, because these little shells and stones were not only surrounded in such a manner by the sponges, as to impress their own figures in them, but even the parts of the sponges had insinuated themselves into the little shells, insomuch that they could not be separated without breaking some of the parts of the sponges.

Fig. 4 represents a small shell or scollop, which by tearing it out of the sponge was a little damaged, as is seen between C and B; at A there grew to it small parts of red coral, and upon C and D there lay many smaller particles of the same; there was also some coral on that side of the shell turned from the sight. Between B and C we also discovered an animalculum, represented like a snake or an eel; and I have observed the same not only upon this shell but also on several little stones that I have taken out of sponges.

I had also a small piece of a sea-shell, which we call a horn, upon which, in four several places, there grew little particles of red coral. This small piece of a sea-horn was grown over with a petrified matter, in which there was a great many small holes; and observing several small long animalcula, that were also surrounded with a petrified matter, and whose figures exactly agreed with those little holes, I began to consider whether these animalcula might not have belonged formerly to those sea-horns or shells. Fig. 5 represents the said particle of the sea-horn, upon which there grew several small particles of coral between G and H. I met likewise with two small pieces of an ossified or bony matter, which were hollow, and upon which likewise there grew a little coral.

Now, that red coral should grow in the bottom of the sea is impossible to be conceived; or that the coral matter, which is found on the forementioned shells and stones, can be said to grow there, is what, with submission, we cannot allow of; but it ought rather to be called a coagulation of such kind of matters, and who knows but that all the white and red coral that is found in the sea, is produced by such a coagulation of parts. As for the particles of the said coral, they are not composed of branches, but they lay by and upon one another, like the great sands that were joined to each other; and when viewed through the microscope, one can easily perceive that the parts of which they are composed were firmly united to each other, and that the ground to which they were fixed was more broad than high.

I took this forementioned scollop and sea-horn, which was overspread with coral, out of a great sponge; but though I examined several other sponges, yet I could meet with no shells that were covered with coral.

Now, whereas in the growth of all plants we may observe a sprouting out, which in the beginning we call a stem or stalk, from which stalk boughs or branches are produced, by which their parts become larger and more extended; but in the growth of sponges it happens quite otherwise, for they have no stem as far as appears, nor growing thicker and larger; for their beginning and ending is much of the same magnitude, and out of one of their first productions another like particle sprouts, and out of the second several others, but all of them very short. To conceive this, I have represented a small particle of sponge as it appeared through a microscope, in fig. 6, which I pared off a sponge, as thin and as small as possible; where the parts of the sponge are shown coming one out of the other, and then united together again; and though we cannot conceive how the sponge grows larger, yet we may see that this is the manner of its increasing.

For suppose that the parts broken off, at KL or MN, grows out so far that they come to touch one another, and to join, new parts will grow out of them, and unite themselves again, as we may see in the dissection of this small particle of sponge, which is wholly formed after this manner.

Concerning a large Ball voided by Stool. By Mr. Ra. Thoresby, F.R.S. N° 304, p. 2164.

Madame C—ly voided a ball, after such severe pains that her life was in danger, but she is not willing to have it cut, and the bulk of it is much short of what I can now give you an account of from another hand; but this may serve as a further instance of the danger of swallowing the stones of fruit, for immediately after, she voided several plum-stones, though she had not for a twelve month before eaten any of that fruit. Captain West told me he had once seen two stones voided by a neighbour, larger than any of those three formerly mentioned in N° 281 and 282: he has since procured them for me, and the lesser of them somewhat exceeds the largest of the others; the form of it is not much unlike the echinus shell, or helmet stone, flat on one side, and roundish on the other; it is above 6 inches one way, and 7 the other, in circumference. They weighed 9 ounces when first evacuated, and were voided by diet drink with an alkali powder, and a magistral stomach-plaster. The person died 7 years after, of one that was too large to be voided; for on griping it, between the hypochondria and share-bone, it felt to be as large as a goose's egg.

Experiments on the Attrition of Bodies in Vacuo; made at Gresham College. By Mr. Fr. Hauksbee, F.R.S. N° 304, p. 2165.

A description of the machine for giving a swift motion to bodies in vacuo, without admitting the external air, represented in fig. 22, pl. 8.—AA is a step-ladder, such as is commonly used in houses; BB a bar of iron passing through the middle of the upper step, and is fastened to the back board of the ladder by 2 nuts and screws, through both the board and iron; CC the jaws of the iron frame, which hold the great wheel D, 23 inches diameter, within its groove; E the brass plate of the air pump, on which the recipient ff is placed gg the spindle, to which bodies of different sizes may be fastened, by a hole passing through the middle of them, sufficient to receive the spindle; and by means of the 2 nuts, hh, a larger or a smaller body may be screwed fast between them; ii a brass plate, turned true to the ground edge of the recipient, on which it is placed, having a brass box in the middle, full of collars of leather well oiled, through which the spindle passes, the hole of the brass being also just fit to receive it; kk two pillars with nuts, to screw down a piece of board, which has an iron fastened to it to receive the upper point of the spindle, the lower one falling into a brass socket, screwed to the middle of the plate of the air pump; ll the supporters, reaching from the upper board of the ladder to the pillars, to prevent the recipient's being drawn from its place by the motion and tug of the wheel-band; mm the small wheel which the band surrounds from the great one, and is 1 inch and a half diameter; nn the winch which gives motion to the whole, the small wheel mm making about 15 revolutions to one of the large wheel D; so that a body fastened to the spindle gg, of the small wheel m, must be turned 15 times round to once of the great wheel; and accordingly as it shall exceed in diameter the small wheel, so will the velocity of the motion of the extreme parts be proportionably increased; oo are two screws, which fasten the ladder to the floor.

EXPER. I. *Showing that Light is producible on a swift Attrition of Amber on Woollen in Vacuo.*—Having prepared the machine above described, for giving a swift motion to bodies in vacuo, the amber made use of was beads, about the size of small nutmegs, and threaded; by which means a piece of wood was surrounded with them, which I had purposely caused to be turned, with a groove on the edge, to keep the beads from being displaced on a smart attrition; and between every bead a string was tied over from pin to pin, which were so many pieces of small wire, driven through the wood, for their better security, the beads appearing about half their diameter beyond the wood to which they were fixed, as in fig. 23. In this manner it was put on the spindle, and fastened there by the two nuts, before described, as in fig. 24. Then the brass plate on which the woollen was wrapped, being screwed to its place, (by means of the socket, which receives the lower point of the spindle) would then spring back, and embrace the amber with a moderate force, as in fig. 24. Thus prepared, and the receiver placed over, with its upper plate and box for the spindle to pass through, the pump was worked, and in a very little time the mercury in the gauge was elevated to about 29 inches and a half, which showed the recipient was well exhausted of its air. The large wheel being then turned, gave a very swift attrition of the amber on the woollen, but no light at first appeared; yet in a second or two of time it became visible enough: which shows it requires some degree of heat to produce the phenomenon. I could not observe that the continuance or increase of the motion, contributed any thing to the increase of the light, after the first production of it: nor would the light thus produced, remain on the amber to complete a circle, notwithstanding the velocity of the motion given; but died as soon as it had deserted the fricated woollen. However, the light continued without intermission, on the woollen, during the motion, where the attrition of the amber was made, and was discernible at 3 or 4 feet distance. What was further observable in this experiment, was, that it strongly confirms those made on the production of heat in vacuo by attrition: for the amber was not only moderately heated, but appeared to be burnt and cracked by the intenseness of it. And the woollen, against which it rubbed, appeared likewise not only discoloured, but perfectly scorched and burnt through: that although the same motion and the same attrition were given the amber in the open air, yet very little light ensued, in comparison of its appearance in vacuo: that the velocity of the extreme parts of the amber, was equal to something more than one third of a mile in a minute: supposing the large wheel to make 2 revolutions in a second of time, whose diameter is 23 inches; the diameter of the small one, moved by it, one inch and a half; and the diameter of the wood and amber, on the same spindle with the small wheel, 4 inches and a half.

EXPER. II. *Showing the Necessity of the Air's Presence, at least some Degree of it, in the Production of Fire, on the Attrition of Flint and Steel.*— Having provided a steel ring, about 4 inches diameter, and one-eighth of an inch thick, which between two pieces of wood of a less diameter, I fixed on the spindle with the nuts, as mentioned in the foregoing experiment; its edge verging about half an inch beyond the extremity of the wood that held it. The brass plate, which I made use of for fastening the woollen for the attrition of amber, served likewise to fix a piece of flint, an edge of which stood exposed to the steel, while the brass plate by its spring held the flint pretty strongly to it, notwithstanding some might be worn or chipped off by the rapidity of the motion. In this manner it was covered with a receiver, and a brass plate and box, as the former. But before any air was exhausted, the great wheel was moved, which gave motion to the small one, and consequently to the included steel; which exhibited sparks of fire in a very plentiful manner. After some air had been withdrawn, the great wheel was turned, as before, but the number of sparks then produced did not only seem to be lessened, but a sensible decay of their lustre and vigour was manifest. And so at every stop that was made, to repeat the experiment at greater rarefactions, the sparks produced still diminished in their quantity and light; till at last, when the receiver was well exhausted of air, then, although a more violent motion was given to the steel than before, yet not the least spark appeared to be struck from it: but a small continued light was visible on the edge of the flint, that was rubbed by the steel. On admitting a little air, some sparks, on the motion given, were discovered of a dull gloomy hue; but on letting in a little more air, I know not by what accident, the whole quantity insinuated, and then on repeating the wheel's motion, the sparks appeared as numerous and as vivid as at first.

EXPER. III. *On the Production of a Purple Light, on the Attrition of Glass on Woollen in Vacuo. With the various Phænomena of the same Experiment at several Trials, &c.*—The glass used in this experiment was globular, about 4 inches diameter, having a passage through the middle, to receive the spindle, to which it was fastened with corks and a screw. The woollen, against which it was to rub, was such as is commonly sold for gartering, the coarser sort of which I purposely chose for its harshness, thinking it likely to improve the phænomenon, beyond the list of cloth formerly used for the same purpose. This woollen was wrapped about the arms of the brass spring, described in former experiments, and being screwed down to its place, gently embraced the glass globe, as fig. 25. Being thus prepared, and the large receiver put over all, the pump was wrought, and in a little time the receiver was exhausted. The large wheel being then turned, it gave 15 revolutions to the included globe, to one of its own; in which swift motion, rubbing on the woollen, a fine purple light ensued, the included apparatus being distinguishable by it; and it continued so during the attrition. On admitting a little air, both the light and its colour diminished. And as the air was at several times permitted to re-enter the receiver, so the light became manifestly more pale, and less vivid. And even when the receiver was replete with air, a small faint light discovered itself on the same attrition given, as at first. What is farther observable in this experiment is, that the purple light seemed to be about the breadth of half an inch, and about one inch high, being no where visible but on each arm of the brass spring, where the glass in its motion rubbed on the woollen. That the light remained steady without the least undulation, notwithstanding the motion of the glass was considerably swift.

The first time I made this experiment, its success was much the same as I have just now related: but when I came to repeat it twice or thrice, with the same glass, no purple light appeared, a pale one then succeeded in its stead; nor could all the means I used recover it, till I took a new glass, which after I had used for the same purpose two or three times, served me as the former;

nor could it be revived again without a new one. In this experiment it was sometimes observable, that the glass, when taken out after a violent attrition, would be so hot as not to be held in the naked hand without a sensible offence. That the woollen on which it rubbed would not only appear discoloured, but sometimes perfectly burnt through. Sometimes a light would be carried quite round the glass globe, so making a continued circle of it, during its motion, notwithstanding it touched the woollen in no more parts than in the former experiments. At other times, a perfect halo would appear around the stagnant light; which seems to proceed from some few drops of water, that will sometimes insinuate by the spindle, through the box on the upper brass plate, where water is always kept, to prevent the entrance of air. This water descending by the spindle, till it reached some more extensive part, would there, by the violence of the motion, be thrown all about the receiver in small drops, some of which were likely to fall on the woollen, where being heated by the motion of the glass, evaporated, appearing like a halo around the light; for since I have made a contrivance, to prevent the waters scattering, no such appearance has happened.

Sometimes I have observed the light to break from the agitated glass, in as strange a form as lightning: particularly, when I used some list of cloth, that had been drenched in spirit of wine, which was fastened to one arm of the brass spring, on the other of which was tied some of the same list, that had been steeped in water impregnated with salt-petre; but both the pieces of list were very well dried before I made use of them.

At another time I made use of two flat shells of oysters well dried, instead of the woollen, for the attrition of the glass, each arm of the brass spring having one fixed to it. On the usual motion given the glass in vacuo, a light appeared, resembling a fierce flaming spark, just on the spot where the glass and shells touched. The light seemed not to extend itself, but was comprized in a small compass. I tried next what would ensue on the attrition of woollen on the shells in vacuo. The success was, that there was a light produced, but it appeared very dim and gloomy, at best like a faint halo.

In order to try in what degree the woollen might contribute to the phænomena of the foregoing experiments, I took some of the list formerly mentioned, and bound it about the edge of a wooden wheel, I had caused to be turned for that purpose; the wheel, with its wooden edge, I fastened on the spindle, as usual, and upon the brass spring was used the same gartering, as in the first experiment. These being put together, as in all the other experiments, and the receiver exhausted, the large wheel was turned, and on the attrition of the woollens, a small glimmering light succeeded; but the continuance of the motion, gave no encouragement to hope for any increase of it. I expected to have found the woollens at least discoloured by the friction, which was sometimes made more than moderate. On the contrary, not the least sign of any such thing appeared. The light totally disappeared, on the re-admission of less than a quarter of the recipient's natural contents of air, although the attrition then made, was as great as it had been at any time before. I cannot discover that the various colours of woollen contributes any thing to the different colours of light, exhibited in any of these experiments.

EXPER. IV. *On the Production of a considerable Light, by the Attrition of Glass on Glass in Vacuo, and in common Air.*—In this experiment I made use of a globular glass, about 3 inches diameter, fixed on the spindle, as in the last, and to the two arms of the brass spring were tied two slips of thin board, to each of which were first fastened two pieces of glass tube, by putting some small nealed wires through their cavities; which wires likewise passing through some holes of the board made for that purpose, kept them tight in their places, as in fig. 26. In this manner, being covered with a large receiver, and fitted in all respects as usual, the pump was wrought; which in a little time had exhausted the air from it. Then upon turning the large wheel, a swift motion was given to the included glass globe, in which motion, rubbing on the said tubes, a considerable light was exhibited; so that the whole included apparatus became perfectly distinguishable by it, and would have been much more so, had not the day-light prevented: it being then but very little after 5. P.M. with a clear horizon, and the experiment made in an open room. The colour of the light produced, resembled melted glass; not only just on the parts where the friction was made, but seemingly likewise at the extremities of the tubes A, B, where the globe in its motion did not touch them.

It was further observable in this experiment, that upon suffering the air to re-enter the recipient at several times, at each time, the motion being given as at first, no sensible decay of light or its colour appeared; excepting at last, when the tubes by much rubbing became worn, and consequently lessened in their spring, the light (in proportion to the abatement of their force on the moving body) was diminished; as I have often observed, when the motion has been made for some small time, either in vacuo, or in common air. And had the experiment been begun where it finished, the lesser quantity of light, from that cause, would then have been exhibited in vacuo. Hence it follows, that the different mediums did no ways contribute to the augmentation or diminution of the light produced, but seems wholly to proceed from the weakening of the spring, by the wear of their bodies, which is caused by the violent attrition of one upon the other in either medium. I do not find that the polish or glaze, on the outside of a glass, adds any thing to the light; having produced the same by a glass much worn by often using.

I have repeated the same experiment about noon in a clear day; and the success was, that the light produced in vacuo was then as sensible to sight, as a piece of red hot glass of the same size, at the same time, would have appeared in the open air. But though the appearance is such, it is no longer so than the motion is continued. Hence it is to be observed, that though it seems to be of the colour of red hot or melted glass, yet it is not really so; for if it were, it must of necessity some small time outlive the motion; which in the darkest night I could never yet discover.

Exper. V. *On the Production of Light by the Attrition of Glass on Glass under Water.*—This experiment was nothing more than a repetition of the last; saving only, that the included apparatus was entirely covered with water; from the surface of which, when the air came to be withdrawn, and the great wheel turned as usual, it was easy to discover a pretty smart light, on the first motion of the included glass globe against the tubes, illuminating the whole body of the water. The parts of the tubes, where the friction was made by the globe, were distinguishably red; but soon lessened in their appearance, and in a little time extinguished; the water, by the continuance of the motion, approaching nearer and nearer to the colour of whey; and at last became so thick, by the grit or powder produced by the attrition of the glasses, that the light could then be but just discovered through the body of it, and that not constantly, but like faint flashes at a distance. This experiment I have made, when it has succeeded so as to appear more luminous than at this trial. I have observed the dust, produced from the glasses by their attrition, through a good microscope, but could not discover, by the greatest attention, that the parts were any ways melted; only they appeared of a long and slender figure.

Of a Puppy in the Matrix that received no Nourishment by the Mouth. By Mr. Samuel Brady. N° 304, p. 2176.

In a conversation held about the nourishment of the fœtus in the womb, in which we seemed to agree that it was performed by the umbilical vessels only, without the assistance of the mouth, I mentioned a puppy at its full growth, which could never have received any nourishment this latter way. It had no appearance of a mouth at all, and lived some time after it was whelped; pulling the skin off its head, there was not the least passage through it; the head was one solid bone, without sutures, somewhat round like a man's skull, with a little prominence on the fore-part, resembling the os nasi of a man, but without any aperture. It had no place for eyes, nor meatus for ears, only the outward resemblance of one on each side, unperforated, and placed lower than naturally: no jaw-bone, nor conveyance to the top of the larynx and pharynx; but thence downward it was natural. The whole sufficiently showing that the animal could not receive any nourishment by the mouth.

An Account of an extraordinary sleepy Person. By Dr. Wm. Oliver, F.R.S. N° 304, p. 2177.

Samuel Chilton, of Tinsbury near Bath, a labourer, about 25 years of age, of a robust habit of body, not fat, but fleshy, and of a dark brown hair, happened, on the 13th of May 1694, without any visible cause, to fall into a very profound sleep, out of which no means employed could rouse him, till after a month's time; when he rose of himself, put on his cloaths, and went about his business of husbandry as usual; he then slept, eat and drank as before, but spake not one word till about a month after. All the time he slept, victuals stood by him; his mother fearing he would be starved, in that sullen humour, as she thought it, put bread and cheese and small beer before him, which was spent every day, and supposed by him, though no one ever saw him eat or drink all that time.

From this time he remained free of any drowsiness or sleepiness, till about the 9th of April 1696, when he fell into his sleeping fit again, just as he did before. After some days his friends were prevailed on to try what effect medicines might have on him; and accordingly, one Mr. Gibs, an apothecary, bled, blistered, cupped and scarified him, and used all the external irritating medicines he could think on; but all to no purpose; and after the first fortnight, he was never observed to open his eyes. Victuals stood by him as before, which he eat of now and then, but nobody ever saw him eat or evacuate, though he did both very regularly, as he had occasion: and sometimes they have found him fast asleep with the pot in his hand in bed, and sometimes with his mouth full of meat. In this manner he lay about 10 weeks, and then he could eat nothing at all; for his jaws seemed to be set, and his teeth clenched so close, that with all the art they used with instruments, they could not open his mouth, to put any thing into it to support him. At last, observing a hole made in his teeth, by holding his pipe in his mouth, as most great smokers usually have, they now and then poured some tent into his throat through a quill: and this was all he took for 6 weeks and 4 days; and of this, not above three pints or two quarts, some of which was spilt also; he had made water but once, and never had a stool all that time.

August the 7th, which is 17 weeks from the 9th of April, when he began to sleep, he awaked, put on his cloaths, and walked about the room, not knowing he had slept above a night, nor could he be persuaded he had lain so long, till going out into the fields he found every body busy in getting in their harvest, and he remembered very well, when he fell asleep they were sowing barley and oats, which he then saw ripe and fit to be cut down.

There was one thing observable, that though his flesh was somewhat wasted with so long lying in bed, and fasting for above 6 weeks, yet a worthy gentleman his neighbour assured me, when he saw him, which was the first day of his coming abroad, he looked brisker than ever he saw him in his life before; and asking him whether the bed had not made him sore, he assured him that he neither found that, nor any other inconveniency at all; and that he had not the least remembrance of any thing that passed or was done to him all that time. So he fell again to his husbandry, as usual, and remained well from that time till August the 17th, Anno 1697, when in the morning he complained of a shivering and coldness in his back, vomited once or twice, and the same day fell into his sleeping fit again.

Being then at Bath, and hearing of it, I took horse on the 23d, to inform myself of a matter of fact I thought so strange. I found him asleep, with a cup of beer and a piece of bread and cheese on a stool by his bed, within his reach: I took him by the hand, felt his pulse, which was at that time very regular; I put my hand on his breast, and found his heart beat very regular too, and his breathing was easy and free; and all the fault I found was, that I thought his pulse beat a little too strong. He was in a breathing sweat, and had an agreeable warmth all over his body. I then put my mouth to his ear, and as loud as I could called him by his name several times, pulled him by the shoulders, pinched his nose, stopped his mouth and nose together, as long as I durst, for fear of choaking him; but all to no purpose, for in all this time he gave me not the least sign of his being sensible. I lifted up his eye-lids, and found his eye-balls drawn up under his eye-brows, and fixed without any motion at all. Being baffled in all these trials, I was resolved to see what effect spirit of sal ammoniac would have, which I had brought with me, to discover the cheat, if it had been one; so I held my phial under one nostril a considerable time, which being drawn from quick-lime, was a very piercing spirit, and so strong I could not bear it under my own nose a moment without making my eyes water; but he felt it not at all. I then threw it at several times up the same nostril; which made his nose run and gleet, and his eye-lids shiver and tremble a very little; which was all the effect I found, though I poured up into one nostril about a half ounce bottle of this fiery spirit, which was as strong almost as fire itself. Finding no success with this neither, I crammed that nostril with powder of white hellebore, which I had by me, in order to make my farther trials, and I can hardly think any imposter could ever be insensible of what I did. I remained sometime afterwards in the room, to see what effect all together might have upon him; but he never gave any sign that he felt what I had done, nor discovered any manner of uneasiness, by moving or stirring any one part of his body, that I could observe. Having made these experiments, I left him, being pretty well satisfied he was really asleep, and no sullen counterfeit, as some people supposed.

On my return to Bath, and relating what I had observed, many gentlemen went out to see him, as I had done, to satisfy their curiosity, who found him in the same condition I had left him the day before; only his nose was inflamed and swelled very much, and his lips and the inside of his right nostril blistered and scabby, with my spirit and hellebore, which I had plentifully dosed him with the day before: his mother upon this for some time after would suffer nobody to come near him, for fear of more experiments on her son. About ten days after I had been with him, Mr. Woolmer, an experienced apothecary at Bath, called at the house, being near Tinsbury, went up into the room, finding his pulse pretty high, as I had done, took out his lancet, let him blood about 14 ounces in the arm, tied his arm up again, nobody being in the house, and left him as he found him; and he assured me he never made the least motion in the world when he pricked him, nor all the while his arm was bleeding.

Several other experiments were made by those that went to see him every day from Bath, but all to no purpose. I saw him myself again the latter end of September, and found him just in the same posture, lying in his bed, but removed from the house where he was before, about a furlong or more; and they told me, when they removed him, by accident, carrying him down stairs, which were somewhat narrow, they struck his head against a stone, and gave him a severe knock, which broke his head, but he never moved any more at it than a dead man would. I found now his pulse was not quite so strong, nor had he any sweats, as when I saw him before. I tried him again the second time, by stopping his nose and mouth, but to no purpose; and a gentleman then with me ran a large pin into his arm to the very bone, but he gave no manner of token of his being sensible of any thing we did to him. In all this time they assured me nobody had seen him either eat or drink, though they endeavoured it all they could; but it always stood by him, and they observed, sometimes once a day, sometimes once in two days, all was gone. It is further observable, he never fouled his bed, but did his necessary occasions always in the pot.

In this manner he lay till the 19th of November, when his mother hearing him make a noise, ran immediately up to him, and found him eating; she asked him how he did? He said, very well, thank God: she asked him again, which he liked best, bread and butter, or bread and cheese? He answered, bread and cheese: upon this, the poor woman overjoyed left him to acquaint his brother with it, and they came straight up into the chamber to him, but found him as fast asleep again as ever, and all the art they had could not wake him. From this time to the end of January, or the beginning of February, he slept not so profoundly as before, for when they called him by his name, he seemed to hear them, and to be somewhat sensible, though he could not make them any answer. His eyes were not now shut so close, and he had frequently great tremblings of his eye-lids; on which they expected every day he would wake; which however happened not till about the time just now mentioned; and then he waked perfectly well, not remembering any thing that happened all this while. It was observed he was very little altered in his flesh, only complained the cold pinched him more than usually, and so presently fell to husbandry, as at other times.

END OF VOLUME TWENTY-FOURTH OF THE ORIGINAL.

Concerning some Roman Inscriptions found at York, proving that the Ninth Legion some time resided there. By Mr. Ra. Thoresby, F.R.S. N° 305. p. 2194. *Vol. XXV.*

I here send you two Roman inscriptions found at York, which prove that the ninth legion was not only in Britain, but that it resided at York; which was heretofore unknown. It is a funeral monument, on which, under the statue (in basso-relievo) of the standard-bearer of the 9th legion, is this inscription:

L DVCCIVS
*L. VoL. RVFI
-NVS. VIEN
SIGNF. LEG. VIIII.
AN. XXIIX.
H. S. E.†

This monument was found in Trinity-yard in Micklegate, at York. That this 9th legion was in Britain in Galba's time, and that it was also Hispaniensis, appears from Sir Henry Saville's notes, at the end of his edition of Tacitus; but that it, as well as the 6th and the 20th, was also called Victrix, or that it resided at York, has not been observed before; and yet both are evident from this inscription upon a Roman brick found there:

LEG. IX. VIC.

This is also an argument of the peace which these parts enjoyed at that time, (perhaps the latter end of Severus's reign,) making bricks, casting up highways, &c. being the usual employment of soldiers at such vacancies.

* Lubens voluit. † Hic situs est.

Sir H. Saville was of opinion, that this nona hispaniensis in Britannia, was one of those established by Tiberius, Caius, or Claudius, or perhaps in the latter times of Augustus; but however that it was certainly here in Nero's reign; and that Pætus Cerealis was then lieutenant of it, is indisputably evident from Tacitus, lib. 14, cap. 10, who gives a lamentable account of the slaughter of 70,000 citizens and confederates, by the enraged Boadicea, in which number was all the foot of this ninth legion: Cerealis with the horse hardly escaping. I suppose it needless to add, that this number is frequently by the Romans written VIIII as well as IX; for one that is but competently versed in their coins or inscriptions, cannot but have observed instances of both kinds. Those who have written about the Roman legions, have said nothing about the place of residence of this 9th legion; only Ursatus, in his book de Notis Rom. remarks that it must be somewhere in Britain, because Tacitus tells us, that when the colony at Camalodunum, was destroyed by Boadicea, Pætilius Cerealis, legate of the 9th legion, came to their assistance; but yet he makes no mention of its being styled victrix.

De Quadrupedibus Philippensibus Tractat. a Reverendo Georg. Jos. Camel. transmissus Jacobo Petiver, Pharmacop. et Societ. Regiæ Soc. Londini. N° 304, p. 2197.

A catalogue, with very short remarks, of 49 specimens of the Philippine quadrupeds.

Microscopical Observations on the Seeds of several East-India Plants. By Mr. Anthony Van Leuwenhoeck, F. R. S. N° 305, p. 2205.

The euwane is a tree in the East Indies, much about the size of the elder; and in flower, scent, and figure, not very dissimilar; but the branches are armed with thorns. It is used inwardly by nobody, excepting some women, who, disagreeing with their husbands, make use of it in order to kill themselves; it being an infallible poison. When these women have taken such a resolution, they take half a handful of the leaves of the tree, and boiling them in water, they put to it a certain oil, called sinselen, and so drink or eat it up; half an hour after which, they perceive a kind of convulsion in their head, and vomit or retch four or five times; lastly, they lose their senses, and foaming at the mouth, they fret and speak like fools or mad persons till they die: so that it seems that the poison thickens the humours or fluid parts of the body, till their circulation quite ceases. Some end their lives in one, others in two or three days, according as they have taken more or less of the leaves. The seed of this tree is mostly of a triangular figure, and not above the 12th part of an inch where it is broadest. I took a little of it, and putting it into a clean paper, bruised it with a hammer, and after that into a little glass phial, then poured some fair rain-water on it, till the water stood half an inch above the seed. After the seed had been infused in the water some hours, I took a little of the water, and mixed it with my blood, as it dropped from my finger by pricking with a needle, and immediately observed that the blood was extremely coagulated. But as blood, when mixed with common water, keeps its clear red colour, and a great many globules, which are the cause of its redness, being dissolved in the water, they so incorporate themselves with it, that you can distinguish none of them from the water itself, which thereby acquires a fine crimson colour: the appearance was quite otherwise with the blood that was mixed with the seed water; for its particles being coagulated, assumed a blackish or dirty colour: and though I observed a very great number of blood globules that were not coagulated, yet they all lay like stiff particles; neither could I perceive that one of them was dissolved, or united to the water; so that not the least redness, that looked like blood, was communicated to the water, neither did there break forth the least air bubble out of the mingled liquor. I took also a little of the said water, that was inclinable to a reddish colour, and dropped some of it on six several places of a glass plate, in order to observe what salt particles might be coagulated in the exhaled liquor. I observed in the liquor, most of which evaporated, that besides the salt particles, there remained a great deal of a coagulated matter, in which I could discover no figure. I perceived likewise abundance of exceedingly small salt particles, which were mostly of an exact square figure, and some few were oblongs, with four right angles; some of those salt particles were broad in the middle, and pointed at both ends; but where a great many of them were coagulated together, their figures were irregular. I observed likewise, that where the water had lain a little time together, it was not altogether exhaled, but left a balsamic matter behind it. I put some of the said seed into water, in order to soften its skin, that I might dissect it the easier; and having accordingly opened several of them, I took out the plant, in which, though it was no larger than a small grain of sand, I could perceive two leaves, and that part of it which was to be the root and body. It was also affirmed, that the oil of singelan, or singely, is esteemed a good softener, and is given to lying-in women, and other persons that are in pain, also to children, with or without other ingredients.

The seed is about the size of the euwane seed, but something longer. I had also some few other seeds, named cancie; this seed is used by the Mahometans, being ground small and infused in water, which will make them as drunk as wine does others: if a person that is not used to it, should take but 10 or 20 grains, it would have the same effect as if they had drank 10 bottles of wine.

They say it makes them very stout and luxurious, but those that use it daily, and too often, bring themselves at last to 1 or 2 ounces; and then it will not have that effect, but rather the quite contrary, rendering them dull and doting, depriving them of their memory and appetite, and at last making them so lean as hardly to have any flesh on their bones; and this is the use the moorish kings make of it, when they wish to be rid of their great lords, whom they would make to die a lingering death; they cause such a drink to be made, into which they infuse also the seeds of poppies, and give it them twice a day to drink in the prison, more or less, according as they have a mind to dispatch them sooner or later; so that they shall live half a year, or a whole year, without knowing any thing of the matter. They call this drink bosta.

This seed is little used by them in physic, though I doubt not that it might be excellently well applied, because it does not only imitate the effects of opium but also, if there be not too much of it used at a time, it has the same operation as the best wine. This seed is about the size of hempseed, and has likewise such a hard skin, that one would be apt to take it for the same. I took some of these seeds, and stripped the hard skins from them, after that the thin membrane that covers the plant, and observed that the matter which lay within was nothing else but two leaves, with the root and body of the plant; but on separating these two leaves, I found that they involved two other very small leaves, long and slender, and of the figure of the former; and I also discovered that these small leaves had each of them four or five small ones, standing out about each other; from whence I concluded that the tree or plant which produces these leaves is notched or indented.

Afterwards I took some of the hempseed, which I thought I had well dissected, and of which I believe I have formerly given a description to the Royal Society; and examined the said seed anew, to try whether I could discover any such small leaves as I have found before in the cancie seed; when I found that all the parts agreed with that of the cancie seed; at first indeed, when I took the small leaves out of the larger, in which they were folded, I could not see those indented parts above mentioned; but on separating the leaves from each other, I could easily perceive them; and then appeared the two exceedingly long leaves, lying so regularly within each other, that the indented parts could not be discovered. I bruised a few of these little cancie seeds, and poured rain-water on them, in order to discover whether there were any salt particles in them; and though I let some of the drops of this water stand several days together, it did not not all evaporate; but there remained behind a thick moist oily matter, which I suppose was the cause that I could discover so few salts to be coagulated, and those that were there were very few, and that are hardly worth naming, of the figure of those found in wine-vinegar.

Account of River and other Shells, with various Vegetable Bodies, found underground. By the Rev. Mr. Morton, A. M. and F. R. S. N° 305, p. 2210.

On digging a moorish pasture, in Mears-Ashby field, in Northamptonshire, we found a vast number of snail shells of various kinds. At about a foot deep they lay very thick; and digging downwards the number rather increased till we came to the depth of about three feet. It was troublesome to sink deeper on purpose; but we made trials for a considerable extent of ground, viz. about 250 feet in length, and 130 in breadth. Besides, the same shells were thrown up in several places by the moles. What we principally observed in this search was, 1. A moist moorish black earth, in some places a foot and a half, in others somewhat above two feet in thickness. The lower half of it is blacker and denser than the upper, is of a bituminous nature, and has all the characters of peat earth. Besides shells, we found stalks and leaves of grass, and also of many other vegetables reposited, as usual, in like bituminous moors. 2. White earth; so at first we called it: but on closer inspection, it appeared to be little more than hay half wasted. So deep as we sunk into it, we found it every where copiously interspersed with shells.

The fossil shells were, some the exuviæ of land-snails; the rest of river or fresh water-snails: of the former there were the three following kinds. 1. A small buccinum of five wreaths, the buccinum exiguum quinque anfractuum, tit. 7. List. in tractat. de Cochleis Terrestr. Angl. A kind observed by Dr. Lister to live in moss upon old garden walls, at Estrope in Lincolnshire; by myself, at the mossy roots of old trees in many of the Northamptonshire woods, as also among moss on the boggy sides of several standing springs. 2. A cochlea of the compressed kind, but not so much compressed as some of them are. It has five wreaths, and a small circular sinus in the centre. This, if it is not the cochlea umbilicata, &c. N. 79, List. Hist. Conchyl. lib. 1, has not hitherto been mentioned by any writer; though common enough in the woods in Northamptonshire: I found a greater number of them, for the compass of ground, inclosed in the earth, than ever I have done in any of the places where they naturally breed. 3. The cochlea citrina tit. 3. List. de Cochl. Terrest. Ang. The common striped snail-shell. But most of these in the moor are white, of the colour of the shells that have been a long time dead. In some I saw faint traces of their former stripes. Most of the shells of this kind were lodged about 4 feet deep.

We met with only two different kinds of river shells. 1. A periwinkle shell of three wreaths, generally less than the buccinum trium spirar. tit. 24. List. de Cochleis. Fluviatil. Ang. There were a greater number of these buried in the moor than of any of the former kinds. 2. A periwinkle shell of five wreaths, much smaller and more prominent than those of the buccinum longum sex spirarum tit. 21. List. de Coch. Fluviat. It is otherwise very like that buccinum in the fashion of its wreaths. It has not yet been described by any author. We find the kind now living in one of the Northamptonshire brooks, called the Ise.

The moorish ground where these shells were buried, extends from near the top to very near the foot of a small hill. Above the moor, on the top, and at the brow of the hill, is a sandy soil, of a reddish colour. The whole face of the moor is plain and even, conformable to the rest of the hill, not thus moory, of the same declivity with it; and appears to be in a natural, and undisturbed state, as much so as any of the slades in the neighbouring fields, excepting that three or four trenches have lately been cut through it.

It is evident, that these shells were left here at the deluge, when those from sea were also deposited on land, and not buried since by deterrations from the ground above; for then the upper parts of the moor must have been covered with a reddish sand, such as the ground above is mostly composed of; but nothing like that appears near the shells in this moor. Besides here are dug up several shells that probably never bred here, but were inhabitants of a different soil, particularly the striped snail-shell. For these animals have peculiar soils, and affect particular regions.

An Account of a very large Tumour in the fore Part of the Neck, &c. By Dr. James Douglas. N° 305, p. 2214.

I lately had the opportunity of opening a woman, about 50 years of age, who had a very large tumour, or hard swelling, on the fore part of her neck; occupying all the space between the whole extent of the lower jaw and the upper part of the sternum, with a considerable rising in its middle; latterly its point inclining to the left side, though the largest part of the tumour was on the right. The skin on the apex of this protuberance was thin and shrivelled, of a colour different from the rest, and looked as if the swelling would have broken in that place. The skin was exceedingly thin, having no fat under it: only, in a cavity between two lobes, on its right side, there was a small appearance of some; for the skin being less stretched there, the cells of the membrana adiposa were not quite emptied. The fleshy fibres of the latissimus colli were scarcely visible. The mastoideus and coraco-hyoideus were extremely thin, and in their ascent they adhered very firm to the subjacent tumour. The sterno-hyoideus and the sterno-thyreoideus, that run up the fore part of this swelling, were distended so that it was difficult to separate them from it, especially the latter. The right carotid artery, in its ascent to the head, ran along its outer edge, which increasing, did much obstruct the current of the blood that way. The internal jugular, the par vagum, and the intercostal pair, went also over some part of this swelling, in their descent to the thorax. Two of the lymphatic glands of the jugular vein were swelled to the size of little eggs, being placed at some distance from each other, with a hollow between, where some fat was found; these two lobes made the tumour also very uneven on its right side. These muscles, the jugular, with the two glands adhering to it, and the rest of the fore-named vessels, being removed on both sides, I could easily observe the size, the figure, and the circumscription or limits of this preternatural tumour, with all its adhesions to the adjacent parts. In magnitude it seemed to exceed that of two fists joined together. Its figure was almost triangular, with a broad basis under the chin, sloping a little on each side, as it descended to the upper part of the sternum, where its point was pretty narrow; its surface was made uneven by three risings, of which the largest was turned to the left side, the other two being placed on the right, as above remarked. It adhered by membranous filaments to the maxillary glands, to the digastric muscle, and to the stylo-hyoideus; under which, on the right side, a small portion of it, in the form of a nipple, intruded itself as it were under the tongue; in the upper and fore part it also adhered to the os hyoides. Latterly it was connected to the levator scapulæ, and lower down to that part of the cucullaris that terminates in the clavicle; backwards to all the fore part of the aspera arteria, between its third or fourth cartilaginous ring and the os pectoris: as also to that muscle of the head called rectus internus major, and to some part of the scaleni; its lower part was engaged under the jugulum, or lunated part of the breast bone, to which it adhered.

It was easily freed from its connections to all these different parts, but not so from the glandulæ thyreoideæ, to which it adhered after a far different manner; for where the thyroidal glands are joined to each other, a little below the cartilago cricoides, on the fore part of the rough artery, there was no separating it, without cutting its substance; whence it plainly appears, that the union of these glands was the root or beginning of this excessive tumour: and yet, which is very remarkable, the glands themselves kept their usual figure, and were no larger than ordinary.

This tumour was hard and very firm, being exactly of the consistence of a cow's udder, when boiled: yet in a few places it was softish, containing a liquid and thick juice. Its colour was chiefly of a whitish yellow, only in some places it was exceedingly red, from its having a greater store of blood-vessels, and in others it was very white.

In cutting this tumour, I was not a little surprised to hear the edge of my knife grate against something hard, which made me proceed with caution, not to spoil whatever it was that made the resistance; I therefore pared off all the soft part, and the hard substance that remained I boiled, and then cleared it very well, having left sticking to it at one corner a soft cartilaginous body, which possibly, had the patient lived longer, would have acquired the same degree of induration. It very much resembles a piece of white unpolished rock coral; but whether it may be reckoned osseous, or if it be rather the viscid humour of the glands hardened and concreted into this irregular chalky or gravelly substance, or whatever else it may be, I leave to others to determine.

I remember that about two years since, I found in the prostates of a very old man, a great many hard bodies, like white peas, being of a substance exactly like this, only smoother on the outside; some of these were in the body of these glands, others adhered by small roots to the muscular membrane that invests them.

The first appearance of this large swelling was about 20 years before, caused by the breaking of a vein, as the good woman used to express it, in a hard and very difficult labour. It increased but very slowly, not arriving to any considerable bulk till a few years before she died; it was never very painful, being a true scirrhus; many things by several persons had been used and applied unsuccessfully. Its size at length became very troublesome, by impeding her swallowing and free breathing; and at last it quite choked her, by compressing the windpipe, upon which it lay.

But besides this, I observed another remarkable accident, which much hastened her end, being very painful and troublesome for a year or two before she died. The uterus was entirely scirrhous, and distended to that degree, that it filled up the whole capacity of the pelvis. Part of the colon and ileum adhered so firmly to it, that there could be no separation without tearing; both the ovaria and the tubæ grew close to it; and indeed the confusion and mixture of all these parts were so great, that if the ovaries had not been swelled here and there with hydatidal tumours, I could not have distinguished them. The neck of the womb was pressed down so low, that on a very gentle dilatation of the labia, it offered itself to view, being extremely hard, but yet smooth and even, and so closely shut, that I could pass nothing without cutting. It had squeezed the vesica urinaria so close against the os pubis, that it could contain but little or no urine, which obliged her to make it often, and with pain. The pressure of this part backwards was so great upon the intestinum rectum, that the evacuation of fæces had been obstructed for the space of 5 weeks before she died.

Indeed there was observed to come away per anum, for some considerable time, a great deal of pus and slimy matter, but that proceeded from the uterus; for the acrimonious humour, which was wont to be discharged per vaginam, having been pent up within its cavity by the close constriction of the collum uteri, had corroded, and eaten its way through the substance of the womb into the rectum, by which it had its vent; which deplorable case I have more then once observed in dissection. The thickness of the womb was near 2 inches, and in its bottom there was a great deal of this humour, white and thick, which upon touching made the ends of the fingers white and rough, by shrivelling the cuticle, as if I had washed them with a strong solution of some acrid lixivial salt. Thus the caustic salt lodged in soap affects the hands of those women that wash linen. It was very hard to take the uterus out of the pelvis, by reason of its so close adhesion to the neighbouring parts.

Concerning a Glade of Light observed in the Heavens. By the Rev. Mr. W. Derham, F.R.S. N° 305, p. 2220.

As I was observing the immersions of the 3d and 4th satellites of Saturn on the 20th of March, 1706, in the evening, I espied a very odd sort of light in the constellation of Taurus, the lower end of which was below the bull's eye, and the other a good way above it, and that star about the middle of its lower extremity, as in fig. 7, pl. 9. This glade of light had the same motion that the heavens had, and was much like the tail of a comet, but pointed at the upper end, as in the figure. This light, I doubt not, is such as Dr. Childrey first observed in England, and which Cassini and others afterwards observed in France, as Dr. Hook says.

Of an Experiment made before the Royal Society, concerning the Proportion of the Weight of Air to the Weight of a like Bulk of Water, without knowing the Quantity of either. By Mr. Fra. Hauksbee, F. R. S. N° 305, p. 2221.

Having procured a bottle, somewhat of an oval form, that it might more easily librate in water, that held upwards of 3 gallons, I put into it as much lead as would sink it under the surface of the water, and was, when weighed in that element, balanced by a small weight in the scale on the other end of the beam. I chose to include my weight, to prevent the inconveniency of bubbles of air, which I knew would plentifully adhere to and lurk in the irregular body of the weight, had it been fixed to the outside. Thus provided, and the bottle closed with common air, it was suspended by a wire at one end of a very good balance, and being in the water it was counterpoised by a weight of 385½ grains in the scale hanging by the other end. Then being taken out and screwed to the pump, it was in 5 minutes time pretty well exhausted, the mercury in the gauge being elevated to near 29½ inches. It was then taken off the pump, but first, by turning a cock that screwed both to it and the pump, the air was prevented from returning into it. In this manner it was again put into the water, and suspended as before to the balance, and it then weighed but 175½ grains, which subtracted from the first weight, gave 183 grains the difference, and was the weight of the quantity of the air drawn from the bottle by the pump. Then opening a cock under water, this was at first violently impelled into the bottle, but abating gradually of its force, till such a quantity had entered as was equal to the bulk of air withdrawn. So that by making the experiment after this manner, we need not be very solicitous in the nice exhaustion of the receiver, for it must of necessity answer reciprocally to the respective quantities taken out, the remaining air being weighed at last as well as at first, and no greater quantity of water can enter the receiver than what will supply the space deserted by so much air. The bottle now being again weighed, it was found to be 162132 grains. From which 175½ grains being subtracted, which is the weight of the bottle more than its like bulk of water, there remained 161956½ grains, which being divided by 183 grains, the weight of the air taken out of the receiver, gave the proportion as 885 to 1. The avoirdupois weights being brought to ounces, I reduced to grains, by multiplying them by 438, the exact number of grains contained in an ounce of that weight. The column of mercury in the barometer at the same time measuring 29.7 inches. The season of the year is to be considered, which was May, and I doubt not but if the experiment be repeated in December or January, a sensible difference will ensue.

An Experiment made at Gresham College, showing that the seemingly Spontaneous Ascent of Water in small Tubes, open at both Ends, is the same in Vacuo, as in the open Air. By Mr. Fr. Hauksbee, F.R.S. N° 305, p. 2223.

I took three pieces of small tubes of different bores, and fixed them in a piece of cork directly perpendicular, with their lower orifices as nicely even as I could. The same cork I likewise fastened to a wire, which passed through some collars of leather, included in a box on the upper plate of the receiver; by which means, I could at pleasure elevate or depress the tubes without any danger of the air's insinuation. Thus prepared, and tinged water set on the plate, the small tubes, which never had been wetted, were drawn to the upper part of the receiver by the moveable wire. Then the air being exhausted, the tubes were caused to descend, by the same wire that drew them up, till their lower ends were plunged just under the surface of the tinged liquid; where they no sooner arrived, than the water rose in each of them a considerable height above its surface in the glass, and higher in the smaller tubes than in the larger; and would retain such a quantity as voluntarily arose in them, if I may call it so, notwithstanding their lower orifices were drawn out of the water. Upon admitting the air again, they continued just the same as in vacuo. I found, by plunging tubes of several sizes in the tinged fluid, that so much of it would remain suspended in them, when taken out of it, as it would in such tubes, when plunged, be elevated above the surface of the stagnant fluid. I have likewise since observed, on bending some small tubes by the flame of a candle, in the manner of syphons, that it would require the orifice of the longer leg to be at least so far below the surface of the stagnant water, as that water in the same tube would spontaneously ascend in it, before it would run.

Of a Roman Sudatory, or Hypocaust, found at Wroxeter in Shropshire, Anno 1701. *By Mr. Thomas Lyster.* N° 306, p. 2226.

About 40 perches north of a ruinous wall, called the Old Work of Wroxeter, once Uriconium, a famous city in Shropshire, is a piece of arable land, a small square parcel, which was observed to be barren, and not to be improved by the best manure. On digging, there were discovered several bottoms of old walls, buried in their own rubbish, as are often found in those fields; in one of which, at the western corner of the said unfruitful spot of land, there was found a little door place, which led into a square room, walled about, and floored under and over, with some ashes and earth in it. This, as some suppose, was built in former times, for a sudatory or sweating house for Roman soldiers; being set with 4 rows of small brick pillars, 8 inches square, and laid in a strong sort of very fine red clay; each pillar being founded on a foot square quarry of brick; and upon the head of every pillar was fixed a large quarry of 2 feet square, almost as hard as flint, as most of those Roman bricks are, and within as red as scarlet, and as fine as chalk. These pillars were to support a double floor, made of very strong mortar, mixed with coarse gravel, and bruised or broken bricks: the first of these floors was laid upon the large quarries, and, when dry, the second floor was laid upon it. But first there was a range of tunnel bricks, fixed with iron cramps up to the wall within, having their lower ends level with the under sides of the broad quarries, and their upper ends level with the surface of the upper floor; and every tunnel had 2 opposite morticeholes alike, one on each side, cut through for a cross passage, to disperse the heat among them all.

Concerning the forementioned Hypocaust. By Dr. John Harwood, F. R. S. N° 306, p. 2228.

Wroxeter was one of the most considerable military stations or colonies the Romans had in this island: the city wall, as appears from a survey taken by Mr. Lyster, was not much less than 3 miles in circumference; and it is not improbable, but that it was founded by Suetonius Paulinus, or after by Agricola, in their march to subdue Mona, now Anglesey: vide Baxter's Glossarium Antiquitatum Britannicarum. Sir Christ. Wren discovered the remains of such another hypocaust, when they were laying the foundation of the king's house at Winchester. And Mr. Christ. Hunter in his Trans. N° 278, gave an account of an antiquity of this kind dug up in Yorkshire. Also Mr. Edward Lhwyd, in his additions to Camden, takes notice of another discovered at Kaerhyn in Caernarvonshire. And Mr. Camden himself mentions an hypocaust discovered at Hope in Flintshire, an account of which is to be met with in his Britannia, p. 688, of the English addition.

On the Hypocaust of the Ancients. By Mr. Baxter. N° 306, p. 2232.

The ancients had two sorts of hypocausts; the one called by Cicero, vaporarium, and by others, laconicum, or sudatio, which was a large sweating bath; in which there were tria vasaria ahena, called caldarium, tepidarium, and frigidarium, from the water contained in them. The other sort of hypocaust is not so distinctly treated by antiquaries: it was a sort of fornax, or kiln, to warm their winter parlours. This hypocausis was called alveus, and fornax: and the man that tended the fire fornacator. The tubuli seem to have been contrived to convey away the smother, that otherwise would choke the fornacator. This kind of stove seems to be graphically described by P. Statius in Balneo Hetrusci,

—— Ubi languidus ignis inerrat
Ædibus, et tenuem volvunt hypocausta vaporem.

The terrace floor is called by Vetruvius, testudo. Of the terrace Argol has these words: testudines sunt pavimenta sub quibus fornax ardet. And, by the bye, I take the word stove to be derived from æstus, quasi æstuvium; there wanting hitherto a probable etymon.

Concerning the Jaundice occasioned by a Stone obstructing the Ductus communis bilarius, which was afterwards voided by Stool. By Dr. Wm. Musgrave, F.R.S. N° 306, p. 2233.

Mr. Harvey, nephew to the physician of that name, showed me a stone, which he voided some years since, by stool; and which he said came from the ductus communis bilarius. The figure of this stone is oval; the length almost an inch; the breadth, or shortest diameter, $\frac{7}{10}$ of an inch: it weighed 59 grains, when I saw it; but, at its coming away it was above a drachm in weight; some part of it being rubbed off by frequent handling. The surface is rough, unequal, and divided into several little risings, each about the size of half a vetch, or somewhat less.

The many strong annular fibres, which appear not only at the orifice, where the ductus communis opens into the duodenum; but also all along the oblique passage of that duct, between the coats of the intestine, (which passage is, according to Dr. Glisson's measure, about half an inch in length) do, by way of sphincter, keep this end of the ductus communis very straight and close. And besides this straitness of the duct, the two oblique insertions it makes at some distance from the other, through the two outer coats of the duodenum, render it still more difficult, for a substance of any bulk to pass this way. So that, however large the stones may be that are generated in the gall-bladder, the ductus cysticus, the hepaticus, or communis, it is not easy to conceive how a stone, of the magnitude here described, could possibly, through a passage of itself so very narrow, strait, and difficult, be conveyed into the duodenum.

To prove that this stone was not formed in the alimentary duct, but, large as now it is, had come into it from the ductus communis, Mr. Harvey told me, that before the discharge of this stone, he had the jaundice; which came suddenly on him, and continued several months, in a severe and most excessive manner: that this jaundice, besides the discolouring of his urine and skin, to a very great degree, and besides loss of appetite, faintness, and many other symptoms, usual in this distemper, was also accompanied with a pain in, or near the stomach: that during the jaundice, his stools were of a white colour, as having very little or no mixture of choler in them: that, travelling under these circumstances, more especially with a constant pain, in his coach from London to Clifton, and soon after to Bath; he found, a little after his arrival there, that this stone came off by stool; and, together with it, almost a spoonful of gravelly matter; and a considerable quantity of choler, as appeared from the yellowness of the stools: all which happened so soon after he came to

Bath, as plainly to prove, that the discharge of both choler and stone proceeded from the motion of the coach.

That his deliverance from the jaundice, commenced from the expulsion of this stone: for soon after that, the colour of the skin and urine, indeed all the bad symptoms vanished; and in a very little time, weakness only excepted, he recovered.

All these circumstances put together, make a considerable argument, that the orifice of the ductus communis (how strait and how strong soever) was, in this gentleman, so far dilated, as to give way to the stone, here described; that is, dilated to a circle, $\frac{7}{10}$ of an inch in diameter, in circumference one whole inch and $\frac{3}{4}$*.

The jaundice is often observed to be a most stubborn distemper, not easily yielding to our most probable methods; and often to none at all. Riverius positively affirms, that when it proceeds from a stone, obstructing the current of the choler, it is incurable: urging this reason for his opinion; calculus, cum dissolvi non possit, morbum facit incurabilem. Capite de ictero. When the jaundice is thus difficult of cure, especially when there is a probability (whether from a pain fixed in or near the region of the liver, or from any good argument whatever) that it arises from the cause now mentioned; it may be adviseable to take exercise on horseback, in a coach, or any such way, as shall be likely to dislodge the stone, and bring it off. But to make this exercise effectual, it ought to be as violent, as the patient can well bear; and in such manner, as may, by much agitation of the body, be most conducive to the cure.

A further Account of an Eruption of Waters in Craven. By Mr. Ralph Thoresby, F.R.S. N° 306, p. 2236.

In Phil. Trans. N° 245,† is some account of an extraordinary eruption of water in Craven. I was lately inquiring concerning it, and am not only fully satisfied of the truth of what Mr. Pollard there affirms, but also that, as he conjectures, a great part of the land is not to this day recovered from the sand and stones, though a great number of people were employed about it. On the opening of the rock, at the foot of which the town of Starbotham stands, the water gushed out in so vast a quantity, as if it would have swept away the whole town: the waves came rolling down one upon another. Several houses were quite ruined; and others wrecked up to the chamber windows;

* There appears to be some mistake in one of these numbers: for $\frac{7}{10}$ of an inch diameter will answer to $2\frac{2}{10}$ inches in circumference; and $1\frac{3}{4}$ inch circumference would answer to but about $5\frac{1}{2}$ tenths.

† Abridgement, vol IV. p. 322.

one in particular so covered, that a large piece of the rock was left upon the top of the chimney. These things my neighbour was an eye-witness of, and had many a toilsome day in clearing some part of his land. His house was for some time full of people, who had lost their habitations by this sudden accident.

Observations of the Solar Eclipse, May 1—12, 1706, at the Royal Observatory at Greenwich, &c. By the Rev. Mr. John Flamsteed, Math. Reg. and F.R.S. N° 306, p. 2237.

The morning was cloudy and moist, till about 8 o'clock, when the clouds began to break, and afforded a sight of the sun through the spaces between them. A seven-foot telescope was fitted up with a scene, to receive the species of the sun cast through it, and on which it was about 7 inches diameter, divided into digits by 6 concentric circles. But clouds intervening, frequently rendered this way of observing inconvenient; therefore laying aside the apparatus of the scene, I viewed it through the same telescope with smoked glasses, to save my eyes, and noted as follows; where the times, in the morning, were those of the pendulum clock corrected.

8h. 21m. 30s. a very small part of the sun's diameter was eclipsed.—8h. 28m. the cord of the arc of the sun's periphery eclipsed was 14′ 40″: then followed frequent clouds through the spaces between, then some zenith distances of the sun were taken, for correcting the clock; and afterwards, near the middle of the eclipse; at 9h. 21m. 46s. the parts of the diameter remaining clear 5 00; at 26m. 20s. the same clear parts 4 30. At 10h. 31m. 50s. frequent large clouds again till the sun appeared through the breaks, and we saw the eclipse was not ended. Clouds again till 10h. 33m. 50s, when the sun shone out again, we saw his limb entire, and the eclipse certainly over.

At Canterbury Mr. St. Gray had prepared a scene placed behind his seven-foot glass, so that the species of the sun projected on it was 7 inches over; but having the same sort of weather we had at Greenwich, he did not see the beginning, by reason of clouds; but other phases with the end he noted as follows: corrected times by the clock.

At 8h. 53m. $5\frac{1}{2}$ digits darkened—9h. 8m. 7 digits—31m. 10 or more digits—36m. decreasing—55m. $7\frac{1}{2}$ a little clearer—57m. $6\frac{3}{4}$—10h. 2m. 6—4m. $5\frac{3}{4}$—14m. 4—16m. $3\frac{3}{4}$—20m. $2\frac{1}{2}$—30m. 1—31m. $0\frac{3}{4}$—10h. $36\frac{1}{2}$m. the end accurately with a tube of 16 feet.

At Horton, near Bradford in Yorkshire.—Mr. Abr. Sharp* cast the species

* Mr. Sharp, by private study, became an eminent astronomer, mathematician, and mechanist. He was descended of an ancient family at Little Horton, near Bradford, Yorkshire, where he was

of the sun on a scene plate, behind his 7-foot glass, so as it appeared 7 inches over. By reason of cloudy weather, he saw neither the beginning nor end: but other phases near the middle, as follows; the times by the clock corrected.

8h. 35m. 3 digits, dark;—9h. 1m. 7; by ocular estimation.—4m. 54s. $8\frac{3}{10}$, eclipsed on the scene.—6m. 33s. $8\frac{1}{2}$ digits.—7m. 53s. $8\frac{6}{10}$ digits.—12m. 50s. 9 digits.—16m. 8s. $9\frac{4}{10}$ digits.—18m. 48s. $9\frac{1}{2}$ digits exactly, the sun shining out clear.—20m. 45s. $9\frac{1}{2}$ digits, the sun still shining clearly. Greatest obscuration.—21m. 48s. $9\frac{1}{2}$ digits, still clear.—28m. 46s. 9 digits.—44m. 45s. 7 digits.—54m. 42s. $5\frac{1}{4}$ digits.—10h. 6m. 10s. $3\frac{1}{2}$ digits.—19m. 55s. 1 digit, precisely.—24m. the sun seen through clouds, the eclipse not ended.—30m. the sun seen again perfectly round and entire.

At Berne in Switzerland, Captain Stannyan observed, that the sun was totally darkened there for $4\frac{1}{2}$ minutes of time; that a fixed star and a planet appeared very bright; and that his getting out of the eclipse was preceded by a blood

born in 1651 or 1652, and where he died in 1742, in the 91st year of his age. Becoming very early acquainted with Mr. Flamsteed, and being an exceedingly accurate engraver and ingenious operator with all kinds of tools, he remained a considerable time with him, to assist in contriving, adapting, and fitting up the astronomical apparatus in the Royal Observatory at Greenwich, which had been lately built, viz. about the year 1676; where, with his own hands, he constructed, divided, and set up the large brass mural quadrant, of $79\frac{1}{2}$ or 80 inches radius. With this instrument he continued some time in making there various astrononomical observations, and assisting Mr. F. in settling the places and catalogue of 3000 fixed stars. But the fatigue of continually making such observations at night, and in a cold thin air, impaired his constitution, which was naturally delicate; so that he was obliged to quit this favourite situation, and retire to his family estate at Little Bradford, worth about 200l. a year, and which had devolved on him. Here he resided the rest of a long life, spent in close study and calculations, and ingenious mechanical devices. Here he furnished an observatory with instruments of his own construction entirely, as telescopes, quadrants, &c. and those of the very best kinds; here he filled a workshop with delicate tools of his own making, for various mechanical operations, as those of joiners, clockmakers, turners, opticians, mathematical instrument makers, &c. In this retreat also it was that he still continued to assist Mr. Flamsteed, by calculating most of the tables in the 2d volume of his Historia Cœlestis, and making the curious drawings of all the planespheres and constellations, &c. Here also he kept up a correspondence with the principal mathematicians in and about London, as Newton, Halley, Wallis, Hodgson, Sherwin, &c. to whom he was the common resource in many nice and troublesome calculations.

Though Mr. Sharp wrote and calculated so much for others, he published but little himself; indeed nothing it seems besides one volume in 4to. in 1717, intitled Geometry improved, by A. S. Philomath; which contains some curious and intricate geometrical disquisitions, illustrated with several copper-plates, most delicately engraven by himself. He undertook the quadrature of the circle in 1699, as deduced from two infinite series, which he extended to 72 decimal figures; he made also various improvements in the calculation of logarithms, with the tables of sines, tangents, secants, &c. the particulars of all which may be seen in the introduction to Dr. Hutton's Logarithmic Tables.

red streak of light, from his left limb; which continued not longer than 6 or 7 seconds of time; then part of the sun's disk appeared, all of a sudden, brighter than Venus was ever seen in the night; and in that very instant gave a light and shadow to things, as strong as moon-light uses to do.

Captain Stannyan is the first man I ever heard of that took notice of a red streak of light preceding the emersion of the sun's body from a total eclipse. And I notice it the rather, because it infers that the moon has an atmosphere; and its short continuance of only 6 or 7 seconds of time, shows us that its height is not more than the 5 or 6 hundredth part of her diameter.

Abstract of a Letter from Geneva, May 31, 1706, *N. S. by M. J. Chr. Facio Duillier, F.R.S. to his Brother Mr. Nic. Facio, F.R.S. Containing some Observations of the Sun's Eclipse, on the* 12*th of May,* 1706. *N. S.* N° 306, p. 2241.

There was a total eclipse of the sun observed at Geneva, which happened on the 12th of May, 1706, N.S. A little after the sun's rising, the sky seemed clear; though the air was already thick with some vapours. Many little clouds afterwards arose here and there, and the vapours increased much. For want of a pendulum clock, in a convenient place, the moment of the total immersion, that of the first immersion, and that of the end of the eclipse, could not be accurately observed. Though the sky was somewhat overcast, the heat of the sun was already felt, when the eclipse began: but a very sensible coldness took place, as the moon by degrees covered more and more of the sun, and the light decreased. The eclipse was observed only with some glasses, either darkened with smoke, or but little transparent; and by receiving the sun's image, through a six-foot telescope, which represented the objects inverted, on a white paper, placed at some distance, from the eye-glass. When the sun was near being totally dark, the bright crescent, which remained, was seen to diminish more and more, on the paper, where its image was received. And when that crescent was reduced to a very narrow breath, and to a very little length, it was seen suddenly to disappear: and in that moment the whole sun was eclipsed. At the same time, the darkness, which was already very considerable, became much greater. The clouds suddenly changed their colour, became first red, and then of a pale violet. There was seen, during the whole time of the total immersion, a whiteness, which seemed to break out from behind the moon, and to encompass it equally on all sides. The whiteness was but little determined on its outward side, and was not so broad as the 12th part of the moon's diameter. This planet appeared very black, and her disk well defined, within the whiteness; which encompassed it about, the colour of which was like that of a white crown, or halo, of about 4 or 5 degrees in diameter, which accompanied it, and had the moon for its centre. The planet Venus was seen at the same time, at some distance, without that crown, between the east and N. E. in respect of the sun. Saturn and Mercury were also seen by many, eastward from the sun's place. And had the sky been clear, many more stars might have been seen, and with them the planets Jupiter and Mars; that towards the east, and this toward the west: so that the seven planets might have been seen, almost all at once. Accordingly some persons in the country saw more than 16 stars; and many people, on the neighbouring mountains, saw the sky starry, in some places, where it was not overcast, as during the night in the time of the full moon.

The total immersion began about 3 quarters past 9. The duration of the total darkness was precisely 3 minutes, or 180 seconds, to the moment that the first ray of the sun began to appear again, with much brightness. And this time was observed with a simple pendulum; which was afterwards compared with a pendulum clock, showing the seconds, and regulated by the sun's mean motion. A little after the sun had begun to appear again, the whiteness and the crown, which encompassed the moon, entirely vanished. The sun then showed itself more and more; appearing at first like a small crescent, which gradually increased; and whose concave side seemed terminated, as by an arch described with the compasses. A little before the total obscuration, the country on the west side already seemed overcast with darkness; and after the total obscuration, the darkness was seen to leave us gradually, and to fly eastward. According to Mr. Professor Gautier's observations, the time from the first emersion of the sun, to the end of the eclipse, was just 1h. 9m. 30s.

According to observations of the same eclipse made at Marseilles, in the Observatory of the Jesuits of St. Croix; by Monsieur Chazelles, Engineer of the Galleys, and by Father Laval, Jesuit, Royal Professor of Hydrography.

The eclipse began at 8h. 28m. 40s.; it reached the sun's centre at 9h. 6m. 11s.; it was total at 9h. 34m. 15s.; the sun began to appear again at 9h. 37m. 9s.; the eclipse came again to the centre at 10h. 12m. 23s.; it entirely ended at 10h. 47m. 50s. Three stars were distinctly seen; and during three minutes it was not possible to read. And there remained one bright digit, all about the globe of the moon.

The manor house of Duillier is in the latitude of 46° 24′, and longitude 4° 13′ 45″ to the eastward of the Royal Observatory at Paris. And St. Peter's church at Geneva is in latitude, 0° 12′ to the southward, and in longitude, 0° 5′ 2″ to the westward of Duillier.

above 2 inches, which is considerably larger than ever I remember to have seen: the aorta in the abdomen, and iliacs, was mostly cartilaginous: the bones of the skull were sound and good: on the inside of the dura mater, by the falx, was a small ossification: the brain was more firm and solid than usual; and in cutting, hardly moistened the sides of the knife: the ventricles were full of serum: he had lost the use of his eyes for some years; but his hearing was good till he died: his genitals, both testicles and penis, were of a large size.

There is no doubt but that the weakness of his stomach, and the hardness of the aorta, were the causes of his death: the coats of the stomach were so thin, that they had not strength enough to keep out the air, and consequently his digestion must have been spoiled. He had not eat meat for some years, and latterly he lived only on small beer, bread and butter, and sugars. And it was impossible that his blood could circulate duly, while the great artery, having lost its elasticity, by being become cartilaginous, could give no motion to the blood: it is very probable that this was the cause of his irregular and intermitting pulse, which I have felt some years before he died. It is observable, that the greatest part of his blood (which was in greater quantity than I expected) was contained in the arteries; whereas generally in dead bodies the veins are full, and the arteries almost empty; for the arteries being distended by the blood, which they receive on the last systole of the heart, by their natural elasticity contract again, and empty themselves into the veins, from whence it returns no more; but in this man, the great artery having lost the power of contracting itself, it retained the blood it received by the last systole of the heart.

This account agrees with that given of old Parr,* by the famous Dr. Harvey, in most particulars, except in the causes of their deaths. But in both, nothing shows more remarkably the effects of old age, than the smallness of their spleens, which was doubtless owing to the contraction of their fibres in such a lax and spongy intestine. The whiteness of the viscera in both must be likewise owing, either to the same contraction or closeness of the coats of the blood vessels, or to a want of blood. Dr. Harvey says nothing of the quantity of blood he found in old Parr; but if we may conjecture from his body being fleshy, from the goodness of his stomach and appetite, and from the disease he died of, there could be no want of blood in him. In old Bayles there seemed to be more blood, than in several others I have seen, whose viscera appeared redder: and it can hardly be conceived, that the aorta could be so large, without a large quantity of blood, unless there had been some stricture on some other parts of it, which I did not perceive: and therefore it seems not improbable, that this whiteness of the viscera was owing to the closeness of the blood

* Vol. i. p. 321 of these Abridgments.

vessels in both. It is no small confirmation of this opinion, that the flesh and skin felt hard, and the brain firm and solid. I might add that it is highly probable, that the same disposition might give a closeness or hardness to the vessels every where else. It is true, this was a distemper; but then it is as true that it is a disease of old age, and may justly be reckoned one of the effects of it. And for a further proof of what I have said, I observed, that in preparing a piece of the small gut for an injection, the tunica villosa felt more like a fine file, than the softest velvet; and that I could use more violence in injecting the vessels, than these parts will usually bear. Whoever considers how soft a substance an animal body is at its first beginning, and how from time to time it acquires a firmness and solidity, will easily be induced to believe, that old age brings a more than ordinary hardness on all the fibres and vessels.

The necessary consequence of this hardness, and contraction of the fibres and vessels of old people, is a diminution of their secretions, which, cæteris paribus, are always proportional to the orifices of the glands. Hence it is that we find the skin of old people always dry, their perspiration being very little. They are likewise generally costive: old Bayles went to stool but once in 10 or 12 days, for some years; and old people are always complaining of a want of moisture; not that the radical moisture is dried up, but because the natural secretions, by reason of the contraction of the glands, are diminished. I have already observed, that we found in this old man more blood than could have been expected in such an emaciated body, and doubtless it had been larger, if his stomach and appetite had been as good as old Parr's. The fulness of the vessels, and the frequent rheums and catarrhs of old people, evince this necessary consequence of the closeness of the coats of the vessels: all which agrees with what the writers of institutions say, that old men are ratione partium solidarum frigidi et sicci, ratione excrementorum frigidi et humidi. From this retention of the excrementitious parts of the blood, we may expect all the bad consequences of a vitiated plethora, and languid motion of the blood; for the fibres of the arteries being now become hard, instead of assisting, they obstruct the heart in circulating the blood: and the quantity of animal spirits, separated in the glands of the brain, must likewise be less, not only because of the retention of the excrementitious humours, but also because of the closeness and firmness of the brain itself; so that the contractions of the heart and all the muscles must be weak, and consequently the motion of the blood languid. Gelidus tardante senecta sanguis hebet.

A due conformation of all the vital parts is most certainly necessary to bring a man to a full old age; but above all the rest, there are two which to me seem to have had the greatest share in procuring a longevity to old Parr and Bayles, by retarding the ill effects just now mentioned: the first is the heart, which in both was strong and fibrous; for that being left alone to labour the circulation of a large quantity of languid blood, a great force is absolutely requisite to propel it through unactive vessels, to the extremities of the body, and back again: no doubt this is more easily done in men of a low stature (as old Bayles was) which I am apt to think is a qualification to old age. The second was the largeness of their chests, and goodness of their lungs, by which the air had its full effort on every particle of the blood, in rendering it florid, and attenuating it so that it might easily move through the contracted channels of an old body. Few have the happiness of such a heart and lungs, yet most men wish to live long; nor was it easy for physicians to give rules for preventing the ill consequences of extreme old age, while the effects of a long circulation of the blood were unknown; of which we can be certain only by dissections of old persons, and these are not numerous enough to ground any thing certain upon: but if future observations shall confirm the remarks that have been now made, no doubt the indication will be to preserve such a softness in all the fibres, that they may easily yield to the pressure of the blood, and by their elasticity restore themselves to their former state, thereby giving a new impetus to the blood.

The Construction and Properties of a new Quadratrix to the Hyperbola. By Mr... Perks. Communicated by Mr. Abr. Demoivre, F.R.S. N° 306, p. 2253.

The circle, ellipsis, and hyperbola, being not geometrically quadrable, two ways have been used to find their areas. 1. By converging series; by which approaches are made nearer and nearer, according to any desired exactness. 2. By quadratices, that is, mechanical curves, which determine the length of certain lines, whose squares or rectangles give the area of the figure desired. Of this sort is the old quadratrix of Dinostratus, by which the circle and ellipse are squared; and another sort, for the same purpose, I inserted in the Transactions about 5 years ago. Since that, having found the construction of a curve, from whence, besides its own quadrature and rectification, the quadrature of the hyperbola is derived, I thought the following account might not be unacceptable.

Let AB, CD, fig. 8, pl. 9, be two straight rulers, of a convenient length, joined at A, and there making a right angle: EE is another ruler somewhat longer than AB; near the one end E, let a small truckle-wheel, represented edgewise by gh, and made of a thin plate of brass or iron, be fastened to the ruler by a pin, i, through its centre, so that the wheel may turn on the pin, i, tight to the ruler, without joggling. On the under side of this ruler let there be pinned or glued a small piece of wood, in the form of a quadrant, the part which is seen being marked kl, whose edge or limb kl, is an arch of a circle to the centre i, and radius *ih*, the same with the small wheel. The design of this piece is, that in the several positions of the ruler EF, the circular limb *kl* always touching and sliding by the edge of the ruler AB, the centre of the wheel may be always in a line, *im*, parallel to the ruler AB. In the ruler CD make MB = *ih* or *ik*, and at M fasten a small pin, and another to the ruler EF near the wheel, as at P. To these two pins let be fastened the two ends of a string MR, so that its whole length, from pin to pin, + P*i*, be equal to the intended axis, TW, of the curve.

The instrument being thus prepared, let a strong ruler SO, be held fast on the paper or plain that the curve is to be drawn on. Lay the ruler EF from M towards A, and parallel to AB, so that the string lie straight along the edge of the ruler EF from M to P, the point *k* of the quadrantal piece of wood resting on the edge of the ruler AB. Then with a small pin at M keeping the string close to the edge of the ruler EF, and with the other hand on the end E, keeping the wheel tight to the paper or plane, move the pin, string, and ruler EF, from M towards O, the ruler CD sliding along by the fastened ruler SO, in a right line; the wheel *h* will by its motion describe the desired curve TV.

Note.—The semidiameter of the little wheel must be about the sum of the thicknesses of the two rulers EF and AB, that it may touch the paper. Also it will be convenient that its edge be thin, and a little rough, that it may not slide flatwise, and that it may leave a visible impression.

From this construction the following properties are demonstrable:

I. It is evident from the construction, that the sum of the tangent and subtangent is every where equal to the same given line = MR + R*i* = TW. For the string, first straight at TW, afterwards making an angle at R, being every where the same, the line R*i*, or RP + P*i*, is always the tangent, and the remainder RM the subtangent; the contact of the wheel with the plane being the point of the curve to which they belong.

II. It hence follows, that any assignable part of the curve is rectifiable, or equal to eny assignable straight line. In fig. 9, let FAE be a part of the curve, its vertex F. HD*d* is the line described by the motion of the pin R in fig. 8, and may be shown to be an asymptote to the curve. FH a perpendicular to HD. Let A be a given point in the curve, AD the tangent, and BD the subtangent, to the same point A. Let *a* be another point in the curve, infinitely near to A, to which let *ad* be the tangent, and *bd* the subtangent. Draw AG, *ag*, perpendicular to FH, and AB, *ab* perpendicular to HD. By the construction, AD + DB = *ad* + *db*. Let *a*δ be made equal to *a*D, and draw Dδ. Then because *ad* + *bd* = AD + DB; subtract *b*D and *a*D, or *a*δ, from both sums, and there remains δ*d* + *d*D = A*a* + B*b*, or C*a*. A*a*C, D*d*δ are similar triangles, therefore C*a* (B*b*) : A*a* :: δ*d* : D*d*; and compounding, B*b* + A*a* : A*a* :: δ*d* + D*d* : D*d*; alternating, B*b*

$+$ Aa : $\delta d + $ Dd :: Aa : Dd. But Bb + Aa = δd + Dd, as shown above; therefore Aa = Dd. Aa is the fluxional particle of the curve FA, and Dd the fluxional particle of the line HD; these fluxions or augments, being equal, and their flowing quantities beginning together, are themselves therefore equal, viz. FA = HD.

Let FG = x, GA = HB = y, AD = t, BD = s. So is the curve FA = HD = $y + s$; that is, the curve from the vertex to any given point in it, is equal to the sum of its ordinate and subtangent, to the same point; which is its second property.

III. The next property, and from which I call it the hyperbolic quadratrix, is this, in fig. 9, let FAE be a part of the curve, &c. as before; FIKH a square on the line FH; AIL an equilateral hyperbola, whose vertex is I, its asymptotes HO, HR, and its axis HIμ. From a given point L in the hyperbola, below its vertex I, draw LA parallel to the asymptote RH, intersecting the diagonal IH in M, FH in G, and touching the quadratrix in A. I say, that the hyperbolic area ILM, is equal to a rectangle, whose sides are the ordinate GA and twice FH, the axis to the quadratrix, that is, the trilineal ILM = 2FH $\times$ GA.

Let FH = a, FG = x, GA = y. Because of the hyperbola GL $\times$ GH (LS) = FH^2; therefore GL = $\frac{\text{FH}^2}{\text{GH}}$; and LM = $\frac{\text{FH}^2}{\text{GH}}$ — GH (MG), that is, LM = $\frac{aa}{a-x} - a + x = \frac{2ax - xx}{a - x}$, and consequently the fluxion of the area ILM = $\frac{2ax - xx}{a - x}\dot{x}$.

In the rectangle triangle ADB, AB = $a - x$, BD = s, AD = $t = a - s$: then is AD^2 = AB^2 + BD^2: or $aa - 2as + ss = aa - 2ax + xx + ss$, which being reduced, gives $s = \frac{2ax - xx}{2a}$.

Let la be a right line supposed infinitely near and parallel to DA, and intersecting AB in C. Because of the similar triangles ACa and ABD, AB : BD :: AC : Ca, that is, $a - x : s\ (= \frac{2ax - xx}{2a}) :: \dot{x} : \dot{y}$, therefore $\dot{y} = \frac{2ax - xx}{2aa - 2ax}\dot{x}$; multiply each by $2a$, then it is $2a\dot{y} = \frac{2ax - xx}{a - x}\dot{x}$. The flowing quantity of $2a\dot{y}$ is $2ay$; and the flowing quantity of $\frac{2ax - xx}{a - x}\dot{x}$ is the hyperbolic area ILM, as above. These two areas beginning together at F and I, and having every where equal fluxions, or augments, are therefore themselves every where equal.

Note.—The quadrature of the trilinear figure ILM being thus found, any other area bounded by the curve line IL, and any other right lines, is also given.

IV. Supposing the same things as in the precedent proposition, I say, that the area of the quadratrix FabHF, is equal to half the square of FG, wanting the cube of Fg divided by 6FH, or FabHF = $\frac{xx}{2} - \frac{xxx}{6a}$. The fluxion of this area is the rectangle $caбB = a - x \times \dot{y} = \overline{a - x} \times \frac{2ax - xx}{2aa - 2ax}\dot{x} = x\dot{x} - \frac{xx}{2a}\dot{x}$. The flowing quantity of $x\dot{x}$ is $\frac{1}{2}xx$; and the flowing quantity of $-\frac{xx}{2a}\dot{x}$ is $-\frac{xxx}{6a}$. And hence also it follows, that the whole area, continued on infinitely towards E, is one-third of the square FIKH ; or $\frac{1}{3}aa$. For supposing $x = a$, the area above becomes $\frac{aa}{2} - \frac{aa}{6} = \frac{aa}{3}$.

While I was considering the other properties of this curve, and had given some account of them to my ingenious friend Mr. John Colson, he returned me a letter with the addition of the quadrature of the curve's area, which I had not then inquired into.

V. Supposing still the same things: I say that the solid made by the conversion of the area F*ab*HF about the line H*b*, as an axis, is equal to a cylinder whose radius is FH $= a$, and height equal to $\frac{xx}{2a} - \frac{x^3}{2aa} + \frac{x^4}{8a^3}$. And the whole solid made by conversion of the whole figure infinitely continued, is equal to an 8th part of a cylinder, whose radius and height are each equal to FH or a.

Let $\frac{p}{d}$ express the proportion of of the periphery and diameter of a circle. Then is $\frac{p}{d}\overline{ab}^2$ the area of a circle whose radius is ab. And because $ca = \dot{y} = \frac{2ax - xx}{2aa - 2ax}\dot{x}$, the fluxion of the solid is $\frac{p}{d} \times \overline{a - x}^2 \times \frac{2ax - xx}{2aa - 2ax}$, or $\frac{p}{d}(ax - \frac{3}{2} + xx\frac{x^3}{2a})\dot{x}$, whose flowing quantity is $\frac{p}{d}(\frac{axx - xxx}{2} + \frac{x^4}{8a})$; which solid being divided by $\frac{p}{d}aa$ (the area of a circle whose radius is a) gives $\frac{xx}{2a} - \frac{xxx}{2aa} + \frac{x^4}{8a^3}$, for the height of a cylinder on the said circular base, and equal to the solid made by the conversion of the area F*ab*HF about the line H*b* as an axis. When $x = a$, that is, when the whole figure is turned about its asymptote, the height $\frac{xx}{2a} - \frac{x^3}{2aa} + \frac{x^4}{8a^3}$ become $\frac{1}{8}a$.

VI. The curve surface of the solid generated by the conversion of the figure F*ab*HF about HB, is equal to the curve surface of a cylinder whose radius is a, and height equal to $\frac{x}{2} - \frac{xx}{4a} + \frac{xxx}{12aa}$. And the whole curve surface of the solid infinitely continued, is equal to one-third part of the curve surface of a cylinder whose radius and height are equal to FH or a. Which may be demonstrated after the manner of the preceding proposition.

VII. The radius of the curvature of any particle of the quadratrix is $\frac{tt}{a - x}$, and thus found geometrically. In fig. 10, FAE is the quadratrix, HD the asymptote, AD the tangent, BD the subtangent to the given point A. Make BV = AD; on

$+$ Aa : $\delta d + $ Dd :: Aa : Dd. But Bb + Aa = δd + Dd, as shown above; therefore Aa = Dd. Aa is the fluxional particle of the curve FA, and Dd the fluxional particle of the line HD; these fluxions or augments, being equal, and their flowing quantities beginning together, are themselves therefore equal, viz. FA = HD.

Let FG $= x$, GA = HB $= y$, AD $= t$, BD $= s$. So is the curve FA = HD $= y + s$; that is, the curve from the vertex to any given point in it, is equal to the sum of its ordinate and subtangent, to the same point; which is its second property.

III. The next property, and from which I call it the hyperbolic quadratrix, is this, in fig. 9, let FAE be a part of the curve, &c. as before; FIKH a square on the line FH; AIL an equilateral hyperbola, whose vertex is I, its asymptotes HO, HR, and its axis HIμ. From a given point L in the hyperbola, below its vertex I, draw LA parallel to the asymptote RH, intersecting the diagonal IH in M, FH in G, and touching the quadratrix in A. I say, that the hyperbolic area ILM, is equal to a rectangle, whose sides are the ordinate GA and twice FH, the axis to the quadratrix, that is, the trilineal ILM = 2FH × GA.

Let FH $= a$, FG $= x$, GA $= y$. Because of the hyperbola GL × GH (LS) = FH^2; therefore GL $= \frac{\text{FH}^2}{\text{GH}}$; and LM $= \frac{\text{FH}^2}{\text{GH}}$ − GH (MG), that is, LM $= \frac{aa}{a-x} - a + x = \frac{2ax - xx}{a - x}$, and consequently the fluxion of the area ILM $= \frac{2ax - xx}{a - x}\dot{x}$.

In the rectangle triangle ADB, AB $= a - x$, BD $= s$, AD $= t = a - s$: then is AD^2 = AB^2 + BD^2: or $aa - 2as + ss = aa - 2ax + xx + ss$, which being reduced, gives $s = \frac{2ax - xx}{2a}$.

Let la be a right line supposed infinitely near and parallel to DA, and intersecting AB in C. Because of the similar triangles ACa and ABD, AB : BD :: AC : Ca, that is, $a - x : s\ (= \frac{2ax - xx}{2a}) :: \dot{x} : \dot{y}$, therefore $\dot{y} = \frac{2ax - xx}{2aa - 2ax}\dot{x}$; multiply each by $2a$, then it is $2a\dot{y} = \frac{2ax - xx}{a - x}\dot{x}$. The flowing quantity of $2a\dot{y}$ is $2ay$; and the flowing quantity of $\frac{2ax - xx}{a - x}\dot{x}$ is the hyperbolic area ILM, as above. These two areas beginning together at F and I, and having every where equal fluxions, or augments, are therefore themselves every where equal.

Note.—The quadrature of the trilinear figure ILM being thus found, any other area bounded by the curve line IL, and any other right lines, is also given.

IV. Supposing the same things as in the precedent proposition, I say, that the area of the quadratrix FabHF, is equal to half the square of FG, wanting the cube of Fg divided by 6FH, or FabHF $= \frac{xx}{2} - \frac{xxx}{6a}$. The fluxion of this area is the rectangle cabB $= a - x \times \dot{y} = \overline{a - x} \times \frac{2ax - xx}{2aa - 2ax}\dot{x} = x\dot{x} - \frac{xx}{2a}\dot{x}$. The flowing quantity of $x\dot{x}$ is $\frac{1}{2}xx$; and the flowing quantity of $-\frac{xx}{2a}\dot{x}$ is $-\frac{xxx}{6a}$. And hence also it follows, that the whole area, continued on infinitely towards E, is one-third of the square FIKH; or $\frac{1}{3}aa$. For supposing $x = a$, the area above becomes $\frac{aa}{2} - \frac{aa}{6} = \frac{aa}{3}$.

While I was considering the other properties of this curve, and had given some account of them to my ingenious friend Mr. John Colson, he returned me a letter with the addition of the quadrature of the curve's area, which I had not then inquired into.

V. Supposing still the same things: I say that the solid made by the conversion of the area FabHF about the line Hb, as an axis, is equal to a cylinder whose radius is FH $= a$, and height equal to $\frac{xx}{2a} - \frac{x^3}{2aa} + \frac{x^4}{8a^3}$. And the whole solid made by conversion of the whole figure infinitely continued, is equal to an 8th part of a cylinder, whose radius and height are each equal to FH or a.

Let $\frac{p}{d}$ express the proportion of of the periphery and diameter of a circle. Then is $\frac{p}{d}\overline{ab}^2$ the area of a circle whose radius is ab. And because c$a = \dot{y} = \frac{2ax - xx}{2aa - 2ax}\dot{x}$, the fluxion of the solid is $\frac{p}{d} \times \overline{a - x}^2 \times \frac{2ax - xx}{2aa - 2ax}$, or $\frac{p}{d}(ax - \frac{3}{2} + xx\frac{x^3}{2a})\dot{x}$, whose flowing quantity is $\frac{p}{d}(\frac{axx - xxx}{2} + \frac{x^4}{8a})$; which solid being divided by $\frac{p}{d}aa$ (the area of a circle whose radius is a) gives $\frac{xx}{2a} - \frac{xxx}{2aa} + \frac{x^4}{8a^3}$, for the height of a cylinder on the said circular base, and equal to the solid made by the conversion of the area FabHF about the line Hb as an axis. When $x = a$, that is, when the whole figure is turned about its asymptote, the height $\frac{xx}{2a} - \frac{x^3}{2aa} + \frac{x^4}{8a^3}$ become $\frac{1}{8}a$.

VI. The curve surface of the solid generated by the conversion of the figure FabHF about HB, is equal to the curve surface of a cylinder whose radius is a, and height equal to $\frac{x}{2} - \frac{xx}{4a} + \frac{xxx}{12aa}$. And the whole curve surface of the solid infinitely continued, is equal to one-third part of the curve surface of a cylinder whose radius and height are equal to FH or a. Which may be demonstrated after the manner of the preceding proposition.

VII. The radius of the curvature of any particle of the quadratrix is $\frac{tt}{a - x}$, and thus found geometrically. In fig. 10, FAE is the quadratrix, HD the asymptote, AD the tangent, BD the subtangent to the given point A. Make BV = AD; on v rise the perpendicular VW; from A draw AW perpendicular to the tangent AD, till it meet VW in W. So is AW the radius of the curvature at A.

VIII. This curve may be continued on infinitely above the point F, but by a different and more operose way of construction; the properties of which will be these. 1. The difference of its tangent and subtangent (taking the subtangent in the line HS) will be always equal to the same given line FH or a. That is, as $t + s = a$, below F, so $t - s = a$ above F. 2. As below F the curve line is equal to the sum of its ordinate and subtangent, so above, it is equal to their difference, or $s - y$. 3. As below F, $2ay =$ ILM, so above, $2ay = \iota\lambda\mu$. All which, and its other properties, may be demonstrated as the preceding, mutatis mutandis.

IX. With a little variation in the foregoing construction, may the logarithmic curve be constructed, which is also a quadratrix to the hyperbola. In fig. 8, omitting the string MRP, let the distance MR be equal to the subtangent of the intended logarithmic curve, which is invariable; stick a pin at R in the ruler CD, to which apply the ruler EF, so that the edge of the little quadrant kl resting on the ruler AB, the distance RI may be equal to MR. Then keeping the ruler EF tight to the pin R and ruler AB, slide the ruler CD along in a straight line by the ruler or line SO; so will the wheel gh describe a part of the logarithmic curve TV, whose subtangent is every where MR.

X. In fig. 9, let FAE represent the logarithmic curve, whose subtangent is equal to FH. LIλ is an equilateral hyberbola, &c. as before in § III. Let FG $= x$, GA $= y$, FH $=$ BD $= a$, GH ($=$ LS) $= a - x$, AC $= \dot{x}$, C$a = \dot{y}$. Then AC : Ca :: AB : BD, that is, $\dot{x} : \dot{y} :: a - x : a :: a : \frac{aa}{a-x}$; therefore $a\dot{y} = \frac{aa}{a-x}\dot{x}$. The flowing quantity of $a\dot{y}$ is ay; and the flowing quantity of $\frac{aa}{a-x}\dot{x}$ is the hyperbolic area FILG, for by the nature of the hyperbola GL $= \frac{aa}{a-x}$; therefore is the hyperbolic area FILG, equal to ay, a rectangle whose sides are the subtangent BD $=$ FH, and ordinate GA (as here accounted) of the logarithmic curve.

An Account of a Book, viz.—Samuelis Dale, Pharmacologiæ seu Manuductionis ad Materiam Medicam Supplementum: Medicamenta Officinalia simplicia, priore Libro omissa, complectens: ut et Notas Generum Characteristicas, Specierum Synonima, Differentias, et Vires. Cum duplici Indice, generali altero Nominum et Synonymorum præcipuorum, altero Anglico-Latino, in gratiam Tyronum. N° 306, p. 2263.

In the year 1693 our author published his Pharmacologia seu Manuductio ad

Materiam Medicam, of which an account was given in N° 204.* After which he thought a supplement necessary, to render it more useful to other countries: and because it has extended its progress to the neighbourhood of Greece, he concluded, that it would not be improper to add, out of Dioscorides and the foreign dispensatories, all those things which he had omitted publishing.

Our author had made a considerable progress in this design when he received advice that M. Tournefort was, by the French King's order, gone into Greece and the adjacent islands, in search of plants, especially those of Dioscorides; this induced him to stop the prosecution of his work for some years, in hopes that botanist would, on his return, gratify the curious with his discoveries of the true and genuine plants of the ancient Grecians, which had perplexed the herbalists of these late ages.

In this supplement our author took care to set the Materia Medica of Dioscorides in a clear light; and for that end consulted all the authors that he could meet on that subject, keeping as close as he could to his text, in which he chiefly adhered to the translation and commentary of Matthiolus. And because, in this performance, he travelled in an untrodden path, knowing of no precedent in any language, he therefore consulted both the dead and living, i.e. not only books, but many persons of ingenuity and learning. The nature of the work requiring the virtues of each simple to be annexed, and the design of the book requiring brevity, he chose to transcribe them from authors, which had already contracted to his hand; but at the same time, to avoid the imputation of plagiarism, he at the end of every transcription inserted the name of the respective authors. Nor did he think it any discredit to him, that he had the assistance of others, but rather a glory, and therefore throughout the work he acknowledged the persons he had advice from.

De Monstris, quasi Monstris et Monstrosis; item de Serpentibus, &c. Philippensibus, ex M.S. R.P. Geo. Jos. Camelli. Communicavit D. Jac. Petiver, Pharmacop. Lond. et S.R.S. N° 307, p. 2266.

An Account of an Experiment made before the Royal Society at Gresham College, showing the Production of a considerable Light on a slight Attrition of the Hands on a Glass Globe exhausted of its Air: with other remarkable circumstances. By Mr. Fra. Hauksbee, F.R.S. N° 307, p. 2277.

The experiments already made on this head, as the attrition of amber on woollen, glass on glass, and with several other bodies in vacuo, which though

* Vol. iii. p. 588 of these Abridgments.

affording but a weak light, yet the manner of making them seemed to open a way to further improvements, which, during the late interval of meeting, I have pursued with my utmost diligence. The results of the many experiments made on this occasion are comprised in a very few, which shall be repeated before this honourable Society, as opportunity shall permit.

One is as follows: I took a glass globe, about 9 inches diameter, and having exhausted it of air, took it off from the pump, having first turned a cock, to prevent the air from re-entering it. Thus secured, and fixed, to give it motion by the great wheel, described in Phil. Trans. N° 304, which when turned gave a swift motion to the globe, on whose surface was applied my open and naked hands, which in a very little time produced a considerable light. And still as I moved my hands from one place to another, that the humid effluvia, which very readily condense on glass, might be discharged from every part of it; so the light improved, till words in capital letters were legible by it. At another time, when I made the experiment, the light produced was so great, that large print without much difficulty might be read by it: and at the same time, though in a pretty large room, the whole became sensibly illuminated; so that the wall at the farthest distance, which was at least 10 feet, was visible. The light appeared of a curious purple colour, and was produced by a very slight and tender touch of the hands, the globe at the same time hardly being sensibly warm. Nor do I find that a more violent attrition increases the light any thing. Nor is the highest degree of rarefaction of the air in the globe, absolutely necessary in the production of this light; for it seemed to continue very little lessened in its colour or vigour, till more than a fourth part of its air was let in. I have often observed the same, as to the light produced in the mercurial experiments, only that its colour in these was always pale: and there being such a seeming congruity of appearances in all the circumstances, with those made on the attrition of glass without it, that one might with some probability conclude, that the light produced proceeds from a quality in the glass, on such a friction or motion given it; and not from the mercury, any other ways than as a proper body, which falling or rubbing on the glass, produces the light. And what would seem further to corroborate such a conclusion is, that some time ago I took a mercurial barometer, and rubbed the upper or deserted part of the tube between my fingers, and a light ensued, without the motion of the quicksilver. Yet, for all this, the conclusion is doubtful, and there may be such a quality as light in mercury, as well as in glass, or any other body, as seems probable from the following experiment, made on purpose. I took a small quantity of quicksilver, and put it into a gallipot, in which varnish had often been used, by which means it had got a pretty thick lining of it; the weather at that time was moist (which I mention, because the humidity in the air would sometimes render the experiment unsuccessful even in glass, or at least greatly impair the appearance) which had an influence on the varnish, somewhat to soften it. However, the success of the experiment was, that when the gallipot, with its contained mercury, came to be in vacuo, on shaking the pump a light appeared, and that without the concurrence of glass, or the favour of a more proper season to assist it. I am also informed by several persons of credit, that the medicine called mercurius dulcis, when broken in the dark, gives remarkable flashes of light; which perhaps may be owing to the salts, with which each little globule of the medicine is enveloped; since I have often observed that loaf-sugar, when struck or broke in the dark, affords a light; and I cannot tell, but that salts as closely united in their parts as the sugar, may give a light on a violent separation of them, till I have made some trials; whether the medicine when broken in vacuo, will afford any light. 2. What the salts will do without quicksilver, both in the open air and in vacuo; for there are some bodies that appear light in the dark in the open air, which altogether lose that shining quality in vacuo.

After the attrition of the exhausted globe was continued for some time, the cock was turned, which permitted the air to insinuate into the globe, through the joints of the screws; the motion of the great wheel, and the application of the hands continuing all the while: and as the air filled the globe, so the appearance of the light continued to alter, till the like quantity of air had re-entered, as was taken from it; then there appeared as great a difference of light from what was produced when evacuated of air, as when the experiment was made with quicksilver in vacuo and in the open air. Certain specks of light were then seen on the fingers that touched the globe, but without any great lustre; and it was very remarkable, that while my hand continued on the glass, in motion, if any person approached his fingers towards any part of the glass in the same horizontal plane with my hand, within an inch or thereabouts, a light would appear to stick to the fingers, notwithstanding they did not touch the glass; and my neckcloth at the same time, at an inch or 2 distant from it, appeared of the colour of fire, without any communication of light from the globe. The former part of both trials was alike, save only, that upon application of white sheep's leather in the latter, a very good light was produced, while the wool side was held next to the globe; but when the other side of the same piece of leather was turned to the globe, no light ensued, though continued for some time; but as soon as it was changed again, the light would appear as at first, and so on several repetitions the same.

As to the latter part of this trial, the air was not let in all at once, as before, but at several times, by which the modes of light produced in the different mediums were the better observable, though no very great alteration happened, either to its colour or vigour, till more than a quarter part of the globe's natural content of air was let in; but sometimes, before half the air was suffered to re-enter, it was not without some pleasure to behold, how the light began to break in branches from that side the globe touched by the hands, filling its whole body with very odd figures; and these branches of light, at the entrance of more air, became more slender, striking then against the opposite side of the glass, and thence reverberating again in a very pleasing manner; but after more and more air was let in, so the light and figures diminished, till the appearance became the same as in the first trial.*

Account of what Manuscripts were left by Mr. John Ray; with some Anatomical Observations made at Padua by Mr. Ray. By Mr. Samuel Dale. N° 307, p. 2822.

Herewith you will receive some anatomical observations made at Padua, (and contained in the following paper) by our late friend Mr. John Ray, on the dissection of some human bodies, by Signor Antonio Marchetti; and containing besides those things which Mr. Ray himself remarks, divers observations of the operator's which did not occur in those bodies, to some of which Mr. Ray has added notes. To these are subjoined two dissections of Mr. Ray's, viz. of a hare, and the mountain hen.

Besides these, there are in his Adversaria many observations, inscriptions, epitaphs, antiquities, &c. which being collected together, would make a large supplement to his observations already published.

Nor must I forget his travels, in our own and the neighbouring kingdom, of which he has left divers itineraries: these may not be unuseful to our English travellers, he being as careful in making observations and collections at home, as he was in foreign countries.

Observations made in dissecting a Dead Body at Padua. By Marchetti, Dec. 10. 1683, *O. S.* N° 307, p. 2283. *An Abstract from the Latin.*†

After various unimportant observations on the teguments, muscles, &c.

* In the experiments mentioned in the above paper, we may recognize the beginnings of the phænomena and doctrine of electricity.

† Marchetti was professor of anatomy in the university of Padua, at the date abovementioned. This account of his observations on a dead body was collected, as above stated, from the posthumous papers of Mr. Ray, who had attended his anatomical demonstrations, and taken notes at the time. It appears that this professor taught many opinions, and advanced many assertions that are extremely erroneous.

M. Marchetti proceeds to notice that the os sacrum is composed sometimes of 6 bones, but commonly of 5. When it is composed of 6, the os coccygis has only 3 bones; but when the os sacrum consists of 5 bones, the os coccygis has 4. In cases of difficult parturition, by introducing the finger up the rectum and pressing back the os coccygis, he says the exclusion of the fœtus may be expedited.—The spleen in this subject was præternaturally enlarged, which he attributed to an intemperate mode of living.—The colon adhered to the peritonæum.—Respecting the liver he remarks, that in the living and healthy subject it does not extend over the stomach, and therefore that liniments, fomentations, &c. are (in certain cases) properly applied to the region of the stomach. He asserts from his own observation, that the iliac passion or volvulus is occasioned by an inflammation of the valve seated in the beginning of the colon, whereby the descent of the fæces is prevented. He had seen the passage so constricted, as not to be capable of admitting even the point of a pin (ne cuspidem aciculæ potuerit admittere)—In the fundus of the gall-bladder no vessels can be traced which convey bile into it; but only certain porosities which transmit that fluid; hence when the gall-bladder is separated from the liver, there is a manifest oozing out of the bile. From the parenchymatous part of the liver certain capillary veins are distributed over the membranes of the gall-bladder, so that this last cannnot be detached from the liver without some effusion of blood.—In the part where the gall-bladder is joined to the liver, it is composed of a single membrane only; but in the other part of two membranes.—The meatus cysticus, where it terminates in the ductus communis, is not furnished with a valve, but merely with an ostiolum, which prevents the regurgitation (refluxum) of the bile.—He asserts that when the meatus cysticus is obstructed, the yellow jaundice takes place; when the porus choledochus is obstructed, the black jaundice follows. That the branches of the vena portæ and vena cava are not united in the liver by inosculations, but per harmoniam aut incumbentiam mutuam vasorum. That the vena portæ does not acquire a new membrane or tunic within the parenchyma of the liver. That he had seen lacteal veins inserted into the trunk of the vena portæ. That he never could find, nor believes that there exists, a common receptacle of the chyle.* That he had seen a considerable branch of the ductus chyliferus terminating in the pancreas. That he supposes it to be the office of the spleen to separate black bile from the blood; and to transmit it by the ramus splenicus, together with the blood, to the liver, where it (the black bile) is discharged into the intestines by the meatus choledochus. That the lacteal veins absorb the chyle from the intestines, and

* Here Mr. Ray remarks, that his experience proves the contrary.

carry it to the pancreas; whose office is further to elaborate and exalt the chyle, and to evacuate the excrementitious part into the intestines by the new duct of Virsungus. That he had seen lacteal veins going from the mesocolon to the intestines.* That the external hæmorrhoidal veins are derived not from the vena cava, but from the vena portæ. That many branches of the splenic vein are distributed through the whole parenchyma of the spleen, contrary to the assertion of Sylvius. That when a person dies of a chronic disorder, the spleen becomes black; when suddenly, or of an acute disease, it is of a redder colour. That the new pancreatic duct, and porus choledochus, open into the duodenum at the same place; there are instances however, as in dogs, where they form distinct apertures.

In this, as indeed in all other subjects, the left kidney was larger than the right, and seated higher up. As this kidney is more remote from the trunk of the vena cava, its emulgent vein is longer. He assigns as a reason for the difference in these respects, between the right and left kidneys, that the right kidney is pushed down, and confined in its growth, by the weight of the liver lying above it. In this subject there were two emulgent arteries to the right kidney.

He asserts that he had seen lacteal veins [lymphatic veins] dispersed over the uterus in cases of pregnancy; from which vessels he supposes to be derived the watery fluid in which the fœtus swims in utero.†

The tubes of the uterus answer to the cornua uteri of brutes; they are hollow from one end to the other, and capable of admitting a probe. Their inner coat is white, and they frequently contain a whitish serous humour, which he supposes to be the semen muliebre. The testiculi muliebres (ovaria) have no epididymides, &c. &c.

Quòd testes tàm in maribus quam in fœminis ad generationem nihil conducant memorabili imprimis experimento probavit. Canis nimirum masculi testes execuit epididymidibus integris relictis, deindè canem fœminam in cubiculo conclusit per tres annos, nec ullum admisit ad eam canem cum salaxasset præter castratum hunc, qui canem iniit et cum eâ sæpiùs implicatus est; triennio hæc ter peperit, una vice 7 catulos, altera 9, tertia 5. Re satis exploratâ fœminam dimisit. Alias duas vel tres historias huic parallelas nobis narravit; unam de equo castrato relicta tamen epididymide una, qui equas sæpiùs imprægnavit, fuitque in venerem admodum proclivis: alteram de cane quem ipsius servus execuit: tertiam de homine quodam rustico, qui ob bubones venereos utrumque testem

* These, says Mr. Ray, I have also seen.

† These lacteal or lymphatic veins (adds Mr. Ray) are very clearly seen in sheep that are big with young.

amisisset epididymide unica duntaxat manente, qui tamen uxorem duxit, et tres masculos filios genuit. Credit ergo ille testes non alii usui inservire quàm quem Aristoteles adfert, nimirum ut sint pondera impedientia ne spermatica vasa implicentur; et revera vasa seminalia in eos non terminantur, nec transeunt, sed epididymides solum.

Uteri cavitas perangusta est et minima, verùm tunica uteri spissior densiorque quàm ego credidissem.

Ligamenta uteri rotunda non sunt in uterum perforata, verum vasis deferentibus in masculis quoquomodo respondent.

Orificium internum uteri in gravidis lentâ et viscosâ materiâ observatur, ut nos sæpiùs in bove observavimus, adeò ut nihil omninò in uterum penetrare possit: undè nihil seminis in uterum projici possit, adeoque nec superfœtatio fieri. Narravit tamen nobis se audivisse de muliere quadam rustica in montibus vicinis degente, qui tribus mensibus postquam unam peperisset prolem aliam denuò peperit.

Os ipsum uteri tincæ piscis ori persimile, corpus uteri cucurbitæ tonsoris.

Vagina uteri ampla est atque intus rugosa, in meretricibus verò longo veneris et assiduo usu rugæ istæ abolentur, et omnino lævis evadit.

N. B. Ad hanc uteri vaginam vasa quamplurima (venæ sc. et arteriæ) tendunt, à ramis iliacis interius sive hypogastricis orta, miris plexibus et anastomasibus junçta, quæ in superficiem vaginæ sparguntur, et probabile est oscillis suis sive capillaribus extremitatibus in cavitatem ipsius hiant, in eamque effundunt sanguinem menstruum, quanquam se nunquam horum vasorum orificia potuisse invenire asserit Marchettus; nec mirum. Nonnulli ex his ramis in cervicem etiam uteri sparguntur.

In pudendo demonstravit nobis labia, clitoridem in supremo rimæ angulo, alas seu nymphas in superiore etiam parte, urethram seu meatum urinarium, et circulum membranaceum qui pudendum à vagina uteri distinguit, quique in virginibus membrana hymene dicto occupatur totus, excepto foramine in medio per quod menstrua defluunt.

In defloratis etiam apparet hic circulus qui pudendum hoc loco coarctat, ponè quem vagina laxior et amplior est.

Vagina hæc ultra interiùs uteri orificium inferius percurrit, unde si membrum virile longius quàm par est fuerit, ultra orificium interius uteri in hunc sinum sperma projicit, unde uxorem imprægnare nequit.

In the angina spuria the tonsils are inflamed; in the angina legitima, the muscles of the larynx, particularly the arytænoides. In a case of angina legitima M. Marchetti's father made an incision between the two upper annuli, and introduced a silver tube, through which the patient inspired and expired, and thus a cure was accomplished. In making the incision, the author cautions the operator to divide and draw aside carefully the sterno-hyoides and sterno-thyreoides muscles. He says he had seen a small branch of the ductus thoracicus sive chyliferus going to the pericardium, and that by introducing a pipe and blowing through it, he was enabled to inflate the pericardium; he therefore conjectured that the lymph in the pericardium was derived from this source. He compares the shape of the human lungs to a cow's hoof, &c. The pericardium, in this subject, adhered preternaturally to the diaphragm.—There are two glands seated below the larynx under the sterno-thyreoidean muscles, one on each side of the trachea. In the bronchocele these glands become surprisingly enlarged. He observed that the thoracic duct sends off a small branch to the parotid gland.—In performing the operation of trepanning, he admonishes surgeons to avoid the sutures of the cranium, lest the dura mater should be wounded; the consequence of which (he says) might be fatal convulsions. The human brain is large, in proportion to the rest of the body. The brain does not pulsate per se, but by means of the arteries. For if the cranium of a live animal be opened, and the brain denuded, removing from one side or hemisphere the pia mater with its vessels; the side thus stripped of its membrane will not pulsate, but the other side will. He said that he had made this experiment himself, and that he had seen the brain pulsate for the space of a quarter of an hour and more, after the separation of the cranium.*—The glandula pituitaria is larger and firmer in men than in brutes—Under the membrane of the infundibulum he showed 2 corpuscula alba of the size of a vetch, and of the shape of a testicle, which he says were first discovered by his brother.†

We saw the operation of cutting a child out of the womb, performed on a dead body by Marchetti the younger: this is called partus Cæsareus, the Cæsarean operation.

He told us that himself had taken a child out of the mother's womb, after she was dead, which lived 2 or 3 days. Then follows an account of the manner in which this operation was performed, a detail of which it would be superfluous to insert here, seeing that this operation is fully and circumstantially described in numerous treatises on surgery.

In a hare dissected we observed the intestinum rectum of a very great length, having large pilulæ of dung secundum intervalla. I call here the gut (so far as it had no cellulæ) rectum, though indeed it had one or two convolutions.

* Concerning the pulsatory movement of the brain, see vol. v. p. 71, of these Abridgments.

† It is remarked, however, by Mr. Ray, that these corpuscula alba are figured in Vesling.

The intestinum cæcum in size far exceeded the colon, and was full of excrement. Just at the entrance into it out of the ileum was another appendix of a globular figure; the tunicæ of it more fleshy, and fuller of veins and arteries than the adjoining cæcum; there was also a little round hole in it. The cæcum towards the farther end of it was small, round, fleshy, full of vessels, red coloured like the jejunum in a man; the inner tunica granulated, and this for more than 4 inches in length.

The spleen was small and long, thicker at one end, it had no vesicula fellea that I could find; (in another we found the vesicula fellea manifestly:) the kidneys large, and the left situate higher than the right. The glandulæ renales received not their vessels from the emulgents, but from great veins on each side going to the loins. The stomach was full of grass (as I conjectured) which smelt like the wax of a honey-comb when the honey is newly drained from it. It was a female, and had long cornua uteri, but did not gestare when we cut it up. It seemed to have such a cavity under the tail, above the foramen ani, as I have observed in a badger. I believe now that the matter contained in the stomach was fir chewed small, which the smell argued. In the mountain hen (gallina montana) he found no vesicula fellea, but 2 pori biliarii opening into the duodenum by 2 distinct orifices.—The interior coat of the gizzard was almost as hard as horn.

Of Hydatides inclosed with a Stony Crust in the Kidney of a Sheep. By Mr. W. Cowper, F. R. S. N° 307, p. 2304.

In the sheep's kidney I found a large whitish body, inclining to yellow, and tinged with red, as it lay under the membrane of the kidney. This was very hard, as is usual in animal petrifactions, and 2 thirds of it lay hid within the substance of the kidney: it was inclosed with a thick hard membrane, that could not easily be separated from it, even with a needle fixed in the end of a stick. The branches of the emulgent veins and arteries lay between it and the pelvis of the kidney; all which vessels were somewhat pressed by this petrified body. As I was picking off its thick strong membranous inclosures, I found the needle slip into a cavity at an aperture. By this I perceived that this hard and heavy petrified body was hollow, and finding a membranous interstice in it pulled it asunder, and found its inside divided by many petrified cells, of irregular figures, and filled with hydatides, of various shapes and sizes.

Microsopical Observations on the Structure of the Spleen, and Proboscis of Fleas. By Mr. Anthony Van Leuwenhoeck, F. R. S. N° 307, p. 2305.

It has been commonly observed, that the spleen is composed of a spongy sort of flesh. And having examined the spleen in several sheep, I found that the many fibrous parts, of which it generally consists, and which many suppose to be arteries and veins, are in reality no veins, but are united to, and draw their nourishment from the membranes in which they are rooted, and spread themselves into many branches, and join with the fibrous parts, which likewise appear with roots and branches growing out of the opposite membrane, that I could not forbear viewing them with astonishment; imagining that all the innumerable fibrous parts were constituted to no other end, than to protrude the blood which is conveyed into them by the arteries; which blood in great quantities is contained in the veins, as may appear in great measure in those veins which resemble arteries; for the spleen can have no blood conveyed into it, but what is brought to it from the heart by the arteries.

They say that the spleen consists of a spongy flesh. I must own I could not discover that; for I allow of nothing to be flesh, but where the parts are extended in length, and lye in a regular order by one another, and so compose a muscle, and the ends of these flesh particles are joined in a membrane, or make a tendon of a muscle; whereas the parts of a spleen, setting aside the fibrous parts, the arteries and veins, are composed of very small particles, which are so exceedingly fine, that I can give no figure of them; and it seemed to me, that as the said fibrous parts spread themselves out into an unspeakable number of very small branches, the said very small particles are depending on the fibrous parts.

In dissecting a flea, in order to take the heart out of the body, its sting appeared much more plainly than I had ever seen it before; and the more as I had broken off the two fore-legs, which are as it were joined to the head, and then placed the fore part of the flea before the microscope just as if it lay upon its back; by which means the sting of the flea appeared so distinctly, that I could discover an orifice in the extremity of it, which appeared to have a cavity throughout; but what surprized me most was, that the sting had a scabbard or sheath, in which the flea shut up his sting when he was not using it, and to preserve it from harm; and I imagine that the flea could so order the sting and its case, as to place it between his legs, that it might not be entangled in the hair or wool when he runs along.

This scabbard of the flea is divided into two parts, each having a cavity like a canal, in order to contain the sting when those parts are close shut together; and it is very remarkable, that each of those hollow parts, that compose the sheath or scabbard, is composed of parts resembling the teeth of a saw. These teeth, I conclude, are so made as to indent within one another when the sting is in the sheath, in order to hinder the opening of it, except when the flea would make use of it: and we even discovered at the end of each of the scabbards three teeth projecting out, which I judge is for no other end than to shut within one another.

Fig. 11, pl. 9, exhibits the several parts of this machine: LM shows one half of the scabbard, with its cavity, the teeth like saws, and the 3 teeth at the extremity M: NO represents the other part of the sheath, likewise furnished with the same sort of teeth; and QP is the sting itself, placed between the two parts of the scabbard, where P represents the little orifice or hole in it. Now if we suppose that each of the parts of this sheath, as also the sting itself, are furnished with divers muscles and fibres, necessary to produce all the motions that belong to them, they may be deemed large instruments, in comparison of those muscles that produce their motion: but then if we extend our thoughts to those animalcula that are many million times smaller than a flea, and consider also their respective instruments for motion, &c. we must be lost in amazement at the thought.

Description of the Pediculus Ceti, &c. By Sir Robert Sibbald. N° 308, p. 2314.

The pediculus ceti* is the balanus balænæ cuidam oceani septentrionalis adherens, Lister Hist. Conchyl. Bocconi, who was the first that mentioned it, in his Recherches et Observations Naturelles; but his description of the shell is better than the figure he gives. The shell approaches to a sexangular figure, and consists of one valve, in which it differs from all the balani I have seen; it has no spiral circumvolutions, nor apex, but it opens at both ends; the orifice of the upper end is narrower, and through it the animal puts forth its cirrhi or brachia; the orifice of the lower end is much broader, and the animal is lodged in it. The lower is divided, as Boccone observes, into 18 lines, which are raised, 12 of them being simple and straight, and the other 6 branched: these last are so placed, that two straight lines are between each of them: and there is a cavity between all of them, in which the cirrhi or arms of the animal are probably placed, though in this subject they stood in the middle of the upper part of the shell, with their ends contracted; for the upper orifice is deeper than the lower. There is an opening from the under part to the upper, by which these cirrhi mount from the head of the animal. The orifice of the upper part is narrow below, but wide in the middle, and then again contracts somewhat. The body of the shell is convex, and has 6 divisions, each consisting of 4 protuberant tubes; which are narrower at the

* Lepas Diadema. Lin.

upper end, but grow sensibly wider towards the lower: the outermost of these tubes are narrow, the middle are broader, and all of them have striæ crossing them; the distances between their parts are smooth, and appear hollow; their superficies are wider at the top, and grow sensibly narrower towards the bottom. All the tubes are hollow on the inside, forming cavities between the lines, both simple and branched, which compose them. They arise from the orifice in the middle of the inner part of the shell, and proceed towards the sides; the branched part being nearest the side of the shell.

As to the animal; in the upper part there appeared something like a gaping mouth; the upper and lower parts were both semicircular, but narrower towards the point of the aperture; they were membranaceous, and took their rise from the inside of the shell. The upper lip, if I may so call it, was altogether membranous; the lower seemed of an osseous consistence towards the shell, and appeared like the dentes molares: a little below the mouth appeared the cirrhi, which were continued with the rest of the body of the animal. I doubt not but when the animal is alive, the under part below the cirrhi resembles the under part of the mollusci of the polypode kind: this resembles the parenchyma of a buccinum, but was much firmer, and when pressed it yielded a fat juice; it was white without, but blackish where it adhered to the shell; it was all contracted within the under part of the shell, which it filled: it was somewhat exsiccated, so that I could not perceive any distinction of parts in it, though some are of opinion there may be viscera and other vessels traced in it, when the animal is newly taken. There are two sinewy bodies, which arise from the sides of the upper part of the shell, exactly opposite to each other, and end as it were in two claws; by these it is probable the animal attaches itself to any thing; and by these it hung to the whale; it can dilate and contract them at pleasure. This animal is a new species of the polypus kind, which seems to be peculiar to some sort of whales in our northern seas.

An Account of a Hydrops Ovarii, with a new and exact Figure of the Glandulæ Renales, and of the Uterus in a Puerpera. Communicated by Dr. Jas. Douglas. F.R.S.* N° 308, p. 2317.

I lately opened the body of a woman, aged 27, who died the 3d day after delivery, on which I made the following remarks.

* Dr. James Douglas was a distinguished anatomist and practitioner in midwifery, of the last century. Besides various papers inserted in the Phil. Trans. he wrote the following works: 1. Bibliographiæ Anatomicæ Specimen; containing short notices of the lives, with complete catalogues of the writings, of anatomists of all nations, from the days of Hippocrates to the time of Harvey. 2. Myographiæ Comparatæ Specimen; a tract which shows the author to have been a most expert

1. She measured round the waste a yard and three quarters, and from the scrobiculus cordis to the os pubis a yard and a quarter. 2. All the cutaneous veins of the abdomen were of a very unusual and extraordinary size, and very much distended with blood; and from the largest of them, being opened, I extracted several polypous concretions. 3. The cuticula, from the umbilicus downwards, was rough and scaly to the naked eye: in several parts it appeared gangrened, occasioned probably by the sharpness of the serum that always oozed out of it, when she scratched the little pimples or wheals that arose on its surface; these for some time used to go off without any scar, but as her strength decayed they became mortified.

4. Upon all the regio epigastrica the outward integuments were very thin, little or no fat being visible: but from the upper part of the regio umbilicalis, down to the os pubis, the skin was almost half an inch thick, of a whitish colour, and hard, some of it appearing as if it were granulated, caused by some obstructions in the miliary cutaneous glands. 5. The fat under this part of the skin was upwards of an inch thick, being distributed into several lobules of an irregular figure, and lodged in so many cells adhering to the membrana adiposa, which in this place was also much thicker than it usually is in a natural state. 6. Her thighs, legs, and feet were all anasarcous, being extremely thick and swelled, and easily retaining any impression made by the fingers; and her nurse told me, that she used to wet a great deal of linen in drying up the water, that would always issue out from these parts on the least rubbing; yet all her superior parts were extremely lean and emaciated.

7. The fleshy part of the abdominal muscles was much extenuated by the great distension, yet their tendons were as thick as usual; and being very easily separable from each other, I could plainly observe that the tendon of the obliquus internus adhered firmly to that of the transversalis, along the edge of the musculus rectus, and was not double, as Realdus Columbus, and all anatomists after him, down to Diemerbroeck, who was first aware of this mistake, have maintained: however, this straight muscle derives the same benefit from this situation, being as it where hemmed in on one side by his firm adhesion, and on the other side by the linea alba, as if it had indeed been inclosed between the two supposed tendons of the obliquus ascendens; that is, it is much strengthened thereby in time of acting. I observed also that the tendons

dissecter and accurate observer. In particular it contains an excellent comparison of the muscles of the dog with those of the human subject. 3 A Description of the Peritonæum, containing several new and valuable observations relative to the structure of that membrane. By his lectures as well as his writings, Dr. James Douglas contributed largely to the advancement and diffusion of anatomical knowledge in this country.

of the two oblique muscles, and the fleshy part of the transversalis, between the anterior spine of the os ilium and the pubis, near its commissure, did inseparably join and unite with each other, forming as it were a thick and hard border, from the outer of which, there was continued over the blood vessels, nerves, and muscles, on the fore-part of the thigh, a large aponeurosis, which braced them down; the two laminæ of the membrane of the abdomen being expanded on its inside. Now this border is what authors call the ligamentum pubis, and what I have in my Myographia Comparativa, p. 5, supposed to be the firm union of the tendons of these three abdominal muscles with the peritonæum.

8. Having perforated the abdomen in the most convenient depending part, there issued out in a rising stream with great impetuosity a vast quantity of slimy viscid water, in colour and consistence very much resembling a brown, thick, and ropy syrup. This water measured between 16 and 17 gallons, besides what was lost on the floor, and imbibed in sponges and linen made use of in drying it up.

9. When the water was quite emptied, I fancied it had been all contained in a duplicate of the peritonæum, and had caused a dropsy in that membrane, because none of the viscera appeared; for in such a case, I have sometimes observed, that the inner lamella of that membrane of the abdomen, being separated from the outer, is forced inwards by the weight of the water upon the bowels, to which it closely adheres, contracting the guts and mesentery into a very small volume. But on a narrower view, I perceived that the thick membrane, including the water, could be easily separated from the viscera, having freed it from its adhesions by membranous filaments to the peritonæum, and by blood vessels to the omentum. Now this bag reached from the pubis to the midriff, and from the left region of the loins to the right; in short, it filled up the whole cavity of the abdomen, distending her belly so far, that a plate could easily lie upon it, when alive. Having gradually freed it from all the neighbouring parts, and rolled it up, I found that it adhered inseparably to the left tuba Fallopiana, the spermatic vessels being ramified upon it; and observing no ovarium, which in the other side was naturally disposed, I concluded that the bag was nothing but the membrane of the ovarium covering the ova, preternaturally thickened and distended by the collection of the abovementioned humour, and that the distemper was a true hydrops ovarii; as all this vast quantity of water was included in one bag, being all of the same colour and consistence.

10. All the other viscera in the abdomen were sound, and in their natural state. 11. In both cavities of the thorax there was contained a great quantity

of reddish water. 12. The liquor in the pericardium was very abundant, and of a greenish hue. 13. The right lobe of the lungs was tied to the membrane of the thorax, covering the upper part of that cavity; but the left lobe was free from any adhesion. 14. In the left ventricle I found a large polypus, or serous concretion, of a round figure, a white colour, and of a pretty hard consistence, with several long roots of a red colour, which extended through the auricle and bulb of the pulmonary vein, into its nearest divarications in the lungs.

Having carried home this large bag, with the uterus appendant, cut off below the orifice of the meatus urinarius, and viewed it at leisure, I observed, 1. That the right spermatic vein, which opens into the cava a little below the emulgent, was three times larger than the left; and from a little above the ovarium it was continued, without any division to its termination. 2. The right ovarium was in a very natural state. The cicatrix or caruncula, whence the fecundated ovulum had dropped, was yet remaining, and the blood vessels were ramified upon this testis, in a very beautiful manner. 3. The tuba Fallopiana, and its fimbriæ, were all in a natural state. 4. The diameter of the left spermatic vein, which opens into the emulgent of that side, was much less than ordinary. And from the extraordinary narrowness of the bore of this vessel, we may assign a probable cause of this watery swelling; for the blood being hereby hindered in its reflux to the heart, a great deal of serum or lympha, by reason of its slow return, must needs be thrown off upon the ovarium, already indisposed, whence the gradual increase of the tumor proceeded. 5. The two spermatic arteries were contorted, and full of turnings and windings, from their meeting with the veins to the ovaria and tubæ. 6. A little below the kidneys each artery sent out a branch, which was lost in the peritonæum and the fatty membrane of the kidney: and from the same places, the veins received two considerable branches. 7. One of the arteries went off by a narrow orifice from the side of the aorta; and the other rose up from its middle, a little below the first. 8. Between the bag and the uterus all these were much dilated, making several turnings and circumvolutions on the peritonæum, called in this place the ligamentum latum uteri. 9. The left tuba Fallopiana was only remarkable in its being much longer and larger than usual.

10. In the bag, which was nothing but the membrane called dartos, which covers all the vesicular glands of which the ovarium is composed, I observed several little bladders of different sizes, distinct from one another, which contained a limpid or clear slimy serum, in colour and consistence like a mucilage of the semen cydoniorum; and these were either hydatidal tumors only, or the ova themselves distended. This liquor hardened by a slow heat into the con-

sistence and colour of the white of an egg. 11. All the fundus uteri was about an inch and a half thick; but near the collum minus it grew somewhat thinner; which proceeded from the distention of its spongy and vesicular substance, by the blood in the vessels running through it in various meanders; so that when it was cut, it very much resembled the substance of the lungs. 12. Upon the inner membrane of the uterus, on wiping it with a sponge, I observed several small eminences, which I took to be the glands mentioned by Malpighi, which separate a humour, to lubricate and moisten its cavity.

13. On the upper part of the fundus uteri I remarked a great number of small vessels, like slender filaments or threads, running off from its membrane, and terminating into a reddish and soft spongy sort of substance, not unlike the uvula, except its colour, which hung down from that side of the uterus in form of a nipple. These perhaps are the vessels, which, according to some, do separate and secern the matter of the lochia and menses; for they are visible only at those times. 14. Near the beginning of the tubes, I perceived two tubercles, or little bunchings, about the size of a nut, to which perhaps the placenta was fastened; and to these adhered several glandules of a blackish colour, of different sizes. 15. The collum minus was composed, as it were, of two labia, the uppermost being the most protuberant; and on it I observed several small glands, out of which, on compression, issued a viscid clear liquor, which is said to seal and close up this part in time of pregnancy. The lower labium was longer and thinner, its edges being cut or indented in several places. 16. The rugæ, in the lower part of the vagina, ran as they are usually represented; but those in the upper part had quite a different course, as they are exactly delineated in fig. 13, pl. 9. 17. Near the orifice of the meatus urinarius there were observable two very large caruncles, shaped like a mulberry.

This is what I observed in opening this woman. I come in the next place to relate, as far as I was informed, the symptoms that accompanied her big belly, and the method used for her recovery. About three years ago, not long after she had laid in of her first child, she had a violent blow on the left side of her belly, very painful at first, but in two or three days, on keeping herself quiet in bed, the pain and anguish went off. About two months after this, she began to feel some small pains in the left hypogastric region, where she had received the blow; and she observed that side of her belly was become much larger than the other: these pains increased more and more, till they became very violent; but upon conception, which was three months after she was first afflicted with them, they went off, and her belly swelled gradually, as is usual in pregnancy, having no other symptoms but what are incident to that state; only she was much larger than ordinary; and on that account she for-bore the use of medicines, which possibly might have been of service in the beginning of her distemper, had she been well aware of her danger.

After her delivery, the swelling and bulk of her belly continued much the same as before the birth; only, on a plentiful evacuation of the lochia, it decreased a little. When her month was up, she advised with several physicians, apothecaries, &c. who used emetics, strong cathartics, diuretics, diet-drinks, and all the train of medicines commonly used in a dropsy, her supposed case. All the effect they had, was to prevent the farther increase of the swelling while she used them; but, being weary of the trouble and charges to no purpose, she left them wholly off, and then the tumor increased very remarkably.

Thus she continued about a year, and then she conceived again, which she suspected by the stoppage of her catamenia, having always been very regular but at such a time. Her appetite was always good; she never was very thirsty, so drank but little; made water freely, and in great quantity; and was attended with none of the symptoms of an ascites, excepting the swelling of her belly: only when she was half gone with child, her legs began to swell and pit, growing very thick all of a sudden; from these, and likewise from her belly, there would often issue out a great deal of a watery humour upon rubbing, as I have mentioned already; especially if she scratched the little pimples, that would often arise in these parts. About this time she began to be afflicted with a difficulty in breathing, with a violent trembling and palpitation of her heart, and to be often subject to great and involuntary sighings. She was not able to lye down, but was obliged to sleep in a sitting posture, for fear of being choked. Now I think it probable, that all those symptoms proceeded from the large quantity of water contained in the cavities of the thorax and pericardium; which doubtless did more effectually hasten her end, than the size of her belly, with which she might have lived several years.

After she was brought to bed of a live child she became exceedingly weak, being unable to fetch her breath, and complained much of a heavy load and oppression on her breast; and the third day she expired.

The Explanation of the Figures—Pl. 9, fig. 12, represents the glandulæ renales, the uterus, with the parts belonging to it, and the large bag or membrane of the ovarium præternaturally distended. a the glandula renalis on the right side; b an eminence, or rising in its middle; c a vein that runs from it to the cava; d the glandula renalis on the left side; e a sulcus or furrow in its middle; f a vein running from it to the emulgent; g a small vein that comes from the diaphragm, and opens into this vein before it leaves the gland; hh two small arteries from the aorta; ii two nervous twigs from one of the intercostal plexuses; AA the kidneys; BB the uterus cut off; C the cava cut off; D its division into the rami iliaci; EE the internal branches into which the hypogastrics open; FF the emulgent veins; G the aorta cut off; H its division into the iliacs; II its internal branches, which are spread upon the uterus; KK the external iliacs of both vessels; LL the emulgent arteries; MM the spermatic veins; NNNN the spermatic arteries, very much contorted in their progress, that on the right side being cut off; O the union of the branches of the spermatic vein on the right side; P the right ovarium, with blood vessels ramified on its outer membrane; Q the right tube; q its fimbriæ; R the tube on the left side, its fimbriæ adhering to the large bag; SSS the membrane of the left ovarium, distended to a vast bigness, with the blood vessels ramified upon it; T some of the ovula grown large; W some hydatidal tumors on the inside of the great bag; VVV the ligamenta lata; U the fundus uteri; XX the ligamenta rotunda; the membrane that covers them, being laid open, that the vessels of which they are composed may be viewed; Y the vagina cut off; Z the vesica urinaria; ** a small artery and vein on each side, the first going off from the spermatic, is spread upon the membrana adiposa and peritonæum, under the kidney; the latter bringing back the blood from these parts, opens into the spermatic vein.

Fig. 13, shows the vagina and uterus cut open. AAA the fundus uteri laid open, and its sides folded back; 2 2 the glands appearing on the inner membrane of the uterus; 3 3 the small vessels, by which the lochia, &c. are separated; 4 a soft substance, depending from the upper part of the uterus, into which the aforesaid vessels terminate; 5 two tubercles, seated near the beginnings of the tubæ, to which the placenta adhered; BB the vagina laid open; 6 6 the two labia of the collum minus; 7 7 several small glands placed on the upper labium; 8 the course of the rugæ on the upper side of the vagina; 9 9 their direction on the under side of that part; 10 10 two orbicular substances, near the orifice of the meatus urinarius.

An Experiment made before the Royal Society at Gresham-College, showing the extraordinary Electricity of Glass, producible by a smart Attrition; with a Continuation of Experiments on the same Subject, and other Phænomena. By Mr. Fra. Hauksbee, F. R. S. N° 308, p. 2327.

I took a hollow tube of fine flint glass, about an inch in diameter, and 30 inches long, and after rubbing it pretty smartly with paper, till it had acquired some degree of heat; it was then held towards some pieces of leaf-brass, and as soon as its effluvium had reached them, they were suddenly put in motion, flying towards the tube, even at 9 or 10 inches distance; and it seemed that the hotter the tube was made by rubbing, the farther it would attract,* but that it would do so to any degree of heat, I dare not determine. And it was further observable, that sometimes the bodies attracted would adhere to the tube, and there remain quiet: sometimes would be thrown violently from it to a considerable distance: sometimes in their motions towards the glass, and sometimes even touching it, they would suddenly be repelled back to the distance of 4 or 5 inches, repeating the same several times with great velocity in a very surprising manner. Sometimes the bodies would move but slowly towards the tube, sometimes remain a small time suspended between the glass and the table on which the brass-leaf was laid; and sometimes seem to slide along the sides of the glass without touching it. All which phænomena, I have observed several times; yet the force and vigour of the effluvium is sometimes less than it is at other times. The reason of which seems to proceed from the different temperatures of the air at the time the experiments are made; for when it happens that abundance of humid particles are swimming in the air, it is easy to believe, that the resistance of such particles may greatly impede the force and extent of the effluvia: or they may suddenly condense on the warm tube, and thereby hinder or choke the passage of the effluvia; for I find moistness at all times an utter enemy to attempts of this nature: besides, the quality of the effluvia seems to be such, that I could not, in an experiment lately made, with all my endeavours, cause them to affect one of the forementioned bodies through a piece of fine muslin, though it was held very near the tube, and yet at the same time they would attract or give motion to the same body at 3 or 4 times that distance, if the muslin was not interposed. Moreover, I know not also, but the coldness of the air at the same time may concur; for when this experiment was first made, it was summer time, and dry weather; and then it seemed to succeed rather better than it has done of late. But to proceed: when the glass became hottest by the greatest attrition, it then emitted such a quantity of effluvia, as not only to produce the effects beforementioned with seemingly greater vigour, but being nearly applied to the face, or any tender part, it might be sensibly felt, as if the part was pushed with the points of a considerable number of weak hairs. Considering the vigorous action of the effluvium, I attempted to discover the figure of its motion, by approaching

* Mr. Hauksbee here erroneously thinks that the attraction was owing to the tube's heat, instead of to the friction only: for if the tube be only heated, as at the fire for instance, though ever so much, without rubbing it, there will be no attraction. But this, and several other phænomena in this paper, which surprized Mr. Hauksbee here in the infant state of electricity, are circumstances that are now easy and familiar to every person.

the fricated tube to the flame of a candle, to smoke, steam, dust, and to the surfaces of the liquids; but always without any manner of success: which I attribute to the reason before given, of the humid effluvia suddenly condensing on the warm glass; so that the oleaginous quality of the flame and smoke, the moistness of the steam, the smallness of the dust, or the effluvia, of the liquids, would immediately adhere to all parts of the fricated tube, as it was brought within their spheres of activity, preventing the operation of its effluvia, which then seemed to be stopped, or retired within itself; and required a new attrition to give it vent.

What next occurred in this experiment was, that on exhausting the air from the tube by the pump, although the like attrition or greater was given it than before, yet very little of the effluvium could be discovered, by any motion or disturbance given the leaf-brass, even when held within a quarter of the distance, at which it had been attracted before. After this had been continued for some time, with little success; I say with little success, because, sometimes small parts of the leaf-brass, when the tube was held near, and at the same time very warm, would have a motion given them; but not to be compared with what it had, when the experiment ws made without exhausting the air. Besides, I doubt not but some small quantity of air might be left in the tube, and so the attraction might continue in proportion to the quantity of the remaining air. Or the heat produced on the smart attrition of the tube, may, as well in this, as in other experiments, supply the effect and space of such a quantity of air: on re-admitting the air, it was observable, that before any new attrition was given the tube, or was removed from the position and distance it was held at when in vacuo, that several of the forementioned bodies, at rest as to appearance, began suddenly to move, and were some of them attracted towards the tube, which, on a fresh attrition, recovered its electrical quality as vigorous as at first.

When the attrition of the tube was made in the dark, it was very observable, that as the glass became warm, a light would continually follow the motion of the hand, backward and forward; and at the same time, if another hand was held near the tube, a light would be seen to dart from it, with a noise, much like that of a green leaf in the fire, but nothing so loud: though when the experiment has been very silently made, I have heard several cracks, at 7 or 8 feet distance, or more; when any thing else, besides the hand, was brought near it, a light would fix upon it, though it touched it not, as I have tried with gold, silver, brass, ivory, wood, &c. giving much the same appearance as the hand. But after the glass came to be exhausted of its air, then on the first attrition, a much larger light ensued, but the quality of giving a light to a body brought near it seemed to be quite lost; also the light produced on the attrition of the exhausted tube, appeared wholly within it, whereas in the former case it seemed to be altogether on its outside.

I have procured also a solid tube, or rather rod, of glass, about the size of the other; and upon trial find no great difference from the other, only its effluvia seemed to continue a little longer, though it attracts not at a greater distance than the former. With this new tube I made the following experiment: I took a little lamp-black, and having dried it on a paper before the fire, and the tube being rubbed till it was warm, then being held near the lamp-black, it was pleasing to behold the brisk agitation of a number of the little bodies, seeming promiscuously ascending and descending with great velocity; and it was admirable to see, that bodies so light in specie, and which by their own gravity falling on paper, would make no sensible noise, yet the same returned with such force from the tube that their striking the paper was very audible.

I further procured a glass nearly cylindrical, about 7 inches both in length and diameter; which was put into motion by a machine of a new contrivance: its axis lying parallel to the horizon, which in the like experiments before made was perpendicular to it. After this cylinder was exhausted of its contained air, and the motion made by the wheel, it succeeded in respect to the light produced on its attrition, as in the experiments before. But when all its air had been readmitted, and the attrition and motion continued as at first, it was very surprising to behold from the point of one's finger to the glass a strong light, which began first at the finger; and seemed to gravitate on it, being sensibly felt there, though the moving body was about half an inch from it. This light seemed to issue from the glass with a considerable hissing noise. To try whether it would exhibit any phenomenon by day light, one day in the afternoon, between 2 and 3 o'clock in a very light room, I found that immediately after the attrition was made on the moving glass, and the finger approached as before, a pure purple light became very visible, extending from the finger to the cylinder, and was accompained with the like noise as before. This experiment I have repeated several times since, at different hours, and with like success, being always made with a glass unexhausted of its air. To proceed.

As to the electricity of this body on such a motion and attrition being given as usual, I do not find that it exceeds in that quality what already has been related in former experiments. I then took a piece of fine muslin, which was sewed to two wires bent archwise, that it might surround the upper surface of the glass, almost at 4 inches distance from it. I made the muslin as ragged as I could, by breaking the threads of it every where; then the motion and attrition being given, it was pleasing to see a multitude of small sparks of light every where on the ends of the torn threads, which resembled so many little stars observable in a good telescope in the via lactea; and the whole was attended with such a whiteness, by the small light proceeding from them, as is seen in that part of the hemisphere with the naked eye.

After that I tried whether the addition of heat, by placing a red-hot iron just under the moving glass, would advance any thing the appearance of light, which I found without the attrition of my hand would do nothing, and with it no more, that I could discover, than if it had been absent; both, with the glass exhausted of its air and without.

Having always observed that light bodies approached near any part of the fricated cylinder, would seemingly be equally attracted or gravitate; so that I contrived a semicircle of wire, which I could fasten at a constant distance, environing the upper surface of the glass at 4 or 5 inches from it. This wire had twisted round it some pack-thread, by which I could with ease hang the threads at pretty nearly equal distances; the lower ends of which reaching within less than an inch of the glass, when directed towards the centre, but when at liberty they appeared as in fig. 14, pl. 9. And when the cylinder was turned round pretty swiftly, those threads would appear by the agitated air as in fig. 15. But when on the lower part of the glass a hand was applied, the threads would then represent a form like fig. 16; and from all parts seem to gravitate, or were attracted in a direct line to the centre of the moving body, suffering no inconvenience or disorder of posture by the wind occasioned by the rapidity of the motion; and by shifting the attrition, I could draw them in a line towards either end of the cylinder; yet still pointing to its axis. And when the wire with the threads is reversed, that is, encompassing the under part of the cylinder, as before the upper, it answered exactly the same, all the threads pointing towards the axis, as in fig. 17. I have likewise given a motion to the same glass in a perpendicular position, by which means I had the opportunity of placing a hoop-wire horizontally, with threads as before, and left only one small part exposed for the touch of my fingers between them, yet the threads, upon the motion and attrition given the cylinder, elevated themselves from their hanging position, making an horizontal plane all round, and directing their loose ends to the axis as before. But how far this experiment may serve to explain the nature of electricity, magnetism, or gravitation of bodies, I leave to others to determine.

A Vindication of Mr. James Gregory against the Misrepresentations of the Abbot Galloise, in the History of the Academy of Sciences for the Year 1703. *By Dr. David Gregory.* N° 308, p. 2336. *Translated from the Latin.*

Twelve years ago* I undertook the defence of my learned uncle, Mr. James

* Philos. Trans. N° 214. Abridgment, Vol. III. p. 673.

Gregory, against the calumnies of the Abbot Galloys, who also impeached before the learned world the great Dr. Barrow,* as if he had stolen from Roberval his propositions concerning the transformation of curves. And now, since M. Galloys has thought fit to revive the same controversy again,† I may be again allowed to vindicate my uncle's reputation.

M. Roberval lived seven years after Mr. J. Gregory's book was published. Now is it to be supposed that he, who was catching at every little advantage, who was arrogating every thing to himself, and who would leave no one in quiet possession of his own, can it be supposed that he would allow himself to be rifled of his propositions while he was living, and having the use of his eyes? but M. Galloys says, he did not see it, he read no new books at that time, he patiently suffered himself to be robbed of his discoveries, he gave up his fame together with his mathematics. It is surprising with what face he can utter such fictions as these, which can so easily be refuted. So little truth is there in hi assertion, that from the year 1668 Roberval lived in retirement, remote from the conversation of learned men, and had renounced his mathematical studies; that, on the contrary, from the year 1670 he was a professor of mathematics in the Academy of Paris, and communicated to the Royal Academy of Sciences his invention of a new balance, as testified by their acts, published for that year.‡ Roberval was therefore present at the meetings of the academicians, and if he then read nothing himself, can it be supposed that he heard nothing in conversation about Gregory's inventions, which were then so celebrated in France? Did he hear nothing about them from Huygens, who at that time spoke very eagerly against Gregory among the academicians?§ But if there was no familiarity between him and Huygens, as Galloys affirms, perhaps because he was displeased that Huygens had discovered the most useful property of the cycloid, could he yet hear nothing for the whole space of seven years from all the rest of the academicians? or if he did hear, did he make no complaint to his brethren and friends? who can believe he had such a contempt for fame, that has but once heard of his squabbles with the Italians, with his own people, and with every body? If of a sudden he was become so indolent, and so indifferent to reputation, that he could quietly bear to see all his discoveries ascribed to others, and to suffer what he had happily invented rather to lie dormant in his study, than to bring them to light; how then could Gregory steal these things from him?

But let us see by what force of argument it is that Galloys proceeds to fix

* Hist. of the Royal Academy, 1693. † Ibid. 1703. ‡ Journal des Sçavans, 1670.
§ Ibid. 1668.

this accusation upon him. "First, says he, it appears,* that this method for the transformation of curves, which was invented by Roberval, was known in Italy before the year 1668; for Torricelli, who died in 1647, testifies in his letters, that it was communicated to him by Roberval. Secondly, the adversary, however unwilling, is obliged to confess that this method is the same as Gregory's. Thirdly, it must therefore appear very probable that Gregory, when on his travels in Italy, might learn this method from the Italians, which had been so long known in Italy."

Now, that this method, which appeared in the year 1692, in the name of Roberval, is the same with that which Gregory had published 24 years before, prop. 11, Math. Univers. as it is plain to any one that views them both, so I had granted it without any hesitation. Indeed I said, that in the writings of the French, where it is ascribed to Roberval, it was accompanied with a miserable and shameful demonstration. But that it was the same as Gregory's method I never once questioned, nor made any dispute of; though Galloys made this the chief point of the controversy, and triumphs as if I had yielded him the victory. But I by no means grant him, either that it was known before to the Italians, or that it was communicated by them to Gregory. For how does it appear that it was known to them? because Roberval had communicated it to Torricelli. How does this appear? from the letter of Torricelli himself. But where is this letter? Galloys has it. When was it written? about 60 years since. Where has it been hid so long? where all wonderful things are hid, in Roberval's own depository. Whether this epistle be genuine or not, or whether there be any such thing or not, we must not presume to doubt, since there are so many credible witnesses. But by what literary monuments does it appear that Torricelli communicated these inventions to the Italians? About this, there is still a profound silence. Or, if he had imparted these to any, they might by this time have been quite extinct and unknown, since Torricelli himself had been dead 20 years before Gregory went into Italy. Or if they had not been yet out of memory, Galloys should tell us, who among the Italian mathematicians imparted these secrets to Gregory, which had been entrusted to him by Torricelli. Perhaps he will say, for he can take the liberty of saying any thing, that they were known to many in Italy. But would the Italians trust these geometrical secrets to Gregory, a mere foreigner, which they had concealed from every body for 20 years? Would he dare, in the midst of Italy, for his book was printed at Padua, publish things as his own, which he had but just learned of the Italians? or if he had been so devoid of shame, could he have

* Oeuvres des Mathem. par Mess. de l'Acad. Royal.

done it without being censured by the Italians, by whom he was rather applauded? this I must own is beyond my faith to believe.

An Account of a Storm of Rain that fell at Denbigh in Wales. Communicated to Dr. Hans Sloane, R. S. S. N° 308, p. 2342.

Tuesday the 16th of July 1706, about 8 o'clock in the morning, it began to rain in and about Denbigh, which continued incessantly for 30 hours, but not very violently till about 3 or 4 o'clock on Wednesday morning, when it rained somewhat faster, attended with a terrible noise like thunder, with some flashes of lightning, and a boisterous wind. About break of day the rain and wind began to abate of their violence, lessening gradually till about 1 or 2 o'clock in the afternoon, when it quite ceased, and the air became clear and somewhat calm. On the Tuesday the wind blew south west, but on the Wednesday it was come to the north west.

The effects of this great storm were dismal, for it caused the overflowing of all the rivers in Denbighshire, Flintshire, and Merionethshire, &c. which spoiled a great deal of corn, and took off all the hay that was mowed, near the banks of the rivers, which was carried by the stream in such vast quantities down to the bridges that it choked the arches and inlets, so as to break down above a dozen large bridges. Great oaks and other large trees were rooted up and swept away, with several quickset hedges; and some quillets, by the side of the river Elwy, were so covered with stones and gravel, that the owners cannot well tell whereabouts their hedges and landmarks stood; and the same river has altered its course in some places, so as to rob the landlords on one side of some acres, and bestowed as much on the opposite side. Two or three rivulets that conveyed water to some mills have been so choked up with stones and gravel, as to make it hardly worth the expence of clearing.

It is affirmed by many people that the great floods were not so much the effects of the rain, as the breaking out of a vast number of springs, in such places as they were never known to flow from before. In the town of Denbigh a great many broke out in the houses and stables, especially in that part which lies next the castle on the north side; some of them with a great deal of violence, and in such a quantity, that it is said that three of these new springs, which flowed out of the stables of the three noted inns, viz. the Bull, Cross Keys, and Boar's Head, were sufficient to turn any corn mill.

At a small distance, northward of Denbigh, lies Park-Snodiog, a rocky hill, out of which issued a great many springs, which flowed so plentifully for 9 or 10 days, that the cattle watered at them all that time; whereas, before and after, the people were forced to water them all summer at a well in the highway, at some distance from Park-Snodiog. There are several deep holes and trenches cut in the highways adjoining to the river Elwy, &c. and some of them very large, which is attributed not so much to the overflowing of the river as to the breaking out of springs in those very places.

In Comb mountain there is a pit of a circular form, which in the summer time used to have little or no water in it, and in winter as much water as would swell the surface to about 14 or 16 yards over: but now in the midst of summer it rose up at least a yard and a half higher, than it was ever known to do in the wettest winters; and overflowing its banks, it fell down the hill with such violence, as to penetrate into the very body of a rocky road, and dug pits in it, so that the road, which was a common highway, is now become irreparable.

Of a Tumour on the Neck, full of Hydatides, cured by Mr. Anthony Hewnden, Surgeon: Communicated by Dr. Edw. Tyson, F. R. S. N° 308, p. 2344.

A gentlewoman in London, 25 years of age, had a large wenny tumour; the basis, taking its origin from all the lower hinder part of the skull, stretches down the neck near each jugular, extending itself almost as low as both the scapulæ; on the upper part was a phlegmon. The radix being so large, I applied a transverse caustic the whole length and breadth of the tumour, intending to separate the cutis from the membrane of the cystis; but it being so thin where the phlegmon was, it obliged me to divide the cystis; out of which I extracted above 60 hydatides, of the size of a small wallnut; and several more were broken. These hydatides swam in a liquor of the consistence of the whites of eggs. In this cystis I found a large quantity of atheromatous and steatomatous matter, and at the basis a large sarcoma; the greatest part I cut off, but fearing to hurt the muscles of the neck, I deferred it to the next dressing, intending to take away the rest of the sarcoma and the radix of the cystis by caustics, which I applied without success; for they came off without making an eschar, the radix being of a cartilaginous substance; searching with my probe to find some interstice, it dropped into one; and touching some membranous or nervous body, caused the patient to cry out furiously; into this interstice I put a piece of Roman vitriol fitted to the place, which came out the next day all dissolved, with some of the radix: by constantly applying the vitriol, I extirpated the whole radix, and healed the part S. A.

It is observable, 1. That 7 years before this operation, this tumour was very nearly as large as at this time, and subsided of itself. 2. That when I began with caustics, the first I used was præcip. rub. with which I covered the whole radix, and it came off without any eschar, but it salivated the patient for 5 weeks.

Concerning a Monstrous Birth. By Mr. Robert Taylor. N° 308, p. 2345.

A poor woman near Hitchin being in hard labour, was delivered of twins conjoined together; there being but one trunk of a body, with two necks, on each a head, 4 arms, 2 forwards and two backwards, those backwards crossing each others shoulders, like two persons side to side. There is but one navel, two matrixes, two fundaments, two pair of hips, and four legs. They had gone the full time, having hair on their heads, and nails on their fingers and toes. The midwife tells me they were alive within less than half an hour before delivered: they look very clear and well; and by reason of their being conjoined, are about 7 inches over.

An Account of Dr. Ehm's Treatise of St. George's Bath near Landeck, in the Lordship of Glats, near Silesia. N° 308, p. 2346.

The doctor states, in his account of this water, that it fills a basin of 21 feet long, 10 broad, and 5 feet deep, every 4 hours. That the smell is a little sulphureous, especially at a distance. The taste a little sulphureous and saline, but not at all subacid. The heat is but temperate in summer; in winter much greater. It does not turn black with galls. With ol. tartar. p. deliquium, spiritus salis ammoniaci, and many mineral acid spirits mixed with it, it made no alteration; nor did the solution of fine silver in aquafortis make any change or precipitation. And the taste is scarcely different from pure limpid water. The warmth of it seems not to exceed the tepid heat of Bristol well.

The bath-water is conveyed into a copper, where it is made to seeth by artificial heat, and is afterwards brought into the common baths to increase the heat as the particular cases require. It is said to heal ulcers, to dissolve coagulations, to brace relaxed nerves, to cure scabs and leprous affections, &c.

Observations made on a Comet that appeared at Rome. By the late Rev. Mr. John Ray. N° 309, p. 2350.

On Dec. 20, 1664, N. S. about 3 o'clock in the morning, I observed the comet; it was in the constellation of Hydra, not far from the foot of Crater. It appeared about the size of a star of the first magnitude, but not near so lucid and bright. It had a very long tail, which pointed almost directly towards the heart of Hydra; the tail showed somewhat like rays of a candle burning in a mist: its figure was conical; its length 5 or 6 degrees; the breadth at the base not above a degree and a half. The body of this comet was about 3 degrees to the south east of the most southerly star in the foot of Crater; it stood very nearly in a right line with the two lowermost stars in the foot of Crater, which are common to it and Hydra.

Dec. 21, at about the same time it was removed about a deg. and half more westward, and a little to the south; the tail pointing still towards the heart of Hydra, and appeared at the least 10 degrees long.—Dec. 22, at the same hour, it was removed in the same direction, and about the same distance, as the night before. Its tail still pointed to Cor Hydræ, or a little above it, as the two former days, and was rather longer than shorter: I thought it also appeared brighter and larger; its body being larger than any fixed star, except Sirius.—Dec. 23, it was removed still in the same direction, and about the same distance, as the day before; the comet brighter, the tail as long as ever, and pointed almost directly to Cor Hydræ.—Dec. 24, 25, 26; all these 3 nights were cloudy, so that I could make no observations.—Dec. 27, we found it strangely removed from the place where it was: but it was still westward, and a little to the south, as before. The body of the star was still brighter, and the cauda about it greater, and more bushy, and yet as long as before; it pointed almost directly against Canis Major. Its body was among the stars of Argo.—Dec. 28, at the same time, it was removed above 2 degrees towards the same point, and come within 4 or 5 degrees of the most eastern stars in the bright triangle in the hips of Canis Major. The moon shining, we could not so well judge, either of the size of the body, or the length and bushiness of the tail.—Dec. 29, it was strangely removed, and got before, not the eastern star only of the bright triangle, but also the most northern. I think, at least, in this last 24 hours, it had moved 4 degrees. The moon shining bright, the tail could not well be observed, yet still it seemed to point directly to Canis Minor.

*The Universal Resolution of Cubic and Biquadratic Equations, as well Analytical as Geometrical and Mechanical, By Mr. J. Colson.** N° 309, p. 2353. *Translated from the Latin.*

§ I. Of the general cubic equation

$$x^3 = 3px^2 + 3qx + 2r \\ \qquad\quad - 3p^2 + p^3 \\ \qquad\qquad\quad - 3pq,$$

* We find very few circumstances concerning this mathematician. Besides the present paper he gave two others to the Royal Society, viz. one in vol. 24, being an account of negativo-affirmative arithmetic, and the other in vol. 39, on the construction and use of spherical maps. He was author of the British Hemisphere, being a map of a new contrivance, in the form of a half globe, of about 15 inches in diameter, but comprehending the whole known surface of the habitable earth, the city of London being the centre or vertex of the map. In 1736 he published a translation from the Latin of Sir Isaac Newton's Method of Fluxions and Infinite Series, with a large comment on the whole

The three roots are:

$$x = p + \sqrt[3]{r + \sqrt{r^2 - q^3}} + \sqrt[3]{r - \sqrt{r^2 - q^3}},$$

$$x = p - \frac{1 - \sqrt{-3}}{2} \times \sqrt[3]{r + \sqrt{r^2 - q^3}} - \frac{1 + \sqrt{-3}}{2} \times \sqrt[3]{r - \sqrt{r^2 - q^3}},$$

$$x = p - \frac{1 + \sqrt{-3}}{2} \times \sqrt[3]{r + \sqrt{r^2 - q^3}} - \frac{1 - \sqrt{-3}}{2} \times \sqrt[3]{r - \sqrt{r^2 - q^3}}.$$

Or, to make the arithmetical calculation easier and readier, if we put $m + \sqrt{n}$ for the cube root of the irrational binomial $r + \sqrt{r^2 + q^3}$, then will the same three roots of the equation be $x = p + 2m$, and $x = p - m \pm \sqrt{-3n}$.

Therefore when there is given any cubic equation, we must compare its terms with the several terms of the general equation, by which the values of p, q, r, will be easily found: then these being known, all the roots of the given equation will thence be known. And of this solution here follow some examples in numbers.

1. Let it be proposed to find the root x of this equation, $x^3 = 2x^2 + 3x + 4$. First, by comparing or equating the like terms, there will be $3p = 2$, or $p = \frac{2}{3}$; secondly, $3q - 3p^2 = 3$, that is, $3q - \frac{4}{3} = 3$, or $q = \frac{13}{9}$; thirdly, $2r + p^3 - 3pq = 4$, that is, $2r - \frac{70}{27} = 4$, or $r = \frac{89}{27}$; also $r^2 - q^3 = \frac{212}{27}$; therefore $x = \frac{2}{3} + \sqrt[3]{\frac{89}{27} + \sqrt{\frac{212}{27}}} + \sqrt[3]{\frac{89}{27} - \sqrt{\frac{212}{27}}}$. The two other roots are impossible.

2. In the equation $x^3 = 12x^2 - 41x + 42$, it will be first $3p = 12$, or $p = 4$; next, $3q - 3p^2 = -41$, that is $3q - 48 = -41$, or $q = \frac{7}{3}$; thirdly $2r + p^3 - 3pq = 42$, that is, $2r + 36 = 42$, or $r = 3$; hence $r^2 - q^3 = -\frac{100}{27}$: then the cube root of $r + \sqrt{r^2 - q^3}$, that is, of the binomial surd $3 + \sqrt{-\frac{100}{27}}$, extracted by the methods of the arithmetic of surds, is $-1 + \sqrt{-\frac{4}{3}} = m + \sqrt{n}$: therefore $x = p + 2m = 4 - 2 = 2$, also $x = p - m \pm \sqrt{-3n} = 4 +$

work, consisting of annotations, illustrations, and supplements. At the date of this publication Mr. Colson was in the situation of master of Sir Joseph Williamson's free mathematical school at Rochester, and a F. R. S. Three years after this we find he succeeded Mr. Sanderson as Lucasian Professor of Mathematics at Cambridge, a situation which he held for 20 years, after which he was succeeded, in 1759, by Dr. Waring. Though Mr. Colson died only Dec. 20, 1760. The character of our author seems to have been chiefly that of minute precision, and patient laborious industry. The present paper, on the solution of cubic and biquadratic equations, is founded nearly on the idea of the solution by Descartes, by assuming the biquadratic as equal to the product of two quadratics with indeterminate coefficients. The construction of the equations also, by means of the circle and parabola, is in imitation of the like constructions of Descartes in his geometry, and of Mr. Baker in his Geometrical Key. Mr. Colson, it seems, was so well pleased with the Analytical Institutions of the Signora Agnesi, that he made an entire translation of that lady's work, from the Italian, the copy of which has lately been found among his papers, and published at the exoence of Baron Maseres, in two volumes, 4to.

$1 \pm \surd 4 = 7$ or 3. Or again, another cubic root of the same binomial $3 + \surd - \frac{100}{27}$ (for it has three) is $\frac{3}{2} + \surd - \frac{1}{12} = m + \surd n$; and hence the root $x = p + 2m = 4 + 3 = 7$, and also $x = p - m \pm \surd - 3n = 4 - \frac{3}{2} \pm \surd \frac{1}{4} = 3$ or 2. Or lastly, the third cubic root of the same binomial $3 + \surd - \frac{100}{27}$ is $- \frac{1}{2} - \surd - \frac{25}{12} = m + \surd n$; and therefore the root $x = p + 2m = 4 - 1 = 3$, and also $x = p - m \pm \surd - 3n = 4 + \frac{1}{2} \pm \surd \frac{25}{4} = 7$ or 2.

3. In the equation $x^3 = - 15x^2 - 84x + 100$, it will be $p = - 5$, $q = - 3$, $r = 135$; and the cube root of the binomial $135 + \surd 18252$ is $3 + \surd 12$: therefore the root is $x = - 5 + 6 = 1$, and the other roots $x = - 5 - 3 \pm \surd - 36 = - 8 \pm \surd - 36$ are impossible.

4. In the equation $x^3 = 34x^2 - 310x + 1012$, it will be $p = \frac{34}{3}$, $q = \frac{226}{9}$, $r = \frac{5536}{27}$; and the cube root of the binomial $\frac{5536}{27} + \surd \frac{707560}{27}$ is $\frac{16}{3} + \surd \frac{10}{3}$: therefore the root $x = \frac{34}{3} + \frac{32}{3} = 22$, and the two other roots $x = \frac{34}{3} - \frac{16}{3} \pm \surd - 10 = 6 \pm \surd - 10$ are impossible.

5. In the equation $x^3 = 28x^2 + 61x - 4048$, it will be $p = \frac{28}{3}$, $q = \frac{967}{9}$, $r = - \frac{25010}{27}$; and the cube root of the binomial $- \frac{25010}{27} + \surd - 382347$ is $\frac{41}{6} + \surd - \frac{243}{4}$: therefore $x = \frac{28}{3} + \frac{41}{3} = 23$, and $x = \frac{28}{3} - \frac{41}{6} \pm \surd \frac{729}{4} = 16$ or $- 12$, the two other roots.

6. In the equation $x^3 = - x^2 + 166x - 660$, it will be $p = - \frac{1}{3}$, $q - \frac{499}{9}$, $r = - \frac{9658}{27}$; and the cube root of the binomial $- \frac{9658}{27} + \surd - \frac{1147205}{27}$ is $- \frac{22}{3} + \surd - \frac{5}{3}$: therefore $x = - \frac{1}{3} - \frac{44}{3} = - 15$, and also $x = - \frac{1}{3} + \frac{22}{3} \pm \surd 5 = 7 \pm \surd 5$, irrational.

7. In the equation $x^3 = 63x^2 + 99673x + 9951705$, it will be $p = 21$, $q = \frac{100996}{3}$, $r = 6031680$; and the cube root of the binomial $6031680 + \surd - \frac{47887175043136}{27}$ is $183 + \surd - \frac{529}{3}$: therefore $x = 21 + 366 = 387$, and also $x = 21 - 183 \pm \surd 529 = - 139$ or $- 185$.

And thus we are to proceed in other examples. Now the theorem may be investigated in the following manner. Suppose the root of any cubic equation to be $z = a + b$, the cube of which is $z^3 = a^3 + 3a^2b + 3ab^2 + b^3 = a^3 + 3ab \times \overline{a + b} + b^3$. Now instead of $a + b$ substituting its value z, it gives $z^3 = 3abz + a^3 + b^3$, which is a cubic equation raised from the root $z = a + b$, of which the second term is wanting. But to reduce this to a better form, assume the equation $z^3 = 3qz + 2r$, instead of the former $z^3 = 3abz + a^3 \times b^3$; then to change the one into the other, there is first $3q = 3ab$, or $q^3 = a^3b^3$; secondly $2r = a^3 + b^3$, or $2ra^3 = a^6 + a^3b^3 = a^6 + q^3$; then resolving this quadratic equation gives $a^3 = r + \surd \overline{r^2 - q^3}$, and hence $b^3 = 2r - a^3 = r - \surd \overline{r^2 - q^3}$. Therefore at length we obtain $a = \sqrt[3]{} \overline{r + \surd \overline{r^2 - q^3}}$, and $b = \sqrt[3]{} \overline{r - \surd \overline{r^2 - q^3}}$. Therefore in the cubic equation $z^3 = 3qz + 2r$, we shall have the root $z = (a + b =) \sqrt[3]{} \overline{r + \surd \overline{r^2 - q^3}} + \sqrt[3]{} \overline{r - \surd \overline{r^2 - q^3}}$.

But this root is really threefold, from the triple value which $\sqrt[3]{r + \sqrt{r^2 - q^3}}$ and $\sqrt[3]{r - \sqrt{r^2 - q^3}}$ can assume; for the cube root of any quantity whatever is threefold, and the cube root of even unity itself is either 1, or $- \frac{1}{2} + \frac{1}{2}\sqrt{-3}$, or $- \frac{1}{2} - \frac{1}{2}\sqrt{-3}$; as will appear by cubing each of these expressions, which gives unity. Therefore if $1 \times (r + \sqrt{r^2 - q^3})$ or $r + \sqrt{r^2 - q^3}$ be considered as a cube, its cubic root or roots will be $\sqrt[3]{1} \times \sqrt[3]{r + \sqrt{r^2 - q^3}}$; that is, first $1 \times \sqrt[3]{r + \sqrt{r^2 - q^3}}$, which we have above called $m + \sqrt{n}$, or $1 \times \overline{m + \sqrt{n}}$. Secondly $\frac{-1 + \sqrt{-3}}{2} \times \sqrt[3]{r + \sqrt{r^2 - q^3}} = \frac{-1 + \sqrt{-3}}{2} \times \overline{m + \sqrt{n}}$. Thirdly $\frac{-1 - \sqrt{-3}}{2} \times \sqrt[3]{r + \sqrt{r^2 - q^3}} = \frac{-1 - \sqrt{-3}}{2} \times \overline{m + \sqrt{n}}$. In like manner the residual $r - \sqrt{r^2 - q^3}$ or $1 \times (r - \sqrt{r^2 - q^3})$, being considered as a cube, will have three cubic roots, viz. first $1 \times \sqrt[3]{r - \sqrt{r^2 - q^3}} = 1 \times \overline{m - \sqrt{n}} = m - \sqrt{n}$: secondly, $\frac{-1 + \sqrt{-3}}{2} \times \sqrt[3]{r - \sqrt{r^2 - q^3}} = \frac{-1 + \sqrt{-3}}{2} \times \overline{m - \sqrt{n}}$: thirdly $\frac{-1 - \sqrt{-3}}{2} \times \sqrt[3]{r - \sqrt{r^2 - q^3}} = \frac{-1 - \sqrt{-3}}{2} \times \overline{m - \sqrt{n}}$. And by a due connection of these roots, we have the three roots, viz. $z = \sqrt[3]{r + \sqrt{r^2 - q^3}} + \sqrt[3]{r - \sqrt{r^2 - q^3}} = m + \sqrt{n} + m - \sqrt{n} = 2m$; and $z = \frac{-1 + \sqrt{-3}}{2} \sqrt[3]{r + \sqrt{r^2 - q^3}} + \frac{-1 - \sqrt{-3}}{2} \sqrt[3]{r - \sqrt{r^2 - q^3}} = \frac{-1 + \sqrt{-3}}{2} \times \overline{m + \sqrt{n}} + \frac{-1 - \sqrt{-3}}{2} \times \overline{m - \sqrt{n}} = -1 \times m + \sqrt{-3} \times \sqrt{n} = -m + \sqrt{-3n}$; and $z = \frac{-1 - \sqrt{-3}}{2} \times \sqrt[3]{r + \sqrt{r^2 - q^3}} + \frac{-1 + \sqrt{-3}}{2} \sqrt[3]{r - \sqrt{r^2 - q^3}} = \frac{-1 - \sqrt{-3}}{2} \times \overline{m + \sqrt{n}} + \frac{-1 + \sqrt{-3}}{2} \times \overline{m - \sqrt{n}} = -1 \times m - \sqrt{-3} \times \sqrt{n} = -m - \sqrt{-3n}$.

These therefore will be the three roots of the cubic equation $z^3 = 3qz + 2r$. And that the parts are duly connected in the foregoing manner, by multiplying them together in the usual way. Finally make $z = x - p$, and the equation $z^3 = 3qz + 2r$ will become $x^3 - 3px^2 + 3p^2x - p^3 = 3qx - 3pq + 2r$, which by transposition will be the same as the general equation first proposed, and its roots are the same as there exhibited.

Here it will be worthy of notice, that all the roots of any cubic equation are real and possible whenever the irrational member $\sqrt{r^2 - q^3}$ of the binominal includes an impossibility; that is, when q is a positive quantity, and q^3 greater than r^2. But if that member be possible, that is if q be a negative quantity, as also if it be positive, and q^3 less than r^2, then the equation has only one real and possible root, the two others being impossible ones. In this theorem, if p be $= 0$, that is, if the second term of the equation be wanting, then it becomes the case of Cardan's rules, the solution of which is contained in what is done above.

§ II. Of the General Biquadratic Equation $\left\{ \begin{array}{l} x^4 = 4px^3 + 2qx^2 + 8rx + 4s \\ \qquad\quad - 4p^2 \quad - 4pq - q^2 \end{array} \right.$

the 4 roots are $x = p - a \pm \sqrt{p^2 + q - a^2 - \frac{2r}{a}}$,

and $x = p + a \pm \sqrt{p^2 + q - a^2 + \frac{2r}{a}}$;

where a^2 is the root of this cubic equation $a^6 = p^2a^4 - 2pra^2 + r^2$.
$\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad + q \qquad - s$

Now when any biquadratic equation is given, a comparison must be made between its terms, and the corresponding terms of this general equation, by which means the quantities p, q, r, s will soon be found. Then the value of a will be discovered by the theorem in the former section; and lastly the four roots of the proposed biquadratic equation by the theorem just given.

An example or two may suffice to illustrate this solution.

Example 1.—To extract the roots of the biquadratic equation, $x^4 = 8x^3 + 83x^2 - 162x - 936$. First, by comparing the terms, $4p = 8$, or $p = 2$. Secondly, $2q - 4p^2 = 83$, or $q = \frac{99}{2}$. Thirdly, $8r - 4pq = -162$, or $r = \frac{117}{4}$. Fourthly, $4s - q^2 = -936$, or $s = \frac{6057}{16}$. Hence $p^2 + q = \frac{107}{2}$, and $2pr + s = \frac{7929}{16}$, and $r^2 = \frac{13689}{16}$; and therefore $a^6 = \frac{107}{2} a^4 - \frac{7929}{16} a^2 + \frac{13689}{16}$. Now that this equation, which is in effect a cubical one, may be resolved into its roots, we must have recourse to the first theorem, in which there will be $p = \frac{107}{6}$, $q = \frac{22009}{144}$, $r = \frac{2903923}{1728}$, and $r^2 - q^3 = -\frac{11940075}{16}$. But the cubic root of the binomial $\frac{2903923}{1728} + \surd - \frac{11940075}{16}$ is $-\frac{53}{12} + \surd - \frac{400}{3}$; and therefore $a^2 = \frac{107}{6} - \frac{53}{6} = 9$, and also $a^2 = \frac{107}{6} + \frac{53}{12} \pm \surd\, 400 = \frac{169}{4}$ or $\frac{9}{4}$. Or, which comes to the same, the six roots of the same equation, which is really cubo-cubic, are $a = \pm\, 3$, and $a = \pm\, \frac{13}{2}$, and $a = \pm\, \frac{3}{2}$, any one of which may be taken indifferently for the root, and will answer the purpose. As suppose in the present case we take $a = 3$: then by the theorem

$$x = p - a \pm \sqrt{p^2 + q - a^2 - \frac{2r}{a}} = 2 - 3 \pm \sqrt{4 + \frac{99}{2} - 9 - \frac{39}{2}} =$$

$-1 \pm 5 = 4$ or -6; and $x = p + a \pm \sqrt{p^2 + q - a^2 + \frac{2r}{a}} = 2 + 3 \pm \sqrt{4 + \frac{99}{2} - 9 + \frac{39}{2}} = 5 \pm 8 = 13$ or -3; which are the four roots of the given equation.

Example 2.—In the equation $x^4 = 20x^3 + 252x^2 - 6592x + 21312$, it will be $p = 5$, $q = 176$, $r = -384$, $s = 13072$. Hence $p^2 + q = 201$, and $2pr + s = 9232$, and $r^2 = 147456$; and thence $a^6 = 201a^4 - 9232a^2 + 147456$. Now in the cubic theorem it will be $p = 67$, and $q = \frac{4235}{3}$, and $r = 65219$; and the cubic root of the binomial $65219 + \sqrt{\frac{38889307072}{27}}$ will be $\frac{77}{2} + \sqrt{\frac{847}{12}}$. Therefore $a^2 = 67 + 77 = 144$, or $a = 12$; and hence $x = 5 - 12 \pm \sqrt{25 + 176 + 144 + 64} = -7 \pm 11 = 4$ or -18, and $x = 5 + 12 \pm \sqrt{25 + 176 - 144 - 64} = 17 \pm \sqrt{-7}$, two impossible roots.

Now the investigation of this theorem is as follows. By multiplying together the two quadratic equations $z^2 + 2az - b = 0$, and $z^2 - 2az - c = 0$, is formed the biquadratic equation $z^4 = \overline{4a^2 + b + c} . z^2 + \overline{2ac - 2ab} . z - bc$, where the second term is wanting, and which put equal to the equation $z^4 = ez^2 + fz + g$. Hence, first, $4a^2 + b + c = e$, or $b = e - 4a^2 - c$. Secondly, $2ac - 2ab = f$, that is, $2ac - 2ae + 8a^3 + 2ac = f$, hence $c = \frac{f}{4a} + \frac{e}{2} - 2a^2$, and thence $b = e - 4a^2 - \frac{f}{4a} - \frac{e}{2} + 2a^2 = -\frac{f}{4a} + \frac{e}{2} - 2a^2$. Thirdly, $-bc = g$, or $-\frac{f^2}{16a^2} + \frac{e^2}{4} - 2ea^2 + 4a^4 = -g$, that is, $a^6 = \frac{1}{2}ea^4 - \frac{1}{4}ga^2 - \frac{1}{16}ea^2 + \frac{1}{64}f^2$, which is in effect a cubic equation, composed of a^2 and the known or assumed quantities e, f, g. Now that root may be exhibited by the first theorem, and b and c will become known by the same calculation. But the roots of the equations $z^2 + 2az - b = 0$, and $z^2 - 2az - c = 0$, are $z = -a \pm \sqrt{a^2 + b}$, and $z = a + \sqrt{a^2 + c}$, or $z = -a \pm \sqrt{\frac{1}{2} - a^2 - \frac{f}{4a}}$, and $z = a \pm \sqrt{\frac{1}{2}e - a^2 + \frac{f}{4a}}$, which therefore will be the roots of the equation $z^4 = ez^2 + fz + g$, when a or a^2 is known from the equation $a^6 = \frac{1}{2}ea^4 - \frac{1}{4}ga^2 - \frac{1}{16}ea^2 + \frac{1}{64}f^2$. Now that this equation may become general, and furnished with all its terms, make $z = x - p$, then will $x^4 - 4px^3 + 6p^2x^2 - 4p^3x + p^4 = ex^2 - 2pex + p^2e + fx - fp + g$, also $x = p - a \pm \sqrt{\frac{1}{2}e - a^2 - \frac{f}{4a}}$, and $x = p + a \pm \sqrt{\frac{1}{2}e - a^2 + \frac{f}{4a}}$. Lastly, for convenience and brevity, make $e = 2q + 2p^2$, and $f = 8r$; then is $x^4 - 4px^3 + 4p^2x^2 = 2qx^2 - 4pqx + 2p^2q + p^4 + 8rx - 8pr + g$, $x = p - a \pm \sqrt{p^2 + q - a^2 - \frac{2r}{a}}$, also $x = p + a \pm \sqrt{p^2 + q - a^2 + \frac{2r}{a}}$, and $a^6 = \overline{p^2 + q} . a^4 - \overline{\frac{1}{4}q + \frac{1}{4}p^4 + \frac{1}{2}p^2q + \frac{1}{4}q^2} . a^2 + r^2$. Finally, make $g = 4s - q^2 + 8pr - p^4 - 2p^2q$, and the foregoing equations become

$$x^4 = \begin{matrix} 4px^3 \\ -4p^2 \end{matrix} + \begin{matrix} 2qx^2 \\ -4pq \end{matrix} + \begin{matrix} 8rx \\ -q^2 \end{matrix} + 4s, \text{ and } a^6 = \begin{matrix} p^2a^4 \\ +q \end{matrix} - \begin{matrix} 2pra^2 \\ -s \end{matrix} + r^2.$$

That is, every thing comes out as above supposed.

§ III. Hitherto has been concerning the analytical solution of cubic and biquadratic equations. But because their geometrical effection by the parabola is commonly taught, and is much valued by some, I shall exhibit it here more generally, and yet more compendiously.

Any cubic or biquadratic equation being proposed, a comparison must be made between its terms, and the corresponding terms of this following equation,

$$\begin{aligned} x^4 = \tfrac{2p}{q}x^3 &+ \tfrac{4pr}{q}x^2 + \tfrac{2p^2}{q}x + p^2 \\ &- 4r^2 \quad - \tfrac{2ps}{q} \quad - q^2 \\ &+ 2s \quad + 4rs \quad - s^2 \\ &- 1 \quad - 2q \quad + t^2, \end{aligned}$$

by means of which the values p, q, r, s, t will be easily found; any one of them being assumed at pleasure. Then in any given parabola AVB, (fig. 1, pl. 10) of which the principal vertex is V, axis VS, and perpendicular to the axis VT; take VS $= p$ within the parabola, and in the angle SVT inscribe ST $= q$, which being produced cuts the parabola in two points N and O. Bisect NO in M, and through M draw MA parallel to the axis, and meeting the curve in A. Parallel to NO draw AL, such that AL may be the latus-rectum of the parabola to the diameter AM, calling it unity. In AL, produced both ways if necessary, take AG $= r$, and from G draw GR parallel to the axis, to cut the curve in B, from whence take BR $= s$. From R draw RE parallel and equal to VT, to the left hand when q is a positive quantity, or to the right when q is negative. And the same thing is to be understood of AG and BR, which must be drawn on the contrary side when r and s are negative. Lastly with centre E, and radius EC $= t$, describe the circle CKkc, which will cut the parabola in as many points, as there are real roots of the given equation. For, from those points P, K, &c. draw CP, KΠ, &c. parallel to ST, and terminating in the line GR, produced if necessary; then each of these will be a value of x, or the required root of the given equation, viz. those lying towards the right-hand being affirmative roots, but those on the left negative ones. And when there is a point of contact, instead of an intersection, it is considered as two points of intersection that are infinitely near each other.

The only difference between cubic and biquadratic equations, constructed after this manner, will be this, that in the former, because of the last term being absent in the foregoing equation, it will always be $p^2 - q^2 - s^2 + t^2 = 0$, or $t = \sqrt{s^2 + q^2 - p^2}$. Therefore with the centre E and radius EC $= \sqrt{\text{BR}^2 + (\text{ER}^2)\ \text{ST}^2 - \text{VS}^2}$ any circle CKkc being described, one of the roots CP, in the above construction, becomes nothing.

The demonstration of the foregoing is as follows. Every thing remaining as in the construction, and CP being produced, if necessary, to cut AM in H, then CH will be an ordinate of the parabola to the diameter AH, and therefore $CH^2 = AL \times AH = AH$ because $AL = 1$. But $CH = CP + AG$, and $AH = GB + BP$, therefore $CP^2 + 2AG \times CP + AG^2 = GB + BP$. But, by the nature of the parabola, $AG^2 = BG$: hence $CP^2 + 2AG \times CP = BP$. Now from the point C let CD be perpendicular to BP, which may also meet EI parallel to BP in the point I. Then, because of the similar triangles CDP and TVS, it will be $DP = \frac{VS \times CP}{ST}$, and $CD = \frac{VT \times CP}{ST}$; therefore $CP^2 + 2AG \times CP = BP = DP + DB = \frac{VS \times CP}{ST} + BR - IE$; or $CP^2 + 2AG \times CP - \frac{VS}{ST} CP - BR = - IE$. But $IE^2 = CE^2 - CI^2 = CE^2 - CD^2 - VT^2 - 2CD \times VT = CE^2 - \frac{VT^2 \times CP^2}{ST^2} - VT^2 - \frac{2VT^2 \times CP}{ST} =$ (because $VT^2 = ST^2 - SV^2$) $CE^2 - CP^2 + \frac{SV^2}{ST^2} CP^2 - ST^2 + SV^2 - 2ST \times CP + \frac{2SV^2}{ST} CP$; this therefore will be equal to the square whose side is $CP^2 + 2AG \times CP - \frac{VS}{ST} CP - BR$. And when this equation is reduced to the terms p, q, r, s, t, it becomes the very equation proposed.

Hence it appears that any one biquadratic equation will admit of innumerable different constructions by the parabola, according to the different values of that quantity which we said might be assumed at pleasure. But the most simple case is, by making $VS = p = 0$, and then the construction coincides with the common one, in which the right lines CP, &c. representing the roots, are perpendicular to the axis; in which case the equation becomes

$$x^4 = - 4rx^3 \begin{matrix} - 4r^2x^2 \\ + 2s \\ - 1 \end{matrix} \begin{matrix} + 4rsx \\ - 2q \\ \ \end{matrix} \begin{matrix} - q^2 \\ - s^2 \\ + t^2, \end{matrix}$$

which is easily constructed as above.

§ IV. But lest the organical description of the parabola should seem too difficult, we may have recourse to a certain mechanical artifice, to be performed by means of a plummet, or thread with a weight hanging at the end of it; by help of which the last equation may be constructed very easily and exactly, and thence the roots of any cubic or biquadratic equations may be found; and that without drawing any other than right lines and a circle. Now this construction which may be called a mechanical one, is in the following manner.

Against a smooth and upright wall, or any other plane perpendicular to the horizon, at any point F, (fig. 2, pl. 10) let there be hung a very fine flexible thread FP, with any weight P at its extremity. In this thread mark any point

N, at a sufficient distance from the point of suspension F; or it may be marked by a small knot at N. Then taking at pleasure NO for unity, by the middle point A, in the said plane, draw the right line AQ parallel to the horizon, and produced both ways as far as necessary. These general preparations being made, then for the application to any particular case, make AQ $= r$; the quantities q, r, s, t, being first determined in the last equation above, either arithmetically or geometrically, as the exigence of the proposed equation may require. Then with a sharpe style or bodkin, or with the fine point of a pair of compasses, let the thread be inflected and moved from its place to such a point B as that the point N may fall on the point Q just found. In BQ take BR $= s$, and at R raise the perpendicular RE $= q$. But those lines AQ, BR, RE, must be taken the contrary way when the values of q, r, s are negative. Lastly, fix one leg of the compasses in the point E, and let the other leg, extended to the distance EZ $= t$, be carried round with a circular motion, taking with it the thread FZP. By this circulation of the thread the weight P will sometimes ascend and sometimes descend with a reciprocal motion, and the knot N will be sometimes above and sometimes below the horizontal line AQ. But whenever the knot N shall be found in the line AQ, as in the points D, d, Δ, δ, it will cut off the right lines DQ, dQ, ΔQ, δQ, which will be all the real roots of the given equation, viz. those on the right hand the affirmative roots, and those on the left the negative ones. The demonstration of which is manifest from what is before done, and by attending to the parabola passing through the points B, C, c, k, K. For making F the focus of the parabola, whose distance from the vertex is $\frac{1}{4}$ON, it is known that all the lines, as FB + BQ, FC + CD, &c. always make up the same sum.

And from the principles here laid down, it will not be difficult to construct a sufficiently neat and accurate instrument, by means of which the roots of all such equations may be easily found, and exhibited to the eye.

The Analytical Solution of any Equations, of the Third, Fifth, Seventh, Ninth, and the other higher uneven Powers, by Rules similar to those called Cardan's. By Mr. Abr. Demoivre, F. R. S. N° 309, p. 2368. *Translated from the Latin.*

If n denote any number whatever, y an unknown quantity, and a the absolute known quantity, or what is called the homogeneum comparationis: also let the relation between these be expressed by this equation,

$$ny + \frac{nn-1}{2 \times 3} ny^3 + \frac{nn-1}{2 \times 3} \times \frac{nn-9}{4 \times 5} ny^5 + \frac{nn-1}{2 \times 3} \times \frac{nn-9}{4 \times 5} \times \frac{nn-25}{6 \times 7} ny^7 + \text{\&c.} = a.$$

From the nature of this series it is manifest, that if n be taken any uneven integer number, either affirmative or negative, then the series will stop or be finite, and the equation become one of those above-mentioned; the root of which is,

$$(1)\quad y = \tfrac{1}{2}\sqrt[n]{\sqrt{1+aa}+a} - \frac{\frac{1}{2}}{\sqrt[n]{\sqrt{1+aa}+a}},$$

$$\text{or } (2)\quad y = \tfrac{1}{2}\sqrt[n]{\sqrt{1+aa}+a} - \tfrac{1}{2}\sqrt[n]{\sqrt{1+aa}-a},$$

$$\text{or } (3)\quad y = \frac{\frac{1}{2}}{\sqrt[n]{\sqrt{1+aa}-a}} - \tfrac{1}{2}\sqrt[n]{\sqrt{1+aa}-a};$$

$$\text{or } (4)\quad y = \frac{\frac{1}{2}}{\sqrt[n]{\sqrt{1+aa}-a}} - \frac{\frac{1}{2}}{\sqrt[n]{\sqrt{1+aa}+a}}.$$

For example, let it be required to find the root of this equation of the 5th power, $5y + 20y^3 + 16y^5 = 4$; in which case it will be $n = 5$, and $a = 4$ Then the root, according to the first form, will be $y = \tfrac{1}{2}\sqrt[5]{\sqrt{17}+4} - \frac{\frac{1}{2}}{\sqrt[5]{\sqrt{17}+4}}$, which is very expeditiously reduced into common numbers in this manner. First $\sqrt{17} + 4 = 8.1231$, its log. is 0.9097164, the 5th part of which is 0.1819433, the natural number corresponding to which is $1.5203 = \sqrt[5]{\sqrt{17}+4}$. Also the arithmetical complement of 0.1819433 is 9.8180567, to which corresponds the number $0.6577 = \frac{1}{\sqrt[5]{\sqrt{17}+4}}$. Therefore the half difference of these numbers is $0.4313 = y$.

It may be here observed that, instead of the general root, it may be sufficient to take $y = \tfrac{1}{2}\sqrt[n]{2a} - \frac{\frac{1}{2}}{\sqrt[n]{2a}}$, whenever the number n is very large in respect of unity. As, if the equation were $5y + 20y^3 + 16y^5 = 682$; then the log. of $2a = 3.1348143$, its 5th part is 0.6269628, to which corresponds the number 4.236: also the arithmetical complement is 9.3730372, the natural number of which is 0.236: then the half difference of these two is $2 = y$.

Again, if in the foregoing equation the signs be made alternately affirmative and negative, or, which is the same, if the series be of the following kind, $ny + \frac{1-nn}{2\times 3}ny^3 + \frac{1-nn}{2\times 3}\times\frac{9-nn}{4\times 5}ny^5 + \frac{1-nn}{2\times 3}\times\frac{9-nn}{4\times 5}\times\frac{25-nn}{6\times 7}ny^7 + \&c = a$, its root will be

$$(1)\quad y = \tfrac{1}{2}\sqrt[n]{a+\sqrt{aa-1}} + \frac{\frac{1}{2}}{\sqrt[n]{a+\sqrt{aa-1}}},$$

$$\text{or } (2)\quad y = \tfrac{1}{2}\sqrt[n]{a+\sqrt{aa-1}} + \tfrac{1}{2}\sqrt[n]{a-\sqrt{aa-1}},$$

$$\text{or } (3)\quad y = \frac{\frac{1}{2}}{\sqrt[n]{a-\sqrt{aa-1}}} + \tfrac{1}{2}\sqrt[n]{a-\sqrt{aa-1}},$$

$$\text{or } (4)\quad y = \frac{\frac{1}{2}}{\sqrt[n]{a-\sqrt{aa-1}}} + \frac{\frac{1}{2}}{\sqrt[n]{a+\sqrt{aa-1}}}.$$

Here it is to be observed that, if $\frac{n-1}{2}$ should be an odd number, the sign of the root when found must be changed to the contrary sign.

Let there be proposed the equation $5y - 20y^3 + 16y^5 = 6$; here $n = 5$, and $a = 6$; then the root is $y = \frac{1}{2}\sqrt[5]{6 + \sqrt{35}} + \frac{\frac{1}{2}}{\sqrt[5]{6+\sqrt{35}}}$. Or, because $6 + \sqrt{35} = 11.916$, its log. will be 1.0761304, and its 5th part 0.2152561, the arithmetical complement of which is 9.7847439: hence the numbers answering to these logarithms are 1.6415 and 0.6091, the half sum of which is $1.1253 = y$.

But if it happen that a be less than unity, then the second form of the root is rather to be preferred, as more convenient for the purpose. Thus, if the equation be $5y - 20y^3 + 16y^5 = \frac{61}{64}$, it will be $y = \frac{1}{2}\sqrt[5]{\frac{61}{64} + \sqrt{-\frac{375}{4096}}} + \frac{1}{2}\sqrt[5]{\frac{61}{64} - \sqrt{-\frac{375}{4096}}}$. And if by any means the 5th root of binomials can be extracted, the root will come out true and possible, although the expression seems to include an impossibility. Now the 5th root of the binomial $\frac{61}{64} + \sqrt{-\frac{375}{4096}}$ is $\frac{1}{4} + \frac{1}{4}\sqrt{-15}$, and the fifth root of the binomial $\frac{61}{64} - \sqrt{-\frac{375}{4096}}$ is $\frac{1}{4} - \frac{1}{4}\sqrt{-15}$, and the half sum of these two roots is $\frac{1}{4} = y$. But if this extraction cannot be performed, or should seem too difficult, the thing may always be effected by the table of natural sines in the following manner.

To the radius 1 let $a = \frac{61}{64} = 0.95112$ be the sign of a certain arc, which therefore will be 72° 23′; the 5th part of which (because $n = 5$) is 14° 28′; the sine of this is $0.24981 = \frac{1}{4}$ nearly. And the same way for equations of higher degrees.

Several Experiments showing the strange Effects of the Effluvia of Glass, producible on its Motion and Attrition. By Mr. Fr. Hauksbee, F. R. S. N° 309, p. 2372.

EXPER. I. *Containing further Observations on the Attrition of Glass.*—In the late experiments, which seem something to illustrate attraction or electricity, by the ends of the surrounding threads pointing to the axis of the fricated glass, there is something further very remarkable; viz. that after the attrition of the glass has been a little while continued, and the effluvia has laid hold on the hairy woolly threads, hanging loose, that then, though the rubbing was ceased, and the glass without motion, yet all the threads would continue their directed position for 4 or 5 minutes, and sometimes longer, before they could disengage themselves from the attracting or electrical effluvia. And if one's finger, or any thing else be approached near the pointing ends of the threads, while the effluvia act with so much vigour, as to sustain them directed, that then they would flee and avoid a touch from it; just as if the north pole of the magnet were applied towards the south point of a needle: and at the same time, if the finger be held near, at about an inch from the end of the aforesaid thread, it will there seem to be attracted, removing itself somewhat out of its direction to the approached body. But if any thing be held between the glass and the directed thread, then the thread immediately quits hold of the effluvia, and returns to its first position; yet on withdrawing the interposed body, (if it has not removed itself too far out of the reach of the effluvia) it will again return to its tendency, and so remain, till the weight of its body be too great for the declining strength of the effluvia to support it in such a direction. I have since tried the same experiment with a globe glass, which when the attrition was made, would in all manner of positions attract the surrounding threads, directing them towards its centre.

EXPER. II. *Showing the Direction of Woollen Threads every way from the Axis, towards the Circumference of a fricated Glass.*—In order to try what appearances might be afforded by placing the woollen threads on the axis within, and making the attrition on the outer surface as usual, I took a globe glass about six inches diameter, and having conveyed into it some woollen threads tied to a stick, which was placed in it as an axis, and being fixed on the machine, the great wheel was turned, and the hand applied as usual; but I soon found the inconvenience of a glass of that form, the threads entangling with each other, and there was no way to loosen or separate them; however they seemed evidently disposed, had they been at liberty, to have answered my expectation. Further, on bringing my hand near the glass, which was then at rest, I was surprised to see a motion of the bodies within side; and on inquiry, I found it was occasioned by the approach of my hand, since I could by a motion of my finger towards the point of any of the threads that touched not the inside of the glass, drive it any way; it seemed to fly my finger held on any side of it, and this without touching the outer surface by half an inch or more. Now when this experiment was made by hanging the threads near the outside, it was very strange, to see them fly the approach of a finger; yet how much more surprising is it, to see the same performed even when a body so solid as glass interposes; which shows the subtilty of the effluvia, the body from which it is produced seeming to be no impediment to its motion: besides, it seems very much to resemble or emulate a solid, since motion may be given to a body, by pushing the effluvia at some distance from it: but what is still more surprising is, that this body, though so subtle as seemingly to perviate glass, will not, as has been before observed, affect a light body through a piece of muslin: now whether the muslin absorbs the effluvia, or what other laws it may be subject to, I cannot determine. I have tried the same experiment with a glass exhausted of its air, but it afforded nothing observable.

Exper. III. *Being a Repetition and Improvement of the former.*—I procured a glass of a more suitable form, for a repetition of the foregoing experiment, being nearly a hemisphere, with a neck to it, by which it was screwed to one end of a spindle, and had motion given it by the large wheel as usual. This manner of fixing, and the figure of the glass, gave me the liberty of rubbing it as well within as without; though on trial I find, that either way is much the same; for when the threads are held within, and the attrition made on the outside, or the contrary, or the friction made on the same side the threads are used, it makes very little difference. To proceed: when the threads were fixed on an axis within, and the motion and attrition made as usual, the threads then pointed to the axis; and then, if a finger was approached near the outside of the glass, a motion would be given to the point of the thread that was nearest it within; and at the same time, if the threads were removed to the outside, and the finger held within, the like motion would be given to them there. Generally, the threads seem to fly the approach of the finger; yet sometimes I have seen them jump suddenly towards it, at more than an inch distance. It is remarkable, that the figures represented by the directed threads, from, and towards the centre, not only mimic, but seem very much to resemble the centripetal and centrifugal tendencies of bodies in their motions either way.

Exper. IV. *Showing that the Effluvia of Glass are capable of performing the Office of Attrition; causing a Light, by falling on an exhausted Glass in Motion, as if rubbed by the Hand.*—That the effluvia of glass are very considerable in the productions of divers phænomena, has already been abundantly proved; but that they should act the part of a solid body, by performing the office of one, is still more extraordinary: and that they do so, the following experiment sufficiently demonstrates, and seems to corroborate a hint I gave in the 2d experiment of their emulating such a body, by causing a thread to fly the approaching finger. I took a large globe glass, about 9 inches diameter, which having exhausted of its air, I fixed to give it motion, by the machine described in Philos. Transact. N° 304, its axis standing perpendicular. Another globe glass, about the size of the former, was placed to give motion to it by a new machine, and was wrought with its axis parallel to the horizon. This last globe, with its contents of common air, was fixed so as to move within an inch of touching the others. In these positions the machines were worked, and the naked hand applied to the unexhausted glass, the effluvia of which in a little time reaching the exhausted glass in motion, immediately produced a light on that part of it nearest to the other, without the assistance of a touch from any thing else to influence it. This light is pretty strong, and extends itself so far on the globe as the effluvia could reach. It is not so much of a purple colour, as when it is caused by the attrition of the hands; but it will continue, or live on the globe for half a minute or more, after the motion of the rubbed glass has ceased: but if the fricated glass be kept in motion, and the other at rest, the light instantly dies, yet recovers again on the first motion given it. After this I took a long glass, which had lain by me exhausted of its air for more than six months: this glass having been rubbed a little with my hand, to expel the humidity on its outside, I held it over the unexhausted glass in motion, which at the same time was rubbed by my hand; and it was very surprising to see what large flashes of light would be produced in the long glass without touching the glass in motion, though itself was neither moved nor excited by any immediate attrition.

Tabula exhibens Cœli Tempestates, et Mutationes, ter unoquoque die: item Plagam Ventorum, et Nubium; altitudinem Mercurii in Barometro, et Spirituum in Thermometro; et denique Pluviæ quantitatem, quæ quibusdam Diebus, et unoquoque Mense, per Infundibulum 12 *Pollices Latum, apud Upminster in Comitatu Essexiæ decidebat Anno* 1705. *Per W. Derham, Rectorem Upminsterensem, et S. R. S.* N° 309, p. 2378.

This register of the weather it is of no use now to retain.

An Account of Balls of Hair taken from the Uterus and Ovaria of several Women; by Mr. James Yonge, F. R. S. N° 309, p. 2387.

In Nov. 1705, I was called to deliver a woman 30 years of age, who was 4 days in labour of her first child: the head, being too large for the passage, stuck immoveable at the os pubis; so that I could neither fasten a crotchet, nor draw it out by a cupping-glass fixed to the scalp with an air pump. In this extremity I directed my son to open the child's head, and take out all the brains, with so much of the skull as he could; and then by a cord fastened round the neck with a noose, to draw it out, which was soon and easily done.

The child was corrupted and stunk much, as also the lochia, which flowed for 3 weeks; soon after they ceased, the menstrua appeared, and the woman went abroad: about 6 weeks after her delivery, she was seized with violent convulsions, and hysteric fits; which lasted near three days; when a painful tumor arose on the left side of her belly, which ended in an eruption of near a pint of white thick matter, with small knobs of a substance like the yolk of boiled eggs. All symptoms immediately vanished, only she complained of the great hollowness where the tumor had been. Four days after this, the like swelling appeared on the right side of her belly, which continued with a small flux of matter about 5 or 6 months, notwithstanding the many remedies I used to cure her.

About that time there appeared in the pudenda a bunch of something like greasy wool, which being drawn forth, proved to be a ball, or wad of hair, the size of a turkey's egg, immersed in an unctuous slime; adhereing on one side to a membrane as large as the palm of a man's hand: and in the middle of it a small pyramidal bone, resembling a split tooth. On this the tumor sunk, and the fluor immediately ceased, and her lunary flux (which all this while had not appeared) flowed as usual, and she has continued in perfect health ever since, full 9 months.

This case, though rare and extraordinary, has sometimes happened to others.

In Dr. Hook's Phil. Collect. N° 2, Dr. Tyson tells us he dissected a young gentlewoman, and found the right testicle, or ovarium, swoln into two bags, almost as large as a man's head, full of a pale serum, in which floated several lumps of a soft fat matter, which dissolved in part when put into hot water. One of those pieces was half as large as a man's fist, in which lay a great deal of hair (as there did, though not so plentifully, in all the rest) of a silver colour, soft, fine, strong, and above two feet long; it was not fastened to, nor seemed to grow from any part, but lay entangled in this matter, and in it a bony substance exactly resembling that which is commonly called the eye or dog-tooth.

Another time, dissecting a woman 40 years old, Dr. Tyson found near the uterus, a bag as large as a turkey's egg, and in it a fatty substance, like that above mentioned, with a great quantity of light soft hair fastened to a fleshy substance: within this cystis a bone, in some sort resembling a mandible, having several sockets, in which were seated 3 dentes molares, or grinder teeth, and a 4th not yet quite grown out.

Dr. Grew tells us, that in the museum of the R. S. is such another tooth, found by Dr. Tyson, after the same manner. And the Doctor himself tells us, ubi supra, that Dr. Needham found a tooth and hair in the ovarium of a dead woman. And Dr. R. Hook says, that Dr. Samson found the like in two great globose tumours depending on, or rather parts of the extended ovarium, wrapt up in dissolvable and inflammable fat, of a yellow colour.

About 10 years since, Sir Andrew Leak gave me a small bunch of hair, being part of what had been found in the belly of a young woman at Deal, by Mr. Jos. Nichols a surgeon there; the account of which is as follows: a woman 30 years of age fell into a periodical fever, and afterward a total suppression of her menses; which was soon followed by a pain and tumour in the right side of her belly, which grew and increased, notwithstanding all the remedies advised by the neighbouring physicians, till it became larger and harder than that of a woman in her last month. When it had grown a full year, it began to soften, and then the censorious people who suspected her honesty, thought her in a dropsy. At the end of 15 months, her belly was so distended, that it seemed ready to burst; which made the patient desire the physicians to advise Mr. Nichols to make the paracentesis; but all were surprised, when instead of water there rushed out a pint and a half of sweet well-digested matter: the next day he let out as much more, and then perceived hair 4 or 5 inches long issue forth with the matter, but so fastened in the inside, that he could not draw it out, the woman complaining he would draw out a piece of her belly. She lived but 4 days after the operation; and on opening her belly there were found 10 quarts of the same matter, which flowed through the tap hole, and in it floating a lump of hair as large as a halfpenny loaf, wrapt up in a fatty matter, from which being cleansed, it weighed full half an ounce. On the right side of the womb he found a protuberance larger than any walnut, from which the hair grew 8 inches long; that tumor, or rather the ovary, being separated from the matrix, there was found in it a perfect dog-tooth socketed in a bone of a triangular figure, in which another tooth was growing; the bone had a periosteum on it, surrounded with flesh, fastened at the calvaria to the scull.

My patient's case has two difficulties in it, which I cannot get over, viz. how these substances got in where they lodged? And how they got out thence by the way they did? Doubtless they were nested in or near the testicle; the place of tumor and pain, and the many anatomical discoveries made by those great philosophers I have quoted, demonstrate it: they could not be conveyed into that bowel, and must therefore be formed in it; but how, and of what materials, is the question? Such philosophers as call those extraordinary appearances lusus naturæ, seem like those of old, who wearied in their natural searches by some puzzling difficulty, take refuge in words, ascribing the cause of things which they cannot discover or discern, to occult qualities, &c. If they mean by lusus naturæ, the sport or recreation of nature, they accuse her who does nothing in vain, and is the author of all the order, beauty, and benefits we enjoy, as delighting to make monstrous, deformed, useless and mischievous things; things preternatural and contrary to nature, because destroying its best works, man. If by it they mean that nature, being on the work of generation, mistook, failed, or was disappointed; and instead of forming an embryo or fœtus, made a chaos, turned into a confused lump of bone, fat, hair and membranes, the materials or elements of animal bodies, they greatly err; for, in all such acts of nature, the coition of both sexes is requisite, according to the old, or either of the new hypotheses de generatione animalium, which in the girl of Deal was wanting; she being found, upon a very nice and strict scrutiny, to die a virgin, untouched.

We are told by many authors of the best credit, that great quantities of hair have been found in all the parts of human bodies, the fluids not excepted. Dr. Tyson published a large collection from them. He reasons like a philosopher, on the nature and production of air in human bodies, living or dead; especially in those parts we are writing of: but the teeth and bones seem too hard, even for so acute an investigator. He has indeed given some very fine thoughts, and ingenious conjectures, concerning their origin and production. This is the only difficulty that all those accounts I have related from others are incumbered with; but mine has another, no less hard to resolve. It is obvious how those things were got out of the women that died; but my patient, who survived the evacuation, puzzles me to find the ductus for such a lump to pass, from without the womb into the vulva. It was certainly lodged without the uterus; but which way could such a lump of greasy hair, with a bone, and a large membrane adhering, pass into it; I know none but the tuba Fallopiana; but the orifice of that into the womb is so small, that it sometimes will not admit an egg the size of a peppercorn to pass: whence those conceptions which are made in that trunk are occasioned. It will distend very largely, so as to hold a large fœtus; but where it is inserted to the matrix, the foramen is too narrow for substances of such magnitude to pass, unless some very extraordinary accident expanded it; and what that can be, I cannot apprehend.

A Lunar Eclipse, observed at Zurich, April 17, 1707. *By Dr. John James and John Scheuchzer.* N° 310, p. 2394.

At 12h. 18m. 40s. the beginning in the true shadow.—1h. 23m. 20s. the greatest obscuration.—3h. 9m. 40s. beginning of the emersion.—1h. 46m. 30s. duration of the total obscuration.—4h. 14m. 20s. end of the emersion.—3h. 55m. 50s. the total duration.—1h. 5m. 40s. from the beginning to the total immersion.—1h. 5m. 40s. from the emersion to the end of the eclipse.

An Essay on the Invention of Printing, by Mr. John Bagford; with an Account of his Collections for the same, by Mr. Humphrey Wanley, F.R.S. N° 310, p. 2397.

It is the general notion of most authors, that we had the first hint of printing from the Chinese; but I am far from being of that opinion; for at the time of the discovery we had no knowledge of them. I think we might more probably take it from the ancient Romans; as from their medals, seals, and the marks or names at the bottom of their sacrificing pots. But if it be certain that cards are as old as our king Henry VI. nothing that I have seen or considered of, seems to give so fair a hint for printing, as the making of cards; as is evident by the first specimen of printing at Haarlem, and by some books in the Bodleian library at Oxford, one in Junius's collection, another in archbishop Laud's, and a third in the same, being the lives of the Russian Saints in a thin folio; the leaves are not pasted together as the former two are, but cut on wooden blocks, and illuminated. There is also another rare specimen of the first, in that valuable collection of archbishop Parker, in Bennet college library, at Cambridge, bound up with a MS. book; which differs very much from those at Oxford; it is the Life of Christ in figures, or rather the types of the old and new Testament They have not so many specimens of the first printing at Haarlem, as we have in England.

The cutting of the blocks or molds for making our playing cards, is after the same manner as those for the books printed at Haarlem. They lay a sheet of moist or wet paper on the form or block, being first lightly brushed over with ink, made of lamp-black mixed with starch and water: they then rub it off with a round list with their hand, which is done with great expedition; this is for the picture or court cards: they then paste them together threefold, the coarsest in the middle. They colour them by the help of several patterns, or stanesiles, as they call them; they are card paper cut through with a penknife, for every colour, as red, &c. for at the first printing, the card has only a mere outline: these patterns are painted with oil colours, to keep them from wearing out with the brushes; they lay it upon the picture, and by sliding a brush, that is full and loose, gently over the pattern, it fixes the colour into the cut holes, and leaves it on the print that is to be a card; and so on through all the colours you see on cards; but this cannot be so well understood by a description, as by seeing it performed. This I conceive to have been their first way of printing at Haarlem. This hint might have been taken from MSS. 900 years old; for in them the great letters are done by the illuminators the same way as card making.

The next method of printing at Haarlem, was by cutting whole forms in wood, from MSS. exactly written, and without pictures: such I take the Donatus to be, mentioned in histories; and this might bear date in 1450, some say 1440. This may be as plainly demonstrated, as the former, from copy-books which we have seen printed at Rome, Venice, Switzerland and England, as high as 1500. This writing is harder to perform than either the Roman, Italic, or any other letters used in printed books.

The third method of printing was with single types made of wood; but to whom the honour of the invention is due, is not well known; it was then esteemed so great a rarity, that the printers carried their letters in bags at their backs, and got money at gentlemen's houses, by printing the names of the family, epitaphs, songs, and other small matters.

The fourth improvement of this noble art was the invention of single types made of metal. Here we must entirely give the honour to Peter Scheffer of Grenschen, servant and afterwards son in law to Faust, who engaged him to work in his house at Mentz: he observing how industrious his master was every day to improve this art, undertook it himself, and with much study and industry brought it to perfection. After making several essays, he at last showed it to his master Faust, who having tried some experiments with his new invented single types, finding that it would answer his expectation, was so transported with joy, that he promised him his daughter in marriage, which he some time after performed, and they continued together, improving this art with great secrecy, till it became known, and spread itself over all Europe. Sometimes you have their names at the end of the books they printed, and sometimes not; sometimes with dates as high as the year 1457, as the Psalms printed by them, now in the emperor's library, and mentioned by Lambec in his Bibliotheca; and as low as the year 1490; for which we have the authority of Erasmus, in a preface to Livy, printed at Basil by Froben, in 15.. As for John Guttenburgh, though by several authors he is said to be the first inventor of printing, we cannot find one book with his name and printing.

We may rationally conjecture, that printing with plates of pewter, brass, or iron, either engraved or eat with aquafortis, was first practised by the working goldsmiths; for they have a way of taking off the impressions of their work, by the smoke of a lamp, which perhaps gave the hint to the graving on brass: and of this we have a dark account in some authors.

The Haarlem printing at first was a book with pictures; they took off the impression with a list coiled up, as the card-makers use to this day. But when they came to use single types, they employed stronger paper, with vellum and parchment: then they made use of a press, though they afterwards contrived and made it more useful. Nor was their ink for printing brought to perfection at the first, but improved by degrees. Rolling-press printing was not used in England till king James the first, and then it was brought from Antwerp, by our industrious John Speed.

Since my second voyage to Holland, to satisfy my curiosity and remove some scruples about the book at Haarlem, and the statue of Coster, having recollected myself after my first voyage, and discoursing with Mr. Talman, junr. about Holland and the statue of Coster: he told me he had seen the same in Holland, and that it was in the Haarlemar-street in Leyden. This very much run in my mind, to be further satisfied that it should be in Leyden and not Haarlem, although asserted by several of our modern travellers.

At my last being in Holland, for my further satisfaction, though I had got Mr. Ball to make the inscription for me the year before, in June 1705, having an opportunity in the company of my good friend Walter Clavel, Esq. on Wednesday the 23d of October, 1706. We took boat for Leyden, where we arrived about six the same day, and next day in the morning, in the company of Mr. Bovell, a student, I saw at Leyden, in the Haarlemer-street, so called because it leads to Haarlem, over the door of a glazier's house, the figure of Coster cut in wood, and painted with an inscription. This statue was set up by a private person, the owner of the house, perhaps for the name and sake of the street; and I suppose is not older than about 1630. This statue is done after the graved print in the book at Haarlem, or the painting over the door of Laurence Johnson Coster, where they say he first practised the art of printing; but I rather take it, that he lived in this house in his old age, and was church-keeper, or sexton; for so the word signifies both in the German and Dutch language.

Some days after leaving Leyden, I went to Haarlem, to compare and collate the book which Mr. Bullord had procured for me, with one of the same impression. The title of the book at the latter end runs thus:

This book was finished in the good city of Culenburgh, by me John Veldener, in the year of our Lord 1483, on the Saturday after St. Matthew's day; with the device of the printer hanging on the bough or snag of a tree, a custom they much used in those days, as may be seen by ancient monuments cut on grave stones, not only in the great church at Haarlem, but also in several other cities in Holland: which device I will insert.

The title of the book, in low Dutch, (the language in which it is printed,) is, De Spiegel onser Behondenise, i. e. The Mirrour of our Salvation. On the chest where this book was kept at Haarlem, in the prince's garden, the date 1618, was inlaid in the wood. On collating it with the other we brought from Amsterdam, we found it to agree both in the words of the text, and also the pictures; they only differed in this, the Haarlem copy being in folio, with two pictures in a page, and the words column-wise, with 25 lines in a column, containing 60 pages, and printed only on one side, and not pasted together, as those at Oxford and Cambridge are. At the entrance into the garden, at the upper end of the summer-house, on the right-hand, was Coster's statue, leaning with his left-hand on the inscription, which bore date

1440; and in its right-hand the letter A in a square, with other figures, as little boys naked, and in their hands ABC, with the picture of Fame, holding the letters CD and E; which was taken from the story of Junius in his history of the low countries. There are other stories painted on the walls of the summer-house, as one of the lords of Haarlem in his armour, &c. All these pictures, with the statue of Coster, are painted in distemper, and are no older (as appears by the date on the ceiling) than 1655.

Now as printing is only another way of writing, and brought to perfection by degrees, like other arts; and as pictures, either painted, cut in wood, or engraved, were called the Laymen's books; for every one could read a picture, and say this is a house, and that a tree; so I may say, that the pictures, or drawings of the ancients, gave the first hint of printing; and if the scribes, in process of time, had not brought their art of writing into the decorum, uniformity, and rule in their several volumes, the printers could not have followed them so exactly in the imitation of their letters and pages of their books. Pictures first were those of devotion; then the making of cards was another introduction to the invention of printing: the making of cards I take to be very ancient. The first specimen of printing, was on one side only; as that at Bennet college, most in figures, with some few words only on the side in labels like that at Oxford. The next step is that book at Haarlem; the designs of the prints are better performed, and then they came to have not only lines, but whole pages of words, besides the pictures on a page. The next step was ballad-printing, with the like pictures, and that only on one side. The next improvement of this noble art, was the cutting of the whole pages on wooden blocks or moulds, and printing on both sides of the page; and the first specimen of this nature was a Donatus, and, as authors say, was printed at Haarlem and at Mentz; although some say a bible was printed the same way in 1457.

A Pyramidal Appearance in the Heavens, observed near Upminster in Essex. By the Rev. Mr. William Derham, F. R. S. N° 310, p. 2411.

On the afternoon of Thursday April 3, 1707, I perceived in the west, a quarter of an hour after sun-set, a long slender pyramidal appearance, perpendicular to the horizon. The base of this pyramid I judged to be the sun, then below the horizon. Its apex reached 15 or 20 degrees above the horizon. It was throughout of a rusty red colour; and was, when I first saw it, pretty vivid and strong; but the top-part much fainter than the bottom, near the horizon. At what time this appearance began, whether at, or how soon after sun-set, I cannot say, being at that time in a friend's house. But after a while, it grew by degrees weaker and weaker, so that in about a quarter of an hour after I first saw it, the top-part (A, B, D, in fig. 3, pl. 10,) was scarcely visible. But the lower part remained vivid much longer, but yet grew by degrees shorter and shorter. I saw the remains of the lower half (BDEF) a full hour after sun-set; and should perhaps have seen it longer, had the horizon been open; instead of which it was often in my walk obstructed by trees. The whole atmosphere seemed hazy, and full of vapours, especially towards the sun-set. The moon and stars were bearded at that time, and succeeded with a halo about the moon afterwards. Which disposition of the air was probably the cause of the phenomenon. But the pyramid was doubtless imprinted on the far distant vapours of the atmosphere; it being manifestly farther off, or laying beyond some small thin clouds (CLCL) that intercepted it, and in those parts covered and hid it. I do not remember I ever saw any thing like it, except the white pyramidal glade, which is now called the aurora borealis. And it being, except in colour and length, so like that, I have thought it may perchance in some measure conduce to the solution of that odd phenomenon, the aurora borealis.

An Experiment, confirming the Production of Light, by the Effluvia of one Glass falling on another in Motion. By Mr. Fr. Hauksbee, F. R. S. N° 310, p. 2313.

Having observed that the effluvia of glass were capable of exhibiting a phenomenon falling on an exhausted glass in motion, as if rubbed by a visible solid body; I thought a farther confirmation of the same, would not be unacceptable. For that purpose I took a large receiver, as shown by a a a a, fig. 4, pl. 10. Within the body of which I fixed another, as b b b b, their axes lying parallel to the horizon, and fixed one within the other at c c. The outer surface of the inner glass was at least an inch distant from the inner surface of the outer one; and they were turned by two large wheels, whose bands communicated with the small wheels d d d d, fixed on their axis. The inner glass was first exhausted of its air; then being fixed, as abovementioned, I moved only that wheel, which gave motion to the large glass; thinking that when the effluvia of that glass, by the application of my hand on it, should reach the other, though it was at rest, it would nevertheless be affected by it, and give a light; which happened accordingly, spreading itself in flying branches all over. Then turning the other wheel, the light became more considerable, and the greatest that has yet been produced in any experiment made on this subject. I caused both the great wheels to give motion to the glasses one and the same way, with as equal a velocity as they could; yet I did not discover but the light was then as strong, and as lasting, as when their motions were made reverse: so that I do not perceive that contrary motions any way contribute to the phenomenon; but motion itself, without being prescribed by rules, as this experiment seems to insinuate, is found absolutely necessary, as indeed the whole course of experiments on this head abundantly confirm. I further observe, that although the effluvia seemed to be equally distributed on the the outer surface of the inner moving glass, yet the light was most vigorously apparent on that side of it nearest the attrition: and when the motion either of the outer or inner glass ceased, and the other in motion, (for upon trial I found very little difference either way,) the light would continue to appear a considerable time within the exhausted glass, till the effluvia of the other could no longer act with so much vigour, as to affect the inner one. I likewise observed, that after both glasses had been for some time in motion, and the hand applied all the while on the outer one, that then the motions ceasing, and no light appearing, it was but approaching my hand near the surface of the outer glass, to produce flashes of light like lightning in the inner one, the effluvia seeming then to be more vigorously pushed upon it by the approaching hand. Now how these effluvia of glass became capable to act or perform the office of a solid body, or why such a medium is required in the inner glass to produce the light, I think are worthy the consideration of the society. For I have tried, that upon letting in a little air, the appearance of it died, nor could it then, by any means, be recovered in that state.

An Experiment made before the Royal Society at Gresham College, May 28. 1707, *showing the Difficulty of separating two Hemispheres, on injecting an Atmosphere of Air on their outward surfaces, without withdrawing the included Air. By Mr. Fr. Hauksbee, F. R. S.* N° 310, p. 2415.

Since the greatest satisfaction and demonstration that can be given for the credit of any hypothesis, is, that the experiments made to prove it, agree with it in all respects, without force: as in that of sound, the air is proved to be the proper vehicle to communicate it, not only by its lessening according to the degrees of rarefaction, but by its increasing according to the degrees of condensation. Now although the pressure of the air is evident by a number of undeniable experiments made with the air pump; yet the several phænomena of it, being liable to be accounted for the suctionists and funicularians, to proceed from some unintelligible internal cause; therefore to put the matter of fact past all dispute, I devised the following experiment.

I took a strong glass receiver, open and armed with brass hoops at top and bottom; to which parts were applied two brass plates, with wet leathers between them: but first were included two brass hemispheres, which joined on a wet leather, their diameter was 3 inches and a half. A mercurial gauge was also included. To the upper hemisphere was screwed a large brass wire, which past through a box of leathers screwed on the upper plate, and could easily be moved up and down without suffering any air to pass with it. To the upper part of this slip-wire was screwed a cock, through which the air was to be injected. In this manner the lower and upper plate were screwed strongly to the receiver; into which, after an atmosphere of air had been thrown, (which was easily discoverable by the gauge, the air in it possessing but half the space it did before,) the syringe was taken off, and an iron with an eye was screwed on in its place, by which the whole apparatus was suspended on a triangle. Then into the scale, which hung at its bottom, was put so much weight as, with its aggregate, amounted to full 140 pounds,* before the hemispheres could be parted: the friction of the slip-wire through the box of leathers was very inconsiderable.

I afterwards repeated the same experiment with the like success. And to try how it would answer in every way, I exhausted the same two hemispheres of their air, and then found that the like weight was required for their separation, as when the additional atmosphere of air was thrown on their outer surfaces, without withdrawing the included. And further to confirm the same, I not only exhausted the inner air from the hemispheres, but after that including them within the receiver, I likewise injected the same quantity of air on their outer surfaces, as in the former experiment, and then found that 280 pounds (which was double the weight before required) did not separate them. I was unwilling to add more (though I knew a small addition must have done it) fearing the breaking some of the weaker parts, which I thought were in danger by the fall of such a weight: the experiment being apparent and satisfactory without it.

Some Natural Observations in the Parishes of Kinardsey and Donington in Shropshire. By the Rev. Mr. George Plaxton. N° 310, p. 2418.

November 6, 1673, I was inducted into the parsonage of Kinnardsey, where I was incumbent upwards of 30 years; at my induction I found a great many aged people in the parish, and taking the number of the inhabitants I found that every sixth soul was 60 years of age, and upwards; some were 85, and

* This experiment yields a pressure of the atmosphere of about $14\frac{1}{4}$ pounds averdupois, on each square inch, not of the convex surface of the hemisphere, but of the flat area of its plane section, or great circle. And it shows that in such pressures, the effect is to be estimated from the flat section, and not from the convex or outer surface.

some 90; this seemed surprising, considering that the town was surrounded with a large morass, overflowed in winter, and that you could not come into the parish any way upon arable land. At my entrance there, I found neither gentleman nor beggar, nor any kind of dissenter from the church; there had been no law suit among them in the memory of man, nor was any commenced during my incumbency as rector there, for above 30 years together. The morasses or moors are of great extent, and the parish being surrounded by them, the village was thence called Kinnardsey or Kinnardus his island; ei, ea, ey, being all watery terminations: thus the next parish is called Eyton, the town upon the waters, Edney, or Edwyney, Edwin's island; there is also Buttery, or Butterey, the island of butter, being a long grazing tract of land, with some others of the like termination. All that vast morass was called the weald moor, or the wild moor, that is, the woody moor: thus the wood lands of Kent are called the weald of Kent; the wolds of Yorkshire most probably have been woody formerly, and called the wealds, for the word weald or wold is by our Saxon antiquaries rendered woody; and I have been assured by aged people, that all the wild moors were formerly so far overgrown by rubbish wood, such as alders, willows, salleys, thorns, and the like, that the inhabitants commonly hung bells about the necks of their cows, that they might the more easily find them. These moors seem to be nothing else but a composition of such sludge and refuse, as the floods left on the surface of the ground, when they drained away, and yet this sediment is full 3 or 4 feet thick; for I have often observed, that the black soil cast up by moles, or dug out of ditches, was a mere composition of roots, leaves, fibres, spray of wood, such as the water had brought and left behind it; in digging they often find roots and stumps of oaks 3 or 4 feet under the surface, and they are very common in the bottom of the ditches and drains: the soil is peaty, and cut up for fuel in some part of the lordship; in the bottom of these peat pits, are found clay, sand, and other sorts of soil. These grounds have formerly been much higher, for I have observed oaks and other trees, where the present soil is so much shrunk and settled from them, that they stand as on high stilts, being supported on the great fibres of the roots, so that sheep may easily creep under them.

That large tract, formerly called vasta regalis, is now by draining become good pasturage, and yields great quantities of hay, though much of it is of such a nature, as to dry up a new milch cow, starve a horse, and yet feed an ox surprisingly; I suppose proceeding from its dry and binding quality that makes the oxen drink much.

About half a mile from the parish church, is a pretty farm, called the Wall, which I judge was formerly a British fortification: it is encompassed with a morass, and raised up from sand, broken stones, gravel, and rubbish, to a great height and breadth, being above 1900 yards in compass, and 16, 18, and 20 yards in breadth: in some places it seems to have been built before the moors became boggy; for I could never find any way over the moors, by which they could carry those vast quantities of earth, clay, sand and rubbish, to raise that vast rampart.

As to my rectory of Donington, to which I was presented Anno 1690, I found there as many old people as I did at Kinnardsey, nay more; and in the two parishes there was only a difference of 3 in the number of the people; at Kinnardsey there were 135 souls, at Donington 138: of the 135, I had 23 aged 60 and upwards; of the 138, 24; both which numbers multiplied by 6, the one at Kinnardsey was 138, the other at Donington would have been 144. I find nothing very remarkable at Donington, except the royal oak, which stood at Boscobel; it was a fair spread thriving tree, its boughs all lined and covered with ivy; here in the thick of these boughs the King sat in the day-time with colonel Carles, and in the night lodged in Boscobel-house; so that they are strangely mistaken, who judged it an old hollow oak, whereas it was a gay and flourishing tree, surrounded with a great many more.

The people here live to great ages: I saw in one house three healthy people whose ages numbered together made 278, and I think they lived some years after; they were the man and his wife, and his wife's brother. I was at Donington about 13 years and some months; in all which time I buried only 27 people; of which number, 4 came from neighbouring parishes, 4 were young ones; and, of the remaining 19, the youngest was about 60, and the oldest 96 years of age.

An Account of the Cape of Good Hope; by Mr. John Maxwell. Communicated by the Rev. Dr. John Harris, F. R. S. N° 310, p. 2423.

The Cape of Good Hope, which is part of Monomotapa, and the southernmost part of Africa, lies in the latitude of 34° 29′ south, and 18° 23′ east of London. It was first it seems discovered by Bartholomew Diaz, A. D. 1493, under John II. king of Portugal. He gave it the name of the Cape of Tempests, because of the storms he met with there; but king John gave it the name of Bona Esperança, or of Good Hope, which it still retains; because, when that Cape was doubled, he had good hopes of finding out a way by sea to the East Indies, about which he was then very solicitous.

The Hottentots, natives of this place, are a race of men distinct both from negroes and European whites; for their hair is woolly, short and frizzled, their noses flat, and their lips thick, but yet their skin is naturally as white as ours.

Their stature is a middle size; they are clean limbed, well proportioned, and very nimble; and I never saw a fat person among them. They besmear their faces and bodies all over with grease, or other oleaginous stuff, which, with exposing their bodies to a warm sun, makes their skin of a tawny colour. They adorn their hair with shells, pieces of copper, &c. Both sexes are clad with the skin commonly of a sheep, but sometimes of such wild beasts as they happen to kill, the hairy side outwards in summer, and inward in winter, off which I have seen them pick and eat the lice in the streets: the women wear skins cut in thongs about their legs, to the length of a great many yards; which when dry, with the inside out, look so like sheep's guts, that most strangers mistake them for such. The men hang their privities in a bag, and the women cover theirs with a flap or apron made of skin. The women also wear a cap of skin only dried and stitched together; but the men commonly go bare-headed; they also go bare-footed, only when they travel they wear a piece of a skin fastened about their feet. Their weapons are javelins, with which they are very dexterous at hitting the mark, and bows with poisoned arrows, which are said to kill on drawing blood. Their houses are hemispherical, made of mats, supported with stakes, so low that a tall man cannot stand upright in them: these they remove upon occasion, as the ancient Nomades did their tents.

It is said, they are the most lazy and ignorant part of mankind; so that no kind of arts are practised among them; no ploughing or sowing, no going to sea in so much as a boat; no use of iron or money; no notion of God, providence, or of a future state; no tradition of creation, or a flood; no prayers or sacrifices; no magical rites; nor, in short, any notion of any invisble being capable of doing them either good or harm, upon the strictest enquiry that I could make. The only thing that looks like the least knowledge of any thing of this kind among them, is a custom they have, in moon-shine nights, of dancing in the fields, of which, when asked the reason, all their answer is, that it is a custom of the Hottentots, and was so of their forefathers.

All the resemblance they have of government is, that in every neighbourhood the eldest is first in order and dignity; his advice as to what concerns the whole being most followed, as having most experience. The ceremony of marriage is performed among them by the eldest person in the company sprinkling the persons to be married with his urine, upon which, and cutting out one of the man's testes, the business is over. When a woman bears twins among them, she exposes one to death by hunger or cold, and nurses the other; the reason of which last two customs is by some alleged to be the fear they have of their nation's growing too numerous: the custom of revenging, rather than punishing adultery with death, has prevailed among them. I was informed there, that they abhorred polygamy. When any person grows decrepid with age, their children, or nearest relations, shut them up in their houses, and starve them to death, they bury their dead with the skins, they wore when alive, about them. Their food is chiefly roots; their drink is milk and water; they are great lovers of tobacco and brandy, to purchase which from the Dutch, is all the use they have of money.

Notwithstanding their great ignorance, they distinguish several of the more remarkable stars by names of their own imposing: yet they have no distinction of weeks, of months, or of years, any otherwise than by their rainy seasons; so that if you ask a Hottentot how old he is, he answers, so many rains. They watch the elephants where they use to water, whom they shoot in the eye, where only they can wound them.

This country produces lions, tigers, elephants, rhinocerots, elks, leopards, wild asses, of which one sort is finely streaked with white and dark brown; several sorts of beautiful wild goats, jackals, baboons, monkeys, deer, large cows, and large sheep without horns, with hair like a goat, instead of wool, and with large tails, small horses, &c. ostriches, pelicans, hawks, magpies, wild peacocks, cranes, Guinea hens, penguins, flamingos, rock-ducks, partridges, pheasants, geese, common hens, turkeys, and ducks, &c. Here are also manatees or sea cows, and serpents of various kinds, lizards, salamanders, and porcupines. Their soil produces most sorts of fruits and plants that grow with us, as grapes, apples, quinces, olives, oranges, apricots, cherries, aloes, pompions, cabbages, &c. corn, as wheat, barley, &c. of Dutch cultivation. In short, it is fit to produce whatever is planted in it, both the soil and climate conspiring to its advantage.

The Variation of the Compass, or Magnetic Needle, in the Atlantic and Ethiopic Oceans, Anno Dom. 1706. N° 310, p. 2433.

Variation, East and West.		Latitude, North & South.		Longit. from Lond. E. & W.		Variation, East and West.		Latitude, North & South.		Longit. from Lond. West.	
8°	32′ W.	49°	18′ N.	7°	29′ W.	0°	40′ E.	3°	58′ S.	20°	27′ W.
6	42	44	31	13	45	1	2	5	9	21	39
5	30	41	6	15	8	1	30	6	21	22	8
5	4	40	22	14	54	1	50	8	3	23	15
4	22	39	11	15	35	2	10	9	7	23	35
3	30	32	21	15	39	3	32	12	3	25	3
3	35	32	42	15	38	6	4	18	53	26	30
1	20	18	50	20	52	6	19	19	51	27	2
1	14	9	26	17	59	6	20	21	26	28	14
1	10	0	49	18	42	6	30	21	48	28	10
1	00	1	9 S.	18	58	7	00	21	58	28	23
0	16	2	32	19	48	6	45	24	45	27	56
0	00	3	17	20	5	6	36	27	11	27	17

Variation.		Latitude.		Lon. from Lond.			Variation.		Latitude.		Lon. from Lond.	
5°	4′ E.	33°	53′ S.	16°	58′ W.		9°	44′ W.	29°	51′ S.	11°	44′ E.
0	00	34	21	1	29½		9	34	29	28	11	31
1	00 W.	24	15	1	33 E.		9	22	28	56	11	5
4	16	33	41	6	23		9	4	27	38	10	1
8	46	34	39	13	2	at the Cape	8	30	26	55	8	45
11	56	34	30	16	15		8	2	25	41	7	22
11	30	32	51	15	41		7	32	24	32	5	43
10	0	30	21	11	46		1	52	16	00	6	30 W. at St. Helena.

Epistola, in qua ratio redditur Libri nuper Editi, cui Titulus, De Arthritide anomala, sive Interna, Dissertatio. Auctore Gulielmo Musgrave, M.D. et R.S.S. N° 310, p. 2435.

In this work the author treats of the symptoms and cure of various morbid affections consequent to a retropulsion of the gout, or to an incomplete determination of it to the extremities. His remedies in such cases are, first emetics and gentle cathartics, then cordials, aromatics, diaphoretics, chalybeates, &c. with the application of blisters and other stimulent plasters to the joints.

An Account of a Book, entitled, The Whole Art of Husbandry. By J. M. Esq. F. R. S. N° 310, p. 2442.

This book gives an account of the several methods of inclosing land; of ordering and improving all sorts of pasture and meadow land; and of overflowing and draining march and boggy lands, &c.; of making hay, and the management of clover, sainfoin, and other French grasses; the ordering of all sorts of arable lands for all sorts of grains; the making of several sorts of ploughs, and the different ways used in several counties, for the improvements of chalky, sandy, gravelly, stony, clay, and other sorts of land. An account of the several sorts of manure used for the improvement of land; as of burning of land, of several sorts of marl, of sea sand, dung, ashes, &c. and what is proper and best for each sort of soil, &c.; of the ordering of the several sorts of corn and grain, as wheat, rye, &c. and what soil is best for each particular sort, &c.: of the several ways of preserving corn, the making of granaries, and the ordering of each sort for keeping, &c.: of the ordering of beasts, fowls, insects, and other things necessary for the stocking of a farm; &c. &c.

The whole work contains a complete collection of what the ancients and moderns have written on this subject, and near a third part of it is new experiments and improvements; giving an account of all the ways and methods used in several counties, especially about London.

De Conchyliis Turbinatis, Bivalvibus et Univalvibus, item de Mineralibus, Fossilibus, et Thermis Phillippensibus, ex MSS. R. P. Geo. Jos. Camelli. Communicavit D. Jacobus Petiver, Pharmacop. Londin. et S.R.S. N° 311, p. 2397.

An Experiment showing the Quantity of Air produced from a certain Quantity of Gunpowder fired in common Air. By Mr. F. Hauksbee, F. R. S. N° 311, p. 2409.

I took a fine glass tube about 36 inches long, the diameter of its bore about three quarters of an inch: its upper orifice had a brass ferrel soldered to a screw cemented on it, to which was screwed a cock: the lower or bottom part was naked and open, wanting the bladder formerly used: near the upper part of this tube within, was fixed a piece of cork, notched on its edges, to give the greater liberty for the explosion to vent itself; the cork had a small cavity in its middle, the better to receive and hold the gunpowder, which was let down on it, through a small glass funnel, before the cock was screwed on. In this manner the lower orifice was plunged under the surface of a vessel of water; the cock being then screwed on and open, it was easy, by sucking at it with one's mouth, to draw out the included air, by which the pressure of the external air would raise the water in it to any determinate height: the tube was previously measured by an accurate cubical inch, and graduated on its outside by a file. When the water had ascended to the designed mark, by the before-mentioned means, the cock was turned, which suspended it there: then the focus of a burning glass being cast on the powder, it soon fired, blowing the water down violently; but suddenly rising again, it rested so much below the mark it stood at before firing, as was equal to the quantity of seeming air produced from it. The quantity of gunpowder used in this experiment, was one exact grain weight; and I found the quantity of space the water had deserted, just after the explosion, was nearly equal to the bulk of a cubical inch of gunpowder, whose weight was 222 grains: so that 222 grains weight of the same powder, as soon as fired, seems to produce something to possess the space of so many cubical inches of air. Now, whether the space deserted by the water is possessed by a body of the same weight and density, or is of the same quality as common air, I dare not affirm; since an experiment I have lately made, to try how much the heat produced by the explosion of the gunpowder, might contribute to the size of the space dispossessed by the water, seems to conclude it otherwise: for I found that when the gunpowder had been fired an hour, the water had ascended about $\frac{2}{20}$ of the whole deserted space, which was in length about $2\frac{1}{2}$ inches, and was equal to about a cube inch in quantity, the space in length being divided into 20 equal parts: at two hours after firing, it had ascended near $\frac{4}{20}$ of the same. By this time I judged it might have become of an equal degree of temperature with the outer air: but still continuing the experiment, I found (to my great surprise) that two hours after the last observation, the water had reached to about $\frac{3}{20}$. Next morning, which was at about 18 hours distance, I observed that it had arrived to near $\frac{10}{20}$, or half of the first deserted space. Thus continuing rising, I found that at the end of 12 days, the water had ascended something above $\frac{17}{20}$ of the same. At 18 days it had arrived to 19 of the 20 parts at first deserted; and at that station it continued without alteration for 8 days: so that the seeming real air, produced from the fired grain weight of gunpowder, was only equal to the bulk of 11 grains of the same; that number being nearly the 20th part of 222, the number of grains contained in a cubical inch, as aforesaid. Which shows that the whole space at first deserted by the water on firing the gunpowder, was not supplied with real air.* The temperature of the air I all along considered, and found it contributed nothing to this odd phenomenon, which how to account for I know not; I only suggest, that the springs, or constituent parts of the ambient air, as well as those contained in the body of the gunpowder, may, on firing, be capable of being broken, or at least so distended, as to possess so large a space, and require so long a time to recover their natural state again. And this, I presume, could never have been discovered but by the confinement of the same air in which the explosion was made.

Notwithstanding the account of this experiment seems to run counter with the accounts formerly given of the firing of gunpowder in vacuo; yet considering the different mediums in which the experiments were made, they may be the easier reconcilable: for when the gunpowder was fired in so thin a medium as the near approach to vacuum, that then the remaining air in the receiver could suffer by the explosion, only in proportion to the quantity, which must be so inconsiderable, as not to be taken notice of.

An Experiment, showing that the Springs or constituent Parts of Air are capable of suffering such Disorder, by a violent Impulse, as to require Time to recover their Natural State. By Mr. Fr. Hauksbee, F. R. S. N° 311, p. 2412.

The foregoing experiment being so very extraordinary, gave me the curiosity to inquire a little further into the fact, and to try whether air could be capable

* Quere, was this great and continual decrease in the strength of the produced air, after it had cooled to the general temper of the surrounding bodies, owing to its absorption by the contiguous water? The like diminution did not occur in the former experiment (N° 295) made with the mercurial gauge, without the water. Other experiments of the same nature may be seen in Mr. Hauksbee's book of Physico-Mechanical experiments, where the subject is further pursued. It may also be remarked, that it is on the results of these curious experiments that the ingenious Mr. Robins founded his celebrated treatise on gunnery.

of being wounded, (if I may so call it), or to suffer such a disorder of parts, by a violent impulse, as to require time to recover their natural state: I devised the following experiment.

I took my condensing engine, and into the bottom part of its brass receiver put about half a pint of water; then the upper part being screwed strongly on, I threw into it, with the syringe, about 3 or 4 atmospheres of air (as near as I could guess), suffering it to remain in that state some time more than an hour; than letting out as much of the air (by taking off my syringe) as would readily depart, I immediately screwed on in its place a box of leather collars, through which passed a small glass tube, whose lower orifice was plunged under the surface of the included water. I found in a very little time the water had ascended in the tube near a foot, and continued rising for some time, till it had reached near 16 inches. Which plainly shows, that the springs of air, by being somewhat overbent, do not presently, although at liberty, recover their pristine state. And were they to suffer a more violent compressure, and to remain for a week, month, or a year, in the same state, I doubt not but according to the length of time, and the degrees of condensation, a proportional time would be required to recover them to their natural state again. But what is the force made use of in this experiment, in comparison to that of fired gunpowder, where the suddenness, and violence of the impulse, are so great? However, it serves well to confirm the suggestion I had, that air might so suffer in its parts by force or an unnatural extension of them, as to require time to recover their pristine natural state.

On a repetition of the same experiment, only the condensed air remained in the same state, as at first injected, for about 18 hours: then letting out the air as before, the box with its tube was screwed on; and on observation I found, that as the springs of the air unbent themselves, so they pressed more and more on the surface of the included water, which raised it higher and higher in the tube, as they approached nearer their natural state. This continued for about 6 hours, at which time the little tube was accidentally broke, and our farther observations for that time frustrated.

Concerning a Monstrous Calf, and some Things observable in Dissecting a Human Ear. By Dr. Archibald Adams of Norwich. N° 311, p. 2414.

This monstrous calf had something like wings, which seem to be bags formed out of the membranes, torn and distended from the adjacent parts, and by fresh supplies from the circulating fluids were enlarged to the size we now see them. Whether the substance contained in these bags was fibrous and muscular, or only a heap of vessels enclosed in a cystis, like the placenta, the assistant's ignorance, and the distance of time and place, it being 3 years ago, make me uncapable to acco unt for.

The bony cavity of the ear is covered at each end by a membrane; the former is called the membrane of the drum, and the other is directly opposite to it; the exterior one is stronger than the interior. They are connected together by the handle of the malleus adhering to the external membrane, and the upper part of the stirrup to the internal, which, by the intervention of the incus and the orbicular bone, form a chain, and they seem to be acted on, and to react by these small bones reciprocally.

Whether artists had any respect to this original, when they first devised drums, I cannot say; but nothing can more nearly represent the natural than the artificial does; the skins of this answering to the membranes of that, the wooden cylinder to the bony cavity; the sound of the drum would be flat without a hole in the side, and nature has given a passage from the palate to the ear. The skins of the drum would lessen the sound, if they were not kept on the stretch; so would those of the other flag, if the handle of the hammer and the stirrup did not keep them tense.

This inner membrane is closely stretched before the labyrinth, the foramen rotundum, and the passage into the cochlea, that so the sound may be the more augmented on its approach to the nerves. The stirrup is generally broken in dissecting the ear, particularly that cover which goes over the bone on each side; but if it be carefully opened, the stirrup is entirely covered with a membrane, which forms a cavity flatly oval, and the inside excavated.

Microscopical Observations of the salts of Pearls, Oyster-shells, &c. By M. Leuwenhoeck. N° 311, p. 2416.

Pearls are prescribed as a very wholesome medicine on divers occasions, to all those that are able to pay for them.* Now in order to try their supposed virtues, I took seven small ones, all which being laid in a row together, did not exceed the length of an inch. I put them on a strong charcoal fire, and made them red-hot; after which throwing them into clean rain water, they immediately burst in pieces; on which I took the pearls and put them into a glass tube, and placed the tube and pearls over a sharp fire, till they were both red-hot. On the bursting of the pearls there arose a smoke, and there was also a yellowish oil drawn from them. Those oily particles adhered to the sides of the glass in divers places, and were divided into such exceedingly small globules, that several thousands of them together did not equal a single grain of sand; but in other places the particles of oil were coagulated into much larger figures.

When the glass tube where the pearls lay began to melt, I threw those pieces

* Such an expense need never be incurred, crabs-claws or common oyster-shells being just as good for medicinal purposes.

of them that were burst with heat into clean rain water; and after several hours I poured it on a clean glass plate, that it might evaporate; which being done, I discovered abundance of salts that were coagulated in rose-like figures of several magnitudes, and all different. On another glass there were a great number of salt particles coagulated in figures like branches and boughs of trees, which was a very agreeable object.

The pieces of pearl that came out of the glass, and had been thrown into the water, were blackish; I then caused the water, in which those fragments lay, to evaporate, and put them on such a strong fire, that they turned white again; on throwing these again into clean rain water, many of these particles separated from one another, and sunk to the bottom, in appearance like white chalk. From this water a scum arose on the top, which was coagulated salts. I took a drop of water, which was very clear, from under that scum, and put it on a clean glass, and observed in the space of 2 minutes, that there was, as it were, a new scum drawn over the said water. But the next day this water was wholly evaporated, and where it had lain thickest, there was nothing to be seen but a white matter, as it appeared to the naked eye, but in reality, there was an incredible number of exceedingly small particles, which for the most part were so strongly coagulated, that there could be no particular figure discovered in them; but where the water had lain thinner, there the salt particles were coagulated in the form of boughs and branches of trees.

Some of the particles of pearls being beaten small like powder, then mixed with water, and boiled till two-thirds of the fluid were evaporated; then some drops of this fluid evaporated on clean glass, left a white-like matter to the naked eye, which the microscope showed to be very clear crystals of salt, which were very soft and tender; whereas those that resulted from the burnt pearls were quite hard and inflexible.

Now since we see, that notwithstanding the boiling of pearl powder in water, very few salts are extracted from it; yet we have reason to believe, that the stomach and bowels have a much less power over the pearl particles that are given to the sick; and as for what belongs to the salt particles, with which the water is impregnated by burning the pearls, and which coagulate in the water, like a petrefied matter, it is probable that those do rather harm than good to our bodies; and the more, as the juices in the stomach and bowels do so coagulate the salts of our meat and drink, that few or none of them mix with the blood, but are mostly discharged with the rest of the excrements; and those salt particles, which do not coagulate, we ought to consider as bad as poison, and especially those which put our bowels into such a motion, as to protrude the chyle too hastily: this is plainly seen in the sea-fishes, which though they swim in salt water, and always receive the same into their stomach and bowels; yet none of the salts mix with their blood, but coagulate in such a manner in the stomach and guts, that they assume the figure of diamonds, and pass through their bodies with the excrements. In short, we may conclude, that pearls are useless as a medicine; and I must own that I have the same opinion of gold too, though I have often heard it mightily cried up by some people, as also silver by others.

Now, since pearls are produced as it were accidentally in the shells of oysters, there is no doubt but that they have one and the same salt particles, and consequently that their operations are uniform. For my farther satisfaction, I took some very old oyster shells, that were quite dry with age, and found that they were composed of a multitude of exceedingly thin laminæ. Some of the inner fine particles being burnt and thrown into water, &c. produced a scum of coagulated salt particles, just like the pearls when treated the same way. And the like happened by boiling the pounded shells, and evaporating the water, as when the pearl powder was boiled, &c.

Concerning the Particles of Silver dissolved in Aquafortis, &c. By Signor Antonio Magliabechi. N° 311, p. 2425.

I communicated to you, some months ago, my opinion concerning diamonds, viz. that they do not grow larger by lying in the earth, but that their magnitude and figure is assumed at once, and at the very time of the coagulation or coalescence of the particles which compose them. I was the more confirmed in this, by putting silver into a glass tube, about the thickness of my finger, and length of my hand; and poured on it as much aquafortis as was sufficient to dissolve it. I put this glass tube, which was a third part filled with aquafortis impregnated with silver, into a pot filled with sand, and placed it almost horizontally, and so as that it might not stir any way, in hopes that I might the better observe, after a few days, the coagulating particles, subsiding to the bottom, all along the length of the tube. Viewing this glass tube with a microscope, I observed divers small long particles coagulated, which I judged to be particles of salt petre; for as I turned the tube a little before my eyes, and as gently as I could, I put those particles into a little motion, and thereby at once discovered three sides of them, which I imagined to be the half of those bodies, and consequently that their figure was hexangular; they appeared also as clear as crystal. I saw a few long particles, some of which were inclining to a red, others to a peach colour. I further observed exceedingly small particles, that had the figure of polished pointed diamonds; others were coagulated more irregularly.

Hereupon I took a second glass tube, and treated it the same as the former, and let it lie longer, and put a little fire under the pot that was filled with sand, that I might cause the diamond-like particles to coagulate more largely. After that, I poured the said aquafortis gently out of the glass tube, so as that the coagulated particles might remain in it; and then I turned the tube with the orifice downwards, that all the moisture might drain out of it.

I then viewed the tube through a microscope, and saw that there stuck a great number of crystalline particles on the inside of the glass, of which several were a hundred times larger than those which I had observed in the first glass; I then separated, with a small copper wire, the particles that lay together, in order to distinguish them the better, and saw with great amazement the abovementioned crystalline particles, lying together like a heap of diamonds, all of them as it were of a hexangular figure, and having each of them two sharp hexangular points; they were of several magnitudes, and in one place we saw them scattered, in another lying on a heap. In short, it would have been impossible to have disposed any real diamonds before the naked eyes, after uch a manner, as to exceed this phenomenon. I could not then discover, among these wonderful coagulated silver particles, that had assumed a crystalline nature, any saltpetre particles: I showed them to several gentlemen, who viewed them with great attention, and amazement.

Now, to be more sure that the abovementioned coagulated crystalline particles were real silver, (though I made no doubt of it myself) since saltpetre and copperas, from whence aquafortis is distilled, never produce such crystals, at least in all observations that ever I made of them; I took some of those crystalline particles, and laid them on a piece of wood-coal, and with the flame of a wax candle, which I blew upon them, put them into such a fusion, that they presently became round globules, which globules were plainly visible silver.

I caused several of these silver crystals to be neatly drawn, which are represented in pl. 10, fig. 5, 6, 7, 8, 9, 10, 11, 12.

I took again two other glass tubes, something larger than the former, into which also I poured aquafortis, and then threw in some fine silver; I then turned the glass tube upside down, and placed it in warm sand, that the aquafortis might dissolve as much silver as it was capable of. After this had stood some few days, and the upper part was become very clear, I poured the clear water off from the silver, that still lay in it, into another glass tube; and turning the orifice downwards, I kept it in that position 14 days, almost always in warm sand, to try whether the crystalline particles would not by this means coagulate larger; but I could not observe that they did; and as for the second glass tube, that fell out of the sand in the night and broke to pieces.

After this, I took a little of the aquafortis that was impregnated with silver, and having weakened it with common rain water, I put some of it on a clean glass, and spread it over the glass as thin as I could; and then put upon the said glass a small particle of red copper, no larger than a grain of sand; and presently viewed it with my microscope, and observed, that the silver particles were coagulated out of abundance of almost invisible particles in the said water; and though I viewed those particles with a glass that magnified them as much as possible, yet they were inconceivably small, that I could perceive nothing else, but that these slender particles were made up of other particles still smaller; but though I observed them ever so nicely, I could not discover their figures, even after their coagulations.

Now as we see these small crystalline particles, which are really silver, coagulated into such exact pointed hexangular figures, just as if they were so many polished diamonds; and that these figures grew larger and larger; we cannot doubt but that those crystalline small particles have the same form, even before they are obvious to our sight.

Now, let us compare the coagulated silver particles, which are all of them, as it were, changed into hexangular crystalline figures, with the pieces of rock crystal, which are likewise all hexangular, and we shall observe, that the first coagulations of the rock crystal are exceedingly small, as they are congealed out of the air; and from time to time, so long as that matter is in the air, it preserves the figure which it had in the beginning, unless it be hindered by other particles lying about it, as we may in some manner observe in the coagulated silver particles, which, though they have lain some months within the glass wherein they were coagulated, during a very rainy season, yet I could not discover the least alteration in them.

Now it seems very strange, that most of the rock crystals are hexangular, and end in a hexangular point; and though some of them are slanting and almost flat, where they are joined to the rock, yet one end or point of them is likewise hexangular; but when we see with our eyes saltpetre dissolved in water, and united with it, and afterwards coagulating therein, we discover all its exceedingly long and slender particles of a hexangular figure; excepting such as coagulating in a heap together, are irregular; and as the crystals end in a hexangular point, so the ends of these saltpetre particles run into a flattish or beetle-like figure. So we daily see in coagulated sugars, that we call sugar candy, most of their particles of a quadrilateral figure, of which two of the opposite sides are often broader then the other two, and that their ends, when they do not stick to other sugar particles, run into a sharper beetle-like figure.

In short, we see that the coagulated silver particles, appearing like crystals, are all of them hexangular, and end in two sharp points, and that the rock crystal is almost always of the same figure: and moreover, that saltpetre also coagulates into hexangular figures, with a beetle-like sharp point: but why some coagulate one way, and others another, is a thing inconceivable in my opinion, and which can no ways be accounted for.

I likewise put a little gold into aqua regia, and placed the tube, in which the said water and gold was, in warm sand, that as much gold as possible should be dissolved: but I could observe no coagulations in it; but only in some particles, branching out, the figures of which, by reason of their smallness, I could not perceive. But as to the mixed salts, of which aqua regia is composed, viz. saltpetre, vitriol, and sal ammoniac, I saw abundance of their salt particles coagulated; all which had the figures of exact square diamonds, having two sharp and two obtuse angles; they were of different magnitudes, some so small that they were hardly to be perceived with a microscope; most of them as clear as crystal, excepting some very small particles that lay upon them, which had no transparency.

Fig. 13, shows three of those diamonds, of several sizes; in which we could perceive a thickness, and the painter has described it accordingly: we saw likewise some few oblong four-sided figures, with two acute and two obtuse angles, as in fig. 14. I imagined that in the abovementioned figures there was no gold at all, because I scarcely ever discovered any such figures in the aquafortis impregnated with silver. There lay also upon, and about, the said diamonds, long crystalline figures, which I conclude were particles of saltpetre.

An Account of a Book, intitled, A Voyage to the Islands of Madeira, Barbadoes, Nieves, St. Christopher's, and Jamaica; with the Natural History of the Herbs and Trees, four-footed Beasts, Fishes, Birds, Insects, Reptiles, &c. of the last of those Islands, &c. &c. By Hans Sloane, M. D. F. R. S. 2 vol. fol. N° 311, p. 2433.

The author of this work accompanied the duke of Albemarle, as physician, to the West Indies, which gave him an opportunity of making the remarks contained in this volume; which is highly valued for the descriptions and observations in natural history.

A Letter from Mr. William Baxter to Dr. Hans Sloane, R. S. Secr. containing an Account of a Book intitled, Archæologia Britannica, giving some Account, additional to what has hitherto been published, of the Languages, Histories, and Customs of the original Inhabitants of Great Britain: from Collections and Observations in Travels through Wales, Cornwall, Bas Bretagne, Ireland and Scotland. By Edward Lhuyd, M. A. of Jesus College, Keeper of the Ashmolean Museum in Oxford. Vol. I. Containing, 1. *A Comparative Etymology; or, Remarks on the Alteration of Languages.* 2. *A Latin Celtick Dictionary: or, a Vocabulary of the original Languages of Britain and Ireland.* 3. *An Armoric Grammar.* 4. *An Armoric English Vocabulary,* 5. *Some Welsh Words omitted in Dr. Davies's Dictionary.* 6. *A Cornish Grammar.* 7. *A Catalogue of British Manuscripts.* 8. *An Essay towards a British Etymologicon.* 9. *A brief Introduction to the Irish or ancient Scottish Language.* 10. *An Irish-English Dictionary. Oxford, printed at the Theatre for the Author,* 1707. *And delivered at the Ashmolean Museum.* N° 311, p. 2438.

Microscopical Observations on the Peruvian Bark. By Mr. Anthony Van Leuwenhoeck, F. R. S. N° 312, p. 2446.

In all woods known to me, the bark proceeds out of the wood, and every year there is produced a new bark between the wood and the old one of the former year; by which means the barks of trees grow every year thicker and thicker; so that at length the extreme bark, that lies farthest from the tree, not only receives no nourishment, but even dies.

By examining, I perceive that the bark called china chinæ, or peruvian bark, consists mostly of long particles, both ends of which run into a point, some of which, at first view, one would judge to be twice or thrice as long as the rest; but examining them more nicely, I found that they were several particles sheathed, as it were, within one another, in such a manner, that without looking very closely, one would take them to be one continued particle. These particles are somewhat transparent, inclining to a yellowish colour, and almost round.

Fig. 15, pl. 10, represents a long particle, which lay the length of the wood in an oblique position, from among some of those that were near the extremity or superficies of the bark. Fig. 16 represents an exceedingly small part of the abovementioned particles, as they are cut across, by which they appear in an oval figure; and if we view them very nicely we may discover, that they are composed of screw-like parts, as seen in four of them between I and H. From this observation I supposed, that they were not at first made in an instant of time, but that they gradually receive their increase.

Whether the china chinæ be of two sorts of trees, is not now the subject of my inquiry, but in the mean time I judge by those pieces of bark which I had, that they are for the most part taken from the exterior part of the bark, which is in a manner perished, for want of enjoying any longer its nourishment from the tree; and since the smoother inner bark, which I steeped some days in brandy, would not subside, but floated therein almost equal with the surface, one would be apt to conclude, that the heaviness of the bark depended on the multiplicity of those long particles represented by fig. 15. To give a true idea of the horizontal vessels in this bark, I caused a small part of them to be drawn, as in fig. 17, which vessels lay very near the extremity or outside of the bark, and in which the painter could discover only three long particles, KM, PM, and PO.

Having infused the bark in various liquids, to examine any salts that might be obtained, I took a little of the brandy, about the quantity of 3 or 4 grains of sand, in which some of the china chinæ had been infused, but not in whole pieces, and mixed it with about a like quantity of my blood, which by the prick of a needle I had drawn out of my finger, and very quickly placed it before my microscope; and then with great amazement observed the operation of this mingled stuff, in which there was such a fermenting and running about of the parts, as is impossible to be expressed; and in these commotions I observed, that most of the globules of the blood, which are the cause of its redness, were dissolved, and I judged that this fermentation lasted about a quarter of a minute.

I also mixed my blood with some French wine, in which the bark had been infused, but discovered no such fermentation as I had observed before; but I could perceive in some few places the globules of blood coagulated after such a manner, that it appeared like a very thin membrane torn to pieces, and several very thin fibres or threads thereof lay about, such as I had never seen before; and I think I never saw so little coagulation of the globules of blood when mingled with any liquid, as I perceived in this mixture; but when the blood was dry, and where it had lain pretty thick, there it was so much coagulated, that there could be no globules any longer observed therein.

Now if we consider, that our stomachs deliver out such juices as coagulate the common salts which are in our meat and drink, and discharge them with the excrements, it is possible that many more parts of the china chinæ are dissolved in the stomach, and such a coagulation caused in the chyle, that the juices which go into our bodies, have such an affinity with the serum of the blood, as to hinder its separation, and so keep the blood in such a fluid state, that the distemper which we call a fever is thereby prevented.

Concerning the Whiteness on the Tongue in Fevers, &c. By Mr. Anthony Van Leuwenhoeck, F. R. S. N° 312, p. 2456.

In the beginning of last September I was seized by a violent fever, which however lasted only three days: on the fourth day I viewed my tongue with a magnifying looking-glass, and observed that it was all over covered with whiteness, only about a finger's breadth of the tip was of its natural colour; this whiteness is judged by most people to proceed out of the stomach or bowels, by the swelling of the guts, or else from a sharp humour out of the head. I scraped off a little with a penknife, and placed it before a microscope, when I perceived that this white matter has no analogy or agreement with that which is coagulated upon the tongue from without, but that it is certainly protruded out of the tongue, as it appeared to me very plainly, when I viewed it with my microscope; for I could then observe, that it was not only closely united to the tongue, but that it was also forced out of it, just as plants proceed from the earth: yea, that it extended itself into boughs and branches, like other plants; and I have observed such out sproutings as looked like flowers; and whereas my fever had left me about a day or two before I scraped off the white matter from my tongue, I imagined that the extreme parts of the aforesaid matter were almost worn or rubbed off when I made that observation.

There was so much to be observed in all these particles, which I had scraped from my tongue, that it was impossible for any painter to describe them; they seemed outwardly to be convex, and were as transparent as crystal, that is, at the very time I took them from my tongue and viewed them with a microscope; but when dry, they did not appear so neat; which was occasioned by the slimy or glutinous matter which we have always in our mouths, and which makes these particles cleave together. That I might free them from the glutinous matter or spittle, I put them into a little rain water, and stirred it gently about, that the said matter might be diluted and united to the water; I then took some of those particles, which by their weight had subsided to the bottom, and placed them before a microscope, when I observed with wonder, how very strongly they were fastened to my tongue when I scraped them from it, and that though I had let them lie eight days in water, they were as strong as when they were first taken off.

Hearing that a young man was so grievously troubled with the thrush, that he could scarcely draw his breath, I sent to his doctor, and desired him to let me have a little of the stuff which was taken from the tongue of the patient, which accordingly was brought to me two days after one another. This matter, which lay upon a paper, stuck so fast together, that it was very difficult to separate it, and the most part of it appeared as clear as any water to our naked eye; having viewed it through a microscope, I saw that the clear sticking moisture was encompassed with an exceedingly great number of very small globules, which appeared to be much smaller than those that make our blood red; and when this white matter was thoroughly dry, it appeared to be of a green colour, much like that matter which we discharge by the mouth when we take cold, and which is commonly called green phlegm. The doctor told me, that a day or two before, there peeled off whole skins from the tongue of his patient, and that his tongue was so very much swelled, that it filled the whole mouth. I remarked to the doctor, how much those persons were mistaken that affirm that these skins on the tongues proceed from the vapours or fumes of the stomach, in which the doctor agreed with me; but when I told him that the great thickness of the tongue was occasioned by the want of the blood circulating therein, while the heart was continually sending up fresh blood into the tongue, by which means it was forced through the tunics of the vessels, and turned to that matter which was found upon the tongue, and which we call the thrush; and whereas that matter which I found in my illness upon my tongue was nothing but the serum of the blood, the reason of it was, that the protrusion of the blood was not so strong in me as it was in the young man, neither were there any globules to be observed in it; on which the doctor agreed with me entirely in this opinion: to wit, that the matter, which was found upon the tongue, does not proceed from fumes and vapours out of the stomach, but is protruded out of the tongue; and added moreover, that when he scraped such like matter from the tongues of his patients, in half an hour's time they were covered again with the matter which we call the thrush; and further, that when the patient, being somewhat better, had scraped off the matter from the tip of the tongue somewhat too harshly, he caused his tongue to bleed; but soon after it had done bleeding, it was again covered with the thrush.

Concerning a Mineral Water at Canterbury. By Dr. Scipio des Moulins. N° 312, p. 2462.

About 12 years since a mineral water was accidentally discovered in this city. In digging the ground, they first met with a fat black mold, extending itself 3 feet deep, and gradually changing into another sort of earth, very fat and like butter. This second layer was 2 feet thick; the colour yellow, something mixed; is odour strong and mineral; and a piece of it being for some time exposed to the sun it smelled much like burning sulphur. After this they found a quick-sand of a darker colour than the first earth, mixed with several little stones, and the smell still stronger than before. Two feet further, under the quick-sand, a hard rock appeared, out of which water gushed with some violence. They dug two wells, at about 7 feet distance from each other; one about 8 or 9 feet deep from its surface, and 12 from the surface of the ground about it, and reached the rock; the other is not so deep by 2 feet, and only touches the sand. This last is somewhat stronger of the sulphur, but the other is stronger of the mineral spirit and ferruginous particles.

Two drachms of the second layer of earth, found in digging, being put into 4 oz. of spirit of vinegar, there presently arose a considerable ebullition, and soon after the spirit was tinged with a yellow brownish colour, which suffered no alteration from the infusion of logwood, nor with galls; but with oil of tartar per deliquium it turned greenish, and with the infusion of lignum nephriticum, of a pale red.

The water taken up at the spring is exceedingly clear, but becomes somewhat whitish in a quarter of an hour, and in half an hour the spirit is lost, and the mineral hangs first on the sides of the glass, and then falls gradually to the bottom. It will not keep quite so well as the Spa or Tunbridge water. Its taste is strong and austere; the smell ferruginous and strong, somewhat sulphureous; people say it smells like gunpowder. It will make the root of the tongue of the drinkers look blackish. Linen washed in it turns yellow. It will not lather with soap. Glasses dipped in the water become yellow, which no scowering can take off, and they are apt to fly. In frosty and cold weather, it is so warm as to melt ice and snow; in other seasons it is cold, though not so cold as some spring waters are.

The weight of this water varies much, according to the seasons and weather. In May 1704, it weighed 3 grains lighter than common water, in the quantity of a lb. In the spring of 1705, it was equal in weight to common water; and was still heavier in the August following, because of the exceedingly dry weather of that summer. But in general about Midsummer, if the weather be no ways extraordinary, it is pretty near to common water in weight.

A single grain weight of good galls will instantly turn a pint and a half of this water of a very deep red. Syrup of violets turns it to a grass green. With the infusion of Brasil it gives a deep lively blue: with that of lignum nephriticum, first a light green, then a light yellow, with a blue crown: with the infusion of logwood, a bluish black: with that of fustic wood, a dusky yellow: with the flowers of pomegranates, a fair violet: with tea-leaves, a fine purplish blue: with good Nantes brandy, an elegant sky-colour. It turns a solution of the saccharum saturni milky in an instant: and the solution of sublimate in some time longer. Oleum tartar per deliquium spir. sal. ammoniac, spir. vitriol, &c. make no sensible alteration.

In calm weather, especially in winter, a thick oily film covers the surface of these waters, of as great a variety of colours as a rainbow; a spoonful of it drank, has the same effect, and composes as much to sleep, as a moderate dose of opium. Some of this scum, being dried by evaporation, tasted very fat, and felt so between the fingers. Some of this powder being cast upon a red-hot iron, most of it immediately burned away with some sparkling; and what remained was of the colour of rust of iron, and tasted partly styptic and earthy and partly saltish.

The water itself, being gently evaporated, yields a yellowish sediment, more or less according to the seasons. Last spring a quart of it yielded 6 grains; but in September following, the same quantity afforded 9 grains; whereas a pound of Tunbridge water gave only 1 single grain of sediment to Mr. Boyle, as appears by his Memoirs of Mineral Waters. This sediment, being boiled in common water, made a strong lixivium, with which acids caused no sensible fermentation; but syrup of violets turned it green. This lixivium being evaporated, yielded a fat sulphureous salt, that would not coagulate into crystals. I can get only 3 or 4 grains of it out of 10 grains of sediment; but from the colour and taste of the lixivium there is reason to suspect, that there is a larger proportion of saline particles, which, as I conceive, being volatile, evaporate away with the water.

From the many surprising cures performed by it, I believe it to be one of the most excellent waters of this kind, as yet discovered in England. The little well is very serviceable in diseases of the breast, as in asthmas, coughs, rheums, and catarrhs. It has cured several desperate consumptions; most disorders of the stomach are cured by this water. It seldom fails in the cure of rheumatic gouty pains of the limbs, or other parts of the body; in the scurvy and melancholic distempers, jaundice, vapours, all sorts of stoppages, scabs, itch, &c. But in the gravel, colic, and green sickness, it is a true specific, as also in internal ulcers, if not too far gone. A certain person, who had been under the care of several physicians, and was last spring discharged out of St. Thomas's hospital as incurable, has been cured of an ulcer in his bladder, by drinking this water for 3 months together.

In agues it excels the bark: I have seen some obstinate ones, that could not be removed by the bark, perfectly cured by this water; and some constitutions, quite worn out by the frequent relapses of this distemper, restored again. This is also remarkable, that it agrees best with old, decayed, and weak constitutions.

The water sits pleasantly on the stomach, works off by urine very briskly, causes a good appetite, cheers the spirits, and procures sleep. It is not binding, as some other chalybeates are, but keeps the body open to most people, and upon some it brings now and then a gentle looseness, which carries off the distemper. For these 4 years I have prescribed these waters to many scores of people every season, and I could never observe any inconveniency or ill symptom arise from drinking them.

An Account of the Cure of two Sinuous Ulcers possessing the space of the whole Arm, with an extraordinary Supply of a Callus, which fully answers the Purposes of the Os Humeri lost in the Time of Cure. By Mr. John Fawler, Surgeon to the Sick and Wounded at Deal. N° 312, p. 2466.

One John Marsh, of the parish of Denton, in the county of Kent, about 16 years of age, was troubled with a tumour on his arm at the end of a continued fever, which seems to be a critical discharge of the humour of the fever on his arm; he was under a surgeon for 2 years; but at length, there being no appearance of a cure, he came to me. At first dressing I found two sinuous ulcers in his right arm, one upwards about the deltoid muscle, and the other on the under part of his arm, within an inch and a half of the juncture of the cubit; the upper sinus passed upwards within an inch and a half of the juncture, and downwards to the cubit. The lower sinus passed downwards to the cubit, and upwards about an inch and a half. When both these sinuses were laid open the bone soon showed itself carious and loose, so that I easily took it out; it was about 5 inches long. Three weeks after there came off another splinter of bone from the inner side, about 2 inches long, having the channel of the marrow. These ulcers, with care and diligence, were cured very well in 9 months; and the place of the bone is so well supplied by a strong callus, that the patient is not only very healthy, but can lift 50 lb. weight with that arm.

Concerning two deaf Persons, who can speak, and understand what is said to them by the Motion of the Lips. By Richard Waller, Esq. N° 312, p. 2468.

There lives in our town a man and his sister, each about 50 years old, neither of whom have the least sense of hearing; they both live by their daily labour, and both know by the motion of the lips only whatever is said to them, and will answer pertinently to the question proposed to them, of any thing within their capacity. So that you need only whisper, provided the lips and mouth be but moved as they ought, and you do not speak too fast. I was told by their mother, that they could hear very well, and speak, when they were children; but that both lost that sense afterwards, which makes them retain their speech:

though that, to persons not used to them, is a little uncouth and odd, but intelligible enough, especially the man's.

Of a Deaf and Dumb Person, who recovered his Speech and Hearing after a violent Fever; with some other Medicinal and Surgical Observations. By Mr. Martin. N° 312, p. 2469.

One Daniel Fraser, a native of Stratharig, about 6 miles from Inverness, continued deaf and dumb from his birth, till the 17th year of his age, when he was taken ill of a violent fever, but being let blood it abated, and had not its natural course; about 5 or 6 months after he had a fever again, and had no blood drawn from him, and then this proceeded with its natural course. Some weeks after his recovery, he felt a motion in his brain, which was very uneasy to him, and afterwards he began to hear, and in process of time to understand speech; this naturally disposed him to imitate others, and attempt to speak; he was not understood distinctly for some weeks, but now he is understood tolerably well.

When the small pox is epidemical in the main land over against Skie Isle, on the S. E. and east, and likewise in the isle itself, the natives bathe their children in the infusion of juniper wood, and they generally escape; whereas those who neglect this precaution, are observed often to die: of this I have seen several instances.

The plant water-lily being applied to a whitlow, it is observed that it penetrates quickly the skin of the part.

They use silverweed instead of hops in brewing beer.

An Observation of a Lunar Eclipse at Boston in New England, April 5, *in the Evening,* 1707. *By Mr. Tho. Brattle.* N° 312, p. 2471. *Translated from the Latin.*

Time correct, by altitudes.

6^h	52^m	0^s	. . The penumbra very sensible.
8	1	15	. . Total emersion.
9	46	30	. Beginning of the emersion.
10	54	0	. . End of the eclipse.

END OF VOLUME TWENTY-FIFTH OF THE ORIGINAL.

Experiments and Observations on the Motion of Sound, &c. By the Rev. Mr. Derham, Rector of Upminster, and F. R. S. N° 313, p. 2. *Vol. XXVI. Translated from the Latin.*

§ 1. *Of the Differences, in the Velocity of Sound, among Authors.*—The disagreement among the best authors concerning the velocity of sound is exhibited at one view in the following table, which shows the number of feet passed over by it, as assigned by several of those philosophers.

Sir Isaac Newton.	968 feet	Princip. Nat. Phil. l. 2, prop. 50.
The Hon. Mr. Roberts .	1300	Philos. Trans. N° 209.
The Hon. Mr. Boyle . .	1200	Essay on Languid Motion p. 24.
Mr. Walker	1338	Philos. Trans. N° 247.
Mons. Mersenne.	1474	Balistic. prop. 39.
Flamsteed and Halley. .	1142	
Florentine Academy. . . .	1148	Exper. Acad. del Ciment. p. 141.
The French Observatory	1172	Du Hamel, Hist. Acad. Reg.

There is no great difference among the last three, though there is among the others: the reason of which is plainly this, viz. either from the insufficiency of the instruments, or the different distances at which the observations were made, or because of the wind.

1. The instrument employed by some of those philosophers was not a regular movement, as a watch or clock, but a simple suspended plummet vibrating seconds. But it is plain that a common plummet is much less convenient and accurate than a proper movement; because it is necessary, first to have the eye employed in observing the flash of the gun, and next the plummet or pendulum, which takes up some time, and causes confusion; all which, together with the slowness of our senses, and that of our capacity or attention, may cause a considerable error, especially if 2, There is but a small distance between the sonorous body and the observer. Now it is manifest, that most of those celebrated authors made their experiments only at the distance of a few feet, and measured by the return of the sound or echo; for some of them scarcely extended their measures above 6 or 700 feet, and others again not above a mile. But I have always observed, that there was an uncertainty in experiments made at so small distances, even though a person should use the very best instruments; and the least error in such small distances is to be reckoned considerable; for perhaps the pendulum has run over half its arch, after the last pulsation, from the first emission of the sound; but that pulse is also brought into the account, as if the vibration had been full and complete, or possibly we anticipate a vibration, and after the sound has reached us, perhaps we reckon more or less than we

should do. Or if the distance be a sufficient length, yet an error may arise, if 3. There be not a regard had to the winds.

All these are certain, inevitable, and perpetual inconveniences, that attend the mensuration of the progressive motion of sound, which at small distances as mentioned above, especially if the instruments are bad, may occasion great errors; and doubtless, these were the chief cause of the difference among the authors. But it may be observed, how nearly the spaces in the table, assigned by the last three observers, agree, which undoubtedly is owing to their using good movements, while the ear is only employed in receiving the vibrations of the pendulum, and the eye observes the flash, or some other signal of the emission of the sound: and these observations were made at pretty large distances, so that a small error is but of little consequence. Mr. Flamsteed and Dr. Halley made their observations on Shooter's-hill, about 3 miles distant from the Royal Observatory at Greenwich, and the sound reached them in $13\frac{1}{2}$ seconds of time. The Academy del Cimento made their experiments nearly at an equal distance, and some again they made at only a mile distance. And lastly, M. Cassini, Picard, and Roemer, made theirs at the distance of 1280 French toises, which is upwards of an English mile and a half.

In order to find out the truth amidst such a variety of observations, I have made several experiments at different distances, viz. from 1 to 12 miles and upwards: and for measuring the time, employed a very accurate portable movement, with a pendulum vibrating half seconds. And, to proceed with the greater certainty, I proposed to myself to resolve the following queries: 1. How much space sound passes through in a second, or any other interval of time? 2. Whether a gun discharged towards the observer, transmit the report in the same space of time, as when discharged the contrary way? 3. Whether, in any state of the atmosphere, when the mercury either ascends or descends in the barometer, sound pass over the same space in the same interval of time? 4. Whether sound move with greater velocity in the day-time than in the night? 5. Whether a favourable wind accelerate sound, and a contrary wind retard it; and how winds affect sound? 6. Whether sound move with a greater velocity in a calm day, than when the winds blows? 7. Whether a violent wind blowing transversely accelerate or retard the motion of sound? 8. Whether sound have the same degree of motion in summer and winter, by day and by night? 9. And whether also in snowy and fair weather? 10. Whether a great and small sound have the same degree of motion? 11. Whether in all elevations of a gun, viz. from point blank to 10, 20, &c. to 90 degrees, sound reach the observer's ear in the same space of time? 12. Whether all sorts of sounds, as those of guns, bells, hammers, &c. have the same degree of motion? 13. Whether the different strength of gunpowder vary the motion of sound? 14. Whether sound pass over the same space in the same interval of time on the tops of high mountains, and in the bottom of valleys, or in the highest and lowest parts of the atmosphere? 15. Whether sound in acclivities and declivities have the same degree of motion; or whether it descend from the top to the bottom of the hill with the same velocity, as it ascends from the bottom to the top of the same? 16. Whether sound move swift in the beginning, and slower in the end, as is the case in a great many other violent motions? 17. Or whether it be not rather equable? viz. moving in half the time over half the space, in a fourth part of the time a fourth part of the space, &c. 18. Whether sound have the same degree of motion in all climates, both north and south, in England, France, Italy, Germany, &c.? 19. Whether sound pass from one place to another in a straight line, or in the shortest way, or whether it move along the superficies of the intermediate earth?

To determine all these inquiries, I caused guns to be fired from towers, and other eminences, at the distance of 1, 2, 3, to 8 miles, but the guns that served this purpose best, were those at Blackheath, called sakers, whose flashes I could see from the turret of Upminster church, and hear the report almost in all weathers, and even in the day-time I could with the telescope observe the flash.

The following experiment was made at that place, viz. two guns, called sakers, were planted near each other, with the muzzle of one turned towards me, and that of the other from me, and on February the 13th, 1705, they were fired every half hour, from 6 o'clock in the evening till midnight, a gentle wind blowing directly against the sound; the time between the flash of each explosion (which I could observe with the naked eye) and the report, was always about 120 or 122 half seconds; for the report was double, the first, which was weaker, reached in about 120 half seconds, and the other, which was stronger, in about 122 half seconds; and in the same manner there was a double report of each explosion, during the whole time of the observation.—This reduplication of the report seemed to be an echo, reflected from the mill of Blackheath, or the houses thereabouts; and of this I had no reason to doubt, only it happened to interfere with the opinion of a certain friend, who supposed that no echo could be heard but what was produced by phonocamptic or reflecting objects, not far off from the observer, and not by those near the vocal or sonorous body, or other distant objects.

§ 2. *Of a distant Echo, or the Repercussion of Sound at a Distance.*—And first I suppose, that an echo at a great distance, is not contrary to its laws; in the next place, that the reduplication of the sound came directly from Blackheath; for the first report did not come from thence, and the other as an echo from somewhere else, as beyond me, either on the right or left, or from any other part. And I have often observed the same thing, when great guns were fired on the Thames (especially if the air was clear and calm) as the watch-guns morning and evening: after the report had reached my ears, I heard it running along the river a great way, and for several miles echoing back from the banks, mountains, and rocks, that are very numerous on the Kentish coast. Now all this my friend would ascribe to the repercussion of the houses near me. But not to mention the weakness of the sound, after running over a great many miles, and its incapacity, had it come so far, of being reflected by phonocamptic objects near the observer, rather than by those near the sonorous body, I shall give an instance or two, whence it plainly appeors, that an echo caused by reflecting objects near the sonorous body, may be heard for several miles, equally as well as the primary sound, and sometimes be more intense than it. I have often observed that great guns fired in the evening on the Thames, about Deptford and Cuckold's Point, gave generally a double, triple, quadruple, &c. report, and that the latter reports were the loudest. And when I have gone to either hand, a furlong, or even a quarter or half a mile, still the sound continued the same. Several great guns were fired somewhere between Deptford and Cuckold's Point from a ship, which I saw on the Thames from Upminster church; the report was five or six times repeated in this manner, pl. 10, fig. 18. Between the flash and the report I reckoned 122 half seconds, the wind blowing transversely; therefore at that time the guns were distant from me upwards of 13 miles. The first two reports were weaker than the third, but the last was the loudest of all. When I went a quarter of a mile to the right hand, tha sound was repeated in the same manner, as also when I went to the left: and in some of my stations I plainly heard, besides the repetition of the sound, a languid echo reflected from Upminster church, or the adjoining houses; and this I frequently observed then upon firing the guns. Also, discharging a great gun somewhere on the Thames, either on this or the other side of Gravesend, the report was repeated eight, nine, or ten times at least, according to this measure of time in fig. 19. Many persons took this repetition of the report to be the firing of several guns in a sea-fight; but I supposed it to be no other than a polyphonous echo, proceeding from the explosion of one or two guns, reflected either from several adjoining ships, or from the shore; and what confirms this was, that I not only heard it when walking in the garden, but likewise several others, who were at a distance; and Mr. Barret heard the same repetition of the report at his house, about 4 miles from Upminster.

§ 3. *Of an Echo, or the Repercussion of Sound in the Air.*—When I heard the reports of great guns, especially in a calm and clear air, I often observed that a murmuring noise, high in the air, preceded them; and in a thin cloud, I often heard the firing of guns run for several miles high over head in the air, so that the murmuring noise continued for 15 seconds of time; the continuance of this murmuring noise I suppose proceeded from vaporous particles suspended in the atmosphere, which oppose the course of the undulations of sound, and reverberate them to the ears of the observer, like indefinite echos, which we call the murmuring noise in the air. On thoroughly considering these things, it will appear, that an echo, made at a distance, may be heard; and that the said reduplication of the report of the guns on Blackheath, undoubtedly proceeded from Blackheath itself, as was said above.

§ 4. *Of the Report of Guns fired in all Directions.*—What was before suggested of the report of the guns on Blackheath, I found the same to hold in all others, viz. that the motion of sound is neither swifter nor slower, whether the gun be discharged with its muzzle towards the observer, or from him; as also, that there is no variation of the sound in any position of the gun, whether horizontal or vertical, nor in any elevations, as 10, 20, &c. degrees, as the Academy del Cimento truly observed: gunpowder, whether strong or weak, and a greater or less quantity of it be used, though it may increase or diminish the sound, yet it neither accelerates nor retards its motion.

§ 5. *Of the Motion of Sound in any Weather, and at any Time of the Year.*—Kircher affirms that he always found the velocity of sound, different, at different times in the morning, at noon, in the evening, and at night: but I, having a better chronometer, and being at a more convenient distance, never found the velocity of sound different at these times: for in all weathers, whether fair and clear, or cloudy and lowering; and whether it snow or rain (both which weaken very much the audibility of sound) and whether it thunder or lighten; in hot or cold weather; by day or by night; in summer or in winter; or whether the mercury ascend or descend in the barometer; in short, in all the various states of the atmosphere (excepting only the winds) the motion of sound is neither swifter nor slower; only it is more or less clear by that variation of the medium, which perhaps, might have deceived the sagacious Kircher. Hence it follows that the conclusions drawn by Dr. Walker, from his own ingenious observations, and from those of Dr. Plot and Kircher, are erroneous.

§ 6. *Of the Motion of an intense and languid Sound, and of various sonorous Bodies.*—Though Kircher be of a different opinion, yet I doubt not, but that the sounds of all bodies, as guns, bells, hammers, &c. have the same degree of velocity; and for this end I compared the strokes of a hammer, and the report of a gun, at the distance of a mile (being the greatest at which I could hear the sound of a hammer) and found that the sound of both reached me in the same time; and that it passed over $\frac{3}{4}$, $\frac{1}{2}$, and $\frac{1}{4}$ of the same space, in $\frac{3}{4}$, $\frac{1}{2}$, and $\frac{1}{4}$ of the same time. And as to intense and languid sounds, I am also persuaded that they pass through the same space in the same time; as appears in some measure from the following experiments. At Tilbury Fort were fired a gun or two, and a great mortar, into which the powder was well rammed: the report of all these reached me, at about 3 miles off, at the same time. After sun-set also, some muskets, sakers, and mortars were discharged on Blackheath: I could not hear the muskets, either by reason of the great distance, or because the air was not clear enough; yet I heard the sakers and mortars in the same space of time, though the report of the mortars was more languid and weak than that of the sakers.

§ 7. *Of the equable Motion of Sound.*—And this I found to be the same with what the academy Del Cimento had determined; for sounds pass over half the distance in half the time, and a fourth part of the distance in a fourth part of the time, and so on; as appears from the examples in the following table:

Table of Experiments on Sound.

Places where the Explosion was made.	Number of Vibrations of the pendulum.	Distance of places, By Trigon. Miles.	By Sound. Miles.	Directions of the winds.
At Horn-church	9	0.988		transverse.
North Okendon church..	$18\frac{1}{2}$	2.004	2	transverse.
Upminster mill	$22\frac{1}{2}$	2.4	2.4	favouring.
	23		2.48	snow, trans.
Little Warley church....	$27\frac{1}{2}$	3	2.97	strong, fav.
Rainham church.........	$33\frac{1}{4}$	3.58	3.59	transverse.
Alvely mill............	33	3.58	3.57	transverse.
Dagenham church......	35	3.85	3.78	favouring.
South Weal church	45	4.59	4.86	transverse.
East Thorndon church..	$46\frac{1}{2}$	5.09	5.03	rather fav.
Barking church........	$70\frac{1}{2}$	7.7	7.62	favouring.
Blackheath............	116	12.5	12.55	transverse.

I measured very accurately, either with a rod, or by trigonometry, the distances set down in this table, of the places from Upminster, where I made my observations, the truth of which, and the goodness of my instruments, appear from the near agreement between the distances measured in that manner, and those measured by the motion of sound; so that the difference was either nothing, or but a few centesimal parts; unless when the wind was favourable, excepting at South Weal church, of which hereafter; so likewise in the observations made at the churches of Dagenham, Warley, Thorndon, and Barking, the distances, measured by the sound, seemed to be a little shorter, because the wind accelerated the sound; but in computing this column of the distances by the sound, I made no allowance for the acceleration of the winds; only divided the number of vibrations, or half seconds, by $9\frac{1}{4}$, or 9.25, the number of half seconds that sound moves in a mile. The equal motion of sound is also evident from this table, by comparing the vibration and distances; or from the single column of distances by the sound.

To confirm all this, I went to Foulney-sands, on the Essex coast, which form a large and regular plain, of several miles in length: there I measured 6 miles, and almost at the end of each mile made experiments, by firing guns; by which I found that all my former observations were very just and true; viz. that sound moves a mile in 9 half seconds and $\frac{1}{4}$, two miles in 18 and $\frac{1}{2}$, and three miles in 27 half seconds and $\frac{3}{4}$, and so on.

§ 8. *Of the Motion of Sound in Ascents and Descents, &c.*—As to the 15th and 19th queries, I confess I could not be satisfied by all the experiments made to that purpose. And first, as to the progressive motion of sound in the shortest line, quer. 19, I had reason to doubt of it, from the difference of the distance as measured trigonometrically and by the sound, between Weal and Upminster, as in the above table. The trigonometrical mensuration was taken so many different ways, and with so large angles, as to leave no doubt on that head: but because the distance seemed, by the motion of the sound, to be greater, and because the superficies of the intermediate ground is very uneven, I suspected whether the sound had not a wavy or undulated motion; or whether the intermediate hillock did not repress and retard its undulations. In order to solve this difficulty, in some measure, I caused a musket to be fired from the top of Langdon-hill, into the valley below it, at 3.79 miles off: the distance was accurately measured trigonometrically, by taking pretty large angles and a base; and when the experiment was made, a gentle wind blew somewhat against the sound: between the flash and the report I reckoned 35 half seconds and $\frac{1}{8}$; which number so well accords with the distance, and so nearly agrees with the other experiments, that there is no manner of doubt, but the sound descended through the air, in a right line from the top of the hill to the bottom of the valley, and not according to the incurvated surface of the intermediate ground: and therefore I suppose that there was some small oversight in the observations made at Weal; because I neither observed any such thing in the experiment made at Langdon, nor in any others. Whether sound move equally in ascents and descents, that is, from the top to the foot of a hill, and from the bottom to the top of the same, I despaired of ever satisfying myself; for want of hills of a sufficient height, for making a competent number of experiments to this purpose.

§ 9. *Of the Motion of Sounds in Italy.*—I was informed by Mr. Richard Townley, that sound is seldom heard at Rome, at such distances as in England, and the northern climates; particularly he affirmed, that when he was at Rome, and the guns of the castle of St. Angelo were fired, the report was much more languid on Monte Trinito, than any other place at the same distance. And Mr. Townley's brother also affirmed, that being once at the castle of Gendolfo (a place on an eminence near lake Albanus, about 12 Italian miles from Rome) the report of the great guns from the castle of St. Angelo seemed to him languid and weak; and at another time, riding in his chariot round the walls of the said castle, the report did not seem so loud there, as elsewhere. But Dr. Newton, envoy at the court of Florence, informed me, that travelling from Bologna to Florence, he heard at St. Michael's in Bosco, near Bologna, the report of the great guns of Mirandula, though 40 miles off; and the following night he heard them in the Apennine mountains, 20 miles farther off.

S. Averrani, by order of the great duke, caused a culverine to be discharged several times, in the lower castle of Florence, between the first and third hours of the night; and some were appointed at Leghorn to observe the report. Those stationed at Lanterna and Mazzoco, heard nothing of it (perhaps the roaring of the sea drowned the sound); but others, that were on the works of the old castle, called Donjon, and sent to Monte Rotondo about 5 miles from Leghorn towards Monte Nero, heard it distinctly; the distance of this castle of Florence from Monte Rotondo, is in a right line computed to be little less than 55 miles; and it is observable, that the intermediate country is hilly, which must necessarily retard somewhat the velocity of the sound: add to this, that the same evening there was a gentle westerly wind, which (Leghorn being situated to the south west in respect of Florence) must be supposed to hinder in some measure the free expansion of the sound.

But, in order to have an open free place, that tract of the sea between Leghorn and Porto Ferraio was pitched on, which, according to the calculation of the most skilful sailors, is about 60 miles; and any wind, whether with or against it, retards the sound and renders it less sonorous; the reason of which may be, that the roaring of the agitated sea mars it more, than the current of the air the same way assists it; therefore the sound is then only heard when the wind is quite still, or blows but very softly, when the air is clear, and the sea calm; nor is it then heard indifferently in all places, but only where the situation happens to be a little higher than ordinary: such are the two forts, Stella and Falcona, and a place called Mulini; besides it is requisite, that the observer be as attentive as possible, and be not disturbed with the least noise: and then it is heard in the day time, as well as in the night, provided the atmosphere be clear and calm; only that the sound seems stronger and somewhat acuter in the night, when there happens no noise, as is frequently the case in the day time. Moreover, S. Averrani was credibly informed, that at the siege of Messina, the report of the great guns reached the ears of the inhabitants of Augusta and Syracuse, at almost 100 Italian miles; and likewise when the French besieged Genoa, the firing of their guns was plainly heard as far as Monte Nero, upwards of 90 Italian miles. From these observations Averrani is apt to think, there is no difference in this matter, between Italy and the northern climates.

As to the other query, viz. whether a favourable or contrary wind, accelerate or retard the sound? The experiments Averrani had hitherto tried were insufficient to resolve it; only he gives an account of the following one, viz. a culverine was brought upon the curtain of the lower bastion of Florence, and planted with its muzzle towards Artemino, a palace of the grand duke of Tuscany, situated on a rising ground, and facing the west flank of the said bastion, from which it is also distant about 12 miles: he chose a day to make the experiment, when there was a pretty strong westerly wind, that the motion of the sound might be repelled by the contrary wind. But all this was to no purpose; for the evening was so calm, that there was hardly a breath of air: Averrani, having left in this place some persons skilful in those matters, with proper directions, retired to the palace of Artemino, and the culverine was several times discharged, between the first and third hours of the night, and each time he reckoned 49 seconds between the flash and the report; he also caused some guns to be discharged at Artemino, and between the flash and the report of each explosion the persons above-mentioned only reckoned 48 seconds: whence it appeared, that the sound moved swifter by a second from Artemino to Florence, than from Florence to Artemino; yet he could not so far depend on his observation, as to ascribe this small difference of velocity, either to the concurrence or resistance of the wind; because it might be owing to the observer, who reckoned the vibrations of the pendulum; for it must often happen, that he cannot see the flash till the pendulum begin to vibrate, nor hear the sound before the vibration is quite ended; so that his calculation is greater by one vibration, than it should be, though the space be the same.

As to the space sound passes over in any given time, it is not hitherto determined; but he conjectured from some experiments, that the case stood according to those made by the academy del Cimento. From these observations it plainly appears, that sound may be heard at a much greater distance in Italy, than Mr. Townley would have it; and I cannot help thinking, that sound is propagated as freely in southern, as in northern climates; though there are not wanting instances of the more considerable progressive motion of sound in some northern parts of the globe. For a certain Danish gentleman assures me, he heard distinctly the firing of the guns of Carlescroon, at 80 English miles distance; and Dr. Hearn informed the Royal Society, that the firing of the guns at Stockholm was heard at the distance of 30 Swedish miles, which are almost equal to 180 English; and in the naval engagement, A. D. 1672, between the English and the Dutch, the report of their guns was heard upwards of 200 miles off, even as far as Shrewsbury and Wales

As to what the two Mr. Townleys have observed, it seems to be peculiar to the castle of St. Angelo, or at least to Rome; and that diminution of sound, which they observed, might be owing either to the situation of the castle, or to the intermediate houses in that vastly large city; or to the several reflections of the sound, or to contrary winds, or in fine, to some such similar cause: or perhaps these gentlemen made the above observations, when the air was in such a state, that though the sound had the most favourable wind to concur with it, yet it was much more languid than at other times, when the wind is entirely against it: and formerly I was of opinion, that there was always the same temperament of air at Rome, though not in the other parts of Italy, till I read Kircher's opinion, viz. that at Rome, which is surprising, the echo or sound acquires great strength with a northerly wind; grows languid with a southerly wind; and is in a mean state between both with an easterly, and an east-south-east wind.

§ 10. *Of the various Remissness and Intenseness, or Audibility of Sound, according to the different States of the Atmosphere.*—I have often observed that sound had an exceedingly languid motion in summer, when the air was warm; but at other times, especially in the winter, if it happened to freeze, it was much more acute and strong. I also found that sound is more clear and shrill with a northerly or easterly wind, however contrary, than if it blow from opposite points, as Kircher likewise observed at Rome; though this is not a constant rule. Nor could I determine any thing with more certainty, as from the ascent or descent of the mercury in the barometer; for I observed the sound to be sometimes most clear and shrill, and at other times most weak and languid, when it rose the highest; and on the contrary, when it was lowest have found the sound sometimes very shrill and at other times very weak. And the case is likewise uncertain as to fair and cloudy weather: for I often observed, that in a rainy wet season the sound was impaired, and after heavy showers of rain, as Kircher found at Rome, it acquired a great deal of strength; yet the case is often quite otherwise. At Upminster, May 31, 1705, the air was clearer and freer of vapours, than ever I observed it before, so that I could easily and distinctly see objects at a considerable distance; yet I could not hear at that time the report of the great guns on Blackheath (excepting one, which was quite languid, when it reached my ears) though I plainly saw all the flashes, and at the same time the motion of the clouds and wind conspired with the sound, there being a very gentle gale abroad; and in fine, though every thing seemed to contribute both to the force and motion of the sound. But on the contrary, when the atmosphere was quite changed, and full of vapours, and all seemed louring, I often heard the sound shrill, and again as often languid, and remiss.

The causes of these variations I leave to others to determine, as also to assign the proper medium or vehicle of sound: whether it be the æthereal and more subtile part of the atmosphere, or the vapours and grosser parts thereof, or both together? But as to thick clouds, it is certain they very much weaken sound which for the most part seems to be then languid and dull; and this doubtless proceeds from the interposition of vapours, and the gross particles which form the cloud. I likewise observed the same thing in snowy weather: for when snow is newly fallen, sounds are presently weakened; but as soon as its surface is frozen, they suddenly become more acute; and I have heard the ringing of bells and firing of cannon, as distinctly as if there had fallen no snow at all. Mr. Townley affirmed he had once observed, as I had also done, that riding through a certain town, the sound of the bells that were ringing but a little way of, could hardly reach his ears, if a house covered with snow happened to interpose; so that when he entered the town, he was exceedingly surprised to find, whilst he passed by the first houses that interposed, the bells became silent of a sudden; but as soon as he came to the next vacant space, presently they were heard again. And this Mr. Townley observed all the way through that town; viz. that the sound of the bells either did, or did not reach his ears, according as the houses covered with snow happened to interpose or not.

§ 11. *Of the Influence of the Winds on the Motion of Sounds.*—The academy del Cimento found by experiments, that the motion of sound was neither retarded by contrary winds, nor accelerated by favourable ones, but that from whatever quarter the wind blew, sound passed over the same space in the same time. Gassendus, and almost all the other philosophers, were of the same opinion, though the contrary appears from experience; and they seem to have fallen into this error, from the experiments being made at too short distances; as at 1 or 2, or at most 3 miles; whereas, had they made their observations with accurate instruments, at 10 or 12 miles distance, they would easily have found out the mistake. I had (from the authority of those philosophers) given in for some time to this common error, till by the frequent observation of the firing of guns on Blackheath for three years and upwards, I was happily undeceived: at first when I observed the report reach my ears, sometimes sooner, and sometimes later, I began to suspect my accuracy, either in reckoning the vibrations of the movement, or in exactly observing the flash of the gun, or that I had, through inadvertency, fallen into some such other mistake; but after I caused guns to be fired every half hour, from 6 in the evening till midnight, and found the report always reach the ear, without any remarkable variation, in 120 or 122 half seconds of time, though the wind was directly against it; but at other times, when the wind was favourable, and blew either direct transversely, or obliquely; observing the report of the same guns reach in 111, 112, 113, 114, 115, 116, or at most in 117 half seconds of time; I was at length assured, that some real difference caused this variety in the observations. And not only do winds with or against the sound accelerate or retard its motion, but likewise according to the various degrees of their strength and weakness, is the sound more or less promoted or impeded; of which I made particular observations, as in the following table; where it is to be observed, that the guns were about 60 degrees from the south, that is, pointed something better than S.W. and by W.

A Table of the Sounds of Cannon on Blackheath, according to the variety of the Winds, and the Forces with which they blow.

Dates.	Barometer.	Winds.	Vibrations.	Dates.	Barometer.	Winds.	Vibrations.
1704.				1705.			
Feb. 13	29.99	N. E. b. E.	120	Sept. 11	Saker.	W. 2	115
—	29.99	E. 1	122	11	Mortar.	W. b. N. 2	115½
21	30.22	E. 2	119	29	29.38	S. S. W. 6	112
1705.				Oct. 6	29.34	E. S. E. 1½	117
Mar. 30	29.30	S. W. 7	113	Nov. 30	29.10	S. S. W. 4	115
April 2	. .	S. b. W. 1	114½	1706.			
3	29.80	S. 4	116½	Feb. 15	29.60	S. b. W. 1	116
5	29.70	S. W. b. W. 7	111	Nov. 29	30 06	S. W. 0	116
13	29.26	N. b. E. 2	120	—	30.06	S. W. b. S. 1	118
24	29.59	S. W. b. W. 0	126	1707.			
				Feb. 7	29.83	S. W. b. W 4	113

I have selected these observations from a great many others, which were all carefully made, each of them being repeated twice or thrice, or oftener; so that it appears from the experiments in April 5th and Sept. 29th, that the stronger winds accelerated the motion of sound; for on the 5th of April, when the motion both of the wind and sound almost conspired, and the same wind was something stronger (as the annexed number 7 shows, in the same manner that 0 denotes a calm, and 1, 2, 3, 4, &c. the different strength of the winds) the sound at that time reached the ear in 111 half seconds of time; but on April 24th, when the wind blew from the same point, and the air was calm, the sound only moved through the same space in 116 half seconds; so likewise on Feb. 7, 1706, the wind blowing on the same point, and carrying the sound along with it, but with only half the force, it passed over the same space in 113 half seconds; and in fine, on Sept. 29, 1705, the wind being strong and less favourable, the sound moved through the same space in 112 half seconds: from which, and other examples in the table, it plainly appears, that a strong wind accelerates sound, while a weaker wind does not. The same may likewise be said of those winds, or currents of air, which either directly favour or oppose the progressive motion of sound; viz. that they either accelerate or retard it; and that intermediate currents also cause intermediate progressions of sound, or number of vibrations of the pendulum. The greatest difference I have observed in the motion of sound in the space of about 13 miles, was about 9 or 10 half seconds, when a strong wind promotes, and only a gentle wind impedes it; but when only a gentle wind, or almost none at all, opposes or favours the sound, the difference hardly exceeds 2 or 3 half seconds.

§ 12. *Of the Velocity of Winds.*—To discover the quantity of space that winds pass over in any given time, I took some light bodies, such as down, &c. and from the several experiments I made with these, when the strength of the wind was different, found that the strongest wind scarcely passed over 60 miles in an hour; for instance, on August 11, 1705, the violence of the wind was such, as almost to beat down a wind-mill, near the place where I made my observations; and estimating by the numeral characters, 0, 1, 2, 3, 4, 5, 6, to 10, 15, or more degrees, the strength of winds, the strength of this I reckoned at about 12 or 14 of these degrees; and from repeated experiments I found, that that hurricane passed over about 33 feet in a half second of time, or 45 miles in an hour; whence I conclude, that the most violent storms, not excepting that in Nov. 1703, do not move about 50 or 60 miles in an hour.

Having thus determined the velocity of rapid winds, we may from hence more easily conjecture the velocity of such as are less so; and I have found from several experiments, that some of them move 15 miles, others 13, some more and some fewer, in an hour; and that others again have so slow a motion, as scarcely to pass over one mile in an hour: also some are so slow, that a person, walking or riding, may easily outrun them, as is apparent to sense; for as often as we stop our pace, we perceive a gentle gale softly fanning us; but if we walk with the same pace, that the wind moves, we hardly feel any at all; if faster, we find that, instead of accompanying and blowing with us, it blows in our faces: so that when the atmosphere is quite calm, if we are walking or riding, we perceive a gentle gale, corresponding to the motion we are in; and the gale of wind, or flux of air, moves with the same degree of velocity, we find it affect us, while we stand still.

From these observations on the velocity of winds, we may make many useful remarks; especially, give one good reason, why the mercury ascends and descends so long before fair weather, or rain. Since sound is swifter than wind, it is plain, that those parts of the atmosphere, whereon it is impressed or conveyed, are not the same with those of which wind consists, but some other more æthereal and volatile particles; for the swiftest wind does not move above 60 miles in an hour, whereas sound moves in the same time upwards of 700 miles. But if it should be objected, that winds either accelerate or retard the motion of sound; it may be answered, that this does not proceed from the proper flux or tendency of the windy particles alone, but rather from the joint and concurring motion of both the gross and æthereal particles of the atmosphere; which course or direction of motion, should it favour the undulations of sound, its appulse, it is very probable, would thereby be accelerated, but contrary thereto, it would be retarded.

§ 13. *On the Velocity of Sounds.*—From what has been said above, I firmly conclude, that sound is propagated with this degree of velocity, viz. that in 9 half seconds and $\frac{1}{4}$ it moves the space of a mile, or 5280 English feet; or, which is the same thing, 571 feet in a half second of time, or 1142 feet in a whole second. Thus, sound moves through the above space, if the flux of the atmosphere or wind be transverse or across, and is its mean motion; but should the wind increase the rapidity of sound, it is impossible that it may move upwards of 600 feet in a half second of time; or, on the contrary, should it retard sound, it may move not above 560 feet in the same time.

All the above-mentioned observations and experiments may be useful, 1. To the philosopher, for investigating the nature of sound, and explaining its abstruse phænomena. 2. To the sailor, for discovering the distances of ships, either under sail or at anchor, and of land seen a great way off; all which he may know with ease and certainty, by the firing of guns. 3. To the soldier, for finding the distance of an enemy's camp, and that of a city, castle, or arsenal, &c. that is to be besieged, in order to place his batteries, and direct his mortars and shells. 4. To the geographer, for measuring with the greater ease and certainty the distances of places; for any one furnished with a large quantity of gunpowder, may by this means, in an hour or two, make a very accurate map of almost a whole country: the report of guns will show, as has been said, the distances of the places; and any mathematical instrument that measures angles, as the plane table, &c. will give their situation, so that they may afterwards be easily delineated; and in like manner, by this means may any one readily discover the justness and accuracy of maps, and correct their errors: in short, this method of observation may be of singular service in measuring inaccessible distances, especially very broad rivers, &c. that cannot be otherwise measured; as also for finding the breadth of bays and straits.

5. To the echometrician; though several learned men, both ancient and modern, have carefully examined into that ludicrous and agreeable phenomenon of sound, called echo, yet they are not well agreed in a great many points relating to it; especially as to the space necessary for the repetition of 1, 2, 3, or more syllables, or, which is the same thing, the space an echo moves through in any given time; Mersenne allows . . . . . paces, Blancanus 24 paces, with whom agrees Dr. Plot; but Kircher asserts, that nothing certain can at all be determined therein; because the variation of the winds, the intenseness and remissness of sound, and a great many other circumstances may cause a great difference. It is an easy matter to assign the reason of the variety of these observations: as, the slowness and various disposition of our senses, the different audibility of sound, the grave or acute sound of the syllables themselves, or their length or shortness, or some other cause, that protracts the time of their pronunciation; for I am persuaded, that though any reflecting object were capable of returning all the syllables of the following verse; vocali nymphæ, qua nec reticere loquenti, yet it could not reflect all the syllables of this other, because its pronunciation is a little longer; corpus adhuc echo, non vox erat, et tamen usum; and much less repeat all the rough and long syllables of the following verse, though fewer in number; viz.

Arx, tridens, rostris, sphinx, præster, torrida, seps, strix.

But from the abovementioned observations on the motion of sound, we may conclude, that as sounds, so do echos in like manner move through certain and determinate spaces in given times, as I have often found by experience; viz. that the echo returns in double the time wherein the primary voice arrives at the reflecting object: for instance should the phonocamptic, or reflecting object, be at the distance of a furlong, the return of the echo will be made in the same time, that the primary sound would move through two furlongs, if it were not reflected: and this I have often found to be very serviceable in measuring distances: for example, standing on the banks of the Thames, opposite to Woolwich, the echo of a monosyllable was reflected from the opposite houses in 6 half seconds of time; hence I concluded the breadth of the river in that place, from one side to the other, to be 1712 English feet, or upwards of $\frac{1}{4}$ of a mile; for as 9.25 half seconds : to 5280 (the number of feet in a mile) :: 6 half seconds : to 3424.8 feet, the half of which is 1712.4 feet.

6. Lastly, the height of thunder-clouds, as also the distance of the thunder and lightning itself, may by this means be easily known.

Account of the strange Effects of Thunder and Lightning. By Samuel Molyneux, Esq. S. Phil. Soc. Dublin. N° 313, p. 36.

Mrs. Close gave Mr. Molyneux the following account of the effects of the thunder and lightning, on her house at New Forge, in the county of Down, in Ireland, on Aug. 9, 1707: she observed, that the whole day was close, hot, and sultry, with little or no wind stirring, till towards the evening; that there was a small breeze with some mizzling rain, which lasted about an hour; that as the air darkened after sun-set, she saw several faint flashes of lightning, and heard some thunder claps, as at a distance; that between 10 and 11 o'clock both were very violent and terrible, and so increased, and came on more frequently until a little before 12 o'clock; that one flash of lightning and clap of thunder came both at the same time, louder and more dreadful than all the rest, which, as she thought, shook and inflamed the whole house; and being sensible at that instant of a violent strong sulphureous smell in her chamber, and feeling a thick gross dust falling on her hands and face as she lay in bed, she concluded that part of her house was thrown down by the thunder, or set on fire by the lightning; that arising in this fright, she called up her family, and candles being lighted, she found her bed-chamber, and the kitchen beneath it, full of smoke and dust; and the looking-glass in her chamber was broken.

The next day she found that part of the cornish of the chimney, which stood without that gabel-end of the house, where her chamber was, had been struck off; that part of the coping of the splay of the gabel-end itself was broken down, and 12 or 16 of the shingles on the adjoining roof were raised or ruffled, but none shattered or carried away; that part of the ceiling in her chamber, beneath those shingles, was forced down, and part of the plaster and pinning stones of the adjoining wall, was also broken off and loosened, the whole breach being 16 or 20 inches broad: that at this place there was left on the wall a smutted scar or trace, as if blacked by the smoke of a candle, which pointed downwards towards another place on the same wall, where a like breach was made, partly behind the place of the looking glass; that the boards on the back of a large hair trunk, full of linen, standing beneath the looking-glass, were forced in, and splintered as if by the blow of a smith's sledge; that two thirds of the linen within this trunk were pierced or cut through, the cut appearing of a quadrangular figure, and between two or three inches over; that one end of the trunk was forced out, as the back was driven in; that at about two feet distance from the end of the trunk, where the floor and the side-wall of the house joined, there was a small breach made in the plaster, where a small chink or crevice was to be seen between the side board of the floor and the wall, so wide that a man could thrust his fingers down; and that just beneath this in the kitchen the ceiling was forced down, and some of the plaster of the wall broken off; that exactly under this there stood a large tub or vessel of wood, inclosed with a crib of brick and lime, which was broken and splintered all to pieces, and most of the brick and lime-work about it scattered about the kitchen.

I observed that the looking-glass was broken with such violence, that there was not a piece of it to be found of the size of a half crown; that several pieces of it were sticking like hail-shot in the chamber door, which was oak, and on the other side of the room; that several of the edges and corners of some of the pieces of the broken glass were tinged of a light flame colour, as if heated in the fire; that the curtains of the bed were cut in several pieces, supposed to be done by the pieces of the glass; that several pieces of muslin and wearing linen, left on the large hair trunk, were thrown about the room, no way singed or scorched; and yet the hair on the back of the trunk where the breach was made, was singed; that the uppermost part of the linen within the trunk was not touched, and the lowermost parcel, consisting of more than 350 ply of linen, was pierced through, of which none was anywise smutted, except the uppermost ply of a tablecloth that lay over all the rest; that there was a yellow singe or stain, perceivable on some part of the damaged linen, and that the whole smelt strongly of sulphur; that the glass of two windows in the bed-chamber above, and two windows in the kitchen below, was so shattered, that there was scarcely one whole pane left in any of them; that the pewter, brass, and iron furniture in the kitchen were thrown down, and scattered about, particularly a large girdle about 20 pounds weight, that hung upon an iron hook near the ceiling, was found lying on the floor; that a cat was found dead next morning in the kitchen, with its legs extended as in a moving posture, with no other sign of being hurt, than that the fur was singed a little about the rump.

It was further remarkable, that the wall, both above and below a little window in the same gabel-end, was so shattered, that the light could be seen through the crevices in the wall; and that upon a large stone on the outside of the wall, beneath this window, was a mark, as if made by the stroke of a smith's sledge, and a splinter of the stone was broken off, of some pounds weight. I was further informed, that from the time of that great thunder-clap, both the thunder and lightning diminished gradually, so that in an hour's time all was still and quiet again.

Concerning an apoplectic Person, in whom, from an Obstruction in the left Ventricle of the Brain, the Nerves on the opposite Side were affected. By Dr. Archibald Adams. N° 313, p. 40.

Some time ago I opened the head of a woman, who died of an apoplexy, and in the left ventricle of the brain I found between 4 and 5 ounces of clotted blood, and no blood at all in the right ventricle, but every thing as usual; and all the nerves on the right side of the body were as strong as any I ever observed in a sound animal, especially in their origin, and as far as I could trace them in their course. It had been my opinion, that whichsoever ventricle the obstruction was in, the nerves and muscles corresponding to that side would be affected; but here the contrary plainly appeared; for though the obstruction was in the left ventricle, yet the sense and motion of the right side were entirely lost, and the small remains of either were observable only in the left side.

Concerning a Woman 62 Years of Age (who is still living, in 1707) that lost her Leg and greatest Part of her Thigh by a Gangrene. By Mr. Ralph Calep. With a Remark by Mr. Cowper. N° 313, p. 41.

The wife of one Tho. Steven of Maidenhead, Berks, about 62 years of age, was seized with a fever about the latter end of Nov. 1697. Her physician used various remedies to remove the fever, which in about 14 days terminated in a tumor and numbness in her left foot, both which by degrees crept half way up her thigh. A fomentation was ordered, of centaur. absinth. hyperic. &c. boiled in a strong lixivium; and after fomenting, to anoint her foot and leg with ol. terebinth. in which galbanum was dissolved. This method was daily used for a month, before I saw her, when I found her in the following condition; her foot and leg cold, insensible, withered, and hard, as if dried in a chimney, and of a tawny colour. Her knee was swelled, and had several large black spots on it, which pitted when pressed with the finger. There were several discolorations in the skin, half way up her thigh. She complained of great pains, especially at nights, in her knee and thigh, yet could not feel when I touched those parts. Her fever was now increased again, and she was delirious at times. I advised her friends to continue the use of the fomentation, which they did almost night and day. About a month after there was a discharge of a black fetid matter, at a small orifice about the middle of the inside of her thigh, which I enlarged to obtain a better discharge. I likewise cut into a tumor that appeared on her knee, but found in it only wind. About a month or 5 weeks afterwards, to my great surprise, I saw that nature had made a perfect separation of the mortified flesh from the sound, quite round the thigh, the bone of the thigh lying quite bare for more than 4 fingers breadth, and without its periosteum. The flesh above was fresh and florid, and had good white matter upon it. I now persuaded her to let me take off her thigh, which I did about 2 fingers breadth in the sound flesh, (because the flesh ran tapering down to the bone) by which I made the stump pretty even. The bleeding was little, because that the veins and arteries, which were eaten asunder by the mortifying matter, nature had closed again. I dressed the stump with pulv. restring. mixed with album. ovor. spread upon pledgets, and dipped in ol. terebinth. made hot. For the next dressings I used digestives, and performed the rest of the cure according to the usual rules. The woman is now alive. (Aug. 23, 1707.)

*** *Remark on the above Case by Mr. Cowper.*—About the beginning of October 1707, I saw the woman whose case is here related: she appeared to be very decrepid, and would have shown me the stump of her thigh bare, but the coldness of the weather, she said, would make it uneasy to her. I felt it through her clothes, and the end of the stump seemed to be not above 4 or 5 inches below the trunk of her body.

Since I have so often found the large trunks of the arteries of the thighs and legs of aged people putrified, as I have mentioned in the Transactions, N° 280, and most commonly in those who have had gangrenes in the legs, &c. I am apt to suspect the like happened in the crural artery of this woman; which, like a ligature, did at length put a total stop to the influent blood below that stricture.

The Manner of making Styrax Liquida, or Rosa Mallas. By Mr. James Petiver, F. R. S. N° 313, p. 44.

Rosa mallas grows in the island Cobross, at the upper end of the Red Sea, near Cadess, which is 3 days journey from Suez: it is the bark of a tree† (taken off every year, and grows again) boiled in salt water, till it comes to a consistence like bird-lime; then separated, and put into a cask, and brought to Judda, and so to Mocha in June and July, where it sells from 60 to 120 dollars

† Of the liquidambar styraciflua Linn. The styrax calamita, or common storax, is obtained from a very different tree.

per barrel, according to its goodness: the best sort is that which is freest from clay and dirt, which are commonly mixed with it: and the way of trying it, is by washing it in salt water, which will cleanse it. The Arabs and Turks call it cotter mija. N. B. A barrel is 420lb.

The Practice of Purging and Vomiting Medicines. Inscribed to Dr. Garth, F. R. S. By W. Cockburn, M. D.* N° 314, p. 46.

By my solution of the problem (Phil. Trans. N° 303) for determining the proper doses of purging and vomiting medicines, in all their cases, it is manifest in general, 1. That these medicines operate either by mixing with the blood, or by stimulating the stomach and guts. 2. That their operation is more or less, according to the quantity and thickness of blood; that is, a greater quantity, and the thickest blood require the greatest doses: and 3. That when the quantities of blood are the same, the doses of purging and vomiting medicines are in a duplicate ratio of the thickness of the blood: as also, that in every case, these doses must be in a proportion compounded of the quantity of blood and the squares of its thickness.

Now, since the operations of purgative and vomitive medicines depend so much on the quantity and viscidity of the blood, which have not hitherto been duly considered; it is no wonder that the practice of physic, in these evacuations, has been so uncertain, and that the most expert physicians, from their most accurate observations, could never determine the true doses of medicines, which alter so much according to the various subjects they work upon; as they are not acquainted with the true method of determining either the quantity of the blood, or the degrees of its thickness.

Now, as experience is equally the foundation and touchstone of all reasoning in physic, we will here submit the solution to common observations; and try whether every thing, proposed in it, does not exactly answer matters of fact, and the visible operations of nature. First then, it plainly follows, that these medicines always purge best, and most constantly, in a liquid form; because they are more easily conveyed into the blood; and can stimulate more parts; whatever may be the way that purges and vomits work, or whatever their

* The name of Dr. Samuel Garth has been perpetuated by a satirical poem, entitled the Dispensary, written (as one of his biographers has remarked) in defence "of charity against the intrigues of interest, and of regular learning against licentious usurpation of medical authority." He was a native of Yorkshire, and took his degree of M. D. at Cambridge in 1691; after which he removed to London, where he soon got into great practice; particularly among the Whig party, of which he was a zealous supporter. On the accession of George I. he received the honour of knighthood, and was appointed physician to the king, and physician general to the army. He died in 1718.

nature may be. This explains very easily a common observation, hitherto very difficult to physicians, about the different operation of the same medicine in different forms: viz. Why the infusion of a due quantity of purging medicine produces its effects sooner and more constantly, than a like quantity of the same medicine in a powder; though still more constantly in a powder than a bolus; and sooner and more constantly in a dry bolus, than in pills made into that form with gums that do not purge; and this difference in purging shall even be considerable, according to the solubility of the gums. From whence it follows, that the evacuation made by such medicines, is in proportion to the quantity of those medicines that happens to be dissolved, and not to the quantity administered. Secondly, that purging by draughts is the best form, and will always have the most constant effect.

The next consideration is, that a certain quantity of any purging medicine affects us after a different manner, according to the different quantity and constitution of the blood, or its thickness; and it was shown n the solution of the problem; that if its thickness were the same, the dose should always be as its quantity; but the blood also differing in thickness, the doses must be augmented on account of its thickness. This is confirmed by daily experience; where we find, that people sick with a manifest thickness of blood, as in dropsies, the jaundice, &c. take far greater doses than they did at any other time when they were not sick, or ailing in that manner.

By a further disquisition into this matter, we find that the doses must not only be greater where the thickness of blood is greater; but that they must be increased in a duplicate ratio of their viscidity. This is evident by the tables in Cassia, viz. 9 : 8 ʒ :: 4 : 3 ʒ, 1 ℈, $13\frac{1}{3}$ gr. Therefore the doses are as the squares of the constitutions. So likewise 9 : 8 ʒ :: 16 : 14 ʒ, $13\frac{1}{3}$ gr. and alternando 9 : 16 :: 8 ʒ : 14 ʒ, $13\frac{1}{3}$ gr. h. e. the doses are as the squares of the constitutions. The same holds true in any other constitution, besides the mean; for example, in the lowest and highest 4 : 16 :: $213\frac{1}{3}$ gr. $853\frac{1}{3}$. So that by this means we are not only led directly to a right use of these medicines, and are able to find the true cause why the ordinary doses produce so very different effects in different constitutions; but we also have the solution of this problem, viz. "The quantity of blood in any person being given, together with the ordinary and extraordinary effect of a dose of a purging medicine, the change of that person's constitution, and the nature of that change may be determined."

It cannot but be a great satisfaction to the mind, to find a doctrine founded on a few simple experiences, leading us into the cause of many more that are very complex, difficult, and obscure; which is sufficient to prove its conformity to nature. But my present endeavour being to rectify the common practice of these medicines by this doctrine, I shall frame, by this method, tables of the purging and vomiting medicines in present use; better adapted to experience than are hitherto to be found. The method of framing such tables, is by setting off the practicable constitutions in the different ages, that I have observed to take notable quantities of purging and vomiting medicines; so that by comparing these constitutions with the ages, we have the different doses in all those cases, which is all that is required for a better practice.

The ages wherein these different doses are taken, I find to be four: when a man is about 16 or 20 years of age, and weighs about 12 stone, he then takes the common dose; one of 9 years of age takes three quarters of that; one of 6 years the half, and one of 3 years a quarter. Moreover, it having already been shown, that the notable healthy constitutions are but three, as also the notable pulses of each of these; let then these constitutions be as 2, 3, 4; that of the most fluid blood as the first number, and so on; in that case the dose proper for any person will be found by multiplying the common dose for his age into the square of his constitution, and dividing by the square of the middle constitution.* For instance, if ℥j. cassiæ be the common dose, or the dose of the middle constitution, then ʒiij. ℈j. and gr. 13$\frac{1}{3}$ is the dose of the first constitution; and ʒxiv. gr. 13$\frac{1}{3}$ that of the grossest or last constitution; and so proportionably for every medicine in all the ages, as appears by the tables.

This method seems to answer so exactly, that nothing more seems necessary, except a person is more loose or costive than ordinary, which is to be reputed the same as if he had taken an equivalent quantity of a medicine proper to produce these effects. Any physician, who has considered this case in some people after fluxing, will allow the justness of this exception.

As vomiting medicines have the same common doses with those that purge, they admit also of the like divided doses, which therefore may be found by the same tables. Only, as people that are more costive than ordinary require a proportionably greater dose of a purging medicine, so they require their dose of a vomiting medicine to be considerably less, as is very well known in hot countries. It must be observed, that in the tables, age stands instead of quantity of blood, because they increase pretty equally, and it makes the practice more easy to such as are not accustomed to weights and numbers. And the more skilful are to observe, that the mean ages multiplied into the mean constitutions give doses more nicely.

* It has been already observed at p. 250 of this vol. of the Abridgments, that this mode of reasoning is not applicable to the living body.

Tables showing the Doses of Purging and Vomiting Medicines, according to the Solution of Dr. Cockburn's Problem.

	Medicines.	Ages.	Constitutions.	Doses. ʒ.	℈.	Gr.
Medicines whose common dose is ʒj.	Cassia	16	2	3	1	13⅓
	Catholicon		3	8	0	0
	Diacarthamum		4	14	0	13⅓
	Elect. lenitivum					
	Succus radic ir.					
	—— Fumariæ	9	2	2	2	0
	Syrup. derham.		3	6	0	0
	—— de pomis		4	10	2	0
	—— Rosar. cum					
	Helleb. tamar.	6	2	1	2	6⅔
	Sal cath. amar.		3	4	0	0
	—— Mirabile		4	7	0	6⅔
	Mann. ʒij.					
	Emetica.	3	2	0	2	13⅓
	Vin. emetic. seu		3	2	0	0
	Ben. suc. asar.		4	3	1	13⅓
	Senecionis					
common dose is ʒss.		16	2	1	2	6⅔
			3	4	0	0
			4	7	0	6⅔
	Confectio ham.	9	2	1	1	0
	Elect. caryocost.		3	3	0	0
	Diaphœnicon		4	5	1	0
	E succo rosar.	6	2	0	2	13⅓
	Emet.		3	2	0	0
	Syrup. emeticus.		4	3	1	13⅓
		3	2	0	1	6⅔
			3	1	0	0
			4	1	2	6⅔

	Medicines.	Ages.	Const.	Doses. ʒ.	℈.	Gr.
Common dose ʒj.	Agaricus					
	Aloe	16	2	0	1	6⅔
	Carthamus		3	1	0	0
	Ebuli sem.		4	1	2	6⅔
	—— Cortex					
	Hermodactylus					
	Mechoacanna	9	2	0	1	0
	Pil. aggregat.		3	0	2	5
	— Coch. major		4	1	1	0
	— Fætidæ	6	2	0	0	13⅓
	— sine quibus		3	0	1	10
	Pulv. diasennæ		4	0	2	13⅓
	Rhabarbarum					
	Soldanella	3	2	0	0	6⅔
	Senna		3	0	0	15
	Turbith		4	0	1	6⅔
common dose ʒss.	Jalappa	16	2	0	0	13⅓
	Nuc juglandis		3	0	1	10
	Lap. lazuli		4	0	2	13⅓
	— Armenius	9	2	0	0	10
	Opoponax		3	0	1	2½
	Pil. Coch. minor.		4	0	2	0
	— de Gut.	6	2	0	0	6⅔
	Gamandrâ		3	0	0	15
	— Rudii		4	0	1	6⅔
	Pul. warvicens,					
	Sagapenum	3	2	0	0	3⅓
	Emet.		3	0	0	7½
com. d. ℈j.	Rad. ipecacuanæ		4	0	0	13⅓
	Resina jalappæ	16	2	0	0	$8\frac{8}{9}$
	Extr. rhabarbari		3	0	1	0
			4	0	1	$15\frac{5}{9}$

	Medicines.	Ages.	Const.	Doses. ʒ.	℈.	Gr.
	Pil. de Hermodactylis ℈ij.	9	2	0	0	6⅔
			3	0	1	15
	Pulv. cornach.		4	0	1	6⅔
	℈ij.	6	2	0	0	$4\frac{4}{9}$
	Emet.		3	0	0	10
	Gilla vitrioli		4	0	1	$17\frac{7}{9}$
		3	2	0	0	2
			3	0	0	5
			4	0	0	$8\frac{8}{9}$
Common dose gr. vj.	Colocynthis	16	2	0	0	2⅔
	Euphorbium		3	0	0	6
	Esulæ cortex		4	0	0	10⅔
	Elaterium	9	2	0	0	2
	Gum. guttæ		3	0	0	4½
	Gran. Gnid.		4	0	0	8
	Ricini sem.	6	2	0	0	1⅓
	Scammon.		3	0	0	3
	Tr. Alhand.		4	0	0	5⅓
	Emet.	3	2	0	0	0⅔
	Croc. rulandi		3	0	0	1½
	Turbith miner.		4	0	0	2⅔
common dose gr. iii.	Emet.	16	2	0	0	1⅓
	Hep. antimonii		3	0	0	3
	Vitr. antim.		4	0	0	5⅓
	Merc. vitæ	9	2	0	0	1
	Tart. emet.		3	0	0	2¼
	Merc. præc.		4	0	0	4
	per se	6	2	0	0	0⅔
	—— solar.		3	0	0	1½
	—— ruber		4	0	0	2⅔
	Hercules bovii	3	2	0	0	0⅓
			3	0	0	0¾
			4	0	0	1⅓

The doses of these tables, arising from calculation, agree perfectly well with the common observation of the best authors, though their observation is very general and ill made, if we except the very first steps.

Microscopical Observations on the Blood-vessels and Membranes of the Intestines. By Mr. Anthony Van Leuwenhoeck, F. R. S. N° 314, p. 53.

Viewing with the microscope a small piece of the gut of a woman, I discovered in one of the thin membranes, of which the gut is mostly composed, a great number of little fibres and vessels, which lay in multitudes over aud across each other, as also some particles of fat, which lay like bunches of grapes upon the said fibres. It is impossible to judge whether these vessels are arteries, veins, lacteal or lymphatic vessels; for though there are divers arteries and veins in such a thin membrane, and though there were blood in them, yet cannot that blood be discovered, because in such fine vessels it loses its colour: besides, the globules of blood in such exceedingly small veins and arteries, if they are not dissolved of themselves, yet by the expansion of the gut, to bring it into a flat position, they must necessarily be dispersed and dissolved.

The gut, as far as we have been able to examine it, consists in substance or thickness of eight skins or membranes, lying upon one another. Between two of these membranes I observed that there lay some fibres without any branches or sprigs proceeding from them, and pursuing my observations, there occurred to my sight some other small fibres lying close to the rest, which seemed to be torn from other parts.

When we consider the great protrusion of blood without the vessels, as it appeared to our eyes by the help of a microscope, we may suppose that such protrusion or expulsion of the blood was occasioned by a great and sudden fright, or some other affection; whence we may conclude, that in any such accidents, bleeding is highly necessary, in order to give the blood room enough in the vessels for a free circulation. Now if the all-wise Creator had not covered those blood-vessels that lie upon our bowels with a very thin, but strong membrane, that blood, which is forced through the veins, would run into the cavity of the belly; and there stagnating in great quantities would rot and putrefy, and consequently death must follow; whereas now, as it is found to lie in small parcels on the bowels and other places, it may be easily dissolved again, and the person may recover.

On the Manner of manuring Land with Sea-shells, in the Counties of Londonderry and Donegal in Ireland. By the Archbishop of Dublin. N° 314, p. 59.

Both these counties are very mountainous, and the mountains so covered with bogs and heath, that there is little arable ground in them, except what has lately been made so. There are three ways of reducing heath and bog to arable land: the first is by cutting off the scurf of the ground, making up the turf so cut in heaps, and when the sun has dried them setting them on fire; when burnt as much as they can be, the heaps are scattered on the ground, and after ploughing, it produces barley, rye, or oats, for about three years. The inconveniences of this method are, first, that the burning defiles the air, causes rain and wind, and is not practicable in a wet summer; also by destroying the sap of the earth and the roots of the grass, and all other vegetables, renders it useless for several years after the third, in which it is ploughed.

The second way is by liming. This is much better than the former, because it does not so much impoverish the ground, it lasts longer, and produces better grain, and does not destroy the grass if due care be taken not to over plough it. But this is very dear, and lime-stone is not every where to be had, and in many places fuel is wanting. Dung is the common manure in all places, and therefore I shall say nothing of it.

Marl is not used in the north parts of Ireland; but about the sea side the great manure is shells; towards the eastern part of the bay of Londonderry, commonly called Loughfoyle, lie several eminences, that hardly appear at low water; these consist of shells of sea-fish of all sorts, more particularly of periwinkles, cockles, limpets, &c. The countrymen come with boats at low water, and carry loads of these shells away; they leave them in heaps on the shore, and there let them lie till they drain and dry, to render them lighter for carriage; they then carry them by boats as far as the rivers will permit, and then in sacks on horses, perhaps 6 or 7 miles into the country. They allow sometimes 40, but mostly 80 barrels to an acre. These shells agree with boggy, heathy, clayey, wet, or stiff land, but not with sandy. They seem to give the land a sort of fermentation, as yest does to bread, opening and loosening the clods, and by that means making way for the roots to penetrate, and for the moisture to enter into the fibres of the roots. The manure continues so long, that none can determine the time of its duration. The reason of which seems to be, that the shells dissolve every year a little, till they be all spent, which requires a considerable time; whereas lime, &c. operates all in a manner at once; but it is to be observed, that in 6 or 7 years the ground grows so mellow, that the corn on it grows rank, and runs out in straw to such a length, that it cannot support itself, and then the land must be suffered to lie a year or two, that the fermentation may abate a little, and the clods harden, and then it will bear as long again, and continues to do so, with the like intermissions, for 20 or 30 years. In the years in which the land is not ploughed, it bears a fine grass, mixed with daisies in abundance, and it is pleasant to see a steep high mountain, that a few years before was all black with heath, on a sudden look white with daisies and flowers. It fines the grass, but makes it short, though thick.

Observing that this manure produced flowers in the field, I made my gardener use these shells in my flower garden, and never saw better carnations, or flowers fairer or larger than in that cold climate. Besides, it contributes to destroy weeds, at least it does not produce them so much as dung; it likewise produces very good potatoes, at about a foot distance from each other. And this is one method of reducing boggy barren land; they lay a little dung or straw on the land, and sprinkle it with shells; sometimes they cut the potatoes if large, that they may go the farther, and then dig trenches about 6 or 7 feet distance, and throw the soil they take out of them on the potatoes, so as to cover them; then fencing the plot of ground, they leave them to grow. Planted in April or May they are ripe in August; they dig them as they have occasion, and let them lie till next year, then dig them again, and so the third year; every year they by this means go deeper in the earth, and the last they dig them, they pick them out as carefully as they can, that little seed may remain; and the fourth year they plough the ground and sow barley, and the produce is very good for some years. Some potatoes will remain and grow up, without any hurt to the barley or oats; those they dig and pick out, and the ground remains good and arable ever after.

It is observable, that shells answer best in boggy grounds, where the surface is turf; for turf is generally nothing else but the product of vegetables, such as grass, heath, &c. which rotting, the salt is washed away by the water, and there only remains the earthy, and especially the sulphureous parts, as appears from the inflammability of turf; now shells being chiefly a salt, it incorporates with the sulphur of the plants, and renders them fit for the vegetation of new plants. Which further appears from this, that shells that have been under the salt water are much better than such as have been in the earth, or dry on the strands: on digging a foot or two deep about the bay of Londonderry, it yields shells, and whole banks are made up of them; but these, though more entire than such as are brought out of the shell island, are not so good for manure. I observed in a place near Newtown Lamavady, about 2 miles from the sea, a bed of shells, such as lie on the strand; the place was covered with a scurf of wet spouty earth, about a foot thick; the country people used the shells, but they were not reckoned so good as those that are found in the sea, or near it.

The land about the sea-side bears very indifferent wheat: nor will the shells, in that respect, do without some dung; yet this may be owing to the ignorance of the farmers, who generally understand nothing of wheat. Some thousands of acres have been improved by the shells, and that which formerly was not worth a groat per acre is now worth 4 shillings; they have in many places thus improved the very mountains, that before were mere turf bogs. In these they meet with this inconveniency, that if the season for ploughing proves wet, their horses sink so deep in the soil that they cannot plough it, especially after two or three years.

They commonly made lime of the shells formerly, and some do so still; it binds very well, and I believe it is not so corrosive as lime made of stone; for I find that in the history of Ceylon, that they make up their land with lime of oyster-shells, which I believe would be impracticable with common lime. About

30 years ago they made lime of the shells, and manured their lands with it; but a poor countryman, who out of laziness or poverty had not provided to make lime, threw the shells unburnt on his land, and his crop proved as good as his neighbours, and the second and third crop better; from this, all took the hint, and have used them so ever since. Where shells are not to be procured, sea-wrack or sand supply the want of them, but are not so good: sea-wrack lasts but 3 years, and sand not much longer.

It is certain that Ireland has formerly been better inhabited than it is at present: mountains, that now are covered with bogs have formerly been ploughed; for on digging 5 or 6 feet deep, they discover a proper soil for vegetables, and find it ploughed into ridges and furrows. This is observable in the wild mountains between Ardmah and Dundalk, where a redoubt is built, and likewise on the mountains of Altmore; the same, it seems, has been observed in the county of Londonderry and Donegal; a plough was found in a very deep bog in the latter, and a hedge with wattles standing, under a bog that was 5 or 6 feet deep above it. I have seen the stump of a large tree in a bog 10 feet deep at Castle-Forbes; the trunk had been burnt, and some of the cinders and ashes lay still on the stump. I have also seen large old oaks grow on land, that had the remains of ridges and furrows. And I am informed, that on the top of a high mountain in the north, there are still remaining the streets and traces of a large town; and indeed there are few places which do not visibly, when the bog is removed, show marks of the plough; which must prove that the country was well inhabited. It is likely that the Danes first, and then the English, destroyed the natives, and the old woods seem to be about 3 or 400 years standing; which was near the time that Courcey and the English subdued the north of Ireland; and it is likely made havoc of the people that remained, after the Danes were driven out of Ireland.

Of the Length of Curve Lines. By the Rev. John Craig. N° 314, p. 64.
Translated from the Latin.

LEMMA. *To divide the Sum of two Squares into two other Squares.*—Let $\dot{z}^2$ and $\dot{u}^2$ be two given squares, whose sum $\dot{z}^2 + \dot{u}^2$ is to be divided into two other squares $\dot{x}^2$, $\dot{y}^2$; also let m and n be any two numbers taken at pleasure. Now, from the condition in the problem, it is $\dot{x}^2 + \dot{y}^2 = \dot{z}^2 + \dot{u}^2$; hence, as appears from Diophantus, $\dot{x} = \frac{(mm - nn)\dot{z} + 2mn\dot{u}}{mm + nn}$, and $\dot{y} = \frac{(nn - mm)\dot{u} + 2mn\dot{z}}{mm + nn}$.

PROBLEM. *To find innumerable Curves, of the same Length with any proposed Curve, whether Algebraical or Transcendental.*—Let z, u denote the co-ordinates of the proposed curve, and x, y the ordinates of the required curve, which may be of the same length with it: hence, from the elements of curves, $\dot{x}^2 + \dot{y}^2 = \dot{z}^2 + \dot{u}^2$; then by the foregoing lemma, $\dot{x} = \frac{(mm - nn)\dot{z} + 2mn\dot{u}}{mm + nn}$, and $\dot{y} = \frac{(nn - mm)\dot{u} + 2mn\dot{z}}{mm + nn}$; and the integrals of these are $x = \frac{(mm - nn)z + 2mnu}{mm + nn}$, and $y = \frac{(nn - mm)u + 2mnz}{mm + nn}$. And thus the co-ordinates x and y of one of the required curves become known: in like manner from this one may be found a second; from the second, a third; and so on, as far as we please. Q. E. I.

I add no examples now, as a fitter opportunity will occur hereafter, by applying this method to several problems of this kind, and illustrating it by examples. And I have more than once so plainly pointed out the solution, that it might easily have been deduced from what is subjoined to the solution of a particular case of this problem, in which the proposed curve is algebraical, as exhibited in the Philos. Trans. N° 289. So that Mr. John Bernoulli, the learned proposer of the problem, may see that the solution is obtained from the common rules of the inverse method of fluxions, though he insinuated in his private letters to Dr. Cheyne, that it could not be exhibited by our theorems in the Philos. Trans. N° 284. And as I perceive from the Leipsic Acts of August 1705, that our solution did not please that learned man, though more than sufficient to the purpose, I have therefore published the foregoing solution, which cannot be liable to any objection.

I shall now notice some things which I cannot approve in Mr. Bernoulli's own solution of his problem. As first, that he has applied it only to algebraical curves. Secondly, that it is mechanical, and depends wholly on what he calls creeping motion. As to Mr. Huygens, he is deserving of immortal honour, for his invention of the motion of evolution, from whence both he himself and others have derived curious geometrical theorems. But neither Leibnitz's motion of traction, nor Bernoulli's creeping motion, will ever be comparable to Huygens's motion of evolution, till those ingenious men, as Huygens has done, shall reduce the curves generated by their motions to the laws of geometry. But since neither of them has yet performed this, the solution of problems, depending on curves produced by their motions, can only be reputed as mechanical.

An Account of a New Island raised near Sant-Erini in the Archipelago. By Dr. W. Sherard, Consul at Smyrna, &c. N° 314, p. 67.

On the 12th of May, 1707, an island began to rise up, a musket-shot distant from the island of Sant-Erini, which continually increasing from day to day in the same manner, and troubling the sea, there rose up several rocks, that fixed themselves to this island; so that now, June 21, it is about half a mile in circumference.

From the Paris Gazette, April 14, 1708.

Constantinople, Jan. 4, 1708.—They write from the island of Sant-Erini in the Archipelago, about 28 leagues north of the town of Candia, that there lately sprung up from the bottom of the sea an island, formed of stones cast up by a volcano, which has often produced the same effects, and after the same manner. In the year 726, in the time of the emperor Leo Isauricus, an island was formed on the north side, called the Burnt Island, by matter thrown up and heaped together by this volcano. In 1427, in the month of December, this Burnt Island was increased by huge rocks cast up by subterraneous fires. In 1650, in the month of September, the volcano again took fire, and produced the same effects, without forming any island, but only a shelf or bank 10 fathoms under water. Lastly, in the month of November last, 1707, the volcano made an island, which is already two miles in circumference, and still increases by rocks and other new matter thrown up. This burning was preceded, as at all other times, by violent shakings of the earth, followed by a thick smoke, that rose out of the sea in the day time, and by flames in the night, and accompanied with a terrible roaring under ground. There is no instance of the effects of any volcano at land, like these in the sea; and yet what renders them the more credible, is, that the island of Sant-Erini itself is almost all of it composed of burnt rocks and pumice-stones: it produces some sorts of grain, but has neither rivers nor springs, nor any other water but what is saved in cisterns.

Experiments on the Luminous Qualities of Amber, Diamonds, and Gum Lac. By Dr. Wall. N° 314, p. 69.

It is well known that human urine and dung abound with an oleosum and common salt; so that I take the artificial phosphorus to be that animal oleosum, coagulated with the mineral acid of spirit of salt; which coagulum is preserved, and not dissolved in water, but accended by air. These considerations made me conjecture that amber, which I take to be a mineral oleosum, coagulated with a mineral volatile acid, might be a natural phosphorus; so, after many experiments upon it, I at last found, that by gently rubbing a well polished piece of amber with my hand in the dark, it produced a light; on which I got a large piece of amber, made it long and taper, and drawing it gently through my hand, then very dry, it afforded a considerable light. I then used several kinds of soft animal substances, and found none answer so well as wool. And no new phænomena offered themselves; for upon drawing the piece of amber swiftly through the woollen cloth, and squeezing it pretty hard with my hand, a prodigious number of little cracklings were heard, every one of which produced a little flash of light; but when the amber was drawn gently and slightly through the cloth, it produced a light, but no crackling; but by holding a finger at a little distance from the amber, a crackling is produced, with a great flash of light succeeding it; and, what is very surprising, on its eruption it strikes the finger very sensibly, wheresoever applied, with a push or puff like wind. The crackling is full as loud as that of charcoal on fire; nay, five or six cracklings, or more, according to the quickness of placing the finger, have been produced from one single friction, light always succeeding each of them. Now I doubt not, but on using a longer and larger piece of amber, both the cracklings and light would be much greater, because I never yet found any crackling from the head of my cane, though it is a pretty large one; and it seems, in some degree, to represent thunder and lightning; but, what is more surprising, though on friction with wool in the day time, the cracklings seem to be full as many, and as large, yet very little light appears, even in the darkest room; and the best time of making these experiments, is when the sun is 18 degrees below the horizon; and then, though the moon shine ever so bright, the light is the same as in the darkest room, which makes me chuse to call it a noctiluca.

As the artificial phosphorus led me to consider that of amber, so amber directed me to that of a diamond, from its being electral as well as the other, which is also a natural phosphorus, or rather a noctiluca, exceeding all others, and may, without any exception, be called a mineral phosphorus, it being, I suppose, the most pure of all oleosums. A diamond, by an easy slight friction, in the dark, with any soft animal substance, as the finger, woollen, silk, &c. appears in its whole body to be luminous; nay, if you keep rubbing for a little while, and then expose it to the eye, it will remain so for some little time: but if the sun be 18 degrees below the horizon, and any one holds up a piece of baize or flannel stretched tight between both hands, at some distance from the eye, and another rubs the baize or flannel with the diamond swiftly and pretty hard on the other side of it, the light to the eye of him that holds it, seems much more pleasant and perfect than any other way I have yet tried. But, what seems most surprising, a diamond being exposed to the open air in view of the sky, gives almost the same light of itself without rubbing, as if rubbed in a dark room; and if in the open air you put your hand, or any thing else, a little over it, to hinder its communication with the sky, it gives no light.

I have tried all or most of the other precious stones, but could find no such phenomenon in any of them. All these experiments were made at the latter end of May and beginning of June, and therefore I cannot pretend to account for the phænomena that may attend experiments made while the sun is on the other side of the equator.

Some other bodies are known to afford light, and perhaps there are many more which remain yet undiscovered; but I am well assured, that all or most of the bodies which have an electricity, yield light; for it is the light that is in them, that is the cause of their being electral; yet this electricity never shows itself without friction; if you rub any body that has an electricity, and apply it near to some light bodies, as particularly very thin slices of cork, it will put them into a great agitation, and make them seem to the eye as if hanging at the body by a fine hair. Jet seems to be a kind of black amber, having most of the same properties, though less perfect and pure.

Another natural phosphorus, or noctiluca, is gum lac, and also red sealing-wax, which is made of gum lac and cinnabar, the cinnabar no way impeding but rather promoting its luminous quality; for I caused long taper rolls to be made up of lac alone, and of pure red sealing-wax, both being well polished; the sealing-wax, upon friction, seems to emit its crackling and light sooner than the lac, which I impute to the cinnabar constringing its parts; though I think lac by itself has the greatest electricity, both having all or most of the properties of amber; and by all the trials I have hitherto made of lac and sealing-wax, I find that though the cracklings are as plentiful in the day-time, as when the sun is down, yet in the darkest places I could discover but a little appearance of light. I know not in the animal kingdom any thing except pismires that affords a volatile acid; and in the East-Indies there is a large kind of them, that live on the sap of certain plants, yielding both a gum and a colour; which sap, passing through the body of these insects, is by their acid spirit converted into an animal nature; which is the reason that with the colour extracted from gum lac (which is nothing else but the excrement of these insects) almost as good and full as lasting colours are made, as from cochineal: I am the more confirmed in this, because I know of an artificial way of converting vegetable colours into an animal nature, very much like this, by which the colours are made more pleasant and permanent. After the same manner, the remaining gum, which is an oleosum, being digested, and passing through the bodies of those insects, is by their volatile acid converted into a vegetable animal phosphorus, or noctiluca. The artificial phosphorus is a mineral animal phosphorus; whereas I take the others to be altogether mineral.

The Repetition of an Experiment on Motion given to Bodies included in a Glass, by the Approach of a Finger near its outside: with other Experiments on the Effluvia of Glass. By Mr. Fr. Hauksbee, F. R. S. N° 315, p. 82.

This experiment having been but imperfectly made before, I thought a repetition of it would not be unacceptable to the society, since not only the apparatus was better adapted, but the appearance was much more conspicuous. For it was observable, that after the motion and attrition had been continued about 2 or 3 minutes, and then ceasing, the threads within seemed to hang in a confused position, and were not instantaneously erected, but in about 3 or 4 seconds of time they were so, every way towards the circumference of the glass; and seemingly with so much eagerness, that a motion of the glass alone would give them no great disorder: but the most surprising thing, was to see a motion given them by the approach of one's hand, finger, or any other body, at more than 3 inches distance from the outer surface of the glass, although the threads within did not touch the inner one. And it was further observable, that every repetition of the motion of the wheel, and a new attrition of the glass, the distance at which the threads might be moved, seemed to be increased. And I have since found that the threads could have a notable motion given them, by blowing towards the glass with one's mouth, at 3 or 4 feet distance; by which means the air was put in motion, and consequently the effluvia of glass were so too. At another time, suddenly clapping my spread hands on the upper and lower parts of the globe, there was such a violent agitation of the threads within, as was very surprising, and which continued for some time.

In the experiment where the directed threads on the outside of the glass would fly the approach of any thing held near them, it seems to me that the parts of the effluvia are stiff, and continued; so that when any part of them are pushed, all that are in the same line suffer the same disorder. So likewise in the above experiment, allowing a continuity of parts, the effluvia within, and those without, are all of a piece or continued, (for they are both produced by the same attrition) so that when the effluvia are pushed or disturbed without, the effluvia within, in the same direction, are so too, and consequently the threads which are upheld and directed by them. The effluvia arising from the glass, seem to be nothing else but part of the same body emitted from it by rubbing; and therefore I think can be no impediment to the motion of its own effluvia, for otherwise I do not see how the effluvia within can be produced by an attrition without. And for a further confirmation that the effluvia of glass act only by a continuity of their parts, the following experiments may serve.

Exper. I. I took a piece of leaf brass, and laid it between two pieces of wood, about an inch in thickness, and the same distance asunder. I then applied a well rubbed tube to attract the brass, as near as the wood would permit, but gave it no manner of motion; then as soon as the wood was removed, and the continuity of its sphere restored, the brass-leaf was driven to it very vigorously, without any fresh attrition; which I think very plainly proves that the action of the effluvia, at least in a great measure, is lost, if its parts are discontinued by any thing interposing or interrupting its spherical figure. Nay, I have tried, by holding the tube in such a manner that its sphere might meet with no interruption from the wood, in its circle round the axis of the glass, yet neither would this exhibit any thing; by which I find, that if the parts of the effluvia are in a manner interrupted, their action is destroyed, or at least greatly impaired.

Exper. II. Again, after the tube had been fresh rubbed, and the brass-leaf scattered on the table, as usual; if a piece of paper were held to touch the upper part of the tube, it would not attract at all, though approached very near; but as soon as the paper was removed, it recovered its sphere of activity, which was very sensible, by giving a brisk motion to those bodies, which just before were at rest. And it was mentioned in the experiment for producing light by the effluvia of the outer glass falling on the inner exhausted glass in motion, that after the motions ceased, it was but approaching one's hand near the surface of the outer glass to produce a light in the inner one: whence, by these experiments it is plain, that the effluvia within the glass were pushed more vigorously on the inner glass, by the approach of the hand without, otherwise no light would have ensued. And further to prove the stiffness of the body of the effluvia, it is observable, that when a piece of brass-leaf is hunted about a room, that the brass swims or floats on the surface of the effluvia; and as these are more or less exerted, so the brass keeps its distance from them, nor will it by any means be suffered to sink within their sphere, unless it meets with a body in its way, and then it is attracted and returned again several times, with great swiftness.

Exper. III. Having tried the effect of discontinuing or interrupting the effluvia of the fricated tube on its outer surface, I was willing to try what would ensue by filling its cavity with a body; which I did, by corking up one end of it, and pouring in at the other dry writing sand, till it was nearly full: the attrition was made, and when held towards the pieces of brass, as usual, there was no motion till it arrived within about an inch of them. And thus on several trials it answered much alike. And if at the same time the sand be suddenly shot out, the tube would attract the same bodies at double or treble the forementioned distance, without any fresh attrition; which plainly shows that although the tube will attract when approached near, yet the body within is a sensible impediment to the extension of its action without. This brings to my mind the unsuccessfulness of the attempt I made to attract bodies with a tube exhausted of its air; which seems to conclude, there being no air within to bear the effluvia from its body, a continuity of their parts must consequently be prevented.

Some Experiments showing Electricity and Light producible on the Attrition of several Bodies. By Mr. Fr. Hauksbee, F.R.S. N° 315, p. 87.

I caused a piece of wood to be turned into the form of a short cylinder, about 4 inches in diameter, and 3 in length. This being fixed on an axis, I melted in a ladle about a pound and a half of the best sealing-wax I could procure, and when fluid I plunged the wooden cylinder into it, where I kept it moving round till it had got a coat of wax about half an inch thick on its surface, (I mean that part of it which is most remote from its axis): when it was perfectly cold, I placed it on the machine, which gave motion to it by a large wheel, as usual in the experiments on the attrition of the glass globe; after the motion and attrition had been continued for some small time, I held the ring of threads over the cylinder, on which they were attracted and directed towards its centre, as in the like experiment made with the glass globe. The threads likewise, while they continued directed, would fly the approach of a finger. Thus in all respects relating to electricity, the effluvia of wax seems very agreeable to those producible on the attrition of glass: for, on rubbing a stick of the same wax, the brass leaf would be attracted and returned with great velocity; and sometimes a piece of the same brass might be carried all about a room, seemingly riding or floating on the surface of its effluvia. In short, I find no difference in the laws of the different effluvia, though those of glass seem to be much the strongest, and to act with the greatest vigour.

On the approach of night, I caused the same motion to be given to the wax cylinder, as I had done in the day time, to see what light might then be produced on its attrition. I applied some clean new flannel on it, but could discover little or no light: yet afterwards, on holding my naked hand, as usual, on the glass globe, a considerable light was visible, though only where the attrition was made, nor would it continue any longer than the motion. I tried if a light would be communicated to one's finger, and approached near it, as in the experiment of the above-mentioned glass, but could obtain no such appearance without touching it. This in a great measure shows the weakness of its effluvia. I likewise tried what light might be produced from it, by giving it motion in vacuo; and though I was forced to use flannel there, yet a very distinguishing light appeared on each arm of the brass spring, that gently embraced it; and I doubt not but if my hand could be made use of to rub the wax in such a medium, the light would have been much greater; for the light produced by the attrition of the flannel on the wax in vacuo, was rathar better than that which was produced by the attrition with my naked hand in open air. From all which experiments it appears, that the largeness or smallness of light or attraction, producible from bodies by attrition, proceeds from the number and strength of their respective effluvia.

Further, I caused two wooden cylinders to be turned, of the same dimensions as mentioned in the experiment of the sealing-wax; and in the same manner as in that, I coated their outer surfaces, one with melted sulphur, the other with colophony or rosin, mixed with brick-dust, (put into it to bind and make it the more hard); and first I fixed the cylinder, which was clothed with the melted flour of sulphur, to give it motion as usual; and after my hand had been held on it a little while, I caused the motion to stop, then bringing near it the ring with threads, mentioned in former experiments, the threads were attracted and directed to its centre, but not so strongly as to the sealing-wax. And on several trials this was much the same. I then tried the rosin in the same manner, and found its electrical quality much stronger than the former: for the threads were driven towards its centre, seemingly with greater vigour, than towards that of the sealing-wax; but the rosin at that time was not quite cold. In both these experiments, the threads would fly the approach of the finger; but if sealing-wax or amber were held near them, they would very eagerly fly and adhere to them, without being rubbed; and this I never took notice of before. I further observed, that the rosin, while warm, would attract brass-leaf at an inch or two distance without any attrition. But next day when I came to repeat the experiment, its electricity was very inconsiderable, as well as that of the sulphur. At night I tried what light these bodies would yield on their attrition in the dark; but could produce none from the rosin, and indeed but very little from the sulphur, and that not by my hand, but by holding the ends of my nails very hard on it while it was in motion. I tried likewise whether the sulphur would emit any light by its attrition in the dark in vacuo; but could discover none.

The most surprising of all experiments that I have met with yet, are the following. I took my glass globe, that I use for showing the experiment of the included threads, which would point every way from the centre to the circumference on its attrition; and in that state a motion might be given to those threads, by the approach of one's hand near its outside. But this proceeded from the effluvia of its own body exerted by rubbing, therefore not so much to be wondered at. But that those threads contained in the same globe, should have motion given them by the effluvia of a heterogeneous body separate from it, and the globe at the same time to have no manner of motion or attrition given it, is very surprising; and that it is so, is matter of fact; for when I held rubbed sealing-wax near the outside of the globe, the threads within would have motion given them in a very odd manner, though the body of wax touched not the glass by 3 or 4 inches. The like I found might be performed by a rubbed glass tube, or by amber; and if the threads were placed in a bottle well corked up, or any other close glass, I suppose it would answer the same.

I have since repeated this experiment with brass leaf, covered with a glass dish on a table; and it was observable, that though the dish was very thick, on holding the well rubbed sealing-wax over it, the pieces of brass-leaf within would have a brisk motion given them, and continue so for a considerable time, ere the wax would require any fresh attrition. But this appearance will not always succeed; for some time after, attempting the same experiment, I could by no means make it answer as before; the temperature of the air being then altered, its moist effluvia were condensed on the glass; and so long as it remained under such circumstance, it was attempting it in vain. But I found if the glass was a little warmed by the fire, or placed a while in the sunshine, or well rubbed with a warm dry linen cloth, any of which, whereby the humid effluvia might be evaporated, that then the included pieces of brass leaf would have as brisk a motion given them, from the fricated wax as before. Now, that the fire, sunshine, or the rubbing the glass with a warm dry linen cloth, not only clears it from the moist effluvia condensed on it, but likewise gives motion to the particles of the glass itself, which motion seems to produce effluvia, which in conjunction with that of the sealing-wax, facilitates its action on the said bodies, I conclude from this particular, viz. that when I had warmed the glass by the fire, or had evaporated the humid effluvia by any of the other means, I found I could give motion to the included brass-leaf, by only rubbing my finger on the outside of the glass, without the assistance of the wax. But when the well rubbed wax was held over it, the motion of those bodies would be much increased; and it was observable, that sometimes the brass-leaf would continue to be in motion after the wax was withdrawn from it. But when the air is naturally warm, and free from humid vapours, there need none of the above-mentioned means to assist the effluvia of the wax to give motion to the included brass leaf. Yet at the same time I must believe, that the particles of the glass are then in a greater motion, than when the experiment will not succeed. What I farther observed was, that the effluvia of the wax may very sensibly be felt on the back of the hand, the wax being moved to and fro near it; as I have formerly observed the like sensible strokes given by the effluvia of glass.

An Experiment showing the different Densities of the Air, from the Greatest Natural Heat, to the Greatest Natural Cold, in this Climate. By Mr. Fr. Hauksbee, F. R. S. N° 315, p. 93.

I took a glass tube about 2 feet long, and near 2 tenths in diameter; which, at about 6 inches from one end, I bent in form of a syphon, as represented in fig. 20, pl. 10: at whose farthest extremity I cemented a brass screw, with a small perforation in it; by which means, when I put a little quicksilver into the shorter leg, I could, by declining the tube, or longest leg, bring it to rest any where; as suppose at A, the superfluous air within passing through the said perforation. Then screwing a cap on it, the mercury was detained in the same place, and occupied about half an inch in length. In this manner it was conveyed into a wooden trough, with a thermometer: then putting in as much warm water as would cover its ball, the syphon lying at the bottom in a horizontal position, its shorter leg appearing above the water, (which was so contrived as to prevent any inconvenience that might arise by the water's getting into it, and to give a free liberty for the pressure of the outward air to exercise its full power); when the spirit of wine had ascended by the heat of the water into its small ball at top, which I thought was necessary, that I might make my observations with the more exactness on its descent; supposing by that time it should fall to the degree designed to begin at, that the spirit in the ball would have received an equal degree of heat in all its parts. Accordingly, I began my observations when it had descended to 130 degrees above the freezing point; at which time I found the length of the column of air, from the closed end of the syphon, to the nearest surface of the quicksilver, to be just 144 tenths of an inch. After the spirit had descended 10 degrees lower, the air, which before possessed 144 parts, was now one part less; and so on successively, at every 10 degrees descent of the spirit, the column of the contained air was lessened in its length exactly one tenth. When it had descended to 30 degrees above the freezing point, the air was found to possess only 134 of the said parts: so that from hence it will be easy to conclude, that at the freezing point, the air in the syphon would be reduced to 3 tenths less than the last observation. Consequently at 50 degrees below the freezing point, which I am informed is the greatest degree of cold that has happened in our climate, it would be reduced to 126 parts of the whole, and in that state would be one 8th more dense than when at the greatest degree of our natural heat. The reason why I could not prove this latter part by experiment was, that when I came to expose the thermometer and syphon in the open air, or to the freezing mixture, the syphon would instantly receive the impression of the cold, and the air contained in it be considerably contracted, before the thermometer would give any sign of such alteration. But seeing the former part of the experiment succeeded so exactly regular, I think there can be no doubt of the truth of the whole calculation. This experiment was made February the 11th, 1708; the mercury in the barometer at the same time standing at 30 inches.

On the Laws of Attraction and other Physical Principles. By Mr. John Keill, A.M. of Christ-church College, Oxford.* N° 315, p. 97. *Translated from the Latin.*

The three following principles are to be laid down, as the foundation of all physics; viz. 1. A vacuum. 2. The divisibility of quantity in infinitum. 3. The

* Dr. John Keill, an eminent mathematician and F. R. S. was born at Edinburgh in 1671, and studied in that university, where he made great proficiency in the mathematical sciences, under Da. Gregory the learned professor there, who was one of the first that had embraced and publicly taught the Newtonian philosophy. In 1694 Mr. Keill followed his tutor to Oxford, where he obtained one of the Scotch exhibitions in Baliol college, but afterwards removed to Christ-church college, about the year 1700. It is said that Keill was the first who taught Newton's principles, by the experiments on which they are founded: and this it seems he did by an apparatus of his own providing; by which means he acquired a great reputation in the university. The first public specimen he gave of skill in scientific knowledge, was his examination of Dr. Burnet's Theory of the Earth; with Remarks on Mr. Whiston's New Theory; which appeared in 1698. These theories were defended by their authors; which drew from him, in 1699, an Examination of the Reflections on the Theory of the Earth, with a Defence of the Remarks on Mr. Whiston's New Theory. In 1700 Mr. Keill was appointed deputy professor of natural philosophy in Oxford; and the year following came out his celebrated treatise, Introductio ad Veram Physicam, being the substance of his public lectures, and is supposed to be the best and most useful of all his performances. This edition contained only 14 lectures; but in the second, of 1705, he added two more: and an English edition of the same was printed in 1736.

About the year 1708 he became F. R. S. and the same year had published in the Philos. Trans. the above paper on the laws of attraction and its physical principles. About this time being offended at a passage in the Leipsic Acts, calling in question Newton's right to the first invention of the method of fluxions, he communicated another paper, asserting the justice of that claim: a circumstance which occasioned some disputes with Mr. Leibnitz on that point, which terminated in favour of Mr. Keill. In 1709 Mr. Keill was appointed treasurer to the Palatinates, and in that station attended attraction of matter. That there is a vacuum, is plain from the motion of bodies: geometricians demonstrate the divisibility of quantity from the nature of continued quantity: and experience proves that matter has an attractive force. From the first two principles follows:

Theorem 1.—Any particle of matter, however small, may fill a space however large, in such a manner, as that the diameters of all the pores and vacuities in it, may be less than any given right line; or that the distances of the particles from each other may be less than a given right line.

Theorem 2.—Two bodies may be given of equal bulk, however unequal in weight and density (i. e. quantity of matter) the sums of whose vacuities or pores may also approach to a ratio of equality. Let there be, for instance, a cubic inch of gold, and another of air: then though the quantity of matter in the cube of gold exceed 20000 times that in the cube of air: yet it is possible that the vacuities in the former may be almost equal to those in the latter, viz. that the vacuities in the gold may be to the vacuities in the air; as 999999 to 1000000.

Theorem 3.—The particles which constitute water, air, or such like fluids, if they touch each other, are not absolutely solid, but consist of other particles, that contain vacuities and pores. The most minute and absolutely solid particles of bodies, that is, such as have no vacuities at all, may be called particles of the first composition: the moleculæ arising from the coalescence of several of these particles may be denominated particles of the second composition: and again the masses made up of several of these moleculæ, may be called particles of the third composition; and so on, till at length we come to particles which constitute the ultimate composition of bodies, and into which they may be ultimately resolved. That matter is endued with an attractive force, by which its particles mutually attract each other, Sir Isaac Newton first deduced from the phæno-

them in their passage to New England in America. And soon after his return, in 1710, on the death of Mr. Caswell, Savilian professor of astronomy at Oxford, he was chosen to succeed him. About the year 1711 several objections being urged against Newton's philosophy, in support of Descartes's notions of a plenum, M. Keill presented to the Royal Society a paper on the Rarity of Matter, and the Tenuity of its Composition. And while engaged in this controversy, he was apointed decipherer to queen Anne, an office which he held also under king George the First, till 1716. Other ingenious compositions of his are also found in the Philos. Trans. In 1713 the university of Oxford conferred on Mr. Keill the degree of M.D.; and two years after, he published an edition of Commandine's Euclid, with additions of his own. In 1718 came out his Introductio ad Veram Astronomiam; an English edition of which was also published in 1721, entitled, An Introduction to the True Astronomy: or, Astronomical Lectures, read in the astronomical school of the university of Oxford. This was Dr. Keill's last gift to the public; being this summer seized with a violent fever, which put a period to his useful life, Sept. 1, in the 50th year of his age.

mena: this attraction in a given quantity of matter, at different distances, is reciprocally proportional to the squares of these distances: from whence arises that force we call gravity, by which all terrestrial bodies tend towards the earth in a right line, and the weight of bodies is always proportional to the quantity of matter they contain; from this attractive force Sir Isaac, in a very beautiful manner, explained all the motions of the planets, and the phænomena of comets, and happily perfected physical astronomy. After repeated experiments, Mr. Keill observed, that from this attractive force several phænomena might be accounted for.

Theorem 4.—Besides that attractive force, by which both planets and comets are retained in their orbits, there is also another power in matter, by which all its particles mutually attract, and are mutually attracted, by each other; which power decreases in a greater ratio, than the duplicate ratio of the increase of the distances. This theorem may be proved by several experiments: but it does not yet so well appear by experiments, whether the ratio, by which this power decreases, as the particles recede from each other, be in a triplicate, quadruplicate, or any other ratio of the increase of the distances.

Theorem 5.—If a body consist of particles, each of which has an attractive force, decreasing in a triplicate, or more than triplicate, ratio of the distances, the force by which a corpuscle is attracted by that body, in the points of contact, or at an infinitely small distance, will be infinitely greater than if that corpuscle were placed at a given distance from the said body. Vide Princip. Newtoni. Prop. 80, 91.

Theorem 6.—From the same data, it follows, if that attractive force, at a given distance, have a finite ratio to gravity, that in the points of contact, or at an infinitely small distance, it will be infinitely greater than the force of gravity.

Theorem 7.—But if the attractive force of bodies in the points of contact have a finite ratio to gravity, it is, at all assignable distances, infinitely less than the force of gravity, and consequently vanishes.

Theorem 8.—The attractive force, that each particle of matter exerts, in the point of contact, exceeds almost immensely the force of gravity; yet is not infinitely greater than it, and consequently, at a given distance, that attractive force will vanish. Therefore, this power, which is superadded to matter, diffuses itself only to very small distances; and at greater distances is none at all: from whence the motions of the heavenly bodies (that are at great distances from each other) are no wise disturbed by this attractive force, but move continually in the same manner, as if these bodies had no such force at all.

Theorem 9.—If any corpuscle be in contact with a body; the force by which that corpuscle is attracted, that is, the force by which it coheres with that body, will be proportional to the quantity of contact: for the parts more remote from the contact, contribute nothing to the cohesion. Therefore, according to the various contact of particles, various degrees of cohesion arise; and the force of cohesion is greatest, when the superficies, where bodies touch each other, are planes; in which case, cæteris paribus, the force with which a corpuscle coheres with other corpuscles, will be as the parts of the superficies that touch each other: hence appears the reason why two marbles, exactly polished, and touching each other in their plain superficies, cannot be separated, but by a weight that far exceeds the gravity of the incumbent atmosphere: hence likewise may be given the solution of that noted problem, the cohesion of matter.

Theorem 10.—Those corpuscles are most easily separated from each other, which have the fewest and smallest points of contact with other corpuscles; as is the case in spherical corpuscles infinitely small.—Hence fluidity is accounted for.

Theorem 11.—The force by which any corpuscle is attracted towards another body very near it, does not change its quantity, whether the matter of the attracting body be increased or diminished; the density of the body, and the distance of the corpuscle remaining the same.—For, since the attractive force of particles extends only to very small distances; it is plain that the more remote particles at C, D, and E, (plate 10, fig. 21) contribute nothing to attract the corpuscle A: therefore the corpuscle will be attracted with the same force towards B, whether these be present or removed, or whether others be added to them.

Theorem 12.—If the contexture of any body be such, as that the particles of the last composition should by some external force (such as the pressure of a weight, or the stroke of any other body) be removed a little from their primogenial contacts, but so as not to acquire new contacts; the particles mutually attracting each other will soon return to their primogenial contacts; and the same contacts and positions of the particles that constitute any body, being restored, there will also be a restitution of the same figure of that body: consequently bodies may by their attractive force again recover their pristine figures. Hence we may account for elasticity: for, as bodies impinging on each other, do by their elastic force recede from each other, as is demonstrated in the physical lectures; so the resilition of bodies from each other should arise from their attracting force.

Theorem 13.—But if the texture of a body be such, as that its particles, when removed from their primogenial contacts by a force impressed on them, should immediately run into others of the same degree; that body will not return into its pristine figure.—Hence appears the nature of that contexture which constitutes soft bodies.

Theorem 14.—The particles of matter, according to their different structure and composition, will be endued with different attractive forces: for instance, the attraction will not be so strong, when a particle of a given magnitude has several pores, as when it is entirely solid, and without any.

Theorem 15.—The attractive forces of particles perfectly solid, depend very much on their figure. For should any small particle of matter be formed into a circular lamina of an indefinitely small thickness; and a corpuscle be placed in a right line passing through the centre, perpendicular to the plane of the circle; and the distance of the corpuscle be equal to $\frac{1}{10}$ of the semi-diameter of the circle; the force with which the corpuscle is attracted, will be 30 times less, than if the attracting matter coalesced into a sphere, and the power of the whole particle exerted itself from one physical point; and the same circular lamella more strongly attracts the corpuscle, than another particle of the same weight does, formed into a slender and oblong cylinder.

Theorem 16.—Salts are bodies whose particles of the last composition are endued with a great attractive force, though there are several pores interspersed between them, that are previous to the particles of water of the last composition; which being therefore strongly attracted by the saline particles, they violently rush upon them, and disjoin them from their mutual contact, and dissolve the cohesion of the salts.

Theorem 17.—If two corpuscles mutually tend towards each other, with attracting forces decreasing in a triplicate, or more than triplicate ratio of their distances; the velocity of their mutual impulse will be infinitely greater than at a given distance. Vide Princip. Newtoni, Prop. 39.

Theorem 18.—The magnitude of a body heavier than water, may be so far diminished, as at length to remain suspended in it, and not descend by its proper gravity.—Hence appears the reason why saline, metalline, and other such like particles, reduced to the smallest parts, remain suspended in their menstrua.

Theorem 19.—Greater bodies mutually tend towards each other with less velocity than smaller bodies.—For the force with which the bodies A and B, plate 10, fig. 22, mutually tend towards each other, is principally only in the nearest particles, whilst the forces of the more remote are none at all; therefore a greater force is not applied to move the bodies A and B, than the particles c and d; but the velocity of bodies moved by the same force are reciprocally proportional to the bodies themselves; consequently the velocity with which the body A tends towards B will be to the velocity with which the particle c, separated from the body, would tend towards the same body B, as the particle c is to the body A; therefore the velocity of the body A is far less than would be that of the particle c, separated from it: hence it is that the motion of great bodies is naturally so languid and slow, as to be commonly retarded by the circumambient fluid, and other circumjacent bodies, while the smallest corpuscles are vigorous, and several effects are produced by them: so much greater is the energy in smaller, than in greater bodies. From hence appears the reason of that chemical axiom, viz. that salts do not act till dissolved.

Theorem 20.—Two corpuscles that do not touch, may be placed so near each other, that the force with which they tend towards each other, shall far exceed that of gravity.

Theorem 21.—If a corpuscle, placed in a fluid, be on every side equally attracted by the circumambient particles; there will be no motion of that corpuscle; but if it be attracted more by some particles, and less by others, it will tend to that part where the attraction is greater; and the motion produced will correspond to the inequality of the attraction, viz. the motion will be greater in a greater, and less in a less inequality.

Theorem 22.—Corpuscles floating in a fluid, and attracting each other more than they attract the intermediate particles of the fluid, will drive away the particles of the fluid, and approach each other, with that force by which their mutual attraction exceeds the attraction of the particles of the fluid.

Theorem 23.—If any body be placed in a fluid, whose particles attract those of the fluid more than the said particles are attracted by each other; and there be a great many pores in the body, pervious to the particles of the fluid; that fluid will soon diffuse itself through these pores: and if the cohesion of the parts of the body be not so compact, but that it may be overcome by the impetus of the rushing particles of the fluid, there will hence arise a solution of the immersed body.—Hence there are three things requisite to make a menstruum fit to dissolve a given body: viz. 1. That the parts of the body attract the particles of the menstruum, more than the said particles are attracted by each other. 2. That the body have pores, open and pervious to the particles of the menstruum. 3. That the cohesion of the particles, which constitute the body, be not so great, as that it may not be overcome by the impetus of the rushing particles of the menstruum. Hence it also appears, that the particles of which spirits of wine consist, are attracted more by each other, than by the particles of the saline body immersed in them.

Theorem 24.—If corpuscles floating in a fluid, and tending towards each other, be elastic; after their concourse, they will mutually recede from each other, and then again striking against other corpuscles, they will be reflected a second time: whence there will arise innumerable conflicts with, and continual resilitions from other corpuscles; and by their attractive force, their velocity will be continually increased, and there will be a sensible intestine motion of the parts: but according as the mutual attraction of the corpuscles is stronger, or weaker, and according to their different elasticity, these motions will be various, and become sensible in different degrees, and at different times.

Theorem 25.—If corpuscles, attracting each other, mutually touch, no motion will ensue: for they cannot approach nearer; if placed at a very small distance from each other, there will be a motion; but if at a greater, they will attract each other with no greater force, than they attract the intermediate particles of the fluid; and therefore no motion will be produced.—On these principles all the phænomena of fermentation and effervescence depend: and hence appears the reason, why oil of vitriol, into which a little water is put, has an ebullition and effervescence; for, on pouring in of the water, the saline particles are forced from their mutual contact; whence necessarily a motion arises, since they attract each other more than they attract the particles of the water, and are not equally attracted on all sides. Hence also appears the reason, why so great an ebullition is produced, on throwing into the said mixture the filings of steel; for the particles of the steel have a great elasticity; whence a strong reflection or resilition arises: hence also the reason may be assigned, why some menstrua, if diluted with water, act with greater violence upon, and sooner dissolve any body.

Theorem 26.—If corpuscles, attracting each other, have no elasticity, they will not be reflected from each other, but form a congeries or moleculæ of particles, whence a coagulum will arise: and if the gravity of the said congeries of particles exceed that of the fluid, a precipitation will also ensue: which may likewise arise from the increase or diminution of the gravity of the menstruum, in which the corpuscles swim.

Theorem 27.—If the figure of corpuscles attracting each other, and floating in a fluid, be such as to have a greater attraction, as also a larger contact in some given parts, than in others; these corpuscles will unite into bodies of given figures: and hence crystallizations will arise, and the figures of the component corpuscles may be determined by geometry, from the given figure of the crystal.

Theorem 28.—If corpuscles be attracted more by the particles of a fluid, than by each other; they will fly as it were, and recede from each other, and be soon diffused through the whole fluid.

Theorem 29.—If any corpuscle interpose between two particles of a fluid, whose two opposite superficies have the greatest attractive forces; this intermediate corpuscle will unite to itself the particles of the fluid; and many such corpuscles, diffused through the fluid, will form all its particles into a compact body, and reduce the fluid into ice.

Theorem 30.—If any body emit a large quantity of effluvia, whose attractive forces are very strong; when these effluvia approach any lighter body, their attractive forces will at length exceed the gravity of that body, and the effluvia will attract that body up towards themselves: and since the effluvia are much more copious at smaller, than at greater distances from the emitting body, the light body will always be attracted towards the denser effluvia, till at length it adhere to the emitting body. Hence a great many of the phænomena of electricity may be explained. Perhaps some may object against this doctrine, and say, that if this attractive force were inherent in all matter, then the more ponderous bodies, and such as contain a greater quantity of matter in a given space, would attract more than the less ponderous, which is repugnant to experience; but this objection may be easily answered in this manner: the particles of the last composition, (to which alone the attractive force is ascribed) when placed close to each other, may constitute a ponderous body, though of themselves they may be more rare than the particles of the last composition, that constitute a light body, being more remote from each other, and having more and larger pores. Several other phænomena of nature, Mr. Keill thinks, may be explained by the same principles; as the ascent of the sap in plants and trees; the determinate and constant figures of leaves and flowers, and their specific virtues, &c. As also several things that duly occur in the animal economy; particularly what relates to the circulation and secretion of the fluids, and which depend on the same qualities of matter: and hence the theory of diseases, and the effects of medicines are best investigated.

Microscopical Observations on the Tongue. By Mr. Anthony Van Leuwenhoeck, F.R.S. N° 315, p. 111.

Having taken some neats' tongues, and separated some thin parts of the outer skin, where I conceive is the place that admits the juices into the tongue, by which that sensation is produced which we call the taste; I separated those aforesaid external particles as well as I could from those that lay under them, and observed that the latter, that is, the internal, were furnished with a multitude of pointed particles, the tops of which were mostly broken off, and remained sticking in the outer skin: and one of those internal particles of the tongue, before a microscope, appeared as a transparent body, something larger than a thimble, having small internal holes or cavities, through which a greater quantity of light was admitted, than by the other parts; and it seemed that the extreme parts of those cavities had exceedingly small orifices in them. On viewing with a microscope that space of the tongue which is between the protuberances, it was all over covered with abundance of exceedingly small rising round particles, so close to each other, that you could not put in two hairs between them. I stripped off also the surface of the tongue with a sharp knife, and repeated the same a second time, and then discovered an unspeakable number of small holes, some of which seemed to be filled, others were cut through lengthwise. From this appearance I inferred, that when we press our tongues against the roof of the mouth, in order to taste any thing, the said long particles, the ends of which are exceedingly slender, press through the uppermost skin, which at that place is also very thin, and endued with small pores or holes, and so receives a little juice; from all which proceeds the kind of sensation, which we call taste. Similar appearances, on examination, were also observed in the tongues of hogs.

I have often thought that our taste proceeds alone from the tongue, but within these few days I am become of another opinion; for when I viewed that part of the roof of the mouth, opposite to the top of the throat, where the notched or jagged parts of the hog's tongue are determined, I judged that to be the place from whence the head partly discharges itself, and the matter to be cast out, which comes into the mouth without its proceeding from the lungs; as also that there are a great many parts in it, which receive the matter which we call the taste: but this wants a further inquiry.

Concerning the Migration of Birds. By the Rev. William Derham, F.R.S. N° 315, p. 123.

What I would suggest concerning the migration of birds is, that the members of the society would note down the very day they first see or hear of the approach of any of the migratory birds. And it may be convenient also to observe how the winds sit at the same time, especially towards the sea coasts. The several observations ought to be communicated to the society. Which when compared together, we may probably make a good guess which way those birds come, whether from the east, or any other point. The jynx or wryneck, for instance, which I take to be undoubtedly a bird of passage, I first heard this year on March 29, the wind S. or S.W. that and the preceding day, but E. before. The certhia also, or creeper, which leaves us in Essex till the spring, but whether a bird of passage I cannot tell, this bird I saw first on March 23, the winds that day varying from S. to N. but blowing strongly the day before from W. Now if those birds in the more westerly, or any other parts, at 100, 200, or more miles distance, should be discovered to come sooner or later, we might conclude, that accordingly they came from eastward, westward, or other point, especially if about the same time the winds seemed to favour their flight. These are all the migratory birds I have seen as yet this year. But for a further sample I shall annex my observations of last year, viz. the swallow came March 31, making a great outcry at his approach, as if he saw something strange. April 1, the jynx first yelped here. April 4, I first espied the ruticilla or redstart. The 5th I saw the martin. The 6th, the nightingale first sang with us. The 7th, the cuckow, I was told, was heard, and the 9th I heard it myself. The 17th, I heard the swift or black martin squeek in a hole in my house, in which he has quietly built for several years: but it being cold weather, he did not fly abroad till some days after.

Microscopical Observations on Red Coral. By Mr. Anthony Van Leuwenhoeck, F.R.S. N° 316, p. 126.

I had formerly several times slit, both in length and thickness, pieces of blood coral, that were very fair, and of a shining redness; and cut off as thin scaly particles as possible, that I might discover the vessels in it; in doing which, I fancied that sometimes I could perceive some very small orifices of the said vessels, but so exceedingly minute, that I could make no certain observations of them, though I could easily perceive, that in the parts which I had cut through across, there ran such fibres from the centre to the circumference, as are found in roots; and notwithstanding all my endeavours I could not find any pores in them, at least so as to say any thing certain of them; but it seemed as if most of the coral consists of roundish particles, such as some fruits are composed of; but their roundness was such, that they were in a manner all of different figures, such as might best suit with all the rest, and so as to leave no vacuity in them; and thus the sap, which is not in the vessels, is conveyed from one of those round parts to the other, and so they serve for canals.

In order to account for those pieces of blood-coral that are preserved as rarities, and resemble little trees with their branches, fastened to stones or other substances, I suppose that coral, while growing at the bottom of the sea, is very soft; and that those plants of coral, or the branches, being broken off by the coral fishers, the thick ends of them may accidentally fall upon a stone, or some other substance; and by reason of the said softness, and of a glutinous matter with which it is endued, might be fastened to the stone, &c. I have two pieces of coral no thicker than a hen's quill; one of these I broke into several pieces, and found in three places cavities, that occupied more than half the space of the coral itself, and between these cavities the parts of the coral were solid and close. In each of those cavities there was a thin membrane, that resembled a bit of a dried leaf, because the long parts, that appeared in great numbers in them, seemed to be canals or vessels; but, on a strict examination, I judged them to be coagulated salt particles, and the rather, because they were sharp at both ends. But I own, that I cannot conceive how such particles should be found in the middle of the coral, especially if we allow that substance to receive its nourishment and increase in the same manner as other plants. For further satisfaction, I took a little piece of very fine shining red coral, and put it into the fire, when a little heat caused the fine red to vanish, and turn it to an ash colour. Its superficies still retained the same smoothness, but under it the particles seemed like ribs extended lengthwise in the coral; and these rib-like particles composed a circle of round scales, and several roundnesses were made by those rib-like particles, till the middle point of the coral, in which there was no opening, became a long single particle. Having observed that the coral, by heating, did not burst in pieces, but was only split or rent in one place, I imagined that the matter, which was driven out of the coral by the heat of the fire, evaporated through that rent; or else that the parts of the coral being opened by the heat, the moisture in the internal parts might be drawn up towards the external ones.

Afterwards I put a small piece of coral into a glass, and the glass into the fire, increasing the heat till the glass was melted; then preserving the matter that was extracted as well as I could, and viewing it through a microscope, I discovered a waterish moisture, which to the naked eye seemed yellowish, and mingled with a vast number of small particles, which made the liquor thick and turbid, as also a yellowish oil, which, where it lay thickest, was of a reddish colour.

I also laid some other pieces of coral upon glowing wood coals, and put them into so great a heat, that the colour turned from red to a fine white; and in that condition I threw it into some clear rain water, then its parts were immediately separated, and most of it changed into a white and chalky-like substance; and the reason why all the rest of the coral was not dissolved was, because a sufficient heat could not reach it; for when I took the remainder, and heated it as I had done before, the same effect was produced in that likewise. The water in which the coral was quenched had not stood a minute, when I perceived a scum or film of salt particles, with which it had been impregnated, upon its surface, and consisting of such a vast number of small salts joined together, that it was impossible to discover their figure.

After the water had stood some hours, I observed abundance of salt particles, and of such several figures, that it was impossible to describe them; some of them were clear as crystal, and it was very pleasant to see so many several figures of such different shapes and sizes, lying together in so narrow a compass, and fine and shining as they were when surrounded with water: no less dark were almost all of them when the water was evaporated, when they appeared as if dissolved into a great many small particles, seemingly of a whitish substance. At another time I fancied that I saw the watery part, which lay about those salt particles, impregnated with abundance of other salts, much smaller than the former, which in evaporating the water, were coagulated on the first mentioned clear salts, and so obscured their shining. After some days, decanting the water off the whitish matter, and pouring fresh upon it, I observed that there were salt particles still coagulating on the superficies of the water, which were extracted from the coral in vast numbers. From hence we may conclude, that the hardness of the coral proceeds only from the great number of its fixed salts.

Now as the heat of the fire was sufficient to take away the redness of the coral as soon as put into it, I laid three small pieces of coral on aquafortis, to try whether that would have the same effect; when immediately the air bubbles, which came out of the coral, took up four times the space the aquafortis had filled before, and the coral, by the great multitude of air bubbles that continually proceeded from it, some of which also adhered to it, was raised from the bottom to the surface of the water; yet the said water was not in the least tinged with the red coral, but it became whitish, which was occasioned by all those parts separated from the coral; and when the aquafortis had no more power to dissolve the coral, because I had poured only a very little upon it, the said dissolved parts subsided to the bottom, and the superficies of the aquafortis resumed its former clearness. After the aquafortis had stood about two hours on the red coral, I took a little of the whitish matter that had sunk to the bottom of the glass, and putting it on a clean glass plate, I discovered a vast number of oblong particles, that seemed like very fine hairs. I took also some of the upper part of the aquafortis, that was clear, and pouring it on a clean glass, I likewise discovered in it a great many oblong particles, like the former; and on examining more strictly the white matter that had subsided to the bottom of the glass, I found it consisted only of the slender particles above-mentioned, some of which were longer than the rest.

The aquafortis having not been sufficient to dissolve all the coral, I added a little more to it, and then observed, that in a short time the remaining part of the coral was dissolved, saving that a very few parts, which were composed of much smaller, (or rather those smaller were again coagulated) floated on the top of the water, but it was impossible to discover their figure; and then the aquafortis which had been impregnated with the red coral was very clear, but when I came to view it through a microscope, I discovered, that there were still a vast number of the long particles floating in it. On placing the said very small particles before a greater magnifier, I discovered such exceedingly slender particles, as almost escaped the sight, and which I suppose were altogether invisible before. With this glass I discovered long particles, which not only exceeded the rest in length, but also in thickness, and the ends of which were obtuse; and having discovered in some few of them three distinct sides, I concluded that they were of a hexangular figure, and consequently that they were particles of saltpetre.

From these observations, I considered whether all those salt particles, in which I had discovered so many different figures, were not originally of the same shape with those very slender salt particles I discovered on dissolving the coral in the aquafortis, notwithstanding they were a thousand times smaller than they appeared through the microscope; and the difference of their figures may perhaps be only occasioned by the accession or coagulation of other particles, which may be greater or less in one place than in another; and accordingly their figure and size be determined by their nearness to or distance from one another.

After this I broke off two small pieces of red coral, and placed them on a piece of wood coal, which I made red hot by blowing on it the flame of a wax candle; and in that condition threw them into a little clean rain water, and presently observed the coral to be dissolved into a fine white substance, and soon after the matter overspread with a scum, which gradually increased in thickness; and about two hours after, among the infinite number of exceedingly small salt particles, I saw some of a larger size coagulated, agreeing with the above salt particles; in short, one would imagine that the salt particles that were separated from that little piece of coral, and which were coagulated in and upon the water, made altogether a greater body, than even all the parts of the coral itself would amount to.

Having been informed, that a certain physician made use of coral in his medicines; and being myself of opinion, that coral can be of no manner of service to the bodies of men, I beat some of the finest coral to powder, then mixed it with fair rain water, and caused the mixture to boil; then put some drops on clean glass plates to evaporate; after which there remained nothing more than by evaporating rain water alone; so that nothing of the coral was dissolved by the boiling. From whence I conclude, it is impossible that those fixed salts, of which coral is for the most part composed, can possibly be dissolved in our body, but only by sharp salts or by fire; and consequently, that it is altogether an unprofitable thing in physic.

Concerning some Roman Coins found in Yorkshire. By Mr. Ra. Thoresby, F.R.S. N° 316, p. 134.

Some Roman coins were ploughed up at Cookridge, which are a confirmation of the following conjecture, viz. that the Roman via vicinalis (which comes from the great military road upon Bramham-Moor) passed from that station at Adellocum (of which there is an account N° 282 of the Phil. Trans.) through these grounds to Ilkley. The coins are mostly very fair. The oldest is of Domitian, An. Urb. Cond. 846, which coincides with A. D. 95; his head is surrounded with this inscription, IMPerator CAESar DOMITianus AUGustus GERManicus P. M. (Pontifex Maximus) T. R. P. (Tribunitia Potestate) XIIII. The reverse shows that he was then saluted Emperor the 22d time, IMP. XXII. COS. (consul.) XVI. CENS. PP. (Pater Patriæ); the Flavian family particularly affected the title of Censors, and Domitian is the last emperor who has that title upon his medals; the figure on this reverse has a helmet on the head, and a spear in the right hand.

The next is of Nerva's.—IMP. NERVA. CAES. AUG. PM. TRP. COS. Reverse CONCORDIA EXERCITUUM. Dextræ junctæ. The next seven are of Trajan's, but all different.—IMP. CAES. NERVA. TRAIANUS. GERM. REV. PM. TRP. COS. III. PP. figura stolata stans, sinistra cornucopiæ. The next has the same inscription, save that it was in his 4th consulship, and has figura galeata cum hasta. The rest that are legible, are of Hadrian's, viz.—HADRIANUS. AUGUSTUS. Rev. COS. III. victoria cum palma. The other has upon the reverse, FELICITATI AUGUSTI. Above the navis prætoria, and below it COS. III. PP. These are all of silver: there was a large one of copper of the Emperor Domitian,—IMP. CAES. DOMIT. AUG. GERM. .... but the reverse was not legible.

One is inscribed HADRIANUS AUG. COS. III. PP. Rev. RESTITUTORI HISPANIÆ. This was upon his peaceable settling of affairs in that his native country; Spain is represented here as a woman with a branch in her right hand to denote her fruitfulness, kneeling before the emperor, who kindly takes her by the hand to raise her up.

By these it appears, that this station flourished when the Roman empire was in its prime; and there being none of a later date, makes it probable, that it perished in some of the insurrections of the native Brigantes, about Severus's time, as it was conjectured from the form of the letter A in the inscriptions upon the funeral monuments. Phil. Trans. N° 282.

An Account of a Storm of Thunder and Lightning, at Ipswich. By Orlando Bridgman, Esq. F.R.S. N° 316, p. 137.

On the 16th of July, 1708, at Ipswich, a most violent storm of thunder and lightning took place, the effects of which have been both wonderful and dismal. About 6 in the evening it was to be perceived at some distance, in the south west. I happened then to be on the highest eminence about this town, from whence I could very plainly distinguish the working of the storm, which I judged to be about 4 miles distance from me: the instant I perceived the flash, it seemed to extend itself like a bow, and cast its light a considerable way round it; and the shaft of lightning did not run in a waving angular figure as usual, but in a straight shaft of fire, like the fuze of a bomb, directly from the cloud to the ground. Soon after there were two or three of the greatest flashes of lightning, and the noise of thunder that succeeded them was so very great, and caused so great an emotion in the air, that it made the rooms shake, and the windows rattle, as in a great storm of wind. Dr. Dade assured me, that the lightning seemed to continue some considerable time on the ground, and that he could sensibly feel its heat in his face. The passage-boat was at that time coming from Harwich, and just got to the town, or very near, when a terrible flash killed the master and three more that were on board. I saw one of them the next day; he had a wound in his thigh, his breast was lacerated as if he had been whipped with wires, and his face and body as black as if he had been blown up with gunpowder, and thousands of small black spots about him. The master of the vessel was not at all disfigured, but had one wound on his side, like a fresh burn, but no other mark about him, only the chain of his watch was melted, and no harm or burn could be perceived on his breeches or cloaths. The third person was very much torn and shattered about the head; the crown of his hat was taken clear out, as if it had been cut out, and several parcels of his hair driven into the substance of the hat. The fourth was very little disfigured, only he had a black spot on his side, and a small wound, as if made with a cauterising iron. Several others on board were wounded and stunned. One of them had his hair burnt close to his head behind; but his peruke untouched; he had a scratch on his arm about 4 inches long, and a small hurt below the elbow; he fell that night into a violent fever, grew delirious, and if not dead yet, is pronounced irrecoverable; whether he received any hurt on his brain, or the violence of the fever causes the delirium, remains undetermined; there was no mark to be seen on his coat, waistcoat, or shirt, where he had the hurt on his arm. Two of the persons killed were on the outside, and the other two under the tilt of the boat; and, what is remarkable, the two that were within the tilt, sat on each hand of a woman who received no damage: one person had the sole of his shoe unripped from the upper leather, but no hurt. There was another boat that followed them, but it received no damage; the master of which affirms, that he saw the fire light on the bow-sprit of the former boat, where, meeting with a small resistance, it flew into small streams like a rocket, part into the boat, part into the water; which might be the cause of the mischief being done in so many different parts of the boat; and does in some measure solve the seeming difficulty of the woman's being unhurt between the two persons that were killed.

Concerning the Effects of the abovementioned Storm of Thunder and Lightning at Colchester. By Mr. Jos. Nelson. N° 316, p. 140.

On Friday July 16, 1708, about 8 o'clock at night, I heard a thunder-crack so loud, as if it were close to me, and such as I never heard before; at which time the thunder and lightning broke into Mr. King's house, beginning at the south-side at the gable-end, breaking several roof-tiles, and continuing its course in a perpendicular straight line, it entered into the strong-beer buttery through the laths, and forced a cork out of the lower tap-hole of a butt; in its way, it shivered a stud about three inches square, so that one side remained nailed to laths, yet not much thicker than a lath, and also broke it in two. Below the beam it clave or split a stud, about 4 inches square, several feet downwards, caused by its violent grazing on the outside, attended with a strong sulphureous smell. It threw the broken wall to several rods distance. Some damage was also done to Allhallow's church in the said town.

About the same time some boats were carrying persons from Harwich to Ipswich on the Orwell; when the violence of the thunder and lightning killed four of them dead immediately, made a young lad go mad, and wounded the rest that were in that boat, being 12 in number; it melted a watch and the chain all of a lump, which was in one of the dead men's pockets. There was an intolerable smell of sulphur. This was about 18 miles N.E. of Colchester; but at only one mile S.E. it was no more violent than an ordinary storm.

On the Manuring of Land in Devonshire with Sea-Sand. By Dr. Arthur Bury. N° 316, p. 142.

The burning of the surface of the land is so much practised in Devonshire, that it is elsewhere known by the name of Devonshiring; but it is used only for bad lands, and by worse husbandmen; for it robs the ground, as the archbishop of Dublin remarks in N° 313, Phil. Trans.

Salt quickens dead land, and is used in the south-west part of that country, which would otherwise be the most barren, but is now the richest part of it. They go as far as the sea will permit at lowest ebb, there take the sand in bags, and carry it on horseback 14 miles into the country, and spread it upon the land, thereby improving it both for corn and grass. In other parts they force their barren land, by mingling the earth with lime, and casting it upon the ground. In this they differ, that crude salt alone, if strewed on the ground, does not improve, but corrode it; but lime, though unmingled, betters it: but in this they agree, that they produce not grass fit for the scythe, but for pasture, short and sweet, and growing all the winter; so that their sheep need not either hay or water, nor are their highest grounds parched by the sun in the hottest summer. The sea-salt is too strong and active of itself; the lime has a more balsamic and gentler salt; and when regularly joined with the other, it is thereby invigorated. How to mix these two, Glauber thus directs: "Take quicklime, let it slack by time, without water; then take salt and water, mingle them together, and make them into balls or pieces, which you please; dry them as you do bricks, then burn them for about two hours. This compost will enrich your poorest land."

An Account of a Book, intituled, Ουρεσιφοιτης Helveticus, sive Itinera Alpina Tria, &c. Authore Joh. Ja. Scheuchzero, M. D. Lond. 4to. 1708. By Mr. Edward Lhuyd, Keeper of the Ashmolean Museum, in Oxford. N° 316, p. 143.

In the three journies among the Alps, of which Dr. S. has favoured the public with an account, he considers the nature of the Alpine waters and meteors; the height, &c. of the mountains, and the most remarkable minerals and plants they afford. He has also some occasional observations on animals; and others on the practice of physic; as well as on the customs and diet of the inhabitants: with some instances of the superstition of the common people, and a few notes relating to antiquities. The whole is illustrated by numerous plates.

Of an Ideot at Ostend, who swallowed Iron, &c. with two other Surgical Cases. By Mr. Claud. Amyand. N° 317, p. 170.

A person who had been an ideot from his infancy, died at Ostend, in the 33d year of his age; his death having been preceded by 12 days continual fever, and a considerable tumor and pain about the region of the liver. For 6 weeks before his death, he spit or vomited blood, and had bloody stools. On opening the body, a large abscess or imposthume was found in each lobe of the liver; the stomach was extremely contracted and ponderous, containing 9 cart-wheel nails, and 6 others of a less size; also a large and long iron screw; two pair of compasses, the one having a circle 2 inches in diameter; a middle size key; a large iron pin, as thick as my thumb, and 4 inches long, with a ring at the end of it; another of brass, but much less; the handle of an iron spring-knife, swallowed as it is believed entire, but the sides and two pieces forming its spring, found asunder; though the pegs of the knife, tying those several pieces together, were not found; the upper and lower ends of a brass pommel of a fire grate, weighing 9 ounces; a broad piece of lead weighing three ounces and a half: the whole consisting of 28 pieces, weighing between 2 and 3 pounds. These had been missed, and much sought for by his brother, at different times. They were found all in a bundle, with the larger ends one way, and the smaller the other; the small end of one of the large nails was so bent, that it would have made a perfect circle, had not the very tip of that same nail been bent back again; this end was forked and very sharp, as were likewise the points of the compasses. None of the pieces were found polished, nor was the brass nor the lead anywise impaired or damaged; but the iron pieces were exceedingly corroded, especially one side of the knife, which had lain in the stomach about 8 months, was eaten quite through in two or three places, towards the end of the blade; and three or four nails greatly corroded, as if some particular menstruum or dissolvent had been poured upon them, but all the other metals untouched: the lead had lain in the stomach about 8 months, and the brass pin above 12. It was very easy to guess at the time those different pieces of iron had been in the stomach, by considering how much one piece had suffered more than the other. This observation seems to be inconsistent with the notion of those who believed that ostriches dissolve brass and iron by friction only; for if so, I see little reason why the iron legs of the compasses should have been found so very much worn out, and the brass parts not in the least impaired. I was told by the surgeon who opened him, that the stomach had been no ways wounded or damaged; which does not appear to me probable, as the patient was known to have vomited and evacuated blood by stool for 6 weeks before he died.—It is to be wished the gullet and guts had likewise been opened; for it is plain, that some of the pieces had passed the pylorus, as the pegs of the knife; and perhaps some smaller pieces than those that were found in the stomach. This ideot from his youth had accustomed himself to swallow large morsels, without chewing; which, doubtless, made the passage of the œsophagus wider, and disposed it to give entrance to all those extraneous bodies. It may be also remarked, that this ideot, (and sometimes madman) was never known to sleep a wink, though he was often compelled to go to bed, and had been very much harassed and fatigued; he always eat three times as much as other persons, and when furious, was quieted by giving him victuals.

I was shown the largest tumour I ever yet saw; it is of a scirrhous nature, arising from the thigh-bone, somewhat tending to be cancerous. It first took its rise about 2 years ago, in a child of 10 years old, just above the patella, without any evident cause; and notwithstanding all possible care, has expanded itself in such a manner, that it now occupies the whole thigh to the very groin, and has extended it to above a Dutch yard in circumference, and daily increases very much.

In the hospital at Ghent I observed a very remarkable fracture of the skull, in the interior part of the squamose bone, occasioned by a splinter of a fellow soldier's piece bursting, that struck the patient there. Some time had passed, before the accident made us suspect a fracture, and obliged us to make a triangular incision on the temporal muscle; when a fissure was discovered, which indicated the necessity of the trepan. It was applied twice, the first not making room sufficient to extract a large piece of the internal table, very much depressed. After this, all the symptoms disappeared; but 12 days after the operation, rigors, cold sweats, an intermitting pulse, and some other signs of an approaching death, made us despair of the recovery of our patient. He died the 15th day from the operation, and about the 20th from his wound. His skull was opened, and in it three very remarkable fissures were observed. The first, notwithstanding the sagittal suture, had crossed from one parietal bone to the other, as far as the coronal suture on that side opposite to the wound; another had gone across the coronal bone; and the third was in the parietal bone on the side of the wound, pretty near the sutura squamosa; but, what is most remarkable, none of these fissures reached that on which the trepan had been applied. An empyema was found in the thorax, and a considerable imposthume in the liver.

Of the Laws of Centripetal Force. By Mr. John Keill, of Christ-church College, Oxford, A. M. N°. 317, p. 174. *Translated from the Latin.*

The learned Mr. Halley having showed me a theorem, by which the law of centripetal force can be exhibited in finite quantities, which was communicated to him by Mr. Demoivre, who said that Mr. Is. Newton had before discovered a similar theorem; and as the demonstration of this theorem is very easy; I wish to communicate it to the public, with some other thoughts on the same subject.

THEOREM. If a body, urged by a centripetal force, move in any curve; then, in every point of the curve, that force will be in a ratio, compounded of the direct ratio of the body's distance from the centre of force, and the reciprocal ratio of the cube of the perpendicular on the tangent to the same point of the curve, drawn into the radius of curvature of the same point.

Demonstr. Let QAO, fig. 1, pl. XI. be any curve, described by a moving body, urged by a centripetal force tending to the point S. And let AO be an arc described in any very small time, PM its tangent, AR the radius of the circle of equal curvature, that is, of which the element of the periphery coincides with the arc AO. And let SP be a perpendicular on the tangent; also draw OM and ON parallel to SA and SP. Let OM express the force by which the body at A is urged towards S: then the force by which the body recedes perpendicularly from the tangent, will be as ON; that is, the force tending towards R, and causing the body, moving with the same velocity as before, to describe a circle equicurved with the arc AO, will be to the force tending towards S, by which the body moves in the curve AO, as ON to OM, or, by equiangular triangles, as SP to SA. But the centripetal forces of bodies, moving in circles, are as the squares of the velocities applied to the radii, by the Cor. to Theor. 4, Newton's Principia; and the velocity is reciprocally as SP; therefore the force ON, or the force by which the body can move in an equicurved circle, will be as $\frac{1}{SP^2 \times AR}$: but it has been shown that SP is to SA, as the force tending towards R, by which the body can move in an equicurved circle, is to the force tending towards S; and the force tending towards R is as $\frac{1}{SP^2 \times AR}$; therefore as SP : SA :: $\frac{1}{SP^2 \times AR} : \frac{SA}{SP^3 \times AR}$, which therefore is as the force tending towards S. Q. E. D.

Corol. If the curve QAO be a circle, the centripetal force tending towards S, will be as $\frac{SA}{SP^3}$, fig. 2. Therefore if the centripetal force tend towards S, a point situated in the circumference, then, by Eucl. 32, iii, the angle PAS = the angle at AQS; therefore by the similar triangles ASP, ASQ, it will be AQ : AS :: AS : SP; therefore SP = $\frac{AS^2}{AQ}$, and $SP^3 = \frac{AS^6}{AQ^3}$; hence $\frac{SA}{SP^3} = \frac{SA \times AQ^3}{AS^6} = \frac{AQ^3}{AS^5}$; that is, because AQ is given, the force will be reciprocally as AS^5.

Let DAB (fig. 3) be an ellipsis, whose axis is DB, its foci F and S, also AR and OR two perpendiculars to the curve very near together: draw KL and OT perpendiculars on SA, and KM perpendicular to OR. Then because SA : SK :: (by Eucl. 3, vi) FA + SA : FS, that is in a given ratio, the fluxions of SA, SK, that is AT, K*h*, will be proportional to SA, SK: also, by Conic Sections, AL = half the latus rectum = $\frac{1}{2}$L: further, because KA is parallel to SP, the angle ASP = KAL = TOA, because TAO is the complement of each to a right angle; therefore KA : AL :: SA : SP, hence $SP = \frac{L}{2} \times \frac{SA}{KA}$, and $KA = \frac{L \times SA}{2SP}$.

Again, since by the equiangular triangles KM*k*, GPS and OTA, SPA,

it is KM : K*k* :: GP : GS :: AP : SK

also K*k* : AT :: . . . . SK : SA

and AT : AO :: . . . . AP : SA,

it will be KM : AO :: $AP^2 : SA^2 :: SA^2 - SP^2 : SA^2 :: SA^2 - \frac{L^2 \times SA^2}{4AK^2} : SA^2 :: 4AK^2 - L^2 : 4AK^2$; hence $L^2 : 4AK^2$ :: (AO — KM : AO ::) AK : AR, and therefore $AR = \frac{4AK^3}{L^2}$. In like manner $\frac{4AK^3}{L^2} = \frac{L \times SA^3}{2SP^3}$ is found equal to the radius of curvature in the hyperbola.

But in the parabola the calculation will be easier. For, because of the subnormal being given, it is always K*k* = AT the fluxion of the axis, fig. 4; and the equiangular triangles K*k*M, ATO, SPA, AKL, hence KM : K*k* :: AP : SA; also AT or K*k* : AO :: AP : SA; hence KM : AO :: $AP^2 : SA^2 :: SA^2 - SP^2 : SA^2$; hence it will be $SP^2 : SA^2$ :: AO — KM : AO :: AK : AR, and therefore $AR = \frac{SA^2 \times AK}{SP^2}$: but AL = half the latus rectum = $\frac{1}{2}$L, and AK : AL :: SA : SP; therefore SP =

$\frac{L \times SA}{2AK}$, and $SP^2 = \frac{L^2 \times SA^2}{4AK^2}$; therefore $AR = \frac{4AK^3}{L^2} = \frac{L \times SA^3}{2SP^3}$, because $AK = \frac{L \times SA}{2SP}$.

And hence arises a very easy construction, for determining the radius of curvature in any conic section. For let AK be perpendicular to the section, meeting the axis in K, fig. 5, erect KH perpendicular to AK, meeting AS produced in H; erect HR perpendicular to AH; then will AR be the radius of curvature. In the parabola the construction becomes a little simpler. For since, by the nature of the parabola, it is SA = SK, and AKH is a right angle, S will be the centre of a circle passing through A, K, H; whence the radius of curvature is found by producing SA to H, till SH = SA, and erecting the perpendicular HR: then R will be the centre of the circle osculating the parabola at A.

The centripetal force tending to the focus of the conic section, in which the body moves, is reciprocally proportional to the square of the distance. For since $AR = \frac{L \times SA^3}{2SP^3}$, it will be $\frac{SA}{SP^2 \times AR} = \frac{SA \times 2SP^3}{SP^3 \times L \times SA^3} = \frac{2}{L \times SA^2}$; that is $\frac{1}{SA^2}$ is as the centripetal force, because L is a given quantity.

Let BAD, fig. 6, be an ellipsis, to which GE is a tangent at A; and to which tangent AK and PS are perpendicular, and S the centre. Then SP × KA will be equal to a 4th part of the figure of the axis, or = the square of the less semiaxis = BO × DE. For because of the equiangular triangles, GBO, GLA, GAK, GPS, GDE, it will be,

$$SP : SG :: BO : GO$$
$$SG : GD :: BG : LG :: GO : GA$$
$$DG : DE :: GA : AK,$$

hence SP : DE :: BO : AK; and SP × AK = DE × BO = $\frac{1}{4}$L × SB.

Hence, if a body move in an ellipsis, with a centripetal force tending to its centre, that force will be directly as the distance. For it is $\frac{SP^3 \times 4AK^3}{L^2}$ = a given quantity, because SP × AK is given. Therefore the force, as $\frac{SA}{SP^3 \times AK^3}$, will be as the distance SA.

In fig. 3, from the other focus F drawing FI perpendicular to the tangent; then by the equiangular triangles SAP, FAI, it will be SA : SP :: FA : FI = $\frac{SP \times FA}{SA}$; hence it will be SP × FI = $\frac{SP^2 \times FA}{SA}$ = the square of the less semi-axis. So that, if the greater axis be called b, and the less $2d$, it will be $SP^2 = \frac{d^2 \times SA}{b - SA}$, and $SP = d\sqrt{\frac{SA}{b - SA}}$.

But in the hyperbola it is $SP = d\sqrt{\frac{SA}{b + SA}}$.

And in the parabola it is $SP = \sqrt{d \times SA}$, putting $4d$ for its latus rectum.

Because $TA^2 : TO^2 :: AP^2 : SP^2 :: SA^2 - SP^2 : SP^2 :: SA^2 - \frac{d^2 SA}{b - SA} : \frac{d^2 SA}{b - SA} :: SA - \frac{d^2}{b - SA} : \frac{d^2}{b - SA} :: bSA - SA^2 - d^2 : d^2$, it will be $\sqrt{(b\dot{SA} - SA^2 - d^2)} : d :: \dot{TA} : TO$; and since $\dot{TA} = \dot{SA}$, it will be $TO = \frac{d\dot{SA}}{\sqrt{(bSA - SA^2 - d^2)}}$.

Now let QAO, fig. 7, be any curve, the element of the arc being AO, to which AP and op are tangents, also AR the radius of curvature, and SP, sp perpendiculars to the tangents: then will $AR = \frac{SA \times TA}{fP}$. For, by similar triangles, it is fP : AO :: PA : RA,

and AO : TA :: $\dot{SA}$: PA; hence ex æquo it will be fP : TA or $\dot{SA}$ :: SA : RA;

but $fP = \dot{SP}$, therefore it will be $RA = \frac{SA \times \dot{SA}}{\dot{SP}}$.

Hence if the distance SA be drawn into its fluxion, and divided by the fluxion of the perpendicular, it will give the radius of curvature. By which theorem the curvature is easily determined in radial curves or spirals. For example, let AQ be the nautical spiral; then because the angle SAP is given, the ratio of SA to SP will be also given: let that ratio be a to b; then will $\mathrm{SP} = \frac{b\mathrm{SA}}{a}$, and $\dot{\mathrm{SP}} = \frac{b\dot{\mathrm{SA}}}{a}$, and $\mathrm{AR} = \frac{\mathrm{SA} \times \dot{\mathrm{SA}}}{\dot{\mathrm{SP}}} = \frac{a\mathrm{SA}}{b}$. Hence it plainly appears, that the evolute of the nautical spiral, is the same spiral in another position.

Because $\mathrm{AR} = \frac{\mathrm{SA} \times \dot{\mathrm{SA}}}{\dot{\mathrm{SP}}}$, it will be $\frac{\mathrm{SA}}{\mathrm{SP}^3 \times \mathrm{AR}} = \frac{\dot{\mathrm{SP}}}{\mathrm{SP}^3 \times \dot{\mathrm{SA}}}$. And hence again, from the given relation of SA to SP, the law of the centripetal force will be easily found.

Example.—Let VAB, fig. 8, be an ellipsis, whose focus is S, the greater axis $\mathrm{VB} = b$, the less axis $= 2d$, and latus rectum $= 2r$. And let VaQ be another curve, so related to this, that the angle VSA may be always proportional to the angle VSa; and let Sa = SA. Then it is required to assign the law of the centripetal force tending to S, by which the body may move in the curve VaQ.

Because the angle VSA is to VSa in a given ratio, the increments of these angles will be in the same ratio, which let be the ratio of m to n: hence it will be $ot = \frac{n}{m}\mathrm{OT}$. But $\mathrm{OT} = \frac{d\dot{\mathrm{SA}}}{\sqrt{(b\mathrm{SA} - \mathrm{SA}^2 - d^2)}}$; hence $ot = \frac{nd\dot{\mathrm{SA}}}{m\sqrt{(b\mathrm{SA} - \mathrm{SA}^2 - d^2)}}$. And because $\mathrm{SA}^2 + \mathrm{SP}^2 : \mathrm{SP}^2 :: ta^2 + ot^2 : ot^2 :: \dot{\mathrm{SA}}^2 + \frac{n^2d^2\dot{\mathrm{SA}}^2}{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2)} : \frac{n^2d^2\dot{\mathrm{SA}}^2}{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2)} :: \frac{n^2d^2}{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2)} : \frac{n^2d^2}{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2)} :: m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2) + n^2d^2 : n^2d^2$; hence $\sqrt{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2) + n^2d^2} : nd :: \mathrm{SA} : \mathrm{SP} = \frac{nd\mathrm{SA}}{\sqrt{m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2) + n^2d^2}}$. To find the fluxion of this, put x for $m^2(b\mathrm{SA} - \mathrm{SA}^2 - d^2) + n^2d^2$; then will $\mathrm{SP} = \frac{nd\mathrm{SA}}{\sqrt{x}}$, and $\mathrm{SP}^3 = \frac{n^3d^3\mathrm{SA}^3}{x\sqrt{x}}$: hence $\dot{x} = m^2b\dot{\mathrm{SA}} - 2m^2\mathrm{SA}.\dot{\mathrm{SA}}$, and $\dot{\mathrm{SP}} = nd\dot{\mathrm{SA}}x^{-\frac{1}{2}} - \frac{1}{2}n\mathrm{AS}\dot{x}x^{-\frac{3}{2}} = \frac{2nd\dot{\mathrm{SA}}x - nd\mathrm{SA}\dot{x}}{2x^{\frac{3}{2}}}$ by reducing to the same denominator. And substituting the values of x and $\dot{x}$, and reducing to order, it will be $\dot{\mathrm{SP}} = nd\dot{\mathrm{SA}} \times \frac{m^2b\mathrm{SA} - 2m^2d^2 + 2n^2d^2}{2x^{\frac{3}{2}}}$; hence it will be $\frac{\dot{\mathrm{SP}}}{\mathrm{SP}^3.\dot{\mathrm{SA}}} = \frac{\frac{1}{2}m^2b\mathrm{SA} - m^2d^2 + n^2d^2}{n^2d^2\mathrm{SA}^3}$. But the former of these being as the centripetal force, therefore the latter is so too: or, because n^2d^2 in the denominator is a given quantity, that force will be as $\frac{\frac{1}{2}m^2b\mathrm{SA} - m^2d^2 + n^2d^2}{\mathrm{SA}^3}$: or, putting $\frac{1}{2}br$ for d^2, and omitting the given quantity $\frac{1}{2}b$, the force is as $\frac{m^2\mathrm{SA} - rm^2 + rn^2}{\mathrm{SA}^3} = \frac{m^2}{\mathrm{SA}^2} + \frac{rn^2 - rm^2}{\mathrm{SA}^3}$. All which exactly coincide with what is delivered by Newton, on the centripetal force of a body moved in the same curve, in pr. 44 of the Principia.

Because the centripetal force tending to the point S, by which the body can move in a curve, is always as $\frac{\dot{SP}}{SP^3 . \dot{SA}}$; hence, from the law of the centripetal force being given, the relation between SA and SP may be found; and therefore, by the inverse method of tangents, the curve may be exhibited, which shall be described by a given centripetal force.

For instance, let the force be reciprocally as any power m of the distance, that is, let $\frac{\dot{SP}}{SP^3 . \dot{SA}} = \frac{b}{a^2 SA^m}$, it will be $\frac{\dot{SP}}{SP^3} = \frac{b\dot{SA}}{a^2 SA^m}$; then the fluents give $\frac{1}{2} SP^{-2} = \frac{bSA^{1-m} \mp e}{m-1 . a^2}$, and hence $\frac{m-1 . \frac{1}{2}a^2}{bSA^{1-m} \mp e} = SP^2$; and here multiplying the numerator and denominator by SA^{m-1}, and putting d^2 for $m - \frac{1}{2}a^2$, it becomes $\frac{d^2 SA^{m-1}}{b \mp eSA^{m-1}} = SP^2$; therefore $SP = a\sqrt{\frac{SA^{m-1}}{b \mp eSA^{m-1}}}$ or $= d\sqrt{\frac{SA^{m-1}}{b}}$ only, when e is equal to nothing.

Thus, if the force be reciprocally as the square of the distance; it may be either $SP = d\sqrt{\frac{SA}{b}}$, or $d\sqrt{\frac{SA}{b - SA}}$, or $d\sqrt{\frac{SA}{b + SA}}$; the curve, in the first case, being the parabola whose latus rectum is $\frac{4d^2}{4}$, in the second case it is an ellipsis, and in the third an hyperbola.

If the force be reciprocally as the cube of the distance, it may be either $SP = \frac{dSA}{b}$, or $= \frac{dSA}{\sqrt{b - eSA}}$, or $= \frac{dSA}{\sqrt{b + eSA}}$; in the first case the curve being the nautical spiral; in the second case the curve is the same as that which Newton has constructed by the sector of an hyperbola; and the third case the same as he constructed by elliptic sectors, in cor. 3, pr. 1, lib. 1, Principia.

If the centripetal force be reciprocally as the distance, the relation between SA and SP cannot be defined by any algebraic equation; yet the curve may be constructed by the logarithmic line, or by the quadrature of the hyperbola; for it is then $SP = \frac{d}{\sqrt{b - \text{log. of } SA}}$.

All these things follow from the now so much celebrated method of fluxions, of which our Newton was doubtless the first inventor, as will be evident to any one who shall read his letters published by Dr. Wallis. Yet afterwards the same method was published by Mr. Leibnitz in the Acta Eruditorum; only changing the name and the manner of notation.*

* This is the sentence which gave such offence to Mr. Leibnitz, that he made a formal complaint

Let now a body move in the curve QAO, fig. 1, by means of a centripetal force tending to S; and let the velocity of the body at A be called C, also the velocity with which a body, urged by the same force, at the same distance, can move in a circle, be called *c*. It appears from the first theorem, that if SA express the centripetal force tending to S; then the centripetal force tending to R, by means of which the body, with the velocity C, may describe a circle whose radius is AR, will be expressed by SP. But the centripetal forces of bodies describing circles, are as the squares of the velocities applied to the radii of the circles. Therefore it will be, $SP : SA :: \frac{C^2}{AR} : \frac{c^2}{SA}$; hence $SP.AR : SA^2 :: C^2 : c^2$, and $C : c :: \sqrt{SP.AR} : SA$.

If SP coincide with SA, as is the case in the vertices of the figures, it will be $C : c :: \sqrt{AR} : \sqrt{SA}$. So that if the curve be a conic section, AR the radius of curvature at its vertex, is equal to half the latus rectum, or $\frac{1}{2}L$; and therefore the velocity of the body in the vertex of the section, will be to the velocity of the body describing a circle at the same distance, in the subduplicate ratio of the latus rectum to the double distance.

Since $AR = \frac{SA.S\dot{A}}{S\dot{P}}$, then $C^2 : c^2 :: \frac{SP.SA.S\dot{A}}{S\dot{P}} : SA^2 :: \frac{SP.S\dot{A}}{S\dot{P}} : SA :: SP.S\dot{A} : SA.S\dot{P}$. Therefore, from the given relation of SP to SA, the ratio of C to *c* will be given. For example, if the force be reciprocally as the *m* power of the distance, that is, if $\frac{S\dot{P}}{SP^3.S\dot{A}} = \frac{b}{a^2SA^m}$; then $S\dot{P} = \frac{bSP^3.S\dot{A}}{a^2SA^m}$; therefore $C^2 : c^2 :: SP.S\dot{A} : \frac{bSP^3.SA.S\dot{A}}{A^2SA_m} :: a^2SA^{m-1} : bSP^3$. Whence if we put $SP^2 = \frac{d^2SA^{m-1}}{b} = \frac{m-1.a^2SA^{m-1}}{2b}$, it will be $C^2 : c^2 :: a^2SA^{m-1} : m-1.\frac{1}{2}a^2SA^{m-1} :: m-1 : 2$; and therefore $C : c :: \sqrt{2} ; \sqrt{m-1}$.

But if $SP^2 = \frac{d^2SA^{m-1}}{b-eSA^{m-1}} = \frac{m-1.\frac{1}{2}a^2SA^{m-1}}{b-eSA^{m-1}}$, then $C : c :: a^2SA^{m-1} : \frac{m-1.\frac{1}{2}a^2bSA^{m-1}}{b-eSA^{m-1}}$, that is, as $b-eSA^{m-1}$ to $m-1.\frac{1}{2}b$. But the ratio of $b-eSA^{m-1}$ to $m-1.\frac{1}{2}b$, is less than the ratio of b to $m-1.\frac{1}{2}b$, or than the ratio of 2 to $m-1$; hence C will be to *c*, in a less ratio than $\sqrt{2}$ to $\sqrt{m-1}$.

In like manner, if there be taken $SP = \frac{d^2SA^{m-1}}{b+eSA^{m-1}}$, it will be found that C will be to *c*, in a greater ratio than that of $\sqrt{2}$ to $\sqrt{m-1}$.

Corol.—If a body move in a parabola, and the centripetal force tend to the focus S; then the velocity of the body, will be to the velocity of a body describing a circle at the same distance, every where as $\sqrt{2}$ to 1: for in that case

against Mr. Keill to the Royal Society, which occasioned the famous dispute concerning the invention of fluxions.

it is $m = 2$, and $m - 1 = 1$. But the velocity of a body in an ellipsis, is to the velocity of a body moving in a circle at the same distance, in a less ratio than that of $\sqrt{2}$ to 1. And the velocity in an hyperbola is to the velocity in a circle, in a greater ratio than that of $\sqrt{2}$ to 1.—If the body move in the nautical spiral, its velocity is every where equal to the velocity of a body describing a circle at the same distance: for in this case it is $m = 3$, and $m - 1 = 2$.

PROBLEM.—Supposing that the centripetal force (the absolute quantity of which is known) is reciprocally as the square of the distance, and a body is projected in a given direction, with a given velocity; to find the curve in which the body will move.

Let the body be projected in the right line AB, fig. 9, with the given velocity C. And since the absolute quantity of the centripetal force is known, there will thence be given the velocity with which a body can describe a circle at the distance SA, by the same force: for it is equal to that acquired by a body in falling through $\frac{1}{2}$ SA, while urged constantly by the same force. Let that velocity be c. Erect AK perpendicular to AB, in which take AR a fourth proportional to c^2, C^2, $\frac{SA^2}{SP}$; then AR will be the radius of curvature at A. Draw RH perpendicular to AS, and HK perpendicular to AR; then drawing SK, it will give the position of the axis. Make the angle FAK equal to the angle SAK. Then if FA be parallel to SK, the figure in which the body moves will be a parabola. But if it meet the axis SK in F; and if the points S and F fall on the same side of the point K, the figure will be an hyperbola; but if S and F fall on contrary sides, the figure will be an ellipsis. Hence, with the foci S, F, and the axis = SA ± FA, the section may be described, in which the body will move.

Concerning the New Philippine Islands. By the Jesuits Father Paul Clain and Father Le Gobien. N° 317, p. 189.

The Pintados are large islands separated from each other by arms of the sea, where ebbing and flowing renders their navigation very difficult and dangerous. At the town of Guivam, in the isle of Samal, or Ibabao, the last and most southern of the eastern Pintados, were 29 Palaos, or inhabitants of certain new discovered islands; the easterly winds, that blow on these seas from December to May, having driven them 300 leagues from their own islands, to this town, where they arrived in two small vessels, called paraos: of which adventure we received this following account.

They embarked to the number of 35 persons, to pass over to one of the neighbouring islands; when there arose a very strong wind, that forced them out into the main sea, so that they could not gain any of the neighbouring ones. They were thus driven before the wind for 70 days together, without being able to make any land. At last, out of all hopes of returning to their own country, and half dead through want of water and provisions, they resolved to give themselves up to the mercy of the winds, and land at the first island westerly that they should come to; and thus at length were driven to this coast, when a Guivamois, then on shore, perceiving them, and judging by the make and smallness of their vessels, that they were strangers, and out of their way, took a piece of cloth, and made them a signal of entering the road he directed, to avoid the shoals and banks of sand they would otherwise run upon. These poor people were so frighted at the sight of this stranger, that they began to put out to sea again; but notwithstanding all their endeavours, the wind forced them back a second time towards the shore. When they came near, the Guivamois again made the signal as before; but seeing they did not mind it, but would unavoidably be lost, he threw himself into the sea, and swam to one of their little vessels, on purpose to bring them safe into shore. He was no sooner got to them, but the women with their children at their backs, and all that were in that vessel, threw themselves overboard, and swam to the other; seeing himself alone in the vessel, he resolved to follow them, and getting on board the second, showed them how to avoid the shoals, and brought them safe to land. In the mean time they stood immoveable, and resigned themselves up entirely to the conduct of this stranger, as so many prisoners.

The inhabitants of Guivam, running to the shore, received them very kindly, and brought them wine and other refreshments. They eat cocoas very freely, being the fruit of the palm-trees of this country. They gave them rice boiled in water, which is eaten here and all over Asia, as bread is in Europe. They looked on it with surprise, and taking some grains of it threw it on the ground, imagining it to be worms. But they eat greedily of the large Palavan roots.

They brought to them two women, who had formerly been driven on shore on the coast of Guivam, and who understood a little the language of this country. One of the women found among those strangers one of her relations, who as soon as they knew each other fell a weeping. The inhabitants of Guivam strove with each other who should entertain these strangers at their houses, and furnish them with provisions and clothes, and other necessaries. Of the 35 persons that embarked there remained only 30, 5 dying through want of provisions and other hardships in so long a voyage, and some time after their arrival here another died.

They relate that their country consists of 32 islands, which cannot be far distant from the Marianas, as may be judged by the make and smallness of their vessels, and the form of their sails, which are very like those of the Marianois. It is likely that these islands may be 11 or 12 degress of latitude more southerly than the Marianas, and under the same degree of longitude* as Guivam; for sailing directly from east to west, they came ashore at this town. It is also probable, that it was one of these islands that was discovered some years ago at a distance, when a ship belonging to the Philippines, leaving the common course, which is from east to west under the third degree of longitude,* and running farther to the south-east, first perceived it. Some called this island Carolina, from Charles II. of Spain, and others the island of St. Barnaby, because discovered on that apostle's day. It was again seen last year, 1695, by another vessel, driven out of its course by a storm, in going from hence to the Marianas. These strangers add, that of these 32 islands, 3 are uninhabited, unless by wild fowls, but the others are very well peopled. If asked the number of inhabitants, they point to a heap of sand, to show that their numbers are infinite. The names of these islands are Pais, Lamululutup, Saraon, Yaropie, Valayyay, Satavan, Cutac, Yfalcu, Piraulop, Ytai, Pic, Piga, Lamurrec, Puc, Falait, Caravaruvong, Ylatu, Lamuliur, Tavas, Saypen, Tacaulap, Rapiyang, Tavon, Mutacusan, Piyla, Olatan, Palu, Cucumyat, Piyalacunung; the 3 that are uninhabited are, Piculat, Hulatan, Tagian. Lamurrec is the most considerable of all these islands: it is there that the king of the country keeps his court, the governors of all the other islands being subject to him. Among these strangers there is one of these governors, and his wife, who is the king's daughter. Though they go half naked, yet their carriage, and a peculiar air of greatness, sufficiently distinguish them from the rest. The husband has his body painted all over with certain lines, in such manner that they form several figures; the rest of the men are also painted in like manner, more or less. The women and children are not painted at all. There are 19 men and 10 women, of different ages. The make and colour of their faces are much like those of the Philippinois. The men have no other clothes than a sort of sash, several times wrapped about their body, that covers their reins and thighs. They wear upon their shoulders above an ell and half of coarse linen cloth, like a cowl, tied before, and hanging loose behind. Both men and women are dressed much alike, except that the women have a piece of cloth somewhat longer, that hangs from their waist down to their knees.

Their language is different from that of the Philippinese and Marianese; their manner of pronouncing comes nearest that of the Arabs. The woman that seems the most considerable among them, has several rings and necklaces of tortoise-shell, called here carey, and others made of a substance resembling

* It seems as if there is some mistake here, of longitude for latitude.

ambergris, but not transparent. The manner of their subsistence at sea, which was for 70 days together, continually driven by the wind, was thus: they cast out a sort of net, made of a great number of small twigs of trees tied together, having a large mouth for the fish to enter in at, and ending in a point to prevent their getting out again. The fish they took after this manner was all the nourishment they had, and rain-water saved in cocoa-shells.

They have no cows in their islands, at the sight of some here they ran away, as they did likewise at the barking of a dog. Neither have they cats, stags, horses, or in general any quadruped, nor any fowls but sea-fowls, excepting hens, which they breed up, but never eat their eggs. Notwithstanding this their want of every thing, they are very cheerful and contented with their condition; their songs and dances are exact and regular; when they sing it is in concert, every one observing the same mode and gestures, which makes it very agreeable. They are surprised at the government, politeness, and manners of the Europeans, of whom they had not the least knowledge. They admire not only the solemnities and ceremonies of the church in celebrating divine service, but also the music, instruments, dances, and arms of the Spaniards, but gunpowder is what raises the greatest admiration in them. They wonder at the whiteness of the Europeans, in respect of whom they are perfectly tawny, as well as the inhabitants of this country.

It does not yet appear that they have either any knowledge of a Deity, or that they worship idols. Their life is perfectly savage, minding nothing but eating and drinking, in which they observe no set time, but eat and drink at any time or place, when hungry or thirsty, or they can find any thing to satisfy nature. Their civility and respect consists in taking hold of the hand or foot of the person they honour, and gently rubbing his face. Among their utensils they have some saws, not made of iron, but of a large shell, called here taclobo, rubbed and sharpened on a certain kind of stone. They were surprised to see the number of carpenter's tools that were used about building a ship here, viewing them all, one after another, with a great deal of attention. They have no metals in their country. The father missionary made each of them a present of a large piece of iron, which they received with as much joy as if it had been so much gold; and they are so afraid it should be stolen from them, that they lay it under their heads when they go to sleep. They have no other arms but lances or darts, made of human bones, very well sharpened and fixed on. They are naturally very peaceful, but if any quarrel happens among them, it is decided with some blows on the head with the fist, which yet very rarely happens; for when they would come to a close fight they separate them, and they are soon reconciled again. They are not dull and heavy, but, on the contrary, have a great deal of liveliness and courage. They are not so lusty as the inhabitants of the Marianas, yet they are well proportioned, and of a shape much like those of the Philippines. Both men and women let their hair grow long, and hang loose on their shoulders.

When they understood they were to be conducted to the presence of the father missionary, they painted their bodies all over with a yellow colour, which is considered as a great beauty. The oldest of these strangers was once before cast on the coast of Caragan, in one of our islands. They are very expert at diving; and they say, in fishing they lately took two large pearls in their shells, but threw them into the sea again, not knowing their value.

These new Philippines, situated between the Moluccas, the old Philippines, and the Marianas, which have been known near 200 years, are 87 in number, and make one of the finest archipelagos in the east; being enclosed on the north and south between the line and the tropic of Cancer, and on the east and west between the Marianas and the Philippines. Though these people seem barbarous to us, yet they have among themselves a sort of politeness and regular government. Every island obeys its chief, who himself is subject to the king of the country. This prince holds his court in the island of Falu, called likewise Lamurrec.

An Account of the new Island thrown up near the Island of Santerini. By Mons. Bourgignon. N° 317, p. 200.

On Monday the 23d of May, 1707, at sun-rising, we observed between the two Burnt Islands, commonly called the Little and Great Cameny, as it were a floating rock; which we thought at first had been some vessel shipwrecked on that coast, and seemed as if it would in a little time be dashed to pieces against the Lesser Cameny, that was hard by; on which account some mariners, in hopes of booty, put out immediately to view it. Soon after we were surprised to hear by them, that it was a shoal, which began to spring up from the bottom of the sea, and was not as yet very plainly to be discerned. Next day several persons went out of curiosity to satisfy themselves. Some of them went upon this new shoal, which was still moving, and sensibly increased under their feet. They brought back several curiosities, and among others a kind of oysters, very large, and of an exquisite taste, which they found sticking to the rock, and raised out of the water, as the shoal had increased in height; also a remarkable find pumice-stone.

Two days before the springing up of this shoal there was an earthquake over the whole island; and this was the only trouble and fear that this new island gave us; for from its first appearance to the 13th or 14th of June it has continually increased very sensibly, both in extent and height, being now about half a mile in circuit, and from 20 to 25 feet high. This shoal is very pleasant to the view, being of a white colour, and round figure. The earth composing it is light, with a small mixture of clay. The sea appears now more and more troubled every day; not so much by this shoal lately removed, and still floating, as on account of the mixture of a vast quantity of different matters, continually thrown up night and day from the bottom of the sea; so that one might easily distinguish several sorts of minerals, by the diversity of colours they made on the surface of the water; but sulphur was in the greatest abundance, insomuch that the sea was coloured with it about Santerini, to near 20 miles distance. The excessive rolling of the waves about the new shoal was greater than ever, and a more than ordinary heat was sensible to any one that approached too near, which was doubtless the cause of such quantities of fish being found dead on the shore. There was perceived a noisome stench, that infected the neighbouring air, and which we, at more than 3 miles distance, often found of dangerous consequence. The boiling of the waters grew every day considerably greater: and on Friday July 16, at sun-set, there was perceived, between this new island and the Lesser Cameny, as it were a chain of black rocks, that rose up from a prodigious depth of the sea, to the number of 17 or 18, not very distinct from each other, but seemed as if they would shortly unite together, and join themselves to this new island, as they actually did some few days after. Next day we saw them plainer, and those whose tops we could only see the night before, now appeared extraordinarily large. On Sunday we first perceived smoke to break out, much resembling in thickness and colour that of a burning furnace, and at the same time heard certain murmurings under ground, which seemed to proceed from the centre of this new island, as yet too deep in the sea, to be plainly distinguished.

Whole families went for refuge to the neighbouring islands, and others contented themselves only with changing their habitations, and living in the open country, thinking themselves safer there. In the mean time the rocks abovementioned united together, and seemed already to form another island, distinct from the former. The smoke appeared in greater abundance, and the fire which we so much dreaded, at last began to break out about the 19th of July, at first small, but gradually increased. It was no less frightful and amazing than curious, to see every night on the top of this mount, that nature had lately formed a vast number, as it were, of burning furnaces, all of a bright flame. One night at the end of July, about an hour after sun-set, as we were observing the different phænomena of this new island, there suddenly appeared in the middle region of the sky a fiery lance, seeming to come from east to west; but disappearing again soon, we could not exactly observe its dimensions. In the mean time the burnt island increased prodigiously, and extended itself principally on the south and north sides; the sea also seemed much more disturbed and loaded with sulphur and vitriol: the boiling of the water was more fierce and violent; the smoke thicker, and in greater abundance; and the fire larger and more frightful. But above all, a stench that infected the whole country grew so insupportable, that persons of the strongest constitutions could scarcely breathe in it; others, that were weaker, fell into frequent faintings; and almost every body was seized with vomitings. I could not then but imagine myself on board some man of war, where at a general discharge of all the guns, the confused stink of the powder, tar, and stench of the ship, especially in foul weather, often overcomes the strongest seamen. Just such a nauseous stench we were forced to breathe in, without being able anywise to avoid it, or defend ourselves from it. This ill scent was very mischievous, it spoiled most of the vines; and a great smoke that rose out of the midst of this new island like a mountain, uniting to a thick fog, that commonly hangs over Santerini when the wind is at south, burnt and destroyed, in the beginning of August, in less than three hours time, all the fruit that was ripe and ready to be gathered, especially in such vineyards as lay most exposed to the south. A few days after I was obliged to go to Naxus, so that I was absent 13 or 14 days; in which short space of time there was so great an alteration in these two new islands, that I scarcely knew them again at my return. The white one, that did not seem to increase any more, was now grown considerably higher; and the black one was much longer. Both of them, though different in colour, were now united together, and made but one island, as they remain at this time. The fire and smoke had made new vent-holes, and the noise under ground was more frequent and audible. They told me, that in my absence they heard from the midst of the island, as it were, so many large cannon shot off, and at the same instant saw a great quantity of burning stones thrown into the air. And a few days after I was myself an eye-witness of so prodigious and frightful a spectacle. I watched day and night these furious discharges, which made the doors and windows of our chambers shake, and sometimes the very best built houses; and I saw several times stones all on fire darted into the air out of sight, and afterwards fall again like a bomb, and quenched in the sea at more than 5 miles distance.

When these discharges happened, which were as loud as those of a cannon, we commonly saw immediately a great flash of fire like lightning, and after that, there sprung up very swiftly a black and dismal smoke, mixed with ashes, and so exceedingly thick, that when spread in the air, it made a thick cloud of several colours, which gradually dissolving in a fine dust, fell like rain on all the neighbouring country, and more particularly on our island, where it was in such abundance, that the ground was often covered with it. The noise grew stronger, and was louder than if six or seven cannons had been discharged together; the fire made itself every day more vent-holes, and became more dreadful. We commonly sat up the greatest part of the night to observe it.

Every night nature represented as great variety of scenes as the fire broke forth in different forms; sometimes burning ashes spread themselves in the air, like a plume of feathers, which falling again on the shoal, made it appear all of a light fire. Other times one would think it was actually the discharging of so many mortar-pieces, which threw up rocks, like so many bombs, capable of destroying the largest ships; though for the most part these stones were of a middle size, but in such quantities, that I often saw this little island all covered with them, and so pleasantly illuminated, that one would never be weary of looking on it.

These dreadful discharges were less frequent at the end of August, but increased in September, were daily in October, and at this time (November 20) are almost incessantly, the island being now at least 3 miles in circumference, and from 35 to 40 feet in height. It is true, the noise is not so loud; the stones, that are cast up, are not so large nor so many; the boiling and disorder of the water is much abated; the sea begins to recover its former colour; the stench, that was before insupportable, has been very little for these six weeks. Yet the smoke grows every day thicker, blacker, and in greater abundance; the fire is more than ever, and seems sometimes to strike the very sky; the subterraneous noise is continual, and so violent that it cannot be distinguished from thunder; dust and ashes fall daily on this our island. The countryman is dejected at the loss of his corn, which, scarcely sprung out of the ground, begins to fade already; and the mariner, not so bold as before, contents himself with viewing it at a distance, since the loss of a bark of this country, which going too near, took fire and was burnt. In short, our new island grows every day more curious, more dreadful, and less accessible; and is continually increasing on the south-west side; where nature seems as if she laboured to make a large port, capable of holding all manner of ships, which may one day render Santerini less practicable than heretofore.

Observations on the White Matter on the Tongues of Feverish Persons, &c. By Mr. Leuwenhoeck. N° 318, p. 210.

Having scraped from his tongue, after an attack of a fever, the whitish slimy matter with which it was covered, and having boiled the same in some water he observed, on examining the liquor with a microscope, "several particles which had the shape of a pear; some of which, at the smaller end, were bent a little, others roundish, but none of them had any part that answered to the stalk of a pear."

After the matter, which he had taken from his tongue, had lain above a fortnight in the water in which it was boiled, and the water was almost evaporated, he poured a little fresh rain-water upon it; and 5 or 6 days afterwards he took a thin slender glass-tube, one of the ends of which was open, and turned it hastily upside down into the bottom of the china cup, in which most of the particles which he scraped from his tongue lay, with this design, that by the rushing in of the water into the tube, some of those particles of the tongue should be likewise carried upwards into the glass tube, and that by this means he might have a fresh opportunity of viewing them, when he discovered an inconceivable number of exceedingly small animalcula of different sorts; the greatest number of them were of one and the same size, but they were so little, that without a careful observation, and a very good microscope, they would have escaped the sight. Most of these animalcula were in that part of the water where the said matter of his tongue lay, which made him think whether those animalcula might not receive their nourishment from the aforementioned particles; after these animalcula had been about 2 hours in the glass tube, he perceived that a great many of them were dead.

Concerning a Colliery that took Fire, and was blown up near Newcastle, killing 69 Persons. Communicated by the Rev. Dr. Arthur Charlett, Master of University College in Oxford. N° 318, p. 215.

On Wednesday the 18th day of August, 1708, at Fatfield, in the parish of Chester-le-street, about 3 o'clock in the morning, by the sudden eruption of a violent fire, which discharged itself at the mouths of three pits, with as great a noise as the firing of cannon, or the loudest claps of thunder, 69 persons were destroyed in an instant. Three of them, viz. two men and a woman, were blown quite up from the bottom of the shaft, 57 fathom deep, into the air, to a considerable distance from the mouth of the pit; one of the men with his head almost off, and the woman with her bowels hanging about her heels. The machine by which the coals were drawn up, and is of a great weight, was blown off by the force of the blast; and what is more wonderful, the fish which were in the rivulet, that runs 20 yards under the level, and at as great a distance from the mouth of one of the pits, were in great numbers taken up dead, floating on the water, by several of the inhabitants.

Stith, commonly so called by the pitmen, I think corruptly from stench, or stink, is such a foulness in the air, as both suffocates the men, and extinguishes the candles. Sulphur differs in this, that as the other suffers not the candles to burn, this makes them burn too fast; and the flame, by the impulsive quality of the air, or attracted by the sulphur, extends itself upwards into a prodigious length, and, like a match lighted for the discharge of a cannon, as speedily sets on fire that vapour, equally destructive as gunpowder.

Now to prevent both these inconveniences, as the only remedy known here, the viewer of the works takes the best care he can to preserve a free current of air through all the works, and as the air goes down one pit, it should ascend another. But it happened in this colliery, that there was a pit which stood in an eddy, where the air had not always a free passage, and which in hot and sultry weather was very much subject to sulphur; and it being then the middle of August, and some danger apprehended from the closeness and heat of the season, the men were with the greatest care and caution withdrawn from their work in that pit, and turned into another; but an overman, some days after this change, and upon some notion of his own, being induced, as is supposed, by a fresh, cool, frosty breeze of wind, which blew that unlucky morning, and which always clears the works of all sulphur, had gone too near this pit, and had met the sulphur just as it was purging and dispersing itself, upon which the sulphur immediately took fire by his candle, which proved the destruction of himself and so many men, and caused the greatest fire ever known in these parts.*

An Experiment for continuing several Atmospheres of Air, condensed in the space of one, for a considerable Time. By Mr. Fr. Hauksbee, F.R.S. N° 318, p. 217.

On March 20, 1708, I injected with my syringe, into a very thick flint-glass bottle, between four and five atmospheres of air, as appeared by the included gauge, and which continued in that state till about the 7th of August following, when looking on it, as I commonly did once in four or five days, I found that the injected air had made its escape, the weather for a week before having been very hot; especially one day I observed the spirit in the thermometer had ascended 120 degrees above the freezing point. And notwithstanding the bottle was continually kept under water, yet the cement, used to fasten the brass cap to it, was so softened, as to render it unable to resist the spring of the injected air. I observed, that though all the air that was capable of elasticity was fled, yet the mercury in the gauge remained about three quarters of an inch in height, above the surface of that in which its opened end was im-

* What is here termed sulphur is inflammable air or hydrogen gas, called by miners fire-damp. See note at p. 16, vol. i. of this Abridgment.

mersed, which was about a sixth part less space than what the same air possessed before the injection, and still remains so, notwithstanding it is constantly exposed to the open air. Which plainly shows that the springs, or constituent parts of the air, do not in time totally restore themselves after standing so long bent. And had not the heat come on, but the air continued in that state as at first injected for a year or two, I doubt not but its springs would have been rendered much more incapable of restoration. From hence it is easy to conclude, that if nine or ten atmospheres of air were condensed in the space of one, and to remain in that state for a year or two, that when the vessel containing them shall become exposed open to the air, that then bodies, such as very thin glass bubbles, supposing them not to be above five or six times specifically heavier than their like bulk of common air, would float on such a medium. But I am not sure of this, and do not know but it may be the means of rendering air visible.

An Experiment, showing the Production of Light within a Glass Globe, whose inner Surface is lined with Sealing-wax, on an Attrition of its Outside. By Mr. Fr. Hauksbee, F. R. S. N° 318, p. 219.

I took a glass globe, about 6 inches diameter, into which putting a convenient quantity of broken sealing-wax, I held it over a moderate fire till the wax was melted, then turning it about from part to part, it soon had got a pretty thick lining to it, on more than half its inside. Thus placing it in a convenient position, I left it till it was perfectly cold. After having fixed the brass-work to it, I caused it to be exhausted of its air, then fixing it on the machine, to give it motion as usual, I no sooner held my hand on that part of it where it was lined with the wax, but the figure of the parts that touched it was as visible on the inner surface of the sealing-wax as when the glass alone is used for that purpose. Where the sealing-wax is spread thinnest on the glass, one can but just discern the light of a candle through it in the dark; but some parts are so covered with it, that it is at least one 8th part of an inch in thickness; and even on those parts, the light and figure appeared as vivid and distinguishable as any where else. The light produced is not at all discernible through the body of the wax, but only to be seen through the transparent part of the glass; and notwithstanding some parts of the sealing-wax did not adhere so closely to the glass as others, yet the light appeared on those parts as on the other. Now whether the light produced on the sealing wax, was from the effluvia caused by the attrition of the surrounding body of glass, or from its own disposition to do so in such a medium, I cannot determine; it being of the same colour and likeness to that of glass in all respects, except that, on a small quantity of air being let into the receiver, the light wholly disappeared in that part lined with the wax, and not in the other parts. I further observed, when all the air was let in, that the ring of threads being held over the glass, the threads would be attracted at a greater distance, from that part of it lined with the wax, than from the other parts, which seems to proceed from the united strength of both their effluvia.

On a repetition of this experiment, I observed, that the wax within the glass would attract bodies approached near its outside, and that even in vacuo (which is a discovery that I never could make from any other body, in such a medium, except the magnet.) For holding the ring of threads over it, while it was in that state, the threads would be directed, but not with that vigour as when all the air was admitted; yet here was that sensible difference, that when the threads were held over that part of the glass free from the lining of wax, the threads would not be attracted; but approaching them within the reach of the effluvia of the wax, they would eagerly fly towards it. Hence it seems deducible, that the figure of the parts of glass and sealing-wax are much alike, otherwise I cannot conceive how the effluvia of one can penetrate and pass with such ease the body of the other, and there to act as if it was one and the same with it.

An Account of some Experiments, in Relation to the Weight of Common Water under different Circumstances. By Mr. Fr. Hauksbee, F. R. S. N° 318, p. 221.

Taking a glass of common water, and having weighed nicely a glass bottle in it, whose bulk was found equal to the bulk of 575 grains of the same fluid; I then caused some of the water to be boiled over the fire, and after that included in vacuo, where it remained till it became of the same temperature with common water. Thus to the utmost of my power, I endeavoured to extricate all the air out of the water, thinking in that state it would become more dense than when I first weighed the bottle in it: but, contrary to expectation, I found that the bottle had just the same weight in it, as before; which seems to confirm the impossibility of compressing water by force into a less space than it naturally possesses: for if, on the removal of such a quantity of air from out of its body, the parts do not slide any closer together, how should a weight laid upon its surface, when its interstices seem to be replete with air, make any impression on it. The body which was forced out of the water by the foregoing means, I call air, since it is subject to all the same laws with it: but that the water, on its absence, should not unite more closely than before, seems very surprising; for I cannot conceive what matter must supply the vacancies, since the particles of water themselves remain at the same distances as if the air was not withdrawn, otherwise the water of necessity must become more dense. But to proceed, I caused some water to be heated about blood warm, when weighing the bottle in it, I found the bulk of water equal to the bulk of the bottle, which was about 3 grains less than when cold: which shows, that the component parts of the water are easily separated by heat, and the matter lodged in its interstices, capable of dilation. I then took that water which had been purged of all its air, and gave it a degree of heat, not exceeding luke-warm; and on weighing the bottle in it, I found that although the heat it had received was very inconsiderable, yet the bulk of the water, in that state, equal to that of the bottle, was now diminished 2 grains: which plainly shows, that notwithstanding the water contained no air that I could discover, yet there seems to be a matter latent in it capable of intumescence.

An Account of some strange and wonderful Effects of the Scurvy, which happened at Paris in the Year 1699. *By Mr. Poupart.* N° 318, p. 223.

In 1699, I went to the hospital of St. Lewis at Paris, with intent to make observations on the great number of scorbutic patients there; I soon perceived that this distemper had something in it of that cruel plague, with which the Athenians formerly were afflicted. It had the common symptoms; as pains in the thighs, calves of the legs, the belly and stomach, and the patients were deprived of the motion, or use of their limbs, though they still retained their feeling: they were troubled with head-achs, convulsions, and such strange itching in the gums, that the children pulled pieces off them with their nails; and the blood which came from them, was watery, salt, and corrosive; and the stench, which came from their mouth, was intolerable. They had hard blue spots on their legs and thighs, frequent hæmorrhages, or bleedings at the nose and fundament, and so great a weakness in their knees, that they could not go without reeling or staggering. Such were the symptoms which they had in common with other scorbutic persons. The other particular affections, were the following: on removing these sick persons, there was heard a small clattering of their bones, which particular Mr. N. V. a physician of Rochelle, has mentioned in his Treatise of the Scurvy, but he could not assign the true reason of it. I observed, on opening all those bodies in which the said noise was heard, that the epiphyses were entirely separated from the bones, which by rubbing against each other occasioned this clattering. We opened several young persons, in whom we also perceived a small low noise when they breathed; in all these bodies we found, that the cartilages of the sternum were separated from the bony parts of the ribs; and as the cartilages are of a softer substance than the epiphyses, the noise, which their rubbing produced, was less than that of those bones which rubbed against the epiphyses. All these last are dead, except one young man, whose ribs were visibly re-united to the cartilages; for after his cure, we heard no more of this noise.

All those, in whose breasts any matter or serosity was found, had their ribs separated from their cartilages, and the bony part of the ribs, which were over against the sternum, was carious or rotten for 4 fingers length; which shows, that the lympha of these bodies was exceedingly caustic. Most of those bodies which were opened, had their bones black and carious.

Most of the patients went staggering, as usual in scorbutic cases; the reason of which is, that the support of the joints is owing to the force and spring of the ligaments, which bind the bones close to each other; and as the ligaments of these patients, were corroded by a sharp lympha, and loosened, the bones were separated from each other. All the young persons under 18, had in some degree their epiphyses separated from the body of their bones, and by the least force we separated them entirely. The reason of it is this, that young persons have not yet their epiphyses so strongly fastened to the bones; so that when they are ever so little soaked with that corrosive lympha which is in the joints, that caustic liquor may easily separate them entirely. All the bones thus separated from their epiphyses, were more than twice as large than in a natural state, because well soaked with a water which had penetrated into their very substance and made it swell.

The bones of such as recovered, or were recovering, remained swelled, without giving them any pain: they might become less in time, as it happens to children troubled with the rickets, whose bones gradually dry as they grow up. All those who had any difficulty in breathing, or their breasts stuffed or stopped up, had there a great deal of lympha, or matter. And some patients were so oppressed, that they died suddenly, though no serum was found either in their breasts or lungs: but the pericardium entirely adhered to the lungs, as did the lungs to the pleura and diaphragma; and all the parts were so blended together, that they all made up but one confused mass or lump. Now as the lungs were compressed together amidst this mass, they were deprived of their motion, and the patient was choked for want of breath. The close adhesion and confusion of these parts one with another, was owing to their being ulcerated.

Common scorbutic patients have the glands of the mesentery much obstructed, and swelled; those we treat of, have theirs corrupted, and imposthumes in its substance. In the liver of some few, the matter or pus was hardened, and as it were petrified; the spleen was three times larger than usual, and fell to pieces, as if it had been composed of clotted blood: and sometimes the kidneys and the breast were full of imposthumes. There were some bodies of those of 15, in which, if we squeezed the end of the ribs, which began to be separated from the cartilages, there issued abundance of corrupted matter, which was the spongy part of the bone; so that afterwards there remained nothing of the rib, but two bony plates.

Some patients had no other symptoms of the scurvy, but some slight ulcerations in the gums: they had afterwards some small, red, hard tumors on the hands, insteps, and on some other parts of the body. Afterwards, there appeared large imposthumes on the groin, and under the arm-pits, attended with several blue spots over all the body, which were the certain fore-runners of death. We found that the glands under their arm-pits were very large, and surrounded with corrupted matter or pus; as well as the muscles of the arms and thighs, the interstices of which were all filled with them. There were some whose arms, legs, and thighs were of a reddish black, and as it were burnt; which proceeded from that black and coagulated blood, which was always found under the skin of those persons. Their muscles were also swelled, and as hard as wood; which was owing to the blood, fixed in the body of the muscles, which were sometimes so full of it, that their legs remained bent without being able to extend or stretch them out.

The blue, red, yellow, and black spots, which appear in the bodies of such as have the common scurvy, proceed purely from extravasated blood under the skin. As long as the blood retains its red colour, the spot is red; if the blood become black or coagulated, the spot is also black; when some bile is mixed with it, the spot is of a yellowish black; in short, according as the blood is mixed with humours of different colours, so the spots appear of different colours also.

On the bodies of these patients were certain small tumours, which grew larger every day: we applied emollient unguents to soften them, and on their breaking, they formed a scorbutic ulcer, owing to the blood with which the tumour was filled; for as often as the plaster was removed, we still found under it a great deal of coagulated blood; and continuing to dress them, and take away the blood, we entirely dried up the tumour, and the person was cured. Some old persons had such large bleedings at the nose and mouth, that they died of it, it being impossible to stop it, because the lympha of these persons was so sharp and corrosive as to eat through the coats of the veins. And this kind of hæmorrhage was so much the more difficult to stop, because the blood of old people is more fluid and watery than that of young persons, who are seldom subject to this accident.

Both old men and women were troubled with such violent fluxes, that the weakest died under them; but if they had strength enough to withstand them, they were soon cured.

Some of these patients were so costive, that they never could go to stool, without taking some clysters. And some of them had such large swellings over all their bodies, their hands, arms and feet, that they seemed to have been blown up. But many were cured by proper medicines, clysters, and sweetening juleps. A youth of 10 years of age, had his gums much swelled, and ulcerated; his teeth were eaten to the very roots, and his breath was intolerably stinking. The surgeon was obliged to pull out all his teeth, for the better dressing of his mouth, though they would have fallen out of themselves: his gums were healed, but a tumour arose on the side of his tongue as large as a walnut, in the middle of which was a bluish hole, which degenerated into an ulcer, which eat up half the tumour, the other half remained whole and entire. Some short time after, there appeared another tumour in the cheek, which was very hard: it was blue in the middle, and turned to an ulcer also as the first. This youth died suddenly, and when least expected; all the inward parts of his body were mortified.

All those who died suddenly, without having any visible cause of their death, had the auricles of their heart, as large as one's fist, and full of coagulated blood, which stopping the circulation of the blood, caused inevitable death. Several patients had on their cheeks a small white ulcer, which was hard all round; unless we took care to stop it presently, and to take it off with the spirit of vitriol, it soon grew livid or blue, black and stinking, and eat up part of the cheek, so that one might see the teeth through it. Several from the age of 18 to 30, who were without pain, dropped down stupid and motionless, with their mouths open, their eyes sunk in, their looks frightful, and appearing rather like statues than men. All these persons had no apparent sickness, only their gums were ulcerated; their skin was smooth and fair, without any spots or hardness: yet their muscles were gangrened, and all wet with a black corrupted blood, and in handling them they fell to pieces.

One patient had a carbuncle on his instep; his lips and his nostrils were chopped; and a stinking water flowed gently from the latter. He lingered out a long time and then died. His body made me afraid, so that I durst not open it. A young man, who to all outward appearance seemed not to be very ill, died suddenly. We found his pericardium so eaten up, that there remained but a little of it; and his heart was ulcerated all about very deeply.

Scorbutic persons are commonly better in summer, than in winter; which may be owing to their great perspiration. On the contrary, these were indifferently well from the month of April, to the beginning of June, the spots, hardness, and other accidents of the scurvy then disappearing; but on the coming of the great heats, all those symptoms returned. Those who were so well, as to be ready to quit the hospital, relapsed again: their legs and thighs became all black, and many of them died. This disorder might arise from a too great quantity of corrosive lympha that it was in a manner impossible to be carried off by perspiration; so that by stagnating in their bodies, it grew hot, fermented, soured, and putrefied, from thence arose those corrosions, ulcers, large imposthumes, mortifications, &c.

All these patients ate very heartily to the last moment of their lives; a sharp humour, with which their stomachs always abounded, created in them a kind of fames canina.

Nothing is so apt to corrupt the blood as long abstinence; the use of bad food is still worse: cold stops the circulation of the blood, and makes it remain too long in the parts, where it sours and soon corrupts; sadness and grief (which these people are subject to) is worse than all the rest: and what all these may effect, when they meet altogether in one person, we may easily judge. They produced there lymphas of different colours, with which the belly, the breast, and several other parts of the bodies were filled; and so caustic, that on putting our hands into their dead bodies, the skin would come off, and the face become ulcerated. So that we were obliged to rise in the night to wash our faces with fresh water, to take off the inflammation. But what was very surprising in this extraordinary disease, the brains of these people were always very sound and entire.

Extract of a Memoir, concerning the Discovery of a Passage by Land to California; with a Description of that Country. By Francis Maria Picolo. N° 318, p. 232.

California is pretty well placed in our common maps. The heats in summer are very great along the sea-coasts; and it seldom rains: but the air of the inland countries is more temperate, and the heats not so excessive. It is proportionally the same in winter. In the rainy season there are floods; but when that is over, instead of rain, the dew falls in such plenty every morning, that one would think it had rained; which renders the earth very fruitful. In the months of April, May, and June, there falls with the dew a sort of manna, which congeals and hardens on the leaves of reeds, from whence they gather it: it is as sweet as sugar, though not quite so white.

The climate is very healthy; and the country abounds in large plains, pleasant vallies, excellent pastures, at all time, for large and small cattle; fine springs of running water, brooks, and rivers, with their banks covered with willows, reeds, and wild vines. In their rivers are plenty of fish, especially crayfish, which they keep in a kind of conservatories, till they have occasion for them, which are very large and beautiful. There is also plenty of xicames, of a better taste than those of Mexico. On the mountains there are mescales all the year round, a fruit peculiar to this country; and in most seasons, large pistachios of several sorts, and figs of different colours. The trees are very beautiful; and among others, that which the chinos, who are the natives of the country, call palo santo, bears a great deal of fruit, from which they draw excellent frankincense.

As this country abounds in fruit, so it does no less in grain; of which there are 14 sorts that the people feed on. They use also the roots of trees and plants, and among others, those of the yyuca, to make bread of. There are excellent skirrets; a sort of red strawberries, of which they eat plentifully; and citrons and water-melons, of an extraordinary size. The land is so good, that most plants bear fruit three times a year: so that with some labour in cultivating it, and skill in managing the water, they render the country exceedingly fertile. Indeed there is no fruit or grain, but what they gather in great abundance.

Besides several sorts of animals that we knew, which are here in plenty, and are good to eat, as stags, hares, coneys, and the like; we found two sorts of deer, that we knew nothing of: we call them sheep, because they somewhat resemble ours in make. The first sort is as large as a calf of one or two years old: its head is much like that of a stag; and its horns, which are very large, like those of a ram: its tail and hair are speckled, and shorter than a stag's: but its hoof is large, round, and cleft like an ox's. Their flesh is very tender and delicious. The other sort of deer, some of which are white, and others black, differ less from our sheep: they are larger, and have a great deal more wool, which is very good, and easy to be spun and wrought. Besides these animals, that serve for food, there are lions, wild cats, and many others of the like, as in New Spain. As for fowls, there are in California all that are in Mexico and New Spain; as pigeons, turtle-doves, larks, partridges of an exquisite taste, and in great quantities: geese, ducks, and many other sorts, both of river and sea-fowls.

The sea affords plenty of good fish: they take pilchers, anchovies, and tunnys; which last they catch with their hands on the shore. We often see whales, and all sorts of tortoises. The shores are covered with heaps of shells, larger than those of mother of pearl. Their salt is not from the sea, but out of pits: it is as bright as crystal, and so hard that they are often forced to break it with hammers. It is a very good commodity in New Spain, where salt is scarce.

California has been known near two centuries; and its coasts are famous for the pearl fishery, which has made the Europeans so desirous of establishing a trade here. I doubt not but there are mines to be found in several places, if they were sought for; since the country is under the same degree as the provinces of Cinalao and Sonora, where there are very rich ones. Yet the Californians, amidst all this plenty and riches of their country, content themselves with what is only necessary for life. The inland parts of the country are very populous, especially towards the north: and though there is scarcely a town, but what has 20, 30, 40, or 50 families in it, yet they have no houses, but defend themselves from the heat of the sun in the day time under the shade of the trees, and of their leaves and branches make a sort of roof against the inclemency of the night. In winter they shut themselves up in caves in the earth, and live there together little better than so many beasts.

The men go naked: they wear about their head, a fine linen fillet, or sort of net-work; and about their neck, and sometimes about their arms, for ornament, mother of pearl in divers figures, very finely wrought, and prettily intermixed with little round fruits, somewhat like the beads of a chaplet. They have no other arms than bows and arrows, and a sort of javelin, which they always carry in their hand, either to kill their game, or defend themselves from their enemies; for their towns often make war on each other. The women are somewhat more modestly clothed, wearing from their waste down to their knees a kind of apron, made of reeds very neatly wrought and matted together. They cover their shoulders with the skins of beasts, and like the men, wear about their heads a curious kind of net-work. They also have necklaces of mother of pearl, mixed with the stones of some sorts of fruit and sea-shells, hanging down to their waste; and in like manner bracelets of the same.

The common employment of both men and women, is spinning. They make their thread of long plants, which serve them instead of hemp and flax; or else of a cotton-like substance, found in the shell of some sorts of fruit. Of the finer sort of thread, they make the ornaments above-mentioned, and of the coarser fishing-nets, and sacks or bags for several uses. The men also, of certain plants, whose fibres are very close and thick set, and which they are very well skilled in working, employ themselves in making dishes, and other kitchen utensils, of all fashions and sizes. The smaller pieces serve for drinking cups: those that are larger, for plates and dishes, and sometimes for umbrellas for the women; and the largest sort for baskets to gather fruit in, and sometimes for pans and basins to dress their meat in: but they take care to keep them continually moving, while they are over the fire, for if the flame catch them, they are soon burned.

The Californians have a great deal of liveliness, and are naturally addicted to raillery; as we found when we began first to instruct them. We have not found among them any form of government, religion, or regular worship. They adore the moon, and cut their hair in her decrease, in honour of their deity; which they give to their priests, who employ it to several superstitious uses. Every family makes laws as they please, which is plainly the reason that they are so often at war with each other.

De variis Animalibus Philippensibus, ex MSS. R. P. Geo. Jos. Camelli. Communicavit D. Jac. Petiver, S. R. S. N° 318, p. 241.

An enumeration of various animals of the Philippine isles. In Sect. I. a catalogue of frogs, toads, lizards, &c. In Sect. II. a catalogue of insects and worms.

On the Circulation of the Blood in Fishes, &c. By Mr. Leuwenhoeck, F.R.S. N° 319, p. 250.

I viewed the hearts of several fishes, particularly that of the large silver eel, the motion of which lasted near 4 hours, after it was taken out of the body of the fish, which motion was very regular; for when the blood is protruded out of the heart, it is not carried into the great arteries with the same velocity, which, in that case, would be overcharged with the great quantity of blood: but the blood thus coming from the heart, is forced into a small white vessel, almost of the shape of a pear, and which one would take for a kind of bladder; one orifice of which was united to the great artery, and the other to the heart; in the latter orifice is a valve, the use of which is, to prevent the blood, protruding from the heart into the said vessel, from running back again into it: which vessel having been cut across, I observed the inside of it to be furnished with a great many small particles, so that it was in a manner filled with them; and the design of these internal particles I conceive to be, that when the blood is protruded into the vessel, by dilating and contracting itself, it may presently force the same into the great artery. So that the blood is almost always running with an easy and constant course; though at every protrusion it must be in some manner quickened, yet that is so insensibly, that it cannot be observed or felt. And the case is probably the same in beasts and other large animals.

I have formerly communicated some discoveries relating to the circulation of the blood in eels; viz. that the blood, coming out of a great many small vessels in the tail of an eel, is united in one greater blood-vessel, where the fish-bones begin, and where the blood runs through a valve; for I observed that the blood-vein was not only moved in that part where the valve is, but also the parts about it, of the breadth of 4 or 5 hairs breadth; from whence it appeared, that at every protrusion of blood into the heart through the valve, the blood stood still for an instant of time, and then falling through the valve, it ran with great swiftness, and was thickest just at its protrusion out of the valve, but ran thinner or slenderer like the figure of a pear; and the vein that received this protruded blood, was not entirely filled with it, but seemed for a small space to be as it were empty, and its parts contracted; and further observing it, I saw the blood run slowly and leisurely along the same vessel.

From this observation I imagined, that the same thing happens in the heart of a human creature, viz. that there is a gentle and slow protrusion of the blood out of the heart into that vessel, called the artery; and consequently that there is no such motion there, as is called a pulse, and which is felt in the extreme parts of the body; but that the pulses are only caused by the protrusion of the blood through the valves in the veins; for I never observed any violent or swift protrusion of the blood into the arteries, as often as I have viewed its circulation: and though the blood, by the contraction of the heart, be suddenly and hastily protruded out of it, yet it is slowly carried into the artery; whereas, on the contrary, it runs into the heart from the veins with a violent and swift course: from whence it happens, I suppose, that the remaining part of the blood in the veins, being unable to follow with so swift a motion, is, as it were, violently and per saltum drawn or forced through the valves, and that it is this sort of motion which we take for pulses in the arteries.

To satisfy myself in the above observations, I have often viewed that sort of motion in my arm, called the pulse, at the time when my body was without motion and warm; and I judged that the motion, which we perceived in the blood vessel, was not derived from the heart to the hand, but contrariwise from the hand to the arm, and so to the heart: from whence I concluded, that, like as in the tail of an eel, there are no valves in the blood vessels, as far as I could perceive, and that a great many small blood vessels, are, as it were, united in that part where the fish bones begin, and make one large blood vessel, where the first valve is; in the same manner in human bodies, a great many single blood vessels running out of the hand, are joined in the arm, where likewise the first valve is, through which the blood at each protrusion falls into the heart, producing what we call the pulse.

I have several times observed in the exceedingly small veins or capillary vessels, a little rising or swelling occasioned by a stronger motion of the blood, which I now firmly conclude, to proceed only from the sudden motion or running of the blood through the valves: I have also observed, that in sudden frights, and otherwise, one feels such motions at the end of one's fingers, just as if there were valves likewise in them, through which the blood gushes; but these sort of motions, I suppose, do only depend on that quick motion made by the blood, when it runs through the valve in the arm by the hand, to which we give the name of a pulse.

In the month of September, having opened an eel, the diameter or thickness of which was about an inch and a half; and having laid open the heart, I could not discover that part which receives the blood out of the great vein in order to bring it into the heart. But that I might the better discover that part, I prepared a little glass tube, and put it into the great vein at a little distance from the heart, and then blew some air into the said vein, as much as might take up the space of about half a pea: this air passed through the great vein into a little bladder that lay on the side of the heart, and no sooner was the air got into that bladder, but it first contracted, and then dilated itself, so regularly, and in such a manner, that when the heart contracted itself, just as if it were going to protrude its blood, the said little bladder with air in it was dilated, and continued in such a motion above 5 full hours together: though indeed in the last hour it was so faint, that one could but just perceive it; and as for the heart, its motion was discontinued.

I also took a pike-fish, about 2 feet long, and opened it immediately while it was in its full strength of life, and observed not only the motion of the heart, and the regular motion of that part which receives the blood, and brings it into the heart, but also the motion of that other part, which receives the protruded blood from the heart, and carries it gently into the arteries.

Pl. 10, fig. 23, shows the heart of a pike; DEFA represents that part into which the blood is brought from the veins; and CDG that other part which receives the blood from the heart, to carry it into the arteries. Now when the heart receives the blood which is conveyed into it, it dilates to its utmost roundness; and then that vessel represented by ADEF at that very instant collapses; and discharging its blood into the vessel CDG, this becomes distended by the sudden pouring in of the blood; and no sooner is it so dilated, but it contracts again, that it may force the blood into the arteries. In short, when ADEF is contracted, and throws the blood into the heart, this is dilated; and when the heart contracts and discharges the blood, CDG is dilated: and these three several motions happen in so short a time, and are performed so regularly, that it is quite surprising: and from hence we cannot but conclude, that such a motion as this could not be brought about, unless the vessel ADEF had a valve at AD, where it is joined to the heart, which valve is to prevent the blood, that is thrown into the heart, from returning the same way. And so likewise there must necessarily be another valve at CD, to prevent the blood, that is protruded from the heart, from flowing back again.

Also fig. 24 represents the heart of a salmon, where KLM shows that instrument or vessel that was represented in fig. 23, by ADEF, as INO shows the same as CDG in the said figure.

Also the instrument KLM being cut open, to discover with the naked eye, the sinewy parts and their branches; these appeared as in fig. 25, in which QR is the part that was joined to the heart, and is the same that in fig. 24 is represented by KL; in the said fig. 25, we may observe how the sinewy parts and their branches run from QR to T. This instrument, or vessel, is very soft in its parts, and it seemingly is not strong.

Fig. 26 is that vessel dissected, which in fig. 24 is represented by ION; which vessel is exceedingly thick and strong, and like that represented in fig. 25, provided with strong sinewy parts, that when the parts are extended by the blood poured into them, they may be able both in roundness and length to convey the blood into the arteries: these parts, by reason of their great numbers, cannot be delineated in such manner as they ought to be.

From the whole I conclude, that the heart protrudes the blood gently into the arteries; and that the blood, which flows from the veins into the heart, causes that sudden revulsion, called the pulse; both because it cannot so immediately pass through the valves, and because the veins in that part are a little narrower, by which means there is a kind of stop or intermission in the circulation of the blood: and this I conceive is the cause of that motion which we call the pulse.*

Several Experiments on the seeming Spontaneous Ascent of Water. By Mr. Fr. Hauksbee, F.R.S. N° 319, p. 258.

EXPER. I. To satisfy myself, whether the form of the vessel might not contribute to the spontaneous ascent of water in tubes, &c. I procured a couple of glass planes, about 7 inches long, and 1½ broad; these planes were part of a broken looking glass; and though when clapped together, they seemed to touch one another in so many parts, yet when they came to be immersed in a liquid, it would ascend between them; but it was so thin and colourless, that it could not easily be discerned; but on separating them, they would be found

* It is scarcely necessary to remark, that Mr. L.'s conjectures respecting the cause and nature of the pulse are extremely erroneous and absurd.

wet on all their parts: therefore to make it more obvious, I put a small piece of thin paper on each corner; by which means, when laid the one on the other, they became separated by such a distance, as is equal to the thickness of the paper. In this manner I plunged one end into some strongly tinged liquor, when immediately the water ran slowly and gradually, sometimes higher in one part than in another, shooting itself very prettily into branches, and so would continue, till it had arrived at its greatest height; which would be according to the distance the planes were placed asunder: for if two pieces of paper were laid on each corner of the planes, the water would not ascend so high between them, as when they were separated by only one. And then, if the planes were at all declined, the water would still spread itself farther and farther, according to the degree of declination.

Exper. II. Having seen the success of the former experiment in the open air, I wished to try what would be the effect in vacuo. Accordingly I fixed the two planes in such a manner to a brass wire, which passed through the cover of a receiver, that I could make them descend at pleasure. In this manner I conveyed them with a dish of tinged liquor, within the receiver; which being placed on the pump, I proceeded to exhaust its contained air. That being done, I plunged the planes (separated by pieces of thin paper as before) into the water, where also it ascended between them; only there appeared more intervals, or spaces, between the branches of the ascending liquid, than in the former experiment; but when I admitted the air, those intervals vanished, and an intire body of the liquid succeeded: yet the exact form of the upper parts of it remained unaltered.

Exper. III. By these experiments I found, that neither the figure of the vessel, nor the presence of the air, did anywise contribute to the production of the appearance. To try therefore whether the quanity of matter would help to resolve the mystery, I took two tubes of an equal bore, but of very unequal substances, one of them being at least ten times the thickness of the other; yet when I came to plunge them into the liquid, its ascent seemed to be alike in both. In order therefore to endeavour to account for the phenomenon, I would propose a magnet, that operates with equal vigour under similar circumstances. Thus, 1st. A magnet of any form will attract iron; so by the first experiment, the figure of the vessel seems no way to contribute to the ascent of the water. 2d. The magnet is no ways lessened in its vigour of attraction, even in so thin a medium as a vacuum: so, by the second experiment, the presence of the air is no ways necessary to assist in the ascent of the water, in small tubes, or between the planes. 3dly. A magnet, as suppose one of a pound weight, that will take up, or suspend a piece of iron of the like weight, motion as this could not be brought about, unless the vessel ADEF had a valve at AD, where it is joined to the heart, which valve is to prevent the blood, that is thrown into the heart, from returning the same way. And so likewise there must necessarily be another valve at CD, to prevent the blood, that is protruded from the heart, from flowing back again.

Also fig. 24 represents the heart of a salmon, where KLM shows that instrument or vessel that was represented in fig. 23, by ADEF, as INO shows the same as CDG in the said figure.

Also the instrument KLM being cut open, to discover with the naked eye, the sinewy parts and their branches; these appeared as in fig. 25, in which QR is the part that was joined to the heart, and is the same that in fig. 24 is represented by KL; in the said fig. 25, we may observe how the sinewy parts and their branches run from QR to T. This instrument, or vessel, is very soft in its parts, and it seemingly is not strong.

Fig. 26 is that vessel dissected, which in fig. 24 is represented by ION; which vessel is exceedingly thick and strong, and like that represented in fig. 25, provided with strong sinewy parts, that when the parts are extended by the blood poured into them, they may be able both in roundness and length to convey the blood into the arteries: these parts, by reason of their great numbers, cannot be delineated in such manner as they ought to be.

From the whole I conclude, that the heart protrudes the blood gently into the arteries; and that the blood, which flows from the veins into the heart, causes that sudden revulsion, called the pulse; both because it cannot so immediately pass through the valves, and because the veins in that part are a little narrower, by which means there is a kind of stop or intermission in the circulation of the blood: and this I conceive is the cause of that motion which we call the pulse.*

Several Experiments on the seeming Spontaneous Ascent of Water. By Mr. Fr. Hauksbee, F.R.S. N° 319, p. 258.

EXPER. I. To satisfy myself, whether the form of the vessel might not contribute to the spontaneous ascent of water in tubes, &c. I procured a couple of glass planes, about 7 inches long, and 1½ broad; these planes were part of a broken looking glass; and though when clapped together, they seemed to touch one another in so many parts, yet when they came to be immersed in a liquid, it would ascend between them; but it was so thin and colourless, that it could not easily be discerned; but on separating them, they would be found

* It is scarcely necessary to remark, that Mr. L.'s conjectures respecting the cause and nature of the pulse are extremely erroneous and absurd.

wet on all their parts: therefore to make it more obvious, I put a small piece of thin paper on each corner; by which means, when laid the one on the other, they became separated by such a distance, as is equal to the thickness of the paper. In this manner I plunged one end into some strongly tinged liquor, when immediately the water ran slowly and gradually, sometimes higher in one part than in another, shooting itself very prettily into branches, and so would continue, till it had arrived at its greatest height; which would be according to the distance the planes were placed asunder: for if two pieces of paper were laid on each corner of the planes, the water would not ascend so high between them, as when they were separated by only one. And then, if the planes were at all declined, the water would still spread itself farther and farther, according to the degree of declination.

Exper. II. Having seen the success of the former experiment in the open air, I wished to try what would be the effect in vacuo. Accordingly I fixed the two planes in such a manner to a brass wire, which passed through the cover of a receiver, that I could make them descend at pleasure. In this manner I conveyed them with a dish of tinged liquor, within the receiver; which being placed on the pump, I proceeded to exhaust its contained air. That being done, I plunged the planes (separated by pieces of thin paper as before) into the water, where also it ascended between them; only there appeared more intervals, or spaces, between the branches of the ascending liquid, than in the former experiment; but when I admitted the air, those intervals vanished, and an intire body of the liquid succeeded: yet the exact form of the upper parts of it remained unaltered.

Exper. III. By these experiments I found, that neither the figure of the vessel, nor the presence of the air, did anywise contribute to the production of the appearance. To try therefore whether the quanity of matter would help to resolve the mystery, I took two tubes of an equal bore, but of very unequal substances, one of them being at least ten times the thickness of the other; yet when I came to plunge them into the liquid, its ascent seemed to be alike in both. In order therefore to endeavour to account for the phenomenon, I would propose a magnet, that operates with equal vigour under similar circumstances. Thus, 1st. A magnet of any form will attract iron; so by the first experiment, the figure of the vessel seems no way to contribute to the ascent of the water. 2d. The magnet is no ways lessened in its vigour of attraction, even in so thin a medium as a vacuum: so, by the second experiment, the presence of the air is no ways necessary to assist in the ascent of the water, in small tubes, or between the planes. 3dly. A magnet, as suppose one of a pound weight, that will take up, or suspend a piece of iron of the like weight, and no more; this magnet, (supposing it to be in every part of equal virtue) when separated and broken into a number of very small parts, and these dressed and armed according to art, will then be capable of suspending 50 or 100 times more the weight of iron among them, now separate, than they could when all in one mass; whence it appears, that the attractive quality of the stone seems to be increased in proportion, as its superficies is to its bulk of matter: so, by the third experiment, I found that the greater quantity of matter in one vessel, more than in the other, contributed nothing to the ascent of the water, which seemed wholly to depend on the largeness or the smallness of their cavities, as to the height it would rise in them; and as their cavities are lessened, so the disproportions of their inward surfaces to their cavities are increased.

And as the magnet, when broken into the said number of small parts, will attract more, than when united in one mass, and is no more than separating, or working the thick body of glass into a number of small tubes, that is multiplying the surfaces; the water then would rise in each of them singly, as it would when all in one body, its cavity being the same with the others; by which means, the quantity of water ascending in them is augmented from the same quantity of matter.

To conclude: There seems to be such an agreement in the qualities or dispositions of one with the other, that I see no reason why the facts proceed not from one and the same cause; for as the inner surfaces of the tubes are made smaller and smaller, so the power of their attraction (as is visible by the higher ascent of the water in them) is greater and greater, and is most demonstrable by the experiments of the planes; for their inner area being always the same, so that as they are placed nearer and nearer to each other, the cavity or space between them becomes less and less, and consequently the disproportions are increased, by which the power of their attraction is augmented.

Exper. IV. I took a glass tube, about 32 inches long, the diameter of its cavity near three quarters of an inch: this I filled at one end with ashes, finely sifted, having first tied a piece of linen cloth over the other, to prevent their falling out. At every small portion I put in, I rammed them strongly down with a rammer, whose basis was very little less than the bore of the tube; by which means I crouded the ashes as close together as possible. When the tube was full, I tied over that end of it by the neck a small and limber bladder, having first expressed all the air out of its body, in order to receive that air, which I expected would be forced through the ashes on the ascent of the water. In this manner I plunged the end of the tube, to which I had tied the linen, under the surface of water in a glass, and found the water presently begin to ascend in it: it rose a pretty pace at first; for in 16 minutes time it had ascended near an inch and three quarters: but as it rose higher, its progress became slower; for at the end of 24 hours, the water had attained only to 16 inches; the bladder at the top being near half filled with that air which had deserted the ashes as the water ascended in them. At the same time I found the upper part of the tube, to which the bladder was tied, to be cracked round, and soon after dropped off. However I had the satisfaction desired. And thus continuing the experiment, I found at 24 hours from the last observation, that the water had ascended in the ashes 6 inches higher, which was very discernible by the change of colour it gave them, distinct from those that were dry.

Again, at the like distance of time from the last notice, the water had risen 4 inches and a half, and something better. On the 4th day, at the usual time of observation, it had ascended 3 inches higher: and when the following 24 hours were finished, the water reached within half an inch of the top, by its ascent of 2 inches. About 10 hours after, it had completely reached the extremity of the tube. Then desiring to know what quantity of water the ashes had absorbed, I weighed a glass of water nicely, part of which I poured into the glass, in which the tube had all along been kept, till it reached the mark where the surface of the water stood at, when the tube was first plunged into it; and found the quantity equal to the weight of 1792 grains, which is nearly the bulk of 7 cubical inches; the capacity of the whole tube, in which it rose, being equal to only 13 inches of the same denomination. Now this experiment to me seems surprising enough from the following observations.

1st. That the water not only ascended in the ashes, as between the before-mentioned planes, and in the small tubes, contrary to its natural gravitation; but with such a power too, as to force and drive away pretty strongly imprisoned air, which was contained in the interstices of the rammed ashes. 2dly, That the removal of this imprisoned air could not be done without a power surmounting its resistance, which must be great, since upon endeavouring to force air through the body of ashes by the strength of my breath, when the tube was not above half filled, it proved unsuccessful. Not but that I believe, if the same force had been continued for some time, it would have found its way through. 3dly, That the water ascends fastest at first, when there is a larger quantity of intersticial air to remove, than when the column of the dry ashes becomes shorter, by the higher ascent of the water in them. 4thly, That notwithstanding the tube was rammed as full as possible with ashes; yet their interstices were so many, as to receive or imbibe another body equal in bulk to above half the contents of the whole. 5thly, That the water rose, not only in the ashes adjoining to the inner surface of the tube, but equally in the whole body of it, as I found on examination. 6thly, That the air lodged in the interstices of the ashes, was protruded through them as the water ascended, was manifest by the intumescence of the bladder: and notwithstanding the accident of the bladder's falling off, I cannot but conclude that the quantity of it must be equal to the like bulk of water which supplied its place.

I repeated the same experiment in vacuo, in a tube much about the same diameter as the other, but not above 10 inches in length: this tube, being filled with ashes as before, was placed in vacuo, where it remained some time, to give liberty for the air contained in them to escape. Then plunging the lower end of the tube under some water, I found that the water rose faster in the ashes in that medium, than in common air; for in about 4 hours time, it had reached its top: which plainly shows, that the presence of the air is so far from being necessary in the production of this odd phenomenon, that it is a manifest impediment to it.

The same Experiments continued.—The ascent of water in capillary tubes has been noticed some years since; but that it should rise between two glass planes, whose sides lie open to the air, I had not so much as received a hint of, before I first discovered it. And I find that this phenomenon is not limited to glass bodies alone; for stone, or brass, and perhaps most other bodies that have smooth surfaces, or that their surfaces may become nearly contiguous to one another, may give the like appearance; as is plain by the following experiments. I procured a pair of marble planes, that were ground as true as the workman could make them: these joined together dry, without any thing between, I plunged their edge about a quarter of an inch under the surface of the water, and continued them so for some minutes of time: then taking them out, I found they could not easily be parted without sliding them one from off the other: which when I had done, it was easily discoverable how far the water had made its way between them, which, upon divers trials, I have found different; but at all times, after newly rubbing over the planes with wood ashes, the water would ascend highest. Now whether the small dust of the ashes adhering to the planes may contribute any thing towards it; or that they better clear them from an oily or viscous matter, that may be communicated to them from our hands, I cannot yet determine. I then took a pair of round brass planes, and ordered them as before; the success of which was the same as the former.

We may remark the extraordinary fact of the ascent of spirit of wine, or oil of turpentine between two planes, without any thing to separate them, or held flat together. It cannot be imagined but that these planes must touch each other in a multitude of parts; yet, notwithstanding they are held forcibly together, the spirit of wine will insinuate, and ascend seemingly in an intire body, between all their contiguous parts. And I have since observed, in plunging the planes in spirit of wine, oil of turpentine, and common oil, that all these different fluids arose between them, as the tinged water; only with this difference, that the common oil rose very sluggishly, being near an hour in rising so high between them, as the other liquids would in less than half a minute. They all rose in an intire body, from side to side of the planes, without those intervals or spaces, which generally happen on the ascent of the water. I likewise took a couple of round glass planes, and having laid them on each other, without paper, or any thing else between to keep them separate; in this manner I plunged one edge just under the surface of the tinged liquor, and found the water almost instantly had reached the extremities of them in all parts: by which we find, that the water not only ascends directly upwards, but runs sideways, obliquely, or in any direction.

An Experiment on the different Densities of common Water, from the greatest Degree of Heat in our Climate, to the Freezing Point, observed by a Thermometer. By Mr. Fr. Hauksbee, F. R. S. N° 319, p. 267.

I caused a quart of water to be heated near scalding hot, and then put it into a convenient glass with my thermometer, the spirit in which soon rose to the ball at top, where it remained, till the water cooling caused it to descend: by this time the spirit in the thermometer and the water were become of an equal temperature; and when it had descended to 130 degrees above the freezing point, I began my observations, as below. I weighed a small bottle in it, and found the bulk of water equal to it in that state was 574 grains; when the spirits had descended to 80 degrees above the freezing point, the bulk of water equal to the bottle then weighed 3 quarters of a grain more than before; at 30 degrees above the freezing point, the quantity of water equal to the bulk of the bottle was again increased about 3 quarters of a grain; and at the freezing point, it weighed still something more; in all about two grains from 130 degrees above the freezing point, to that very point. Which seems considerable, and ought to be noticed by gentlemen who judge of a mineral or any other water by its weight, when they have not an opportunity of making the experiment at the fountain head; for there I suppose the water is at the same degree of temperature at all seasons.

Now according to this experiment, I find that water is condensable by cold one 28th part of the whole, from the greatest degree of heat in this climate. Supposing then, that the water in the sea should suffer the same alterations by the change of the different seasons, (as I see no reason but very nearly it must) it would be easy to compute, that a ship which should draw two fathoms, or 12 feet water, in such weather as is understood by the greatest degree of heat, whould draw about half an inch less from the greater density of the fluid, when reduced to the forementioned degree of cold; and consequently would sail better at that time. And since water is thus capable of dilation and contraction by heat and cold, I see no reason why the same may not be performed by force, notwithstanding the many attempts to determine it have as yet been fruitless. For since the constituent parts of the fluid are capable of being removed to greater distances from one another by heat, and become more closely united by cold; so I conclude, that there must be some body contained in it of an elastic quality, which must be subject to the same laws of such a body: that is, be capable of compression by force, as well as to become more dense by cold.

Some Experiments on the Weight of Common Water, under different Circumstances. By Mr. Fr. Hauksbee. F. R. S. N° 319, p. 269.

First, I took a glass of common water, and having nicely weighed a glass bottle in it, whose bulk was equal to the bulk of 575 grains of the fluid, I then caused some of the water to be boiled over the fire; after which, it was included in vacuo, and there remained till it became of the same temperature with common water. Thus I endeavoured as much as possible to extricate all the air out of the water, thinking in that state it would become more dense than when I first weighed the bottle in it; but, contrary to expectation, I found that the bottle had just the same weight in it as before; which seems to confirm the impossibility to compress water by force into a less space than it naturally possesses: for if, on the removal of such a quantity of air out of its body, the parts do not slide any closer together, how should a weight laid upon its surface, when its interstices seem to be replete with air, make any impression on it? The body forced out of the water, by the foregoing means, I call air, since it is subject to all the same laws with it; but that the water, on its absence, should not unite more closely than before, seems very surprising; for I cannot conceive what matter must supply the vacancies, since the particles of water themselves remain at the same distances as if the air had not been withdrawn, otherwise the water of necessity must become more dense. But to proceed; I caused some water to be heated about blood-warm, then weighing the bottle in it, I found the bulk of water equal to the bulk of the bottle was about 3 grains less than when cold; which shows that the component parts of the water are easily separated by heat, and the matter lodged in its interstices capable of dilation. I then took that water I had purged of all its air, (as near as I could) and gave it a degree of heat not exceeding luke-warm; and on weighing the bottle in it, I found that although the heat it had received was very inconsiderable, yet the bulk of the water in that state, equal to that of the bottle, was now diminished 2 grains: which plainly shows, that although the water contained no air that I could discover, yet there seems a matter latent in it, capable of intumescence.

Of the Nature and Properties of Sound. By S. Guido Grandi, F. R. S. N° 319, p. 270. Translated from the Latin.*

The archbishop of Armagh (in N° 156, Philos. Trans.) compares hearing with vision; and as the latter is divided into direct, reflected, and refracted vision, so in like manner he divides the former into three parts, and considers not only direct and reflected sounds, which have been long known, but also refracted ones: for, as he observed that former ages had, in a great measure perfected the doctrine of vision, by optic, catoptric, and dioptric inventions, so likewise he did not doubt, but that the doctrine of sounds, both with respect to the object, and the medium or organ, might be brought to perfection by acoustic, catacoustic and diacoustic, or (as he denominates them both ways) by phonic, cataphonic, and diaphonic instruments. And for that end he lays

* Guido Grandi, a learned mathematician of Italy, was born at Cremona in 1671, named professor of mathematics at Pisa in 1714, and died in 1742. In 1699, this author published, in 4to. the demonstration of Viviani's wonderful or quadrable dome, under the title of Geometrica Divinatio Vivianeorum Problematum: a work which contains more than its title would lead us to expect, and in which the author remarks many other curiosities in geometry of the same kind, and among others, a portion of the surface of a right cone, which is perfectly quadrable, and to which he gives the name of Velum Camaldulense, he being a friar of the order of the Camaldules; from which it seems he did not know that the same thing, in a more general form, had before been given by John Bernoulli, in the Leipzic Acts. In 1701 he published his demonstration of Huygens's theorems on the Logistic Curve, which that author had simply announced without demonstration; being an excellent specimen of the ancient geometrical method; in which piece also, as well as in his letter to the Jesuit Ceva, which follows it, are found several other curious and novel particulars. Another paper of Grandi's is also inserted in the 33d volume of the Philos. Transactions, called a Handful or a Bouquet of Geometrical Roses, being a dissertation on certain curves geometrically described in a circle. This he afterwards enlarged in another treatise, published in 1728, entitled Flores Geometrici ex Rhodonearum, &c. He was also author of several other miscellaneous pieces on the ancient and modern geometry; as, his Quadrature of the Circle and Hyperbola by Infinite Parabolas, in 1703 and 1710; his Dissertation on Infinites of Infinites, &c. in 1710; an Italian edition of Euclid's Elements; and a posthumous treatise on Conic Sections, also in the Italian language, in 1744. Grandi it seems was of a turbulent and quarrelsome disposition, being almost always engaged in disputes on various subjects, geometrical, theological, metaphysical, or philological.

down some problems, which he not only exhibits without any demonstration, but without any construction: yet the bare comparison between optics and acoustics, does not seem sufficient to explain the latter, especially as they differ in so many respects; for light is always propagated in right lines, sound every way, and in curve lines; and notwithstanding the interposition of any opaque body, becomes sensible.

And even what the archbishop lays down about the diffusion of sound, plainly shows the difference between that and the propagation of light; for he says that sound glides with ease and great swiftness along walls, or very smooth arches, of an elliptic or cycloidal, rather than a circular form; and is also more strongly conveyed along the soft surface of water, which yields to those sonorous tremours by which the air is undulated. As to the ellipsis, this only is demonstrated from catoptrics, that rays of light proceeding from one of its foci D, fig. 10, pl. xi, and falling on the elliptic curve ABC, shall after reflection be collected in the other focus E; but if the rays proceed from any other point, besides the foci, they will no longer meet in one point, but be reflected in such a manner, as to form, by their contacts, the caustic curve F f; so that should the eye be posited above its convexity, it would receive one or two reflected rays, and no more; but situated in the curve itself, some of the nearest rays would fall upon it, and within its concavity it would have none at all reflected to it.

As to the cycloid, J. Bernoulli shows, in Act. Lips. 1697, that if a ray of light should pass through mediums, whose densities in every point should vary in the subduplicate ratio of their heights, it would be continually refracted in such a manner, as to form a cycloid: but I do not see how the figure of a cycloid would contribute to the more easy propagation of light, either by reflection, or a direct propulsion through the same medium; for this curve has no foci at all, so that it cannot collect rays in any point, but the rays reflected from it form irregular curves; excepting, when the rays PM and QN, fig. 11, parallel to the axis KL, fall on the cycloid EMKNH, then the caustic formed by the contact of the reflected rays MR, NS, would consist of two cycloids ERL, HSL, generated by a circle of half the diameter, and exhibit the greatest number of reflected rays about L, the termination of both, and the middle of the base of the reflecting cycloid: but in these, as well as in other caustics, resulting from any position of the luminous point and the rays, the same observations hold, as were said above, to agree to caustics formed by the ellipsis.

As to the plane superficies of water, it is manifest that the rays of light pass through it, either altogether refracted, or regularly reflected towards the opposite parts, in the same manner as from the superficies of solid crystal; so far are they from moving with ease and expedition along the surface of water, as sounds are said to do; nay, it may be doubted, whether the best polished mirrors would contribute so much to the reflection of sound as of light, seeing an echo is more frequently reflected from the roughest recesses of caves, from desert vallies, and ruinous edifices, rather than from the smoothest and best plastered walls.

Here I shall attempt to explain, or rather guess at the meaning of the Archbishop's acoustic or phonical sphere, after first quoting his words: "I shall here add," says the Archbishop, "a semi-plane of an acoustic or phonical sphere, fig. 12, as an attempt to explain the great principle in this science, which is the progression of sounds; you are to conceive this rude semi-plane as parallel to the horizon; for if it be perpendicular to it, I suppose the upper extremity will be no longer circular, but hyperbolical, and the lower part of it suited to a great circle of the earth, so that the whole phonical sphere, if I may so call it, will be a solid hyperbola, standing on a concave spherical base."

Instead of this scheme I would substitute another, fig. 13, let CGFE be the globe of the earth, and let a sound be produced at a point of its superficies, as C; this will be propagated every way through the earth itself, as well as through the air, so when it reaches either really (though perhaps insensibly) or at least (were it stronger) would reach on its diffusion through the air a great circle of the earth, described from the pole C, viz. the periphery GBE, it would fill up a certain space, according to the different facility of its passage, not quite spherical, but unequally extended, and circumscribed by the perimeter of the hyperbola GLAKE, described about the axis CAO perpendicular to the sonorous body C; nay defined by the superficies of the hyperbolic conoid, generated by the rotation of the hyperbola ALG round its axis. Therefore the whole phonical sphere, through which sound is propagated in a given time, will be the solid space comprehended by the hyperbolic conoid GAEB, insisting on GBE a great circle of the earth, and terminated below by the concave hemispherical superficies GCEB; which space cut any where by a plane parallel to the horizon, will give the semicircle LIK, such as the Archbishop's figure represents, and which he calls a semi-plane, because it exhibits only one half, the other half not appearing, as lying beyond the vertical hyperbola, which divides in two the phonical sphere through its axis.

But the Archbishop neither intimating the species of the hyperbola, nor the principles on which this doctrine is founded, I propose to find, by the inverse method of investigation, the following particulars. 1. Through what lines sonorous tremours must be diffused, so as to be expanded in a given time into such a hyperbola. 2. The variation of density to be supposed at different heights of the air, to give sound a direction (supposing the common law of refraction of light) in those lines; and 3. The law of refraction, which sonorous tremours forming such curves, do observe, supposing the variation of the density of the air, as most philosophers and mathematicians allow, and as is consonant to experiments, to be in the ratio of the weight of the incumbent atmosphere.

For which, let us consider that the sonorous body c, fig. 14, communicates its tremours every way, in the directions cn, cm, ch; or at least in the direction of the lines wherein the impulse was given to the body on its restitution, repelling the air, and protruding it by frequent oscillations, by which it undulates, and is solicited to a tremulous motion in the same direction. Let therefore these tremours be supposed, in a very small time, to reach the points n, m, h, and from thence after any given time to be propagated together, the first tremour to the point N, the second to M, and the third to H; and again, after any other given time, to be propagated together, the first to G, the second to L, and the third to A. Now I denominate the lines cn, NG, cm, ML, ch, HA, in which each tremour is successively diffused by the sonorous rays; and the lines nmh, NMH, GLA, which the above sonorous rays, and all the other intermediate synchronal rays, reach in any given time at once, the sonorous waves. And indeed it is plain, that in a medium entirely uniform, when the cause ceases, that diverts the sonorous tremours from their direction into this or the other course, the sonorous rays always proceed in right lines, or move directly the shortest way from one point to another, and form waves entirely circular, and concentrical to the sonorous body; because, when they find no greater difficulty in their passage in one place than in another, they will reach equal distances in any given time; and each ray will intersect its wave perpendicularly, and each wave be concentrical and similar, as appears from the elements.

But in a medium of a difform density, as our air, in which it varies according to its different altitude (for here we do not now consider the vicissitudes of heat, cold, moisture, and dryness, which cannot be reduced to any certain law) the ray CHA alone passing perpendicularly through all the lamella, or superficies of air, concentrical to the earth, will be unrefracted and proceed in a right line; while the other rays impinging obliquely on the same superficies, will be continually refracted at each point, and inflected into the curves CmML, CnNG; and according to the various facility of their passage, be propagated in any given time to different distances; wherefore the points A, L, G, or H, M, N, which the sound emitted in the direction of any ray shall reach in the same time, will be unequally distant from the sonorous body C; and therefore the waves ALG, HMN, hmn, will by no means be concentric circles to the sonorous body, but curves of another kind, which yet must be similar to each other, and similarly posited; therefore, if in the Archbishop's hypothesis, the extreme wave ALG, touching the outmost confines of the terraqueous globe, be an hyperbola, all the other intermediate waves HMN, hmn, must also be similar hyperbolas, and similarly posited; and though described from different vertices A, H, h, yet from the same centre to the same axis, and under similar figures of the latus rectum et transversum; for whatever reason proves, on account of the simultaneous appulse of sound to the points A, L, G, in the synchronal rays CHA, CML, CNG, that ALG becomes a curve of such a kind, suppose a hyperbola; the very same reason will, on the same principles evince, that also, on account of the simultaneous appulse of sound to the points H, M, N, along the synchronal rays ChH, CmM, CnN, the wave HMN will become a curve of the same kind, viz. in this case, a similar and similarly posited hyperbola, as is evident. And moreover it is plain that the sonorous rays CHA, CML, CNG, should always intersect those similar waves ALG, HMN, hmn, perpendicularly, or at right angles, as is the case in circular waves; as Huygens demonstrates of the waves of light, in p. 44 of the French edition of his Treatise on light.

Therefore the investigation of the path, along which the sonorous rays, according to the Archbishop's hypothesis, are propagated, is reduced to this purely geometrical problem, viz. "To find the nature of those curves that intersect perpendicularly any similar hyperbolas, and similarly described from the same centre, and about the same axis." Let the similar hyperbolas ALG, HMN, hmn, fig. 15, and other innumerable intermediate hyperbolas, be similarly posited, either above or below, having the same common centre O, and described about the same axis OAH, whose conjugate is OS: through the point C draw the curve CmML, or CnNG, intersecting all the given hyperbolas perpendicularly: and through the given point C describe between the asymptotes OA, OS, the hyperbola CmML, of such a nature, that putting the ratio of the latus transversum of the former hyperbolas AL, HM, &c. to the latus rectum of the same, equal to the ratio of t to r; and supposing the powers of the ordinates LQ, denominated from the exponent r, to be reciprocally proportional to the powers of the abscissæ OQ from the centre; that is, putting OQ $= x$, and QL $= y$; so that $y_r = x^{-t}$; or drawing any other ordinates mi, MI, so that the ratio of the distances OQ, OI, from the centre, be reciprocally as the multiplicate of the ratio of the ordinates IM, QL, as the fraction $\frac{r}{t}$ is multiple of unity. This I say resolves the question: for drawing the tangent LP of any hyperbola AL, to a point where it is intersected by the curve CML, as also SLR the tangent of the hyperbola CML to the same point, it is plain, from what I have shown in the demonstration tangents, drawn from the centre of the earth through the extremities of the vibrating fibre; and indeed the tremours of that fibre would not be propagated in any other direction than in the lines T2, T3, T4, and other intermediate lines, comprehended by the angle 2T4, and corresponding to each particle of the same fibre; therefore the space without the said hyperbolas 298g, 476g, would have no harmonical tremours, nor according to the Archbishop could the phonical sphere be extended to the entire hemisphere of the earth; therefore no one single fibril of a sonorous body can ever really vibrate, but it must also affect, and in like manner solicite to harmonical tremours other fibres, with which it is connected, and between which it lies distended; which again must affect others; just as the tense string of a musical instrument evidently communicates its tremours or vibrations to the wood to which it is fastened; therefore on striking the fibre of a sonorous body, the harmonic oscillations are transfused into other bodies, with which it is either mediately or immediately connected; though they being always more and more weakened, and at length becoming insensible, are farther and farther diffused over the superficies of the terrestrial hemisphere, as appears on applying the ear to the ground, or hearing great sounds at a considerable distance: therefore other sonorous hyperbolic rays proceed also from other places, by which the Archbishop's phonical sphere may be sufficiently filled.

To proceed to the second query, and give a more general solution of it; suppose any ray, either of light or sound, NNG, changed by its continual refraction into any curve; the question is, to find by what law the density of the medium at different heights must be supposed to be varied, that supposing the sign of refraction to be always proportional to the density of the refracting medium, that ray may be formed into the curve sought.

Let the axis of the curve NNG, fig. 16, which the refracted ray describes, be the right line CO, in which taking any point C, describe with any radius CL the quadrant of a circle LPP; and drawing any where a tangent NR, nr, to the refracted ray, from C draw a radius parallel to the tangent, and meeting the quadrant in P, and drawing PF parallel to the axis, let it meet the ordinate NQ, perpendicular to the axis in the point F; the curve, FFF, hence formed, expresses by its ordinates FQ, fq, the densities of the medium, at different heights; for CP being parallel to RN, the angle PCB will be equal to the angle which the refracted ray NN forms with the perpendicular in the point N; and therefore BP, or FQ will always be the sine of refraction, taking CP for the radius; therefore, supposing this law of refraction, viz. that the sine of refraction is proportional to the density of the medium, the same FQ will express the density of the medium, at the height Q or N, a point equally high, through which the ray passes. Q. E. D.

In our problem, where QN $= x^{\frac{-t}{r}}$, because $y^r = x^{-t}$, if FQ, the exponent of the density of air, be called z, it will be z $= \frac{t}{\sqrt{x^{2r+2t}+t^2}}$, or z $= \frac{t}{\sqrt{x^2+^{2t}+t^2}}$ by taking r and CP for unity; and supposing the hyperbolic wave to be equilateral, and consequently the ray a similar equilateral hyperbola, then $y = \frac{1}{x}$, and because $t = 1$, it will be z $= \frac{1}{\sqrt{x^4+1}}$.

But because both Mr. Herman, in Act. Lips. 1706, and Dr. Gregory, in his Astron. lib. 5, demonstrate, that the curve which determines the degrees of the density of the air, is the logarithmic; so that the heights OQ, oq, or x, are the logarithms of the exponents of the density of the air at the points Q, q; it is plain that the curvature NG, of the continually refracted ray NN, is described by such a law, that the cosines of incidence and refraction, raised to the power $\frac{r}{r+t}$, have a ratio compounded of the ratio of the right sines, raised to the like power, and of the ratio of the logarithms of the densities.

Moreover, though we should allow that the common law of the refraction of light gives the sines of incidence and refraction, proportional to the densities of the mediums, yet that proportion may not possibly be so exact, seeing the ratio of the sines in the refraction out of air into glass, is nearly sesquialteral, and air upwards of 1000 times more rare than glass; but when geometricians found that the sine of refraction became greater, in passing into another medium, according to the greater facility with which light penetrates that medium in the common hypothesis, or according to the greater difficulty in the hypothesis of Descartes; who, on the contrary, supposes that light suffers a greater refraction, on account of the greater difficulty, in a rarer than in a denser medium, (as heavy bodies, because of the greater difficulty in penetrating denser bodies, are more refracted in these, by receding from the perpendicular) and that both laws agree in this, viz. that according to the greater rarity of the medium, the greater would be the refraction; hence it came to prevail, that the sines were said to be proportional, not to the facility or difficulty of their passage, one or other of which is called into question by some, but to the rarity of the medium, in which all agree, though the true proportion does not entirely answer in geometrical rigour; therefore, whenever mention is made of density or rarity, perhaps the facility of passage in the common hypothesis, and difficulty in the Cartesian, might be substituted for it: excepting where it is said, that the rarity varied by the weight of the incumbent air answers to the heights, as numbers to their logarithms, which is strictly true.

Concerning some Roman Antiquities found in Yorkshire; and a Storm of Thunder, Lightning, and Rain, that happened there. By Mr. Ra. Thoresby, F.R.S. N° 319, p. 289.

One of the three Roman monuments lately found at Adellocum, has been evidently an altar, having the discus or hearth very plain on the top; another, though made exactly in the form of an altar in all other respects, yet wants the discus or lanx on the top, and I have never yet seen any altar, without one; it seems too small also, (being only 18 inches high and 6 broad) for a commemorative monument; the three rolls or wreaths on the top, are so entire, that it is plain there never was any thing else wrought upon it: now whether any of the Roman aræ, or altaria, were made without a discus or hearth, is what I desire to know.

The more immediate occasion of this is to acquaint you with some of the effects of a late storm of thunder, lightning, and violent rain, which happened the 5th day of the last month (Aug. 1708). I was then at the Spa at Harrowgate, near Knaresborough; where having a spacious view on the open forest, I observed the motion of the clouds and storm, which began in the west, wheeled about, by the north and east, to the south: when night drew on, the lightning appeared dreadful, the intermission between the flashes being very small, and the claps of thunder very loud. The lightning burnt down a barn near Scarborough. One Thomas Horner, with others, flying from the violence of the rain, which seemed rather to fall in spouts than drops, took shelter in a neighbouring barn; from whence, after several dreadful thunder-claps, they were driven by the bolt, as they termed it, but really the lightning; which singed the hair of the said Thomas Horner, threw another man backwards, who was climbing up the hay-mow, left a sulphureous stench behind it, and at length burnt down the barn and hay. As to the inundation, it was surprising; it tore up much of the road, and street, from the church to the bridge, and made pits in some places several yards deep, threw down part of a barn and a stable, and rushed into most of the houses in the town: in some the water was as high as the soles of the windows, and blocked up the door of one house with gravel, almost to the very top: several persons were in great danger, but only one woman drowned; she was hurried away by the violence of the stream, and not found till the 4th day after: it removed the bole of a large oak several yards off; bore down the most part of 4 wooden bridges; and has left at the end of the great stone bridge, or within about 100 yards of it, as much gravel, &c. as is computed at above 1000 cart loads; one neighbour gives 10 pounds for removing the stones and gravel left in a small tract of ground. This seems very remarkable, because effected by the rain alone; for notwithstanding all this deluge, the river Nidd kept within its bounds.

Microscopical Observations on the Palates of Oxen, &c. By Mr. Anthony Van Lewenhoeck, F. R. S. N° 320, p. 294.

I took a cow or ox's head, and cut out the roof or palate, close to the throat, while yet warm; and pressing it gently, I could perceive issue out of several parts of it, small, round, protuberant, transparent drops; and pressing it a little harder, there ensued a yellow moisture. I took the uppermost skin of the said part, and viewing it through a microscope, I observed, that at most of the places from which the said liquor proceeded, there was a round ring or circle, of rather a darker colour than the skin or membrane that lay next it; I could also perceive in some of the said places, out of which the liquor came, that there were small holes or orifices, and these moist places were not all at equal distance from each other.

I discovered also in the said skin or membrane, a vast number of exceedingly small protuberances; as also, in the uppermost thin skin, holes so very small, that they almost escaped the view through a microscope. On viewing the rough skin that lay under the thin skin, I perceived such slender fibres or bristles, of a darkish colour, that passed straight through the said skin, corresponding with the small protuberances and little holes, discovered in the uppermost skin. From this observation I imagined, that the last mentioned holes or orifices, and the little fibres which I saw in the thick rough part, were those long particles that receive the juices, and which also produce that sensation called taste.

Fig. 27, pl. 10, represents through the microscope a small particle of the above-mentioned membrane, in which the round protuberant particles are opposed to the sight; where EF show the valves, which are seldom so close together as they are here shown; and in the middle of which, in the dark part of them, I often discovered an orifice or opening, which is entirely closed up when there were no juices conveyed out of them.

I also discovered several long slender pointed particles, which I conceived to be rooted or planted in the skin with a pointed end, and that these caused the aforementioned protuberances. And as these pointed parts, which were fixed in the said protuberances, were opposed to the sight with the points uppermost, one could not easily make any observation of them; I therefore cut off one of the slender particles from the rest, that I might give the better view of the pointed parts. Accordingly, fig. 28 represents a small particle of the said long particle, as it appeared through a microscope, of which FG shows the undermost part, being as it were the socket of the pointed parts IH.

I was desirous to search into the inward parts of the nostrils of an ox, as well as I was able; in doing which, I saw that each side of the mouth, which one might call the lips, was furnished with a great many pointed parts, that were very thick in the inner skin, and being round ran into a very slender point. I likewise observed the skin of several of the said parts, which were very strongly united to the parts that lay in it; and found that one of those parts that lay within, consisted of a great many pointed particles, which were much thicker and longer than those I had discovered in the inward parts of the tongue. I caused a very small particle of the foresaid parts to be drawn, as it appeared to the naked eye, as shown in fig. 29; only with this difference, that this is not so thick and large as it should be, because the parts were dried and shrunk in, and they were also of the smallest size of any that I had dissected.

An Experiment of the Freezing of Common Water, and Water freed of Air. By Mr. Fr. Hauksbee, F. R. S. N° 320, p. 302.

This experiment was recommended to me, in order to discover what difference would happen in the swelling or bulk of ice, producible on the freezing of common water, and water cleared of air. Accordingly I procured a couple of glasses, in form of fig. 1, pl. 12; which when filled with the different waters to a determinate height, supposing at aa, I conveyed into the freezing mixture, being only a composition of snow and bay-salt powdered pretty fine; where having remained 3 or 4 minutes, the congelation began in each of them, which was very discernible, by the ascent of the water in the tubes, above their first heights aa; and in about an hour's time, it had ascended in that glass, which contained the water freed of air, at least 6 inches; but in the other glass with common water, not quite 5 inches; there being such a disparity in the contents of the two glasses, the last mentioned being less by a 5th part than the other, which contained not full 4 ounces. It was observable, that during their continuance in the frigorific mixture, small bubbles of air continually ascended in that which was filled with common water, but not the least sign of any such appearance in the other. After this, taking the glasses out of the mixture, I poured from them the unfrozen water, which gave me the opportunity of discovering the various forms the newly made ice had shot itself into: that glass which contained the purged water, appearing all over the sides and upper part of it, to the very neck bb, of divers figures, much resembling those of salts: the bottom part of it cc, seemed to be solid, but whitish, as if full of very minute interspersed vacuities; but not like those cavities which are very observable in the freezing of common water. But at the bottom of the other glass, they appeared in great numbers, of a longish form, seemingly pointing all round from the circumference to the centre; and there were none of those salt-like figures on the sides of this, as on the other, but it was almost clear from any adherence of ice, excepting towards the upper part near the neck, where a little had fastened itself with those longish bubbles, pointing from that part downwards, inclining to the centre. From all which I conclude, that the ice produced from the water purged of air, was equally augmented in its bulk to the quantity of water from which it was produced, as that which proceeded from the frozen common water; for had the glasses been of an equal content, I see no reason to doubt, but the water would have been equally frozen in both, and the ascent of the unfrozen part of them would have been much the same in their tubes. But if there be any difference, the water purged of air seems to claim the easier disposition to be frozen.

The water I freed from air in the following manner, viz. I first boiled it well over the fire; afterwards I included it in vacuo, where it remained in that state till it was cold; from whence I took it, and proceeded presently on the experiment, which on two trials succeeded alike.

An Experiment of the Freezing of Common Water, tinged with a Liquor said to be extracted from Shell-Lac. By Mr. Francis Hauksbee, F. R. S. N° 320, p. 304.

This liquor is a very deep red; and a small quantity of it will tinge 20 times as much of common water of a very good sanguine colour, hardly transparent. I found that this liquor, extracted from lac, would not freeze; for during the coldest weather we have lately had, it retained its fluidity; and when it was mixed with water, and exposed to freeze, the water, in which it was mixed, soon congealed; and so much of it as underwent the change, appeared of a fine but pale transparent red; the body of the colour retiring into the middle, in form of aa, fig. 2, pl. 12, and was quite opaque. And when no more of the mixed liquor would freeze, I took the body of ice out of the glass, by just warming its sides by a fire; and pricking the dark part of it with a piece of wire, the red liquor immediately issued out through the hole I had made, seemingly as pure and as abstracted from any mixture of water, as it was before it was put into it. This red liquor I found to be somewhat specifically heavier than common water. Another thing very remarkable, was, that this retired liquid, as it seemed to keep at an equal distance from the sides of the glass, so also did it from the bottom and top; which upon repeated trials answered alike.

I mixed also some common water with a strong purple liquor, made from logwood boiled in water, in which some alum had been dissolved. A little of this would give a strong tincture to a considerable quantity of fair water; and when exposed to freeze, would retire towards the middle leaving the first frozen water of a very pale purple, in comparison of the middle part; which when I had taken out of the glass that contained it, and broken it, I found it was frozen through, but of so dark a colour in the middle, that it came near a black.

An Experiment of Weighing in Common Water, Bodies of the same Species, and of an equal weight in Common Air, but of very unequal Surfaces. By Mr. Fr. Hauksbee, F. R. S. N° 320, p. 306.

I took an exact square inch of sheet-brass, weighing just 482 grains: I then cut as many square inches of brass tinsel, as were equal to the same weight; the number of these square inches being 255. Now these being of an equal weight with the other single piece in common air, I concluded from the inequality of their surfaces, that a considerable disproportion in their specific gravities would ensue, by weighing them in water; the water in one touching so many parts of the superficies more than in the other:* and from what is generally asserted, that the smaller the bodies are, so the disproportion of their bulks to their superficies increases; and that supposing them infinitely small, or as gold dissolved in aqua regia, or silver in aquafortis, that then their superficies being touched by so many parts by the including menstruum, which is in such a disproportion to their diameters or bulks of matter, as disposes them to remain suspended in it. This I take to be the general solution of that phenomenon. Yet when I came to bring it to the test, I was surprised to find only 2 grains difference, the single piece weighing in the water about 422 grains; and all the other bodies together, scarcely 2 grains less. And this upon

* The language employed in this paper shows an extraordinary instance of the vague and confused notions of the writer on some physical properties: and it is truly surprising that Mr. Hauksbee, who invented and executed so many curious experiments, who had the benefit of ingenious authors on hydrostatics, &c. and who enjoyed the advantage of the ideas and conversations of the learned philosophers of his day, should be ignorant of the nature of the specific gravity of bodies, and the manner of determining it. He seems to think it depends partly on the surface of the body, as well as on its bulk or magnitude, instead of the latter only, under the proportional weight. So that, according to him, the half of any body should not have the same specific gravity as the whole of it. His notions also on the support of bodies in fluids seem very crude and incorrect: bodies when reduced to very small parts, as to powder or dust, having such parts often sustained in fluids, not from a change in their specific gravity, but from the viscidity or the cohesion of the parts of the fluid, which the small weight of the particles of the body, in proportion to their surface, is not able to overcome, and force themselves through.

two or three accurate trials succeeded alike. Now since there is is so small an inequality between bodies of the same species weighed in water, whose disproportions of surfaces are, as 1 to 255, (for I reckon the sides of all the tinsel bodies to be equal to the sides of the single brass piece,) I must conclude, that those bodies must be infinitely small, whose inequality of surfaces to their bulks exceeds those in this experiment.

But though the disproportions of the surfaces of bodies, to their bulk of matter, be very great; yet that that is the only reason why a metallic body should be suspended in a menstruum specifically lighter than itself, is very doubtful: for certainly if it was so, we might reasonably have expected to have met with a much greater difference in the bodies made use of in the newly recited experiment: for there it should seem necessary, that where we had so great a difference in point of superficies, there we should also have had a difference somewhat proportional in point of weight; which yet did not happen. I think therefore that there must be some other agent, or quality, not only to assist, but govern in the case. And what is called a corroding menstruum, I take to be a fluid adapted to attract such or such a body, as we find none of them operate alike on all; but aqua regia is adapted to separate the parts of gold, and aquafortis those of silver: now this separation of their parts seems to proceed from a mutual attraction between the menstruum and to the body immersed, and both seem to act on each other with greater vigour, than either of their own particles act on their contiguous ones; by which means a separation of parts must needs ensue. Thus being at liberty, they with the menstruum become as one body, and remain suspended in any part of it by their mutual attraction. And that one menstruum in this case should affect one body more than another, is no more extraordinary than that the magnet should affect iron only.

An Account of some Inundations, Monstrous Births, Appearances in the Heavens, &c. By Mr. Neve. With Observations on a Solar and a Lunar Eclipse. By Mr. Derham. N° 320, p. 308.

On the 7th of October, 1706, after a very rainy day, and a southerly wind, there happened a prodigious flood, which broke down several bridges, and the sides of some of the mountains in the north of Ireland. It came down in vast torrents from some of the mountains, and drowned a great number of black cattle and sheep, spoiled a great deal of corn and hay in the stacks, laid many houses 2 or 3 feet deep in water, and broke down several forge and mill-dams.

Also on July 3, 1707, there happened another flood, which came so suddenly down from the mountains, as if there had been some sudden eruption of water. And on the 26th of the same month, in the county of Antrim, there was a very sudden and surprising flood, which swelled the Six-Mile-River to such a degree, as to break down two strong stone-bridges, and three houses, and carried away 600 pieces of linen cloth, that lay bleaching; it filled many houses several feet deep with water, tore down some large rocks in its passage, and left several meadows covered a foot or two deep in sand. In the south-east part of the county of Derry, they had that day but little rain, with some thunder: but beyond the mountains, in the north-west part of the county, there was a great flood in the river Roo.

There have been also several uncommon or monstrous births, viz. a cow in the year 1706, had calved six living calves. Also, December 6, 1706, a monstrous human birth at Londonderry, viz. with two heads, four arms, and but one body at the navel. It was of both sexes, female on the right side, and male on the left. This child or children were born alive, but lived only a little while.

The last curiosity is, as I imagine, that which some call the northern streaming, which I do not remember the Society had ever any accounts of; and this being one of the most particular accounts I ever met with of it, and very agreeable to such another appearance in the heavens, which was seen in England, in September or October, 1706: Mr. Neve's account being so particular, will I hope be very acceptable to the Society: it is thus. "On Sunday, November 16, 1707, after a frosty morning, and fair still day, the wind north-westerly, about half an hour after 8 in the evening, there appeared a very strange light in the north. The evening was clear and star-light, only the horizon was darkened with condensed vapours in the north, reaching about 10 or 15 degrees above the horizon. From the cloud proceeded several streams or rays of light, like the tails of some comets, broad below, and ending in points above. Some of them extended almost to the tail of Ursa Minor, and all were nearly perpendicular to the horizon, and as bright as if the full moon had been rising in the cloud. But what was most surprising, was the motion of the dark and lighter parts, running strangely through each other in a moment; sometimes to the east, and sometimes to the west. It continued, after I first saw it, about a quarter of an hour, often changing its face and appearance, as to form and light; sometimes broken, sometimes entire and long rays of light in the clear sky, quite separate from, and above the cloud, and none below in the cloud."

To prevent mistakes, says Mr. Derham, I think it necessary to observe, that this light which Mr. Neve saw, is very different from that like the tail of a comet, which was seen in the constellation Taurus, or near it; which I happened to see in 1706, the figure of which is published in the Transactions N° 305, and which some are pleased to call the Aurora Borealis; which name, in my opinion, would better befit this lumen boreale, which is seldom, if ever seen out of the north.

The Eclipse of the Sun on Sept. 3, 1708, *in the Morning, as seen at Upminster, according to correct apparent Time.*—The beginning of the eclipse could not be seen for clouds. At 6h. 44m. 15s. the sun peeped out of the clouds, and I judged, by my eye, that about one-tenth of a digit was eclipsed. —Then clouds nearly all the time of the eclipse. But at 8h. 31m. 15s. a little obscuration appeared through the telescope.—At 8h. 32m. 45s. a very little obscuration, through the telescope.—Then clouds. And at 8h. 35m. 45s. we could discern no remains of the eclipse through the telescope.

From these observations I imagine the end of this solar eclipse was about 8h. 33m. in the morning.

The Eclipse of the Moon, Sept. 18, 1708, *in the Evening, at Upminster.*—As I was that evening coming from London, I observed for half an hour, or more, a thin shade possess that part of the disk where the eclipse began, which remained a good while after the eclipse was over. After I got home, I mounted my telescopes, and got all things in readiness before the eclipse began. And the principal observations I made, were these following:

At 7h. 56m. 30s. a thin penumbra.—At 7h. 57m. 40s. a darker-penumbra. —At 7h. 59m. yet darker, which may pass for the beginning of the eclipse. —At 8h. the eclipse no doubt begun.—At 9h. 1m. the lucid parts of the moon, not long before the middle of the eclipse, were 925 parts of my micrometer.—At 9h. 16m. 40s. diameter of the moon 1634 parts of the micrometer.—At 10h. 23m. 11s. the end of the eclipse draws nigh.—At 10h. 25m. a little obscuration.—At 10h. 26m. less.—At 10h. 28m. 15s. a very little, excepting the duskiness before-mentioned.

Concerning some Roman Antiquities observed in Yorkshire. By Mr. Ra. Thoresby, F. R. S. N° 320, p. 314.

Some Roman monuments have lately been found among the ruins on Adel or Echop-Moor, but having no legible inscriptions. They are only 18 inches high, and 6 inches broad; of a very coarse stone, as Dr. Lister has truly observed, most of those found in the north are. One of these, as appears by the discus, has been one of their portable altars; but another, instead of the hearth, having three entire rolls or wreaths, was certainly never designed for that purpose.

When the vestigia of this Roman station were first discovered, (of which see N° 282 of the Philos. Trans.) I was apt to take it for the Adellocum of the ancients, from some remains of the name in the present Adle or Adel, as it is written, both in the Monastic. Anglic. and some ancient charts in my possession; but having afterwards an opportunity of perusing the Domesday-book, in the Exchequer at London, I found, besides Adele and Echope before-mentioned, another place in the neighbourhood, called Burghederum or Burgdunum, which I am now apt to conclude was the ancient Roman name of this station. That the itinerary is silent herein, is no argument against it: for the names of all the towns in the province are not there recited, but only such as lie on those roads that are particularly mentioned; but that it has at least the appearance of a Roman name, may be argued, because Burgi was the common name for such castles or forts as were convenient for war, and well stored with provisions of corn, as appears by the authorities quoted by Camden and Burton in their notes on the Roman Verteræ, or Burgh, under Stanemoor; and the Burgundians received their name from their inhabiting such castles: it seems probable, that the small squared stones, of which the very antique church at Adel is built, were brought from the ruins of such a castle, and thence gave rise to an old tradition, which continues to this day, viz. that Adel church once stood upon Black-hill, the place where these Roman monuments were found; the elevated situation of which place sufficiently accounts for the termination of the name, the Gaulish or British Dunum, which signifies a hilly or mountainous place, being naturalized in the Roman provincial language. Within a mile of it, there are two scattering houses, that do to this day retain the name of Burden- (for Burgdun-) Head.

Concerning the Icy Mountains of Switzerland. By Wm. Burnet, Esq. F. R. S. N° 320, p. 316.

I went to the Grindlewald, a mountain two days journey from Bern, where I saw, between two mountains, a river of ice as it were, which divides into two branches, and in its way, from the top to the bottom of the mountains, swells into vast heaps, some larger than St. Paul's church; the original of which seems to have been this: the tops of these mountains are covered all the year with snow; this snow melts in the summer, and falls to the bottom, where the sun never reaches; there it is frozen, which happens more easily to melted snow than common water. Thus every year it has increased, till it has reached the very top. The reason why the water has always frozen, though the sun shines on the middle of the mountain, and higher, some part of the day, is that the melted water goes under the ice already formed, and there freezes, and so expanding itself raises the ice above it, and sometimes makes it crack so as to frighten the whole neighbourhood. And the reason plainly appears, because the upper surface being solid, cannot be dilated without making great chinks, and that with a terrible noise. They told me, on the place, that every seven years the mountain increases, and the next seven it decreases; but I doubt their observation is not exact. If there be any foundation for it, it seems to be, that in the hottest summers it increases, and in the more moderate ones it decreases, there being then less melted snow.

Account of a Musket-Bullet, lodged in the Head near 30 *Years*; *and the strange Manner of its coming out of the Head. By Dr. Robert Fielding.* N° 320, p. 317.

At the first Newbury fight, in the time of the late civil wars, the doctor was shot near the right eye on the os petrosum, by which the skull was broken, and a great effusion of blood from the wound, mouth, and nostrils ensued. The surgeon probing the wound for the discovery of the bullet, but failing of his intention, on the third day after the shot, placed him horizontally towards the sun; then, depressing the fractured skull with the probe, he could see the pulsation of the brain, but could not discover the bullet. When the doctor began to grow cold, his mouth closed up in a manner, and so continued for the space of half a year, till many fractures of bones were come out of the wound, mouth, and nostrils; and afterwards, whenever a splinter was to come away, his mouth would close, insomuch that several years after he would prognosticate to some friends, that a bone was then coming out, which continued so for 6 or 7 weeks; at which time finding an itching in the orifice of the wound, he felt a bone with his finger, and it being no larger than a pin's head, he immediately opened his mouth.

At the second Newbury fight it healed up, and no art could keep it open. Afterwards, for the space of ten years or more, there was a flux of sanious matter from the right nostril, and ceasing there, it flowed from the left nostril for some years: at length, for about two years, on riding, the doctor would sometimes feel a pain on the left side about the almonds of the ear, which he attributed to cold, but more especially after riding in a cold dark night, which occasioned a kind of deafness too; and having stopped his ear with wool to recover his hearing, one day in March 1670, it gave such a sudden puff or crack, as made him start. Upon this, all that side of the cheek hung loose, as if paralytic, and a hard knot might be felt under the ear.

Afterwards several tumours succeeded each other on that side under the jaw-bone, which occasioned his consulting some physicians, one of which suspected the bullet. At length the tumours coming to the throat, if he held up his head a little, it seemed as if one with a hook pulled down the jaw-bone; and if any thing touched the throat, it was as painful as if pricked with many needles. Being at last persuaded to make some applications, a small hole appeared, after that another, and then a third, near the pomum adami; by these the bullet was discovered, and cut out in August 1672.

An Account of Books; viz. I. Prælectiones Chymicæ Oxoniæ habitæ a Johanne Freind, M. D. Ædis Christi Alumno. N° 320, p. 319.

The learned author of this treatise, without considering the principles and errors of former chemists, endeavours to give a clear and easy account of the chief operations of chemistry from the true principles of natural philosophy, and chiefly that of attraction; which, he says, is no figment or hypothesis, but deduced from many plain experiments, and grounded on the laws of nature and that relation that is found among bodies, but particularly from the observations that are to be made in chemistry itself. And because chemistry is an art of joining bodies that are separated, or separating such as are joined, he divides the operations of chemistry into two sorts; viz. such as disunite the parts of bodies from one another, and such as compound or mix them together. The chemists not agreeing what are to be put in the second class and what in the first, he follows a new order, and among the first class he reckons calcination, sublimation, and distillation; in the second are ranked fermentation, digestion, extraction, precipitation, and crystallization.

His design in this treatise is to explain first, the method of each operation according to this order, and the mechanical force by which it is produced. Secondly, the different ways by which it commonly is, or may be performed: and thirdly, he gives many particular experiments, which he explains and reduces to the general theory laid down at first. Accordingly we have here the reason of the cohesion of bodies, which he draws from the principle of attraction, and the quantity of contact; the causes of fluidity and liquation; the reason why some bodies, as wax and metals, being melted in the fire, and afterwards cooled, do return to their first form, whereas others by fire acquire a new form: how it happens, that the absolute weight of bodies is generally after calcination increased, and the specific gravity diminished. We have also the reason, why fluids rise in an alembic; and he shows that if a globule of water be so rarefied, as to have its diameter made only ten times greater, it will become lighter than the air, and consequently must rise up in it: but if the diameter be increased in the proportion of 12 to 1, the bubble of water becomes more than twice as light as common air, and must therefore rise so much the faster. Besides this, the air itself being rarefied must necessarily rise up, and the force of its motion carry with it what bodies it meets with in its way; which will more easily ascend according as their surfaces are greater in proportion to their weight. And the author asserts, that if the specific weight of bodies, the force that impels them, and the measure of their surfaces be rightly considered, it will not be difficult to give an account of volatility and fixedness, and all the appearances of distillation and sublimation; in which last only solid bodies are raised by the force of fire.

In his lecture on fermentation, the author deduces the cause of ebullition and effervescence from the attractive force of the particles of matter, and particularly those of salts; which he says are very simple and small bodies, and in proportion to their bulk very solid, and must of consequence be endowed with a very strong attractive power. On which account, and that of the smallness of their force of cohesion, he shows the reason why they are so easily dissolved into water, and not in spirit of wine; as also why water can only dissolve a certain proportion of these salts, so that whatever quantity greater than this is immersed in water, remains undissolved.

The solution of all other bodies is to be deduced from the same principles; but to understand them rightly, it is necessary to estimate the wideness of the pores of these bodies, the force by which the parts cohere together, and the efficacy or force of motion in the parts of the menstruum; which last arises from the difference of attraction of the particles of the menstruum to one another, and to the parts of the body, and from their elasticity. And upon these grounds he explains the various phænomena of solutions; particularly of that hitherto unaccountable one of aqua regia dissolving gold, but not silver, whereas aquafortis, of which the aqua regia is made, dissolves silver, but not gold; which he illustrates and reduces to a plain calculation.

In the lectures on digestion and extraction, he shows that there is a tenacity in all fluids, by which their parts do in some measure cohere together, and hinders their effects from being the same as in a perfect fluid. He gives the method of estimating this tenacity, and of finding out the proportion it may have to the weight of other bodies; and from thence he explains how small particles of matter, that are either specifically lighter or heavier than the fluid, may be sustained in it, which he explains by a calculation; and shows, that if the gravity of the body be to the tenacity of the fluid, as P to 1, if the body be divided into parts, whose diameters are to the diameter of the whole as 1 to P, then these bodies may be sustained in the fluid, though specifically lighter than themselves.

He observes, that tinctures made by digestion are usually very strong, and saturated with the body whose tincture is extracted: but if the tincture be distilled in an alembic, the menstruum generally rises with its former colour and clearness, leaving the body behind it: the reason of which he explains.

He considers the several preparations of opium, and condemns such as are made by the fumes of sulphur, or by acid liquors; in which either the virtue of the opium is lost, by the evaporation of its volatile particles, or destroyed by acid salts, whose qualities are directly opposite to that of opium; the one coagulating or making the blood viscid, whereas the other attenuates it, and renders it fluid: but he approves of such preparations of opium as are made with hot and aromatick medicines, which heighten its virtue; and seems to prefer Dr. Sydenham's preparation with canary wine to all others.

Precipitation, he says, may be made by infusing a liquor, specifically lighter or heavier than the menstruum: for by the first the equilibrium that was between the gravity of the particles swimming in the menstruum, and the tenacity of the fluids, is destroyed, on which account they must sink. By the second, the particles will be carried down to the bottom by the force of a heavier fluid. He shows likewise, how precipitation may be caused by infusion of saline menstruums, whose salts attracting the particles that swim in the fluid, and cohering with them, they will form such bodies, whose gravity will over-power the tenacity of the fluid, and descend. From which principle he deduces the reason of all chemical coagulations.*

In the crystallization of salts, he observes, that a great part of the fluid, in which they are dissolved, is evaporated: on which account, their particles coming nearer to one another, their attractive force is increased, and they will come and unite together; and because the figures of the minute particles of each salt are always uniformly the same, and their attractions being stronger on one side than another, they will always cohere to one another in such sides as have the greatest attractive force. On which account they must necessarily form bodies of certain determined figures, which in the same sort of particles are always the same.

II. An Account of Animal Secretion, the Quantity of Blood in the Human Body, and of Muscular Motion. By James Keill, M. D. N° 320, p. 324.

The author of these discourses, in the preface, shows the necessity of a right knowledge of the principles of true philosophy, and of the animal economy in

* Dr. Freind had no just conception of elective attraction, nor of the nature of metallic precipitates, &c. See his Life, vol. iv. p. 423 of this Abridgment.

the practice of physic; where our skill in curing diseases, whose histories are known, is always proportional to our knowledge of the animal economy, which of itself is a considerable part of natural philosophy. And he affirms, that every man who practises, does it on some knowledge of the animal economy, or some notions of his own, which are more or less clear, according to his skill in natural philosophy. He proves likewise from Hippocrates and Galen, that the principle of attraction of the small particles of matter to one another, was known to the ancients; the philosophy of Hippocrates being built on a certain propension which some things have to one another, by which they attract, retain, and alter each other.

In the first discourse, he proves by observation, that both the red and serous parts of the blood are endowed with an attractive power; and as in the blood the particles attract one another, and cohere together, so likewise do the particles of different fluids, that are separated from it by secretion. He says, it is evident that some of the fluids, that are secerned from the blood by the glands, are really composed by the cohesion of several sorts of particles; for in milk there are 3 or 4 several sorts of substances; urine has the same appearances, and contains perhaps more principles; and there is no doubt but that tears, spittle, and sweat, are all compounded liquors. Now if the particles which attract one another, be more powerfully attracted by the fluid in which they swim, than by one another, they can never of themselves separate from the fluid; which is the case of salts dissolved in a large proportion of water, and of urine when it neither breaks nor settles: but if the particles swimming in the fluid be more strongly attracted by one another than by the fluid, they must necessarily separate from it, and go into parts which will either sink, swim, or ascend in the fluid, according to their specific gravity. This power of attraction, he says, is universally diffused throughout all matter, and it seems to be the only principle from which there can be drawn a satisfactory solution of the phænomena produced by the minima naturæ. And because the whole animal economy depends upon it, he lays down, in eleven propositions, so many of the laws of that universal attraction, with their demonstrations, as are requisite for his present purpose; and then proceeds to show how the corpuscles, that compose the secretions, are formed in the blood, before they arrive at their secerning glands: but because the particles of the blood returning by the veins, and attracting one another, are formed into globules too large for any secretion, he shows how these globules are broken and divided in the lungs, by the force of respiration: and from experiments, and the doctrine of statics, he calculates the pressure of the air on the lungs to be equal to the weight of 100lb; and because the difference between the greatest and least gravity of the air is

$\frac{1}{10}$ of the greatest, he from thence shows how asthmatic people are very sensible of this difference, especially when they breathe thicker; for if they perform their expiration in half the usual time, it will make this difference equal to 40lb. weight, which is almost equal to half the pressure of the air in ordinary breathing.

He next shows, how, from the great velocity of the blood, the friction on the coats of the vessels, the impetus of the particles on one another, and their elasticity, there must needs arise near the heart a strong intestine motion in the blood, on which its heat depends; and by consequence near the heart, where the motion is greatest, the union of the particles will be in a great measure hindered; and therefore the particles that unite first, are such as have the strongest attractive force, and such as have the least, are the last in uniting. The particles endowed with the strongest attractive powers, are the most solid and spherical corpuscles; and their quantity of contact being the least, the secretion they compose must be the most fluid: such is the liquor in the pericardium.

On the same principle, he gives the reason of the situation of the kidneys so near the heart, that the salts in urine, being strongly attractive, and uniting closely with the watery fluid, may quickly be drawn off from the blood. The corpuscles which are slowest in uniting, must be such as have the weakest attractive force; which are such as have the least solidity, but their surfaces most extended; and therefore corpuscles, which have plain surfaces, are longer in uniting than the spherical ones; but when united, they cohere most strongly, and compose the most viscid fluids: such are the mucilages of the joints, which are separated at the greatest distance from the heart.

Though the secretion of the gall by the liver, and of the seed by the testicles, may seem to be considerable objections against this doctrine, yet there is really nothing that more illustrates and confirms it than the manner of forming these secretions. Had the blood been immediately conveyed by the cæliac artery to the liver, it is evident, on account of the nearness to the heart, and the intestine motion of the blood, that so viscid a secretion, as the gall is, could never have had time to be formed in the blood, and secerned in that place; and therefore here nature is forced to change her constant course of sending blood to all parts by the arteries, and forms a vein, by which the blood is derived to the liver from the branches of the mesenteric and cæliac arteries, after it has passed through all the intestines, stomach, spleen, caul, and pancreas. By this extraordinary contrivance, the blood is brought a great way about before it arrives at the liver; and its velocity being exceedingly diminished, the corpuscles have time to unite and form the gall. And here our author calculates the velocity of the blood which comes into the liver, and proves, that what comes by the mesenteric artery into the porta, moves 177 times slower in the branches of the porta than in the trunk of the mesenteric artery; and the blood which comes from the spleen to the liver, moves 200 times slower in the spleen, than in the beginning of the splenic artery; and from thence he deduces the long-sought for uses of the spleen and porta: so productive is one simple truth of many others.

There is another contrivance for diminishing the velocity of the blood in the testicles, which the author explains, and shows that the blood must be 150 times longer in passing to the testicles the way it does, than if it had gone according to the common course of nature. The author then proceeds to explain the ways of forming other secretions, as the cerumen of the ear, the lymph, and animal spirits. He shows likewise how, from the doctrine of attraction, the operation of medicines, which alter the quantity of secretions, may be explained; for medicines that increase the quantity of any secretion, operate by uniting to and augmenting the attractive force of the particles, which compose the humours to be secerned; which may be more effectually done by the particles of one sort of medicine than those of another; and therefore different humours will require different purgatives to carry them off through the glands of the intestines; which consideration will re-establish the doctrine of specific purges, which was confirmed to the ancients by experience and observation, but rejected by the moderns through a false philosophy.

He next proceeds to show, how necessary the doctrine of secretion founded on attraction is, for understanding the nature of diseases; and he gives an example in a diabetes. He likewise explains from it some of the symptoms of rheumatism, gout, and stone; as also the operations of medicines in the human body, especially the attenuaters and thickeners of the blood, but more particularly the power of mercury in the cure of a gonorrhæa or pox; which are all so easily explained by the attractive power of matter, that no one can doubt of the truth of a principle so simple, which yet, like a master key, opens the works of very different contrivances, and discloses a uniformity in all the operations of nature: so that every one may see and read the same thought and hand, in the contrivance and framing of every part of the universe.

Having given the method by which the several sorts of fluids are formed in the blood, before they are separated from it, he then explains the way by which these liquors are secerned by the glands; and he proves, that the orifices of all the glands must be circular, and that they can only differ in magnitude; and therefore all the particles that arrive at the orifice of any gland, and are of a less diameter than that of the orifice, will enter the gland: so that if there were no other contrivance in it, the fluid which contains the largest particles, will likewise consist of all the particles of the other secretions: but this inconvenience is obviated, by imagining several tubes to arise from the side of the canal or duct of the gland, whose orifices are of such dimensions, that they will admit only particles which are smaller than those that are to be secerned by the gland; and a great many of them arising from the sides of the canal, throughout its whole circumvolution, will carry back to the blood the particles which are of a less diameter than those have, which are to be secerned; so that there will at last remain in the gland only these particles, with such a proportion of the watery fluid, as is necesssary for the proper fluidity of the liquor to be secerned.

In the discourse on the quantity of blood, he proves that the common opinion, that there are only 15 or 20 lbs. of blood in the body, is founded on no good grounds; as it supposes, when an animal bleeds to death, that all the blood in the body runs out of the wound, which the author shows to be false; for the larger the vessel that is wounded, the sooner must the animal die; and if the aorta itself were cut asunder, there would be a less effusion of blood from it, than from a small artery: and from this he explains the true reason of fainting on any sudden or violent evacuation, as in bleeding in the arm, cupping in an ascites, &c.

By blood he understands, not only the fluids in the veins and arteries, but all the circulating liquors in the body, they being all parts of the blood, and separated from it by the force of the heart, and many of them by the same force returning again: and in order to estimate its quantity, he supposes that the whole body is nothing but tubes or vessels full of blood, or of liquors derived from it; and then according to the various proportions of the thickness of the coats of the vessels to their cavities, he calculates what the quantity of blood must be; and finds, that if the body weigh 160 lbs. it must at least contain 100 lb. weight of blood and such fluids.

He next considers and determines the velocity of the blood. And first the celerity by which it is thrown into the aorta, which he finds to be such as will make it move 52 feet in a minute; and because the sum of the section in the branch of an artery, is always greater than that of the trunk, the velocity of the blood must constantly decrease as the artery branches: And according to the various proportions which the branches bear to the trunk, he calculates the velocity at the extremities or evanescent arteries; and finds, that if the trunk always bore the proportion to the branches of 41,616 to 43,506, the blood would move at least 4 times slower in the extremities, than in the great artery: but if the proportion of the trunk to the branches, were always as

41616 to 52126, which is often observed, the greatest velocity of the blood will be to the least, as 10000 is to 1.

In his discourse on muscular motion the author proves, that the vesicles of each fibre in the action of a muscle, are inflated by the rarefaction of the blood and spirits within their cavities; and explains the cause of this inflation and rarefaction from the principles of attraction. And then he shows, by increasing the number of vesicles, and diminishing their size, the swelling of the muscle may be made so little, as to be imperceptible, and the expence of spirits very much lessened, and yet there will be the same degree of contraction in the muscle.

He then proceeds to determine the force of the elastic fluid, and its proportion to the weight that is to be raised, according to the various degrees of inflation. His demonstrations here are founded on the same principles with those of Mr. John Bernoulli, but are more easy, and suited to the capacity of those who are not versed in the deeper parts of geometry.

Tables of the Barometical Altitudes at Zurich, in Switzerland, in the Year 1708, *observed by Dr. Joh. Ja. Scheuchzer, F.R.S. and at Upminster, in England, observed at the same Time by Mr. W. Derham, F.R.S. as also the Rain at Pisa, in Italy, in* 1707 *and* 1708, *observed there by Dr. Michael Angelo Tilli, F.R.S. and at Zurich in* 1708, *and at Upminster in all that Time: With Remarks on the same Tables, as also on the Winds, Heat, and Cold, and divers other Matters occurring in those three different Parts of Europe. By Mr. W. Derham, Rector of Upminster.* N° 321, p. 334.

From the comparison of the thermometers, Mr. Derham finds that the warm and cold seasons at Zurich and at Upminster, fall nearly together, the changes taking place from one to five days of each other; so that, in general, what is a warm or cold month or season at the one place, is the same also at the other.—As to the winds at those two places, there is but little agreement among them, being oftener different than alike: but when they do agree, it is chiefly when the winds are strong, and of long continuance.—As to the barometers, the mercury was always lower at Zurich than at Upminster, by a difference sometimes amounting to as much as two inches; but the medium difference is about half an inch; from which Mr. D. infers, that Zurich is situated about a quarter of a mile higher above the surface of the sea, than Upminster is.

Farther, it may be observed, that, as near the equinoctial the barometer is observed to be nearly stationary, but the more the latitude, the greater the range of the mercury, so at Zurich the difference was not so great between the highest and lowest stations of the mercury, as it was either at Paris or Upminster. For at Zurich the difference was only one inch Paris measure; but at Paris it was one inch $2\frac{1}{4}$ lines; and at Upminster it was $1\frac{8}{10}$, which is greater than either of the other two. On the whole, however, there is a considerable agreement between the two barometers, the one being very often high or low, when the other is so; and this one often rising or falling when the other does: also the one rising much or little, or falling much or little, when the other does: but though the matter is often thus, yet it is not so certainly so, as it is nearer home. In our Philos. Trans. N° 286, I have given a table of some heights of the mercury observed at Upminster, and at 200 miles distance in Lancashire at the same time. And in the Hist. de l'Acad. Roy. des Scien. Anno 1699, M. Meraldi, by comparing his observations at the Paris observatory with mine at Upminster, remarks, " That there is a great agreement between the variation of the heights of the barometers in both places; that he finds almost always that when the one rises or falls, the other does so too, though not always alike: that the days in each month when the mercury has been highest or lowest, it has been the same at Paris as at Upminster, but commonly somewhat more than three or four lines lower at Paris than at Upminster." But the agreement between the variations of Dr. Scheuchzer's barometers and mine, although I say often great, yet is not so constantly, nor so certainly great, as nearer home, viz. at London, Lancashire, Paris, and other places, with which I have made the comparison.

With regard to the rain, the first thing that presents itself to our view, is, that the rains for the most part are more frequent at Upminster, than either at Zurich or Pisa, in Italy; that is, we have more rainy days than they. But yet the rains in both these places are much greater in quantity, in the whole year, and in some months, especially the autumnal and winter months, than our rains are at Upminster. As to Zurich, I observe, that although the rains there are less frequent than in Essex, yet they seem more so than at Pisa: but the quantity at Zurich is greater than at Upminster, and less than at Pisa. It is Dr. Scheuchzer's opinion, " That more rain falls in Switzerland than in France, at Zurich than at Paris." To confirm which, he gives a table of the last 8 years rain at Paris, from which, and from mine at Upminster, it appears that in the 8 years, there fell at Paris $152\frac{1}{3}$ inches English measure, but at Upminster $154\frac{2}{9}$ inches in the same years; which give for the medium per year, 19 inches and $19\frac{2}{7}$ inches, respectively. Where it may be remarked, that there is a greater difference between these last 8 years rain at Paris and Upminster, from what I found in the 8 years, in which I formerly compared the rain of Towneley, Paris, Lisle, and Upminster together, in the Philos. Trans.

N° 297. For by that comparison it appeared, that less rain fell at Upminster, than at either of the other three places. But according to these later 8 years in the table, a small matter more falls at Upminster than at Paris. For the mean proportion for Paris, according to former years, was above 22 inches English. The proportions therefore which I shall now lay down for the yearly rain of all places, whose rain I have had information of, are these; for Zurich (till further observations are made) $32\frac{1}{4}$ inches; for Pisa (till further observations also) $43\frac{1}{4}$ inches; for Paris, 19 inches; for Lisle, 24 inches; for Towneley in Lancashire, $42\frac{1}{4}$ inches; for Upminster $19\frac{1}{4}$ inches.

An Experiment, showing that actual Sound cannot be transmitted through a Vacuum. By Mr. Fr. Hauksbee, F.R.S. N° 321, p. 367.

That the experiments already made, endeavouring to produce sound from a bell in vacuo, have been altogether ineffectual, is well known. Yet that the loss of it should be wholly attributed to the absence of the air, I think could not without another experiment be absolutely concluded, since the following query might fairly be started on this occasion; viz. whether the sonorous body in such a medium might not so suffer, or undergo such a change in its parts, as to be rendered incapable of being put into such a motion as is requisite for the action or production of sound. Now to bring this to the test, I contrived the following experiment.

I took a strong receiver, armed with a brass-hoop at bottom, in which I included a bell as large as it could well contain. This receiver being screwed strongly down to a brass-plate, with a wet leather between, was full of common air, which could in no way escape. Thus secured, it was set on the pump, where it was covered with another large receiver. In this manner, the air contained between the outer and inner receivers was exhausted. Now here I was sure, when the clapper should be made to strike the bell, there would be actual sound produced in the inner receiver, the air in which was of the same density with common air; and could suffer no alteration by the vacuum on its outside, being so strongly secured on all sides. Thus all being ready for trial, the clapper was made to strike the bell; but there was no transmission of it through the vacuum, though it was certain there was actual sound produced in the receiver. This plainly shows, and seems positively to confirm, that air is the only medium for the propagation of sound.

An Experiment, showing the Propagation of Sound, passing from the Sonorous Body into the common Air, in one Direction only. By Mr. Fr. Hauksbee, F.R.S. N° 321, p. 369.

Since, by the former experiment, actual sound could not be transmitted through a space void of air; I was inclined to try whether a sound, propagated in a receiver, having a communication with the open air at one small aperture only, but otherwise entirely surrounded by a vacuum, would be increased, or continued sounding longer, at each stroke given the bell, than it would do, were not its body encompassed by such a medium. I therefore included the bell as in the foregoing experiment; only, to the upper part of its receiver, was screwed a box with collars of leather; and on the top of the outward receiver, was laid a brass-plate with a wet leather between; in the middle of which plate, was likewise screwed another brass box with collars, as before. These receivers, when placed on the pump, had their boxes standing directly one over the other. Through both of them in that position, I passed a hollow brass tube, which exactly fitted their perforations: thus the inner receiver had a communication with the outward air, and the outer receiver was thereby secured from the ingress of the circumambient medium. Now when the air contained between the receivers was pretty well exhausted, and the bell struck, the sound was sensibly very vigorous, and very nearly as great as before any air was exhausted; yet if a finger was applied to the aperture of the hollow brass tube, the sound would be so much diminished, as but just to be distinguished. By this we see, that since the sound in that state cannot be transmitted through the receiver that includes it, by means of the surrounding vacuum, yet the receiver is certainly struck with it; but finding no conveyance that way, reverberates and makes its passage where it finds least resistance. Nor did I observe, that though the sound had but one passage from its receiver, and that but a small one, that it continued any longer after the stroke, than if it had been made in the open air.

An Experiment on the Propagation of Sound through Water. By Mr. Fr. Hauksbee, F.R.S. N° 321, p. 371.

As a former experiment showed that sound could not be transmitted through a vacuum, I was induced to try what would be the effect, by surrounding the receiver, that contained the sounding body, with so dense a medium as water. Accordingly, as in the former experiment, the receiver which contained the bell was screwed down to a brass-plate, with a leather between; this receiver with its bell, was suspended in a large glass vessel, by four threads to the top, and as many to the bottom, by which it remained in the middle between both.

Thus provided, the clapper was made to strike the bell, whose sound was somewhat less by the interposition of the glass than it would be when made in the open air; however it was very audible, and might be heard at a considerable distance; it appeared to the ear to be very harsh, in respect to the tone it yielded. But now, when the water came to be poured in, and the inner receiver surrounded by it, at least an inch and a half from the nearest part of the outer glass, the clapper again was made to give the sound; which it did, seemingly very little less in respect of its audibility, but much more mellow, sweet, and grave, at least two or three notes deeper than it was before.

An Explanation of the Figure of a Pagan Temple, at Cannara in Salset. By Mr. Alexander Stuart. N° 321, p. 372.

Fig. 3, pl. 12, represents the rocky mountain Cannara, in the island of Salset, belonging to the Portuguese, with the temple and caverns. A is the temple and sacrificing place, like the choir of a church, arched and supported by 45 pillars, all cut out of the rock; B the altar in the farther end, also cut out of the rock; with a narrow canal about the foot of it, I suppose designed to be filled with water; CC unknown characters engraven on the rock, on each side of the square entry; DDD &c. various irregular paths of ascent, some cut out in steps as stairs; 1, 2, 3, &c. various caverns, mostly of a square figure; some large, others smaller, cut out of the rock; whose roofs are cut plain, like a plastered room; at the door of each without, is a large cistern or two, cut out of the rock, full of rain water, at least I could not perceive them to be springs. On the sides of the doors of some of the caverns, are characters like those of the temples.

Experiments on Metals, made with the Burning-glass of the Duke of Orleans. By Mons. Geoffroy, F.R.S. N° 322, p. 374.

This burning glass is 3 feet in diameter, and it collects the rays of the sun at 10 feet distance, where it forms a focus of about 3 inches over, which is again contracted, by means of another glass lens, to an inch diameter, and consequently is rendered three times as strong.*

There was a great difficulty to find any matter capable of holding metals in fusion in the focus. Charcoal, which is commonly used, is indeed a very proper substance; but it is impossible with it to vitrify any one of the metals: the particles of the metal, when held any long time in fusion in the focus of the glass,

* It should be said 9 times, because circles are as the square of their diameters, and the square of 3 is 9.

dissipate and fly away in fume or small particles. The reason of which I take to be this: charcoal is a substance deeply impregnated with oily or sulphureous particles. The first effect that fire has upon metals is to separate the sulphureous parts; now, if in proportion as the sulphur is separated from the metal, the body that supports the metal furnishes it anew with other sulphureous parts, the other principles will never separate, and the metal will always remain metal; and nothing but the greatest degree of fire can raise and separate the sulphur, and that but by little and little, and in very small particles.

I then had recourse to another matter, which could by no means be suspected of containing any oily parts. Mr. Tschirnhaus, to whom we are indebted for making these large glasses, and the first experiments that have been made with them, says, he has vitrified metals by holding them in China ware. It is true, this succeeds pretty well, provided the pieces be very thick, and the glazing taken off. Of all the different kinds of matter that I tried, what seem best were the common cupels, and plates of grey fire-stone. The cupels hold the metal a long time in fusion in the focus of the glass without melting; excepting lead, which easily runs through them as soon as it vitrifies, and helps to dissolve them. The plates of fire-stone bear the heat of the focus much longer than any other matter, but great care is to be taken in heating them, without breaking, till they become red-hot; for when they are hot, the least cold air makes them melt.

The experiments on iron were the following: I placed in the focus of the burning-glass a piece of forged iron, of about a dram weight; it became red-hot, and its surface was covered with a black matter, like pitch or tar. On withdrawing the iron out of the focus in this state, this matter fixes itself on the surface of the metal, and there forms a small skin or very fine blackish scale, which is easily separated by striking upon it; and that part of the iron that was covered with this scale appears blacker than ordinary. This scale is some of the sulphureous part of the iron, which rises to the surface of the metal when ready to melt, and there remains for some time, before it exhales. It is plainly this sulphureous part that rises upon iron, and polished steel, when heated, and gives them all those different colours, from a yellow to a violet, a water colour, or a black. By continuing to hold this piece of iron on the charcoal, it entirely melts, and at the same time emits very bright sparks in great quantities, sometimes to more than a foot distance from the coal. By saving what flies off during this sparkling, by holding a sheet of paper under the coal, we find that they are so many very small globules of iron, and the greatest part of them hollow.

All the iron, that is held in fusion on the coal, flies away in sparkles after this manner, till none remains. Sometimes the metal leaves off sparkling, when the coal is in part consumed, and covered with a bed of cinders, on which the melted iron lies. For as the sparkling of the iron seems to proceed only from the oily parts of the coal acting on those of the metal, the cinders hinder this oil from passing from the coal to the iron, so that it remains quietly in fusion. But if through any shake, or the like accident, the cinders be so removed that the iron comes to touch immediately the coal, it begins to sparkle afresh.

I exposed to the focus, on a stone slate, iron and steel: they grew red-hot, and melted without crackling or casting off any sparks; they smoked very considerably, and the melted metal changed gradually like an oil. After withdrawing this melted matter out of the focus, it fixed in a regulus-like friable mass, and appeared sometimes lightly striated, or shot into sharp points like needles. Though this matter does not appear at all transparent, yet it may be considered as the beginning of vitrification, or a middle state between metal and glass; for it would vitrify in the end like other metals, if it could be held a sufficient time in the focus without melting, or mixing with what sustains it; but continuing it long in the focus, the extreme heat of the sun necessary to keep it in perfect fusion, melts also the stone or cupel that contains it, the result of which mixture is a brown or greyish sort of enamel.

We may then consider this regulus mass as a half vitrified iron, as it is deprived of great part of its sulphur. If one adds to this mass a sulphur like that which was taken from it, from being friable it becomes very hard and malleable, and its former dulness changes to the brightness of a metal. This is what I have experienced in exposing again this matter to the focus upon charcoal; it melts, and so continues a considerable time in fusion without sparkling; but at last it sparkles with the same briskness as iron itself, and when withdrawn from the focus, appears nothing different from melted iron.

From these experiments it appears, that iron contains a sulphur or oily substance, that renders it bright, malleable, and easy to melt. That this sulphur is raised by the fire of the sun, when the metal is for some time held in fusion in the focus of the glass. That this sulphur may be raised by the flame of common fire, which though not strong enough to melt the iron, yet is able to reduce it to an eschar or sort of rust. That iron deprived of this sulphureous part, melts into a regulus, or brittle and friable mass, in colour much like antimony. That if one can hold a sufficient quantity of this matter long enough in the focus by itself, without melting or mixing with the body that contains it, it perfectly vitrifies. That this glass or metallic regulus, with the help of a little oil, returns to its former state of a metal. That it reassumes this metallic form upon charcoal by drawing thence this oily substance. That, in short, this oily part, contained in the coal, is little different from the sulphur of iron. Yet it must differ in some particulars, as melted iron, that has been saturated with it, crackles and sparkles very much, when melted again on the stone or cupel. Iron being the only metal in which I have observed this sparkling, I take it to be a property peculiar only to iron, and not to any other metal. Perhaps we may attribute it to the vitriolic salt with which this metal so plentifully abounds, which greedily absorbs sulphur. And to this quality in the vitriolic salt of iron we may also attribute the facility with which iron consumes the coals; for there is no other metal that so soon wastes the coal in the focus of the glass as iron does. It is also observable that iron is the only one of the four imperfect metals on which vitrified drops arise, while in fusion on the coal; the reason of which I have not yet been able to discover.

Copper, exposed to the focus of the burning-glass, at first turns white on its surface, but afterwards black, and is covered with a kind of skin, or with black, furrowed, and uneven scales, till at last it quite melts. On withdrawing this metal out of the focus, as soon as this white colour appears, and after it has been cold, I found nothing extraordinary on its surface; but by degrees it recovers again very nearly its former colour. I have not been able to discover from whence this white colour proceeds, unless we may attribute it to some volatile arsenical salt contained in the copper, which is driven by extremity of heat to the surface of the metal; or purely to the alteration made in the grosser parts of the surface of the metal when it begins to melt. The black colour which it afterwards assumes, seems to arise from the sulphureous matter, which melts first in this metal as well as in iron, and is raised to its surface by the extreme heat.

Placing a piece of copper in the focus upon charcoal, it melted, and emitted a very thin fume, which gradually diminished, till it was all evaporated. I put a piece of red copper on a cupel into the focus of the glass; it melted, and emitted some thin fumes; and after being some time in fusion, it became liquid like an oil. I withdrew this melted matter, and as it cooled, it fixed into a regulus of a reddish brown colour, which was hard and brittle. When broken, it becomes a red powder, like cinnabar of antimony; and when viewed with a microscope, there appear many small, red, transparent grains, like small rubies, insomuch that one would readily take this regulus to be a deep-coloured red glass.

I endeavoured to make this vitrified copper spread abroad in melting, by mixing it with common white glass; for which end I powdered some of this vitrified copper and common glass, and mixing them, I melted them together;

but the mixture when in fusion took at first a beautiful green colour, and continuing it longer in the focus, it turned bluish. I believe we may ascribe this change of colour to the alcali salts of the glass, acting on the particles of copper; for these salts usually draw a green or bluish tincture from this metal. To preserve therefore this red colour of the vitrified copper, when mixed with common glass, I made use of this expedient: I melted in the focus, on a cupel, a piece of copper, and as soon as it began to vitrify I cast upon it some common glass; as soon as the glass was melted, I took them together out of the focus without confounding them; and when cold, I separated the regulus from the glass as well as possible, picking out of it some pieces of the glass, loaded with some very small red transparent particles of the regulus. This vitrified copper is therefore nothing but copper deprived, by means of heat, of the sulphureous particles which gave it the form of a metal: for on exposing this vitrified copper to the focus upon charcoal, it reassumes in a little time the colour and consistence of melted copper; and as it cools it fixes into a good, red, malleable copper, as fine and hard as it was before vitrification. From these experiments it follows, that the basis of copper is a red earth susceptible of vitrification: That this earth receives its metallic form from a sulphureous substance, in appearance nowise different from the oil of vegetables or animals: That we may deprive copper of this oil, by holding it long enough in the focus, or by calcining it in the flame of common fire: That charcoal restores again this oily part to copper, and at the same time its metallic form. It appears farther, that the oil of the coal has not so considerable an effect on copper, as on iron. Copper exposed a long time to the focus, on a stone or a cupel, fumes very much, and diminishes very considerably in weight. I do not think that this fume is only the sulphureous part of the metal, the evaporation of which must be insensible; but I believe that with this oil there is mixed a great deal of the earthy, vitrifiable part of the metal, which the heat of the sun sublimes and raises in flowers.

Tin, exposed on coal to the focus of the burning-glass, melts, and emits a gross, white, thick fume, till it is all evaporated.

By melting tin upon a cupel, in the focus of the glass, it fumes very much, and its surface is covered with a white rarefied calx; on which gradually arises a tuft, or heap of sharp, needle-like, transparent crystalline particles, consisting of a vast number of small points. By continuing to hold this mass in the focus on the stone, these crystals at length cease fuming, and remain fixed, while the stone melts and vitrifies.

I took calx of tin, (which is tin reduced to a grey powder by means of fire, which has taken away by calcination great part of its oily substance) and exposed it on a cupel to the focus, where it fumed again very much, and was reduced into sharp crystalline particles consisting of other small points. On re-exposing these crystalline particles to the focus upon charcoal, they melted very easily, and took again the form of tin; the coal having furnished them with the sulphureous part which the fire had before taken away: for it is well known, that by adding any fat, or the like inflammable matter, to the calx of tin when red hot in the crucible, it reassumes immediately the form of tin.

These experiments show, that tin contains a sulphur which is very easily separated, since common fire can do it so readily; and that this metal calcined, or deprived of its sulphur, is easily saturated again with it, from the oily part of any inflammable matter. It also appears that the metallic earth, which is the basis of tin, is a crystalline earth, very difficult to be melted; since common fire cannot vitrify this metal by itself, and that the heat of the sun, in the focus of this large burning glass, cannot perfectly melt the calx, into which this metal is reduced. We may presume that the crystallization, or reducing of this metal into sharp-pointed particles, proceeds from the force of the sun, breaking and melting together into a solder, as it were, some of these small crystals, as the sulphureous parts evaporate; it not being strong enough to melt them all down together in one entire mass.

Lead being held in fusion on the charcoal in the focus of the glass, it wasted all away in fumes. I exposed the like quantity of lead on a stone, to the focus, where it emitted great quantities of fumes, and gradually changed into a fluid liquor, like oil or melted rosin. This liquor, as it grew cold, fixed into glass; which has this peculiarity, that it is disposed into plates, like Venetian talc, and that it is flabby, soft to the touch, transparent, and in some parts of greenish or reddish yellow.

In continuing this matter in the focus, it spread upon the stone like varnish; and at last, penetrating it, helped to melt it. I placed this talcky earth in the focus upon charcoal; when it melted, and in a little time after reassumed the form of melted lead. I withdrew it from the focus, and having let it cool, found it nothing different from lead.

These experiments show, that there is in lead, as well as in the other imperfect metals, a sulphureous part, that is easily separated by common fire, or the heat of the sun; and that this metal has for its basis a foliated or talcky earth.

Quicksilver placed in the focus of the burning glass upon charcoal, or on the cupel, or on the stone, is immediately all dispersed, and exhaled in a very thick fume. I exposed on the stone, to the focus, some mercury precipitate per se, in a degree of heat equal to that of digestion: it seemed to melt, but presently dispersed in vapours: only there remained a small quantity of a very rarefied dust, like a froth or scum; but continuing it in the focus, it melted, and ran into a yellowish glass, in which might be distinguished some particles of metal like silver. I exposed some precipitate per se upon charcoal: it fumed very much; and as it melted, one might see little globules of mercury unite, and form themselves together upon the coal; but they dispersed again presently in vapours.

These experiments seem to prove, that there is in quicksilver, a sulphur which may be separated by a very gentle heat, such as that of digestion: That as soon as this sulphur is taken away, it loses its fluidity and brightness: That the basis of mercury is a calx, or red earth: That this calx does not melt into glass as the calx of other metals, because it is too volatile; and as soon as it melts, is evaporated by the heat: That on restoring a sulphur to this calx, by exposing it again upon charcoal to the focus, it immediately reassumes its metallic brightness and fluidity, and becomes quicksilver.

I know not yet whether this light earth, which remains on the stone after the evaporation of the calx of the mercury, be a part of the earth of the mercury, more exactly deprived of its oil, and consequently more fixed and proper for vitrification; or whether it may not be some matter foreign to the mercury, that fixes itself, and remains behind at its evaporation.

The result of all these experiments is, that these four metals, which we call imperfect, viz. iron, copper, tin, and lead, are composed of a sulphur or oily substance, and of a metallic earth capable of vitrification: That from this sulphur proceeds the opacity, brightness, and malleability of a metal: That this metallic sulphur does not appear at all different from the oil of vegetables or of animals: That it is the same in mercury, as in the four imperfect metals: That these four metals have for their basis, an earth susceptible of vitrification: That this earth is different in every one of these four metals; as it vitrifies differently in each of them. And that on this difference in vitrifying depends the difference of metals. [Modern chemists consider metals as simple substances, and their calces, as compounds.]

Observations on Incisions of the Cornea. By Mons. Gandolphe, M.D. at Durkirk. N° 322, p. 387.

Contusions on the bodies of animals do not always make the greatest impressions on the parts that immediately receive them: I had an instance of this lately, in a blow upon the eye. There was a slight contusion on the outside of the part, with very little alteration to appearance; but a vessel, being broken within, poured forth a considerable quantity of blood: the eye also lost its transparency, and almost its sight; which was so very weak, that it could scarcely perceive the greatest light when placed before it. The cornea appeared all over red, but without any inflammation or blood vessels; receiving its colour from the blood poured in upon the aqueous humour.

I saw the patient the 6th day after he had received the hurt: he had been let blood thrice; and the 8th day I caused the cornea to be opened near the middle; my design being to make a large orifice, I determined not to make it at the bottom of the cornea. The orifice being made, there issued forth some drops of the aqueous humour mixed with blood. The cornea still appeared as red as before, and was protuberant. This circumstance made me resolve immediately on a second orifice, as large as the former, but lower: there issued some drops of the humour; and the eye appeared not so red and convex as before. The humour continued issuing from the orifice for some time. We applied nothing to the eye, but a compress, or stupe, dipped in a mixture of 4 ounces of plantain-water, and 2 ounces of a vulnerary water. The day after the operation, the upper part of the cornea was transparent, the lower part not so red, and the whole membrane appeared to have recovered its natural convexity. It seems that all the extravasated blood would have quite run out, had the lower part of the cornea been opened, and remained so for some time. I observed the alterations of the eye for three days together; in which time the extravasated blood seemed some times to spread over the whole cavity of the cornea. We judged that the motion used by the patient himself, had opened anew some blood vessel, or had mixed the extravasated blood with the aqueous humour; for we did not perceive all that time that there was any fresh effusion of blood.

The 5th day after the first two incisions, I caused a third to be made at the bottom of the cornea: some drops of the humour issued out, and continued so to do for some time; and in two days after, the eye recovered its natural transparency. The pupil was now very much dilated; but gradually it contracted again, but not to its usual smallness. The iris kept its motion all this while; so that we cannot suspect that the lancet, in making the incision on the cornea, anywise touched on the iris, because the pupil continued exactly round: and a blow that is able to divide the continuity of the parts of the eye, and cause a suffusion of blood, is but too capable of depriving the iris of its natural power of contracting. The pupil, which before the blow was one line in diameter when the iris was contracted, is at least two lines in diameter at present. The transparency of the humours, and convexity of the cornea are the same as before. The sight is now restored; and there remains no other alteration than what necessarily follows from the like dilatation of the pupil.

From hence we may draw some remarks, that will be of use in practice, and show that incisions may be successfully made on the cornea. 1. Incisions are made on this part without any pain. 2. The orifices unite again without any scar. 3. We find that plants of a discussive quality have a bad effect, the patient finding himself much worse after using a cataplasm made of cervil and parsley: these plants, which are excellent in discussing extravasated blood in the muscular parts, have bad effect when applied to the eye, by causing pain, and rendering the sight more disturbed. When there is a considerable effusion of blood in the eye, in couching a cataract, and no orifice is made in the cornea to let it out, it may so alter the transparency of the vitreous humour, as to cause a loss of sight; which sometimes follows this operation.

An Experiment, showing that an Object may become Visible, in the Dark, through such an Opaque Body as Pitch, while under the Circumstances of Attrition and a Vacuum. By Mr. Fr. Hauksbee, F.R.S. N° 322, p. 391.

This experiment affords a signal confirmation of another formerly made, and differs only in the matter made use of. I before used sealing-wax, but now made choice of pitch, which I employed as the sealing-wax, viz. I melted it in a globe-glass, and kept it turning about until the larger half had got a pretty thick lining of it, so that no ray of light could penetrate it. This globe I exhausted of its contained air; then, being night, I put it on the engine to give it motion; where, after it had been turned a little while, with my hand on that half lined with the pitch, I could very easily discover through the transparent part, on the inner surface of the pitch, the very shape and lines of the hand and fingers; for their most eminent parts, that touched the glass, appeared all luminous: the other parts discovered themselves by the dark intervals they made between the enlightened parts: and when the fingers were spread or closed, it was very obvious to the sight. Now, after a small quantity of air was admitted, the light disappeared from the inside of the lined part (but not on the other,) which began to discover itself more and more on the outside; though even in vacuo there was always a light attended on the touch of those parts that were most contiguous to the glass: but now a circle of light would discover itself just on the edge of the pitch, which separated it from the transparent part, as also another ring of light somewhat nearer the axis of the glass, but both these appeared when the hand was applied to the under part; for when it was removed to the contrary, no such appearance ensued. The transparent half of the glass was in all circumstances as in former experiments. When all the air was admitted, the electricity of the glass in all its parts, the lined as well as the transparent, performed much alike, the threads seeming to be attracted every where with equal vigour. To conclude; this, and the former experiment of the sealing-wax, plainly discover a transparent quality in some bodies, called opaque, under particular circumstances. It was never so much as suspected that they could exchange that quality for the contrary one, and again resume their former state. What is said towards a reason of such an appearance in the experiment of the sealing-wax, Philos. Trans. N° 315, I think is very applicable to this. See also Physico-mechanical experiments on various Subjects, p. 131.

Concerning some Ancient Brass Instruments found in Yorkshire. By Mr. Ra. Thoresby, F.R.S. N° 322, p. 393.

Weapons of brass having been discontinued for many ages, it may not perhaps be unacceptable to give an account of some that were lately found in these parts. As the servants of Mr. Ellis of Kiddal were ploughing, in a place called Osmond-thick, near Bramham-Moor, they discovered 5 or 6 brass instruments, which are of different sizes, from about 3 to $4\frac{1}{4}$ inches in length, and from $1\frac{1}{2}$ to $2\frac{1}{4}$ inches in breadth: they are somewhat in the form of a wedge, as proceeding from a rather sharp edge to about 2 inches, where they are wrought hollow to put upon a shaft: each of them has an ear or loop. Some suppose them to have been arrow-heads, or axes of the ancient Britains; others, those of the Roman catapultæ; but I think they are as much too heavy for the first, as they are too light for the last. I should rather take them to have been the heads of spears, or walking-staves of the civilised Britains; and though of a somewhat different form from those described by Speed (Hist. of Great Brit. cap. 6.) their figures taken I presume from ancient manuscripts, yet by the loop in the ride we may better conceive how those ornamental labels were fastened, than by the pictures there represented.

That swords or daggers were used in old times, of the same metal, as well in Ireland as in Great Britain, (of which there are several described in the last edition of the Britannia) I conjecture from some found there of late years, one of which was a middle size, viz. 18 inches long in the blade; whereas, of those found in Wales, some were only 12, but others 24. The hilt or handle was probably of wood, (as is that of an old sword that I have now by me, which is 5 feet and a half long) it having been wholly consumed: they have been fastened together by four larger or two less nails, as appears by the holes in the brass, which are still entire. I have also an ancient spur, no less than 6 inches and a half long, from the heel to the middle of the rowel; but this, which is gilded, and of nicer workmanship, I suppose to be of a much later date.

Abstract of a Letter from Mr. Tho. Hearne, M.A. of Oxford, to Mr. Ralph Thoresby, F.R.S. occasioned by some Antiquities lately discovered near Bramham-Moor in Yorkshire.* N° 322, p. 395.

From the great variety of ancient monuments continually found in these islands, it is plain that vast improvements might be made to the accounts that have been hitherto given of the British antiquities.

From the vast number of coins dug up among us, divers places that were of note in the times of the Romans, but are now quite destroyed, have been discovered, and the antiquity of some other towns may, by these helps, be carried much higher than Mr. Cambden has done; and particularly Witney, within 7 miles of Oxford, appears to have been of note long before Edward the Confessor's time, as I gather from Roman coins lately found there. The best of which is one of impure silver, according to the custom of that time, in honour of Julia Mammæa, mother of Alexander Severus.

From your relation of the brass instruments, it is plain that they are exactly like that in the repository adjoining to the Bodleian library at Oxford. This has been kept there for several years; but where it was discovered, there is not the least memorial to inform us. The figures of the heads of the spears of the ancient Britons, both before and after they were civilised, represented by Speed, are quite different from those we are now considering, being exactly of the same make with those we find in the Columna Trajana, and the books that represent the military instruments of the old Romans and Greeks, &c. But had they been of some resemblance, yet I cannot see that those figures in Speed are of any authority: for though you guess that they were copied from old MSS. I could never yet meet with any MSS. of our British history, that have any such figures. Hence I am inclined to think that these figures are modern, and owing to Mr. Speed himself; and it is what he insinuates in the same chapter, acknowledging that they were adapted to the descriptions given of the Britons in ancient authentic authors. But not to examine other particulars, the shape of the spears in their hands is not countenanced by any authority of note: for though Herodian has acquainted us that they used short

* This learned antiquary was born at White-Waltham, Berkshire, in 1680. He was early patronized by Mr. Cherry, of Shottesbrooke, who gave him a liberal education. In 1695, he entered of Edmund-hall, Oxford, where he applied himself wholly to the study of antiquities. In 1703 he took the degree of M. A. and in 1714 he was appointed archetypographus of that university, where he died in 1735. Mr. Hearne published a great many ancient manuscripts, and editions of curious old books; as, the Life of Alfred by Sir John Spelman; Leland's Itinerary, in 9 vols. 8vo; a Collection of Curious Discourses, written by eminent antiquaries, &c.

spears, yet he is silent as to their make. Nor indeed have we any where a good account of the military arms of the Britons. The authors transmitted to posterity by them are modern in comparison of the Roman writers, and are also romantic, and not to be relied on. And as for the bards, they took no care to transmit to posterity these weapons, nor to give us exact relations of their countrymen. It is true, there have been, and still are found several instruments made of flint, which the best judges esteem to be British. The flint heads of their arrows are commonly called in Scotland elf-arrows, as being supposed to have an extraordinary virtue against the elves, and to drop from the clouds. There are other flints, somewhat resembling axes, and these Dr. Plot calls British axes; but Dr. Leigh, in his Nat. Hist. of Lancashire, thinks they are Indian. Sir William Dugdale inclines to Dr. Plot's opinions, and he acquaints us with several, of about 4 inches and a half in length, curiously wrought by grinding. But they might as well have been Roman; the Romans having used flint weapons, as well as the Britons, and it was from the Romans that the Britons learned the art of working them. What seems to convince us that they might be Roman, is, that those mentioned by Sir William, were found at Oldbury, Aldbury, or Ealdbury, which was a Roman fort, and is the same in signification with Alchester in Oxfordshire, Alchester being nothing else but Ealð-ceaꞅꞇeꞃ, so called by the Saxons, to show that it was a place of antiquity, even in their time. And though the anonymous author of the antiquities of Alchester, at the end of the parochial antiquities of Ambrosden, derives it from Allectus, as if he were the founder; yet there is no authority, either from coins, inscriptions, or books, to countenance the conjecture.

Now since there are no authentic authors, by which we may learn what arms were used by the Britons in their wars, I can think of no method for finding this out more proper, than by seeing what arms were in use among those people from whom they immediately had their original. Mr. Sheringham inclines to the story of Geoffry of Monmouth, who deduces the Britons from the Trojans; and this is also the opinion of several other learned men. But whatever their abilities and authority might be in other respects, yet in this they must be reckoned partial, and I rather strike in with those other writers of more authority, who derive the Britons from the Gauls; among whom Mr. Cambden is the chief. He has diligently and accurately proved, that the Gauls and Britons had the same religion; that they both had their bards and druids, enjoyed the same form of government, used the same method of fighting, had the same natural genius, were equally candid and innocent, were addicted to change when provoked, were compassionate to their relations, and always ready to engage in their vindication. He has also shown, that they both affected great numbers of servants, that their buildings were alike, and were surrounded with woods: that they both usually wore chains of gold about their necks, and had rings on their middle fingers; that both wore long hair, and that the garments called brachæ were common to both. These things he confirms from the best and most approved authors. And as the chief argument, he has alleged variety of instances to show that they spoke the same language. Mr. Sheringham was aware of this, and therefore, to evade the force of the argument, he makes the Trojans to come through Gaul, which being then thinly inhabited, he says Brute and his companions soon conquered it, built a city, and continued there till such time as they had well peopled it; after which, they passed over into Britain, and by that means the Britons came to have the same language. But this does not seem consistent with the usual prudence, nor with the other wise acts ascribed to Brute. For it is not probable that Brute would voluntarily leave so large a country as Gaul, for one so much less. It is therefore more likely that the Britons had their immediate original from the Gauls. Cæsar himself thought so, as to those that inhabited nearer the coasts, notwithstanding his observation that the midland people were Aborigines. Nor will Boxhorn's assertion, that the Gallic tongue was the same with the Scythian, overthrow this hypothesis. For it may very well be supposed, that the Gauls came first from the Scythians, who are in Justin observed to have been the most antient people, and to have contended with the Egyptians on that score. This will exactly agree with what Cambden and others have asserted, concerning the Gauls being descended from Gomer, the eldest son of Japhet. I know indeed that Mr. Sammes derives the Scythians from Magog, the second son of Japhet. But (not here to take notice of his contradicting himself in this point) since Strabo and Stephanus mention a city called Gogarena, between Colchis and Iberia; and since the city Hierapolis in Cœlo-Syria, according to Pliny, was called by the Syrians Magog; it is more probable that Magog seated himself in those countries, nearer to which it is agreed his brethren settled, than that he wandered so far out of the way from them. Here I cannot but notice that the Britons were, like the Scythians, a frugal people, and their long lives, often extended to 120 years, might in great measure be ascribed to their temperance, and their milk diet, just like the Hippomolgs, mentioned by Homer. And, as Æschylus tells us, that the Scythians were ἱππικῆς βοτῆρες εὔνομοι, a just nation, and addicted to the feeding of horses, so the same may be said of our ancient Britons, who were very religious, and observed the rules of their priests, and took extraordinary delight in cattle; whence perhaps they might affect to have the figures of beasts cut upon their bodies.

The next inquiry is, what arms were used by the Gauls? Several authors have written of their nature, particularly Cluver and Boxhorn. Their names are spatha, gessum, (gosum or gæsum) lancea, sparum, cateia, mataris or rather materis. The gessum was a javelin; the sparum, cateia, and mataris, different sorts of darts; the thyreos an oblong, and the cetrum a short kind of shield. So that the spatha only remains (for the nature of the lance is well known) to be compared with the weapons we are considering. It is called by the Italians spada, and by the Spaniards espada. From the description that Isidore has left us of it, we are informed that it was a two-edged sword, adapted to cut, but not thrust. Whence it is plain that these arms had not sharp points, agreeable to what Livy has related, that their gladii were prælongi, ac sine mucronibus. And Polybius has the same reason why they did not push with them. Hence it is clear that our instruments which have not two edges, but are dull like wedges, were not spathæ; and since they do not agree to any of the other Gallic weapons, we may carry on our inquiry, and examine whether they agree with any of the arms of some other antient nation, that made a figure in Britain.

Our ancestors, the Saxons, can have no share in this inquiry. For it is plain from their history, given by Verstegan, and also the figures published by him, that spears, halberds, shields, cross-bows, swords, (which were broad and bowing, somewhat like a scythe) and hatchets, which they called bills, were the arms made use of by them; nor did the weapons of the Danes, who succeeded them, vary much, if at all. Coming from the same parts, they used the same customs in their military undertakings. But though these weapons do not resemble either the Saxon or Danish military arms, in Wormius's museum, there are two Cimbric instruments, with which they have some likeness. These he tells us were of brass, and he calls them wedges: the larger of them was 5 inches in length, and 3 in breadth. He is of opinion that they were used in the wars, especially when the armies were very near each other. If they had holes, by which they might have been fixed to helves, he would have believed them to be battle-axes: but being neither hollow, as ours are, nor having any other way of being fastened to other instruments, he concluded that the name of wedges might be most proper. A very ingenious gentleman has informed me, that such kind of instruments had been found in the Isle of Man, and that a great many urns had been also discovered there, as also divers inscriptions with strange characters. I do not question but the inscriptions are Runic. And it is highly probable that the instruments were like those in Wormius; but if they agree exactly with ours, they will appear to be Roman. For though it be commonly held, that the Romans never were in the Isle of Man, yet I see no other reason why it should be thought so, than that the ancient authors now remaining do not mention it. This is only a negative argument, and what we ought not to lay a very great stress on. The urns seem clearly to evince that they were here. I know indeed that it is said that these urns must be perfectly Danish, because of the small black bones and ashes found in them; which however is no sure ground to go upon. For I have seen in the Bodleian repository, a piece of a Roman urn, which was dug up several years ago at an old Roman town in England, with many others, some of which were of different figures. And with it were small black bones, ashes, &c. wrapped up in two pieces of coarse linen, of the same figure with the urn. The smallness of the bones shows that they are the relics of children. It was customary among the Romans, after the bodies were burnt, to wash the bones with wine and milk, and afterwards the women wrapt their children in linen, dried them in their bosoms, and put them into urns to be buried. This custom was also peculiar to the Danes, who learned it from the Romans, from whom also they received urn-burial itself. Such urns are also mentioned by Sir Thomas Brown to have been found at Old Walsingham in Yorkshire. Nor is the Roman history altogether silent as to the Isle of Man being known to the Romans; for Plutarch expressly tells us, that one Demetrius sailed thither, as well as to other British isles, in the reign of Adrian. It is not surprising that Runic incriptions are discovered in the places where Roman urns are found: those inscriptions might have been made on other occasions, after it became inhabited by Danes and Norwegians. The same thing has sometimes happened in England: and Cambden particularly relates, in the close of his discourse concerning Stone-Henge, that in the time of King Henry the 8th, there was found at Stone-Henge a table of mixed metal, on which were engraved many letters, but the character was so strange, that neither Sir Thomas Eliot, nor Mr. Lilly, the famous schoolmaster of St. Paul's, could tell what to make of them; so no care was taken to preserve the monument, the loss of which was afterwards much lamented by Olaus Wormius, who took it to be Runic, as it doubtless was: and yet Stone-Henge itself is a Roman work, as has been made out by Mr. Inigo Jones, who, though he was answered by the learned Dr. Charleton, yet Mr. Jones's opinion was very well defended by Mr. John Webb, who has in his book distinctly examined the methods used both by the Romans and Danes in their buildings.

Having shown that these instruments were not military arms, either of the Britons, Saxons, or Danes, I shall now endeavour to prove that they were Roman. I once took them for a kind of axes, used by the Romans in their sacrifices, of which Dr. Plot notices two sorts, the secures lapideæ, and the secures cupreæ, though Dr. Leigh will have both his instances to be Indian. On a closer consideration of the Roman sacrificing instruments, I have quite changed this opinion, not finding the least vestiges of such axes in any books on the Roman antiquities. On the contrary, they are, in the suovetaurilia or solitaurilia of the Columna Trajana, represented in the same form, and fastened in the same manner, that we use at this day; and so also in other sacrifices, as may partly be seen in the gems, rings, &c. published out of the studies of Augustinus, and Gorlæus, as well as in the monuments of Gruter, Reinesius, Spon, and Fabretti, not to mention the authors collected on this subject by Grævius, in his large body of Roman antiquities. Neither could they have been the heads of spears, as is manifest from the same authorities. The Roman spears and javelins occur very frequently; and yet not one of them, either on their coins or elsewhere, is to be met with in the shape of these instruments. It is true some of their spears had two heads, that they might use either end uppermost, as they pleased. We have one of these in Augustinus. The heads differ from each other, but neither of them answer to our monuments. Nor are the most antient spears of the Romans we meet with, different from those they used in more modern times, as may in some measure be seen in the famous shield lately published at Oxford, which is certainly authentic. It may here be further added to what is there alleged, that Lucius Florus gives us the first instance of the Romans fighting on horseback without bridles; and in the Columna Trajana the horses are placed in full speed, with their riders, without any bridles, or other curbs, to restrain or guide them; a great many of the Romans having made themselves masters of this method of fighting, that they might, like the Numidians, who were famous for it, be the less encumbered in the battle, and rush upon the enemy with the more force: their desultores also are sufficient proof of its being practicable.

But now, though these instruments are not properly Roman military weapons, such as they used in their battles, yet they were of service among the soldiers, and numbers of them were constantly provided, to be carried about in the army; for I believe they are Roman chissels, and that they were used to cut the stones, and other materials, that were judged serviceable for building their camps. This is not a mere conjecture only, as appears from the Columna Trajana, where the soldiers are represented polishing the stones, for the Roman tents in the Dacic wars, with such sort of chissels made of brass, which they beat and worked into the stone, and other materials, with mallets of the same metal. There are other instances of it in the same pillar, which is one of the best monuments we have, by which to judge of the several instruments used in their military enterprises. These chissels were of admirable service in making their aggeres, which consisted of earth, stones, and timber. The stones were sometimes thrown together without any polishing; but that was more rarely, and it was considered a better security to have them worked, that they might lie even. Hence appears the reason why these instruments are hollow, viz. to fasten handles to them, for more convenience in driving them. Had they been wedges, it would have been a great inconvenience to have had them hollow. As for the ears or loops, it is probable they might be put on, that the handles might be fixed the better; or perhaps they were designed for the ease of the soldiers, who in their journies might thus fasten them to their girdles.

Besides the uses these instruments were put to in forming the Roman camps, they were also employed in making and repairing the highways, which swallowed up a large quantity of stone, especially in such places as were marshy and fenny. The Pomptin marshes were vastly large; and when the soldiers were too many to be used against the enemy, it was proposed they should be employed in draining them, which was done accordingly; and the soil was so rich and fertile, that great numbers came and settled here, so that no less than 33 towns were built on the ground. The waters however afterwards got strength again, and it was in a manner wholly drowned; which made Julius Cæsar entertain some thoughts of draining them afresh, and of carrying the Appian way through them, which had before gone round them; but he failed in his design, and it was left for one of his successors, the emperor Trajan, who after he had cleansed the fens, caused a stone-way to be made through them, on which were built large inns, and magnificent bridges for conveying the water in the upper part of the marsh. In memory of which, he had a monumental stone erected, with a proper inscription, by which it appears that the way was 19 miles in length, a mile-stone being placed at the end of every mile; from whence the way itself was in succeeding times called Decennovium.

Besides which, he also made divers other ways here; and perhaps these chissels, that have occasioned this letter, may be some of those used by the soldiers in his reign; though before his time, acts of this kind had been performed by the Roman soldiers, who also forced the Britons to undergo the same drudgery, which occasioned them to complain to Agricola, that they were too severely and hardly dealt with.

If it be asked how it comes to pass that these instruments are of brass, rather than of any other metal? it may be replied, that they, as well as the people of other nations, in former times thought there was an extraordinary virtue in brass. And it was on account of this peculiar virtue supposed to be in brass, that the instruments used in the sacred offices, were in the more early times all of brass; that the Tuscans used brass ploughshares, when their cities were built, and that the priests of the Sabines were shaved with brass razors. Hesiod himself tells us, that the antients used brass instruments before iron ones:

Χαλκῷ δ' ἐργάζοντο· μέλας δ' ουκ ἔσκε σίδηρος.

At which time, not only their arms, but their houses were also of brass:

Τοῖς δ' ἦν χάλκεα μὲν τεύχεα, χάλκεοι δετε οἶκοι.

The brass in those early times, was of a different nature from ours, and so tempered, as to endure much longer, with less inconvenience in the several operations in which it was employed.

As for the other piece of antiquity which is in your collection, namely, a spur, that is 6¼ inches long from the heel to the middle of the rowel; we have one in the Bodleian repository, of much the same length, of which I have made mention in my additions to Sir John Spelman's Life of King Ælfred. There have been several others found in England, and you have justly guessed your's to be more modern than the other instruments; for these spurs are certainly Danish, as appears from Wormius's Monumenta Danica, who has given the figure of one; and there is an account of several others towards the latter end of his museum, one of which is a foot and some odd inches in length.

Concerning a Bunch of Hair voided by Urine. By Mr. James Yonge, of Plymouth, F.R.S. N° 323, p. 414.

A plethoric woman about 50 years of age, who was often afflicted with nephritic pains, employed me to relieve her. I found, by the purulency and stench of her urine, that she had not only stones and gravel, but an ulcer in one or both her kidneys; and therefore gave her a dose of cantharides with camphire, made into pills, and followed it with plentiful draughts of a slippery emulsion. This made her void by urine abundance of blackish gravel, and white thick matter, like bird-lime, without any pain or bad symptoms, and she continued easy for a week. Her pains then returned, and again went off by the same remedy. About 18 days after, her pain seeming to threaten a return, I repeated the medicine; but that night it gave her very great pain in the side of her belly, and at last threw her into convulsions, which went off on the discharge of urine, containing a great deal of matter, and in it a bunch of short hair, almost rotten. For some time after, she used a nephritic course, which has hitherto preserved her from the return of pain, matter, stones, and obstruction of urine.*

* The author sent with the above letter a third part of that bunch of hair, which the last dose of cantharides brought away.

Observations on the Hair mentioned in the foregoing Letter, &c. By Mr. Leuwenhoeck, F. R. S. N° 323, p. 416.

Viewing part of the aforesaid hairy substance through a microscope, I judge it to be the hair or white wool of a sheep; which wool was broken into such small or short particles, that some of them were no longer than 6 hairs breadths. I suppose it had been found in the heel of a stocking; and the oftener I repeated my observations, the more I was confirmed in my opinion; for I could not only discover the short broken woolly particles, but I saw also a great number of the ends ground to pieces as it were; so that not only the outside of the woolly particles were rubbed off, but the inner little hairs, of which the wool is composed, were so divided from each other, that they appeared with their ends like little brushes. Also under these white woolly parts, there lay very small particles, composed of exceedingly slender little tubes or pipes, which seemed to be small bits of straw; there were other small particles of the same figure, seemingly the outer husk or skin of a grain of wheat or rye; and under those I saw one particle covered all over with small hairs, such as we see at the top of wheat or rye; as also some few little bits of wool, rather thicker than a hair: there was also a small particle of the outer skin of a man, for I could see the little scales of which our outer skin is composed very plainly. Now those particles that were not wool, might be very easily brought into the stocking, in case the bare foot is set upon the floor before putting it on.

There lay in the said matter a vast number of exceedingly slender long particles, which I imagine to be those hairy particles, of which a small fibre of wool is composed; as also several earthy particles, which I took to be part of the dirt of the floor, or of the foot itself. There lay also a great many particular small figures, but I could not discover what they were; and these were so strongly joined to some little hairs or wool, by the perspired viscous matter from the foot, as I suppose, that I could not separate them without the help of water: among others, I also saw two slender particles lying, which I should likewise have taken for the outer skin of a man, were it not that they were larger than any of the scales that I could ever take from my skin, which are mostly of an equal thickness. In short, there appeared so many and such particular figures, that there was no account to be given of them; only I observed among them one small particle, not of a single feather, such as it appears to our naked eye on the body of a bird, but rather of the finest down; and the more I unravelled or separated the particles of wool from one another, still the greater reason had I to judge, that the person who had worn the stocking, had been used to go often bare-footed on the floor. Now supposing that these woolly particles might have fallen into any spoon-meat thicker than ordinary, the person might swallow it down without being aware of it.

My reasons for guessing that these woolly particles should come out of a stocking, and that it should be occasioned by the motion of the foot, are these: I myself always wear heavy white woollen under-stockings, and I sleep in the same; so that I can wear them three weeks together, as my feet are not apt to be moist; now having several times viewed the broken woollen particles, which lie in a heap, as it were, cleaving together under the heel, and having also singled out of them several fibres or threads of wool, to prove that they are composed of little hairs, and these woolly particles exactly agreeing with those that were sent to me, I could no longer doubt that the said woolly particles were anywise different from those particles that were found in the heel of the stocking.

Concerning several Solid Bodies voided by Urine. By Mr. James Yonge of Plymouth, F. R. S. N° 323, p. 420.

Some doubts having arisen that Mr. Yonge had been imposed on, by the woman whose case was mentioned in the foregoing two papers, he has collected together a number of uncommon cases, as follows:

The authors and accounts of such cases are numerous: Diemerbroeck, Anat. lib. 1, c. 17, mentions several of his own knowledge, and many more from Plutarch, Langius, Alex. Benedictus, J. M. Hessius, J. Alexandrinus, N. Florentinus, P. Figræus, and others; that needles, lumps of fat, iron keys, roots, seeds, nails, &c. have come away by urine. To these may be added, Tho. Bartholinus, Act. Med. vol. 2, obs. 125; vol. 3, obs. 68; vol. 5, obs. 57, 70. As also in his Tr. de Lact. Thorac. cap. 6, 9; Fabr. Hildanus, cent. 5, obs. 51; who write of pins, &c. discharged by urine. But Dr. Fairfax writes of one more surprising, viz. a leaden-bullet swallowed by a woman for the colic, was passed with the urine some years after, incrustated with a gravelly, gritty, and stony accretion, as mentioned in the Philos. Trans. N° 40.

I was once assured by a Physician in the west part of Cornwall, that he knew a woman who voided by urine a small plum-stone. But there happened at Loo, in the same county, a more surprising accident of that sort, viz.

Nathaniel Mitchel, about 50 years of age, was in the summer of 1690 seized with violent colicky pains, which he mitigated by clysters, but could not perfectly free himself of them. About Michaelmas 1691, his pains being very violent, he was relieved by the same remedy, and by the persuasion of a skilful woman, he drank the powder of nettle-roots in white wine; after the first or second dose he discharged a great quantity of urine, with a very feculent sediment. About the beginning of November 1691, being costive, he eat mallow-roots and currants boiled, and mixed with butter, his usual laxative medicine; in a little time after eating it he was much disordered, and complained of an oppression from wind; at length the wind (as he termed it) settled at the bottom of his belly, and in a very little time with his urine he emitted some of the herbs, with above 40 currants; a few days after he discharged the same way several parsley leaves, which he had eaten a little before. The physician being shown the patient's urine, he at first thought that part of his fæces had been evacuated that way, and that some latent ulcer had made a passage through the intestinum rectum into the bladder, but he found it otherwise; for there was no fætor in the urine, he had no tenesmus, no bloody, nor purulent dejections; but to satisfy himself further in this particular, he ordered him a clyster tinctured with indigo, which he retained above half an hour, but his urine was not at all discoloured with it. He prescribed for him some pills, two of which came away in his urine, of an oblong form, about the size of the end of a goose-quill. Some time after there came away with his urine a piece of a raisin. He lived till Midsummer 1692, in which time he discharged at divers times parts of roots, and other things he had eaten.

Diemerbroeck, Farfax, T. Bartholine, O. Borichius, N. Blegny, Mr. Pecquet, and others are of opinion, that there is a more latent passage for the urine to the emulgents, &c. than those commonly supposed; that they think it appears so by divers phænomena and experiments they have made, and that their objections against the common opinion are insuperable. It is certain, that the matter of an empyema has been discharged with the urine, and to that purpose diuretics are used in vulneraries, &c. See Malpigh. N. Blegny, Wiseman, &c. And I have known a large ripe aposthumation in the thigh of a woman, suddenly sink, and all the matter come away by urine. Mr. Leyser has the like story in his observations. See Philos. Trans. N° 50. A boy, of about 6 years old, was once brought to me, who discharged most of his urine from an orifice in the navel. I remember Blasius, or Veslingius, relates the like, and accounts for it.

Of an unusual Blackness of the Face; and of several Extra-Uterine Fœtuses. By Mr. James Yonge, F. R. S. N° 323, p. 424.

The relation I sent of a hairy bunch ejected by urine from a nephritic woman, I find did not meet that regard and credit which I think it well deserved. I own that Mr. Leuwenhoeck's objections seem to have some strength, but cannot shake my belief at all; for besides a nice examination of all the circumstances, at first, I am still persuaded, that it came through the urethra, and was neither conveyed, nor by any accident dropped into the pot, by such evidence as is little short of demonstration; as, that the tumour in the side of the belly vanished with it; with all those other symptoms which molested her, viz. strangury, fetid and purulent urine, and have not now in 2 years made any return.

I have a couple of other rarities to present to the Royal Society, one of which is matchless, and to me wholly new.

A girl in Plymouth, 16 years of age, had about the end of April a few hot pimples rise on her cheeks, which bleeding and a purge or two cured. She continued very well till about a month after, when her face suddenly turned black, like that of a negro. This surprising accident much frightened the girl, even to distraction, when my assistance was required. By the arguments I used, and some composing anti-hysterical remedies, the violence of her fits was much abated; I also directed a lotion for her face, which took off the discolouration, but it returned frequently, though not regularly, sometimes twice or thrice in 24 hours, sometimes five or six times. It comes on insensibly, without pain, sickness, or any symptoms of its approach, excepting a little warm flushing just before it appears. It easily comes away, and leaves the skin clear and white, but smuts the cloth that wipes it from the face; it feels unctuous, and seems like grease and soot or blacking mixed. It has no taste, which seems very strange, that a fuliginous exudation should be insipid.

She never had the menses, is thin, but healthful; the blackness appears no where but in the prominent part of her face.

A clergyman communicated the following occurrence to me, viz. A gentleman's servant having killed a ewe, which was thought fat, and taking out the bowels, found a very unusual and monstrous lump of fat, proceeding like a wen from the middle of the omentum. The clergyman having cut it open, found inclosed a lamb, of the same parts and dimensions as others of that kind. The question was, how it came there? and how it was nourished? I soon apprehended what it was that seemed so strange and unaccountable, having 30 years since been shown the like, found in a bitch, by a surgeon in Oxford; and from that time observed and considered all of that nature which have occurred to me in books or otherwise; and hence concluded, that the lamb was not conceived in the womb, but in one of the Fallopian tubes; where growing too large to be contained, it either broke out into the place where it was found, or slipped back towards the upper orifice, and through it into the ewe's belly; that afterwards, assisted by the prone and inclining posture of the sheep's body, it slipped forward to the omentum, and was there nourished in the usual way, viz.

by the placenta, which was doubtless fixed in the tube, and the pedunculus remaining entire, would easily extend from thence to the fœtus.

Riolan, jun. Anthropog. Nov. lib. 2, c. 34, was the first that publicly observed these strange conceptions; and he tells us, that they have the same coats, secundine, &c. as the fœtuses engendered within the womb. And considering the late anatomical discoveries, and new hypothesis of the genital parts in women, and their uses in generation, made by Kirkringius, de Graaf, and others, it seems more than probable, that such conceptions happen when in coition one of the ova descends into the tube; and being unable to pass into the uterus through the lower orifice (which is sometimes, and in aged people always, contracted) and being however impregnated by the aura seminalis, or animalcula, with which the testes and seminal parts of some males, according to Leuwenhoeck, do so prodigiously abound; it there grows till too large to be contained, and then breaks forth into the belly: though sometimes they continue in the tube itself. There the placenta fixes, and draws nourishment, as from the fundus uteri; and if the pedunculus hold together, conveys it to the fœtus in the same manner as those in the womb.

Such extra-uterine embryos have been sometimes found in women; but not publicly taken notice of, till the beginning of the last century, by Riolan the younger, with this remark: viz. the Fallopian tubes appear, says he, of the same nature and substance as the uterus, quia carnosa est, in qua, quod est mirabile, fœtum humanum concipi fuit observatum; and then he gives an account of four such strange conceptions, which occurred to him.

Since that time, more marvellous cases have happened in that country; two of them much amused the curious every where. One was found at Paris, January 1669, by Mr. B. Vassal, in the right tube of a woman. It was 4 months old, and so grown, and the tube so distended, as made him mistake it for another womb, and accordingly to call the account he published thereof, Demonstration d'une double Matrice. See Phil. Trans. N° 48:* also the Memoirs of the Nat. Curios. Vol. I. Obs. 110. But the mystery seemed not to be understood, until de Graaff took it right, and made use of this very observation to illustrate and confirm the hypothesis of Kirkringius. And soon after Elsholtius, a learned German, did the same in his tract, De Conceptione Tubaria, qua humani fœtus extra uteri cavitatem in tubis quandoque concipiuntur. He recites the story as from Mr. Vassal, and gives the figure of the supposed two wombs and the fœtus in the distended tube; and among other

* Vol. I. p. 358, of these Abridgments.

such conceptions, mentions two large molas found without the matrix of a woman.

About 10 years afterward a much more extraordinary instance happened at Paris, which puzzled the philosophers to apprehend, or believe. It comes very well attested by Mr. Bayle, who first published an account of it in the Journal des Sçavans, A. D. 1678. Soon after M. Blegny did the same in a particular tract with figures, which I have by me: and afterwards Mr. Oldenburgh put an extract of it into the Phil. Trans. N°. 139.* A woman, A. D. 1652, came to her full time of bearing; but though she had all the symptoms usual at that season, no child came: she continued in that condition 20 years, still feeling the child within her; but from that time she felt no motion it had. In June 1708 she died, and the next day was opened; when there was found in her belly, without the womb, a dead child, wrapped up in the omentum: it weighed 8lb.

Before either of these appeared in France, there happened one in Holland to H. Rhoonhuys. A woman with child, at her full time, was 4 days in labour, and could not be delivered. M. Rhoonhuys being called, Dec. 1658, found the internum osculum uteri close shut, without flooding, or other fore-runners of delivery. Finding the common passage so closely shut up, and a very painful tumour above the navel, he proposed the Cæsarean section: but he delayed it till the woman was past recovery. On opening the belly after death, he found a child among the intestines; and the placenta fastened to the colon, and part of it to the fundus uteri; and that there was a breach in the womb, large enough for the infant to pass through it into the belly; and that wound he thinks was caused by a blow, though it hurt not the external parts, nor made any impression on the tender embryo. I cannot approve, nor will I censure the many things in his report liable to exception. I presume he mistook the extended tuba for a matrix, as Vassal did.

T. Bartholin met such an extraneous fœtus wrapped up in a mola, which he found in the belly of a woman: and thus conjectures, that it was first conceived in the tuba uteri: vide the 92d Observation of his 6th Century.

In the city of Orange, A. D. 1662, D. Baldwin, and Mr. Delafort found a fine male fœtus without the uterus. The report of this discovery is published by Sachs, with very learned remarks, in Miscell. Cur. Vol. I. Obs. 110; which he concludes with one more extraordinary than all I have cited, which he had from the Silesia Chronicle, written long since by N. Polinus, as follows: A. D. 1581, a woman that had borne 10 children in 15 years, conceived again, and at

* Vol. II. p. 435, of these Abridgments.

the full time was delivered through an abscess of the left hypocondrium, of a fine boy, who was baptized, and lived a year and a half, but the mother died the third day in great agonies.

The anomalous blackness of the girl's face, is now divided into a few dark, cloudy specks; which appear but seldom, and nothing so livid as formerly.

I am told by a sportsman, that he lately found in the paunch of a hare, two full grown young ones among the intestines, but almost rotten; as also 3 immature embryos in the uterus. The former were certainly fœtuses broken out of the womb, &c.

An Account of three Cases of the Hydrophobia. By R. Mead, M. D. F. R. S. N° 323, p. 433.

The symptoms from the bite of a mad dog are so surprising and terrible, that it is hardly possible to describe the agony of a patient in this unhappy condition. I have lately had the opportunity to see 2 instances of this case. The first was of a lad, about 9 years of age, a very stout boy. April the 20th, a mad bitch of the mongrel kind was hunted in the street, he struck at her with a stick, on which, flying in his face, she bit him in the right cheek, which was torn with a large wound to the middle of the nose. A surgeon cured the wound in about 14 days, by applying for the first three days, theriac. andromach. in sp. vin. and afterwards dressing it with liniment. arcæi and balsam. terebinthin. No other care was taken, only a bolus of theriac. andromach. was given him every night, while under cure, and soon after he was bitten, he was persuaded to eat the whole liver of the bitch fried.

He continued very brisk and well till the 22d of May; when he seemed dull and sick, would eat no dinner, excepting a little boiled spinage, walked out in the afternoon, and in the evening complained of his stomach and head; his mother gave him a small glass of brandy, for he would drink nothing else. In the night he was very bad, started often, and screamed out as in an agony, especially when desired to drink, and complained miserably whenever he made urine, saying it hurt him. The next morning he vomited up the herbs he had eaten the day before, unaltered. I was sent for that day in the afternoon, and found him in a perfect agony, all in a sweat, trembling, tossing himself up and down, talking continually, looking very wild; his pulse low, and sometimes quicker, then again slower: his urine of the night before, of the usual colour. I desired him to drink; he took a little in his mouth, but as it was going down, he threw it out with violence, saying it hurt him; and praying that he might take no more. We over persuaded him to hold a little in his mouth, and swallow it by degrees, and gently; he did so with a little more ease, but was glad when it was over. We bid him suck the drink through a quill; he tried, but could not get it down by continual gulps, but stopped as soon as a very little was passed, still crying out that it hurt him to swallow it. I presently declared the case to be desperate. However, for the satisfaction of the relations, blistering plasters were applied to the back and on each side of the neck; and a diuretic bolus of sal. succin. camphor. and conserv. lujul. was given every 6 hours; for he seemed from the first of his complaint to have a difficulty of urine.

The next day, the 24th at noon; I found him much worse; he had raved all night; could not bear the sight of any thing white, and said, that if all the women in the room who had white aprons would go out, he should be well presently. He said he would drink if we would give him some in a black cup; but when brought, made many excuses, and could not, though at the same time complained he was dry, and pleased himself with talking of full pots. He ate some bread and butter heartily, but vomited it up quickly, together with a frothy slime. We dipped him in a tub of warm water; he said he was not afraid of water, and was quiet in it for a little while; but he soon fell into a convulsion fit, which obliged us to take him out. I observed his eyes grow more staring, and the pupil prodigiously enlarged. He was thrown continually with such violence from place to place, that it was very hard to keep him in bed; and quite tired and spent, fell into cold sweats, and died this day at 4 in the afternoon.

The next day I obtained leave to open the body. We examined the brain throat, breast, and stomach, but met with no extraordinary appearance any where, excepting that there was a great quantity of greenish viscid bile in the stomach.

The other patient was a very lusty vigorous man, of 45 years of age. He had 10 weeks before been bitten in one of the fore-fingers near the nail, by a little naked dog of the Guinea breed. On the 8th of November in the morning he complained of a great sickness at stomach, and vomited green and yellow choler. The next morning he took a dose of rad. ipecacuanh. While he was vomiting, he complained of a difficulty of swallowing; and when pressed to drink to work off the medicine, contrived himself a way of sucking the gruel given him, through a piece of a tobacco pipe, but could not get down above one pint; and though he afterwards often tried this contrivance, yet it did not succeed. On the 10th he had 8 oz. of blood taken away at the arm; and he took a bolus of theriac. andromach. with lap. contrayerv.

I came to him on the 11th; found him tied in his bed, raving mad, biting and spitting at the by-standers, crying out murder, making an odd noise, as if he coughed up something from the throat; this motion I had also observed in the boy, and I suppose this is what some authors have called barking. He said he would drink, if we would unbind him, and give him water; but as soon as it came to his mouth, he threw away the cup with the greatest fury imaginable, and grew so unruly, that he was with much difficulty tied down again. I observed that he had a palsy of his right arm, for he moved this only by the help of the other; and those who attended him had taken notice that this symptom began the day before, and that at the same time he had endeavoured to read, but could not, complaining of a mist before his eyes. As he seemed afraid of every body, so he showed the greatest enmity to those, for whom at other times he used to have the most love and respect. I ordered a surgeon to take away 20 oz. of blood at his arm: and observed it to be very thick and black. He was very tame after this for a few minutes, but fell again into his outrageous fit, in which he soon laid himself down quite spent, and died.

Since these accidents I have had an account sent me by a surgeon from Stamford in Lincolnshire, of a young man of about 18 years of age, who died hydrophobous by the bite of a mad fox that had been bitten by a mad dog. The symptoms appeared 3 months after the wound, which was on the back of the hand, and being healed by the application of theriaca andromach. had left a small black scab behind. Three days before his death he was seized with a fever, for which he was blooded, vomited, and blistered; he bit to pieces the glass in which the drink was given him. When dissected, the fauces were found very much inflamed; the left lobe of the lungs black, with the vesicles full of black blood; the surface in some places, which the blackness had not covered, appearing blistered, as if raised by cantharides. The liver was hard, and of a yellow bilious colour. During the whole violence of the distemper, the penis was observed to be continually erected, and as hard as a bone. This symptom is particularly taken notice of by Cælius Aurelianus.

The surgeon who opened the body, with his knife slightly wounded his forefinger, and was surprised to find that it festered, and gave him much more pain that a greater cut had at other times done. This I the rather take notice of, because something of the same nature happened to the surgeon who dissected my patient. His hand the following night was taken with an erysipelas, attended with great tension and pain: this was owing to a little wound made in one of his fingers a day or two before, from which, in turning over the parts, he had rubbed off the plaster; and it went not off without the continued application of cooling and discutient medicines.

From all these accounts, it may not perhaps be wrong to conclude, that the hydrophobia, a name not very proper for the distemper, (see Essay on Poisons) is the effect of a particular kind of inflammation in the blood,* accompanied with so great a tension and dryness of the nervous membranes, and such an elasticity and force of the fluid with which they are filled, that the most common representations are made to the mind with too great effect, and the usual impressions of objects on the organs cannot be suffered: hence proceed the timorousness, unaccountable anxiety and inquietude, which are always the forerunners of the dread of liquids; as did also the pain in making water, and the strange aversion observed in the boy at the sight of any thing white; the retina being really hurt by the striking of the rays of light upon it. Nor is it hard to conceive, that when the salival liquor is hot, and the throat inflamed and dry, the swallowing of drink should cause such an intolerable agony; no more than when things are wrought up to this wretched condition, the dismal tragedy should not last above 3 or 4 days at most, in which the patient is perfectly fatigued and torn to death by the violence of his actions and efforts.

An Experiment attempting to produce Light on the Inside of a Globe-Glass lined with melted Flowers of Sulphur, as in the Experiments of Sealing-Wax and Pitch. By Mr. Fr. Hawksbee, F. R. S. N° 323, p. 439.

I melted in a ladle about half a pound of flowers of sulphur, and pouring it into a glass globe, I used it in all respects as in the other experiments with sealing-wax and pitch: and when it was exhausted, and motion and attrition given it, I expected, as before, to have seen a light on its inside; but all that we could do had no effect on it, to produce such an appearance, either when it was exhausted, or when replete with air. Nothing was observed but a very weak light, which, after long rubbing, showed itself in that part where the hand touched the glass. But when I looked upon it, I found the sulphureous lining all in a body disengaged from the concave surface of the glass. As to the electricity of the globe lined with this sort of matter: after its attrition had been continued for some time, and the glass was become pretty warm (at the same time being full of common air) the ring of threads was held over it; but the attraction was very inconsiderable on the lined part, though on the transparent side the threads were pretty vigorously directed; yet not with such force, as when the glass is quite clear within, as this was not; because the fumes of the melted sulphur adhering to it, made it appear somewhat cloudy.

A Repetition of the foregoing Experiment with Common Sulphur.—I repeated

* Yet the hydrophobia is not curable by venesection and the so termed antiphlogistic remedies, as the writer of this note has shewn in a separate tract on this subject.

this experiment with a quantity of common sulphur, nearly equal to what I had used before of the flowers; which having melted as before, I poured it into another globe-glass, which I used in all respects as the former. But when I had exhausted it, and given the usual motion and attrition, the effect was so surprisingly different, that one would scarcely think it could proceed from the same sort of matter. For the figure of my hand and fingers appeared not only on its inside, (though more faint and pale than in the experiments of sealing-wax and pitch) but on its outside there appeared a brisk purple light, very beautiful to the eye. The strength of this light may be judged from hence, that the lines of the palm of my hand, being near the touching parts, were easily discoverable by it. And as this common sulphur differed vastly, in that part of the experiment already related, from the former, so likewise in the latter; for when the ring of threads was held over it, (under the same circumstances as in the other) they were directed towards it as vigorously as in any experiment heretofore made. The parts lined and transparent performed much alike: if there was any difference, it seemed to incline to that part lined with the sulphur. In this experiment also, as in the last, the sulphur was loosened, and separated from the glass that contained it: which therefore cannot be urged, as anywise conducing to the unsuccessfulness of the former.

I repeated this last experiment with a large quantity of sulphur, and I poured about two pounds of it melted into a glass globe of the same size as the former, being about 5 inches diameter; this, when cold, contracted itself, and became loose from every part of the glass, as in the former experiments. The sulphur covered more than half the inner surface of the globe, and its thinnest part was about half an inch in thickness; towards the axis it appeared to be more than a full inch thick. This glass, when exhausted of its air, was used in every respect as the former. The light produced on its outside was very considerable, and attended with the same colour and vivacity as before; nor was that less vigorous on its inside. Comparing it with the former, notwithstanding the thickness of the lining, it was at least 4 times greater; but the figure of the fingers was not now so distinguishable, as in the other. But on the part near the axis, where the substance of the sulphur was much the greatest, no light was produced; which may be attributed in a great measure to the slowness of the motion, and the weakness of it there, in comparison with that which is made more remote from it, where it was that the light was seen within. What farther is observable was, that the light, visible on its outside only, appeared to be produced between the inward surface of the glass and the convex surface of the sulphur; the sulphur being loose from it, gave liberty for the air to be taken from thence, as well as from the other parts: the light which was there produced, being reflected by the hard, polished, and nearly contiguous body of sulphur, seems to be the reason why it appeared with so much vigour. This outward light would sometimes break into branches all over the lined part of the globe, in as odd, and as pleasant a manner, as what has been noticed in former experiments, with the large globe glass, upon letting in a little air. And when the attrition was ceased, but the globe continuing its motion, abundance of sparks of light would appear all round it, and continue so to do for some time, without any fresh attrition.

Microscopical Observations on the Particles of Crystalized Sugar, &c. and the Manner of Observing the Circulation of the Blood in Eels. By M. Leuwenhoeck, F. R. S. N° 323, p. 444.

I have said, in a former Transaction, that the particles of sugar-candy consisted of two broad and two narrow sides; and that the other, i. e. the top and bottom, ran into a sharp point, like the figure of a wedge or chissel. The following are some figures of them. Fig. 4, pl. 12, represents a small bit of sugar-candy, of which one can seldom see so perfect a figure; because they are almost always fastened to some other particles of sugar, so that we can only discover the superficies of one side, as here in this figure HIEFG; but when it is taken out of the syrup or liquor, without being united to any other particles, the other side represented by ABCDK will also appear after the same manner. We also observe, that all the particles of the sugar-candy, even that which comes out of the East Indies, if it be not too irregularly coagulated, and fastened to the sides of other particles, has generally one side obtuse, and different from the other three, which have acute angles, just like a square piece of wood, one of the corners of which is partly cut away, as apears at IK.

Fig. 5 also represents a small particle of sugar-candy, which had been joined to others at the side LMN; and at O there appears a very small particle of the candy, that seems to have been coagulated with the said figure when it was much smaller; which particle appeared like mountain crystal, and under it was another, composed of about 10 small crystals.

For my further satisfaction concerning sugar-candy, and its coagulation in syrup, I took some powdered sugar, and dissolved it in water, and then boiled it until I supposed all the water was evaporated; after which I placed it on several glasses, to observe the coagulation of its small particles. Some days after I observed a great many complete figures, which lay coagulated in several shapes, but all of them as clear and transparent as crystal, forming a pleasant sight: but I expected to have found them all of one and the same shape, and that they would have appeared like figure 4; but when viewed with a microscope, some of them appeared like fig. 6. This appeared at first surprising; but when I considered, that the particles of sugar, some of which are many thousand times smaller, so small, indeed, that they escape the sight through a microscope, do not appear to the eye in the same position, nor that their wedges are represented as in fig. 4, by BC or GF, but on the contrary, the side which is described in the said figure by CDEFIK lies sometimes uppermost or undermost; then it is not surprising, if the same particle of sugar-candy shall appear to the eye as in fig. 7.

In that particle of sugar above-mentioned, I observed several streaks or fibres that lay inwards, and which, by reason of the transparency of the sugar, appeared plainly to the eye, as shown in the said fig. 7, between D and C, and D and E; and so likewise from the centre of the sugar, where those streaks extended on each side to B and F. From this observation I concluded, that the sugar increased from time to time, in proportion to the spaces between each streak or fibre.

I likewise saw a few coagulated sugar particles, that appeared in complete quadrilateral figures, one of which is represented fig. 8, and was as clear and transparent as any diamond. It must also be observed, that these figures 6, 7, 8, were not larger than a small grain of sand. In the middle of fig. 6 and 8 was a very clear particle, of the same figure with the whole body; from whence appears, that the said whole body was much smaller at its coagulation, but increased continually by new accessions of matter round about it; and that in proportion to the number of perimeters, the body increased in size from time to time.

These little figures preserved their complete forms and crystalline appearances as long as it was dry weather; but when it happened to be moist or rainy, we observed moisture about the particles of the sugar, which in dry weather evaporated again; and then there coagulated an infinite number of small sugar particles upon the greater, and those were so exceedingly small, that a thousand of them together were not so large as one of those particles before represented in fig. 6, which was itself not so large as a single grain of sand. Now since we see that from one and the same matter two different figures are coagulated, it is easy to conceive that several other figures might be produced in the first coagulation, especially when any of the parts of those little bodies lie upon each other; and therefore also we should not wonder, to see, in the coagulation of salts, several figures produced out of one particle of salt.

I formerly shewed the circulation of the blood in an eel, by putting the eel into a long glass tube, with the tail uppermost: but I have left off that way for some years, and I now prepare copper plates of about a foot long, and 7 inches broad; one end of which, of the extent of an inch, I bend, and at the other end I make a square hole of 5 inches long and 2 broad, in which I put small glass plates, as clear and as thin as I can possibly procure them: on such a glass plate I lay one of the smallest eels I can get, which is sometimes as thick as a finger; I then blind the head and the best part of the body of the eel about with a linen cloth, to bind it, for then it will lie more still on the copper-plate; and the tail is laid upon the glass: and that part of the body of the eel, that is wound about with the cloth, is also fastened to the plate with a wire, that the eel may not wriggle itself off.

The eel being thus placed on one side of the glass, in the copper-plate, the microscope is fastened by wires and screws on the other side, in such manner that it may be moved in every direction. And this I take to be a better method concerning the circulation of the blood than my former; which if other persons would use, I doubt not but they might observe the same thing in an eel as I have done: and if they would also view the arm, and with great care consider the pulse in the veins, they would certainly discover that the blood, which causes the pulse, proceeds from the hand.

On the Bones of a dead Calf taken out of the Uterus of a Cow; and of a Callus that supplied the Loss of Part of the Os Femoris. By Mr. B. Sherman, of Keldon, Essex. N° 323, p. 450.

The cow being unwell, had continued a great while in a lean condition, and became so reduced, that it was concluded she would die; when on a sudden she began to eat her meat, and to thrive so very fast, that in 6 or 8 months she was so fat as to be sold to the butcher; who, when he killed her, found dry bones in her uterus, there being no manner of moisture in the bag (as he called it) in which the bones were contained. The same digestive humour, which dissolved the skin and muscular parts of the calf, might, I presume, reasonably enough be supposed to dissolve the cartilages, and perhaps even part of the bones. It seems there are many such instances in anatomical writers; and particularly one of a woman, whose fœtus dissolved so perfectly, that some of the bones came through the abdomen, and, what is more surprising, the same woman had children afterwards.

I may here add an observation in my own practice, viz. of a compound fracture, which happened on the thigh of a young man about 17 years of age. I was obliged to take out about 2 inches of the whole substance of the os femoris; and yet, by keeping a due extension, nature in four months' time supplied such a callus, that the part is not a quarter of an inch shorter than the other side; and the person is as strong as ever, and walks without any lameness.

An Account of the Great Frost in the Winter of 1708 *and* 1709. *By the Rev. W. Derham, Rector of Upminster, F. R. S.* N° 324, p. 454.

I believe this frost was greater, if not also more universal, than any other within the memory of man. The longest which has happened within our memory, was that in 1683; but the late frost, although of shorter continuance, was more intense than that; of which I have already given some account in N° 321. My thermometer was much lower on December 30, than it had ever been since 1697, when I first began my thermometrical observations; and the thermometer in our repository, in Gresham-College, was lower than ever it was before. The particulars of its greatest descents are these; January 26, 1696, 41 degrees; January 5, 1683, 40 degrees; and January 3, 1709, 43 degrees: and lastly, in Mr. J. Patrick's thermometer, in London, the spirits were 4 or 5 degrees lower than in 1683.

In London the greatest contraction of the spirits was on January 3; which was also an excessive cold day at Upminster; but the greatest contraction at this last place, was on December 30. The reason of the difference is, because my thermometer is always abroad in the open air, where no sunshine touches it; but those two London glasses are within doors, in rooms where no fires are made. And it is easy to perceive that the frost does not presently exert its greatest force within doors; and when it does, neither does it so soon abate its force within doors, as without. These observations of the intenseness of the cold with us, I have received confirmations of from other places in the southern parts of our island. I observed, that the descent of the spirits in my thermometer on December 30, was within $\frac{1}{10}$ of an inch as great as the descent effected at another time (and that in a cold day too) with artificial freezings, performed both with snow and salt, and also snow and spirits of wine, each of which I have several times tried, and find them nearly of equal power. It is well known that we can in summer freeze with ice and salt, and the same may be then done with sal ammoniac dissolved in water; but we cannot produce so intense a frost then by these means, as in winter, and especially in a very cold day.

But though the frost was so extremely rigorous in the southern parts of our isle, yet the northern felt little of it, but like other winters; as I have been certified by persons that have come from thence, as well as by several letters from such parts. One is from the Bishop of Carlisle, dated Rose, Nov. 5, 1709. "In January last, says he, I had a sufficient opportunity of observing the frost and cold being more intense in the southern parts, than here, and the snow much thicker. I began my London journey on the 26th of that month, three days before the thaw, and found that for several miles (near the banks of the river Eden, in both the counties of Cumberland and Westmorland) my horses hardly ever trod upon snow. When we came to Stanemoor, on the confines of Yorkshire, we found the ground covered pretty thick, and the deeper still the farther we came to the south. None of our rivers or lakes were frozen over; and the extraordinary flocks of swans that resorted hither (nothing like it having been seen by the oldest man living) was a sure argument that the temperature of climates was strangely inverted."

Another account from Edinburgh, November 5, 1709, from Sir Robert Sibbald, says, "I can learn no extraordinary effects of the cold season here. It was a long winter: the cold came early in October, and continued till near May. There was much snow, which lay long on our south hills near this place. We had not much frost to speak of, and it lasted not long." And as in Scotland, so in Ireland, the frost was very moderate; of which, among other things, I have this account in a letter from Dublin, from Mr. S. Molyneaux, who says, they had there a harder winter than usual, but judges they suffered not so much as their neighbours: they had two or three pretty hard frosts, and some snow, but not of any remarkable continuance, as he remembers.

As to the degree of cold abroad, in the comparison I have already given, between Dr. Scheuchzer's observations at Zurich, and mine here, I said, that he noted the cold to have been excessive there; but whether more than usual, he saith not. But by a letter I have lately seen from his brother, it appears to have been in as great and unusual excess there, as it was here with us. In that paper also I said to what excess the frost arrived in Italy, viz. that the cold there was so great, that for 20 years past they had not been sensible of greater, and on Twelfth-day it wanted but half a degree of the extremity.

As to the northern parts, in a letter received from Mr. Otho Sperling, from Copenhagen, he calls it Hyems atrocissima: and I find it noted in the minutes of the Royal Society of May 4, 1709, that Dr. Judichar said the ice was frozen in the harbour of Copenhagen 27 inches thick; and that April 9, N. S. people had gone over between Schonen and Denmark on the ice. In the northern parts of Germany also I find they had the same fare with their neighbours of Denmark: where the alternate changes of the winds, and of the cold, more or less severe, commonly agreed with the same as observed at Upminster.

The snow was more with them, than with us; the winds changed with us from the easterly points to the westerly and southerly, a day or too sooner than with them; then agreed with them; and soon after veered about to the easterly and northerly, as it did with them. And I further observe also, that when the winds agreed in both places, my notes show the wind to have been of some force here.

As to the effects of the frost on fluids, the waters we may easily imagine were the first things that felt them; and these were in many places frozen to an extraordinary depth; though I hardly believe equal to the frost in 1683; of which we have a sufficient instance in the river Thames, the waters being so frozen, that above bridge many booths were erected, fires made, and meat dressed; and on January 10, 1684, I saw a coach and two horses drive over the river into Southwark, and back again, a great number of people accompanying it. But this last winter the case was very different; several people crossed the Thames at some distance above the bridge; but that was only towards low water, when the great flakes of ice that came down stopped one another at the bridge, till they made one continued bed of ice from thence almost to the Temple; but when the flood came, the ice broke, and was all carried with the current up the river. The like happened between Westminster and Lambeth, a little above Whitehall. As for other waters, they also had their share; especially where they lay exposed to the north and north easterly winds. Nay, the sea-waters themselves escaped not, but were covered with ice in many places near the shore, in harbours, and where they lay calm and still. Of this I have already mentioned an instance in the harbour of Copenhagen, and the sea between Denmark and Schonen; and in a letter from Dr. Newton, her Majesty's envoy at Florence, he tells me, the sea was frozen both on the coast of Genoa and Leghorn. And similar accounts are from many parts of Germany.

These effects I am apt to think the waters felt not only in England, Denmark, Germany, France, and Italy; but in all the northern world also, excepting Scotland, Ireland, and probably some other islands, or places near the sea; though even some of these appear to have been great sufferers too. This universality of the frost, I suspect from the multitudes of divers kinds of birds (utter strangers to these parts, and many of them inhabitants of the northern colder countries) which were seen and killed in many parts of England. In our Essex marshes, near us, we had many wild swans, brent geese, many of the rarer gull kind, and divers other sorts of birds, utter strangers to these parts.

The effects of the frost on animals, were also very severe. We are told how animals suffered, both in Germany and other places; that the fresh water fish were every where killed; and that a vast destruction befel their small birds. Nay some did not hesitate to affirm, that they saw birds, as they flew along, drop down out of the air. That the Lusatia letters said many cows were frozen to death in their stalls. And many travellers on the road were frozen to death, or lost their hands, feet, noses, or ears; and others fainted, and were in great danger of life or limb, when brought too soon near the fire. Of these particulars divers instances are given from the newspapers: as of 2 gentlemen, and a smith in England, and above 60 men, and many cattle near Paris; and the like at Venice, and 80 French soldiers near Namur, all killed on the road with the cold. Our fresh water fish also, in England, were many of them destroyed in ponds that were shallow, and especially if long frozen over; some for want of air, where the ponds were not kept open; and some with the cold air at the holes in the ice, where in great numbers they came to get breath. On the Italian coast some of our poor mariners on board our men of war died of the cold; and several lost part of their fingers and toes: as Dr. Newton writes me. But the greatest sufferers in the animal kingdom were birds and insects; as redbreasts, larks, &c. But among all the sufferers by the frost, the vegetables were the most universal; few of the tender sorts escaping, to the great damage of the owners. About us, bays, rosemary, cypresses, myrtles, most of the phillyreas, yea, even junipers, among shrubs; and artichokes, cauliflowers, and a great many other olitory plants, suffered greatly. In a word, so great were the damages done among the gardens, that I have been informed some of them have lost to the value of 80, 100, or £200. But the most exact account I have met with, is from that accurate botanist of the Oxford physic-garden, Mr. Ja. Bobart, in a letter to Mr. J. Thorpe, F.R.S. in which he observes that the damages of this frost do not come up to those in 1683; which frost being of longer continuance, cleft the oaks and bodies of the vines, &c. But in the last frost there were intervals of relaxation, besides several considerable snows, which proved a defence to many plants. But the snow melting, and the cold withal continuing, proved of evil consequence to many bulbous and tuberous roots, and abundance of other things. But, says he, the sharp, dry, and cutting winds from the north, and north-east, were most destructive to many of the ornaments of our gardens, which before seemed to be almost naturalised to our climate, as cypress, bays, rosemary, alaterni, phillyreas, arbuti, laurustines, &c. as also to most of our frutescent herbs, such as lavenders, abrotonums, rue, thyme, and divers others of such race, especially such as had their heads above the kind covering of the snow; and not such exotics only, but some of our own natives, as is visible in most of our furze-fields, and divers hollies, especially of the finer striped race, have felt the smart of such the rigour of the season, by the loss of their leaves, and sometimes their lives. And what has been more observable this year, than in others, is, the sap of our finer mural fruit-trees, as of peaches, nectarines, apricots, &c. was so congealed and disordered, that it proved stagnated in the limbs and branches, and equal to chilblains in human bodies; which in too many parts of the tree, turned to so frequent mortifications, that it is doubtful whether sufficient vigour is ever to be expected from them, to be worth their standing. What he observes concerning the destruction of wheat was, I believe, a general calamity; as also the particulars he takes notice of much the same in other places too, viz. "Where the land was poor, and coldly exposed, the wheat was killed; that many lands of wheat escaped tolerably well on the warm side, when the other side was quite killed with the extremity of cold." By the warm and cold sides, I suppose our ingenious observer means the sunny and shady sides. But with us the wheat suffered rather more on the southern, sunny side, than the northern; I suppose by reason the ground was somewhat opened by the sunshine, and the covering of snow melted, and way was thereby made to the severity of the nocturnal frost: on which account I have heard it said by some skilful observers, that vegetables suffered more the last winter from the sun than the frost. And not only shrubs and plants, but the larger trees have in some places had their share of suffering too. But it was observed by some ingenious persons at one of the meetings of our society, that the calamities which befel trees, arose not purely from their being frozen, but principally from the winds shaking and rocking them at the same time, which rent and parted their fibres.

As to other places, I find the effects were, in the more southerly parts of Europe, much the same on their vegetables as in ours. In Italy, Dr. Newton says, almost all the lemon and orange-trees, with those of the like kind, are destroyed in this country by the frost, and a great many olive-trees. The leaves of the bay-trees have the same colour now, as all others have when they are falling in October. And Dr. M. Angelo Tilli, the learned botanic professor at Pisa, writes me that the frost has destroyed a world of trees, both in the city and in the country about them. And as to the northern parts of Germany, the case was much the same as with us in England, or still worse. In Switzerland too, among the high Alpine ridges, they felt the dire effects of the frost, but yet some places were so happy as to escape.

Microscopical Observations on the Configuration of Diamonds. By Mr. Leuwenhoeck, F. R. S. N° 324, p. 479.

I took a small polished diamond, and broke it to pieces with a pair of pincers; but observed nothing more in the broken particles, than in those of common glass. I afterwards placed before a microscope a small rough diamond, and observing it more particularly, I concluded that all those streaks or fibres, which I saw in it, were only the several coagulations or augmentations it had received from time to time, and that in a very short space of time.

Fig. 9, plate 12, represents a small particle of a little diamond, as it appeared through a microscope; in which, between A and B, as also between CDEF, were observed a great number of lines or fibres; occasioned, I suppose, by the increase or accession of new matter. Now that the increase of diamonds is made in such a manner, we may conclude the rather, because we know that the same thing happens in the coagulation of many salts. I have several times taken some of these particles, and laid them on burning wood coals, until they were red hot, and in that condition thrown them into the water, to see whether they would burst to pieces, or whether there would be any separation of matter from them; but that never happening, I must conclude that there was no air nor any moisture shut up within them.

One particle of a diamond appeared to the sight, as fig. 10, after I had made it red hot, and slaked it in water several times; in which also, between L and G, are several small streaks or fibres: and when I observed it the last time, after I had taken it out of the water, it appeared, between LGHI, as if some small scales had been separated from it; just like the shining or glittering parts often seen in some stones, and particularly in the great flint-stone brought in ships from Greenland for ballast, when the whale-fishing is not good, when its crystalline or diamond transparency is gone.

Fig. 11, represents also a particle of a diamond, as it appeared through the microscope, after it had been made several times red hot, and thrown into cold water; in the middle of which one might perceive such slits or cracks as might be compared to the top or ceiling of an unwainscotted church within side, but could not be so well traced by the painter as it ought to have been; but whether this appearance be natural to the diamond, or whether it proceeds from the breaking it in pieces, is unknown to me; but my opinion is, that it was not occasioned by its being made red hot, and thrown afterwards into the water; for if so, the diamond would have been separated into a great many particles, or there would have been several cracks or flaws in it.

Fig. 12, represents the small particle of a diamond, no larger to the naked eye than a small grain of sand, from whence we may judge also of the size of the other diamond particles, represented by the former figures. We may also observe, at STVWY, the sharp points of the said particle. From whence I conclude, that I was right in my former remarks concerning the particles of sand; viz. that the said very small particles consisting of regular points, and being smooth sides like diamonds, they were soft at their first coagulation, but grew larger by the accession of new matter, until they became large grains of sand; and also, that some diamonds were formed just after the same manner.

Now as we find, that in the solution of silver by aquafortis, some of the small silver particles are coagulated in crystals of the figure of diamonds; and that the sugar which is boiled to a syrup, in order to make sugar-candy, is also coagulated into such particles; so we may likewise suppose, that at the time when the diamond particles coagulate, a great deal of the same matter they are composed of, is in the air, but not to be perceived by our naked eye, nor the quantity thereof to be known, until it is coagulated into a body.

An Account of a Child's crying in the Womb. By the Rev. W. Derham, F. R. S. N° 324, p. 485.

I lately visited a woman brought to bed a week since, of a boy that had cried in her womb, at times for near 5 weeks. The child appears to be strong, and has been since its birth very quiet. The woman's name is Clark, and resides 2 miles from me, at Hornchurch. She told me, the first time the child cried was in the night, as she lay in bed, after a great pain which forced her out of bed, and gave her apprehensions of her labour being nearer than her reckoning. And every time after, whenever the child cried, she had violent pains like those of labour. From the first time of its crying, the child settled itself on the mother's left-side, and she never perceived it stir until its birth approached. Scarcely a day in all the 5 weeks escaped without crying. The midwife told me, she heard it cry 17 times in half an hour. Its crying might be heard into the next room. Both the mother, and midwife, a sensible woman, answered me a great many questions. In general they told me, they found no great difference between her case, and that of other women in the same condition. I asked the woman whether she had received any falls, or hurts, or was troubled with longings more than with her child before? She said she had received no hurt, but was more inclined to longing; but had what she longed for.

A short Dissertation concerning the Child's crying in the Womb. By the Rev. W. Derham, F.R.S. N° 324, p. 487.

Among the authors that question the fact of the vagitus uterinus, two of the most considerable are Etmuller and Diemerbroeck. Etmuller declares Diemerbroeck's opinion, as well as his own, in his Dissertation de abstruso Respirationis Humanæ negotio, ch. 9, where he treats of Dr. Harvey's Problem, why the fœtus after delivery, and before it has breathed, can live for some hours in its after-burden, &c. but having once breathed, it can scarcely live a moment in that manner? Etmuller's words are to the following purpose: some consider the vigitus uterinus to be an invincible argument for the respiration of the fœtus in the uterus. But since the testimony of weak women, the usual evidences in such cases, is not much to be regarded, and that the observation is not so accurately made, as to serve for a foundation of resolving this problem, I justly suspect with Diemerbroeck, that this seeming crying proceeds from the croaking of the flatulent guts compressed by the fœtus, which is often very surprising in some persons, imitating sighs and groans.

The matter of fact being thus called in question, may, in my opinion, receive an answer, in some measure, from the case in question; concerning which I must needs say, that though I am clearly of opinion, that the fœtus does not live in the womb by breathing, yet the evidence is so clear in the present case, that I am fully satisfied it was really the crying of the fœtus, and not the croaking of the guts, or womb, or the effect of any feminine imagination.

For here we have a thing happening not once or twice only, but a great many times; almost every day, and divers times in the day; and that for near 5 weeks together. In the next place, we have the child heard to cry aloud, so as to be distinctly heard by persons in another room. Consequently the hearers could more easily and certainly distinguish whether the noise was crying or croaking: and the description the mother and others gave me of it was, that the noise was as if a born-infant had cried eagerly, shut up close in a tub. Thirdly, the crying seemed to be so eager as to end in sobbing, like what is observable oftentimes in born infants. Fourthly, it was heard not alone by the father and mother, but by many of the neighbourhood, most of them used to children; who all, with the greatest assurance, affirm it to have been as manifest crying, as ever they heard from a born-infant. And lastly, the midwife told me, that laying her hand on the left-side of the woman's belly, where the child lay when it cried, she could plainly feel a motion under her hand, like that of respiration, every blast of the child's crying sensibly touching her hand.

These particulars not only prove the reality of the thing, but show the case to be very considerable. I have met with many instances of this nature in several authors, but not one of so long continuance, and in which there were such frequent reiterations of the crying. Some of the best attested cases may deserve to be recounted here. Verzascha, of Basil, has given a good catalogue of them, in the third observation of his Observ. Medic. But passing by the cases he mentions, attested only by illiterate persons, I shall name a few that seem to have better evidence. Ant. Deusingius, in his Dissert. de Generat.

Fœtus tells us, that he had it from his colleague Monæus, that the child cried in his wife's womb; and the same happened to the wives of Mr. Salmuth, and Mr. Grœnwolt. His next relation is that of Dr. Needham, of the fœtus crying in the womb of an English woman of quality, as she, her husband, and chaplain were together at supper. See the doctor's book De formato Fœtu. The last instance is of Christian II. king of Denmark, who was heard to cry before he was born. Now these being cases attested by persons who may be supposed of better understanding than the fanciful vulgar, seem to claim somewhat more of credit: the latter being the case of a king, and in all probability heard by some of the best quality about the court; the next heard by the chaplain as well as persons of quality themselves, and that three times successively; and the former coming within the cognizance of men of learning.

On this whole evidence (and more I could have added, even from the time of Hippocrates) I conclude, that the fœtus does really cry sometimes in the womb; though how this is performed, is hard to account for: surely not without respiration. And therefore I am apt to think, that though the fœtus does not usually breathe in the womb, yet it is possible for it to have an occasional temporary respiration there. But whether in such respiration, any of the blood passes into the lungs, or whether it does not continue its circulation through the foramen ovale only: or if any more than ordinary blood should by such respiration get into the lungs, whether it may not easily, and without inconvenience be discharged thence, during that state of life the fœtus leads in the womb: all these doubts I must confess myself unable to determine.

As to the pipping of chickens in the egg, about which Etmuller has the same doubt, as of the vagitus uterinus, I have myself often heard that, both from chickens and ducks. And a person, more conversant in such matters than myself, assures me, that a little before the hatching, she has often heard it; and can at any time cause some chickens, and ducklings, to pip in the egg. She says, that sometimes whole nests of eggs will yield a cry, at other times only some particular eggs: but that such eggs as have once afforded a pipping, may be made to pip and cry at any time, by shaking the egg, and putting the youngling into a disorder. And sometimes, where no noise has been before heard, the bird has been made to cry, by shaking the egg in which it was enclosed.

The cause of this pip in the shell, I take to be from some uneasiness the young bird may find there. It being arrived to its perfect state in the egg, is either weary of its confinement and desires more liberty; or else it lies uneasily, or is offended with shaking, and therefore pips and cries, as when uneasy out of the shell.

And after some such manner I take it to be with a human fœtus; that it is in some disorder, and uneasy in the womb, and therefore cries, as well in as out of it.

Observations on the Edge of Razors, &c. By Mr. Lewenhoeck, F. R. S. N° 324, p. 493.

When I view my razor, set in the best manner, through my microscope, I am amazed at the great number of gaps and notches in the edge of it, and wonder how one can shave so softly with it. As to the razor's becoming blunt in cold weather, I can conceive no other reason for it, but that the materia subtilis, or exceedingly fine matter, which is in all metals, and which we may compare to fire, is by the cold driven out of the edge of the razor; by which means the steel becomes so stubborn or hard, that in a fine razor it makes notches, and is blunted by the hair. I have also experienced, that after having shaved the beard with a fine razor, and attempting to cut some of the little hairs in the eye-brows, which were harder than those of the chin, though they were a little softened with water, they made several notches in the razor. I asked a certain skilful barber, what difference he found in his razors in very cold or hot weather; who informed me, that when it was very cold, he always dipped his razors in warm water, which made them cut much better.

M. Leuwenhoeck describes how he incorporates his leather strap with grease and tripoly, or with emery powder, both of which have a good effect in preserving the edge of his razors.

END OF VOLUME TWENTY-SIXTH OF THE ORIGINAL.

On the Usefulness of the Silk of Spiders. By M. Bon, P. R. S. of Montpelier. N° 325, p. 2. *Vol. XXVII.*

Spiders make a silk as beautiful, strong, and glossy, as common silk: the prejudice entertained against so common and despicable an insect, is the reason that the public has been hitherto ignorant of its usefulness. Even common silk, as considerable as it is, was long unknown, and was neglected after its discovery. It was in the island of Coos, that Pamphila, daughter of Platis, first discovered the manner of working it. This discovery became soon after known to the Romans, who brought their silk from the country of the Seres, a people of Scythia in Asia, near the mountain Imaus, where silk-worms narally breed; but far from making any advantage of so useful a discovery, they could never imagine these worms should produce so beautiful and valuable a thread, and made many chimerical conjectures about it. So that in consequence of their ignorance and idleness, silk was for several ages so very scarce, that it was sold for its weight in gold; and Vopiscus relates, that for this reason the emperor Aurelian refused his empress a suit of silks, though she earnestly desired it. Its scarcity continued a long time, and it was to the monks at last that we owe the manner of breeding silk-worms, who brought their eggs from Greece, under the reign of the emperor Justinian, as we learn from Godefridus, in his notes on the Code; and Ulpian assures us, that the price of silk was equal to that of pearls. It was late before France made any advantage of this discovery; when Henry II. brought to the marriages of his daughter and sister the first silk stockings that were seen in his kingdom. To him and his successors we owe the establishment of this manufacture at Tours and Lyons, which has made silk so common, and has so greatly increased the magnificence of furniture and clothes.

So many examples ought to show us of what importance it is not to neglect any thing in the study of nature: what at first seems of no use, or almost impossible to be put in execution, often turns to the greatest advantage, and becomes easy by care and industry. Indeed this is the fate of most new discoveries. The ingenious fable of Arachne shows us, that it is to the spider we owe the first hints of weaving cloth, and laying nets for animals; so the advantage which may arise from this insect, will perhaps make it hereafter be esteemed as highly as silk-worms and bees.

I shall reduce all the different sorts of spiders to two principal kinds, viz. such as have long legs, and such as have short ones: the latter of which furnishes the silk I am now speaking of. In respect of their particular differences, they are distinguished by their colour, some being black, others brown, yellow, green, white, and some again of all these several colours mixed together. They differ also in the number and position of their eyes, some having 6, others 8, and some 10, differently placed on the top of the head, as may easily be seen by the naked eye, but much better by the help of a glass. They are alike in other respects, as in their body, which nature has divided into two parts; the fore part is covered with a shell, or hard scale, set with hairs; it contains the head and breast, to which are fixed its 8 legs, each of them consisting of 6 joints; they have also 2 other legs, which may be called their arms, and 2 claws, armed with 2 crooked nails, and joined by articulations to the extremity of the head; with these claws they kill the insects they feed on, their mouth being immediately underneath them. They have 2 small nails at the end of each leg, and a spongy substance between them, which is doubtless of service to them when they go upon smooth bodies.

The hinder part of the body of this insect is joined to the fore part only by a small thread, and covered with a thin skin, on which are hairs of divers colours; it contains the back, belly, parts of generation, and the anus. It is certain, that all spiders spin their threads from the anus, about which there are five papillæ, or small nipples, which at first sight one would take for so many spindles, that serve to form the thread; I have found these papillæ to be muscular, and furnished with a sphincter. A little within these I have observed two others, from the middle of which issue several threads, in a pretty large quantity, sometimes more, and sometimes less, which the spiders make use of after a very mechanical manner, when they want to go from one place to another; they hang themselves perpendicular by a thread, and turning their head towards the wind, they shoot several others from their anus, like so many darts; and if by chance the wind, which spreads them abroad, fastens them to any solid body, which they perceive by the resistance they find in drawing them in from time to time with their feet, they then make use of this kind of bridge, to pass to the place where their threads are fixed. But if these threads meet with nothing to fix on, the spiders continue to let them out further, till their great length, and the force with which the wind drives them, surpassing the weight of their bodies, they find themselves to be strongly drawn; and then breaking the first thread, which they hung by, they let themselves loose to be driven by the wind, and flutter on their backs in the air with their legs stretched out. And by these two ways it is, that they pass over roads, streets, and the broadest rivers. See fig. 18 and 19, pl. 12.

One may himself wind up these threads, which by reason of their being united together, seem to be but one when they are about a foot in length; but I have distinguished them into 15 or 20 at their issuing from the anus. What is further remarkable, is the ease with which this insect moves its anus every way, by means of the many rings that border upon it. This is absolutely necessary for them, in order to wind up their threads or silk, which in the female spider is of two sorts. However, I believe this insect to be androgynous, or hermaphrodite, having always found the signs of a male in such spiders as lay eggs. The first thread that they wind is weak, and serves them for no other use than to make that sort of web, in which they catch flies. The second is much stronger: in this they wrap up their eggs, and by this means preserve them from the cold, and secure them from such insects as would destroy them. These last threads are wrapped very loosely about their eggs, and resemble in form the bags of silk-worms, that have been prepared and loosened between the fingers, in order to be put upon the distaff. These spiders bags are of a grey colour when new, but turn blackish when long exposed to the air. It is true, one may find several other spiders bags of different colours, and that afford a better silk, especially those of the tarantula; but their scarcity would render it very difficult to make experiments upon them; so that we must confine ourselves to the bags of such spiders as are most common, which are the short legged ones. These always find out some place, secure from the wind and rain, to make their bags in; as hollow trees, the corners of windows or vaults, or under the eaves of houses. And by getting together a great many of these bags, it was that I made this new silk, which is nowise inferior in beauty to common silk. It easily takes all sorts of colours; and one can as well make large pieces of it as stockings and gloves, which I have done.

After I had got together 12 or 13 ounces of these spider bags, I beat them well for some time with the hand and a small stick, to free them from dust. Then I washed them in warm water, till the water that came from them was clear. After this, I let them steep in a large pot, with soap, saltpetre, and some pieces of gum arabic, and let the whole boil 2 or 3 hours over a gentle fire. I then washed them again with warm water, to free them from the soap, and having let them dry for some days, loosened them a little between the fingers, that they might be more easily carded by the common silk carders, excepting that I caused them to use much finer cards. By this means I had a silk of a very particular ash-colour, which is easy to be spun, and yields a thread much stronger and finer than that of common silk. Which shows, that all other sorts of work may be made of it; and there is no reason to fear its bearing any trials of the loom, after having passed that of the stocking weavers.

Having shown the usefulness and possibility of making this silk, the only difficulty now lies in procuring a sufficient quantity of spider bags, to make any considerable work of it. And this would be no difficult matter, if we could breed spiders as they do silk-worms; for they multiply much more, and every spider lays 6 or 700 eggs; whereas the papilios or flies of silk-worms, lay only about 100; and of this number we must abate at least half, on account of their being subject to several diseases, and are so tender, that the least matter hinders them from making their bags. Whereas on the contrary, the eggs of spiders hatch of themselves, without any care, in the months of August and September, in 15 or 16 days after they are laid, and the spiders that laid them die some time after. As for the young spiders that are bred from these eggs, they live 10 or 11 months without eating; and continue in their bags without alteration of size, till the hot weather forces them to come forth and seek food. The reason of this is plain and natural; for all insects, and a great many other animals, as bears, serpents, mountain rats, &c. that lie hid during the winter, abound with a viscid matter, which is not easily put in motion; so that it is not strange that young spiders should live in the cold weather on their own substance, without any loss of spirits. But as soon as the warm weather comes on, it puts this matter in motion, and obliges them to spin, and run from place to place in search of food: and as soon as they begin to eat, one may perceive them to grow larger every day. From whence we may certainly conclude, that if we could find out a way of breeding young spiders in rooms, they would furnish us with a much greater quantity of bags than silk-worms do: for I have always found, that of 7 or 800 young spiders, there scarcely died one in the year; but on the contrary, of 100 young silk-worms, not 40 lived to make their bags.

So great and considerable a difference as this, will doubtless move the curiosity of such as are lovers of the arts, to endeavour to find out a way of breeding these insects. In hopes that some lucky chance, or my own industry, might favour me with so useful a secret, I made use of the following expedient to procure a large quantity of these bags, which I now propose to the curious, who may make the same trial of it as myself. I ordered to be brought to me all the large short-legged spiders, that could be found in the months of August and September. These I shut up in papers, and put them into pots, and covered the pots with a paper pricked full of holes with a pin, as were likewise the several papers that were in it, that the spiders might have air. I fed them with flies; and sometime after found that the greatest part of them had made their bags. But I more easily procured a great quantity of them, by promising to pay the same price for them by the pound as for common silk. This advantage furnished me in a short time with a large quantity: and the people assured me, they found no difficulty in getting them; and that if they were permitted to go into every house, where they saw these spider bags in the windows, they could furnish me with what I pleased. So that we may easily conclude, that there are spider bags enough in the kingdom, to make large pieces of work; and that this new silk which I propose, is not so scarce or dear as common silk was at first. And so much the more, as spider bags, on account of their lightness, yield much more silk than the others; as a proof of which, 13 oz. yield near 4 oz. of clean silk; 3 oz. of which will make a pair of stockings for the largest sized man: those I have made, weigh only 2 oz. and a quarter, and the gloves about $\frac{3}{4}$ of an ounce; whereas stockings of common silk weigh 7 or 8 oz.

It is certain that a great advantage may be made of this insect, which has always been thought troublesome and dangerous, on account of its venom: but I can assure you, that spiders are not venomous, having been very often bitten by them myself, without any ill consequence. And as for their silk, it is so far from having any venom, that every body makes use of it to stop bleeding and heal cuts; and indeed its natural gluten is a kind of balsam, that cures small wounds, by defending them from the air.

Their silk is useful, not only in respect of the manufacture it produces; but its usefulness is much greater, and more essential, on account of the specific medicines, that may be drawn from it. It yields by distillation a large quantity of spirit and volatile salt; and I have found by comparing, that it affords at least as much as common silk, which of all mixed bodies yields the most. This salt and volatile spirit, which is drawn from spider bags, is very active; as may be judged by the following experiments. It changes the tincture of the flowers of turnsole into a beautiful green emerald colour: it congeals, and reduces to a sort of snow, the solution of corrosive sublimate; whereas the volatile alcalis, drawn from the human skull, hartshorn, and some other mixed bodies, only render it white or milky. So that this new alkali, being prepared after the same manner as that which is drawn from the bags of silk-worms, in making the English drops, so famous over all Europe, may serve to make other new drops, which may deservedly be called drops of Montpellier; which we need not scruple to make use of, with much greater success than the old ones, in apoplexies, lethargies, and all soporous diseases, by reason of their great activity: and they will be taken with less reluctance, as their smell is less fetid and disagreeable.

Plate 12, fig. 13, shows the belly of a spider, with the anus and five papillæ, from whence the threads issue. Fig. 14, 15, the side, and fore-part of the penis of a spider, as magnified by a microscope. Fig. 16, the follicle or bag of a field-spider, with a harder shell, at the breaking of which the young spiders come out mixed with the silk. Fig. 17, the follicle or bag of a house-spider, with a softer shell, in which the young ones are inclosed. Fig. 18, a spider hanging on the branch of a tree, with its head turned against the wind, and spinning out its thread, until it finds that it adheres to some body, by which kind of bridge it passes over rivers, &c. Fig. 19, a spider having broken the first thread, by which it hung, and let out several others, is carried by the wind, and floats in the air with its legs extended.

The Moon's Eclipse, Feb. 2, 1710, observed at Streatham, near London, and compared with the Calculation. By the Rev. H. Cressener, M. A. and F. R. S. N° 325, p. 16.

In the last lunar eclipse, on Feb. 2, 1709-10, the time of the end I found to be the same very nearly, which the calculation, according to our most learned president's (Sir J. Newton) admirable theory, promised me to expect. There being therefore no examples of any calculation, that I know of, according to that theory, nor of the theory's agreement with observation, yet made public, I thought it proper to offer this one, that the exact agreement with observation in this, may prompt some of them to try the like in others. I have added the calculation from Mr. Flamsteed's tables, according to Horrox's theory, as published in Mr. Whiston's Astronomical Lectures, with the radixes of the mean motions, corrected according to their first author's later observations, which are the same with those assumed in Sir Isaac Newton's theory. By comparing these two calculations, we may observe, that though most of the additional equations in Sir Isaac Newton's be very small in this situation of the moon, yet they all conspire so as to make its place considerably more agreeable to observation, than those of Horrox's system.

The observation was made at Streatham, about 6 miles nearly direct south of London, with a very good 8-foot telescope. To correct the clock, for want of an instrument, I carried with me next day two watches; that were before adjusted to the clock, and compared them with Mr. Flamsteed's at the Royal Observatory, having first noted its error by an observation of the sun's transit of the meridian, communicated to me by his assistant: on my return, I found my watches still agreed together, and with my clock, which proved that they have gone true, and gave me the exact error of my clock, and the true time of observation. Mr. Flamsteed has since been pleased to acquaint me, that by his observation of the meridional transit of the lion's heart, during the eclipse, his clock needed a yet further correction of one minute, which I have here accounted for.

Feb. 1709-10.

The mean time of the mean opposition	2d.	4h.	9m.	42s.
The mean time of the true opposition	2	10	54	48
At which the true place of the sun is	10s	24°	55′	50″

And its equation to be added.

The Place of the Moon at this Time, from Sir Isaac Newton's Theory.

Mean motion of the moon....	$4^s\ 26^d\ 57'\ 37''$
Annual equation subtract	8 34
The correct mean motion	4 26 49 3
Mean motion of apogee	11 18 13 54
Annual equation of apogee add	14 31
Correct mean motion of apogee ..	11 18 28 25
Second eq. from the distance of ap. from sun add	2 57
Place of the moon the 2d time eq.	4 26 52 0
Mean motion of node	11 1 34 25
Equation of node subtract	6 54
Correct mean motion of node	11 1 27 31
The 3d eq. of the moon from nodes aspect with the sun subtract ...	10
Place of the moon the 3d time eq.	4 26 51 50
Second equation of apogee subtract	7 45 41
True place of apogee	11 10 42 44

Mean anomaly	$5^s\ 16^\circ\ 9'\ 6''$
Equation of centre subtract	1 53 31
Moon's place the 4th time equated	4 24 58 19
The variation, add	11
Moon's place the 5th time equated	4 24 58 30
The 6th equation from the distance of the luminaries and apogee, add	1 20
Moon's place the 6th time equated	4 24 59 50
The 7th equation add	34
True place of the moon in her orbit	4 25 0 24
True place of the sun	10 24 55 50
Moon beyond the opposition	4 34
Which divided by the horary motion of moon from sun, gives	7 42
	d h m s
The mean time therefore of op. Feb.	2 10 47 6
And the true time	2 10 32 20

The Place of the Moon at the same Time from the Tables in Mr. Whiston's Astronomy, according to Mr. Horrox's Theory.

Mean motion of the moon	$4^s\ 26^\circ\ 57'\ 37''$
Physical parts subtract	8 21
Correct mean motion	4 26 49 16
Mean motion of apogee	11 18 13 54
Equation of apogee subtract	7 25 0
Mean anomaly	5 16 0 22
Equation of the centre subtract ..	1 53 53
Place of the moon in her orbit ..	4 24 55 23
Distance from the opposition	27
That is in time to be added	45½
The mean time of true opposition is exactly	d h m s 2 10 55 33
The apparent time	2 10 40 41

Place of moon in ecliptic	$4^\circ 24' 57'' 27'''$
Reduction between the true opposition and middle of eclipse, add ..	2 47
	d h m s
Middle of eclipse	2 10 43 34
Continuance of eclipse	2 55 6
Digits eclipsed	9 55
Beginning of eclipse	2 9 16 1
End of eclipse	12 11 7
End of eclipse by the moon's place from Newton's theory	12 2 0
End by observation	12 1 30
End by calculation from Horrox's theory	12 11 8

The error therefore of Sir Isaac Newton's theory is, by this observation, only half a minute, or none; of Horrox's system, 9 minutes and a half.

Microscopical Observations on the crystallized Particles of Silver dissolved in Aquafortis. By M. Leuwenhoeck. N° 325, p. 20.

After having examined the coagulation of those particles found in aquafortis impregnated with silver, and shown that they had assumed the figure of so many rough diamonds; I separated the silver, and melted it, and then poured fresh aquafortis upon it, that I might once more discover the crystalline or adamantine figures of it; but in vain, for I could only now and then meet with one crystalline figure of the shape of a diamond.

For further satisfaction therefore, I took a piece of silver, being part of a Portuguese piece-of-eight, many years ago, and threw it into aquafortis, when the water was soon tinged with a green colour; from whence I inferred there was a great deal of copper in it. After the silver had lain eleven days in the aquafortis, I saw a great many long particles coagulated in it, which I judged to be hexangular, and as clear as crystal. I poured off this water very gently, that the long particles discovered by my microscope might remain in the glass; and then I poured upon them 4 times as much rain water as there had been aquafortis before in the same place, that the salt particles still in the glass might go over to the water: I then drew this water off again, and poured on fresh; and then viewing the particles through my microscope, observed them in great numbers sticking to the sides of the glass; but found, that those which were as clear as crystal before, had lost a great deal of their transparency, and assumed a pale red colour, which gradually became redder; and after some hours the colour was so deep, that it appeared blackish, having no manner of transparency; and where the particles lay thick together, they appeared to the naked eye like a whitish matter.

I likewise poured off very gently the aquafortis from another glass, in which were a great number of these long crystalline particles, and then turned the glass upside down, that the little water remaining in it might be drained out; by which means a great number of the said particles remained sticking to the sides of the glass; and as I had poured no rain water upon them, they preserved their transparency.

As often as I dissolved the silver in aquafortis, I could not discover any diamond-like figures worth speaking of, but only several very long particles, such as are represented, pl. 13, fig. 1, 2, 3, 4, 5, on which I suspected the aquafortis was not good; but was informed it was the same that was sold, not only to the gold and silversmiths, but also to the silk dyers. However, having suffered formerly, in endeavouring to separate gold from silver, by using aquafortis that had been drawn off from foul copperas and salt, (that it might be sold the cheaper,) I procured from a goldsmith some of his aquafortis, in which I dissolved not only my own silver, but also some that had been coined in England, which proved to my satisfaction; for I discovered therein as many particles of the form of bright diamonds, as in any of my foregoing observations; only with this difference, that the crystalline particles of the

English silver were not so transparent as the other; and that this silver, which is supposed to be allayed with a 12th part of copper, tinges the water of a very green colour, and leaves a great deal of dross at the bottom. I observed also in the same water, some few of the long crystalline particles, which had six sides, and ended in two points of the same figure, like the particles of rock crystal, only that most of them have but one hexangular point; the reason of which I suppose to be, that the other end is fastened to the rock. And as this appeared strange, I examined again some of those diamond crystalline particles, which were lying by me; and discovered among them, but in a very small number, some of the last mentioned figures, which are here represented in fig. 6. And I suppose, that all the mountain crystal would be of the same form, if the rocks and stones, among which it is found, did not prevent it, while soft. I also observed one six-sided figure, which only differed from the preceding, that one side of it was at least twice as broad as its opposite; by which means the hexangular point was askew, as it stands in fig. 7.

Fig. 8 represents also another small crystalline particle, which was of the same figure as the former in its sides, and at one end; but the other end was not hexangular, but of the shape of a hatchet. In the middle of the same figure, there appeared a break, or rent, which seemed as if it had been two distinct particles, joined together as they grew, till they became one body; yet still retaining the marks of their joining, as seen in the figure.

On the Manner of making Microscopes, &c. By Dr. Archibald Adams. N° 325, p. 24.

I think that all the microscopes which preceded Mr. Leuwenhoeck's, are so much outdone by his, that it will be proper only to take notice of these and the rest of later invention.

I had not an opportunity of examining Mr. Leuwenhoeck's glasses particularly, which is a favour he allows to none; therefore I am not capable of describing either their make or use, any further than that they appeared to be spherules lodged between two plates of gold or brass, in a hole whose diameter might not be larger than that of a small pin's head, and the objects I saw through them were pretty and diverting; but still their make and truth are unknown.

Mr. Butterfield is very curious in melting his glass, but I suppose unsuccessful in casting his spheres; for besides that a sufficient quantity of beaten glass cannot stick to the moistened point of a fine needle; so neither can it run equally, hold the needle how you will, nor the globule when run stick to the needle, but must unavoidably drop; and wherever it happens to fall, it must, in that almost liquid state, receive impressions sufficient to spoil the figure of a sphere.

Mr. Gray has shown the defect of his method, which he used to recover by grinding and polishing his glasses on a brass plane, and so reduce them to hemispherules; but how far short polished glasses (I speak of small ones) come of those which are cast, I leave to any one to judge who has seen both. His water and quicksilver microscopes I never saw, so can say little to them.

After what manner Mr. Wilson's glasses are made, I know not; but sure his greatest magnifiers are ill placed, being sunk to so great a distance from the eye, the object cannot appear to that advantage it otherwise would. If therefore, instead of a hollow cap, he would contrive a plain plate of any metal, for the reception of the glass, then the eye and the object might come to their due distance: neither ought there to be any calx or glass between the object and the spherule, when we use the greatest magnifiers, because if the focus of a sphere be at the extremity of its circumference, any small distance from that must spoil the truth of the object's appearance.

I cannot say, that the glasses I have made are without fault, but I think they magnify more than any I have yet seen; and were they placed to the best advantage, they would magnify much more than they do: they are made thus. I take a piece of fine window glass, and I raise it with a diamond into as many lengths as I think needful, not exceeding an eighth of an inch in breadth; then holding one of these lengths between the fore-finger and thumb of each hand, over a very fine flame, till the glass begin to soften, I draw it out till it be as fine as a hair and break: then inuring each of the ends into the purest part of the flame, I have two spheres presently, which I can make larger or smaller as I please: if they remain long in the flame, they will have spots, so I draw them out presently after they turn round. As for the stem, I break it off as near the ball as I can, and lodging the remainder of this stem between the plates, and by drilling the hole exactly round, all this protuberance is buried between the plates, and the microscope performs to admiration; so that the same thread of very fine muslin appeared 3 or 4 times larger in one of these, than it did in the first or second of Mr. Wilson's. I thought I saw animals in fine old brandy, but they were so nimble in their motion, that I can give no particular description of them. Human blood is so far from showing any red globules swimming in serum, that immediately after its emission it appears to be a body of infinite branches, running in no certain order, variously coloured; where it lies thickest on the glass, it is of a dull red; where thin, inclining to yellow; but the whole so blended, as to represent very nearly the top of a yew-tree in a very fine landscape, having its supposed branches of red and yellow confusedly intermixed.

On the Mischiefs arising from Swallowing Plum-Stones. By Dr. W. Holbrooke of Manchester. With Remarks on the same, by Dr. Wm. Cole. N° 325, p. 28.

One Crumbleholm came to me, and complained of a great loss of appetite, with scorbutic itch, and at times severe convulsive colics below his navel, all along the hypogastrium. They last not above a quarter of an hour, but often return, and raise tumours the size of a large walnut, which disappear and remove as the pain shifts. He has been troubled with it some years, and took physic of almost every one he met with; but not in any regular method. I began with mild, emollient, and carminative clysters; and purged with decoct. sen. gereon. syr. de spin. cervin. et tinct. sacr. In the intervals of the purges I gave Ethiops mineral, with bitter alterative decoctions, made more carminative with rad. zedoar. and castor. He was relieved for that time; his appetite and complexion mended, but presently he was as ill as ever. He then showed me the stones voided by stool, on a slight mercurial purge taken last Easter. On opening one of them, I found he had swallowed either some plum or apricot stones, which by their stay in the intestines seemed to be inclosed in the fæces, as I suppose; and being dislodged by the purge, came away. Hoping then by stronger evacuations if I could remove any others that might remain, it might tend to his cure, I ordered stronger medicines, but without any further success.

Dr. Cole on the foregoing case remarks, I suppose these stones to be formed not of adhering excrements, as you seem to think, but in this manner: when the plum-stones happen to be included in a fit glandulous receptacle, I conceive they may come to be thus coated over by the viscous liquor secreted out of the ducts of those glandules, which by long lying there may come to acquire so great a bulk, by the continual appulse of the same liquor. This receptacle I guess to be the cæcum, which, though naturally small, may be, as other membranous and glandulous parts are, capable of a considerable distension: so that, when by reason of the peristaltic motion of the intestines above, one of the plum-stones by its pointed extremity may happen to be intruded; the whole may, by the same repeated, though slow motion, dilate the cavity so, that the whole body of the stone may by the same method be still farther and farther protruded, till it come to the farther extremity; which being shut, must be presumed to detain it there, since it is hard to conceive how it can quickly get out again, the peristaltic motion being always forward. One of these stones being thus entered, it is easy to conceive how more may be admitted, since the first must dilate the passage for another that follows, and so on till the cavity be full. While these stones lie there, they must offend the part, having extended it beyond its natural state: so that the secretory ducts of the gland, of which the inner coat of that, as well as the rest of the intestines, is constituted, must be proportionally dilated; which by an easier way is made for the liquor, they separate, to be excreted. This being of a viscid and concrescible nature, must, since it cannot get forth, be presumed to adhere to the substratum, the stones, and so by degrees incrust them; which crust by long confinement must grow so much thicker, the ducts being kept constantly open, and the cavity more and more dilated, the greater the incrustation is. So that the symptoms of this patient are easily accounted for, from the offence given to the part, which being sensible, as all membranous and fibrous parts are, the pain must grow greater, in proportion as the distension is greater; and the change of the position of the tumour may very well be conceived to proceed from the different positions of the intestines, by the chyle or excrements passing along them, and sometimes filling one part, sometimes another, as they are protruded further and further, their lubricity on the surface, length, and confinement obviously favouring that phenomenon.

I am of opinion, that the true bezoar stones are formed in the animals, that yield them, in the same manner; but whether their stomachs or intestines have other cavities capable of receiving, and retaining them to their full growth, is to be determined by anatomy. This I think is certain, that they have either a straw, a stick, or other substance, different from the exterior matter, which we call the stone, in the middle of them; and thence I conclude the manner of their formation to be the same. From the continuance of the patient's symptoms, I believe there may be more behind; and cannot think any other method more likely to extrude them, than by having his abdomen well anointed with some emollient oils or liniments, and well agitated backward and forward, as much and as long as he can bear, and this both morning and evening: after a little while, that the stones may be presumed by this agitation to be somewhat dislodged, some gentle purgatives may be of use to be now and then given, to carry them downwards, and withal emollient clysters to solicit it gently, &c.

The Dissection of a Person, who died of an Ulcer in the Right Kidney. By Jas. Douglass, M.D. F.R.S. N° 325, p. 32.

On opening the body of a gentleman, who died the day before, in the 45th year of his age, I observed the following particulars, relating to the unusual structure and morbid disposition of the parts contained in the cavities of the thorax and abdomen. 1. When the skin with the other integuments were taken off, I observed that part of the omentum had thrust itself through the annular holes of the abdominal muscles on the left side, and there formed an epiplocele, or hernia omentalis, as large as a walnut. 2. The omentum reached as low down as the pubis and inside of the ilia, to which it was tied; and by fibrous connections it adhered to all the peritonæum below the navel. 3. All the fat on the omentum and guts was firm, and hard like tallow. 4. The intestines and stomach were quite empty, without either wind or fæces. 5. The left kidney was much larger than ordinary, being near 8 inches long; and its surface was divided into several distinct lobes, as in a fœtus. 6. The right kidney was full of a fetid purulent matter: all its inner substance was wholly wasted; and its external or cortical part was stretched so very thin, that a small touch of the finger could easily break through it. 7. All the fat and glands about this kidney were hard, obstructed, indurate, and large, which made a great compression on the musculus psoas, and the musculus quadratus lumborum. 8. The ureter proceeding from this right kidney, was covered with a crust or bed of indurate glands; and besides, its capacity was straitened and contracted in several places. 9. The cavity of the vesica urinaria was very small; its substance so very thick and hard, that I could not even by the help of a blow-pipe distend it any wider: its inside seemed excoriated with several little fleshy caruncles, or red excrescences, here and there. 10. There was a remarkable corrosion in all the inside of the urethra. 11. All the upper and convex part of the liver adhered firmly to the peritonæum that covers the diaphragm, and to the same membrane where it covers part of the musculus abdominis transversalis: its substance was so very tender and soft, that it seemed to be almost rotten. 12. The gall-bladder was extremely large and full; the bilious liquor it contained, being of a whitish yellow colour. 13. Between the tunica vaginalis and albuginea of the left testicle, there was a large hydatical or watery tumour; and on the last named coat of the same testicle, there were several chalky concretions, each about the size of a barley-corn. 14. In the right auricle of the heart there was a large polypus, that filled up its cavity; extending itself a great way into the ascending and descending trunks of the vena cava. All the rest of the viscera were as in a natural state.

The symptoms this person complained of during his illness, as I was informed by those who attended him, were, that about a year and half since, he began to decline in health. His first complaints were a heat, sharpness or pain in making water; constant desire to urine, though in great misery after. When the water stood a while, there appeared a greasy substance on its surface, not unlike the cream or ice found on the top of aqua calcis vivæ; sometime after, it deposited a purulent matter in great quantity, but without any offensive smell: the water when made was thick and whitish, but when the corruption settled to the bottom of the pot, it became clear. He seldom complained of any great pain in his back or loins; whence they concluded the ulcer was in the neck of the bladder, though the vast discharge of matter was an argument of the contrary: but he was always on the rack when he rose up after sitting, and it was a great difficulty to him to get up, which perhaps was occasioned by the weight and pressure of the kidney and adjacent indurate glands, lying on the head of the psoas muscle, and quadratus lumborum. He had often a total suppression of urine; but was much relieved by sal succini and corn. cervi. He took several doses of cantharides with camphire, without any ill effect from the fly, but had little relief as to his distemper. For 3 weeks past he was seized with a violent looseness, which at last, in spite of all means, carried him off.

An Account of a Book, intitled, Index Plantarum Horti Lugduno-Batavi, per Herman. Boerhaaven. Lugduni Batav.* 1710. *8vo. p.* 278. N° 325, p. 35.

The author, Dr. Boerhaave, has in this catalogue given the world an early specimen of his knowledge in botany, he being elected professor of that science

* This celebrated professor was born at Voorhout, near Leyden, in 1668. He applied himself at first to the study of divinity, which he afterwards relinquished for that of physic. He read lectures for some years on botany and chemistry, in the university of Leyden; and was afterwards elected professor of physic; in which situation he displayed such superior talents, that while he continued to fill the medical chair, Leyden was resorted to by students of all nations as the best school of physic. He died in 1738, aged 70; distinguished as much by his piety and the amiable qualities of the heart, as by his erudition, eloquence, and genius. The following are among his principal works: Institutiones Medicæ; Aphorismi de cognoscendis et curandis Morbis; Elementa Chemiæ; Methodus Studii Medici. Besides these, and his Prælectiones in proprias Institutiones, he wrote a number of academical Orations, and other small tracts on medical subjects, and published editions of Aretæus, Vesalius, Piso, Prosper Alpinus, &c. His Prælectiones, and Method. Studii Medici were edited by Haller, with considerable additions. His Aphorisms have been voluminously commented upon by Van Swieten.

Boerhaave was a great admirer of the ancients, the study of whose works he strongly recommended to all his pupils. In his Aphorisms he imitated the brevity and simplicity of Hippocrates. He held Aretæus next in estimation after Hippocrates; and among modern practical writers he gave the preference to Sydenham. He was doubtless too much addicted to the humoural pathology; not perceiving that almost all the various forms of disease may be referred to altered impressions made upon, or altered actions produced in, the cerebral, fibrous and vascular systems; that the chemical changes (seldom ascertainable during life) which take place in the blood and other fluids, are the consequence of altered actions in the aforesaid solid parts; and therefore that in the treatment of the various at Leyden but the spring before this publication, on the death of Dr. Hotton, who succeeded the celebrated botanist Dr. Paul Herman, whose catalogue of that garden, with his Prodromus and Paradisus Batavus, are living testimonies of his immortal fame. This curious botanist, to repair his loss, obliges us here with a methodical index of all such plants as actually grew in that garden the last year, that the world may see what he can furnish them, as well as what he wants; so that by a mutual commutation each may improve his stock.

The Anatomy and Osteology of an Elephant; being an exact Description of all the Bones of the Elephant which died near Dundee, April the 27th, 1706, *with their several Dimensions. By Mr. Patrick Blair, Surgeon, &c.* N° 326, p. 53.

The elephant is said to live to a great age: some asserting they live to 120 years; others to 200 years; some to 300; and some even affirm that they can live till they be 500 years old, and that they are very strong and robust at 200. Tentzelius tells us, that when a certain German, who had sometimes been in the Indies, saw those bones he treats of, he concluded from certain marks the Indians have, that that elephant could not have been under 200 years old. Mr. Tavernier says, he could never learn exactly how long the elephants lived; but that their keepers have told him, they knew such an elephant to have been in their great grandfather's, grandfather's, and father's custody, which he modestly computes not to have been under 120 or 130 years: and it is memorable what Juba king of Lybia told, as it is related by Philostratus, that the knights of Lybia at a certain time fought upon elephants, some of which had a tower engraven on their teeth; and when they were separated by the night, such as had the tower were beaten, and fled to mount Atlas; and that Juba 400 years after took one of them, which had this ensign so lively engraven, as if it had been but lately done. However this may be, they seem generally to live to a great age; for the keeper told me, that the elephant which fell in our way was

diseases to which the living frame is subject, the curative indications are to be drawn, not from the supposed changes in the quality or composition of the fluids, but from the kind and degree of action induced in the solids; from the increase or diminution of the animal heat; and from the excess or deficiency of the excretory discharges. But whatever objections may be made to his pathology, his histories of diseases cannot be too much admired; and even some of his curative indications, especially on the subject of inflammatory diseases, may be safely recommended. In his Treatise de Viribus Medicamentorum, he gave an improved arrangement of the Mat. Medica; which he further illustrated by his lectures. He was well acquainted with botany; but his favourite pursuit (next to the proper duties of his office as professor of physic) was chemistry; his treatise on which is remarkable for the great body of facts which it contains, and for the order and perspicuity with which they are arranged and described.

26 or 28 years old; and yet she seems to have been young, according to their term of life, for the epiphyses separated from the bones by boiling, as easily as those of the human subject would have done at the age of 10 or 12. And yet it is an animal subject to many distempers; so that though they may live to some of the fore-mentioned ages, yet mostly they die before they come to such a length.

It is an animal of considerable size; but whether ever so large as to contain 32 strong men upon its back, as is related, Maccab. ch. vi. v. 37, besides the Indian that governed it, is much to be doubted; and it is more probable that this is an error in the impression. Indeed Philostratus speaks of 10 or 15 Indians fighting with darts in castles on elephants' backs: and Paulus Vineta says, that in the Ginger islands they have wooden castles on elephants' backs, which can contain 15 or 20 men. But I rather believe what Heliodorus says, that the towers on the elephants' backs could contain 6 fighting men, who from each side threw darts, the hinder part remaining void; or Cadamustus, that they put towers upon the back, which can hold 3 or 4 men that fight upon them; and Ælianus, that they carry 3 warriors fighting from either side, and the 4th which governs them. Which last 3 accounts seem very well to agree with the usual height ascribed to them: about which some authors, talking more largely, tell us of 16 or 18 feet high; but the most received account is, that they are from 8 to 13 feet high.

As to their manner of procreation, all authors agree that it is an animal of extraordinary modesty, and therefore never copulates in view of any one. As to the posture, some assert that it is retrocoient and retromingent; among whom is Dr. Moulins, from an observation he has made of the situation and structure of the penis. Others observing the distance between the anus and vagina, and that the dugs are situated between the fore limbs, are of opinion, that the female is in a supine, and the male in a prone posture: among whom is Tavernier, who tells us that the female gathers a great deal of herbs and weeds, and makes her a bed 4 or 5 feet high from the ground, where she throws herself, and lies on her back in expectation of the male, whom she invites by a peculiar cry; and that may be the reason why the dugs are placed so forward, to avoid the pressure. A third opinion is, that the female descends into a ditch, and that the coitus is not different from that of other quadrupeds. As to the first, I can scarcely believe it probable, because there can be no such thing as a retrocoient animal; for that would quite invert the order of nature, and give a far different motion to the muscles of the thighs, than they can be supposed to have from their situation. As to the second opinion, were it not for Tavernier's assertion, I should think it too unwieldy an animal, and too little inclined to lie down, to acquire such a posture. The third opinion is, that the natural sagacity of the animal disposes the female to go into the ditch; and both fore and hind legs seem to be so articulated as to favour this: for when the female would lower her body, she has only to stretch forward her fore feet, and then the articulation of the humerus with the cubitus will bend backward; and then to bring back her hind feet, so as to bend the knees forward, by which she can bring the fore part of the body so low, as to make the nates protuberant, and bend the hind legs, thereby to put the vagina in a convenient position for reception of the penis, according to that of Aristotle, subsistit fæmina, clunibusque submissis, insistit pedibus ac innititur; and elsewhere, flectit certe suos posteriores poplites modo hominis.*

The natural food of the elephant is grass, vegetables, leaves, and twigs of trees, &c. and when these are wanting, they dig up roots with their tusks. They are very fond of cucumbers and melons, and possess a particular instinct in avoiding whatever herbs may be hurtful to them. They never approach any grass that has been trampled on by men, for fear of snares. When they are tamed, they eat hay, oats, barley, or such other food as oxen and horses. The elephant drinks a great quantity of water, which it sucks up by the trunk, and when that is full, it empties it in the mouth. It naturally affects muddy water, rather than clear: when tame, it drinks clear water well enough. When they are to go to battle, they give them spirituous liquors, such as wine, &c. in order to make them drunk and furious, as appears from the history in the third book of Maccabees, chap. vi. It has a very acute sense of smelling, by which it readily finds out its food. It was pleasant to observe, that when people came to see our elephant with apples in their pockets, it pulled them out, to the astonishment of those who had them.

That the elephant is animal vastissimum, I shall readily acknowledge with Franzius; but cannot allow that it is deformed, since those due proportions, laid down by the Author of nature, are as well observed in this, as in any other animal; for nothing can be deformed, but what swerves from a general rule. The elephant has a thick short head, short neck, long nose, or proboscis, hanging almost to the ground; a back somewhat protuberant, a short and round body, a long tail, four thick round legs, like so many columns supporting his vast weight; and short feet, those before being broader and rounder, and those behind more long and narrow, each shod with 4 hoofs; a small narrow

* The mode of copulation of the male and female elephant is described in Captain Percival's "Account of the Island of Ceylon," 2d edition, page 293, on the authority of a respectable eye-witness.

mouth, with 2 long tusks, proeeeding from the upper jaw, one on each side of the proboscis; 4 strong grinders in each jaw; small, yet piercing eyes; and large flat ears.

As to our particular female elephant, which is the subject of this dissection, being comparatively but young, 26 or 28 years, she may be presumed of a middle size. Pl. 13, fig. 9, represents her stuffed skin, the dimensions of which are as follow; at the fore leg she was 8½ feet high, AA, and 9 at the hind part, BB; in length 10 feet, CC; and a tail 4 feet 3 inches long, CD; round the belly 14 feet, EE; from the top of the head to the end of the proboscis 8 feet, FF, of which the proboscis makes 4½ feet, FG: from the forehead even with the eye, to the lower jaw, measuring backwards, 27 inches, HH; from the top of the head to the lower jaw, measuring downward, 4½ feet, FI; the ear was almost square in this subject, and small in respect of those in other animals. Whether this difference might have been owing to the sex, I know not: it was in length 19 inches, KK; and in breadth 17, LL. The eye, V, was not so small as Dr. Moulins would have it; who makes it no larger than of a sheep; whereas in this subject it is larger than those of an ox. The distance between them, measuring across, was 26 inches; between the anus and vagina 2½ feet, between the dugs 1 foot. The fore feet, measuring round the extremities of the 4 hoofs, 3 feet 10½ inches, NN, of which the external hoof running obliquely forward, was 5 inches: the second on the outside, square before, was 5 inches, and 6 in breadth, i. e. up toward the skin; as was the third, square also before, and 4½, c. The internal was more pointed than the external, and of the same length; the hind part of the foot was covered with a tough thick skin: the diameter of the fore foot, from the right to the left, was 14½ inches; from before to behind, 16½ inches. The circumference of the fore leg, at the upper joint, was 4 feet 3 inches, OO; at the articulation of the carpus 2 feet 6½ inches, PP; the circumference of the hind foot, round the hoof, 3 feet 4 inches, QQ; its diameter from behind to before, 16½ inches; from the right to the left, 12 inches. The breadth of the outer hoof, 4½ inches, b; the fore hoof being semicircular, 3½ inches, a; the third and fourth hoof 4 inches each; both inner and outer hoof go obliquely forward. The circumference of the hind leg is 2 feet 2 inches, RR. Thus you have the dimensions of all its external parts, taken either from the body, when it lay dead in the field, or since from the stuffed skin, wherein for the most part they agree; only that by reason of drying, the legs are smaller, and the back not so protuberant.

As to the cuticula and cutis, Dr. Moulins has already at large judiciously described both. He says, he found the cuticula covered all over with a strange sort of scab, in many places resembling old warts, deeply jagged, and the carneous fibres of the muscles of beef when much boiled and transversely cut, but of a dirty tawny colour. These scabs, if they may be so called, both slit and look like short pieces of whale-bone; they adhered so firmly to the cuticula, that they could not be plucked from it, nor the parts of which they consisted (though they were much divided) from one another, without tearing it, and yet the cuticula was very tough and thick. This is very well expressed, and answers exactly to what I find in this subject. He goes on, and says: The length of these scabs in some places was above $\frac{1}{3}$ or $\frac{1}{4}$, but in other places not above $\frac{1}{10}$ or $\frac{1}{12}$ of an inch. The cause of which difference, he takes to be the elephant's wearing, by rubbing or lying, some parts of them, while others were slightly, or not at all worn. The scabs of our subject were not so long; for as the deepest I could find upon the cuticula was not above $\frac{1}{6}$, so the thinnest was less than $\frac{1}{16}$ of an inch. He says, he could find but very few hairs without this scab, but many within, and even with it. The elephant's inclination to itch, and to rub himself against whatever came in his way, kept those hairs that were even with the outside of the scab, from appearing of any considerable length. The hardness of the scab, by keeping the roots of the hairs fast, very much contributed to their wearing on the outside, as well as to their preservation on that within. In our subject the hairs are every where pretty long, some 2, some 3 inches; others, in places most subject to rubbing, but 1 or $\frac{1}{2}$ inch, though indeed not so numerous as I find; and there are passages for them through the cuticula. I know not what the doctor means by distinguishing between those found in the cutis, and those in the cuticula, since I am convinced that all arise from the cutis, and penetrate the cuticula. They are indeed black, and many of them stiffer and thicker than the bristles in a hog. Some have taken the cutis to be nothing but a certain crust, formed of several mucilaginous particles, covering the cutis, &c. in the uterus; which after the fœtus is come to maturity, is condensed and formed into a skin, such as we see mucilages and poultices have, when after boiling they are exposed to the air: others suppose that the cuticula, as well as cutis, consists of a congeries of membranous fibres, intermixed with a great many capillaries, and endued with pores for perspiration: and some anatomists assert, that they have injected these cutaneous vessels in the cuticula of a fœtus, as well as in the cutis: though when the animal is more adult, these capillaries escape the view not only of the naked eye, but even the microscope. That this has been the structure of the cuticula in this animal, is most plain and obvious; for though I cannot determine its thickness, as Dr. Moulins might have done in a recent one, yet now as it is dry, it seems to be of the thickness of, or rather thicker than, common vellum, with its inner surface excavated, or pitted, like a thimble, or in a honey-comb. Among the interstices of these excavations, the ramifications and divarications of the blood vessels are obvious. At every two lines or $\frac{1}{6}$ of an inch distance, for the most part are observed protuberances, composed of 5, 6, or 7 columns, joining and forming a pyramid or cone; in the top of which is the pore or ductus, mentioned by Dr. Moulins, through which the hairs pass; they are nothing but the interstices of the favi, or depressions arising in the cuticula, and are impacted in the cutis, for the better reception of the hair.

To the outside of this cuticula adhere the scabs, which I rather take to be a supervenient distemper incident to this animal, when out of its own climate, occasioned by the constriction of the pores from cold, than anywise natural to it. Authors tell us, that the first thing they do when they begin to tame them, is to anoint them with oil, by which they keep their skin smooth, soft, and flexible, and relax their pores so, that whatever gross particles may fly of from their blood, whose constitution is now perhaps worse by the alteration of diet, and the hardships they undergo at taming, may not stick to the skin, but be freely evaporated. And I am credibly informed by such as have lived long in the Indies, that they take as much care to keep the skins of the elephants smooth and clear, as we do with our fine horses. So that it is probable these scabs are a disease, and not natural to the animal.

As to the cutis, Dr. Moulins observes, that its inner surface abounds with a great many glands; when cut through, at least as far as the roots of the hair went, it was like the horny or callous part of brawn, and its outer surface abounded with a great many papillæ. I can determine nothing about the thickness of the skin, while recent; but as it is dry, by an incision made on one of the hips, it appears to be less than $\frac{1}{4}$ inch, and of substance not unlike English bend, or sole-leather. I had no opportunity to observe, whether there were any cutaneous vessels, but doubt not but there have been of them, and that in abundance; 1. from the numerous glands dispersed all over its inner surface, which must have had blood-vessels inserted in them; and, 2. from the abundance of ramifications dispersed in the cuticula, proportionable to which, it is probable, they were also in the cutis.

As to the fat, whether by reason of the extraordinary leanness of this subject, or if it be ordinary for elephants to be endued but with little of it, I know not, but I could not have believed so little fat to have been in any animal as was here; for beside that there was neither a membrana adiposa, nor conspicuous omentum, there was not one grain of fat, either among the interstices of the muscles surrounding the kidneys, nor round the anus and vagina, where it is usually found; and when I had spent near a whole day in boiling the bones in a dyer's vessel, without changing the water, except that I supplied what was evaporated, there was not so much as a drop of oil that floated on the liquor. Dr. Moulins takes notice of a very strong nervous membrane, which I confess I had not time to remark, which obliquely descended from the spina dorsi to the sternum and linea alba.

After the skin was wholly removed, there being no time to examine all the muscles of this huge body, I applied myself particularly to those of the proboscis, as being of greatest moment. Wherefore the body being supine, I first considered the neck, and the upper or fore part of the sternum, where I observed two pair of muscles to arise sharp and fleshy; whereof two in the middle, from a small origin, were extended into large muscles, running straight forward, and distinguished from each other by a white line, till they came to the point of the lower jaw, their other side running obliquely outward, till they came over against the articulation of the lower jaw with the upper; from thence keeping the lower part of the lower jaw, they returned to the foresaid point, in figure not unlike the cucullaris in human subjects, with their fibres running obliquely forward from this middle line toward their external part. This pair served to draw back the lower jaw, and like the platysma myoides, covered all its other muscles with those of the larynx, tongue, and pharynx. On the outside of this pair arose two other muscles, small at their beginning, and in their progress passing in between the os zygomaticum and the skull, adhering to the musculus temporalis, and ascending run up below the meatus auditorius, half way between the orbit of the eye and the top of the head; where becoming very thick and round, it passed over a sharp angle of the skull toward the forehead; whence descending from above the eye, it came, and with its partner filled up that hollowness in the os palati (k, pl. 13, fig. 11) and coming still lower, formed the back part of the trunk or proboscis. Afterwards, the body being turned over, I had opportunity to see the tax-wax mentioned by Dr. Moulins; which arises from a spina in the back part of the skull (c, pl. 14, fig. 1) whence running backward along the sides of the seven vertebræ of the neck, it terminated between the sixth and seventh vertebræ of the back, becoming still thinner in its progress. It was about 6 inches broad, pretty thick, and descended obliquely from the top of the spina vertebrarum to above the ribs, and covered all the muscles which arise from the neck and support the head; assisting them, as Dr. Moulins rightly observes, because the heads of quadrupeds, especially of this animal, being more pendent, have more need of supporters than the head of a man, where this contrivance is wanting. Dr. Moulins tells us, that it was placed edgewise, the reason of which may be, because of the spines of the first four vertebræ of the back, which are 4 inches broad; whence the tax-wax, running forward (where the spines are narrow, or where there are no spines at all, as in the first three vertebræ of the neck) in a straight line to the skull, the space below it for the muscles to move in must be the same at the neck as at the spina, where the epiphyses keep their upper sides at such a distance. Above this tax-wax in the neck arise two muscles, thinner and narrower at first, but thicker and broader as they go to the skull, where they firmly adhere to the sides of a large sinus in its back part (b, fig. 1) whence ascending, being lodged in the depression on the top of the head, and between the eminences (dd) they descend till they come over against the hole for the root of the trunk, a, and become thicker and round, and in their whole descent form the fore part of the trunk with the extremity.

Thus then the proboscis is composed of two pair of muscles, one pair forming its back part, which arises from the sternum, and passes with straight fibres in below the os zygomaticum; and from thence forward, till it forms the body of the trunk itself: another pair which, arising from the neck, pass over the head, and descending forms its fore part. The fibres of this muscle descend in a straight line, till they form the body of the trunk, and then begins a strong tendinous interstice, by which they are separated from their copartners; whence their fibres descend obliquely to another strong interstice, by which on each side they are separated from their antagonist, where the same oblique course of fibres is again to be observed, that is, that the erectores proboscidis (for so we may call these which make up the fore part of the proboscis) (gg, fig. 22) unite in a tendinous interstice, h, from whence the fibres on each side obliquely descend: so likewise the retractores proboscidis, for so we may call those which form the back part of the proboscis, have their tendinous interstices running down the middle of its back part; from whence the fibres obliquely descend, almost making an angle of a demi-rhombus on each side in another longitudinal tendinous interstice, by which the fibres of the antagonist muscles are conjoined. Thus we see a wonderful contexture of four muscles, so contrived as to perform all kinds of motion.

These muscles surround two large cavities, about the middle of the proboscis, 2 inches diameter from the right to the left, and 3 inches, each from above to below; for as they proceed from the skull they are very wide, according to the capacity of the hole in the fore part of the skull, whence the proboscis proceeds. They are divided by a strong cartilaginous septum, which runs straight from before to behind, along the middle of the proboscis; and into this septum the muscles situated in the fore and back part are inserted: these holes are cartilaginous, all round obduced with several nerves, and endued with a great many glands for separating a certain mucus, with which the inner surface is always bedewed, to keep it moist, and preserve it from the injuries of the air in breathing. These two cavities are of great use, for they draw up and contain as much water as serves the animal at once, which afterward it empties into the mouth, as it were from a tunnel: they serve also for breathing, smelling, and uttering the voice. The proboscis is not equally large throughout, but from 38 inches in circumference at the beginning, it becomes gradually smaller, till it be 20 inches at the middle, and at the extremity 11 inches. It has a hollow cartilage, where these passages terminate; round this is a cartilaginous margin, which extends itself 1½ inch before, and terminates in a point, and behind it has, as it were, a hollowness, where this point fixes itself, and takes hold of any thing, as it were a thumb passing in between two fingers; this cartilage is of great strength, and by it the elephant can take up any great weight.

In searching for the origin of the proboscis, and how it proceeds from the head, I separated the relevatores proboscidis, below which I observed four considerable blood-vessels, a vein and an artery from each side, lying upon and descending in a straight line above the before-mentioned cartilages, and dispersing their branches every way throughout the substance of the muscles, with two large nerves accompanying them. It is observable, both in human subjects and quadrupeds, that there is a hole below the orbit of the eye in the upper jawbone, through which the superior branch of the second division of the fifth pair of nerves passes, in its progress surrounding a vein and an artery; all which are dispersed in the muscles of the cheeks, lips, and nose, and furnish branches for the roots of the teeth of the upper jaw. This hole is not so considerable in human subjects, but larger in quadrupeds, especially such as feed upon grass or hay; insomuch, that by the size of this branch of the fifth pair in an ox or hart, we may reasonably conjecture that they have a partial taste, and a most acute smelling by the upper lip, the better to enable them to choose their food: for in dissecting a calf's head, we perceive both this nerve and the blood-vessels much larger than what might be thought requisite for furnishing either blood or spirits to this part, were there not some extraordinary use for both. Now in this our subject there is a hole in the upper jaw-bone, represented pl. 13, by rr, fig. 11, by m, fig. 12, and by 8 8, fig. 13, so remarkable for its size, and so commodiously situated, and so well guarded, that there is good reason to believe it may be designed for transmission of the afore-mentioned artery, vein, and nerve, and that all these are distributed into the trunk. I cannot positively determine the capacity of these blood-vessels at the root of the proboscis, but they were very conspicuous, and could admit of a goose-quill, though they were empty, and when they were full, I doubt not but they were above twice as large.

This extraordinary part did not want for nerves sufficient for it, no more than blood-vessels: for first it has the nervus olfactorius, by which it is endued with a most acute sense of smelling. Secondly, the aforesaid second branch of the fifth pair, which accompanying the blood-vessels, is with them dispersed throughout the whole substance of the proboscis; by which it has so acute a sensation of touching or feeling, with which this member is more signally endued, and by which it avoids whatever is hurtful to it, as appears by that memorable instance of Dr. Moulins, who tells us, that such was the care, in that subject he treats of, which was burnt, for the proboscis, that it thrust it 2 feet into hard ground, to preserve it from the fire. Thirdly, the hard portion of the nervus auditorius, which though it be dispersed in the muscles of the face in human subjects, yet in quadrupeds, such as oxen, it continues undivided, till it comes to the angle of the lips, and here we traced it a good way, running forward above the temporal muscle, a little below the ear, till it came to the upper lip, whence it proceeded to the fore-mentioned tendinous interstice, which runs down on each side of the proboscis, dispersing a branch to each of the fasciculi of fibres already named. The head was so mangled in taking off, that we could not well find its origin, as it proceeded from the proper hole; but its situation here, analogous to that in other quadrupeds, removes the suspicion of its being any other than the hard portion. It was indeed very pleasant to behold it, for several physicians and surgeons of us being together, we cut off a portion of it to know its structure, how that several small fibres were knit together into one bundle; and how several of these again were involved by common membranulæ into different fasciculi, till at length all were included in one common tunicle.

Thus we see how signally this member is endued with instruments for the performance of its different functions. It is the principal seat of two of the senses, and partially partakes of the third; for by it the animal smelled: by it feeling is performed, as by the hands with us, whence the proboscis is not improperly called manus nasuta; and by it the fifth pair of nerves affords a partial idea of the taste, to what food it takes hold of, before it conveys it to the mouth; and it has a great analogy to the two other senses, viz. to the eye, by its three pair of nerves, namely, one for its seeing, analogous to the other for smelling, one for its pathetical motions, analogous to the acute sensation, afforded to the other by the fifth pair, and one for the motion of its other muscles, analogous to the hard portion of the other: and to the tongue, by its different motions, and by its partial taste.

Having opened the abdomen, and removed the intestines, I extracted the uterus and bladder, because the partes generationi inservientes are the most noticed in comparative anatomy. I could not get the vasa præparantia preserved, and only got out the uterus itself, with the cornua, ovaria, and part of the ligamenta lata. It is not unlike the uterus of such animals as bring forth several at one litter; for on inflating it, I perceived several protuberances arise, as if they had been so many cellules, such as bitches, cats, hares, &c. have, for containing the several fœtuses, with their proper placentæ and involucra; which might have induced me to believe they bring forth more than one at a time, had not authors affirmed the contrary. For whereas the uterus of such animals as bring forth only one at a time, is proportionably large, and the cornua small; here the body of the uterus was so small, that one would think it only a bivium to the two cornua; for after the tube had passed the corona, which is pretty strong and close, I observed the cornua to swell on every side by inflation, leaving a a sulcus in the middle, and these different protuberances to arise with depressions, as so many interstices between them. Each of the ovaria was the size of a large apple, with the ova fitly distinguished by their proper membranes, being for the most part about the size of a small pea, and all involved within a common thin and pellucid tunicle, through which they shone; but to defend them there was provided a loose thick wrinkled tunicle, which I could remove at pleasure, it nowise adhering to the ovaria, but fluctuated above them, and proceeded from the cornua. I opened one or two of these ova, and found them filled with a thin limpid substance, not unlike hydatides, but that the humour was more viscous. The extremities of the cornua, which received the ova, were very narrow, for when I had inflated the uterus, it retained the air for some time, without passing immediately out by the cornua; though afterwards, when I had strictly tied the vagina, I observed the air insensibly slide out, and now and then I could see small bullulæ arise toward the ovaria. The vagina was very small and narrow, not admitting above two or three fingers: its inner surface was whitish, and moistened by a certain kind of mucus, and all full of plicæ or wrinkles.

The bladder is rounder than that of an ox, and much larger than Dr. Moulins would have it; for he says, it is much about the size of an ox bladder, but I find, when gently inflated, it can contain 6 or 7 English gallons. It is very strong, and the vessels appear very prettily dispersed through the tunicles, which I could have easily separated, but did not design to lose it. The ureters were about half an inch diameter, and I could have easily discovered their insertion, if I had not designed to preserve the bladder. Both uterus and bladder were involved within a duplicature of the peritonæum, so that I had much ado to get them separated.

Having given an account of the parts of generation in the female, I shall add Dr. Moulins' account of those in the male, with my own thoughts about them. In searching for the testes, he found two muscles very like them, which he supposed to have been them, till he had traced them to the inner and lower side of the ischion, where he found them implanted; he traced the tendons likewise, and found, that when they had gone singly near 4 inches, they joined in one, which went directly under the middle of the penis, and reached beyond a crookedness he observed in it. This was in length about 8 inches, and terminated within 6 or 7 inches of the glans, having expanded itself into a membrane. There was beside these a nervous body, that began underneath near the aforesaid tendons, about 8 inches from the root of the penis, and reached (distinct from the yard) 9 inches, before it was inserted again in it, at a place 5½ inches from the glans. He is of opinion, these muscles in that nervous body being so conveniently placed for that purpose, that the elephant is a retromingent, and probably retrocoient animal; which I cannot agree to.

The testes, he says, were not contained in a scrotum or capsula, but lay in the perinæum, close joined on each side to the penis. They were neither of the usual shape nor size, nor included in a processus of the peritonæum. Their shape was very like that of a chesnut, thicker on the side that grew to the penis than on the opposite; they were flat and round, and not suitable to the other parts of the body, being no more than about 3 or 4 ounces in weight. They were joined to the penis by a great many, at least 100 seminal tubes, which may be properly called vasa deferentia, and which deposited the elaborated semen in several rhomboid cells, placed in the body of the penis, which in this creature was the common and only repository, where the seed could be found. These cells were turgid with sperm, and so were the tubes; the latter were very large, but being pursued further into the body of the testes, they became smaller and smaller till they disappeared. The blood came into the testes by the vasa deferentia.

Though these were small and disproportionable, yet he took them to be the testes, nothing else outwardly appearing that contained seminary vessels, till he understood by Dr. Needham, that his description of the testes of the elephant agreed with the prostatæ of a bear: on which he mistook the testes for the prostatæ, there being a great resemblance between these animals; and having found two substances between the kidneys and neck of the bladder, which might very well be testes, and which, till he discoursed with that gentleman, he did not know what to make of. He then proceeds: The venæ præparantes were large: he divided that which was inserted into the emulgent lengthwise; and within a little more than an inch of its insertion he found many valves, to the number of about 8 or 10, of divers shapes, all fitted to prevent the return of the blood into the variously divided spermatic vein, which here from 8 or 10 rivulets became one great channel. Within about an inch of this, and somewhat more than 2 from the kidneys, he found a substance of the shape of a pear, but near three times the size of a very large one. He was at a loss to know what this might be, and confesses he can give but an imperfect account of it, since the butchers cut it out, and so its continuation with the testes, penis, and other parts, could not be discovered. What he observed in it was, that the spermatic vessels entered but a little way into this substance; but below the middle of it he found them more deeply placed, and their branches grew so small, and less numerous to the sight, as if here the veins began. The inner part of this substance looked of a palish, but somewhat muddy red colour. It was very spongy, not much more compact than the lungs of young animals. He doubts not but this substance was designed to prepare the semen; but by what vessels it was brought to the penis or any other repository, (itself containing none) he could not discover; neither could he find any peculiar vessel, or ductus, or any thing that resembled that beforementioned substance, by which he might be directed in his enquiry. It lies lengthwise from the kidney to the testes, with the larger end lowest. He is of opinion, from what he has heard from Dr. Needham of these parts, that these two pear-shaped substances, were testes; their place, size, figure, and occasional cutting the vasa deferentia, being the occasion of his former ignorance in this point.

He could observe no vesiculæ seminales, nor any common receptacle for the semen, except the formerly mentioned rhomboid cells in the penis itself; but doubted not there might be some still, though his being intent upon other things made him neglect the discovery of them. Thus he confesses his mistake of the testes twice, and he leaves them in doubt the third time.

Both the vena cava and portæ were very large, and had their exit and entrance in the concave part of the liver. This had only one lobe; but both veins dispersed themselves, first into two large branches, and then were subdivided there, as in the ordinary manner. I opened several, and found them differ in nothing from other animals; the substance being firm, as is usual, and glands large and conspicuous; the external surface smooth, and its proper tunicle firmly adhering to the glands: it was 36 inches in length, and 22 at the broadest part.

The bile, by Dr. Moulins' account, was deposited at the end of the first gut, 4½ inches below the pylorus; from whence he traced the ductus communis to the liver, to see the vesicula fellea; but it was wanting, and instead of it he found the porus biliarius coming out of the liver, as the ductus hepaticus usually does. He observed likewise, that the bile found in that, differed both in colour and consistence from that he found in the ductus hepaticus; for the latter was of a clear light yellow colour, congealed like a jelly, and the former of a dark green, and somewhat more fluid than the gall of an ox. He hopes time will discover such a difference in the galls of most animals, and that discerning men will be excited to find out their uses.

The pancreas is very long and large; for it reached from about the middle of the stomach to the jejunum, which space could not be less than 6 feet. It was a glandula conglomerata, as the pancreas always is, and had its ductus so wide, that it could easily contain ones little finger. It opened into the gut, where the ductus felleus did.

The spleen was 3½ feet long: on the backside its edge was somewhat curved, almost in shape of an unbended bow: on the fore-side, from a narrow point at each end, it enlarged itself by degrees, till it came towards the middle, where the vessels entered, where it was broadest. It was in breadth from 3 inches toward the extremities, to 8 inches about the middle.

The glandulæ renales were placed after the usual manner: they were about 5 inches long, 2 inches broad, and oval, with a loose outer coat, which I removed, as it had been a sheath; within which was contained the gland itself, being divided into several lobes, like the kidney of an ox; from whose interstices there passed several thin membranes, which passing to the loose vagina, kept it fast; and by which this vagina was only coherent with it.

The kidneys were of a large and proportionable size, being one foot in length and ½ foot in breadth, of the usual figure, much like that of a man; their external surface smooth, and equal with their external coat, closely adherent to the inner substance, without any perspicuous lobes to be seen externally; but when I opened one of them, I perceived 6 large carunculi urinarii. Its substance was very obvious, and correspondent to the structure usually observed in the kidneys; i. e. the glandulous substance externally was very conspicuous, for the space of about ½ inch in circumference; then began to appear the tubuli urinarii, first smaller and less obvious, then another series larger, and a third still larger, till they began to surround each of the carunculi, like so many rays of the sun.

In the thorax there was scarcely any thing remarkable. The viscera were large and strong. One of the lobes of the lungs was opened by the butchers, and the other had nothing observable, but its size, which was proportionable enough.

It did not adhere to the ribs, as in Dr. Moulins' subject; but lay flaccid on the one side of the heart, as the other had done, before it was mangled, on the other side; so that I suppose this adhesion of Dr. Moulins' to have been in a morbid state. When I saw I could not extract the viscera thoracis whole, I traced one of the branches of the aorta ascendens down to the heart; and was surprised, when I cut it above, to see a fat-like substance jet out of it; and pulling it, I got upwards of 2 feet in length of a polypus adapted to the capacity of the artery, which was about 2¼ inches diameter. This polypus was no wise fibrous, but as it were so much fat moulded after such a manner, being not unlike the blade of a broad sword, near ¼ of an inch at the middle, and much thinner at the edges, tough and flexible, with some grumous blood not so firmly compacted at the extremity.

About the heart all the vessels were very large; the bivium aorta very considerably thick and strong; the size proportionable to the body. The auricles were large, and the left as well as the right full of grumous blood. At the opening of the ventricles, I found them both filled with the same polypus; which strangely twisted itself in among the valves, both tricuspides and semilunares, and also among the fleshy columns at the bottom of each ventricle; which here seemed to be so many little strong round muscles, some ¼, others ½, and others near one inch long, with a round fleshy belly, and two tendons variously situated, as in the hearts of other animals. These polypuses, from a massy substance in the middle of the ventricle, sent forth to all parts their branches, which here and there twisted themselves round these fleshy columns, their tendinous insertions, and the tendinous fibres of the valves, with a surprising intricacy: in short, there was no angle, no corner or cavity, which the polypus did not occupy: and yet so much was it disengaged from the substance of the heart, and it was so strong and tough, that by pulling its grosser part in the middle, all the other branches moved; and by cutting a few parts of it, where it was most engaged, and where the fleshy columns were thickest, I got it out altogether; and having stretched it out, I pleasantly beheld these ramifications, proceeding from its grosser part, like so many thongs or laces into which a piece of leather had been cut, some broad and some narrower; but none very thick; of a yellow colour, and fat substance; each of them weighing 1lb. which I may safely say, was more fat than was upon all the body beside.

The mouth is very small and narrow, in proportion to the body, and that on these accounts: 1. Because neither lips nor teeth are employed in gathering the food, as in other quadrupeds; so that the mouth only serves to receive the aliments from the proboscis, which both gathers and conveys them into it.

2. The dentes maxillares are of such a thickness, both in the upper and lower jaw, but especially the latter, that they serve to render the mouth narrow; nor need it be broader, because the strength of the grinders is such, that they can at once render the aliments so small, that there is no need for the tongue to move them to and fro in the mouth, in order to have them further masticated, as in other animals; therefore is the tongue small, short and round, terminating in a point, thick, and not thin and flat as in oxen, with a soft smooth surface, without any perspicuous papillæ; by which it seems not to chew the cud.

The head showed very little remarkable: the brain itself very little differs from that of a human one, except in size, and somewhat in figure; the other being somewhat oval, and this more round. The dura mater was a strong thick membrane, every where disengaged from the pia mater; which, together with all the substance of the brain, was much more tender, soft, and flaccid, than could have been expected. At the opening of the longitudinal sinus, there were also polypuses, which proceeded from the orifices by which the blood empties itself into the sinus.

As to the osteology, and first of the bones of the head, this being composed, of the bones of the upper and lower jaw, on its upper part it is almost round, having two eminences with a depression in the middle before; which depression, as it runs back, becomes a deep sinus; and these eminences drawing nearer to one another, and as they ascend behind, inclining obliquely forward, are not unfitly compared by Mr. Ray to a man's buttocks: about its middle part it is almost quadrangular, being flat before, till it comes to the root of the trunk, where it is depressed, for the more convenient lodging of the proboscis, till it has past over the mouth. At each side it is much contracted for the moving of the muscles of the lower jaw; at its back part it becomes very narrow, with several eminences, sinuses and holes; of all which in order. At its lower and fore part, the bone of the palate is narrow, where the proboscis hangs over: on each side of which are the alveoli for the tusks; and behind, the lower makes up all the rest of the head, as to its external view.

The os maxillæ is a very irregular bone. At the fore part of the skull it begins with a sharp point, having that part of the os frontis which forms part of the orbit, on the one side, and that part of the os palati, which forms the hole for the root of the trunk on the other; whence running 6 inches, and inclining inward by a crooked suture, it terminates in a protuberance; beneath which is a small sinus ascending obliquely to the hole for the root of the trunk, framed by the blood vessels (whereof above) as they go to the nourishment of the trunk; from thence it runs obliquely backward, and is articulated with the os palati by a broad squamous suture. From the middle protuberance of the sinus for the eye, it runs straight backward, being articulated with that part of the os frontis which forms the aforesaid lower edge of the sinus for the nervus opticus, for the space of 18 inches, where it begins to be overlaid with a lamina of the bone, which forms the upper and back part; whence it descends 9 inches, till it comes to the root of the teeth, where we shall leave it, and return to the fore-named protuberance; from whence having made up a part of the sinus for the globe of the eye, as is said, it runs backward 6 inches, and is articulated by a flat suture, which first descends $\frac{1}{2}$ inch, then runs obliquely backward $2\frac{1}{2}$ inches, with the os zygomaticum. At its beginning it is $2\frac{1}{2}$ inches broad; plain on its inner, and convex on its outer surface; bended, as it descends, like a horn, and terminating in a point. From the lower part of this suture it becomes much thicker; and having framed a sinus about 4 inches long, it runs toward the fore part of the skull. From this sinus, as it has returned 3 inches, is formed the side of an oval hole, which running from before to behind, is about $3\frac{1}{2}$ inches long, and from the one side to the other 2 inches. At that side which is framed by the os maxillæ, and toward the processus zygomaticus, it is 2 inches thick; and at its other side, it runs straight backward from the os maxillæ, in a direct line, with the great cavity, which contains the muscles that move the lower jaw and proboscis. This hole is analogous to that in a human skeleton in the os maxillæ, beneath the orbit of the eye; and is larger in quadrupeds, being designated for transmission of a vein, artery, and the superior branch of the second division of the 5th pair of nerves, which in those go to the upper lip and jaw; but in this subject, as I have already shown at large, it is probable they serve for the nourishment and other functions of the proboscis.

The two cavous bones on each side the choana, are filled up from the two firm, solid, white, weighty teeth; the back one does not grind, but serves as it were a wedge, to keep that before firm in its place. This tooth runs obliquely backward 3 inches from the fore tooth. That part of it which is without the jaw bone is half round, being 6 inches in surface from its root on the one side to that on the other, very smooth like glass. The length of each of the teeth is 7 inches. These teeth are not alike on both sides; for that on the right is but 1 inch without the alveolus, throughout its whole extent on the outer side; and on its inner, it is 1 inch protuberant at its fore, and 2 inches at its back part; whereas that on the left side is only 1 inch protuberant before on the outside, and 3 inches behind, where it forms a kind of angle, as it is joined with the hind tooth; and on the outside it is $\frac{1}{2}$ inch protuberant before, and 2 inches behind. The tooth on the right side grinds with that of the lower jaw, throughout its whole extent; whereas that of the left, after it has run back 6 inches, runs up with a half round surface 2 inches, before it is joined with the hind tooth. Not only both the hind teeth are free from grinding, but also part of the fore teeth of the left side. These teeth, as Dr. Moulins well observes, are all molares, being 2 inches broad; that part of them which grinds is 6½ inches on the right side, and 5½ on the left. Their surface, though flat, yet is very unequal; for they have alternately placed (running from the right to the left) a hollowness, and then an eminence, and this eminence is surrounded by a rough protuberant border. There are nine of each of the hollownesses, and as many eminences, undulated, as they use to paint sea waves.

It is well known, that immediately below the coronæ there is a pretty large hole, in proportion to the animal, for the emission of a branch of the external carotid artery, jugular vein, and 5th pair of nerves, called maxillaris inferior, which are dispersed in the roots of the teeth for their nourishment, and for conciliating to them that quick sensation of pain, which those affected with the tooth-ach are very sensible of; and that in this hole in sheep, calves, and other quadrupeds, especially such as are young, as also in children before the 7th year, and even afterwards for some time, in the cavous part of the bone, where the teeth do not penetrate the jaw, there are rudiments of teeth to be seen cavous in that extremity, which is towards the base, (in which the ligaments that keep the root fixed are firmly impacted) and solid at the other extremity; so in this animal from the fore-mentioned large hole, I observed several of these rudiments of teeth lying stratum super stratum, or rather placed perpendicularly across the bone of each other side, from the hole, till the teeth began to appear. Those that were placed nearest the hole were smaller, not above 1 inch in breadth, and ½ inch in length, i. e. from above to below, cavous at the lower or back part, (for reception of the ligament, which is guarded by two thin hard laminæ) and solid at the other. Those nearest the hole were two or three times intersected by membranes, by which they could be disjoined. But after I had taken out several, I found no more such a separation, but that from the right to the left, they were wholly cavous: each of them was invested by a membranous tunicle, as it were a periosteum, and had something like a cartilaginous substance between the two. Their surface is very unequal at the orifice, where they receive the ligaments and vessels, and as if they had been folded into several plicæ, and afterward taken asunder, from which there ran several ridges and sulci, from one extremity to the other; where the ligaments cease, they become extremely solid and ponderous, and at their upper extremities half round, and sometimes formed into digitations. When they approach to that part of the bone at which the teeth appear, they begin to quit the periosteum, by which they were distinguished, and unite close together, so as to form one bone. It is observable, that at their upper extremity there is a lamina, which being convex toward the jaw, and concave toward these rudiments of teeth, do as it were knit their solid extremities together, from which it is also separated by an intermediate membrane at the beginning; but afterward that ceasing, this lamina conjoins them at the extremities, as they are at the sides, before they appear without the jaw. And thus I conceive these teeth to be formed, and it is by these I am persuaded the jaw becomes so ponderous and thick; and that which strengthens this opinion is, that the hind teeth of both jaws (for I doubt not but these rudimenta dentium are in the upper jaw also) before they come to grind, have their upper parts semicircular; and that both before and after the grinders are formed, the lineaments of these rudimenta appear plainly like so many ridges, having intervening furrows, where they formerly had been distinguished by membranes:* and I suppose, though at the upper extremity they are united into one compact bone, yet at their lower extremity they have still the same hollownesses, for reception of the ligaments and vessels as formerly; which opinion is confirmed by Tentzelins's account. The lower jaw has 4 teeth, 2 on each side, as well as the upper, all grinders, but no incisores, or fore teeth. The hind teeth are 8 inches distant, and the fore not 4, between which is placed the sinus for the tongue: and it is observable, that from thence to the bottom the sinus is so contracted, as only to be one inch broad. The hind tooth on the right side is 4 inches, on the left 5. The one half of their surface, where they begin to appear, is semicircular, with the ridges and sulci running transversely, 4 on the right side, and 5 on the left. The other half has 5 of those eminences, where it grinds, and 4 on the left. Each of the fore teeth is 6 inches long, and has 6 or 7 of the fore-mentioned eminences, and as many depressions. The hind teeth of Dr. Moulins' elephant seem to have been of an equal length on both sides, and much longer than the fore teeth. It is observable, that the ridges at the sides are correspondent to the eminences where they grind, and the sulci to the depressions. The teeth of the lower jaw exceed those of the upper about 2 inches in length; by which it appears, that the motion of the lower jaw must be very great in mastication, and that the elephant for the most part moves the jaw from behind to before; and scarcely from one side to the other, as in animals that ruminate, or chew the cud. These teeth are the most firm, solid, and weighty bones of any animal yet known, and are as good ivory as the tusks themselves.

As there is no animal in proportion that is endued with a greater quantity of brains than man, so there is none that seems to have less than the elephant. In the one, it was so ordered by the wise Governor of all things, that they might be sufficient for the generation of so many spirits, as are requisite for

* See Mr. Corse's observations on the mode of dentition in elephants, and Mr. Home's observations on the structure of their teeth; both inserted in the Philos. Trans. for 1799.

the performance of the rational and animal functions; and in the other, had the quantity of brains been greater, the principia nervorum had been more divided; so that instead of being requisite, they had been vastly inconvenient, because the nerves could not so well receive the spirits dispersed in a greater mass, as now, when contracted within less bounds.

The inner surface of the skull, where the brain is lodged, is in figure like a human skull, but more spherical, being from the right to the left 10 inches, from front to back 9 inches, and from above to below at the anterior fossa 7 inches, between the middle 5 inches, and at the posterior, or seat of the cerebellum, $4\frac{1}{2}$ inches. It has 4 fossæ, and 5 eminences. The anterior fossa is circumscribed by the fore part of the inner table of the skull before, and by the two anterior eminences behind. Here the brain sends forth its greatest production; for at the hind part this anterior fossa is depressed straight down near 2 inches, where the os ethmoides begins, which is of a singular figure and structure; for from the fore part of the seat of the brain in the middle, there is here, as in most skulls, an eminence which runs obliquely downward, till it begins to form the crista galli, so called in human subjects. This crista galli divides the os ethmoides into its right and left part; it is pretty thick and broad at the base, whence it rises from each side, till it begins to form a crena, which is perforated by 3 pair of holes; and then there arises a small spina in the middle, at the fore extremity whereof, it being further extended than the ethmoides, there is another hole. From this crista galli run on each side several prominent convex lines, some obliquely forward, others obliquely backward, others transversely; each of which is branched out twice or thrice toward the circumference. These lines have some few perforations running from their highest part, but most of them are between their interstices, where they are pleasantly dispersed after some kind of order, which we could not express in the figure. The os ethmoides is not unlike a heart, as they usually represent it, being narrower at the hind part, where the anterior fossa runs straight down from the fore part of the sella turcica, and broader at the fore part of the bone, which runs obliquely upward from it; it is from front to back $3\frac{1}{2}$ inches, and from the right to the left 4 inches: its circumference is not altogether circular.

The parts of the ear, are, 1st. The meatus auditorius, or that duct which runs from an orifice on each side of the head, to the inner table of the skull, terminating in the os petrosum, being of a cylindrical figure, having the cellules arising from it on all sides. It is in length, from the external orifice to the crena for the membrana tympani, $9\frac{1}{2}$ inches, and about 1 inch diameter throughout the whole extent. Its sides are composed of a firm solid bone, and little thicker than a halfpenny. Next is observable the crena for the membrana tympani, in circumference $2\frac{1}{3}$ inches: after which is to be seen the cavitas tympani, consisting of two parts; the first is $\frac{1}{2}$ inch deep, straight down from the foresaid crena, endued with a great many cellules, distinguished from each other by several osseous solid laminæ, irregularly disposed. These excavations were about two or three lines, or $\frac{1}{3}$, or $\frac{1}{4}$ inch deep. The next cavity is of a surface more smooth, arises much higher than the former, and runs toward the outer table, having several semicircular lines running across. The first cavity is from the right to the left $1\frac{1}{3}$ inch, and from front to back $1\frac{1}{4}$ inch. The second cavity 1 inch in length, and $\frac{1}{2}$ inch at its broadest part. The ossicles, viz. the malleolus, incus, stapes, are of a proportional size, running from the malleolus, which touches the membrana tympani, to the basis of the stapes, which shuts the foramen ovale. It has but a small cochlea in proportion. I searched for the labyrinth, or lineæ semilunares, but could find none; by which I concluded, that these caverns in the bottom of the cavitas tympani served for the same uses in this animal, as the meanders of the labyrinth do in others; and that this second cavity served for receiving and continuing the undulations of the air, for the longer retaining of the sound, as we see the cavous apophysis mastoides does in sheep, cats, dogs, &c. and the spongious one in men. The foramen ovale is but small, and the base of the stapes very thin and slender.

From the head we go to the trunk, which consists of the spine, ribs, and sternum. The spine is divided into the vertebræ of the neck, back, loins, os sacrum, and tail. The vertebræ of the neck differing from each other in several things material, I shall speak of them separately: the first called atlas, has four considerable cavities; two at the fore part, by which it receives the condyles of the skull, and two at the back part, by which it receives the base of the following vertebra; the first two are $2\frac{1}{2}$ inches from above to below, and 2 inches from the right to the left. It has a large hole in the middle, divided into its larger part, 3 inches diameter, which is for receiving the spinal marrow, and lesser, which receives the tooth of the following four perforations, or two pair of holes at the sides; one at its lower and fore part, which receives the arteria cervicalis, or vertebralis from the side of the spinal marrow, and conveys it to a crena, along which it runs, till it again penetrates the same bone, and goes out at the back part; after which in its progress it perforates all the transverse processes of the rest of the vertebræ of the neck, as usual in other animals. This crena is guarded on its outside, or at the extremity of the transverse processes by a protuberance, which runs toward the skull $1\frac{1}{2}$ inch, till it be equal to the sides of the hole for the spinal marrow. At its upper and fore part it inclines obliquely, where it is 3 inches thick, and at its lower and back part it has a protuberance, which is extended where it embraces the tooth. This vertebra is in diameter 12 inches. The transverse processes are in breadth from above to below 2 inches, and in length at their lower part 3 inches.

The second vertebra has remarkable in it, 1st. A large protuberance called the tooth, which is received by a hole in the former, and serves as an axis on which the head is turned round. This tooth runs forward from the body of the vertebra 2 inches above, and 2½ inches below, tapering and terminating in an obtuse point. 2. A large protuberance arising from its upper and middle part, (like the processus spinosus in others, 4 inches from the beginning of the transverse processes, 2 inches broad at the top, terminating in two obtuse points, with a sinus larger at the back than the fore part) in the middle. This protuberance inclines forward toward the first vertebra: 3. Its body or back part, and base of the tooth, transversely 4½ inches, perpendicularly 4 inches. 4. Two oblique processes, by which it is articulated with those of the following, and between which there are 4 inches. 5. Two transverse processes, each 2 inches long. At its fore part, on each side of the tooth, are two protuberances, which are received by the two hind cavities of the first vertebra. This vertebra is 2 inches thick from before to behind, the hole for the spinal marrow 2 inches diameter, those for the cervical artery ½ inch diameter. Between the oblique and extremity of the transverse processes, it is 6 inches.

The third and fourth vertebræ differ from this, 1st. In their four oblique processes, viz. two by which they are articulated with the preceding, and two with the following, which is common to those of the neck, back and loins. 2. In their convex body before, and concave behind, where they are received by, and do receive the preceding and following, which is also common to the other vertebræ. 3. In their eminences at the top (between the oblique processes) at 4½ inches distance, between which there is a depression in the third, and a small protuberance in the middle of this depression in the fourth. Their transverse processes are 2 inches broad at the extremity, from which they descend obliquely 3 inches, having a protuberance on each side, between which and the body of the preceding vertebra, there proceeds a branch of the cervical artery, which it continues to do from between all the other vertebræ of the neck, till it comes to between the seventh of the neck and first of the back, where it is wholly spent. Between the oblique processes and transverse it is 4 inches. The hole for the cervical artery is here oval. The bodies of these vertebræ are thinner below than the former: they are 4 inches in diameter, being of the same dimensions with all the other vertebræ of the spine, till we come to the os sacrum. Those in the neck are more flat before, and those in the back more protuberant. Besides those sinuses in the inside of the forementioned protuberances in the neck, there is likewise a sinus between the transverse processes of each vertebra and its body, throughout the whole spine, for transmission of the several conjugations of nerves from the spinal marrow.

The fifth vertebra is of the same dimensions with the other two, and differs in nothing from them but by its spinal process, which, from ½ inch in the former, rises to 1½ in this, being ½ inch broad, and thin at the extremity. The sixth vertebra differs from all the rest in its transverse process, which is as far forward as the rest, and sends out another process, which runs as backward; so that from the extremity at the fore part to that at the back part, it is 3 inches. It also sends another protuberance obliquely outward 1½ inch. At this process the cervical artery passes out from the transverse process of the vertebra, and only sends a twig to the seventh vertebra, or last of the neck, which differs from all the rest, 1st. In the length of its spinal process, which is augmented from 3 inches in the former to 5 in this. 2. In the smallness of the hole for the cervical artery, this being the last perforated transverse process, as is said. 3. In a sinus on each side of its body behind, into which it receives part of the first rib.

The thorax is divided into the vertebræ, ribs, and sternum: there are 19 vertebræ, correspondent to so many pair of ribs; they differ in nothing from the former, except that their body is more protuberant, that their spinal processes are augmented, and diminished in their length, according to their situation, and that they have a sinus in each side, both before and behind, for the reception of their respective ribs: for the weight and dimensions of their spinal processes see the following table.

The ribs are divided into the true (which are articulated with the sternum) and false ones, with, and without cartilages. There are 8 pair of true ribs, 8 pair false with, and 3 without cartilages. The cartilages here are soft, as in human subjects, and not bony as in oxen, harts, &c.

The sternum consists of four bones; they are placed edgewise, being two inches thick above, and sharp below: from the fore part to the point of the cartilago ensiformis it is 25 inches; of which the first is 8 inches long, and 4½ inches broad at the articulation of the first rib; the second 4½ inches long, and 3 inches broad; the third 3½ inches long, and 3 inches broad; the 4th 4 inches long, and 2½ inches broad; the rest of the length is made out by the cartilago ensiformis.

The loins consist of three vertebræ, whose spinal processes are but short; their transverse processes a little longer than those in the back; which, besides their weight, is all that is material about them.

The os sacrum consists of five bones; they are of a flat surface before, each having three processes, (viz. two oblique, and one spinal, under which the spinal marrow descends) behind. They are perforated before by 4 pair of holes, placed at their interstices on each side. It is 12 inches long, and $7\frac{1}{2}$ broad, where it is articulated with the ossa innominata above, and 6 inches broad at the lower part.

The tail consists of 29 vertebræ, whose differences are to be seen in the following tables. From the os sacrum to the eighth they have five processes, viz. two transverse, two oblique, and one spinal, under which the spinal marrow descends, sending forth a conjugation of nerves from between each of them. From the eighth to the sixteenth each has 4 processes, viz. two longitudinal behind, between which the remainder of the spinal marrow still descends, and two transverse: the rest have no process at all, but are of a kind of quadrangular figure, having a ridge which descends before and behind, and on each side; being somewhat larger at each end, and smaller at the middle. All the vertebræ, as well in the neck and back, as tail, had cartilages, which run between them; they were about $\frac{1}{2}$ inch thick in the back, thinner in the neck, and thicker proportionably in the tail. These cartilages I was obliged to supply with leather in mounting the skeleton.

Because this is a quadruped, we shall divide the extremities into the fore and hind ones. We begin the fore extremities at the scapula, which is usually divided into its head, neck, spine. Processes, fore and hind cavities, (i. e. these parts before and behind the spine) its concave part, which lies upon the ribs, and its convex or outer part, and its margin. The head, which receives the os humeri, is oblong, (because the motion of the humerus performed by this animal, is rather flexion and extension, than adduction or abduction) wherefore the cavity being $5\frac{1}{2}$ inches long, is only $3\frac{1}{2}$ inches broad, and the margin of the bone arising from the fore and back part makes it 2 inches deep; for at the sides the margin is equal to the cavity. At the back part there are two processes; that which regards its convex part is 2 inches long, rugous, thick and obtuse: that which regards the concave, runs 2 inches backward, where it forms an obtuse angle; whence it ascends 3 inches to the neck: this has a sharper edge than the former, being somewhat incurvated. Between these two processes is a rugous sinus, 2 inches broad, ascending from the cavity of the head 2 inches to the neck, and rising somewhat in the middle of its progress. The neck is flat, being more obtuse at its back part, and where the last named protuberance is, and sharper before, where is a little protuberance toward the spina on the convex side, being more plain on the concave. Between the first of the fore-named protuberances and the spina, there is a sinus which ascends

4 inches, and from the same protuberance to the extremity of the processus coracoides, is 3½ inches. This processus coracoides is that part of the spina which runs toward the neck of the scapula, but does not, as in men, defend the humerus from dislocation. It is very rugous and convex before, but concave at its back part, being 3½ inches broad at the point, it ascends 11 inches, where it is raised 5 inches from the body of the scapula; it inclines a little backwards, and ascends 12 inches more, till it be lost where the epiphysis begins, being still concave at the fore, and convex at the back part. From the spina it sends forward a production 8 inches long, 3 inches broad at its upper part, 2 inches about the middle, where it is crooked, and 1½ inch at its lower extremity, where it is thin and sharp; but at its upper part thicker and rugous, concave at the inner, and convex at the outer side. This process serves to keep the musculi supra-spinati within their bounds, when they pull up such a vast weight as the fore leg. As the spina runs up 5 inches from this production, it is rugous and thick in its edge, from thence it becomes gradually thinner till it comes to the epiphysis. The back and upper part of the scapula is very thin, and sharp from the neck, 16 inches upward, from thence it ascends 5 inches to the place where the spina ends, and forms a very rugous and spongious epiphysis, thick at the upper end, thence it descends obliquely 2 feet, whence its fore edge runs obliquely in toward the neck 10 inches, being sharp; thence it tends outwards, and descends other 5 inches, till it comes to the neck. All the upper part of the scapula is covered with an epiphysis, spongious and rugous, which separated by the boiling, and is a further argument that this animal was young, according to their term of life; it is otherwise a very thin bone and solid, except where the epiphyses are.

The humerus is a very irregular bone; its head is in circumference 2 feet, having 2 remarkable epiphyses, one by which it is articulated with the scapula from before to behind, with a convex surface 8 inches, and from the right to the left 4½ inches, and another on the outside rising higher and sharp, about 1½ inches. Round this epiphysis is 11 inches, and from before to behind with a flat outside 6½ inches. Between the epiphysis, which receives the scapula, and this protuberance, is a sinus about 3½ inches broad, and about 1½ inch deep; as it descends toward the inside it becomes deeper, and only 2 inches broad: it is for lodging the external tendon of the biceps, analogous to a crena, for the same purpose in human subjects. The neck of the humerus is in circumference 19 inches, flat behind for the space of 4 inches, then forming an angle, and running obliquely outward 3 inches, then passing forward below the utmost protuberance 5 inches, thence crossing the fore-named sinus it runs back, first flat, then a little convex, 7 inches. Below this outward protuberance there is a rugosity for the insertion of the flexores cubiti, 6½ inches long, and 3½ inches broad at the upper part, and thence descending gradually, it terminates in a point. At the lower part of this rugosity, the bone is 13½ inches in circumference, having 3 faces; one at its back part 5 inches broad, somewhat depressed from the inside, then a little protuberant, as it tends outward; a second on the outside, and 4 inches broad; and the third on the inside, 4½ inches broad, flat also. Here begins another considerable rugosity, small and oblique from the back part of the utmost protuberance of the humerus, and becoming very rugous at this place, continuing 6 inches obliquely downward, and 2 inches broad about the middle. At the lower part of this rugosity the bone is 18 inches in circumference, with its three faces otherwise disposed; that which was before terminated in an obtuse spina, and where it was an obtuse spina behind, now becoming flat. On the outside begins a considerable sinus, being the continuation of the sinus formerly mentioned, between the epiphysis humeri and the outer protuberance, whence in its descent the bone became depressed; and now the sinus is conspicuous, being framed by an obtuse spina, descending obliquely forward from the foresaid rugosity on the one side, and another obtuse spina descending obliquely outward on the other. This sinus is 4 inches broad from before to behind; thence measuring backward, the bone is flat 3½ inches; measuring from thence on the inside, the bone having formed an obtuse angle, is 5½ inches flat also. The spina on the fore side, after it has descended 5½ inches, the bone becomes flat; that on the outside terminating in a considerable protuberance, 4½ inches long, where the bone has only two faces, convex before and concave behind, and 17 inches round. Behind its foresaid outward protuberance is 7 inches; it becomes 1½ inches depressed in the middle, where the sinus for receiving the cubitus begins. From the foresaid external protuberance it descends in a straight line 8 inches, and from its opposite part at the inside it descends obliquely backwards 5 inches; and here the lower epiphysis begins, where it is received by the cubitus and radius. This epiphysis is 1 foot 10½ inches round, being 3 inches thick at the outside and flat, and 7 inches at the inside and protuberant; between which behind is a considerable sinus 5½ inches broad, and 2½ inches deep, and before, another sinus 7 inches broad, of the same deepness. This epiphysis is at its lower extremity and inside, where it receives the cubitus, 5½ inches from before to behind; of a convex surface, and 6 inches at its outside, where it is received by the radius. At the extremity of the epiphysis, it is narrower, being only 6 inches from the right to the left before, and 7½ inches behind. And thus you have an account of the most irregular bone of the body, being at its external part 30 inches in length, and 26 at its internal; having a large head consisting of an epiphysis received by the scapula, a large protuberance on the outside defending it from dislocation, and a sinus between the two, reaching a good way back, thence descending to its neck, whence the bone becomes flat, to about the middle, descending on the outside, flat also, with two rugosities for the insertion of the tendons. Between this outside and back part, is a very large sinus for the biceps, which oblique situation is an admirable contrivance for adding strength, and conciliating length to this muscle. Now the shape of the bone begins to be changed, for whereas it formerly reached from before to behind, now it reaches from the right to the left, and its lower extremity becomes broader, whereas at its upper extremity it was rounder.

The cubitus and radius are two bones of a singular figure, the one lying above the other. The cubitus is in length, from the top of the olecranon to its articulation with the bones of the carpus, 28 inches. The olecranon from right to left, with a surface somewhat convex, is $9\frac{1}{2}$ inches; and from before, where it is articulated with the humerus, to its utmost point behind, in a straight line 7 inches. This olecranon, as it descends, becomes narrower by degrees, till it forms a spine, which runs obliquely forward 13 inches, where it is contracted from the foresaid $9\frac{1}{2}$ inches to 3 in breadth. Afterwards the bone is enlarged on each side, till it forms a convex surface, which is received by, and articulated with the humerus. This articulation is a ginglymus, as in all other animals; viz. the cubitus and radius together receive the humerus on the outside before, which on the inside the cubitus does alone. Between these two is a large protuberance rising $2\frac{1}{2}$ inches, which is also received by the humerus. Measuring from the extremity of the cubitus and radius, which receive the humerus, on the outside, to its opposite part on the inside, including the back part of the olecranon, it is 10 inches. Both the bones from the right to the left, at the articulation before, are 7 inches. Then measuring round the cubitus, below the articulation, is $16\frac{1}{2}$ inches. Here the bone is flat before, from the right to the left 7 inches; from thence obliquely backward to the forementioned spine beneath the olecranon is 5 inches on the outside, and $6\frac{1}{2}$ on the inside. At the lower part of the foresaid spine the cubitus is in circumference 11 inches; viz. flat before 4 inches, where it forms an angle; thence running obliquely backward 2 inches, forming another angle; thence $1\frac{1}{2}$ inch obliquely backward, where it is a little protuberant; and from thence obliquely inward $3\frac{1}{2}$ inches. Round the lower extremity of the cubitus, and 3 inches above the epiphysis, it is 12 inches; viz. from the radius $2\frac{1}{2}$ inches; thence obliquely outward, with another flat surface, $3\frac{1}{2}$ inches; and from thence, with a convex surface, round the back part 6 inches. The cubitus at the epiphysis, from the radius on the fore part, to its opposite side on the back part, is 13 inches; from the upper part of the lower epiphysis, where it is articulated with the external bone of the carpus, obliquely inward is 5 inches. This same epiphysis, with a convex surface behind, from the right to the left, is 5 inches.

The radius in length, from the external and upper part of the cubitus, on which it lies, and with which it is united, running obliquely inward, is 1 foot $9\frac{1}{2}$ inches. At its upper part it sends a production outward $3\frac{1}{2}$ inches, by which, with a part of the cubitus, it receives the external part of the lower epiphysis of the humerus: from thence the bone is contracted at its fore part to $2\frac{1}{4}$ inches; then descending $10\frac{1}{2}$ inches, it becomes $2\frac{1}{2}$ inches broad; thence it enlarges gradually to the lower epiphysis, where, measuring from the fore part of the cubitus to its opposite and back part, the radius is $8\frac{1}{2}$ inches; thence you descend 3 inches, to its articulation with the internal bone of the first rank of the carpus. This bone is quadrangular above, descending to about the middle it becomes more convex; from thence it is gradually enlarged, and during the whole progress pretty free from the cubitus, except where it is conjoined with it at its upper part: at the lower articulation, they are only separated by a cartilage. The lower epiphysis of these bones is of a very unequal surface, and though not separated from them by boiling, yet plainly distinguished by their cartilages, which are not yet ossified.

The fore foot (as the hand in human subjects) consists of the carpus, metacarpus, and fingers, or rather toes. The carpus has 6 bones disposed into 2 ranges, differing in figure from each other, rugous before, endued with several holes for transmission of blood vessels, and of a convex surface behind, having several inequalities for insertion of tendons.

There are six bones in the metacarpus, per ginglymum longum; viz. they are received above by the bones of the second rank of the carpus, and below by the toes. They are all of much about the same figure, but not of an equal length; somewhat flat before, and both convex behind; broader at the upper and lower extremity where they touch one another, and narrow in the middle.

Each of the 6 toes of the fore foot consists of two thick short bones; whereof the first of the external toes is $5\frac{1}{2}$ inches round, and $1\frac{1}{3}$ inch long, the second about 1 inch broad, and $\frac{1}{2}$ inch long, weighing 2℥. The first bone of the second toe is 2 inches long, and $6\frac{1}{2}$ inches round; the second is $1\frac{3}{4}$ inch from the right to the left, and $\frac{1}{2}$ inch from above to below, weighing 3℥. The first bone of the third toe is 2 inches long, and $7\frac{1}{2}$ inches round; the second bone is divided into two in this subject, in figure not unlike an ox's hoof, whether it be a lusus naturæ, or peculiar to all other animals of this species, I know not, weigh 4℥. The first bone of the fourth toe is $2\frac{1}{4}$ inches long, and $5\frac{1}{4}$

inches round; the second bone is in figure not unlike the former, but not divided; from the right to the left 2 inches, and from above to behind 1 inch; weigh 4ʒ. The first bone of the fifth toe is 2½ inches long, and 4½ inches round; its second bone is in figure like the former, but less, and divided; weight 3ʒ. The first bone of the sixth toe is 2 inches long, and 4½ inches round; larger at the upper and becoming narrower at its lower extremity, wherewith a very small bone is articulated, and weighs 1½ʒ.

Besides all these, there are 2 ossa sesamoidea, which are affixed to the lower part of the lower extremity of each bone of the metacarpus, each being about 1 inch long, ½ inch broad, protuberant at the lower part, and concave at their upper, or that side where they are articulated with the metacarpus; separated from each other by a cartilage, which ran down in the middle of this lower epiphysis of the bone in the metacarpus. These ossa sesamoidea were very useful for supporting the foot; for about their middle all the four hoofs of the fore foot terminated. They weigh each ʒij.

The hind extremities consist of the ossa innominata, the thigh bone, the two bones of the leg, and the foot. The ossa innominata consisting, as in other animals, of two large bones, articulated behind with the os sacrum, on each side, and before with each other per synchondrosin, as it is called: each may be divided, as in human subjects, into the ilion, or upper and external part, os pubis, or lower and fore part, and ischion, or lower and back part; though strictly speaking, these bones here should only be divided into the ilion and pubis, there being no remarkable part about them which deserves to be pointed out by the name of ischion. Both these ossa innominata joined together make up the pelvis, which in circumference is 4 feet 6 inches, From the os sacrum above, to the upper part of the os pubis below, it is 18 inches, and from the right to the left 17 inches. The os pubis at the articulation is from above to below 12 inches; between the two outer and lower extremities of the ilion, from the right to the left is 1½ foot; from the os sacrum above, along the margin of the os ilion, down to the fore-mentioned utmost point, is 2 feet 9½ inches; and from that same point to the acetabulum, which receives the femur, 1 foot. This acetabulum is in circumference, round the external edge, 18 inches. The breadth of the os ilion, from the external edge to the side of the pelvis, is 13 inches. Round the neck of the ilion above the acetabulum is 14 inches. The ossa innominata, from the upper part of the ilion to the acetabulum, 22 inches. The breadth of the os pubis, from the articulation with its partner to the outside 8 inches. The length of the oval hole for the musculus marsupialis 5½, its breadth 4, its circumference 13 inches. Between the lower and utmost extremities of the os pubis behind 17 inches. These ossa innominata are flat before, standing almost perpendicular with the two lower and utmost extremities of the os ilion, bending forward, having the os pubis ascending obliquely, convex before, where joined together, and concave behind. This ascent of the os pubis is a further argument, that this is no retrocoient animal.

The femur is 3 feet long with its upper epiphysis, in circumference 15 inches; the neck below it is 13 inches; the breadth from the great trochanter on each side 10 inches; below this trochanter, in circumference 18 inches; about the middle 12 inches; round above the lower epiphysis 16 inches; round the lower epiphysis itself, from the patella on the outside to its opposite part on the inside, 18½ inches. Its diameter, where articulated with the tibia behind, is 7 inches; having two protuberances, whereof the external is 2½, and the internal 3½ inches. The internal epiphysis, which is received by the tibia, is from before to behind 7 inches, and the external 5½ inches. The femur is in general a long straight bone, having a large round head, which in this subject is separated from its body by a cartilage, still an argument of the youth of the animal, received by the ossa innominata. A large trochanter on the outside, where the bone is broadest: from thence in its descent it becomes smaller, flat before and behind, and thicker on the inside than the outside, nearly of an equal size, till it comes to the lower extremity, where it is increased into two large epiphyses, which are received by the tibia, with a sinus in the middle, about 1 inch deep, and as much diameter. It sends forth a large protuberance before, which is received by the

Rotula, or patella, a bone of a very rugous surface, considerably protuberant on the outside, being from above to below 8 inches, and from the right to the left 6 inches. It is articulated with the femur per ginglymum, having a cavity on each side which receives, and a protuberance in the middle received by the femur.

The tibia is in length, measuring behind, 22 inches; its circumference at the upper epiphysis 19 inches, and at the neck 17 inches. Before it has a large depression for facilitating the motion of the patella; of a very rugous surface, for insertion of the extensores tibiæ. It is almost semicircular before and flat behind; its circumference about the middle is 9½ inches, and at the lower extremity, where it again meets with the fibula, and where it receives the astragalus, 12 inches.

The fibula is 21 inches long, 5½ inches round above, where received by the tibia, 3 inches about the middle, and 5 inches at the lower part, where it receives the tibia. Its epiphysis, which forms the external ancle, or malleolus, measuring from before to behind along its outer surface, is 5½ inches.

The bones of the hind foot consist of those of the tarsus, metatarsus, and toes. The tarsus consists of 6 bones, whereof

The first is called astragalus. This is articulated above with the tibia, having a depression in the middle 3 inches, which receives, and two protuberances at the sides, which are received by the tibia, 3½ inches. It receives the talus below with a surface 4 inches diameter. On the inside it sends forth a large protuberance, and on the outside both it and the talus are received by the external malleolus for the space of 2 inches. Before it is received by the os naviculare during the space of 4½ inches. It weighs 6℥.

The talus is a very irregular bone; it is protuberant behind from the astragalus 4½ inches. This protuberance is in circumference at the extremity 10 inches, and at its neck 8 inches; below it is very rugous on the inside. It sends forth a protuberance, which is received by the astragalus, as in human subjects. It has three remarkable surfaces, viz. one upon which the astragalus rests, at its upper side; one at the fore part, articulated with the ossa cuneiformia; and the outside, for the fibula. It weighs 1 lb.

The os naviculare is 10½ inches in circumference, 1 inch thick, concave behind, where it receives the astragalus, and convex before, with its different surfaces, viz. one where it is articulated with the bone of the toe on the inside, and one for each of the three wedge-like bones. It weighs 4℥.

The bones of the second rank of the tarsus are called the three wedge-like bones, two of which are articulated at the back part with the os naviculare, and the third partly with the talus and partly with the os naviculare; each of them is about 1 inch thick; that on the inner side is the least, being from the right to the left 1 inch thick; it weighs ℥iß; the middle 2 inches, in weight ℥ij; and that on the outside 3½ inches, in weight ℥iij. This last has two surfaces at the fore part, where it is articulated with the two utmost bones of the metatarsus. The middle bone of the tarsus being only received by the middle bone of the metatarsus.

The external bone of the metatarsus is very irregular: it is articulated behind with the external os cuneiforme, where it is about 2 inches from above to below; from whence, its external surface being very rugous, it becomes gradually smaller, being somewhat concave below, and protuberant above, till it terminates in a round extremity. It weighs ℥ijß.

The second is a short thick bone, 8 inches in circumference, and 2¼ inches long; in weight ℥ijß. This receives the first bone of the toe, which is 4½ inches in circumference, and 2 long; and receives the second bone of the same toe, being 3 in circumference, and 1½ long; greater at its upper, and smaller at its lower extremity, to which is affixed a small bone. These weigh ℥iijß.

The middle bone of the metatarsus is the largest of all those belonging to the toe, being 7 inches round, and 5 long; ℥iiij in weight. It receives the first bone of the toe, which is 4½ inches round, and 2½ long; to whch also is joined another small bone to make up the extremity: in weight both ℥iiij.

The two remaining bones of the metatarsus are thin, broad and irregular; the first is 2½ inches broad, and 2¾ long; weighing each ℥i. to which also adheres a small bone, as in the former toe, but less. The second and last of the metatarsus on the inside is two inches broad, and as much long, thin like the former, having a small protuberance adjoined instead of a toe. The bones of the 4th toe weigh ℥iß, and the 5th ℥j.

I once designed to have compared more particularly the bones now described, with those of Tentzelius* and Dr. Moulins:† but since both these treatises have been already communicated to the R. S. and I doubt not are in the hands of most of the honourable members thereof; and since I have already insisted largely upon these, I shall only put you in mind in few words, that Tentzelius tells his friend, viz. that in digging in a hill near Erfurt in Germany, for a fine white sand, there were found several huge bones, first mistaken for a giant's; but on trial, and the perusal of Dr. Moulins' treatises, known to be the bones of an elephant: and that among the rest there were found the head 42 inches diameter; two tusks 2½ spans round, and 8 feet long; four grinders, each 12lb. the humerus 4 feet 2½ spans; the vertebræ of the neck, each 4 spans in circumference, and 2 spans high; the ossa innominata 2½ feet long; with the head of the femur inserted in the acetabulum, and part of the tibia 22 inches at the largest, and 17 at the smallest part. That they were obliged to dig 24 feet deep, before they could get out the head; that the bones lay in such a posture, as betoken its being overwhelmed, or having had great strugglings while dying; viz. the left fore foot stretched forward to the side of the head, which lay toward the north, the right inclining backward under the body; the left hind foot drawn in toward the body, and the right distorted here and there, out of its natural posture. From all which he concludes this to have been the largest elephant that ever was seen in Europe; and that it could have been brought thither by no other means than the flood, both from the preternatural posture of the body, and from the different strata of earth lying above it, without the least sign of having been digged to bury it.

* Vol. iv. p. 218 of this Abridgment.

† Anatomical Account of an Elephant accidentally burnt in Dublin, by Allen Moulins, M. D. 4to. 1682.

A Table containing the particular Dimensions of the Vertebræ and Ribs, and Weight of all the Bones of the Elephant.

The Bones of the Head.

	lb
Upper jaw	66
Lower jaw	45

The Vertebræ of the Neck.

Nº	Weight. lb	oz.	dr.	Length of Spinal Process. Inch.
1	1	13		
2	1	6	4	
3		13	4	¾
4		13	4	½
5		14	4	1½
6		14	6	3
7	1			5
	7	10	6	

The Vertebræ of the Tail.

Nº	oz.	Length. Inch.	Breadth. Inch.
1	10	2	6
2	8		5½
3			5
4	6		4½
7			4
8	5		
9	4		3½
10			3
11	3		2½
-13	2⅔		
14	2½		1⅔
15			1
16	1¼		2½
17	1		
18		1⅔	
-20		1½	
21		1¼	
22		1	2¼
23	⅔		2
24			1¾
25	½	½	
-29	⅔		

The Vertebræ of the Back. / The Ribs.

Num. Nº	Back Weight. lb.	oz.	dr.	Length of Spinal Process. Inch.	Breadth of Extrem. Inch.	Ribs Weight. lb.	oz.	dr.	Length of inner Surface. Feet.	Inch.	L. between Extrem. Feet.	Inch.	Breadth of Extr. Inch.
1	2	4		8	2	1	13		1	5	1	4	4
2	2	2		12	3½		12	4		9		8	3
3	2			13	4½	1	1	2		11½		10	
4	1	5					5		2	2	2		
5	1	4	2	12	3½		8	4		6		3	2½
6	1	2	3	11½	3		2	3				3½	
7		15	6	11			1	5		8		3¼	2¼
8		14		10½	2½		4	6		10		4½	
9		14		10			3					5	
10							15			9		3	
11							14	4		7			
12		13		9½	2		13	4		5		1	
13				9			12			4			
14					½		10	6		½	1	11½	
15				6½	1½		9					10	1
16				6	2		5	5	1	9		8	
17				5			5			8		7	
18			2	4½			3	2		5		4½	
19			4	3			2	1					
	20	8	7			16	15	6					
						16	15	6					

The Vertebræ of the Loins.

1	13	4	3
2	12	7	
3	11		
	2	5	3

	lb.	oz.	dr.	
Weight of Ribs	33	15	4	
Tail	4	2	7	19

The Skeleton of the Elephant consists of the Bones of

		lb.	oz.	dr.	scr.
The head divided into those of the					
Upper jaw, viz.					
Calvaria, or upper and back part	1				
Frons, or upper and fore part	1				
Two maxillary bones	2				
Two bones of the palate	2				
Two zygomatic bones	2				
Two styloid processes	2				
Two Tusks	2				
Four grinders	4	66			
Lower jaw	1				
Four grinders	4	45			
	21	111			
The Trunk composed of the					
Spine consisting of the vertebræ of the					
Neck	7	7	10	6	
Back	19	20	8	7	
Loins	3	2	5	3	
Os sacrum	5	4	0	0	
Tail	29	4	2	7	1
Ribs, 19 pairs	38	33	15	4	
Sternum	4	3	0	0	
	105	75	11	3	1
The Fore Extremities					
Scapula	2	19	8	0	0
Humerus	2	16			
Cubitus and Radius	4	15			
Carpus, six on each foot	12	3	12		
Metacarpus	12	3	4		
Toes	24	2	3		
Ossa sesamoidea	24		6		
	80	60	1		
The Hind Extremities					
Ossa Innominata, viz. Ilion, Pubis	2	28	0		
Femur, or thigh bone	2	16			
Tibia and Fibula, or leg and spit bone	4	13	4		
Patella, or knee pan	2		12		
Tarsus	12	5	10		
Metatarsus	12	1	6		
Toes	20	1	2½		
	54	66	2 ½		
Sum total	260	312	14	7	1

By the opportunity I have had of preparing and joining these bones, it may be expected I should give some account of their structure: but as the design of preserving the skeleton entire gave me no liberty to go any further than their external surface, so it cannot be expected I could dive any deeper in the knowledge of them. Tentzelius says, omnia isthæc ossa porosa sunt et rimosa; and I may add, levia too: for there is nothing about them to be seen of that solidity and compactness, that smoothness of surface, and whiteness, which is observable in other quadrupeds of the larger size, such as oxen, horses, harts, &c. or smaller, as sheep, dogs, cats, &c. And I should have readily attributed this to the youth of the animal, had not Tentzelius from his subject, supposed to be 200 years old, told the same. And this differs much from the account of the Behemoth in Job, whose bones are said to be as strong pieces of brass, and bars of iron. The laminæ of the head were thin and solid; the external table thin and more ponderous; the teeth exceedingly solid and ponderous: so that from the computation of the weight of the upper part, which was taken off by the saw, which is only 6lb. weight, I may reckon all the head, which weighs 66lb. beside the teeth, not to weigh above 24lb. at most; which well agrees with what Tentzelius says, that each of the dentes molares were 12lb. weight, and that of all the 45lb. which the lower jaw weighs, the rest of the bone beside the grinders do not exceed 12 or 16lb. For its external surface seems to be both porous and rimous; and at perforating the condyles seemed to be very spongy, as were the ribs, femur, tibia, &c. where, after the drill had passed the external lamina, which was very thin, it would have run forward as if it had been through so much moss. When the epiphysis came off the thigh bone, it resembled very much the epiphysis of the femur in man; its minute cellules were not so large as those of an ox, and the laminæ which circumscribed them, not by much so solid. The humerus indeed, both above and below, was much harder; it heated the drill in passing: and there may be some reason for that too; viz. that since the progression of most quadrupeds chiefly depends upon a more frequent motion of the fore than hind limbs, it does much more here, where the head is proportionably heavier than in other animals. And this perhaps is the reason too, why the fore limbs in this animal are brought so far forward; for measuring in a straight line from the humerus above to the carpus below, and bringing another line directly backward at the articulation between the humerus and cubitus, from the perpendicular line before to the point of the olecranon behind, it is 20 inches; which is the reason why some believe my engraver has made the fore limbs of the skeleton to bend too much at the articulation. The bones of the carpus are pretty solid, and by perforation they seem only to have a little spongiosity about the middle: all the rest of the bones of the fore foot are spongy. The astragalus, os naviculare, and ossa cuneiformia, are more solid; but the talus and other bones of the hind foot spongious. The spine was spongy, as is usual; the ossa innominata of a middle consistence; and the scapula very thin, but solid towards its neck. I cannot positively determine the cavities for the marrow, nor quantity of it; but by comparing the dimensions with the weight and small quantity of fat to be seen at the boiling, we may suppose it not to have been much in this animal: I know not how it may be in others of this species.

I must not forget to tell you, that when I weighed the bones, it was immediately before they were joined; so that their weight was much diminished, in respect of what it was when they were newly boiled. The weight is ℥16 to 1lb. and the measure, according to the English yard, 12 inches to a foot, and 12 lines to an inch.

The Explanation of the Plates.

Plate xiii. fig. 9, represents the stuffed skin of the Elephant, with an account of its particular dimensions; AA the height of the elephant at the fore feet, was 8 ft. 6 inc.; BB its height at the hind feet 9 inc.; cc its length 10 inc.; CD the length of the tail 4 ft. 3 inc.; EE the circumference of the belly 14 ft.; FF from the top of the head to the point of the proboscis 8 ft.; GF the length of the proboscis 4 ft. 6 inc.; HH the distance between the forehead and lower jaw 2ft. 3 inc.; FI from the top of the head to the lower jaw 4 ft. 6 inc.; KK the length of the ear 1 ft. 7 inc.; LL its breadth 1 ft. 5 inc.; M the orifice of the meatus auditorius; NN the circumference of the fore foot round the hoofs 3 ft. 10½ inc.; a the fore hoof fore-shortened 5 inc.; b the middle external hoof 5 inc.; c the third external hoof 4½.—Note, That neither the diameter of the fore foot from before to behind, which was 1 ft. 4½ inches; nor from the right to the left, which was 1 ft. 2 inc. can be so here; oo the circumference of the fore foot at the upper joint 4 ft. 3 inc.; PP at the articulation with the carpus 2 ft. 6½ inc.; QQ the circumference of the hind foot round the hoof 3 ft. 4 inc.; a the breadth of the fore hoof 3 inc.; b the breadth of the outer hoof 4 inc.; c the breadth of the third hoof 4 inc.; RR the circumference of the hind leg 2 ft. 2 inc.; s the mouth; T the tusks broken off by the middle; U the eye; X represents the scabs about the belly; YY the depressions in the skin through the folding of the proboscis; a a protuberance first occasioned by the ossa innominata, when the animal was alive and very lean, and still remaining in the skin; b a protuberance in the fore part of the thigh; c the lower joint of the fore foot, where there is a depression in the skin; dd several wrinkles in the stuffed skin.

Fig. 10, represents the skeleton of the elephant, as it was mounted by my direction, and now stands in the repository of rarities in Dundee; A the scull taken in profile, whereby a part of the fore-side is foreshortened; a the hole for the root of the trunk foreshortened; bb the two ossa palati; cc the 2 tusks as they proceed from the ossa palati; dd the broken off extremities of the tusks; e the grinders of the upper jaw; f the fore grinder of the lower jaw; g the undulating lines of the lower surface of the grinders of the upper jaw; h the inner grinder of the lower jaw; i part of the os malæ; k its articulation with the os zygomaticum; l the os zygomaticum; m the orbit of the eye; n its upper protuberance; o its middle protuberance where the trochlea is inserted; p its lower protuberance; q a sinus at the bottom of the orbit; rr a depression fit for lodging the muscles of the lower jaw and proboscis;

s the orifice of the meatus auditorius; tt the articulation of the os calvariæ with the os zygomaticum; u the processus coronæ of the lower jaw; x the insertion of the musculus masseter; y the space for the mouth betwixt the os palati and lower jaw; z an orifice from which the nervus maxillaris inferior proceeds; B the vertebræ of the neck; 1 the first vertebra; 2 the second vertebra, or tooth which rises higher than the rest; 3 the third vertebra having scarcely any spinal process; 4 the fourth, whose spinal process is not yet seen; 5 the fifth, with the spinal process beginning to appear; 6 the sixth, whose spinal process ascends higher, and is remarkable at its fore part, as in table 4; 7 the seventh, whose spinal process still ascends, and with whose back part the first rib is articulated; c the vertebræ of the back; 1—13 their processus spinosi, which have no protuberance at their extremity, whereof 1—c are the longest, and c—13 become gradually shorter; D the spinal processes of all the rest of the vertebræ to the os sacrum which are shortned by degrees; xxxxx &c. the oblique processes of the vertebræ; E the scapula; aa the spongious margin of the scapula; bb its processus spinosus sending forward a protuberance; cc its neck; dd the epiphysis which receives the humerus; ee &c. the ribs; ff &c. the ribs which appear on the opposite side; gg the cartilages of the sternum; hh the bones of the sternum; iii the three ribs which have no cartilages; lll the bodies of the three vertebræ lumborum; F the humerus; 1 its upper part, spongious and rugous, for the insertion of tendons; 2 its middle part more solid; 3 a large oblique sinus for lodging the biceps; 4 its lower extremity articulated with the cubitus and radius; G the cubitus and radius; 1 the olecranon; 2 a hollowness on the outside of the cubitus; 3 the radius; 4 its lower epiphysis, rugous, and separated from it by a suture; 5 the lower epiphysis of the cubitus, separated likewise by a suture; 666 three bones of the first rank of the carpus; 777 three bones of the second rank; 8, 8 the bones of the metacarpus; 9, 9 the first bones of the toes; 10, 10 the second bones of the toes; HH the ossa innominata represented in profile; I the pelvis; K the tail; LL the two thigh bones; 1 the epiphysis received by the ossa innominata, and articulated with the femur by a suture; 2 the trochanter major; 3 the lower epiphysis; 4 the patella; M the tibia; 1 perone; 2 the talus; 3 the bones of the tarsus; 4 the bones of the metatarsus; 5 the bones of the toes.

Fig. 11, represents the fore part of the head; a the hole for the root of the trunk; b the lower part of the os palati, over which hangs the proboscis; cc a depression of the bone on each side, for lodging of the muscles of the lower jaw; dd the two eminences on each side at the top of the head; e a depression in the middle betwixt these two eminences; ff two beginnings of the angles for forming the depressions for the muscles of the lower jaw, between which the surface of the bone begins to be plain; g the upper production of the sinus where the eye is lodged; h the beginnings of the laminæ which run between the two tables of the skull, and here appear in the bottom of the hole for the root of the proboscis; i the os vomeris, to which the cartilaginous septum of the proboscis was adherent; k the beginning of the depression of the os palati; l the middle of the sinus for the orbit of the eye; m the articulation of the two ossa palati; nn the articulation of the os maxillæ with the os palati; where also is a crena for containing the blood vessels, as they go to the nourishment of the proboscis; oo the place where the tusks proceed from the os palati; pp the upper part of the articulation of the os maxillæ with the os palati; qq the broken extremities of the tusks; rr a great oval hole in the os maxillæ, through which a considerable branch of the fifth pair of nerves, and a large artery from the arteria duræ matris pass to and are dispersed in the proboscis, and by which a large vein returns and joins to the vena jugularis; s the os zygomaticum; t the middle production for the orbit of the eye.

Fig. 12, represents the side of the head; a the beginning of the depression for the muscles of the lower jaw and proboscis: b the insertion of the retractores proboscidis; c the insertion of the musculus temporalis; d the bottom of the orbit of the eye; e its upper production; f its lower production; g the articulation of the os maxillæ with the os zygomaticum; h the os zygomaticum; i the articulation of the os zygomaticum with the os calvariæ; k the orifice of the meatus auditorius; l one of the condyles of the occiput, which is articulated with the first vertebra; m the orifice of the large oval hole in the os maxillæ; n the fore grinder in the upper jaw; o the hind grinder, or rather wedge for keeping the fore grinder fast; p the undulate lines in the lower surface of the teeth; q the beginning of the tusks as they proceed from the os palati; r their broken off extremities; s the sinus in the bottom of the orbit of the eye for the nervus opticus.

Fig. 13, represents the back part of the head; aa the two eminences at the upper part of the head enlarged, whereby the sinus between them becomes narrower and deeper; b the sinus between these eminences shortned; cc the two condyles which are received by the first vertebra; d the hole for the spinal marrow; ee two protuberances above the meatus auditorius; f the orifice of the meatus auditorius; g a sinus whence the processus styloides arises, which is shown by itself; h the cartilage whereby the processus styloides is articulated with the skull; i its longest and smallest part; k its shortest and largest part; l the orifice for the hard portion; mm the hole for the jugular vein and par vagum; nn the bony part of the aqueduct; oo the extremity of the aqueduct where the fleshy part begins; pp the hole for the carotid artery; qq the hole for the arteria duræ matris, and 3d branch of the 5th pair; r the middle of the base of the skull beneath the hole for the spinal marrow, where the bone is somewhat raised; s a depression on the base of the skull before the choana begins; t the choana, or passage between the root of the trunk and the mouth; u a production of the vomer, or septum, which divides the choana in two; x the articulation of the os zygomaticum with the os occipitale; y the glenoid cavity for reception of the lower condylus of the lower jaw; z the sinus for the globe of the eye; 1 the os zygomaticum; 2 the fore grinder on the right side; 3 the hind grinder on the right side; 4 the hind teeth on the left side, which not grinding at all, only serve as a wedge; 5 the fore teeth on the left side, the back part of which does not grind; 66 the lower surface of the grinders, where their undulate lines appear; 77 part of the os maxillæ, where it is articulated with the os zygomaticum; 88 the great oval hole in the os maxillæ; 99 the back part of the os palati; 10 the interstice between the ossa palati on the back side; 1111 the tusks as they proceed from the os palati; 1212 the two broken off extremities of the tusks.

Fig. 14, represents the skull sawed transversely, so that its lower part with the base appear; aa the outward table of the skull; bb the inner table; cc the laminæ which pass between the two tables; dd the cells formed by these laminæ; ee the orifices for vessels which penetrate the laminæ; f the seat of the brain, represented at more length in fig. 3; gg the two condyles which are received by the first vertebra; h the hole for the spinal marrow; i the os zygomaticum.

Fig. 15, represents the upper part of the skull sawed transversely, with the cells running between the two tables and laminæ which cover the seat of the brain.

Plate xiv, fig. 1, represents the outside of the upper part of the skull, sawed transversely; aa two eminences on the top of the skull; b a sinus between these two eminences; c a long spine in the bottom of the sinus.

Fig. 2, represents the fore part of the lower jaw; aa the two condyles; bb the two processes of the corona, shortened by the opposite view; cc the fore grinders of the lower jaw; d the distance between the two jaws for lodging the tongue; e the symphysis menti.

Fig. 3, represents the back parts of the lower jaw; aa two condyles; bb two large orifices of a cavity, into which the vessels for nourishing the teeth enter, and wherein are lodged the rudiments of the teeth, as in pl. ix, fig. 1, cc the two fore grinders of the lower jaw; dd the undulated lines in their upper surface; e the distance between them for lodging the tongue; f the concave part of the lower jaw.

Fig. 4, represents one side of the lower jaw; aa the two condyles; bb the two processes of the corona; c a protuberant part of the lower jaw, where the rudiments of the teeth are lodged; d the inner grinder of the lower jaw; e the outer grinder, where the ridges and interstices of the sides of the teeth are represented; h the symphysis menti.

Fig. 5, represents the bony part of the meatus auditorius of the right ear; a the external orifice of the meatus auditorius; b the processus petrosus; c the orifice where the auditory nerve enters; d the meatus auditorius; e a part of the laminæ, which proceed from it on each side, and by which the cellules between the two tables of the skull are formed, those situated above the meatus being removed; f part of the inner table of the skull.

Fig. 6, represents part of the meatus auditorius opened, with other parts of the inner ear; a the ragged part of the bone from which the os petrosum was separated; b the processus petrosus opened; c the crena for the membrane of the tympanum; d the honey-comb cavity of the tympanum; e its inner cavity, of a smooth surface; f its semicircular, or undulated lines; g the orifice of the aqueduct; h the orifice of the portio dura of the nerve.

Fig. 7, represents the lower surface of the os petrosum, as it was separated from the upper part of the tympanum, and other parts of the inner ear; aa the ragged margin of the bone; bb the upper part of the cavity of the tympanum; c the foramen ovale; d the protuberance, wherein the labyrinth and cochlea are lodged; e the orifice of the portio dura of the auditory nerve.

Fig. 8, represents the malleolus alone in its true dimensions; 1 the protuberant head; 2 the semicircular sinus between it and the margin; 3 the sinus, which receives the head of the incus; 4 the angle below the sinus, for the head of the incus; 5 the angle where the manubrium malleoli begins; 6 the manubrium malleoli.

Fig. 9, represents the incus; 1 the head of the incus; 2 the sinus, or neck of the incus; 3 two apophyses; 4 a long protuberance with the sinus for the os quadrangulare at its extremity.

Fig. 10, 11, 12, represent the stapes; 1 the small part of the stapes, where it is articulated with the incus, with a sinus at its extremity, being the other half of the cavity for the os quadrangulare; 2 2 two small portions of the stapes, where it is articulated with the basis.

Fig. 11, the basis of the stapes separated.

Fig. 12, the whole stapes.

Fig. 13, the malleolus and incus joined together, with their lower side turned up; 1 the malleolus; 2 its articulation with the incus; 3 the incus; 4 the manubrium malleoli; 5 a point of the incus, formed by the other two productions; 6 the long protuberance of the incus; 7 the sinus in the extremity of its long production.

Fig. 14, the malleolus, incus, and stapes, articulated together; 1 the incus; 2 the malleolus; 3 the stapes, where it shuts up the foramen ovale.

Fig. 15, represents the upper part of the lineæ semilunares, or that side which is towards the passage of the auditory nerve; a the five extremities cut off; b the linea semilunaris major; c the semilunaris media; d the semilunaris minor, which is towards the cavity of the tympanum; e the common canal to the major and minor; f the major; g the media; h the cochlea.

Fig. 17, represents the cochlea.

Fig. 18, the vestibulum; b the third gyration, or turning; c the orifice; d the first gyration, or turning opened; e the second turning; g the orifice at the top of the cochlea.

Fig. 19, represents the seat of the brain enlarged, that the orifices for the blood-vessels and nerves may be the more obvious; aa the inner table without its surrounding cellules; bb the anterior sinus; cc the os ethmoides, with its eminences, sulci and foramina for the olfactory nerve; d the crista galli; ee the anterior eminences; ff the orifice for the optic nerve; gg the hole called the foramen lacerum, through which pass the nervi motorii pathetici ophthalmici, or first branch of the fifth and sixth pair; hh the second branch of the fifth pair; ii the third branch of the fifth pair; kk the hole for the artery of the dura mater; ll the hole for the carotid artery; mm the hole for the auditory nerve; nn the hole for the jugular vein; o the hole for the spinal marrow; pp part of the two condyles; q the external hole for the spinal marrow; rr the two middle fossæ; ss the processus petrosus; tt the posterior fossa, or seat of the cerebellum; u the seat of the pituitary gland; x the crena.

Fig. 20, represents the uterus; aa part of the ligamenta lata uteri; b part of the vagina cut off; c the beginning of the body of the uterus; d divided into two portions, with an interstice in the middle; ee several eminences, representing the external part of so many cellules; ff the cornua uteri; g a loose membrane wrapped up, that the ovaria below may appear; h the ovarium stripped of the loose thick membrane, which fluctuates above it; i the ovarium covered with the membrane.

Fig. 21, represents the proboscis cut transversely; aa the two cavities of the proboscis; b the septum, which divides the cavities; cc the tendinous intersection, which runs from the fore to the hinder part; dd the tendinous intersection which runs from the right to the left; eeee the insertion of the four muscles into the tendinous intersection, whereby the fibres of the one ascend, and those of the other descend obliquely.

Fig. 22, represents the dissected proboscis; a the external part of the cartilage, which surrounds the cavity of the proboscis, as it arises from the hole in the fore part of the skull; bb that pair of muscles called the levatores proboscidis, raised from above the aforesaid cartilages, with their inner surface turned up, that the divarications of the blood-vessels therein may appear; cc the orifices of the veins dispersed in these muscles; dd the orifices of the arteries; ee their several branchings; f the descent straight along, above the cavity of the proboscis; gg the oblique descent of the fibres of the erectores proboscidis; h the tendinous intersection running down the middle of the proboscis; ii the orifices of the cavities of the proboscis.

Fig. 23, represents the extremity of the proboscis cut off; a a protuberance arising from the fore part of the extremity of the proboscis, and extended into a cavity in the back part b, whereby the animal takes hold of any thing.

Plate xv, fig. 1, represents one of the rudiments of the teeth, taken out of the large hole in the inner side of the lower jaw, but much enlarged in proportion; a its upper part, which is hard, solid and white; b its middle part distinguished by several furrows and ridges; c its lower part, which is hollow, and into which enter, both the blood vessels, that serve for its nourishment, and a branch of the nerve, called maxillaris inferior, proceeding from the fifth pair.

Fig. 2, represents a portion of the cuticula, wherein is shown its inner surface, and usual thickness; at its margin on the left hand, and lower part, are several white lines, which Mr. Blair takes to be the lineaments of so many blood vessels; the pyramids, from whence the hairs proceed, with the several favi or depressions.

Fig. 3, represents one of the scabs, adhering to the cuticula, where they are thickest.

Fig. 4, the first vertebra of the neck, with its upper part in profile, to show the holes for the vertebral artery; aa two protuberances, which reach on each side to the skull; bb two cavities shortened, which receive the condyles of the skull; cc the two holes, whereby the vertebral artery proceeds from the skull, and perforates this vertebra; dd two holes through which the artery passes out from this vertebra; ee a crena between the two aforesaid holes, where the artery is lodged.

Fig. 5, the fore part of the first vertebra shown at large; a the hole for the spinal marrow; b the hole for receiving the tooth of the following vertebra; cc two cavities for receiving the condyles of the skull; dd two holes for the cervical artery; e the upper part of the vertebra; f its lower part; gg the transverse processes, whose protuberances at the extremities are represented by aa.

Fig. 6, the back part of the first vertebra shown at large; a the hole for the spinal marrow; b the hole for the tooth of the following vertebra; cc the cavities which receive the body of the following vertebra; d the lower part of the vertebra; ee the holes for the cervical artery; ff the two transverse processes.

Fig. 7, the fore part of the second vertebra; aa the forked extremities of the protuberance, which arises instead of the spinal process; b a sinus between them; c the hole for the spinal marrow; d the tooth, which is received by the first vertebra; ee the two convex surfaces, which are received into the hinder cavities of the first vertebra; ff the two holes for the cervical artery; gg two transverse processes; h the lower part of the vertebra.

Fig, 8, the back part of the same vertebra; aa the protuberances of the spinal process; b the sinus between them, enlarged on the side; c the hole for the spinal marrow; d the point of the tooth appearing from the other side; ee the holes for the cervical artery; f the concave body of the vertebra, which receives the convex surface of the following vertebra; gg the transverse processes; hh the two oblique processes, which receive the oblique processes of the following vertebra.

Fig. 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, the five following vertebræ are represented by AB, whereof A represents the fore part, B the back part; aa &c. the hole for the spinal marrow; bb &c. their convex bodies, which are received by the concave surfaces of the following; cc &c. their concave bodies, which receive the convex surfaces of the former; dd &c. the holes for the cervical artery; ee &c. the oblique processes; ff &c. the transverse processes; gg &c. the spinal processes, which in the fore part of the third and fourth vertebræ, fig. 9, 11, scarcely appear, but in their back part. Fig. 10, 12, appear a little, in 5, fig. 13, 14, arise to one inch and a half, and in 6, fig. 15, 16, to three inches; hh, in 6 and 7, fig. 15, 16, 17, 18, are protuberances, which run back to guard the cervical artery, as it passes from between the bodies of the vertebræ, and quits the perforation in their transverse processes; ii two sinus's in the back part of the seventh vertebra, which with the like surfaces in the following form a cavity, into which the condyles of the first ribs are received.

Fig. 19, represents the scapula; a, the head of the scapula, whereby it is articulated with the humerus; bb, the two protuberances on each side of its head; c the neck of the scapula; d a sinus between the processus coracoides, and the neck of the scapula; e the processus coracoides of the scapula; f the processus spinosus; g the extremity of the processus spinosus; h a protuberance running forwards from the processus spinosus; i the fore part of the upper edge of the scapula; l a thick spongy epiphysis, which (at the upper edge of the scapula) was separated by boiling; m the angle at the back part of the scapula.

Fig. 20, represents the lower or fore part of the seven vertebræ of the neck; a, b, c, &c. the lower or fore part of the bodies of all the vertebræ; hh, the transverse processes, which run obliquely forwards; ii, the transverse processes of the sixth vertebra, running both before and behind to guard the cervical artery; k, a sinus in the body of the seventh vertebra, for receiving a part of the first rib.

Fig. 21, represents the fore part of the ossa innominata; A the pelvis four feet six inches in circumference; B the os sacrum; C the upper part of the os pubis; BC between the os sacrum and the os pubis, one foot six inches; DE from the right to the left of the pelvis, one foot five inches; CF, from the upper to the lower part of the os ilium, one foot; GH, between the two outer extremities of the ossa innominata, three feet six inches; BH from the os sacrum above to the aforesaid point, two feet nine inches and a half; from H to K, one foot; L the circumference of the acetabulum one foot six inches; EH the breadth of the os ilium one foot one inch; MM, the circumference of the neck of the ilium, one foot two inches; NN the breadth of the os pubis, eight inches; OO, the length of the foramen ovale for the musculus marsupialis, five inches and a half; PP its breadth four inches; QQ its circumference one foot one inch; RR the breadth of the os pubis before, one foot five inches; ST the length of the os sacrum, from where it is joined with the vertebræ of the loins, to where it is joined with the tail.

Fig. 22, represents the back part of the ossa innominata; A the pelvis; B the back part of the os pubis at their articulation, where there is a large cavity; CC, the oval hole for the musculus marsupialis fore-shortened; D the cavity for the acetabulum; EE the margin of the os ilium, which was separated by boiling; F the back part of the ossa innominata, showing the spinal, and oblique processes.

Plate xvi, fig. 1, represents the back part of the fore foot; aa the bones of the carpus; bb the bones of the metacarpus; cc the ossa sesamoidea, of which there are two upon the lower extremity of each bone of the metacarpus; dd the bones of the toes.

Fig. 2, represents the back part of the hinder foot; a the talus; b part of the astragalus; cc bones of the tarsus; dd bones of the metatarsus; ee bones of the toes.

Fig. 3, represents the bones of the carpus separately; 1. The upper surface of the external bone of the first phalanx of the carpus; 2, the middle bone; 3, the third bone of the first phalanx; 5, 6, the upper surface of the three bones of the second phalanx.

Fig. 4, represents the bones of the tarsus separately; 1, the upper surface of the astragalus; 2, the upper surface of the os naviculare, much enlarged in proportion to the rest; 3, its lower surface; 4, 5, 6, the upper surface of the ossa cuneiformia.

Fig. 5, represents the concave side of the liver; a the vena portæ; b the vena cava.

Fig. 6, 7, represents the os hyoides; a the fore part.

Fig. 7, b the back part; dd the cartilago scutiformis; ee the lateral ossa hyoidea; ff the bones of the base of the os hyoides.

Anatomical Description of the Heart of the Land Tortoise from America. By Mr. Paul Bussiere, Surgeon, F. R. S. N° 328, p. 170.

The heart in this animal is situated in the anterior part of the capacity that forms the abdomen, separated from all the other viscera by a large pericardium, which encloses it. This pericardium is fastened by its superior part to the spine of the back, and by the anterior, to the muscles of the neck; which is the cause that the heart moves forward when the animal puts his head out of the shell, and backward when he draws it in: by the inferior part it adheres to the peritonæum, which is fastened to the lower shell; so that by all these ligaments the pericardium is kept sufficiently distended, and the heart has an entire liberty in it. In this pericardium there is a considerable quantity of transparent water, having the same use there, as that found in the pericardium of other animals.

It is in the middle of this pericardium that the heart is suspended; viz. at its basis by the arteries, and at its inferior part by a small tendon, or a very thin ligament, which from the point or cone of the heart, ascends to insert itself to that part of the pericardium which adheres to the back. This small ligament is remarkable, because by its means the point of the heart is suspended on the level of its basis; without which the point of the heart would fall lower, and bend the vessels of the basis, which might have interrupted the free circulation of the blood, and consequently would have endangered the life of the animal.

The pericardium being opened, the heart appears as if standing by itself, being only fixed to the arteries which go out of it (supposing the animal turned upon its back) its auricles being separated and hid under its basis and arteries, towards the back of the animal; which is very different from the sea tortoise, where there the auricles are situated on the right and left angle of its basis, by which way they push the blood into the heart. The figure of the heart of this animal is almost lenticular; making however three obtuse angles, two on the basis, one to the right, and the other to the left; the third is at the inferior part, where the small tendon, which suspends the heart on the level of its basis, is inserted. If this animal be opened alive, we have the satisfaction to see the circulation of the blood, by reason of the transparency of the membranes of the veins, and the alternate motions, or dilatations of the heart and auricles, and the arteries and veins, which are very slow in this animal.

From the basis of the heart pass out four great arteries, that appear distinctly separated one from the other; whereas in that of the sea tortoise, these arteries are involved, for the length of an inch, in a capsula common to them all, which makes them appear as if they were but one trunk. If these four arteries be entirely cut, the heart is no more suspended, but by the conjunction of the two musculous conduits of the auricles, which pierce the heart in its posterior part, towards the middle of the heart, on the left side; by which the blood runs from the auricles into the ventricle of the heart. These arteries being thus divided, and the heart turned over, the auricles appear lying transversely against the back, in the capacity of the pericardium: they make but one continued fleshy body, a little extended, about two thirds inclining to the left side. It is in this body that the cavities of the auricles are separated from each other, by a muscular septum, situated internally to that place, which appears contracted externally. These auricles make a muscular production about six lines long, which unites them to the heart, towards the middle and left on its backside. This production is composed of two conduits, separated only by the extension of the septum, which divides the two auricles: it is by these two conduits that the blood flows from the auricles into the heart. The body of these auricles has no adherence to the pericardium, nor any support, except that of the veins, which end in it; for if you divide these veins, the heart and auricles come out of the body; and then if you suspend the heart by the auricles, they resemble two funnels joined together, the small end of which opens into the ventricle of the heart, to pour the blood into it: and it is in this manner we are to conceive them, in the natural situation of the animal.

In order to examine the inside of the heart and its auricles, it must be opened at its inferior surface, supposing the animal turned on its back, because all the orifices either of the arteries or veins, and their valves, are in the opposite side: therefore a probe may be introduced through one of the arteries into the heart, and so opened upon it; after that, you cut all this side round about the inferior circumference, from one angle to the other, and then turn over all that part which is cut on the basis of the heart: for then it is easy to view all the internal parts of the heart, and observe that there is but one only ventricle, which comprehends the whole extent of the heart, and is as uniform and plain as either of the ventricles of the human heart, or of any other animal whatever; and that it is impossible to remark any kind of septum, either musculous or membranous, that might make any division or cellule in this ventricle: so that it is very surprising, that the anatomists of the Royal Academy of Paris should have shown, the one three, and the other four ventricles, in the heart of a land tortoise of America.

After having considered the extent of the cavity of the heart, there remain two things to be examined. The first is, that in its back part there are five holes or orifices, two of which are on the left side: these are the orifices of the two funnels of the auricles: they are covered by a large valve lying flat upon them, supported in its middle by the prolongation of the septum, which divides the auricles, in such a manner, that half of it covers the orifice of the right auricle, and the other half that of the left; so that this valve resembles two folding doors of a porch, which have the same support, the one opening or shutting to the right, and the other to the left. It is plain that this valve permits the entrance of the blood into the ventricle of the heart, but opposes its return into the auricles; because this blood being once in the heart, presses by its own weight on this double valve, and keeps it close and flat upon these orifices: which confirms perfectly well the office of the valve, in the foramen ovale in the heart of a human fœtus, the disposition being entirely the same. The other three holes, lying on the right side of the ventricle of the heart, are the orifices of the four arteries which come out of the basis: of these three holes, that which is most to the left, is the orifice of the pulmonary artery; the highest, the orifice of the aorta sinistra descendens; and that most to the right side, is common to the arteria aorta dextra, and to the superior aorta. Each of these orifices is furnished with two semilunary valves, which permit the blood to pass without difficulty from the ventricle of the heart into the arteries, but hinder its return into the heart. It is a mere illusion to place these holes in different ventricles; they are all in one and the same cavity; so that the blood enters into this only cavity by the two holes which are on the left side, and goes out of this same ventricle, by the three holes which are on the right side.

The second thing remarkable in this ventricle, is the fibres of the heart. They are of two sorts; some are external, disposed under the common membrane in several planes, very small, but obliquely circular, extending from the basis, but particularly about the arteries, which serve them instead of tendons, or points of support, towards the inferior circumference of the heart: the other muscular fibres, which compose the heart, are in the manner of several columns, as those of the human heart; they are situated internally in both sides, lying obliquely from the right, where their tendons are about the arteries, to the left; which demonstrates that their action is from the left to the right side, where the orifices of the arteries lie open, to let the blood pass out.

It has been said before, that the two auricles of the heart of the land tortoise of America, make externally but one continued body; but that it has internally two cavities, separated from each other by a muscular septum. This septum separates them so exactly, that there is not the least communication between them; so that the blood of either auricle does not mix with that of the other, except in the ventricle of the heart. The right auricle is as large again as the left; all the blood of the animal (that of the lungs excepted) passing through it, to go into the heart; the left auricle receiving only the blood which comes from the lungs, the pulmonary veins being very small. The internal part of the auricles are furnished with small muscular columns, but particularly at their extremities, and situated in such a manner, that it is plain their action tends to push the blood against the septum, where the conduits, which convey it into the heart, are situated.

There is in the bottom of the right auricle an oblong orifice, by which the blood comes into its cavity from the great reservoir of the veins, situated on the back part of the heart. This orifice is furnished with two semilunar oblong valves, disposed in such manner, that when the auricle is relaxed, the blood enters its cavity, but when contracted, they shut close, to hinder the blood from returning into the veins: the orifice of the funnel, or the conduit into the heart, is to be seen against the septum. The left auricle has exactly the same structure as the right. It is in the bottom of this auricle, that the orifice, common to the two pulmonary veins, is to be observed, furnished with two semilunar valves; and against the septum to the right, that the funnel or conduit into the heart is situated, joining with the funnel of the right auricle. These two conduits are separated from each other by the continuation of the septum, which divides the auricles to the very ventricle of the heart, and is as a support to the middle of the double valve, which covers their orifices in the heart.

It has been said above, that from the basis of the heart of the land tortoise of America, proceed 4 great arteries. Of these, the first which presents itself, (the tortoise being turned on his back) is the pulmonary artery: it is more on the left side than the others, and is much larger for the space of an inch; then it divides itself into two branches, of which the most apparent comes from the right side of its trunk, and turns itself over towards the left side, accompanying the aorta inferior sinistra, till it has pierced the pericardium; after that, it unites with the left branch of the trachea arteria, which it accompanies through all the extent of the left lobe of the lungs. The other pulmonary branch, going out of the left side of its trunk, turns itself over immediately across upon the other arteries, from the right to the left, to join the aorta inferior dextra, till it has pierced the pericardium, where it joins to the right branch of the trachea arteria, which it accompanies through the whole extent of the right lobe of the lungs.

One thing seems very remarkable in this pulmonary artery; viz. that though its trunk, in going out of the heart, be more than double the diameter of the arteria aorta sinistra, yet the two branches which it sends to the lungs, have not either of them one third of the diameter of the aorta sinistra. In the arteries, which I have filled with wax, the trunk of the pulmonary artery is between 7 and 8 lines diameter; the aorta sinistra 4 and a half; and the pulmonary branches, after having pierced the pericardium, have only one line and a half diameter: yet this artery does not produce any other branch; all the blood, which enters from the heart into its trunk, is carried into the two lobes of the lungs, and no where else. The reason of such disproportion I cannot guess; but this is matter of fact, since it is the same in all: yet, if I may be permitted to conjecture, it seems to me that it may be attributed to the alteration that happens to the branches of the trachea arteria, when the tortoise stretches forth his head out of the shell; for these pulmonary branches making a half circle before they join with the trachea arteria, when the animal's head is drawn in, the extension which happens to the branches of the trachea arteria, when the animal goes out of the shell, turns these half circles into sharp angles; so that thereby the passage of the blood is somewhat interrupted, and consequently the blood, which passes continually from the heart into the trunk, not being capable to return back, because of its valves, must out of necessity dilate this trunk more than the other arteries. And what persuades me that there does not go into the lungs of this animal, more blood than that quantity which the pulmonary branches can admit by their small diameter, and not the quantity which the diameter of their trunk could furnish, is, that the pulmonary veins, which bring back all the blood of the lungs into the left auricle of the heart, have not either of them quite two lines diameter, which is very proportionable to the size of the two pulmonary branches of the arteries.

The second artery proceeding from the basis of the heart, is that which I call the left aorta: it ascends, as it comes out of the heart, together with the left pulmonary, till they have pierced the pericardium; after which, it makes a large turning, without any support, towards the left side, which gives it the liberty to extend itself when the animal stretches out of its shell, and to refold itself when it retires into it; after that, this artery descends against the back, where it gives some small branches to the spinal marrow; it then returns through the lungs into the abdomen, and here it produces a considerable branch, which divides into two, of which one is distributed to the liver, the stomach, and the intestines, and the other, turning towards the right in the middle of the abdomen, unites to the aorta dextra; so that these two arteries are only one and the same branch divided into two. This same left aorta continues afterwards to the lower belly, to be distributed to the kidneys, thighs, and the parts below. It is much longer than the right, because of the great circuit it makes on coming out of the heart, to accommodate itself to the motions of the animal, and to make room for its head, which is placed under this artery in the left side, when he draws it into his shell: and it is for this reason that the left branch of the trachea arteria is longer than the right. This artery is also larger than the right aorta, because it furnishes a greater number of parts with blood. It has a distinct orifice into the ventricle of the heart, and has not the least communication with the pulmonary arteries, either in the heart or in any other part. This does not at all resemble the ductus arteriosus in the heart of a human fœtus.

The third artery from the basis of the heart of this animal, is that which I call the right descending aorta: after having pierced the pericardium, it sinks towards the back; then returning through the lungs into the abdomen, where it receives the branch of the left aorta, it is distributed to the right kidney, thighs, bladder, and parts of generation: so that I call these two arteries, the descending aortas, because they distribute the blood to all the inferior parts of this animal, the same as the descending aorta does in all other animals.

The fourth artery, from the heart, is the ascending aorta. It has an orifice in the ventricle of the heart, common with the right descending aorta: it partly appears under the left aorta coming out of the heart, and ascends in a straight line, till it has pierced the pericardium; after which it divides into three principal branches, of which the two lateral go to the fore legs, and make the carotid; the third ascends all along the trachea towards the larynx, and gives branches to all the parts of the neck.

The disposition of the arteries which go out of the heart being examined; there only remain the veins, which bring the blood into it from all the parts of the animal. But first it must be observed, that there are no veins which terminate in the heart; for all the veins open into the auricles, which are, as has been said, separated from the heart. There are two ways to show these veins without dissection: the first is to fill them with wax, by syringing it into them by their orifices in the auricles; for if we inject by the oblong orifice in the right auricle, all the veins of the body (except those of the lungs) will be entirely filled; and afterward by injecting into the oval orifice in the left auricle, the two veins of the lungs will be full at once, through the whole extent of the trachea in the lungs. The other way is to wait till the animal has expired; because the heart losing its vigour insensibly, (it beating for the space of 24 hours,) it has not then the force to discharge itself of the blood which comes from all parts into these veins, which then grow very turgid by the coagulated blood collected in them: then you need only to turn over the heart towards the neck, and cut the small coronary vein which comes out of the substance of the heart, to observe all the great veins without dissection; because they all come and terminate in a common reservatory, situated across in the capacity of the pericardium, joining to the auricles. And here we may observe a great vein, or an irregular reservatory: in the tortoises I have dissected, of 18 and 20 inches long, this reservatory was 10 inches broad, and 18 inches long; and in it the two axillary veins from the upper parts of the body, join together, after having pierced the pericardium, one on the right side, and the other on the left. From the inferior parts there join two large veins, one on the right side, and the other on the left of the inferior part of this reservatory; the first made up of all the branches which come from the right lobe of the liver, which is very large; and the other consists not only of the veins of the left lobe of the liver, but also of a vein which supplies the place of the vena cava, and which I call the vena intestinalis; because after it has received all the veins of the inferior parts of the animal, it runs all along the intestines, from which it receives the veins; and being arrived at the pylorus, it passes across the left lobe of the liver, and terminates in the common reservatory.

Besides these four large veins, there are three, but sometimes only two, coming from the middle part of the liver, which are inserted into the bottom of the reservatory; as also the small coronary vein from the heart. All these veins being thus re-united in one common place, this reservatory terminates upwards in a conduit, inserted into the posterior part of the right auricle, and opens into its cavity by an oblong orifice, furnished with two long semilunar valves, which permit the blood of the reservatory to enter into the auricles, but hinder its returning from the auricles into the reservatory. A little above the reservatory, under the left auricle, are seen the two pulmonary veins: the left, after having entered the pericardium, is hidden under the axillary vein, and does not separate itself from it till a little above the auricles; from thence it bends to be inserted into the posterior part of the auricles. The right pulmonary vein follows after the same manner the right axillary, which it quits after it has entered the pericardium, to traverse almost all the length of the reservatory, and meet the left pulmonary about two lines distance from the auricles. These two veins thus united, open into the posterior part of the left auricle, by a common oval orifice, furnished with two semilunar valves; by which means they pour into this auricle all the blood that comes from the lungs to the heart.

By all that has been observed concerning the structure of the heart of the land tortoise of America, and the disposition of both its auricles and vessels, how extraordinary soever it may appear, it is impossible to find out the least thing that may affect the opinion of Dr. Harvey, and all other anatomists, about the manner of the blood circulating in the heart of a human fœtus, and the use of the valve at the foramen ovale; which is, to permit the blood to pass from the right auricle through this hole into the left, and to hinder the blood's passing from the left auricle by this hole into the right. And I add further, that among all the known animals, we could not chuse one whose heart may be more proper to confirm this opinion, than the land tortoise of America, by reason of the simplicity of its structure, and of the plain and distinct manner in which all the parts appear.

Explication of the Figures.—Plate xvi. fig. 8, aaa represent the heart; bbb its auricles; c the trunk of the pulmonary artery; d the arteria aorta descendens sinistra. E the superior aorta; H the right descending aorta; F the ligament that suspends the cone of the heart in the pericardium; GGGGG the pericardium laid open.

Fig. 9, aaa represent the heart opened, to show the parts of its ventricles; b the double valve covering the orifices of the ductus from the auricles; c the orifice of the right auricle I; d that of the left H; e the orifice of the pulmonary artery K; f that of the left aorta L; g the orifice common to both the right aorta M, and the superior aorta N.

Fig. 10, aaa represent the auricles; b the right auricle; c the left auricle; dd the muscular septum, that divides the cavities of the auricles; E the orifice of the reservoir of the veins; F the orifice of the pulmonary veins; gg the large parts of the funnels; H the muscular duct of the funnels; ii the reservoir of the veins; K the left axillary vein; L the right axillary vein; M the great intestinal vein; N the great hepatic vein; oo two small hepatic veins; P the right pulmonary vein: q the left pulmonary vein.

Fig. 11, a the heart of the tortoise; b the trunk of the pulmonary artery; ccc &c. the branches of the pulmonary artery, accompanying the bronchia in the lungs; dddd the left descending artery; eeee the right descending artery; ff a branch of the left aorta, which communicates with the right aorta; gg the intestinal artery; h the superior or ascending aorta; i the ligament that suspends the heart; K the trachea; LLLL &c. the two branches of the trachea going to the lungs.

An Argument for Divine Providence, taken from the Constant Regularity observed in the Births of both Sexes. By Dr. John Arbuthnot, Physician in Ordinary to her Majesty, and F. R. S.* N° 328, p. 186.

Among innumerable evidences of Divine Providence to be found in the works of nature, there is a very remarkable one in the exact balance maintained between the numbers of men and women; for by this means it is provided, that the species may never fail, nor perish, since every male may have its female, and of a proportionable age. This equality of males and females is not the effect of chance, but Divine Providence, working for a good end, which I thus demonstrate.

Let there be a die of two sides, M and F, which denote cross and pile. Now to find all the chances of any determinate number of such dice, let the binomial $M + F$ be raised to the power whose exponent is the number of dice given; then the coefficients of the terms will show all the chances sought. For example, in two dice of two sides, $M + F$, the chances are $M^2 + 2MF + F^2$;

* Dr. John Arbuthnot is well known as a satirical writer, and as the associate of Pope, Swift, Gay, and other wits, who flourished during the reign of Q. Anne; a reign eminently distinguished in the annals of polite literature. But in this place we are to consider this author in the character of a physician, not that of a critic or satirist.

Dr. A. was a native of Scotland, and took his degree of M. D. at Aberdeen. For some time after he settled in London, his practice produced so little emolument, that he found it necessary, for his support, to teach the mathematics. Like many others of the medical profession, he was indebted to a circumstance wholly fortuitous for his first elevation. Prince George of Denmark was suddenly taken ill at Epsom, at the time Dr. A. happened to be there; he was accordingly called in; the Prince recovered; and he was ever afterwards employed by him as his physician. In 1709 he was appointed physician in ordinary to Q. Anne. Some years before, he had been elected a fellow of the R. S.; and now he had another honour conferred upon him, that of being admitted a fellow of the college of physicians. He died in 1735, after long and severe suffering from an asthma. Besides his satirical and other literary compositions, he wrote An Examination of Woodward's Account of the Deluge; A Treatise on the Usefulness of Mathematical Learning; Tables of Ancient Coins, Weights and Measures; An Essay on the Nature and Choice of Aliments; and an Essay on the Effects of Air on Human Bodies. The subjects of both these medical essays are, it must be acknowledged, of great importance; but they have not been discussed by Dr. A. in the manner they should have been. His book on Aliments is too much incumbered with useless disquisitions relative to the humoural pathology, and he appears to have had very inaccurate views of some parts of the animal economy. Thus, he compares digestion to putrefaction, and supposes the gall to be the principal solvent of the food, &c. &c. With regard to his Essay on the Effects of Air on Human Bodies, it contains less theory, and more matter of fact, than his other medical treatise; but as pneumatic chemistry was then only in its infancy, we shall in vain look in that work for a clear and just account of the phænomena which take place in the process of respiration. Nevertheless many useful hints may be derived from the practical aphorisms with which that Essay is concluded.

that is, one chance for M double, one for F double, and 2 for M single and F single; in four such dice there are chances $M^4 + 4M^3F + 6M^2F^2 + 4MF^3 + F^4$; that is, one chance for M quadruple, one for F quadruple, 4 for triple M and single F, 4 for single M and triple F, and 6 for M double and F double: and universally, if the number of dice be n, all their chances will be expressed in this series,

$$M^n + \frac{n}{1} \times M^{n-1}F + \frac{n}{1} \times \frac{n-1}{2} \times M^{n-2}F^2 + \frac{n}{1} \times \frac{n-1}{2} \times \frac{n-2}{3} \times M^{n-3}F^3 + \&c.$$

It appears plainly, that when the number of dice is even, there are as many M's as F's in the middle term of this series, and in all the other terms there are most M's or most F's.

If therefore a man undertake, with an even number of dice, to throw as many M's as F's, he has all the terms but the middle term against him; and his lot is to the sum of all the chances, as the co-efficient of the middle term, is to the power of 2 raised to an exponent equal to the number of dice: so in 2 dice, his lot is $\frac{2}{4}$ or $\frac{1}{2}$, in 3 dice $\frac{6}{16}$ or $\frac{3}{8}$; in 6 dice $\frac{20}{64}$ or $\frac{5}{16}$; in 8 dice $\frac{70}{256}$ or $\frac{35}{128}$; &c.

To find this middle term in any given power or number of dice, continue the series $\frac{n}{1} \times \frac{n-1}{2} \times \frac{n-2}{3}$ &c, till the number of terms be equal to $\frac{1}{2}n$. For example, the co-efficient of the middle term of the 10th power is $\frac{10}{1} \times \frac{9}{2} \times \frac{8}{3} \times \frac{7}{4} \times \frac{6}{5} = 252$, and the 10th power of 2 is 1024; if therefore A undertake to throw, with 10 dice in one throw, an equal number of M's and F's, he has 252 chances out of 1024 for him, that is his lot is $\frac{252}{1024}$ or $\frac{63}{256}$, which is less than $\frac{1}{4}$.

It will be easy by the help of logarithms, to extend this calculation to a very great number, but that is not my present design. It is plain from what has been said, that with a very great number of dice, A's lot would become very small; and consequently (supposing M to denote male and F female) that in the vast number of mortals, there would be but a small part of all the possible chances, for its happening at any assignable time, that an equal number of males and females should be born.

It is indeed to be confessed that this equality of males and females is not mathematical but physical, which alters much the foregoing calculation; for in this case the middle term will not exactly give A's chances, but his chances will take in some of the terms next the middle one, and will lean to one side or the other. But it is very improbable (if mere chance governed) that they would ever reach as far as the extremities: but this event is happily prevented by the wise economy of nature; and to judge of the wisdom of the contrivance, we must observe that the external accidents to which males are subject (who must seek their food with danger) make a great havock of them, and that this loss exceeds far that of the other sex, occasioned by diseases incident to it, as experience convinces us. To repair that loss, provident nature, by the disposal of its wise Creator, brings forth more males than females; and that in almost a constant proportion. This appears from the annexed tables, which contain observations for 82 years of the births in London. Now, to reduce the whole to a calculation, I propose this

Problem. A lays against B, that every year there shall be born more males than females: to find A's lot, or the value of his expectation:—It is evident from what has been said, that A's lot for each year is less than $\frac{1}{2}$; but, that the argument may be stronger, let his lot be equal to $\frac{1}{2}$ for one year. If he undertakes to do the same thing 82 times running, his lot will be $(\frac{1}{2})^{82}$, which will be easily found by the table of logarithms to be $\frac{1}{4836\ 0000\ 0000\ 0000\ 0000\ 00000}$. But if A wager with B, not only that the number of males shall exceed that of females, every year, but that this excess shall happen in a constant proportion, and the difference lie within fixed limits; and this not only for 82 years, but for ages of ages, and not only at London, but all over the world; which it is highly probable is the fact, and designed that every male may have a female of the same country and suitable age; then A's chance will be near an infinitely small quantity, at least less than any assignable fraction. From whence it follows, that it is art, not chance, that governs.

There seems no more probable cause to be assigned in physics for this equality of the births, than that in our first parents seed there were at first formed an equal number of both sexes.

Scholium. From hence it follows, that polygamy is contrary to the law of nature and justice, and to the propagation of the human race; for where males and females are in equal number, if one man take 20 wives, then 19 men must live in celibacy, which is repugnant to the design of nature; nor is it probable that 20 women will be so well impregnated by one man, as by 20.

Christened.			Christened.			Christened.		
Anno.	Males.	Females.	Anno.	Males.	Females.	Anno.	Males.	Females.
1629	5218	4683	1657	3396	3289	1684	7575	7127
30	4858	4457	58	3157	3013	85	7484	7246
31	4422	4102	59	3209	2781	86	7575	7119
32	4994	4590	60	3724	3247	87	7737	7214
33	5158	4839	61	4748	4107	88	7487	7101
34	5035	4820	62	5216	4803	89	7604	7167
35	5106	4928	63	5411	4881	90	7909	7302
36	4917	4605	64	6041	5681	91	7662	7392
37	4703	4457	65	5114	4858	92	7602	7316
38	5359	4952	66	4678	4319	93	7676	7483
39	5366	4784	67	5616	5322	94	6985	6647
40	5518	5332	68	6073	5560	95	7263	6713
41	5470	5200	69	6506	5829	96	7632	7229
42	5460	4910	70	6278	5719	97	8062	7767
43	4793	4617	71	6449	6061	98	8426	7626
44	4107	3997	72	6443	6120	99	7911	7452
45	4047	3919	73	6073	5822	1700	7578	7061
46	3768	3395	74	6113	5738	1	8102	7514
47	3796	3536	75	6058	5717	2	8031	7656
48	3363	3181	76	6552	5847	3	7765	7683
49	3079	2746	77	6423	6203	4	6113	5738
50	2890	2722	78	6568	6033	5	8366	7779
51	3231	2840	79	6247	6041	6	7952	7417
52	3220	2908	80	6548	6299	7	8379	7687
53	3196	2959	81	6822	6533	8	8239	7623
54	3441	3179	82	6909	6744	9	7840	7380
55	3655	3349	83	7577	7158	10	7640	7288
56	3668	3382						

Logarithmotechnia Generalis: or, a General Method of Making Logarithms. By Mr. John Craig. N° 328, p. 191. *Translated from the Latin.*

For perfecting this very useful part of arithmetic, the only thing wanting seems to be, a general method of finding all logarithmic series. And such is the following; being both easy and genuine, as derived from the very nature of logarithms themselves.

By the letter l, prefixed to any number, in what follows, is to be understood the logarithm of that number, as usual. Now as the logarithm of any proposed number may be found in two ways, we may therefore divide this business into two parts: in the first of which we deduce the log. immediately from the number itself; but in the latter we derive the log. of the proposed number from the logarithms of some antecedent numbers.

PART I.—Let $a + 1$ be any proposed number, and x its log. to be found. Now, by the hypothesis, $x = l. (a + 1)$; which equation may be called a general canon. Then, 1st, assume an equation between terms any how composed of a and y, and combined with any other numbers, in any manner, by addition, subtraction, multiplication, division, or extraction of roots. 2d. By means of the equation so assumed at pleasure, exterminate a out of the general canon, and there will result an equation expressing the relation between the indeterminate quantities x and y. 3d. By the known methods take the differential or fluxion of this equation; and then find the integral or fluent in an infinite series; which will give the value of x the log. sought.

Exam. 1.—Assume $a = y$; then, by the general canon, $x = l. (1 + y)$; the fluxion of which is $\dot{x} = \frac{\dot{y}}{1 + y}$; and the fluent of this in an infinite series is $x = y - \frac{1}{2}y^2 + \frac{1}{3}y^3 - \frac{1}{4}y^4 + \frac{1}{5}y^5 - \frac{1}{6}y^6 + \frac{1}{7}y^7 -$ &c.

Exam. 2.—Assume $y = \frac{a}{a + 2}$; hence $a + 1 = \frac{1 + y}{1 - y}$; then, by the general canon, $x = l. \frac{1 + y}{1 - y}$, the fluxion of which is $\dot{x} = \frac{2\dot{y}}{1 - yy}$; and the fluent of this expressed in a series is $x = 2 \times (y + \frac{1}{3}y^3 + \frac{1}{5}y^5 + \frac{1}{7}y^7 + \frac{1}{9}y^9 +$ &c.

More examples of this are unnecessary; since from these it appears how innumerable logarithmic series may be found, which, without regard to the logarithms of any other numbers, exhibit the log. of the number proposed.

Lemma 1.—Let z be the log. of any fraction $\frac{b}{a + 1}$, and x the log. of the denominator $a + 1$; then will $x = l.b -$ z. Or if z be the log. of the fraction $\frac{a + 1}{b}$, then will $x = l.b +$ z.

Lemma 2.—Let e be the exponent of any power of the number b, then will $l.b^e = e \times l.b$: therefore, having given the log. of the number b^e, and the exponent e, the log of the number b will also be given. And both these lemmas appear from the nature of logarithms.

Part II.—Let $a + 1$ be the number, as before, whose log. is to be found; and let b^e be a number produced by the multiplication of numbers, the greatest of which is less than $a + 1$; also let z be the log. of the fraction $\frac{b}{a + 1}$, that is, z $= l. \frac{b}{a + 1}$; which equation call the general canon. Then, 1st. for b take a quantity any how composed of x and any determinate numbers; and let this value of the number b, so taken at pleasure, be substituted in the fraction $\frac{b}{a + 1}$, whence it will be expressed by a and given numbers. 2d, Let there be taken at pleasure any equation between y and a with given numbers; and by means of this equation exterminate a out of the general canon; whence will be obtained an equation expressing the relation between the indeterminates z, y. 3d, By the known methods find the differential or fluxion of this equation, and then the integrals or fluents in an infinite series, which will give the log. z of the fraction $\frac{b}{a + 1}$: then, from z, being found, the log. $x = l.b -$ z, of the proposed number $a + 1$, will be obtained by lemma 1. For, by the hypothesis, b' is produced by the multiplication of numbers, the greatest of which is less than $a + 1$; and by hypothesis there are given the logarithms of all numbers less than the proposed number $a + 1$; therefore also there is given the log. of b', or the number produced from them all, and thence, by lemma 2, the log. of b also is given.

Exam. 1.—Assume at pleasure $b = a$; hence $z = l. \frac{a}{a+1}$. Then, by art. 2, take $y = 2a + 1$, by which exterminate a, it will be $z = l. \frac{y-1}{y-1}$, the fluxion of which is $\dot{z} = \frac{2\dot{y}}{yy-1}$; the fluent gives $z = -2\left(\frac{1}{y} + \frac{1}{3y^3} + \frac{1}{5y^5} + \frac{1}{7y^7}\text{ \&c.}\right)$ expressed in a series; hence, by lem. 1, $x = l.b + 2\left(\frac{1}{y} + \frac{1}{3y^3} + \frac{1}{5y^5} + \frac{1}{7y^7}\text{ \&c.}\right)$

Exam. 2.—Take $b = \sqrt{aa + 2a}$; hence $z = l. \frac{\sqrt{aa+2a}}{a+1}$; assume also $y = 2a + 2$, then $z = l. \frac{\sqrt{yy-4}}{y}$; its fluxion is $\dot{z} = 4\dot{y}\,(y^3 - 4y)^{-1}$, the fluent of which is $z = -2\left(\frac{1}{y^2} + \frac{2^2}{2y^4} + \frac{2^4}{3y^6} + \frac{2^6}{4y^8}\text{\&c.}\right)$; hence $x = l.b + 2\left(\frac{1}{y^2} + \frac{2^2}{2y^4} + \frac{2^4}{3y^6} + \frac{2^6}{4y^8}\text{ \&c.}\right)$ by the 1st lemma.

Exam. 3.—Again take $b = \sqrt{aa + 2a}$; but now assume $y^2 = 2aa + 4a + 1$; then, by these two equations exterminating b and a from the general canon, it will be $z = l. \frac{\sqrt{yy-1}}{\sqrt{yy+1}}$, the fluxion of which is $\dot{z} = 2y\dot{y}(y^4 - 1)^{-1}$, and its fluent in a series $z = -\frac{1}{y^2} - \frac{1}{3y^6} - \frac{1}{5y^{10}} - \frac{1}{7y^{14}}$ &c; hence $x = l.b + \frac{1}{y^2} + \frac{1}{3y^6} + \frac{1}{5y^{10}} + \frac{1}{7y^{14}}$ &c. by the 1st lemma.

Hence it appears that the logarithmetechny now explained, is very easy and genuine, and so general, that by these two methods innumerable series may be found exhibiting the log. of any proposed number. For we may assume innumerable equations at pleasure, expressing the relation between y and a, every one of which will give a new logarithmic series. Yet care should be taken that the equation be assumed so, as to cause the terms of the series to converge as fast as may be, that the log. may be found with the least labour of calculation. To perform which, the series exhibited in the last example will be very proper, and which is the same as that first given by Dr. Halley, in his elegant method of constructing logarithms.

Here, by the bye, I would desire it may be observed, that the curve, which is derived from our analysis of the problem concerning the length of curve lines, is the same with that proposed in the Philos. Trans. for the year 1708. Being wholly intent about the analysis, I did not observe the coincidence of the curve proposed with that which was found, till Mr. John Bernoulli informed me of it, in his letter to Mr. Wm. Burnet, F. R. S. by which also that learned man was pleased fully to satisfy all my objections against his creeping motion; as I now freely acknowledge, from that pure regard which I bear to truth.

Experiments concerning the Time required in the Descent of several Bodies, of different Weights and Magnitudes, in Common Air, from a certain Height. By Mr. Fra. Hauksbee, F. R. S. N° 328, p. 196.

To make these experiments accurately, I devised the following appartus, to obtain exactly the time of the bodies' descending. At the height from which the balls were to be dropped, I fixed a contrivance in form of a trough, in all about 4 feet long; and the end of it, on which the balls were laid, was loose, swinging on 2 pins at the extremity. This loose end was supported by a thin piece of board, which slid under it through a groove from the other part of the board: to this sliding board was fixed a string, connected to a small wire reaching to the bottom of the descent, where the wire had a communication with a contrivance, to give motion to a pendulum vibrating half seconds. Now when this sliding board was drawn from under that part of the trough on which the balls were placed, the string thereby became so much shortened, as to move the limb of that contrivance at bottom, which dropped the pendulum at the same instant of time that the balls began to descend.

Exper. I.—The first experiment was with two balls: one of them a thin glass bubble, filled with quicksilver; its diameter 8 tenths of an inch, and its weight 840 grains; the other ball was of cork, its diameter was $2\frac{2}{10}$ inches, and its weight 120 grains. When these balls were dropped, the pendulum made 8 vibrations, just as the quicksilver ball struck the ground, and 8 more were repeated before the cork arrived at the same place. The pendulum vibrated half seconds precisely.

Exper. II.—I took a quicksilver ball, much of the same weight and diameter as before; the other was a thin glass bubble, its weight 493 grains, and diameter $4\frac{3}{10}$ inches. For these, when they came to descend, the pendulum made just as many vibrations as in the last experiment; that is, the quicksilver ball struck the ground at 8 vibrations, and the other just at the end of 16.

Exper. III.—The quicksilver ball used in this experiment, was also much of the same weight and diameter as before: the other ball was of glass, its weight

535 grains, and its diameter one way measured $5\frac{1}{4}$ inches, but its opposite diameter only 5 inches. On the descent of these balls, the pendulum made only one vibration more than in the other experiment; that is, the quicksilver grounded exactly at 8 vibrations, and there were 9 more before the other ball arrived at the same place.

These experiments were made from the top of the cupola of St. Paul's, London; from whence to the floor, on which the balls were dropped, measured near 220 feet. It is to be observed, that the quicksilver balls made no sensible impression on the floor on which they descended, which at that time was covered with deal boards, notwithstanding their weight and the velocity of descent.

The following experiments on the descent of bodies in air, were made in the same manner, at the place before-mentioned, answering very exactly with the former.

Quicksilver balls.			Large thin glass balls.		
Weight in grains.	Diam. 10ths of inch.	Time of falling in half seconds.	Weight in grains.	Diam. inch. 10ths	Time of falling in half seconds.
908	8	8	510	5.1	17
993	8	8 a little less	642	5.2	16
866	8	8	599	5.1	16
747	$7\frac{1}{2}$	8 a little more	515	5.0	$16\frac{1}{2}$
808	$7\frac{1}{2}$	8	483	5.0	17
784	$7\frac{1}{2}$	8 a little more	641	5.2	16.

These experiments were made June the 9th, 1710; at which time the height of the quicksilver in the barometer was 29.7 inches, and the thermometer 60 degrees above the freezing point.

Note.—That the quicksilver balls, and the large thin glass balls, were dropped together, as they are ranged in their several lines.

Experiments showing the Effects of Air passed through Red-hot Metals, &c. By Mr. F. Hauksbee, F. R. S. N° 328, p. 199.

To find what effect such a medium as air, passed through red-hot metals, might have on the lives of animals, I contrived the following method: I took a large receiver, open at top, about 4 inches diameter, which was covered with a brass plate and wet leather, as usual in glasses of such a make. To this plate at top, which had a screw with a small perforation, belonged a cock, from which proceeded a small hollow wire, about 3 feet in length; that end of the wire farthest from the receiver was put into a hollow piece of cast brass, pretty thick in substance, but the hole was not quite through; and the hole being larger than the small hollow wire, it was wedged into the same with pieces of steel wire, till the cast brass was filled as full as it could contain. In this manner it was put into a charcoal fire, where it lay till it was thoroughly red-hot. The receiver being then exhausted of its air, the cock on its upper part was turned, which gave liberty for that air only which must necessarily pass through the red-hot metals to succeed. This air first passing down through the small ducts between the red-hot wires, before it could come to enter the red-hot hollow brass wire, must of necessity suffer or undergo such a change as fire or the fumes of such red-hot metals would give it. When the receiver was filled with this air, and had stood some little time, the brass cover was taken off, and a pretty large cat immediately plunged into it; the cover being laid on again, the cat immediately fell into convulsions, and in less than a minute appeared without any sign of life. Then being taken out of the receiver, and laid on the floor, she seemed as dead; but in less than a minute of time she began to discover life by motion in her eyes, and after two or three hideous squalls, she began to recover apace, but was very fierce, and would spit and fly, as well as her weakness would suffer her, at any one that offered to touch her, and it seemed hazardous for any one then to attempt it. But after about half an hour's time, as her strength and ease recovered, so her former temper increased upon her, suffering herself to be handled without any sign of fierceness.

The effect the same sort of factitious air had upon flame was as follows: on plunging a lighted candle into it, it was immediately extinguished; and this I several times observed, that when the candle was slowly immersed, so much of the wick, which before was lighted, as came but just within the verge of the glass, died; and so the rest successively, as it descended to the same place; and this, on several repetitions, answered much the same. But in some time, as the common air came to mix with it, one might plunge the lighted candle lower and lower before it would go out, till at last it would remain burning at the bottom. As to the elasticity and specific gravity of the said medium, I have made several accurate trials, but find it nowise differing from common air, in respect to those properties. Hence it follows, that the foregoing effect, is nowise owing to any imperfection or defect in the last mentioned properties: therefore the following queries seem to offer themselves. Query 1. Whether air itself may so suffer in its own nature, by any sort of fire, as to be divested of the power of subsisting life or flame?* or,

* When atmospheric air is made to pass through red hot metallic tubes in the manner above-mentioned, it is deprived of its oxygen, which combines with the heated metal, whose surface is thereby more or less oxydized. Hence the residual air (consisting of azote, with sometimes a small proportion of carbonic acid gas) becomes unfit for the support of life and flame.

Query 2. Whether the effluvia or steams proceeding from the red-hot metals, which the air may take along with it in its passage near them, do not very much contribute, if not wholly occasion the effect?*

If the latter takes place, I presume it may in some measure be applied to account for the effect that the damps, or steams, which arise from subterraneous caverns, impregnated with metalline effluvia, have on the lives of animals; and yet at the same time, the same air may suffer no change in respect to its other properties, I mean its elasticity and specific gravity, in comparison with other air in the same region.

Experiments concerning the Effect of Air passed through a Degree of Heat, equal to that of boiling Water.—I contrived a brass box, about 4 inches long, and an inch and half over; at one end, which I soldered up, I fixed two small brass tubes; one of which went through, and reached the remoter end nearly, the other tube was but just inserted in it; but each of them was long enough to reach sufficiently above the surface of the water in which they were to be put. These tubes were to convey the air into a receiver exhausted of its air: it passed first into that tube which nearly reached its opposite end, and so into the other which led to the exhausted receiver. But the box, with that part of the tube that was within it, was first pressed full of brass dust, which I had the conveniency to do by means of a brass cap, which screwed on to the end, not before-mentioned. This brass dust I moistened with a little water, thinking thereby to exert a more than ordinary steam, or effluvia, from the metal, which the air might take along with it, as it passed through such straight and narrow avenues, as it must do between the brass dust. In this manner it was put into the water when cold, and continued in it till it had boiled a considerable time; by which means it must, in all its parts, be of the same degree of heat, at least, as the boiling water. Thus it was taken out, and applied to the exhausted receiver, where, on turning a cock, I gave the liberty for that air only to pass into it, which must succeed through the brass box and dust, under the circumstances above-mentioned. When the receiver was full of this air, the cover was taken off, and a lighted candle plunged into it, where it continued burning, even at the bottom, as if it had passed through no such medium, but had been full of common air. I took that method to try it, believing the flame of a candle to be the most tender way of discovering a change in air. Afterwards I repeated the same experiment over again, with dry brass dust instead of the former, but the success was the same. It seems, therefore, that such a degree of heat, as that of boiling water, is not sufficient to cause any considerable change, if any

* The before-mentioned effects of atmospheric air upon animals and flame, after its transmission through red hot metallic tubes, are owing not to effluvia or steams proceeding *from* the metals, but to the absorption of the oxygen *by* the metals.

at all, in the air; nor such a degree of heat, able to strike any injurious, or suffocating effluvia, out of the metalline particles.

Some other Experiments on the foregoing Head.—The passing of air through a red-hot glass tube into an exhausted receiver, had no manner of influence on a sparrow put into the same; but on passing air through red-hot charcoal, before it entered the tube that conveyed it into the exhausted receiver, the animal, in that medium, in about a quarter of a minute, gave signs of presently expiring; but being taken out at the same time, it recovered, and continued living and well for some days after. Yet it was concluded, had the bird's continuance in the receiver been but double that time, her recovery would have been very doubtful. I have likewise tried air passed through the flames of spirit of wine and oil of turpentine; the effect was much the same as to the spirit of wine, the flame of a candle being immediately extinguished when plunged into it; but the air which passed through the flame of the oil of turpentine, took some unctuous fumes along with it into the exhausted receiver; which fumes on the near approach of a lighted candle, suddenly took fire, and continued to burn on the upper surface till they were stifled by covering close the receiver. And on several repetitions, it answered much the same, till the whole quantity of fume was consumed.

A Description of the Apparatus for making Experiments on the Refractions of Fluids. With a Table of the Specific Gravities, Angles of Observations, and Ratio of Refractions of several Fluids. By Mr. Fr. Hauksbee, F. R. S. N° 328, p. 204.

The whole apparatus is fixed on a table, parallel to its surface. On one and the same axis is fixed a sextant, of 4 feet radius, and a moving limb to bear the object. The sextant is divided into degrees and minutes by a diagonal, and remains always fixed. The object, which is placed on the moving limb, is seen parallel with the table when observed through the prism, and at no degrees on the sextant; but when any transparent liquid is put into the same, the object must be elevated till it appears to the eye; then observing how many degrees and minutes the index on the limb cuts on the sextant, we note it, and call it the angle of observation. Thus for different liquids we have different elevations of the object, as in the following table. The sight slit is composed of two pieces of box wood, planed parallel to each other; these pieces are separated only by three slender slips of common cards, and with that intervention are screwed down one upon the other, exactly parallel with the axis of the moving leg and sextant. The prism, through which it directs the sight, is placed pretty near it, and consists of an angle of 44° 54′, which angle is fixed perpendicular to the plane of the table, its upper side being parallel with the same. The object is a piece of white paper, in form of a cross, pasted on a black board, and is fixed at the end of the moving limb, which is about 7 feet long from the sight, its diameter is about 2½ inches, which just comprehends the sight through the slit; so that when the object is wholly within view, we conclude the observation to be exact. With this apparatus the experiments are made as well by candle-light as day-light, the presence of the sun beams being nowise necessary; and I think they may be depended on as pretty accurate. I have taken the specific gravity of the several liquids, where I could obtain a sufficient quantity, as appears by the table; so that if any person should have the curiosity to repeat these experiments, he must expect a different angle of observation, if the specific gravity agree not with the table; for sometimes it happens, that liquids of the same denomination are not always of an equal goodness, and consequently will have a different specific gravity and refraction.

The crystalline humour of the ox eye I pressed into the angle of the prism, by which it received its form, and gave the angle of observation, as specified in the table. I could not see the common object through it, but was forced to make use of a candle for that purpose; the flame of which appeared very broad, at least 5 or 6 inches, nearly in the form of a half moon; but what should occasion such a change of figure I cannot at present determine. Of all the fluids I have tried, I find nothing to refract a ray of light less than water; yet there are several other liquids which make the same angle. I observe oil of bees-wax to be the lightest fluid, and butter of antimony per deliquium to be much the heaviest; the difference of specific gravity between these two bodies is as 662 is to 1976, that is, nearly as 1 to 3: and the ratio of their refractions only as 10000 is to 6885 bees-wax, so is 5941 antimony to the same radius, that is, nearly as 1 to 1.16. Likewise oil of vitriol is in specific gravity to oil of sassafras, as 1510 is to 898; yet the ratio of refraction of the lightest is most considerable, being in proportion as 10000 is to 6475 sassafras; so is the same radius to 7011 vitriol. Thus I find, that a body does not refract in proportion to its specific gravity, but from some quality peculiar to itself, whether it be from its inflammability, or from any different texture, or figure of its component parts; or whatever else it be, I shall, with the application of these experiments, submit to this honourable society to determine.

Specific gravities in comparison with a bulk of water equal to 820 grains.		Angle of observation.	Ratio of refraction, as 10000 is to
		16° 50′	7485.3
Oil of Sassafras	898	29 20	6475.8
Turpentine	713.5	25 25	6741.8
Bees-wax	662	23 30	6885.4
Caraways	752	26 13	6696.5
Oranges	711	25 20	6741.2
Hysop	769.5	25 10	6757.6
Rosemary	747	24 40	6794.7
Savin	789	25 30	6730.9
Origanum	752	25 0	6770.2
Pennyroyal	783	25 30	6730.9
Mint	780.5	26 0	6706.4
Spike	749	24 30	6807.3
Fennel	798	27 10	6616.5
Juniper	729	25 10	6757.6
Cummin	766.5	27 0	6627.7
Tansey	757	23 46	6865.1
Dill	795.5	27 40	6582.7
Amber	783	26 30	6662.3
Cinnamon	828	28 40	6517.7
Cloves	827	27 20	6606.8
Nutmegs	759	25 40	6721.4
Spi. of Wine	703.5	18 50	7287.9
Hartshorn	786	17 0	7468.3
Vinegar	824.5	17 0	as hartshorn
Salammoniac	794.5	16 56	7475.2
Acids, Sp. of amber	825	16 56	as sal ammo.

Specific gravities in comparison with a bulk of water equal to 820 grains.		Angle of observation.	Ratio of refraction, as 10000 is to
Acids, Oil of vitriol	1510	21° 56′	7011.5
Spirit of nitre	1166	20 50	7104
Aqua regis	987	19 50	7195
Aquafortis	1157	20 40	7120.5
Aqua regis from aquafortis and sal ammoniac	1034	20 10	7161.5
Butter of antimony	1976	40 0	5941.3
Spirit of raw silk	916	20 30	7135
Spirit of honey	716	16 50	as water
Tinc. of antimony	693	18 46	7294.3
Jesuit's bark	720	18 46	as tinc. of ant.
Bals. tolu	717	19 34	7219.3
Gum ammoniacum	719	19 10	7257.3
Metals	713	18 54	7281.7
Vitreous humour of an ox's eye		16 50	as water
Crystalline humour of the ox eye		24 10	6832.7
White of a hen's egg		17 40	7401.3
Jelly of hartshorn		17 50	7384.7
Human saliva		16 50	as water
Human urine		17 5	7451.9
French brandy		18 20	7338.6

Oil of turpentine strongly tinged green, with filings of brass, nowise alters its refraction.

An Account of a Book intitled, Dissertatio Epistolaris de Glandulis conglobatis Duræ Meningis humanæ, indeque ortis Lymphaticis ad Piam Meningem productis. Authore Antonio Pacchiono. Romæ 1705, *8vo.* N° 328, p. 208.

This dissertation seems to be only a supplement, or appendix, to a treatise of the Dura Mater, which Pacchionus had published some time before. In this, his chief design is to inquire into the service, and find out the origin of that humour, which moistens the brain and its membranes, in every dead animal.

In examining the cavity of the longitudinal sinus, immediately under its membranous expansions, in the area or middle of its cordæ, mentioned by Dr. Willis, that is, the small transverse fibres, which like ropes keep the walls of this sinus from being over dilated by the influent blood, our author had the good fortune to discover a great number of conglobate glands, which are all contained by a fine and proper membrane, as in a bag. For the most part they appear in clusters, and are seldom scattered here and there. Their figure is roundish, and in size they equal a silk-worm's egg, if inspected as soon as the body is opened; but if the meninx be macerated in vinegar for a month, or more, they are larger than a millet seed. In old people, and in those who have died of a lingering disease, they may be seen by the naked eye, without any microscope, or previous preparation. They are incompassed with fine carneous fibres: whence they put on a pale colour; but when these fibres are much relaxed, as happens in an hydrocephalus, or comatose distempers of the brain, they become white and very large.

From the aforesaid glands innumerable fine threads arise, which are only their excretory vessels, and are spread all over the inside of the pia mater, being accompanied and strengthened in their course by fibres and other blood vessels. It is these fibres which join the meninges to one another: and if you take care that the dura mater be not hurt in taking off the upper part of the skull, in dividing these membranes, you may observe liquors of different colours ooze and drop from them when cut. Our author is not positive that these excretory ducts penetrate into the medullary substance of the brain; but affirms, that they creep along its inner protuberances and accidental cavities. This discovery confirms what Bohn and some others have said about the lymphatics of the brain; but before our industrious author, none ever so much as pretended to fix their origination, which he has traced from the glands lodged in the longitudinal sinus, as above. The use he assigns to these glands, is to separate and strain a particular kind of humour from the blood; which, in his opinion, may serve to keep the membranes and surfaces of the cavities and protuberances of the brain from growing too dry by their continued motion.

De Mensura Sortis, seu, de Probabilitate Eventuum in Ludis a Casu Fortuito Pendentibus. Autore Abr. De Moivre, R.S.S. N° 329, p. 213.

This long paper, on the doctrine of chances, it is unnecessary here to repeat; because its contents were only given as a specimen of this art, which the author, 7 years after, enlarged and perfected into his celebrated treatise, first published in 1718; where the whole may be seen to much more advantage. This specimen also gave rise to several disputes on the subject, with the continental mathematicians; some account of which may be seen in the author's treatise just mentioned; and still more fully in his Miscellanea Analytica, published in 1730.

A Demonstration of the Number of Acres contained in England or South Britain; and the use which may be made of it. By Dr. Nehemiah Grew, F.R.S. N° 330, p. 266.

Several persons of respectability have given us, as they have supposed, the just number of acres contained in England, or South Britain, or very near it. Sir William Petty reckons about 28 millions: others, 29 millions; others a few more. But I humbly affirm, they have all under-reckoned: which seems to have been owing to their reckoning only by the maps; that is, by computed and not by measured miles; by which only the number of acres can be known. I have seen an account of the number of acres in each county: which account, whether taken from dooms-day book, or from any other registry, cannot be true. For though we have lost some land, yet there is a great deal more now gained, which in the conqueror's time lay under sea. Within 120 years, very much has been recovered out of the seas, and maintained by banks, in the marshes and fens of Essex, Kent, and the isle of Ely: and in some parts of Lincolnshire, the land has gained of the sea, 4 miles in a direct line from land to sea, in the memory of men now living. Nor is it the truer, for having been taken from any other record: for if the numbers of acres, according to the said account, in each shire, be put together, they exceed not 39 millions and a quarter: which number, though it comes much nearer the truth than any of the former, is yet a great deal short of it. For however, according to vulgar computation, England be reckoned in length only 305 miles and in breadth about 290 miles; yet it appears, by an exact wheel measure, that from Newhaven in the south of England to London, are 56 measured miles; and that from thence by a straight line continued to Berwick in the north, are 339 of the same measured miles; in all 395 measured miles, the true length of England. And again, that from the south foreland in the east, to the lands-end in Cornwall, are about 367 miles of the same wheel measure, the true breadth of England.

This being known, it is easy, without any laborious and costly survey, to know also, how many square miles, and consequently how many acres are contained in England, or Great Britain; viz. in the following manner. If a line be drawn on a chart of England, pl. 16, fig. 12, from the south foreland in Kent, to Berwick; and from the two ends of this line, two more lines meeting at the lands end in Cornwall, they will make the triangle ABC; which triangle since it excludes as much more of the land, as it includes of the sea, as may answer the small number of miles obtained by the curvity of the roads; it may therefore be allowed to be equal to the area of England. Now the length between Berwick and the south foreland in Kent, being about 5 miles more than between Berwick and Newhaven, which is 395 miles: therefore the line AB, may be taken for 400 miles; and the perpendicular line AG being less by about 7 miles, than between the south foreland in Kent, and the lands end in Cornwall, which is 367 miles, the said line AG may be taken for 360 miles. Therefore AG, 360, being multiplied by $\frac{1}{2}$AB, 200, produces 72000 square miles: and 72000 being multiplied by 640, the number of acres contained in one square mile, produces 46 millions and 80000, the number of acres contained in England, or south Britain.

Whence it appears, first, that if the province of Holland contains, as is computed, but one million of acres; then England is more than 46 times as large as Holland. Next, if in the province of Holland, containing but one million of acres, are 2 millions and 400 thousand souls, or 2 millions and $\frac{4}{10}$, as they are said to be; then England, which contains 46 millions of acres, to be proportionably populous, should have twice 46 millions of people, and $\frac{4}{10}$ of 46; that is, about 110 millions. But to allow room enough for persons of all degrees under our British monarchy, if England were half as populous as Holland, with only 55 millions, it were a good proportion, and would be near five times our present number; and about 22 times as many as in the province of Holland.

To people England, in a competent time, with this number, several ways are practicable: by which, I have computed that the present number may be doubled in 24 or 25 years; and probably quadrupled in about 36 years. One of these ways, though not the speediest, would be the introducing of strangers: yet to make use of this, or of any other way, to multiply the people, before we have provided the means of employing them, as was lately done, would be preposterous.

But when we shall once earnestly attend to our true interest, in employing and encouraging every where our own hands, and the hands of other nations, as the French and Dutch do, in all the sorts of husbandry, manufacture and merchantry: when our nobility and gentry themselves, shall be examples in some or other of these particulars: when we shall hereby be universally engaged to enclose, and to improve every foot of our land; to make the utmost use of all our home growths, above and under ground; and of all our ports (about 200 great and small,) more than in all the kingdoms and states of Europe put together: and when Scotland and Ireland, shall both of them afterwards be improved in like manner: when all men's heads and hands shall be thus employed about some one honest and profitable business; as it will naturally promote every where peace and friendship among us; so is it easy for any considering man to foresee, how highly it will advance the British monarchy and people, at home and all over the world, in beauty, strength, and glory.

Observations on the Spots seen on the Sun, from the Year 1703 *to* 1711 : *with Mr. Crabtrie's Opinion on the Solar Spots. By the Rev. Mr. William Derham, F.R.S.* N° 330, p. 270.

As my former observations of the solar spots, published in N° 288, were imperfect, I will therefore give a better account of the spots and faculæ that have been seen on the sun since; there having, I suppose, few of those appearances escaped my sight, since their first being seen in 1703; and because I am now better provided with good instruments to take their places on the sun, viz. a micrometer, after Mr. Gascoign's manner, to take their distance from the sun's northern or southern limb, which is parallel to the pole of the earth; and a half second's movement, to measure their distance from the sun's eastern or western limb.

In this following table, may be seen at one view, what spots or faculæ fell under my cognizance.

A Table of Spots and Faculæ on the Sun, visible at Upminster, since July 1703.

1703.	July 18	June.... 27	Sept. 7	July 1	1708.
Octob. .. 9	20	28	8	Languid ... 2	July 31
10	21	30	10	Scarcely visible } 4	August 1
11	* 23	July .. * 1	Faint .. 11	More visible } 5	5
Nov.... 19	Sept. * 10	* 3	Extinct.. 12	Extinct 6	6
22	nothing.... 14	24	Oct. .. * 29	Appears 8	22
1704.	18	Sept. ... 30	Nov. 5	* 10	23
Jan. 16	* 21	Octob. .. 2	Faint 6	Aug .... * 31	24
17	Nov....... 17	3	* 8	Sept. ... * 12	28
18	18	5	28	16	Septemb.... 1
19	21	6	29	Spot & * .. 17	Novemb.... 5
21	Dec. .... * 2	7	30	19	Dec. 14
22	1705.	25	Dec. 1	21	26
23	Jan. 1	26	2	27	1709.
Jan. 30	2	30	3	No spot nor * } 28	Jan. 15
Feb. 23	3	31	4	Octob. 31	21
25	5	Nov. 2	5	Nov. 1	22
Mar. 7	* 25	4	* 22	4	August.... 13
8	Feb. .... * 19	* 21	* 31	8	* 17
9	March 14	1706.	1707.	10	Octob. 8
10	16	Feb. 7	Feb. .. 14	N. B. This Nov. 15, another spot arose on the eastern side of the disk, while this was on the western. 15	Novemb. ... 1
11	April 1	March * 7	15	16	2
13	May 5	27	16	17	4
April ... 11	6	30	18	18	5
12	7	April 2	24	19	6
13	8	Vanished 3	March .. 6	20	1710.
May ... 1	* 10	June 7	9	22	Jan. 22
* 11	* 22	8	11	Decemb. .. 4	April ... * 6
Jun 23 none	* 23	July 24	12	10	Octob. 14
Some 24	June 22	25	14	* 29	* 18
Vanished 25	23	Sept. 4	18	* 30	
	24	5	21		
	26	6	June .. * 29		
			Extinct . 30		

In this table, the faculæ are noted with an asterisk; and the duration of every appearance of the same spots or faculæ, or the time they disappeared, with a line below the day: and where any thing remarkable occurred, that could be briefly noted, I have taken notice of it in the table. But I shall besides select and notice some others of the more remarkable circumstances.

And first, as to the figure of the spots: they are well known to change frequently; and therefore I think it of little use to their figures every time I observed them. But it is somewhat remarkable, that the spots generally appear longish near the extreme parts of the disk: for if they are ever so round near the middle of the disk, they become gradually longer towards the extremes, till, at going off they seem to be almost a straight line, nearly parallel to the sun's limb. Which is a manifest argument, that the sun is a globe, and that these spots are on, or very near its surface.

Another thing remarkable is, the mutability of the shape of the spots. I have more than once manifestly perceived them to change in the very time I have been looking on them. Thus, Nov. 19, 1703, I saw three or more spots not far from the middle disk; and while I was looking on them, they seemed to vary, both as to their shape and strength; sometimes seeming longer, sometimes shorter; sometimes dense, sometimes languid. And this they seemed to do, not only through my 16-foot tube, but also when I received the sun's image through a 6-foot telescope, on a white paper, in a darkened room. The weather hindered me from seeing these mutable spots again, till November the 22nd following; and then they were become only like a thin smoke, or nebula. So again April 11, 1704, there were several spots, with umbræ about them. These umbræ or nebulæ, I could plainly perceive, while I was looking on them, to be sometimes very faint and thin, and sometimes much darker and thicker. These maculæ and umbræ I observed suddenly break out in the sun: for, on April 9, the disk was free. But this April 11 last mentioned, I perceived them advanced near a quarter part on the disk: and consequently they break out in the sun within 48 hours before. On April 13 the spots were become umbræ in the morning; and at 4 o'clock in the afternoon, there were no remains of either maculæ or umbræ. From this short continuance of these spots on the sun, it is more than probable they were in a perpetual flux and change; and that those mutations perceived in them, whilst looking on them, were not imaginary, but real.

Also it may be further remarked, (which I have frequently observed, and which as I remember Scheiner observed long ago) that those spots and umbræ which suddenly rise, do as suddenly decay, and are soon extinct. And such spots, I have further observed, do seldom turn to faculæ, as they commonly do when longer on the sun. Again, May 5, 1705, I could perceive two spurs or branches, running from a spot, to change, and be sometimes darker, sometimes thinner. So March 30, 1706, I observed such another variation. This day, or but little before, spots with faculæ arose in the sun, which remained not above three days. One of these spots I could manifestly perceive to be sometimes quite extinct, and then again immediately to appear: and the faculæ also, in half an hour's time, had plainly altered their shapes. October 29 the same year, I could plainly perceive the maculæ and faculæ both to change: and while I was carefully viewing them, I saw a spot arise in one of the brightest faculæ, and again nearly disappear; and then again appear strong and dark.

Another thing I have observed, is, that the maculæ do generally, if not always, become nebulæ or umbræ before they quite vanish; and after that, very frequently turn to faculæ, or bright golden spots, more illustrious and fulgent than the other parts of the sun. When the spots are of short duration, faculæ seldom ensue: or if they do, they are commonly the remains of some spots that had before been seen on the sun, and vanished perhaps on the side opposite to us. But spots that continue long, if they vanish before that part of the sun revolves out of our sight, do very often become faculæ. Of which the table affords several instances, particularly July 3, 1705.

From these particulars, and their congruity to what we perceive in our own globe, I gather, that the spots on the sun are caused by the eruption of some new volcano therein; which at first, pouring out a prodigious quantity of smoke, and other opaque matter, causes the spots: and as that fuliginous matter decays and spends itself, and the volcano at last becomes more torrid and flaming, so the spots decay and become umbræ, and at last faculæ; which faculæ I take to be no other than more flaming or brighter parts than any other parts of the sun. These faculæ I have observed never continue long on the sun: and the reason I conceive is, because the volcano, after its smoke is over, does not long emit its flames, because the fiery pabulum is then nearly spent, when once it begins to flame: after which, the torrid volcano soon returns to the natural temperature of the sun, so nearly at least as to escape our sight, at so vast a distance from us.

Another thing that may be accounted for, and which indeed in some measure confirms what I have said, is the nuclei, or darker part of the spots generally in most spots, and towards their middle. Now it is very usual in culinary fires in this our globe, when they emit smoke, that the middle is the darkest part. And so I take it to be in the eruptions of the sun; that the nucleus is just over the mouth of the ignivomous cavern, and that the misty parts of the spot are the thinner parts of the smoke, swimming about in that fluid or atmosphere, which I suppose surrounds the sun, as well as our globe, and the moon manifestly; and in all probability, every planet of this our solar system.

From what has been said, we may give a reason, why there are sometimes spots frequently on the sun, and sometimes none in many years. One thing I believe there is in this, that there may be spots, but not always seen. But there are doubtless great intervals sometimes when the sun is free; as between the years 1660 and 1671, 1676 and 1684; in which time spots could hardly escape the sight of so many curious observers of the sun, as were then perpetually viewing him with their telescopes in England, France, Germany, Italy, and all the world over; whatever might be before, from Scheiner's time. The reason then of this long disappearance of the spots, I take to be from the want of extraordinary eruptions in that fiery globe. The sulphureous, or other matter, or pabulum of those eruptions, is spent or dissipated, and that globe continues in its natural ordinary burning state, till there happens to be a fresh collection of smoking, displosive, and extraordinary matter, to cause a new eruption. Which eruptions generally happen between what we may call the sun's tropics, or in his torrid zone: for I never observed any spots to be near the sun's poles. And the spots in Scheiner's cuts are all about the middle zone of the disk. The greatest evagation I ever observed of them, was March 8, $170\frac{3}{4}$: on which day, besides the dark spots in the usual zone, I perceived some faint spots, scarcely visible, much nearer the southern pole than I ever had seen them. But this was doubtless in some measure owing to the position of the earth in respect of the sun, as well as to the southerly place of the spots on him: for, about the equinoxes, the spots seem to march pretty far towards the poles of the sun. Having thus observed what part of the sun the spots commonly possess, I shall next take notice of their stages and path over the sun. That the sun moves round his own axis, is manifest, from the motion of the spots. And that the spots seem to traverse the sun, sometimes in straight lines, sometimes in curve lines, curved this way and that way, is as manifest also, and well known to the curious.

All these particulars are confirmations of what I said, that the solar spots are no other than a smoke rising out of the body of the sun. Of which opinion I have been almost ever since I first observed them, and find that I am not singular in this opinion, as I shall show from the following letter (which with some others is lately fallen into my hands) from the admirable Mr. Crabtrie, to the no less admirable Mr. Gascoigne, the inventor of the micrometer.

The beginning of the letter has been torn off; but I find by that part of it that is left, it was Mr. Crabtrie's first letter to Mr. Gascoigne, and that the torn part was only compliments for his writing to him, being a stranger, &c. after which it follows in these words.

"I writ also to Mr. Townley at that time my opinion in brief of the sun's spots, (which you conceive to be stars,) and it seems he, or Mr. Kay, writ to the same purpose to you, desiring your opinion: which you freely deliver; for which I cannot but commend you, and especially for preferring reason before any man's authority. Yet give me leave (pace tua amice desideratissime) to speak my mind likewise freely concerning these appearances. I do not value the authority of Galilæus (though reputed the greatest speculative mathematician in Europe) nor yet Kepler (though astronomorum facile princeps) further than either demonstrative, or the most probable reasons confirm their opinions. Nor will I stick to subscribe to the man whosoever shall bring better reasons for his opinion. I must acknowledge you say more for the stellifying of these solar obscurities, than I have heard before; yet I conceive not sufficient, either demonstratively or probably to countermand those which Galilæus, Kepler, and others have produced to the contrary; nor yet such as can be cleared from such objections, as reason, demonstration, and observation may lay against them. My occasions will not admit a full disquisition hereof at this time; yet something I would say for the present, the better to furnish you where to object when I see you; that so by diligent inquisition, the desired truth may (may we have that happiness) be better found out by us.

"I have often observed these spots; yet from all my observations cannot find one argument to prove them other than fading bodies. But that they are no stars, but unconstant (in regard of their generation) and irregular excrescences arising out of, or proceeding from the sun's body, many things seem to me to make it more than probable.

"For first, for their form; they are seldom round, but of irregular shapes, and, as I have often seen, one side or end of the spot more thin than the rest, like to a certain misty darkness, and by degrees thicker, grosser, and darker, nearer to the main body of the spot; just as the smoke of some pitchy fire, which is in one part very gross, and in another more rare and thin, turning at last into meer air: or like a cloud, fog, or mist, more thick, dark, and gross in the midst; and more thin, fluid, penetrable, and transparent towards the sides; which I suppose is not compatible with any of the stars.

"Secondly, for their colour: The lightness thereof differenceth them from stars or planets; they being never of such absolute darkness as I observed Venus the 24th of November last: though I have seen spots sometimes little less than she, yet always of a far paler and whiter colour, looking (at least in some parts) like some thin dissipated substance.

"Thirdly, for the manner of their appearance. I have seen many spots, which in the middle of the sun appear of a round body, but coming towards the side of the sun, appear long. Which (if you rightly consider it) is a demonstrative argument that they are not globes, as all the planets and stars are: for globes always appear of one form (round) in every position; but exhalations, or such like fluid substances, extended to a broad flat form, like our clouds, which being over our heads, and so in their full breadth, appear large and broad; but driven with the wind, till they turn one edge upon us, seem of a long shape. So these solar clouds, being turned about the sun, may in the middle shew their full breadth to us, and about both edges of the sun, turn their edges to us: which answereth to the appearance.

"Fourthly, for their continuance. Some of these spots, arising at the east side of the sun, vanish before they come to the midst of the sun. Others appear first in the middle of the sun, and vanish before they come to the western limb; and for the most part they vanish before they have made a full revolution about the sun. Which argues them to be but thin, vanishing, fading substances, not like the permanent bodies of the stars.

"But to take off these reasons, you answer, that you conceive these spots to be stars moving regularly in their own orbes, which are many, though none of greater extent than about $\frac{1}{10}$ of the ⊙'s semidiameter from its circumference; and that the swifter movers in the lower orbes, overtaking the slower in the higher orbes, cause an appearance. You seem therefore to think, that they being so thin bodies, the sun's rayes pass through them, and so one cannot be seen alone, till more being together, one heaped behind another, they stop the light of the sun's rayes, and so cause an appearance. This I conceive is your meaning: or else (as you seem to insinuate afterwards) that the higher reflects the sun's rayes strongly enough upon the lower (when they come within the angle of reflection) to make the interjacent planet indiscernable.

"But to these I answer,

1. "If it be by their coming within the angle of reflection, that the light of the sun reflected from the outer planet upon the inner, doth make it (as you speak) indiscernable, then that light so reflected is reflected either upon all places, as the moons and planets light; or but upon one, as is the reflection of a plain looking-glass. If the first, there would never be many seen (seldom above one or two) because the outermost would continually make the inner undiscernable. But Gassendus affirms, there are seen sometimes 40 at once in the sun's body. If the 2d, there would always be many seen, because the reflected light would but occupy a little room, and that but for a small time, till the swifter were past the place of reflection: whereas many days there are

none at all seen in the sun's hemisphere: and in both these cases, the outermost planet of all would always in the space of 27 days, be seen in the same place, being never obscured, none of the inferior being able to reflect light upon it. Add hereunto, if any kind of reflection should make them to appear bright like the sun, and so not distinguishable from the light of the sun, what should hinder, but we should see them also bright bodies by the side of the sun, when they are passing either by the west, or east side of the sun's body? The light being then reflected upon them by the inferior planets as well as at other times, and that also upon much of that side of them which we should behold.

"But if you wave this conceit, as insufficient, and fly to your former, that the swifter movers in the lower orbes, overtaking the slower in the higher orbes, cause an appearance. To this I answer. 1. The thing you suppose seems to me neither necessary nor probable, nor do I conceive why they should not be seen, being themselves alone, as well as conjoined, seeing all other stars and planets are so. 2. If it be because they are of a thin, transparent substance, till many, being one behind another, make them to seem grosser; then they are not of the nature of other planets, as is proved in ☿ and ♀, who of themselves appear dark bodies, when they come between us and the sun; nay, they must be more thin than our clouds, which will easily be seen between us and the sun, and hides it from us. 3. If it be because they are so little, that the imperfection of our glasses cannot discover one alone, there must be, without doubt, many millions of them; which how they can be included within the compass of $\frac{1}{10}$ of the ☉'s semidiameter, we shall consider anon. I have seen one of an ordinary darkness, (yea darker than many greater) yet not above $5''$ diameter. If this consist of two, or many, of themselves invisible, how many were in those which Gassendus saw of $1\frac{1}{2}'$ diameter? 4. The figure of these great ones (being necessarily composed of stars of such different orbes and motions) would quickly vary, by reason of the diversity of their motions; like as we see in a flock of small birds. But fifthly, you say the furthest of these orbes is not above $\frac{1}{10}$ of the sun's semidiameter from its circumference. But there would not, in that small space, be room enough for so many orbes of planets, as have been seen at once. Which I prove thus. 1. Gassendus affirms there are sometimes some of about the $\frac{1}{10}$ part of the ☉'s semidiameter; which is the whole space allowed by you for them all. And I myself have seen of $\frac{1}{15}$ of the ☉'s semidiameter: and yet you must confess these great ones could only be the conjunctions of some, not all. 2. There are many times seen in the ☉'s superficies, a great number of spots, whose diameters added together, would do more than twice fill the space you speak of. I myself have seen it, and so I believe have you. Gassendus affirms, there are sometimes 40 seen at once: if this was by conjunction of planets, in every appearance, there was at least 80 bodies at once on this side the ☉; it may be as many on the other side, besides those unseen (by your reflection or otherwise) which doubtless must be far more than seen. For it is a most rare, and I think unheard of thing, to see but 3 (which is less than the half) of our planets, conjoined in visible ☌ at once: so that without question, if they be planets, they are many hundreds; which must have so many several orbes, and which certainly cannot be done in so narrow a compass, as the $\frac{1}{10}$ of the ☉'s semidiameter. And that they cannot have any larger (I suppose not so large an) extent from the ☉'s superficies, may be proved by their motion through the visible hemisphere of the sun's spherical body, by comparing the swiftness of their motion towards the middle and sides together. 6. If one of these (imagined) planets be swifter than another, as they must needs be, then the ☌ of 2 or 3 swifter ones would make a spot of speedier motion than the ☌ of 2 slower ones: but the motion of all about the ☉'s center, is always equal; yea, and the spots retain the same position one to another, (considering the sun's sphericity, and the angle of their appearance to us) just like the fixed stars. So affirms Gassendus, moveri omnes eodem et uniformi motu, adeo ut, cum plures fuerint, nulla antevertat aliam, sed eundem tenorem in disco ☉ perinde servent inter se, ac servant fixæ in firmamento.

"As for that other annual motion of the spots, you speak of, from west to east, upon their axis inclined above 8 degrees to the ecliptick; I suppose it is not any real motion of the orbes of those solar planets or spots, but only a visible motion so appearing, caused (in Kepler's systeme) by the sun's rolling upon its own center in the midst of all the orbes, not exactly in the way of the temporary ecliptick, but in the via regia (as Kepler calls it) inclined certain degrees to the temporary; thereby turning about with him, the same way, his adventitious or excrementitious parts, the spots, by his magnetical or sympathetical rayes. And hence may be demonstrated the appearance of that annual motion in the sun's spots you speak of. See Galilæus, Syst. Cosm. p. 339, & seq. So also in Ptolemie's and Tycho's systeme, the same appearance may be demonstrated, supposing the ⊖ fixed in the middle of the universe, and the ☉ rolling round upon the same poles of that via regia (or way of the spots) and keeping his axis in parallelism continually towards one and the same part of the universe. This may be certainly demonstrated, although Galilæus there affirms the contrary. Other hypotheses of that motion may be feigned, as by the annual conversion of the poles of the via regia about the poles of the ecliptick in the sun's body: but none I conceive so compendious, as the one of the former. For my part, I incline to the first: yet if when we see you, you shew us any more likely theory, for my part I shall be ready to consent to you in any thing with reason.

"Thus you have, what for the present, I conceive of these maculæ solares. Fromundus mentions one Jo. Tarde Gallus, who thinks them to be secondary planets; who hath written a book of that subject, and calls them astra borbonia: but I could never yet see it. What you, or he, or others may alledge for that opinion, I know not. In the mean time it were too much levity in me, against my judgment, to acknowledge them stars; unless I see at least some possibility how they may be so, or some probability why they should not rather be spots. Which when you, or they, do produce from better grounded reasons, optical experiments, or demonstrations, I shall willingly recant my opinion.

"In the mean time, let me encourage you to proceed in your noble optical speculations. I do believe there are as rare inventions as Galilæus' Telescope, yet undiscovered. My living in a place void of apt materials for that purpose, makes me almost ignorant in those secrets; only what I have from reason, or the reading of Kepler's Astron. Opt. and Galilæus. If you impart unto us any of your optical secrets, we shall be thankful, and obliged to you, and ready to requite you in any thing we can.

"It is true which you say, that I found Venus' diameter much less than any theory extant made it. Kepler came nearest, yet makes her diameter 5 times too much. Tycho, Lansberg, and the ancients, about 10 times greater than it was. So also they differ in the time of the ☌ as far from the truth. By Lansberg the ☌ should have been $16^h\ 31'$ before we observed it; by Tycho and Longomontane $1^d\ 8^h\ 25'$ before. By Kepler, who is still nearest the truth, $9^h\ 46'$ before. So that had not our own observations, and study, taught us a better theory than any of these, we had never attended at that time for that rare spectacle. You shall have the observation of it when we see you. The clouds deprived me of part of the observation, but my friend and second self, Mr. Jeremiah Horrox, being near Preston, observed it clearly from the time of its coming into the sun, till the sun's setting; and both our observations agreed, both in the time and diameter, most precisely. If I can, I will bring him along with Mr. Towneley and myself to see Yorkshire and you. You shall also then have my observation of the sun's last eclipse here in Broughton, Mr. Horrox's between Liverpoole and Preston, and Mr. Foster's at London. Lansberg in eclipses, especially of the ☾, comes often nearer the truth than Kepler, yet it is by packing together errors; his diameters of the ☉ and ☾ being false, and his variation of the shadow being quite repugnant to geometrical demonstration. His circular hypotheses Mr. Horrox, before I could persuade him, assayed a long time with indefatigable pains and study to correct and amend; changing and turning them every way, still amazed and amused with those lofty titles of perpetuity and perfection, so impudently imposed upon them, until we found, by comparing observations in several places of the orbes, that his hypotheses would never agree with the heavens for all times, as he confidently boasts; no, nor scarce for any one whole year together, alter the equal motion, prosthaphæreses, and excentricity howsoever you will.

"Kepler's Elliptick is undoubtedly the way which the planets describe in their motions; and if you have read his Comment. de motu ♂is, and his Epit. Astron. Copern. I doubt not you will say his theory is the most rational, demonstrative, harmonious, simple, and natural, that is yet thought of, or I suppose can be; all those superfluous fictions being rejected by him, which others are forced so absurdly to introduce. And although in some respects his tables be deficient, yet being once corrected by due observations, they hold true in the rest, which is that argument of truth which Lansberg's and all others want.

"Your conceit of turning the circle into 100,000,000 parts were an excellent one, if it had been set on foot when astronomy was first invented. Mr. Horrox and I have often conferred about it. But in respect that all astronomy is already in a quite different form, and the tediousness of reducing the tables of sines, tangents, and all other things we should have occasion to use, into that form; as also some inconveniences which we foresaw would follow in the composing the tables of celestial motions, together with the greatness of the innovation, deterred us from the conceit. Only we intend to use the centesmes or millesmes of degrees, because of the ease in calculation. I have turned the Rudolphine tables into degrees and millesmes, and altered them into a far more concise, ready, and easy form, than they are done by Kepler. My occasions force me to put an abrupt end to my unpolished lines, and without more compliments, to tell you plainly, but sincerely, I am

From my house in Broughton, near Manchester, this 7th August, 1640.

Your loving friend,
(though de facie ignotus)
WILLIAM CRABTRIE."

The superscription of this letter is, To his loving Friend Mr. William Gascoigne, at his Father's house in or near Leeds in Yorkshire.

This with most of the letters between Mr. Crabtrie and Mr. Gascoigne, together with other very valuable papers of Mr. Horrox, Mr. Towneley himself, Mr. Collins, Mr. S luse, and other great men, were imparted to me, the last month, by the great favour of Charles Towneley, Esq. son of the late Richard Towneley, Esq. of Lancashire.

An Account of some Antiquities lately found at Corbridge in Northumberland. In a Letter from the Rev. Dr. Hugh Todd, (S. T. P.) Sub-Dean of Carlisle, to Dr. Edmund Halley, F. R. S. Dated Carlisle, Feb. 17, 1710-11. N° 330, p. 291.

The pretended entire skeleton of a prodigious monster of the human form, about 22 feet long, has lately amused the country. The place where the bones were found is Colchester, a mile west of Corbridge, Northumberland, on the north banks of the river Tyne; formerly a Roman colony, but at present a field of corn; nothing of antiquity remaining but some walls and rubbish; which show it to have been a very large fortress. Stones, which have been dug up, with figures and inscriptions upon them, have been all removed to Corbridge, which has risen out of its ruins. There I saw altars inscribed, one IMP. M. AVRELIO, another LEG. II. AVG. COH. IIII. But the most remarkable, is that which stands in a corner of the church-yard, dedicated to Hercules, in old capital Greek characters. The characters are indifferently plain, and as I could read them, as represented pl. 17, fig. 1; i. e. Herculi Tyrio Divina Dona, Archi-Sacerdotalia, vel, per Summum Sacerdotem offerenda. The altar seems to have been erected by some of the Asiatic Phœnician auxiliaries; who might be in garrison here, near the frontier, under Urbicus Lollius, in the time of M. Aur. Antoninus, about A. D. 140. The altar is very large, hollow at the top, as usual, for incense; on the sides are engraved a bull's head, with garlands, and sacrificing instruments.

The teeth and bones, which were discovered, by the river Tyne's breaking in upon the bank, were found near the foundations of the old fort; and neither higher up, nor lower down, than the ruins of it seem to reach. I examined the person who made the first discovery; and was as exact as possible in the remarks, which I made myself upon the spot. On the whole, it appeared to me, that there never was an entire skeleton found in that place. The teeth and bones, lie in the bank, in strata; sometimes at one, sometimes at two yards depth, for above 200 yards in length. In some places, there appears to have been a sort of pavement or foundation of stone; which runs along with the bones, stratum super stratum; sometimes above, sometimes below them. The bones are of different sizes: the teeth, which are most perfect and entire, are very large; some 3 or 4 inches in compass: ribs, shank-bones, &c. many of them not exceeding the ordinary dimensions of those of sheep and oxen. I could meet with no remains of horns; those being more easily corrupted than the bones, which are of a harder substance. The teeth look as if they were human; but I cannot affirm them to be so: and they lie sometimes at so great a distance from the ribs and shank-bones, that should any one compute the length of the monster, from one to the other, they might calculate to be 200 or 300 yards, as reasonably as 22 feet. The teeth and bones are in such numbers, that, with the help of a labourer or two, you might, in a few hours, gather a bushel of them.

From this account, it may not be unreasonable to infer: that the altar, here dedicated to the Tyrian Hercules, was very famous and much frequented: that there were sacrificed on it oxen, and such like creatures, as bisons and bonasus, with which the country hereabouts anciently abounded. The entire head and horns of one having been lately dug up in a marshy ground; resembling exactly those creatures, as they are described by Gesner, and others; and that their bones being all thrown together, and, according to the superstition of those times, laid under the foundation and pavement of the fort, are the very same bones, teeth, skulls, ribs, &c. which, by the rivers washing away the bank, are now discovered and brought to light. And, if I might be allowed to guess a little further, I might think it not impossible, that as Erkelens in Gelderland, is Herculis Castra, and Hertland in Cornwall, was Herculis Promontorium, so on the recess of the Romans, the Saxons, who succeeded them, might call this noted station Hercul-ceaster, and by corruption Colceaster, or Colchester, its modern name. And, what may somewhat confirm the conjecture, the adjacent town of Corbridge, which has risen out of its ruins, is called in the charter of H. I. whereby that king gave it to the secular canons of Carlisle, before the erection either of the priory or bishopric, Colbruge, and Colburgh, the same as Col-ceaster; the bridge, from whence it may seem to take its denomination, being of a much later erection. That oxen used to be sacrificed to Hercules, there needs no other evidence than the altar itself, on which an ox's head, with sacrificing instruments, are delineated. You will receive by the Oxford carrier a great many teeth and bones, by which you may be able to give a much better judgment, whether they are human or not, than I can; only I would observe this, that if it do not appear that they agree perfectly with the teeth and bones of oxen, it will not follow that therefore they must be human, and that there were men of prodigious stature who made use of them; seeing there were in these parts other creatures of very great size, to whom they might belong, and of whose teeth and bones we have now very few specimens to compare them with.

An Account of the Mosses in Scotland. By George, Earl of Cromartie, &c. F. R. S. N° 330, p. 296.

There are many grounds in Scotland called mosses, where the country people dig turf and peats. The surface is covered with a heathy scurf, under which there is a black, moist, spongy earth, from 3 or 4 to 7 or 8 feet deep; and in some few places twice or thrice that depth. They cut the heathy scurf with a flat kind of spade, which they force horizontally between the scurf and the spongy earth, and so turn up the scurf in flat thin flakes, which they call turf. This turf, over-run with the small roots of heath, when dried, makes a wholesome brisk fire, but with much ashes, of a whitish, duskish, or reddish colour; always the whiter, as it contains more of the woody roots.

The black spongy earth, which is under the turf, they cut out in oblong squares, with iron spades made of that shape, about 8 or 9 inches long, and about 4 or 5 inches broad; and as the men cut them up, the weaker men, women, and children, carry them in small wheel-barrows, scattering them on some dry ground, to be dried by the sun and wind; some become harder, some softer, according to the nature of the mould, or earth; the more solid, the better fire; and those are less esteemed which are more spongy. And when they have cut off one stratum of 4 or 5 inches deep, they proceed downward to another, till at last they come to the hard channel, unless they be stopped by water; which also they usually drain off, by making a trench to some descent, if they can, and if they cannot, there the water stagnates.

And such wasted pits, where water hinders them from cutting the spongy earth to the bottom, will in a good number of years be filled up again with new ground of spongy earth; which in progress of time will come to the consistence of peat moss, as at first, and a scurfy heath turf will at last grow on the top of it.

I have observed, that peat pits, which have been dug since I remember, have grown up again with new peats; and that sometimes oftener than once in the same pits, some mosses growing in shorter time than others. But I have observed also, that when they dig the peats to the channel, or bottom, and in places where the water runs off, and does not stagnate, that the mosses did not grow, nor renew there again. Which induced me to order my tenants, not to cut the mosses to the channel, nor in very large openings, but rather in smaller pits, that they may grow again more hastily; and the event has answered my design. But within these few days, Sir Robert Adaire told me, that without cutting the mosses in the method of pits, but by cutting in fully to the channel, and by laying the heathy turf, which is cut off the top of the moss, on the channel, so as to cover it over, that in progress of time a moss would grow there again, but not so readily as in the pits.

I never observed any of these mosses, which did not lie on plains, though the heathy or heathry turf over-spreads the faces and declivities of the mountains, for the most part; there are many mosses, which lie very high on these hills; sometimes not far from the top. But the peat mosses are always in a plain, though there be descents to them, and descents from them; yet I never observed them to stand on such a plain as the water might stagnate on: and they always have a descent to them, from some higher grounds, by which water descended to that plain; which I take to be the parent of peat.

In many of these mosses, there are found quantities of fir and oak wood; but I never observed nor heard of other kinds in them. They are usually found in whole trees; but the smaller branches are seldom found unconsumed. I have seen many, and very great trees of both kinds: generally the oak is black; the fir sometimes whiter, sometimes redder, as is observed in all firs: but neither fir nor oak are found with any bark on them. The fir is generally as fresh and tough, and as fit for any use, as any other old timber: only they have imbibed so much water, that they take a long time to dry, and be fit for use, especially the oak; so that when put into any small work, it readily warps, and changes its figure. We never find any of the oaks standing in the woods, have that blackness; so that I presume that colour arises from the water.

There are many places, where woods do not now grow; and yet the mosses in these places are well stored with this kind of under-ground timber, more especially the fir; such are the Orkneys, and the Lewes isles, Caithness, Tarbartness, and the coast of Buchan. So that probably there have been woods formerly in these places: and this is further confirmed by the following account.

In the year 1651, I being then about 19 years old, and occasionally in the parish of Lochbrun, passing from a place called Achadiscald, to Gonnazd, I went by a very high hill, which rose in a constant acclivity from the sea; only in less than half a mile up from the sea, there is a plain about half a mile round, and from thence the hill rises in a constant steepness for more than a mile in ascent. This little plain was at that time all covered over with a firm standing wood; which was so very old, that not only the trees had no green leaves, but the bark was quite thrown off; which the old countrymen, who were with me, said was the universal manner in which fir woods terminated; and that in 20 or 30 years after, the trees would commonly cast themselves up from the roots, and so lie in heaps, till the people would cut, and carry them away. They likewise showed me, that the outside of these standing white trees, and for an inch deep, was dead white wood; but what was within that, was good solid timber, even to the very pith, and as full of rozin as it could stand in the wood.

About 15 years after I had occasion to come the same way, and calling to mind the old woods which I had seen, observed there was not so much as a tree, or appearance of the root of any; but instead of them, the whole bounds, where the wood had stood, was all over a flat green ground, covered with a plain green moss. I asked the people what became of the wood, and who carried it away? they told me, nobody was at the pains to carry it away; but that it being all overturned from the roots by winds, the trees lay so thick and swerving over one another, that the green moss or fog had overgrown the whole timber; which they said was occasioned by the moisture that came down from the high hill above it, and stagnated on that plain; and they said none could pass over it, because the scurf of the fog would not support them. I would needs try it: and accordingly I fell into the arm-pits, but was immediately pulled up by them. Before the year 1699, that whole piece of ground was turned into a common moss; where the country people are digging turf and peats, and still continue so to do. The peats were not of the best, being soft and spongy, but become better and better, and I am informed it now affords good peats. This matter of fact shows both the generation of mosses, and whence it is that many of them are furnished with such timber.

These highland woods are usually stored with other kinds of timber; as birch alder, ash, besides shrubs and thorns; yet we never find any of these woods remaining in the mosses. What the reason may be, that the fir and oak do not grow in several countries, where they are found so plentifully in the mosses, is matter of inquiry. It seems remarkable, that in a moss near the town of Elgin in Murray, though there is no river nor water that runs into the moss, yet 3 or 4 feet in the moss, there is a sort of small shell-fish, resembling oysters, found numerously in the very body of the peats, and the fish alive within them; though no such fish is found in any water near the moss, nor in any adjacent river; nor even in the stagnating pits, that are in that moss; but only in the very substance of the turf.

On the Bogs in Ireland. By Dr. Hans Sloane, Sec. R. S. N° 330, p. 302.

What the earl of Cromartie observes of the bogs or mosses in Scotland, and the wood found in them, I can confirm, having seen many such in the north of Ireland; where, when the turf diggers have come to the bottom, or firm ground, by having dug out all the earth proper to make turf or peat, and come to the clay or other soil, by draining off the water, then there have appeared roots of fir trees, with their stumps standing a foot or two straight upright, and their branches spread out on every side horizontally on that firm surface; as if that had been formerly the outward face of the ground, and place of their growth. And I remember to have observed these roots to be sometimes so near one another, as that their branches were, as it were matted, grew over, and gave place to one another, as we every day see in roots of trees where they grow too close. I saw once the body of a fir tree dug up so large, as to be judged fit for the main post of a windmill; which was discovered, as many of them are, which are not found in digging turf, by the grass, which grow over it being, in a very dry summer, of a yellowish colour.

The Rev. Mr. de la Pryme sent me some of the cones found with this timber in the great fens of Lincolnshire, which differed in nothing from those of the Scotch fir, which is so plentifully growing in Scotland at this day, and which some years since were judged so proper by some to afford masts for the Royal Navy, that I think some persons were sent thither for that purpose, But they were not able to bring about what they intended, from the difficulties in the roads by which they were to be conveyed to the sea; which, in Norway, I have heard is in a great measure effected by the rivers. Cæsar indeed, in his Commentaries says, that the sorts of timber in this island, are the same as in France, præter fagum et abietem, except beach and fir. But lord Cromartie's account is a sufficient witness of his mistake as to one sort of these trees, and the beaches in the chiltern countries near London, prove the same as to the other. For the uses of this under-ground timber, besides those of other wood, it is split into pieces; and being lighted, supplies the use of candles. It is also made into ropes; as may be seen in the Musæum of the Royal Society, by a long piece of such rope, bought in Newry market in Ireland, and presented to the Royal Society; the long soaking in water having rendered the wood of those trees fit to be made into ropes. This seems to prove, that as the soaking of hemp, flax, aloe leaves, &c. in water, dissolves the pulpy part, and leaves the fibrous, fit for making into threads and ropes, so the long soaking of trees may, in length of time, effect the same, or a similar change in those of wood and timber.

There are some things remarkable which may here be noticed. One is, that I have seen what I thought had been pieces of wood, not only in clay pits, but even in quaries or stone pits, in the blocks of stone raised out of their strata, or layers; and have been also assured by Mr. Bellers, that he has seen large pieces of wood in the stone pits in Gloucestershire; and also that in Lancashire there is a moss, or turf bog, where the black spongy mould, made use of for peats, smells very strong of bitumen, or petroleum; of the oil of which it yields a very great quantity by distillation. And likewise, the late Sir Edward Hannes told me, that near the lord Blessington's house at Blessington in Ireland, there appeared a light where the horses trampled with their feet on a certain space of soft ground. On my desire he procured me some of this mould, which I have yet by me, and which agrees exactly in its dark colour, lightness, &c. with peat earth. And on examination of this by a microscope, I found the light proceeded from many small half transparent whitish live worms, which lay in it.

The blackness of the oak, in such bogs, in my opinion, arises from the vitriolic juices of the earth soaked into the oak, which being astringent is turned black by them. Ink is made of galls, an astringent excrescence of a sort of oak in Turkey, made by an insect there; and of green vitriol, which is made of the pyrites dissolved by rain water, and iron. Earth of all sorts, and even human calculi, and the ashes of vegetables, have in them particles of iron, in greater or less quantities. The pyrites is also very common. The particles of iron coming to be dissolved by this pyrites, subacid, or other salts, dissolved by water, or perhaps by water itself, and carried into these bogs, there fasten to the tree, soak into it, and turn it black. These particles in some river water, fastening to the oak timber floated in it, give the same a darkish colour; as noticed by Mr. Pepys, in his Naval Memoirs of England, p. 71, where we are told by the chief ship-builders of England, that the best foreign plank for the Royal Navy was brought either from Dantzic, Quinborough, that is Koningsberg, or Riga, of the growth of Poland and Prussia, or from Hamburgh; namely, that sort which is shipped from thence of the growth of Bohemia, distinguished by its colour, as being much blacker than the other, and rendered so, as is said, by its long sobbing in the water, during its passage thither.

In the turf bogs of Ireland 14 feet deep, are found not only the mouse-deers horns, mentioned in one of these transactions, but likewise their whole skeletons, wherein the bones bear the same proportion to the like bones of other deer, as the horns bear to their horns. There are also found in them, gold chains, pieces of money, and roots of heath, several musci, or mosses, and branches of trees so soft, as to give no resistance to the turf spade: and I was told, that in cutting turf in one, they at several feet deep cut through what the Irish call a ruskin of butter, which was a firkin, or vessel, made of the barks of trees, used by the old Irish for putting up their butter. And I remember, that in digging the wet dock at Deptford, there were found at the bottom, about 9 feet deep, grass leaves, hazle-nuts, and roots of trees: and there also was found a piece of money, as they called it; but on examination, it proved to be a leaden seal, to some bull of Pope Gregory IX, who continued Pope from A. D. 1227 to 1241.

From Leland's Itinerary, vol. v. p. 13, &c. written in the reign of king Henry the 8th, we may learn the common opinion in his days, of the cause of the destruction of woods, with the growing of mosses and pools; and that at that time in Wales, the sense of the natives was, that the subterraneous trees found there, had formerly grown there; as appears by the following extracts, in his own words, the language of the times.

"In these deyes in Mone, wher they digge turves, be founde greate rootes of trees that serve men for wood. For after the trees wer cut doune, sogging yerth and mosse overcoverid them, and now the same yerth parid away for turves, the old mayne rootes appere. Likewise at low water, about al the shores of both shores of Aberdein and Towen Merioneth, appere like rootes of trees. I saw hard by, on the lift honde, a great fenny more, owt of wich the inhabitantes therabout digge turfes for fier, and by the same fenne is a fair Llin cawilid Llinridde, ii miles from Strateflure. Strateflure is set round about with montanes, not far distant, except on the west parte, wher Diffrine Tyne is. Many hilles therabout hath bene well woddid, as evidently by old rotes apperith, but now in them is almost no woode. The causses be these; first the wood cutt doun was never copisid, and this hath beene a great cause of destruction of wood thorough Wales. Secondly after cutting doun of wooddys, the gottys hath so bytten the young spring, that it never grew but lyke shrubbes. Thirddely men for the nonys destroied the great woddis, that thei shuld not harborow theves.

"From Whitchirch a mile and a half of I cam by the pale of the large parke of Blakmer, longging to the erle of Shreusbiri, wherin is a very fair place or loge. The park hath both redde dere and falow. In the park (as I hard say) be iii fairle poles, of the wich I saw by the pale the largest caullid Blakein, wherof the park is namid. It is to be supposid that thes pooles for the most part in morisch groundes, and lying sumwhat in low groundes, dreane the moist places about them, and so, having no place to issue owt, stagne there. Sum be likelyhod have begon of marle pittes. For the sandy grounde of sum partes of Shropshire, and especially of Chestreshire and Lancastreshire, wille not bere corne plentifully but it be merlyed.

"From Blakemere to Byklem, in a fosse iii miles of sand, hard by Cholmeley, first I saw the great numbre of firre trees, the wiche the inhabitantes thereby communely digge up for fier wood, but ther did I se no fyrre trees grouing. Oftentimes in diggin in this mosse or more for petes or turves, they finde the hole trees of the first, sum short and sum veri long, without twike or bow, lying sumtime not a foote, sumtime iii or iiii foote depe in the ground. But how or when thes trees cam doune, other be cutting or wind faulle, no manne ther can telle. The wood of them in burning savorith of resine. Morle (in Darbyshire) Mr. Lelandes place is buildid, saving the fundation of stone squarid that risith within a great moote a vi foote above the water, al of tymbre after the commune sort of building of houses of the gentilmen for most of Lancastreshire. Ther is as much pleasur of orchardes of great varite of frute and fair made walkes and gardines as ther is in any place of Lancastreshire. He brennith al turfes and petes for the commodite of mosses and mores at hand. For Chateley mosse that with breking up of abundance of water yn hid did much hurt to landes thereabout, and rivers with wandering mosse and corrupte water is within less than a mile of Morle. And yet by Morle as in hegge rowes and grovettes is meately good plenti of wood, but good husbandes keep hit for a jewell. Syr John Holcroftes house within a mile or more of Morle stoode in jeopardi with fleting of the mosse. Riding a mile and more beyond Morle I saw on the right hond a place nere by of Mr. Adderton, and so a ii miles of to Lidiate Mosse, in the right side wherof my gide said that ther were rootes of fyrre wood.

"Al Aundernesse for the most parte in time past hath beene ful of wood, and many of the moores replenishid with hy fyrre trees."

De Araneis et Scarabæis Philippensibus. Ex MSS. R. P. Geo. Jos. Camelli. Communicavit Jacobus Petiver, S. R. S. N° 331, p. 310.

Microscopical Observations on the Animalcula in the Semen of Young Rams. By Mr. Leuwenhoeck, F. R. S. In a Letter to Mr. James Petiver. N° 331, p. 316.

The latter end of June 1711, I procured the testes of a young ram, and having made a small incision in the lower part of each of them, where was a protuberant roundness, and from whence the vasa semen deferentia proceeded, I squeezed a little whitish matter out of them, which I immediately placed before a microscope, and clearly observed a vast number of animalcula living and moving: and because the animalcula could not be seen so distinctly, by reason of the vast number and the quick motion of them in the little slimy matter, in which they swam, I took a little rain water, about the quantity of a great pin's head, and mixed it with an equal quantity of the said matter; which being thus very much diluted, I placed it again before the microscope, and then we could see very clearly the dead bodies of those animalcula, lying in the liquor.

About 10 or 12 days after, I got two other testes of a young ram, which were something smaller than the former; and proceeding with them as before, I found that the whitish matter was much more fluid than the former; and that there floated in it a vast number of very clear small globular bodies, of which I could not discover any of the least particles to be like those animalcula above-mentioned. From which observation I thought with myself, that as fœtuses lie in the uterus, in such a globular form, as is consistent with our bodies; so in like manner, these globules, which I saw, were animalcula proceeding from the semen masculinum, that were not yet arrived to their perfect shapes.

Eight days after, I got two other testes of a young ram, which were smaller than the former; which having opened in the like manner, as I did the other, I found not only no animalcula that appeared to be living, but the transparent globules were not the 4th part so large as the above-mentioned.

Upon the 13th of July, I again caused the two testes of a sucking ram to be brought me; which was about 14 or 15 weeks old, and its testes were little more than half as large as the first. Having opened these likewise, in that part where the vasa semen deferentia were thickest, and taken one of them out, and viewed it with the microscope, I judged that those globules which I observed in it were so small, that 25 of them were not equal to one of the large ones above-mentioned.

July the 18th, I made an experiment of the same nature with the former, the lamb being almost as old as the preceding; but I could not discover any globules larger than the last mentioned, nor any thing that appeared to be living.

July the 20th, I repeated my experiment on the testes of a young ram, which was very near as large as the first of all; which having opened in the usual place, I saw great numbers of living animalcula: I discovered likewise in the fluid matter, which I had taken out, several globules floating, which I imagined likewise might be some of those imperfect animalcula above-mentioned. All these young rams were killed the day before their testes were brought to me.

July the 25th, at 9 o'clock, I got two testes of a young ram, which they had killed the day before at 11 o'clock before noon: these were larger than any I had seen this year. I immediately examined one of them, opening the part before-mentioned, and discovered the animalcula in so great a number, and like clouds moving among each other, as to afford a very pleasing sight. And this I continued to do till 12 o'clock; and consequently till the testes had been 25 hours out of the ram's body, and had lain wrapt up in a paper upon an earthen dish, and were exposed to the cold of the whole night. I then took a second testicle, on which I had as yet made no remarks, and I bound it up in a linen cloth, and kept it in a warm place, in order to view it the next day; and I observed the first testicle till 5 o'clock in the afternoon, at which time I saw some animalcula living; but at 12 o'clock before, I discovered a hundred living animalcula, for one that I saw at 5 o'clock: for between 12 and 5 o'clock I had made 25 several observations, and those almost always by making new incisions with a knife into the testes, and by squeezing the matter out of the incision, though no larger than a pin's head.

Now while these animalcula were swimming in the fluid matter, I observed often through the microscope, that the first part of their bodies gave a very bright glance; just as we see in small fishes swimming in the water, when they turn on their sides or bellies, and cast a glittering brightness to the eye: from which I imagined, that the upper part of the small bodies of the animalcula in semine masculino of the rams, were flattish, and that the brightness proceeded from their exposing those flattish sides to the sight in swimming. The next morning at 7 o'clock, I viewed the matter of the second testicle, which I had bound up in a cloth, with my microscope; but I could not perceive any thing that had the least life in it; and the testicle had begun to spoil, having a bad smell.

Now since we perceive that the animalcula, in the testes of a ram, can live 32 hours after the ram is dead, we may very well conclude, that the said animalcula in semine masculino of a ram, being admitted into that part of the uterus of the ewe, called tuba Fallopiana, will live much longer, that being the place which nature has provided for them. From whence it may follow, that after the copulation of the male and female, the animalcula may be 2 or 3 days in coming into that part of the uterus, where they receive their nourishment, and consequently before the female is impregnated. And the same may be applied to other animals.

An Account of a Lunar Rainbow seen in Derbyshire, and of a Storm of Thunder and Lightning which happened near Leeds in Yorkshire. By Mr. Ralph Thoresby, F. R. S. N° 331, p. 320.

The iris lunaris being so rarely seen, that Dr. Plot tells us (Nat. Hist. of Oxford, cap. 1, § 7,) that several learned and observing men never saw one in their lives, and that even Aristotle himself observed only two in above 50 years; the ensuing account, which I had from a gentleman of great veracity and ingenuity, will be the more acceptable. He was lately in Derbyshire, where, on Christmas last, he was at Glapwell Hall; and walking towards Patterton-Green, about 8 in the evening, he observed with great satisfaction the bow, which the moon had fixed in the clouds: she had then passed her full about 24 hours; the evening had been rainy, but the clouds were dispersed, and the moon shone pretty clear. This iris was more remarkable than that which Dr. Plot observed at Oxford, the 23d of November 1675, that being only of a white colour, but this had all the colours of the solar iris, exceedingly beautiful and distinct; only faint in comparison of those we see in the day; as must necessarily be the case, both from the different beams that cause it, and the disposition of the medium. What puzzled him the most was the largeness of the arc, which was not so much less than that of the sun, as the different dimensions of their bodies, and their respective distances from the earth seem to require: but as to its entireness, and beauty of its colour, it was surprising. It continued about 10 minutes, before the interposition of a cloud hindered his further observation.

The beginning of the same month had been remarkable here in Yorkshire, for such thunder and lightnings, as are not common at that time of the year; particularly the evening of the 5th day, and the morning of the 12th; when a man and two women coming early for this market, were so furiously encountered, that the women took shelter at the first house they came at; but the man proceeded on his journey, though the lightning was so severe, as he was riding over Bramham-Moor, that he thought his hair had been burnt, and face scorched, at one flash; which being more severe than the rest, did actually set on fire the stick he had in his hand, as he was ready to depose upon oath.

An Account of a Meteor, which was seen in Yorkshire, and other neighbouring counties, May 18, 1710. *By Mr. Ra. Thoresby, F.R.S.* N° 331, p. 322.

A strange meteor was seen at Leeds on Holy Thursday, 1710, which the common people call a flaming sword. It was seen in the neighbouring towns, but a great way north, as also above 50 miles south of Leeds. It appeared here at a quarter past 10 at night, and took its course from south to north: it was broad at one end, and small at the other; and was by some thought to resemble a trumpet, and moved with the broad end foremost. The light was so sudden and bright, that people were startled to see their own shadows, when neither sun nor moon shone upon them. This is remarkable, that all persons, though at many miles distance from each other, when they saw it, thought it fell within 3 or 4 furlongs of them, and that it went out with bright sparklings at the small end. An ingenious clergyman told me, that it was the strangest deceptio visus he was ever sensible of, if it was not absolutely extinguished within a few paces of him; and yet others saw it many miles off, further north, in a few moments. It was likewise seen in the counties of Nottingham and Derby, as well as those of York and Lancaster.

Account of an Ancient Tesselated, or Mosaic Work, at Leicester. By the Rev. Samuel Carte, of Leicester. N° 331, p. 324.

This tesselated work, in a cellar here, near All-Saints Church, is generally called Actæon, by such authors as mention it; but the bare inspection of it shows, that it is a representation of the fable, which says, that a person having found fault with Venus, she, to be revenged of him, engaged her son Cupid to make him fall in love with a monster.

It was first discovered about 40 years ago, on digging the cellar, at about a yard and half under the common present surface of the earth. What extent the whole pavement was of, is not known; but this figure, which, by order of the master of the house, was preserved, is an octagon, surrounded by a list, as you see in part represented in the corners of the drawing, fig. 2, pl. 17. Without this, though not here represented, is a twist or wreath of various colours; and round that, is a second like the former. These two lists, with the wreath between them, are 6¼ inches broad. The perpendicular and transverse diameters of the area, are just a yard; but the others, from corner to corner, are a yard, 2½ inches. The man's figure, from head to foot, is 2 feet 4½ inches. Cupid seems to be 2 feet; but his feet, as well as the bottom of the monster, are wanting, the tessellæ representing them being gone. The whole area of the figure, which is here left blank, ought to be filled up with white tessellæ, in like manner as is seen in some intervals of the figures.

An Account of the Repetition of Dr. Hook's Experiment, concerning two Liquors which, when mixed together possess less space, than when separate: with another Experiment confirming the same. By Mr. F. Hauksbee, F. R. S. N° 331, p. 325.

The experiment related by the late Dr. Hook, in one of his papers delivered to me by Mr. Waller, is concerning two liquors, which when mixed together, possess less space than when separate; which he calls a penetration of dimensions: and further he adds, that this penetration is the cause of heat, of fire, of flame, of the power of heat, fire, and gunpowder, and several other surprising phænomena in nature. On which account I thought it very worthy an examination, by a repetition of the same.

Accordingly I procured a bolt-head, such as Dr. Hook describes, with a long small stem, which I filled nearly full of common water. The stem was marked into several divisions, on a piece of paper pasted on it; by which means I nicely observed the height of the surface of the water. Then pouring as much of it out as filled a certain measure, which being thrown away, I filled the same measure, as nicely as possible, to the same height, with strong oil of vitriol; which I returned into the bolt-head, instead of so much water taken out. On the mixing of these liquors ensued a pretty strong ebullition, and abundance of airy particles were visibly extricated, and the surface was not so high in the stem considerably, as when it was possessed only by water. And it is to be observed, that though they became very warm, yet, contrary to the nature of most liquors in such a state, they continued to possess less and less space; which was visible by the sinking of the surface in the stem of the bolt-head; and in about half an hour's time it had descended above an inch; and on viewing it 4 days after, it had subsided at least 7 inches below the mark I had left it at. Now whether the ebullition produced by the heat, might not evaporate that quantity, which it seemed to lose in space; or whether, in so many hours there might not be such an evaporation of the parts of the fluid, as to become equal in bulk to the quantity of the dispossessed space; I satisfied myself in the following manner.

Into an upright glass, that would hold about 3 ounces of water, I put a quantity of the same fluid equal to 885 grains: into another glass of the same form, but smaller, I put a quantity of oil of vitriol equal to 456 grains; which, with their respective glasses, I weighed altogether in a nice balance: after which, I put the oil of vitriol, glass and all, into that which held the water; where immediately a very great ebullition began, and the glass that contained them became so hot, as to be but just endured in the hand. I found in 2 minutes time it had lost of its weight about 2 grains; and at the end of an hour, or better, it had decreased in all only $6\frac{1}{2}$ grains: by that time the conflict was wholly ceased, it being then nearly reduced to the temperature of the outer air. Afterwards I weighed them at several times, but found them in the same state, as to their weight, as last mentioned. I continued them in the scale till the next morning, when I likewise could distinguish no manner of alteration in the forementioned weight. From whence it plainly appears, that the decrease of bulk on the mixing of these liquors, does not proceed wholly from an evaporation of their parts; since by the last experiment, the evaporation continued no longer than the fermentation lasted; but the decrease of bulk in the bodies, seems not to be performed all at once, or in so short a time, as may be observed in the first experiment.

Account of an Experiment, to produce Light through a Metallic Body, under the Circumstances of a Vacuum and Attrition. By Mr. Fr. Hauksbee, F. R. S. N° 331, p. 328.

It may be remembered what success I had in producing light through bodies, such as sealing-wax, pitch, and common sulphur; which gave me some probability, that under the same circumstances I might likewise make some such discovery through a metallic body. Accordingly I caused a glass hemisphere to be made very strong: to this hemisphere I procured another, of burnish brass exactly made, to fall with its brim about an inch within the glass, that I might the better cement them together; which I did securely from any ingress of the air in that part. Thus, when joined, it became nearly a globe; only the diameter through its axis, was somewhat more than its transverse diameter, which was a disadvantage to its strength, as the sequel of this account will show. In this manner I exhausted all its air, at least nearly so, and then put it on the machine to give it a circular motion, as usual in such experiments. On applying my hand to the brass hemisphere in motion, no light could be discovered within: I then rubbed it with a deal stick, but the success was the same. Afterwards I applied a piece of sealing-wax, which has in itself a very electrical quality: this wax, rubbing roughly on the brass, seemed to shake the parts of it; yet not any the least glimpse of light appeared. I then held the flame of a candle to the brass in motion, which something more than warmed a circle on it; hoping by this means to excite or obtain some discovery from it. Yet, though a smart attrition was made on that part, it was altogether unsuccessful. Being tired, I let in the air, and suspended my further trials till the following night. At which time, when I had exhausted the air from within the globe, I began the attrition with a coal cinder; which being somewhat rough, I thought it might shake the parts of the metal, and put them into such a state or mode, as to exhibit an appearance of light; but this, and whatever else I then tried, was to no purpose. In this exhausted state I left the globe on the engine, to consider a little what further trials to make; with what bodies, and in what manner to proceed with them: but to my great surprise, in about an hour after, being in the next room, I heard a noise almost as loud as a musket when fired; and immediately coming into the room, I found the glass of the globe broken all to pieces, and the brass hemisphere on the ground; which I took up, and found several bruises it had received from the violent strokes of the broken glass, which had dispersed itself in pieces all over the room. A large looking-glass, at least 3 yards from it, was cracked almost from top to bottom, and quite across the middle, by a blow from a fragment of it; for where it struck the glass, the cracks proceeded from it every way, like so many radii drawn from a center. From these experiments we may safely conclude, that if there be any such quality as light to be excited from a brass body, under the fore-mentioned circumstances, all the attritions of the several bodies used for that purpose, have been too weak to produce it. And indeed, considering the closeness of the parts of metal, and with what firmness they adhere, entangle, or attract each other, a small degree of attrition is not sufficient to put their parts into such a motion, as to produce an electrical quality; which, under the forementioned circumstances, I take to be the appearance of light in such a medium.

Johannis Freind, M. D. Oxon. Prælectionum Chymicarum Vindiciæ, in quibus Objectiones, in Actis Lipsiensibus Anno 1710. *Mense Septembri, contra Vim Materiæ Attractricem allatæ, diluuntur.* N° 331, p. 330.

Although in his Prælectiones Chemicæ Dr. Freind has displayed great ingenuity; yet in his eagerness to apply the Newtonian philosophy to the phænomena of chemistry, he has not duly distinguished elective attraction from the attraction of gravitation, and the attraction of cohesion. Hence it would be no ways interesting at this day, to insert the whole, or even an abstract, of this defence, grounded on the before-mentioned general properties of matter, and not upon specific chemical agencies.

An Account of a Book, entitled, Gazophylacii Naturæ et Artis. Vol. I. in 5 *Decadibus. In quo Animalia, viz. Quadrupeda, Aves, Pisces, Reptilia, Insecta, Vegetabilia; item Fossilia, corpora Marina, et Stirpes Minerales è Terrâ eruta, Lapides figurâ insignes, &c. Descriptionibus brevibus et Iconibus illustrantur. A Jacobo Petiver, Pharmacop. Londin. et Reg. Societ. Soc.* N° 331, p. 342.

This volume contains 50 tables of the figures of divers rare beasts, birds, fishes, serpents, and other reptiles; as also several rare insects, shells, plants and fossils, many of which the author obtained from the cabinets of several curious persons, and procured others from all parts of the world.

An Account of a New Island, raised up from the Bottom of the Sea, on the 23d *of May* 1707, *in the Bay of Santorini, in the Archipelago. By Father Goree, a Jesuit, who was an Eye-witness.* N° 332, p. 354.

Among the prodigies of nature, and the most surprising things which she has at any time produced, we may justly reckon an island, raised up from the bottom of the sea, in the bay which makes the harbour of the isle of Santorini, in the Archipelago; especially if we consider the situation, manner, and all the other circumstances of the formation of this new island. For what can be more surprising, than to see fire, not only break out of the bowels of the earth, but also to make itself a passage through the waters of the sea, without being extinguished? Or what can be more extraordinary, or foreign to our common notions of things, than to see the bottom of the sea rise up into a mountain above the water, and to become so firm an island, as to be able to re-roaring noise under ground, sulphureous exhalations, an insupportable stench, and a black smoke, which rose out of the sea with flames to the height of 10 or 12 cubits. The sea was then so tossed backwards and forwards, by the terrible shocks of the earth, that it overflowed and destroyed 30000 perches of land in Santorini; and the air was so infected with exhalations, which came from the fire, that 25 persons, and a great many beasts, were stifled. At last, when this island had not above 8 or 10 fathom of water to rise, so as to appear above the surface of the sea, the force of the subterraneous fire was so violent, as to open a passage before its time, by which the water of the sea entering in like a torrent, extinguished the fire, and in consequence this mass of earth and stones rose no higher.

How great soever the fright of the inhabitants of Santorini was, at the first sight of this new island, yet a few days after, not seeing any appearance either of fire or smoke, some of them, more bold than the rest, took a resolution to go and view its situation: which they did accordingly; and not imagining any danger, went on shore upon it. As they had no other design, but to satisfy their curiosity, they passed from one rock to another, on which they met with several remarkable curiosities; among which we may reckon a sort of white stone, which cuts like bread, and resembles it so well in form, colour, and con sistence, that were it not for its taste any one would take it for real bread. But what pleased them more, was a great number of fresh oysters which they found sticking to the rocks; which being very scarce in that country, by reason of the depth of the sea, they got as many of them as they could.

While they were busy about this, they perceived the island move and shake under their feet. This was sufficient to make them quit it immediately, and to return back faster than they came. In short, the rising of the island was visible to the eye, and it increased not only in height, but also in length and breadth. Though it was already between 15 and 20 feet high above the sea, it could not yet be seen from the mountain Merovigli, or the castle of Scaro, which stands on the shore, by the intervention of the lesser Kammeni. But 15 days after, they began to see it from Merovigli, and in a few days, from the castle of Scaro, situated upon another mountain, which, though it be very high, in respect to the sea, yet it is much lower than that of Merovigli, to which it joins. From whence we may judge how much this new island increased in height in a few days.

As the motion, by which this island increased every day in height, was sometimes equal, and at other times unequal, in respect to all the parts of so great a mass; so it did not always rise equally on every side. It often happened, that while it augmented in height and length on one side, it sunk down and decreased on the other. I one day saw a rock rise out of the sea, at 40 or 50 paces distance from the island, which I continued to observe for 4 days together; at the end of which time, it sunk again into the sea, and appeared no more: but this was different from what happened to some others; which having disappeared, as this did, they re-appeared again some time after.

The lesser Kammeni, which lies very near, was often shaken with the motion which raised this new island. From a small cleft, which we observed on the top of this little island, sometimes stones would break loose; which rolling down its sides into the sea, would raise, as it were, a cloud of dust.

At this time, the sea, which is contained within the gulf or bay of Santorini, several times changed its colour: at first it appeared green, afterwards reddish, and sometime after of a yellowish colour; with a stench, which, spreading itself over great part of Santorini, made us imagine that this colour proceeded from nothing else but the sulphur with which the sea was covered.

The smoke appeared first July 16: at which time, from a place in the sea where they could never before find any bottom, and which was above 60 paces distant from the new island, then called the white island, there rose up a ridge of black stones, which the Greeks, by reason of their hardness, call sideropetres, or iron-stones, which formed another island, named by the inhabitants the black island; and which was afterwards not only the centre of the whole island, but also of the fire and smoke and great noise, that was heard some time after.

The smoke which issued out of this ridge of stones, or black island, was very thick and white, as if it had proceeded from 5 or 6 lime-kilns joined together; and being carried by a north wind towards the castle of Acrotiri, it entered the houses of the inhabitants, but without causing any great annoyance, as it had no very bad smell. Four days after the smoke had thus appeared, they saw in the night time fire issuing out from the same place.

It was then that the inhabitants of Santorini, and especially those of the castle of Scaro, began to be really afraid. They considered that their castle was situated on a promontory, that was very narrow, and near the Black Island; and that the time drew near, in which they must expect it either to be blown up into the air, or overturned by some shock of the earth. They had continually before their eyes fire and smoke; and this dismal spectacle made them apprehend, that there might be several mines of vitriol and sulphur in the island of Santorini, which would soon take fire; and that therefore the safest way for them was, to abandon the country, and retire to some other island. And indeed some took this resolution; and there was no other way left to satisfy the rest, but by telling them, that if they would retire farther into the country, they would be safe there; and that if the castle was in danger, yet they must necessarily see the lesser Kammeni first entirely destroyed, not only because it lay between the castle and the Black Island, but also because it was much nearer to it than to the castle.

The Turks, who were then at Santorini, collecting the tribute which this island pays yearly to the Grand Seignior, were not less afraid than the other people: being amazed to see fire break out of the sea where it was so deep, they entreated the christians to pray to god, and especially to make their young children cry kirie eleison; because, as they said, the children not having offended God, they could more easily appease his anger, than older persons. The fire, however, was then but very little, being not above the breadth of the mouth of a furnace, and did not appear in the day time, but only in the night, from sun setting to sun rising; and was so far from spreading the whole length of the ridge of stones above mentioned, that it possessed only one small part of it, which was always afterwards the common passage for the smoke and fire.

As for the first, or White Island, we did not see there either fire or smoke; yet it continued to increase; but the Black island increased much faster. We saw every day large rocks rise up on every side of it, which made it sometimes longer, and at other times broader; and by their height we could very nearly judge how many feet it rose up every day or night. Sometimes these rocks joined to the island, and at other times they were at a distance from it; so that in less than a month, there were four little black islands, which in a few days after, united together, and made only one island.

As the smoke increased very much, and there was no wind stirring, it rose up to the middle region of the air, so as to be seen at Candia, Naxos, and other islands; and in the night time it appeared as a flame, to 15 or 20 feet high. The sea was at that time covered with a matter or froth, which in some places was reddish, and in others yellowish; from whence there proceeded so great a stench over the whole island of Santorini, that for fear of being infected, several persons were obliged to burn incense, and others to make fires on the tops of their houses, to disperse it, and to purify the air. By good luck it did not continue above a day and half; for a strong south west wind arose, which, together with the motion of the sea, dispersed the frothy matter, but occasioned other ways a great damage to the best part of the island of Santorini. At that time they were in great hopes of having shortly a very plentiful vintage; when this wind carried all the smoke on their vineyards, which burnt them up in such a manner, that the grapes turned in one night like dried raisins, so that they were forced afterwards to throw them away, because of their sourness; which was a great misfortune to most of the inhabitants, the greater part of whose revenue consists in wines.

It is remarkable that silver and copper were changed black by this thick smoke: and though some people, who were forced to pass through the smoke, in going to their houses, assured me, that it had no very ill smell with it; yet several of them were, that and the next day after, troubled with great pains in their head. At this time the White Island, which seemed to be above the lesser Kammeni, and could be seen from the first floor of the houses in the castle of Scaro, sunk down so low, that it could not be seen from the second.

Hitherto the sea had not been observed to boil up, nor any noise heard on the Black Island: but July 31, the sea was seen to emit smoke at two several places; one of which was about 30, and the other above 60 paces distant from the island. In these two places, both of which were perfectly round, the water of the sea looked like oil, and seemed to rise up and bubble: which it continued to do for more than a month; in which time there were a great many fishes found dead on the shore, occasioned by their happening to have been too near these two places.

The night following there was heard a dull hollow noise, much like that of several cannons shot off at a distance: and at the same time there was seen to rise out of the midst of the funnel flames of fire, which darted very high into the air, and disappeared immediately. Next day there was heard several returns of the same noise, which was followed by a smoke, not white, as usual, but blackish; and which, notwithstanding a fresh north wind, rose up in a moment to a prodigious height, in form of a column, and in the night time would, in all probability, have appeared, as if it were all on fire.

August the 7th, the noise altered; and from being dull, as before, it became very loud, and resembled the noise which is made when several great stones are thrown all together into a very deep well: and which probably was occasioned by several large pieces of rocks, which after having been raised up with the island by the violence of the fire, broke off by their weight, and fell back again into the subterranean caverns. What confirms me in this thought is, that I saw then the ends of this island in so great a motion, that after having appeared for some days, they then disappeared, and afterwards re-appeared again anew. This noise, after having continued so for near a month, was followed by another much louder and more extraordinary, it so nearly resembled thunder.

As the passage, which the fire had made itself by its violence through so many rocks, was not, in all probability, in a straight line, and was in some places narrower, and in others wider and more free; so it is probable, that the fire, or rather the sulphureous and burning exhalations, caused this great noise, by turning from side to side in these winding caverns, and endeavouring to get a passage out, which was difficult for them to find: which was the cause that the noise of this subterraneous thunder was sometimes not so loud, and a little while after became more violent, and sometimes was so stunning, that people talking together could scarcely hear one another speak; and that the Black Island, which was already very high, seemed to crack on every side; and in short, that the inclosed fire, after several windings and turnings, having collected a sufficient force, was able to break out with a noise equal to that of several cannons discharged at once.

August 21, the smoke and fire diminished considerably; there did not appear any in the night; but the next day both returned, with greater force than at any time before. The smoke was reddish, and very thick, and the fire so great, that the water of the sea smoked and bubbled up all round the Black Island. I had in the night the curiosity to view with a telescope the great fire that appeared on the mountain of this island, and I numbered 60 openings or funnels, which threw out very bright fires, and were divided from one another by rocks. In all probability there were others, and perhaps as many, on the other side of the mountain, which I could not see. Next morning I observed that the island had been very much raised in the night; that a range of rocks, about 50 feet long, was risen out of the water, which made the island broader than it was before; and that the sea was almost covered over with the reddish frothy matter above mentioned. This matter, or froth, appeared on the sea, every time that the island increased considerably; and occasioned a stench much like that of the sink of a ship: which we may imagine to arise from a slimy earth mixed with sulphur, which being raised up with the rocks, and coming to be washed off by the waves, was loosened and diluted by the water, and so sent up to the surface the salts with which it was loaded.

The fire had hitherto appeared only in one place, on the top of the Black Island; but on the 5th of September it made another passage, and appeared at the end of that island, on the side next Terasia, another island which some authors say was formerly joined to that of Santorini, and was separated from it by an earthquake. The fire continued at this end only a few days, during which it decreased at the place whence it used commonly to issue out. And here we were very agreeably surprized, in seeing the fire 3 several times dart out from this place without any noise, and rise up in the air like a large rocket. The following days there was much the same spectacle; for the subterraneous thunder, after having made a great noise, broke out from time to time with a clap as loud as that of a cannon, and was accompanied with a very beautiful and large fire, which shooting up in an instant to a great height, fell again on the island, and illuminated it almost all over. I cannot better represent the figure that the fire made in the air, than by comparing it to a certain artificial fire-work I have seen in France, and is there called the gerbe: but with this difference, that this fire rose much higher and was much larger, but not so distinct as that of the gerbe. The pleasure, however, which they had, in viewing these natural fires, so nearly resembling artificial ones, was not a little disturbed by a phenomenon, which the inhabitants believed to be an ill omen; for immediately after the fire was darted out in the manner of a rocket, there appeared in the air a blaze, in the form of a long fiery sword, which continued some time, without moving, over the castle of Scaro, and afterwards disappeared.

At that time also, the White and the Black Islands, having increased in length in proportion as they rose in height, united together, and the end of the Black Island, towards the south-east, ceased to increase any more, either in height or length, while the end toward the west increased very sensibly to the sight: whence we may imagine, that the mine of sulphur being at this place, and the fire not finding any passage out here, had force to raise up this part, and not the other; for in the middle of the island it always found openings to issue out at, together with the smoke. It had then 4 passages there, which were so near one another, that one could not well distinguish them asunder, but by the smoke: I do not mean that smoke which commonly issued out and was continual, but that which rose up at some certain times with a great force; for this smoke came out sometimes from one passage, and sometimes from another, and often out of all four together; sometimes with a great noise, and at other times without any noise at all, though then also it issued out with the same impetuosity. Out of these passages came also a whistling noise, like that of an organ pipe; which, by the variety of sound it made, pleased the inhabitants, whenever the subterraneous thunder ceased.

One would think, that the noise of this thunder should not then be so loud, by reason of the several passages; yet it was not at any time so great and so frequent as it was then, and as it was above six months after. It was then like the report of a cannon: and there did not pass a day or night, but we heard 5 or 6, sometimes 10 or 12 of them; and at the same time several large burning stones were thrown into the air; some of which falling one day on the great Kammeni, set fire to some thickets of bushes on that little island; and others being thrown a great way into the sea, had nearly destroyed a small vessel that passed by at above a mile distance. These claps were always attended with the smoke before-mentioned, which was very different from that which issued out continually from the gulph of fire nearly in the middle of the island;

fire, or rather the sulphureous and burning exhalations, caused this great noise, by turning from side to side in these winding caverns, and endeavouring to get a passage out, which was difficult for them to find: which was the cause that the noise of this subterraneous thunder was sometimes not so loud, and a little while after became more violent, and sometimes was so stunning, that people talking together could scarcely hear one another speak; and that the Black Island, which was already very high, seemed to crack on every side; and in short, that the inclosed fire, after several windings and turnings, having collected a sufficient force, was able to break out with a noise equal to that of several cannons discharged at once.

August 21, the smoke and fire diminished considerably; there did not appear any in the night; but the next day both returned, with greater force than at any time before. The smoke was reddish, and very thick, and the fire so great, that the water of the sea smoked and bubbled up all round the Black Island. I had in the night the curiosity to view with a telescope the great fire that appeared on the mountain of this island, and I numbered 60 openings or funnels, which threw out very bright fires, and were divided from one another by rocks. In all probability there were others, and perhaps as many, on the other side of the mountain, which I could not see. Next morning I observed that the island had been very much raised in the night; that a range of rocks, about 50 feet long, was risen out of the water, which made the island broader than it was before; and that the sea was almost covered over with the reddish frothy matter above mentioned. This matter, or froth, appeared on the sea, every time that the island increased considerably; and occasioned a stench much like that of the sink of a ship: which we may imagine to arise from a slimy earth mixed with sulphur, which being raised up with the rocks, and coming to be washed off by the waves, was loosened and diluted by the water, and so sent up to the surface the salts with which it was loaded.

The fire had hitherto appeared only in one place, on the top of the Black Island; but on the 5th of September it made another passage, and appeared at the end of that island, on the side next Terasia, another island which some authors say was formerly joined to that of Santorini, and was separated from it by an earthquake. The fire continued at this end only a few days, during which it decreased at the place whence it used commonly to issue out. And here we were very agreeably surprized, in seeing the fire 3 several times dart out from this place without any noise, and rise up in the air like a large rocket. The following days there was much the same spectacle; for the subterraneous thunder, after having made a great noise, broke out from time to time with a clap as loud as that of a cannon, and was accompanied with a very beautiful and large fire, which shooting up in an instant to a great height, fell again on the island, and illuminated it almost all over. I cannot better represent the figure that the fire made in the air, than by comparing it to a certain artificial fire-work I have seen in France, and is there called the gerbe: but with this difference, that this fire rose much higher and was much larger, but not so distinct as that of the gerbe. The pleasure, however, which they had, in viewing these natural fires, so nearly resembling artificial ones, was not a little disturbed by a phenomenon, which the inhabitants believed to be an ill omen; for immediately after the fire was darted out in the manner of a rocket, there appeared in the air a blaze, in the form of a long fiery sword, which continued some time, without moving, over the castle of Scaro, and afterwards disappeared.

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for this was much thicker and blacker, and rose in an instant much higher, and was not dispersed until some time after, and then it fell in ashes on the country, or into the sea; some of which the wind sometimes carried as far as Anacuphi, an island about 25 miles from the Bay of Santorini.

September the 18th, two hours after midnight, there was an earthquake at Santorini. It did no damage; and had no other effect, than to enlarge very much the island, and to remove for some days the fire and smoke into other places, through new passages which it made, and to increase them very considerably. By the violence of these claps, houses were shaken at above 3 miles distance; and out of the midst of a great smoke, which rose up and appeared like a mountain, one might see and hear large pieces of rocks fall down into the sea, and on the island, which were thrown out with the violence and noise as a ball out of the mouth of a cannon.

The Lesser Kammeni was several times quite covered over with large stones covered with burning sulphur; several of which, rolling afterwards from the top of this little island into the sea, made a very bright light, and pleasant appearance in the night. I thought at first, that the fire had passed under ground from the new island to this; because they are not very far asunder: but I soon found my mistake, and that this fire proceeded only from these stones sulphured over; for the sulphur, with which they were crusted over, being consumed, they were all soon extinguished, except some few, which remained alight above half an hour. By the violence of one of the loud claps, part of the top of the New Island was carried off into the sea, and several stones were thrown to above two miles distance: and, as if the mine had been exhausted by this great clap, three or four days passed without any noise, and almost without any fire or smoke. But the fire kindled again, and the island became more terrible than before. I was then at a village 6 miles distant; where we heard so distinctly, though it lay under a mountain, the blast of the mine, that the inhabitants were so frighted at such an extraordinary clap, that I was obliged to put them in heart, and they ran immediately to church to say their prayers, and recommend themselves to God. At my return to the Castle of Scaro, I found the people much more alarmed than they were in the village; and was informed immediately, that the Castle had suffered so violent a shake, that the doors of the houses, and the windows that were shut, were opened by the force and violence of it.

February the 10th, 1708, at half an hour after 8 o'clock in the morning, there was another earthquake at Santorini; and it seems that our volcano gets new force by other veins of sulphur, which take fire at a greater distance. We have instances of this in the fire and flames which rise so frequently into the air, and fall again over the whole island, making in the night-time as fine a spectacle as was ever seen in artificial fire-works. Besides this, several great rocks, joining to the island, which before were even with the water, have been raised much higher; and the noise, smoke, and boiling of the sea, increase so much, that though the inhabitants of Santorini have been so long accustomed to see all these things, yet they could not help being more afraid than before. And certainly not without reason; for the subterraneous noise was more violent than ever, and continued several days together without intermission; and in the space of a quarter of an hour the mine discharged itself 5 or 6 times; the noise of which, with the great quantity of stones it threw into the air, the shocks it gave the houses, and the fire which now appeared in open day, very much surpasses all that I have before said of it.

April the 15th was remarkable above all other days, for the great number and violence of the claps; by one of which nearly 100 large stones were mounted up all at once into the air, and fell again at above two miles distance in the sea. Though I was then about three miles off from the New Island, I observed one, of a vast size, which did not rise so high as the rest, but was driven farther, in a straight line, like a cannon ball.

From April 15 to May 23, which was a year from the birth of this New Island, what is above described, continued the same; and I did not observe any thing more in particular, unless it were that the island increased in height and not in length; and that one furious clap beat down at once all the top of it; which by means of the ashes and stones of all sizes that fell on it day and night, was not only repaired, but raised much higher than it was before. All circumstances began to abate afterwards: the smoke decreased; the subterraneous noise was not so violent; and the claps, though they were very frequent, were not however so loud, because the funnels, which gave a passage to the fire and smoke, were then much larger than before.

Hitherto I had only seen this island at a distance; for fear the same should happen to me, as to Pliny, when he went to see Mount Vesuvius; and least I should be suffocated, as he was, with the flames, or overwhelmed with the stones which this New Island threw out on every side. But seeing that there was then no danger, I went with others to visit, but could not approach close to it, for the heat and sulphureous smell; and so satisfied ourselves with viewing only the space that was between this New Island and the Lesser Kammeni; which I found to be broader than I imagined, and judged that a galley might pass through the narrowest place of it. After this, we went on shore on the Great Kammeni, that we might from thence view nearer, and without any danger, the whole burning island, and especially that side of it which we could scarcely see from the Castle of Scaro. From hence it was, that after having well examined all particulars, we judged this New Island to be about 200 feet in height, 5 miles in circumference, and a mile over at its broadest part. Its form is oblong, and resembles in some measure the figure of a dragon.

After remaining some time on the Great Kammeni, where we had the pleasure to see often, not far off, a great many stones thrown violently out of the gulph of fire, and fall down again with a terrible noise on the island, we took the resolution to coast it round, and to go to the other end of it, not doubting but that we might get on shore there, because that part of the island had not increased for several months, and no fire or smoke appeared there. We were within a hundred paces of it, when putting my hand into the water, I perceived that it was warm, and that the further we went the hotter it grew. At this instant the mine discharged itself; and the wind drove upon us the thick smoke, which broke out with such violence every time the mine took fire: and a shower of ashes and little stones, not larger than common hail-stones, falling upon us, we were obliged to quit our design of going on shore on the island at this place. This shower of stones and ashes being over, we retired a little from the great heat of the water; and letting down our plummet, we had at this place 95 fathom of water, without finding the bottom, our line not being long enough. Viewing afterwards the space that was at this end between the island and the Lesser Kammeni, we found in several places that it was narrower at this side than at the other; and that if the New Island should continue to increase, several rocks, which were already half above water, and rose up nearer to the Lesser Kammeni, would in time shut up the passage, and join both the islands together, so as to make a little port between them, which would be very commodious for the shipping of Santorini.

During all the rest of the time that I remained at Santorini, to the 15th of August 1708, when I came away, the smoke, fire, noise, &c. continued much the same as I have now described them; viz. always pretty moderate. From my leaving Santorini, to this present, the 6th of July 1711, I have received several letters, and have spoken with several persons who have come from thence within these few days, and they all assure me, that the New Island increases still in length on that side next Terasia: that it is now about 6 miles in compass: that the fire and noise under ground, are more moderate than before; which makes the inhabitants of the island hope, that they shall shortly see an end of it. As for my part, when I consider, that the passages which give vent to the fire, are very large, and almost in the middle of the island; and that there is no appearance that the fire will ever make a passage at the bottom of the sea, so as to let the water in to extinguish it; I am of opinion, that it may yet last longer than they imagine; and that this phenomenon will continue, till the mine of sulphur is entirely consumed.

An Account of many Rare Plants, lately observed in several curious Gardens about London, and particularly the Company of Apothecaries Physic Garden at Chelsea. By Mr. James Petiver, F.R.S. N° 332, p. 375.

An Experiment, showing the Direction of a Drop of Oil of Oranges, between two Glass Planes, towards any Side of them that is nearest pressed together. By Mr. Fr. Hauksbee, F.R.S. N° 332, p. 395.

The glass planes used were about 6 inches square; and being very clean, a drop or two of oil of oranges was let fall on the lower plane, suppose at B, fig. 3, pl. 17; then the upper plane was laid on it; so near as to touch the liquid, that it might become contiguous to both their surfaces. Thus the planes being made to touch one another at the side A, and opened at the side C, as in the figure, the lower plane lying parallel with the horizon, the drop of oil would immediately move towards the touching side of the planes; and when it was arrived there, it was but reversing the angle, and the drop would return from A to C; and after the same manner it might be directed to any side or part of the same. And, if the planes were elevated 8 or 10 degrees at A, yet would the drop ascend towards the side A, though not so swift as when the planes were in the forementioned position. It was further observed, that the nearer the drop approached the touching side, so would the velocity of its motion be increased: the reason of which seems very plain, allowing the ascent of water in small tubes, and between the surfaces of nearly contiguous planes, to be explained from the power of attraction, that one surface has to another at such a nearness. For the drop of oil moving on towards the contiguous surfaces, comes to enlarge its space, and touches the planes in more and more parts, as it approaches nearer and nearer the touching side. Thus in the whole progress of its motion, it is continually increasing in its surface, and consequently the power of attraction must increase in proportion to that surface; so that the celerity of its motion must necessarily be augmented. This experiment seems very powerfully to confirm the experiments made before on the same subject, from the gradual increase of the motion of the drop; thus representing the several appearances of the ascent of water in different sized tubes, or between planes whose surfaces are placed at different distances, the slower motion representing those experiments made in larger, and the swifter in smaller tubes; the same to be observed in different distanced planes. I have since repeated the same experiment in vacuo, where, in all respects, it answered as in the open air; which is a plain indication, that the presence of the air has nothing at all to do in producing this phenomenon.

Additional Observations on the Production of Mites, &c. By Mr. Anthony Van Leuwenhoeck, F.R.S. N° 333, p. 498.

I formerly acquainted you how mites couple and lay their eggs, and how young mites are produced from those eggs; and that I observed the hair on their bodies. Though I then imagined that I had observed every thing about the mite that was to be seen, yet I have thought fit to view one of these animalcula again with a microscope; and the rather, because in observing those small animalcula that feed upon the nutmeg, I found that those particles on their bodies, which one would take to be hairs, are not really so; for I cannot allow those particles to be hair, or wool, unless they are smooth and even, setting aside the bark, or skin of the hair, which may be the cause of its being a little rough and uneven.

Those 6 or 8 long particles, on the body of the mite, and which one would call hairs, are longest on the upper part of the body, being twice as long as those on the side: and when one carefully, and through a microscope that magnifies very much, observes those long particles, one would judge that such a long particle consists of 50 parts, which seem to be little joints; and that from each of those divisions small hairy particles again proceed. I could discover also, that the mites had power to move those long particles, which I formerly took to be hairs, in such a manner, that when they were obliged to creep through narrow passages, they could lay these little hairy particles down close to their bodies; and that these particles had each of them but one moving joint, which was next to that part which was fastened in the skin.

Thus we see the wonderful formation of that small animal the mite: but what shall we say to the unspeakable number of many kinds and particular forms of other animalcula, some of which are so small, that their whole body is not only not so thick as the diameter of one long particle, which is on the body of the mite, but even not so large as one of the slender particles, on those jointed parts of the long hairy particles; and which animalcula are not to be seen, but through some of the most magnifying microscopes: and if one could see the smallest animalcula so large and so clear, as we see a mite, we should be more surprised I believe at their figures, than we are at that of a mite: in short, the smallness of the parts, of which all bodies are composed and set together, is so very minute, that it is not to be conceived by man.

Afterwards I took some mites out of some wheat flour, which had been about 14 days in my house, and viewed them through a microscope; but I could not perceive, though I viewed them very nicely, those jointed parts which are as it were covered with hairs, and are on the body and feet of the mites, and which are so small, that they seem themselves to be nothing but hairs. From which observation I considered, that as the flies, which we judge to be of one sort, are yet of very different kinds; for some of them lodge their eggs in flesh, others in cheese, others again in dung; and accordingly the worms hatched from these eggs, receive their food and growth from the several substances in which they lie: so likewise among the mites, some live upon flesh, and others again on meal or bread.

For further satisfaction, I went to a grocer's shop, and out of a little barrel took some figs that were of the growth of the year 1707, thinking to find a great many mites among them; but I met with only 3 or 4 mites that were living; and those had longer hairy parts on the hinder part of their body, than I had seen on any before; and those hairy parts were also covered with as many small hairs, as I have said before.

In my search after mites, I discovered a kind of animalcula smaller than the abovementioned, and of a quite different figure from the other. The hairs on the body and feet were very short; the body, and particularly the head, were of quite another make; for these had on the side of the head two short instruments, with which they made a very quick motion, and with which probably they convey their food to their mouth, because their head was shorter than that of a mite.

Fig. 4, pl. 17, represents such a hair of a mite; with the hairy particles branching out of the sides, as viewed through the microscope; which hairy particles have joints along their length also. Fig. 5, represents the half of one of those hairy particles, found in great numbers on the bodies of bees, seen through the microscope.

I observed that one of these mites, after she was stuck upon the point of a pin, laid two eggs; one of which appeared to the eye like a large grey pea, and the other I judged to be like a sparrow's egg. Another mite had laid 4 eggs; and another, which I had newly placed before a microscope, laid two. The mite that had laid 4 eggs, was only fastened by the two hinder legs of the left side of her body; so that she could move the fore-part, and even displace it: and I observed, that after the said mite had stuck upon the point of a sharp pin for the space of 10 days, she had eaten two of her eggs.

I have opened the bodies of several of these mites, and took two eggs out of one of them; and once I took three eggs out of the body of another, which were come to their full growth; but in the most of them I could discover no eggs: some of them I imagined to be males. And though the liquid matter of the eggs, which I took out of their bodies, was exhaled in a very short time; yet those eggs, which the mites had laid, preserved their perfect size and roundness. The reason of which was, as I imagined, that the shells of those eggs which I had taken out of their body, had not yet acquired their full hardness, and consequently the liquor was more easily exhaled from them.

The mite that had laid 2 eggs, I put into a box, with a microscope before which it was placed; and on the 17th of October 1708, I put it into my pocket, to see how long time was required for the hatching the young mites from the eggs. I viewed these eggs more than once every day; and after 3 or 4 days the mite was dead. And after I had carried them 9 days in my pocket, I observed that the eggs, which were at first transparent, became dark at one end, and that that darkness increased from time to time; so that on the 30th of October, I could discern but a very small part of the egg to be clear. And on the 1st of November, I observed but one egg; and that which remained of it was so little, and it lay so confused, that I could discover nothing for certain. In the second egg I not only observed the same appearances as in the other, but I saw also a small animalculum lying in the egg, of which I could perceive the head and some of the hairy parts of the body. And as the animalculum, now complete, did not entirely fill the shell of the egg, some part of the shell was now transparent; by which means I could more plainly discover the animalculum and its hairs in the egg. About an hour after my first discovery, I observed the animalculum had forced out about one-third part of its body, and was making a great motion with four of its feet. But forasmuch as the animalculum could not fasten its legs, which it had put out of the shell, on any place, it made a great stirring with them the whole day, without being able to bring its body farther out of the shell; and the next day it was dead.

I observed that a mite, which had been stuck through its back with a point of a pin, had on the smallest part of one of its legs an egg adhering to it; and though he moved his foot very much, yet the egg stuck fast to it all that day that I observed it. From whence I concluded, that the eggs of mites are of the same nature with those of caterpillars, and other creeping animals, whose eggs, wherever they fall, stick fast by reason of a slimy or glutinous matter with which they are surrounded. But the next day I observed that the mite had worked the egg off its leg, and by the help of its claws held it in one foot: and though it made a great stir with its legs, and particularly with that which held the egg, yet it did not let it go, but took it from one foot into the other; which was a very pleasant sight; but the next day I could not find the egg.

I observed also two other mites, which I judged to be females, before two microscopes: one of these mites had laid two eggs, and the other three, and as far as I could judge they were about three days old. I carried those microscopes, together with the mites, in the day-time about me, and after ten days, I observed that two of the eggs, which lay close by each other, had been hatched, and that the young mites were come out of them, and the shells lay open; but I could not find the mites. As for the other three eggs, there came a young mite out of one of them, on the same day; and on the 13th day came out another mite, which could not thrust out its body above half way, and did not live above a day. As for the third egg, it lay lengthwise to the sight, and I expected before three days, that the young one should have come out, computing that the animalculum was arrived to the full growth in the egg, and that it had filled it in length and breadth, and there was also a transparency in the egg; at last there came a little mite out of the third egg, but dead.

On the 10th of November, 1708, by means of glutinous matter, I stuck two mites, which I judged to be females, on the point of a small pin; and on the 12th of the same month, I found they had laid three eggs between them; the next day there were four, and soon after six. I did not keep these eggs warm, but let them stand upon my desk, to see how long they would be in hatching, in that cold weather. December 8, I could perceive that one of these mites stirred three of her legs, but after that time I could perceive no more life in her.

I viewed the said six eggs several times, and observed, that at the thickest end of them (for they seemed to be exactly shaped like hen's eggs) they seemed darker and darker; and on the 22d of December I could perceive on the thickest end of one of the eggs, which stuck fast, a little mite, that had worked itself so far out of the shell, as to make a continual stirring with four of its legs.

The 25th of the same month, I further observed another mite got out of its shell. From whence we may conclude, that as of hens and other creatures, their chickens are not increased nor nourished in the eggs, unless the mother sits on them; and that all their eggs are hatched about the same time; so here on the contrary, the eggs of mites not being sat upon, but from time to time, and the eggs laid on several days, the young ones must consequently be hatched at different times.

From these experiments we may observe what an influence warmth has on the eggs of mites; for from those that I carried in my pocket, young ones were produced on the eleventh or twelfth day, but those which I kept in a box on my desk were not hatched in less than seven weeks.

Thus we see how regular the production of a mite is, which was formerly thought the smallest of animals, and how admirable the structure of such a wonderful animalculum is.

After some very cold and frosty weather, I got some mites that were taken from a bladder broken in pieces, and viewed them with a glass; and saw that some of them were dead, and others still living, but very slow in moving. I put a little piece of dry bladder in a clean glass tube, with four mites; and viewing them in the great frost, I observed that one of the mites had composed all its legs so exactly under its body, that as one viewed it on the upper part of the body, none of them could be seen; from whence I concluded that that mite was dead. Having again viewed the same mite a day or two after the second hard frost, I observed, that it lay in the same place, and in the same manner as it did two months before. The other mites were also very much dried and shrunk up, and their bodies bent double, and were dead. However, fancying that the aforesaid mite was still living, I viewed her again, and saw her begin to move one of her legs; on which I warmed the glass tube a little, and then saw the little animal not only stretch out all her legs, but begin to creep very gently along. It seemed to me very wonderful, that so small an animalculum should live above two months sticking on the sides of the glass in so sharp a frost; nor was its moisture evaporated out of its body; but the dead mites were so shrunk up, that they were not half so large as when they were alive.

In the month of August I stood by a fishmonger's shop, while they were laying dry ling in the water, to soften it; and I observed several mites running about on the fish. I took five of these and put them into a glass tube, with a little bit of the dried ling, to observe what sort of creatures would be produced from them; and after having shut them up for some days, I discovered that they had changed their skins, and that they had made a great many holes in the cork which stopped one end of the tube, and had insinuated themselves so far into it, that one could see none of their bodies. These animalcula are well known to the furriers, being very destructive to their furs. The cast skin of one of these animalcula as above-mentioned, is represented by fig. 6.

It would seem it was not without an important design that these animalcula, having changed their skins, had dug so deep into the cork that I could not see them; for on the 8th of September I discovered two winged insects like little beetles, having the fore and hinder part of their bodies black, and the middle brown, with some speckles; also having two shields upon their bodies, which covered their wings; and the skins, which they had shed, after they came out of the cork, lay about the glass. To satisfy myself that the said animalcula were changed from worms into flying insects, I opened two of the holes they had made in the cork, and observed in one of them an animalculum lying, which indeed had changed its skin, yet was not become black, but was still white, and was about the size of our common small flies. September 11, two more of the animalcula came out of the cork, and the female, which at first was all white, and was still like an aurelia, and stirred very much, became red, and then turned to a darker red. September 17, the aurelia had shed its skin, which was exceedingly thin, and then it ran about the glass; but it was not arrived to that blackness which others had, which had run longer about the glass. Fig. 7 represents one of those animalcula with its two little shields that cover the wings when it does not fly, and then they are folded up within the shells.

Now since we see that the worms, whose skin is represented at fig. 6, are turned into flying insects, which lay their young or eggs in all parts of a house; and since it is natural for all creatures to bestow their eggs where they can be best preserved and hatched, it will not appear strange that these creatures are mostly found on furs that are fat.

About the end of May, I sought for some mites* upon a piece of some dried flesh of a whale, which hung upon a little string fastened to a nail in my closet, and I perceived that several of the said blackish animalcula fell off from the piece of whale's flesh, and this happened four several days. I then put most of the flying creatures into a glass tube, which I stopped in such a manner as not to exclude the air: and put into the said tube a little bit of whale's flesh, on which I observed that they fed greedily, and that a great deal of their excrements lay upon the glass. May 31, I saw three white eggs laying on the glass, but having none of that viscous matter about them, with which the eggs of most of the animalcula are endowed, these eggs did not stick to the glass, but fell among the excrements; so that I could not distinguish them.

In the first glass tube, in which were those animalcula which I had shut up the whole winter, and were turned into flying creatures, I observed two young animalcula running about, whose skins are represented by fig. 6; but how many of them there were I could not discover, because about a month before I had put into the tube the tail of a pole cat, that they might subsist on the fur, which I was told they used to feed upon; and afterwards I put in a little bit of the flesh of a whale into the same tube. These young animalcula were but little larger than the eggs before-mentioned. Since that time I saw the said animalcula but once; and the eggs which I saw, in a day or two after, I could discover no more, nor any of the small animalcula, from whence I concluded, that the flying creatures had eaten up their eggs and young ones.

* Under the word mites M. L. erroneously comprehends the larvæ of certain winged insects; whereas mites (or those insects which belong to the Linnæan genus acarus) are apterous, in both sexes.

I took the tail of the pole-cat out of the glass, and examining it, I found a living animalculum in it as small as an egg, which was very white to the naked eye. I shut it up in a little glass, and put a small bit of the flesh of a whale into it; and I perceived that, after a few hours, it had assumed a rosy colour. I also found three eggs, in one of which the animalculum was so large, that by the help of a microscope, I could discover part of the body through the shell of the egg: and two days after, being the 8th of June, I could perceive through a microscope, on the wreathed or notched parts, red streaks, on which were long hairs; and in the evening the animalculum was got out of the egg-shell, and the next morning it had assumed a rosy colour.

June 13, I saw some few eggs on the glass, on which I took two out, and put them into a glass tube of the length and breadth of a finger, leaving six or eight others in the aforesaid glass; and I took the said blackish animalcula out of the glass, and carried the glass tube, in which the two eggs were, about with me in my pocket, that I might see how much sooner these would be hatched than those which I left on my desk in a large glass tube; and I discovered, June 15, young ones hatched from the eggs which I carried about me, as on the 20th, were those in the tube that lay on my desk.

July 5, I observed that in the two several glasses that contained the animalcula, that some of them had left the bit of whale's flesh, and kept themselves very still on the cork; so that I fancied they were going to be changed into flying insects; but I was mistaken; for they only lay on the skins they had shed, and then the flying insects daily laid their eggs, out of which also came young ones; but most of the eggs that had been laid were eaten up; to prevent which, some of those flying creatures were so cunning, as to thrust their eggs between the cork and the glass, by which some of the eggs became flattish, and yet young ones came out of them.

Soon after July 5, I perceived that some of the running or creeping animalcula had gnawed holes in the cork, into which they had dug so deep, that one could not see their bodies. The beginning of August, I viewed them again, but could not perceive any flying creatures, as I expected, to be changed from the creeping ones; wherefore I cut off a little of the cork in two several places where those creeping creatures had been digging, and then I discovered one animalculum, which had changed its skin for the last time, and lay still on the hinder part of its body, and was lying in the aurelia, which was white. On the notches of its body there were red streaks: and when I touched it with a needle, it stirred its body very much. August 10, I observed that one of the aurelias was stripped of its little skin, which was exceedingly thin, and it was an agreeable sight as viewed through the microscope. In all my observations I never could perceive any copulation among those animalcula when they were changed into flying creatures; from whence I concluded that they were all females, as many other flies are.

Some further Account of divers Rare Plants, lately observed in several Curious Gardens about London, and particularly in the Company of Apothecaries Physic Garden at Chelsea. By Mr. James Petiver, F. R. S. N° 333, p. 416.

These are plants from the East and West Indies; also from America and the Cape of Good Hope.

Of an Hydropical Case, in which the Gall Bladder was distended to an extraordinary Size. By Mr. Ja. Yonge, F.R.S. Surgeon at Plymouth. N° 333, p. 426.

Mrs. Dyer was about 30 years of age, the mother of several children, and very healthful till last January 1711, when, after frequent watchings, on an extraordinary occasion, she was troubled with a pain in her belly, like the colic, but which proved the dropsy ascites, and it increased so fast, in spite of all my endeavours, that by March the 9th, being almost suffocated, I was obliged to tap her with a hollow needle in the usual place, and to repeat the operation as often as she filled, and by that means discharged the several quantities of water, at the times here under mentioned: viz.

		Pints.			Pints.			Pints.
March	9	9	June	14	14	August	17	14
	14	8		24	14		26	13
April	2	12	July	7	17	Sept.	1, 6, & 22	11½
	16	10		21	16	Oct.	1	3
May	17	14		30	16		30	15
	31	14	August	6	14			214½

So that, in the space of 8 months I drew 214½ pints of water. She died November 4, 1711; and opening her belly, we found the following remarkable things, viz. From the belly issued 14 pints of a greenish serum, mixed with a very purulent matter, not a little fetid; the intestines, especially the colon, almost every where livid, and adhered in many places to the peritonæum, though they had been so long immersed in water; the omentum was also black, and almost consumed; the liver, which I expected to be indurated, had no blemish, only two superficial ulcers on the left lobe; both that and the peritonæum, which are usually full of hydatides in dropsical persons, were quite free of them, but there were many on the stomach and guts.

But we were much surprized, to find a large bladder, distended like that of an ox, filling up almost the whole region of the liver and ventricle, and adhering to the adjacent parts so firmly, that we could not separate them without difficulty, and get it out whole. Our surprise at such a prodigious appearance turned into astonishment, when we found it the gall bladder, and that by its distension it had torn the liver asunder, one part of which adhered to the left side of this monstrous cystis, and another part behind it, towards the back; and both expanded with it and fastened to it, like the temporal muscle to the skull. The whole weighed 10 lb. 12 oz. It had no passage to let out the matter it contained, though we squeezed it hard to try, nor could we find any by probes; so that we were obliged to make way by a knife, and so let out of it 7 pints of a black liquor, like coffee; which having stood one night in a basin, near a quart of thick yellow fæces subsided. The liquor in this bladder, and what we found in the patient's belly after her death, added to what was evacuated before by paracentesis, amounts to 235 pints.

Besides the prodigious quantity of matter which filled this great bag, we found several pieces of membranes like gut, or bladder, cut into pieces; what it was, or how it came there, I cannot conjecture. It was very wonderful, that during the whole time of her sickness, she discharged by urine near as much as she drank; and yet by computation, she leaked into the abdomen near a pint every 24 hours, from March to November. When her belly was nearly full, her thighs and legs used to swell, and grew discoloured, like an approaching gangrene, but both went off after tapping, by the help of friction and a warm lotion.

Description of the Head of a monstrous Calf. By the Rev. John Craig, Vicar of Gillingham in Dorsetshire, F. R. S. N° 333, p. 429.

A butcher brought me the head of a calf, which he had taken out of a cow's belly. The upper jaw was divided into two halves, as far as to the dura mater; each half had a distinct eye and nostril; and the under jaw was bent round so entirely, that it lay exactly between the two halves of the upper jaw, making the tongue lie upon the forehead, about 2 inches above the teeth of the under jaw, and in the fissure of the upper jaw. This preternatural division of the upper jaw was not covered with hair, but with a cutis of a florid colour. The calf was come to its full time, and made great strugglings when the butcher knocked the cow on the head, which by some symptoms they judged would have died in the calving. It was so large a calf, that an old experienced butcher says, that he never saw more than one so large at calving. The legs and feet were as large as an ordinary calf of 6 weeks old.

On cutting open the skin of the head, for there was no cranium, I was surprised to find, that there was very little brains in it, certainly not so much as in a rabbit, the whole cavity not large enough to hold an ordinary walnut.

Experiments on Fishes kept in Water, under different Circumstances. By Mr. Fr. Hauksbee, F.R.S. N° 333, p. 431.

The fishes used in the following experiments were gudgeons: which are naturally very brisk and lively in the water, and can live a considerable time out of it. I put three into a glass vessel with about 3 pints of common water; these fishes were to be a standard to compare the others by. Into another glass I put three more to a like quantity of water, which just filled it; I then screwed down a brass plate with a leather between, to prevent a communication with the water in the glass and the external air, and that it might the better resemble a pond of water frozen over, on which account this experiment was made. I suffered as little air as possible to remain on the surface of the included water. The third glass had a like quantity of water, which first by boiling, then by continuing it a whole night in vacuo on the air pump, was completely purged of its air. Into this water also I put the same number of gudgeons as into the former; and then waited the event. It was about half past 10 o'clock in the morning when I began the experiments, and in about half an hour from that time, the fishes in the exhausted water, or water purged of its air, began to discover some uneasiness by a more than ordinary motion in their mouths and gills, or respiration, if I may call it so, differing from the fishes in the other glasses, which at the same time showed no alteration: only I observed that they would now and then ascend to the top of the water, but suddenly swim down again: and in this state they continued for some considerable time, without any sensible alteration. About 5 hours after the last observation, the fishes in the exhausted water became not so active, on a motion given to the glass that contained them, as before: and the gudgeons included without any communication with the outward air, now began considerably to abate of their vivacity. At 7 in the evening, the included fishes lay all at the bottom of the glass, with their bellies upwards: nor on shaking the glass could I put them in motion, or cause them to stir their fins or tail, only I could observe a motion in their mouths, which showed they were not quite dead. In this state they lay for some time; but considering the experiment would not be complete, if I did not attempt their recovery by taking off the brass cover, being very certain they must have died in some small time under the circumstances they were in, I took off the cover, and gave the surface of the water a free and open communication with the external air. At about 10 at night I observed them again; when their recovery was so evident, that on a little disturbing the glass that contained them, they were actually in motion again: and at this time also, the fishes in the water purged of air, began to appear more brisk and lively than at the last observation. Here I cannot but take notice, that though the water was purged of its air to a very great degree, yet the fishes put into it did not so much as once ascend in it; but continued always at the bottom, as those did in the common water. At this time I left them till the next morning; when, about 8 o'clock, I found them as lively in all the glasses, as when first put in. Those in the common water exposed to the open air suffered no change during the whole time. After this, wanting to try whether the air had again insinuated itself into the exhausted water, and whether that might not be the cause of the fishes recovery; I put it on the plate of my pump, in the same glass with the fishes in it; and being covered with another receiver, the air was taken from it; yet I could perceive very little air ascend in it, and to me it seemed to be much in the same state as when the fishes were first put in. I continued it in vacuo about an hour and half; the fishes almost all the time continued at the top of the water, and at that time appeared as dead; for on admitting the air, they sunk hastily to the bottom, without any motion of their fins or tails.

From the whole account I observe; 1st. That water purged of air, so far as the method here made use of, is capable to do it, renders it not altogether unfit to support the lives of water animals. For though when the fish were first put in, and for some hours after, they seemed to suffer some uneasiness; yet at length the water became more familiar to them, or their constitutions in some measure did so conform, as to render the water to them, and them to the water, more agreeable: otherwise I do not see how their recovery should follow, since on examination, little or no alteration could be found in the circumstances of the water, from the time the fish were first put in.

2dly. The fish included with their water from any communication with the external air, plainly demonstrate, that common water in its natural state is not alone sufficient to preserve the lives of its natural animals. Hence it follows, that in ponds, when the water comes to be frozen over with a pretty thick ice, the fish in such ponds are very likely, if not certain to perish, on the continuance of such a congelation for some time on their surfaces; unless (as in the latter part of the experiments) the impediment, which hindered the immediate contact of the air with the surface of the water, be removed; that is, by breaking holes in the ice, by which it is restored, and undoubtedly will perform the same thing as my removal of the brass plate. This is to be understood only in ponds, where the water is stagnant; for where there are springs, or a current of water constantly succeeding under the ice, the effect most likely will not be the same.

Of the Asbestos, or Lapis Amiantus, found in the Highlands of Scotland. By Mr. Patrick Blair. N° 333, p. 434.

The following is the relation of a gentleman in the Highlands, not many miles from this place, (Coupar of Angus) who has lately built a house of a singular kind of stone, dug out of a quarry not far from him. This stone, after the rubbish, which is not very deep, is cleared away, lies horizontally in a bed having parallel fibres, with few interstices. It is soft at first, and easy to be smoothed and polished, without any tool, but rather with sand, or another hard stone, of a bluish colour, which afterwards hardens so, that it resists the injuries of air and fire. When first the quarrier began to dig it, endeavouring to cut and raise it after the ordinary way with wedges, and the other usual tools, it broke and crumbled all to pieces: but afterwards, observing more narrowly the direction of its fibres, he endeavoured to cut it lengthwise with spades; and by this means he procured stones as large as he pleased, which smoothed very easily along the direction of the fibres; but when cut transversely, no means could render them smooth, but their surface remained as unequal as the extremities of a piece of wood. In the said interstices lies the true asbestos, which is of a whitish silver surface, consisting of several fasciculi with parallel fibres, like those of the muscular fibres of salted beef, easily separable from each other, till it becomes so small as the finest flax, and so ductile, that it may be spun into the finest thread, of which it were easy to make the incombustible cloth, so famous for shrines among the ancients. In other places of those interstices, was likewise to be observed a reddish substance, near the colour of dragon's blood, which the gentleman believed might be good for dying. I am apt to think the second kind was fibrous too, which might make a beautiful cloth, being striped with the other. This whole quarry may be said to be asbestos of different colours, the bluish being of a much coarser, and the white and red of a finer grain.

The Dimensions of some Human Bones, of an extraordinary Size, dug up near St. Albans in Hertfordshire. By W. Cheselden, Surgeon, F. R. S.* N° 333, p. 436.

The circumference of the skull lengthwise, is 26 inches; and according to

* This celebrated anatomist and surgeon was educated under Mr. Cowper. He was a native of

its breadth, 23 inches. The greatest diameter of each os innominatum, is 12 inches. The left os femoris is 24 inches long, having only one (and that the great) trochanter. The right os femoris is 23 inches long, having 3 trochanter processes. Each tibia is 21 inches long. If all the parts bore a due proportion, this man must have been 8 feet high.

These bones were found near an urn, inscribed Marcus Antoninus, in the place of the Roman camp near St. Albans.

Microscopical Observations on the Seminal Vessels, Muscular Fibres, and Blood of Whales. By M. Leuwenhoeck, F. R. S. N° 334, p. 438.

I have formerly said, that the animalcula in semine masculino of a horse, and those of a dog, are of the same size; at least they appeared so through the microscope. Since that, I have been very desirous to observe the semen masculinum of a whale, in order to discover, if it were possible, whether the animalcula in those large creatures did not proportionably exceed such as I have discovered in smaller ones. For this purpose I procured from the captain of a Greenland ship a piece of the penis of a whale, viz. of the thickest part of it, where it was joined to the body, in hopes that I might still discover some of the animalcula in the vasa deferentia. Having opened the vas seminale as well as I could, for it was very much dried up, and scraped off a little of the matter that was in it with a small knife, I mixed it with a little rain water, to separate the parts from one another: on which I observed in it a great number of long four-sided particles, most of them having 4 right angles, but of different sizes, and many of them were three times as long as they were broad; but I saw none of them so broad as the diameter of a hair, and the smallest was 100 times less, in some few of which I could perceive no thickness.

All the said particles, or little figures, were as clear as crystal, so that I concluded they were fixed salts; and the rather, because they were so hard,

Leicestershire, and was born in 1688. He was surgeon to St. Thomas's hospital, to Chelsea hospital, and to Q. Caroline. He was a corresponding member of the French Academy of Sciences, a fellow of the R. S. &c. He died of apoplexy in 1752. His principal works are, 1. Anatomy of the Human Body, an elegant compendium, illustrated by engravings; 2. Osteography, or the Anatomy of the Bones, with large folio plates, exhibiting not only the human bones, both in the sound and diseased state, but also skeletons of various animals. In these plates, however, the appearances of many of the smaller bones are rendered indistinct, in consequence of the attention bestowed upon shading and softening; a practice which should always be avoided in scientific engravings, where shape, proportion and exactness of outlines are chiefly desirable. 3. A Treatise on the High Operation for the Stone; a work which procured him a well-merited reputation as a practical surgeon.

that they were not dissolved in the water. And when this matter, which I had scraped out of the vessels of the whale's penis, had lain a little longer in the water, I discovered salt particles; the two longest sides of which run parallel to each other, but the two shortest sides oblique; so that one end of them had a sharp, and the other an obtuse angle: and though the salt particles had lain at least a week in water that was frozen, and the same water afterwards dissolved, yet I saw the salts floating in it. As many observations as I made, I could not discover the least particle that bore any resemblance to the animalcula in semine masculino.

The said captain having informed me, that the testicles of a whale are as large as a firkin of butter that weighs about 100 weight, I intreated him the next whale he took, to cut off the vasa semen deferentia as near the testicles as he could, and to lay them to dry on a plank, or to let the surgeon do it, whom they have always on board, and who is fittest for such a work; for by that means I hoped to discover the animalcula therein: but as yet my request has not been complied with.

The said captain presented me with some slices of a whale's penis; adding, that a little of it grated, or cut into small pieces, and boiled in milk or beer, was very good against the bloody flux; and that a neighbour of his, who had been ill of that distemper a whole year, was cured by that medicine. He told me also, that the female of a whale (on the side of the uterus, but a little nearer its tail) has 2 nipples, or teats, which yield milk, and that he had drank of it; and he showed me the teats, the diameter of one of which was no more than an inch and a half, and it was 2 inches long, but it was dried hard.

Sometime after, he brought me two pieces of whale's flesh, hard dried, in a span length, and as thick as one's largest finger. They were as black as pitch; but, as he says, when the flesh was cut off, it was red. From the blackness of it I concluded, that the blood was of a deep or dark red; which the captain also confirmed; adding, that where the blood fell, it left a stain.

I cut through the flesh both cross and lengthwise, to discover the thickness of its particles; and after I had cut thin scales across, they appeared of a bright red colour; but when I cut them thicker, they were of a dark red; and when very thick, they were blackish. In this operation I observed, that the small fleshy muscles were surrounded with very thin fibrous particles, that looked like little membranes torn to pieces: and on several parts of these little membranes, there lay fat, which, when there is a quantity of it together, they call the trane; and these trane particles lay pressed together in the same manner as I have formerly described those of an ox; nor were they larger than the particles of the fat of an ox; and many of these particles were like melted fat; insomuch, that when I squeezed the flesh, the fat came ont at the end of it. And the particles of the flesh seemed finer than those of an ox.

The captain also acquainted me, that the whale has three distinct skins; the uppermost of which is very thin, and the next to it is as soft as velvet; but the third and undermost is a thick skin, which we call the sward, and is like the skin of a hog. Soon after he gave me a little piece of the first skin, which he said was easily separated from the second; it was no thicker than a leaf of writing paper. Having viewed it through a microscope, I judged it to be composed of such flattish particles as I can call by no other name than little scales, and which were no larger than the little scales of which our own outer skin is composed: but whereas the scales of our skin are very easily separated, and especially the uppermost scales, which are shed or cast often, the little scales of the skin of the whale are firmly united together.

When I came to view the aforesaid skin, with regard to its thickness, I discovered, that there were at least 20 skins upon one another; all which were composed of little scales, and of exceedingly small particles that lay scattered under those scales; but as carefully as I examined the said skin, I could not discover in it the least fibres or vessels.

I got also a piece of the second skin of a whale, about as broad as my hand, which was as black as pitch. This skin was dried hard, and was almost half an inch thick; but it was not strong, because there were no vessels or sinewy fibres running through each other, as in the skins of beasts and our common fish; only there were some small fibres that joined it a little to the skin that lay under, and which passed, as it were, in a right line to the uppermost superficies of the skin.

The undermost, or third skin, was whitish, and so strong, that I concluded, in case the harping iron was struck so deep into the whale, that its barbs penetrated into the aforesaid white or third skin, it would keep its hold; but if it went no further than into the black skin, it would easily be torn out.

I observed, that the black skin was clammy, or glutinous, when it was wet; from whence I concluded, that there might be drawn a very good glue from it.

From the black skin and black blood of a whale, I turned my thoughts to the black blood of a salmon; and supposed, that the redness of the flesh of that creature, was alone occasioned by the blackness of its blood; and that in like manner the blackness of the flesh of whales proceeded from the same cause. From hence I considered, whether the blackness of the men on the coast of Guinea, might not also proceed from their black blood; and whether also their flesh might not be blacker than that of white men, and that the blackness of their skin alone depended on their black blood; which deserves to be considered.*

Now since we see that the particles of flesh, fat, and small membranes of the whale, are no larger than those of an ox; and that the size of creatures depends only on the number or quantity of their parts, we may easily conceive why the animalcula in semine masculino of a whale, are not larger than those in our common quadrupeds.

Epistola D. J. Phil. Breynij, M. D. Gedanensis, et Reg. Societ. Lond. Sodal. ad D. Hans Sloane, M. D. varias Observationes continens, in Itinere per Italiam suscepto, Anno 1703. N° 334. p. 447.

The geographical and other observations, contained in this itinerary, are not now sufficiently interesting for reprinting.

Some Remarks on a Parchment MS. of Greek Surgical Writers, in the Laurentian Library at Florence; taken from Schelhammer's Additions to Conringius's Introd. in Artem Medicam. Cap. xii. p. 401.—N° 334, p. 459. *An Abstract from the Latin.*

We have no treatises (says Conringius) on surgery by the ancients, except those of Hippocrates, Galen and Celsus.

On this it is remarked by Schelhammer, that Spon in his travels into Italy, Dalmatia, &c. makes mention of a MS. in the Laurentian Library at Florence, the only one as he (Spon) imagines of the kind in Europe. It is a large vol. containing the surgery of the ancients; viz. of Hippocrates, Galen, Asclepiades, Bithynus, Apollonius, Archigenes, Nymphodorus, Diocles, Rufus Ephesius, and Apollodorus Cytiensis. In this work there are figures drawn upon parchment, illustrating the method of reducing luxations. The last mentioned writer, Apollodorus of Cytium, as well as Asclepiades, Apollonius and Diocles, are occasionally quoted by Pliny. Archigenes is also frequently mentioned by Galen; but the entire works of these writers are no where else to be found. And, with regard to Bithynus, Nymphodorus and Heliodorus, they are scarcely known to us even by name.

If this account (observes Schelhammer) were correct, there would be much reason to regret that the abovementioned MS. collection of Greek writers on Surgery, should remain unpublished; but Meibomius (who, during his stay at Florence, carefully examined this MS. and was allowed to make extracts from it) gives a very different account of it. This collection (he says) of ancient

* These are very erroneous conjectures. The black complexion of the natives of Guinea and other parts of Africa, is not owing to any specific difference in the quality of their blood, but to the colour of the rete mucosum, which lies immediately under the cuticle.

writers on Surgery, is by Nicetas, a physician, of whom it is not known when he lived. It is deposited in N° lxxiv. of the Laurentian library; but Spon is not accurate in stating, that the Florentine copy is the only one extant; for there is another in the library of the King of France. Neither are all the treatises in this collection unpublished tracts, nor are the authors of them unknown. Some of the treatises are entire: of others there are only fragments. It is not necessary (continues Meibomius) to enter into particulars concerning the well-known treatises of Hippocrates, Galen, Oribasius, Rufus the Ephesian, and Palladius; but of Bithynus, Nymphodorus and Heliodorus, it will be proper to say something. The first of these (Bithynus) was not before known (as Spon observes) even by name; nor was it possible he should, since he never had any existence but in that author's brain. The fragments in the aforesaid collection are from the writings of Asclepiades of Bithynia [Asclepiades Bithynus] of whom Pliny and Galen make frequent mention, and of whom Spon makes two distinct persons, calling the one Asclepiades and the other Bithynus. He might with equal reason have created two persons out of Rufus Ephesius. With regard to Heliodorus, his writings are quoted by Ægineta and Oribasius; and Nymphodorus is noticed by Galen, Celsus, and the aforesaid Oribasius, who make mention of his glossocomon.* Moreover, the eleven chapters of Heliodorus on Fractures, which are in this collection, have been translated and published by Vidus Vidius.

Observations in Natural History, made in Travels through Wales. By Mr. Edw. Lhwyd, Keeper of the Ashmolean Museum in Oxford. N° 334, p. 462.

At Snowdon hills we met with little besides what is in Mr. Ray's Synopsis; only the little bulb I found plentifully in flower; and in one of the lakes I gathered a small plant, which I suspect to be undescribed. I searched diligently in these mountains for figured stones; but met with none at all, except cubical marcasites, and crystals, one of which is about 9 inches long, and thicker than my wrist, transparent as glass for the better half, but opaque towards the root, like white marble. Some small ones I met with of the colour of a topaz; and was informed of others purely amethystine, found in the valley of Nant Phrantcon. I find that our ancestors, for want of more precious stones, made themselves beads of opaque, or marble crystal.

Sir William Williams has several Welsh MSS. though I think no Dictionary. They are chiefly modern copies out of Hengwrt study in Meirionydshire, which

* Glossocomon or glossocomion, an apparatus somewhat resembling a box, used by the ancients in fractures of the leg or thigh.

I am promised free access to; and I have taken a catalogue of all the ancient MSS. it contains. There are the works of Taliesyn, Aneuryn Gwawdydh, Myrdhyn ab Morvryn, and Kygodio Elaeth, who lived in the 5th and 6th centuries (but the small MS. containing them all, seems to have been copied about 500 years ago) as also of several others valuable in their kind.

We have neither the ibex nor rupicapra in Wales, nor any other goat besides the common. In our language the park deer is called geivr danas: the former word signifies goats, but whether the latter implies Danish, as if deer had come from Denmark, or somewhat else, we know not. There came this last May into Cardiganshire, two strange birds, seemingly of the aquatic fissiped tribe. They were almost 2 yards tall, and of a whitish colour, with the tips of their wings dark. I took them to be some sort of exotic crane.

Two years since, there came a flock of birds, about a hundred, to a hemp-yard, at a place called Lhan Dhewi Velfrey, in Pembrokeshire; and in one afternoon destroyed all the hempseed. They were a little less than blackbirds; with bills more stubbed and larger than that of a bullfinch; they were very tame; or at least so intent on their feeding, that being forced from their places, they would not remove above 2 or 3 yards. The cocks were of a deep scarlet colour, without any distinction in the feathers of their wings, excepting that the tail and the lower part of the belly were a little paler. The hen had a lovely scarlet breast; her head and back grey.*

Some further Observations relating to the Natural History of Wales. By Mr. Edw. Lhwyd. N° 334, p. 467.

The most considerable discoveries, since my last, were some new species of glossopetræ and siliquastra, the first ichthyodontes, I suppose, that ever were observed in Wales, on the top of a high mountain called Blorens, near Abergavenny. Advancing about 3 miles further into Brecknockshire, at a place called Lhan Elhi, we searched some coal and iron mines. Their coal-works were not pits sunk like draw-wells; but large inroads made into the side of the hill, so that 3 or 4 horsemen might ride in abreast. The top is supported with pillars left at certain distances; and they make their bye lanes, as in other pits, as the vein requires. The slate above this coal afforded only stalks of plants, which we did not save, as it seemed impossible to reduce them to their several proper species. However, close by the pit we found a valuable curiosity, viz. a stone for substance like those they make lime of; of a compressed cylinder form; and as it were cut off even at each end: about 8 inches long, and 3 in breadth: its superficies adorned with equidistant dimples, like Dr. Plot's Lepi-

* Perhaps the loxia enucleator, Linn. or greater bullfinch.

dotes, Hist. Ox. chap. 5, par. 55, and in each dimple a small circle; and in the centre of each circle a little stud like a pin's head. This is the only curiosity of the kind I have seen; and is not referrible to any thing I can think of, either in the animal or vegetable kingdom. Among the iron oars of the same hills we found some new spars, and several specimens of oars shot into a constant and regular figure, though not reducible to any animal or vegetable bodies.

We had at Pontipool, on the 6th instant, an extraordinary shower of hail; which extended about a mile, and lasted near half an hour. It broke down the stalks of all the beans and wheat within that circumference; and ruined as much glass at Major Hanbury's house, as cost 4l. the repairing. Some of the stones were 8 inches about; as to their figure, very irregular and unconstant; several of the hail-stones being compounded.

Concerning the Proportion of Mathematical Points to each other. By the Hon. Francis Robartes, Esq. V. P. R. S. N° 334, p. 470.

It has hitherto passed for a current maxim, that all infinites are equal. Divines and metaphysicians have not scrupled to ground many of their arguments on that foundation. Yet the position is certainly erroneous, as Dr. Halley has abundantly shown in the Phil. Trans. for Oct. 1696. He there gives several instances of infinite quantities which are in a determinate finite proportion to each other, and some infinitely greater than others. The like may be observed of infinitely small quantities, viz. mathematical points, as the following propositions will make appear.

PROP. I. *The Points of contact between Circles and their Tangents are in Subduplicate proportion to the Diameters of the Circles.*—Let two circles ADCH, AFBG, fig. 8, pl. 17, touch each other internally, at the point A. Draw the tangent PAQ, and parallel to it the line MN; also draw the diameter AC. Let AC the diameter of the greater circle be called r; and AB, the diameter of the lesser circle, s; DH, the chord of the arch DAH $= z$; and FG, the chord of the arch FAG, be $= y$; and the absciss AK $= x$.

If the line MN be supposed to move, till it becomes coincident with the tangent PAQ, the nature of a circle will always give the following equations,

$$zz = 4rx - 4xx,$$
$$yy = 4sx - 4xx.$$

When the line is arrived at the tangent, z and y will become the two points of contact, and then $zz = 4rx$, and $yy = 4sx$. ($4xx$ being rejected as heterogeneous to the rest of the equation, by reason of x being become infinitely little). Therefore

$$zz : yy :: 4rx : 4sx :: r : s.$$

Therefore $z : y :: \sqrt{r} : \sqrt{s}$. Q. E. D.

PROP. II. *The Point of Contact between a Sphere and a Plane is infinitely greater than that between a Circle and a Tangent.*—Let A be the point of contact between the sphere ADQF and the plane BC, fig. 9. About the sphere describe the cylinder NPGM. Draw KH to represent a circle parallel to the plane. Let the circle be supposed to move, till it becomes coincident with the plane. The cylindrical surface KHGM will always be equal to the spherical surface DAF. Now when these surfaces become infinitely small, one terminates in the point of contact, and the other in the periphery of the base of the cylinder. Therefore the point of contact is equal to the periphery of the base of the cylinder, equal to a periphery which has the same diameter as the sphere, and by consequence is infinitely greater than any point of contact between a circle and a tangent. Q. E. D.

PROP. III. *The Points of Contact by Spheres of different Magnitude, are to each other as the Diameters of the Spheres.*—For by the 2d prop. the points of contact are equal to the peripheries of such diameters; whose proportion is the same as the diameters.

An Experiment, concerning the Angle required to suspend a Drop of Oil of Oranges, at certain Stations, between two Glass Planes, placed in the form of a Wedge. By Mr. Fr. Hauksbee, F. R. S. N° 334, p. 473.

I procured two glass planes, that measured a radius of 20 inches each; their breadth was about 3 inches: that which I used for the lower plane, was placed with its surface parallel to the centre of its axis, and to the horizon. The planes being very clean, they were rubbed with a linen cloth dipped in oil of oranges: then a drop or two of the same oil being let fall on the lower plane near the axis, the other plane was laid on it; and as soon as it touched the oil, the drop spread itself considerably between both their surfaces. Then the upper plane being raised a little at the same end by a screw, the oil immediately attracted itself into a body, forming a globule contiguous to both surfaces, and began to move toward the touching ends. When it had arrived 2 inches from the axis, an elevation of 15 minutes at the touching ends stopped its progress, and it remained there without motion any way. The planes being let fall again, the drop moved forward till it came to 4 inches from the centre; then an elevation of 25 minutes was required to give it a fixed station. At 6 inches it required an angle of 35 minutes; at 8, of 45 minutes; at 10, a degree. At 12 inches from the axis, the elevation was 1 degree 45 minutes; and so on, at the several stations, as they stand in the annexed table. This after numerous trials, I take to be the most correct, though the others succeeded very nearly the same. It is to be observed, that when the globule, or drop, had arrived to near 17 inches on the planes from their axis, it would become of an oval form; and as it ascended higher, so would its figure become more and more oblong; and, unless the drop was small, on such an elevation of planes as was required at such a progress of the drop, it would be parted, some of it descending, and the rest of it running up to the top at once: but on a drop that separated there, I found the remaining part of it, at 18 inches, would bear an angle of elevation equal to 22 degrees to balance its weight. Higher than that I could not observe. The planes were separated at their axis about $\frac{1}{16}$ of an inch. I found but little difference between small and larger drops of the oil, in regard to the experiment. The angles were measured by a quadrant marked on paper, of near 20 inches radius, divided into degrees and quarters.

Distance in Inches from the Axis.	Angle of Elevation.	
2	0°	15′
4	0	25
6	0	35
8	0	45
10	1	00
12	1	45
14	2	45
15	4	00
16	6	00
17	10	00
18	22	00

An Account of the Eruption of a Burning Spring, at Broseley in Shropshire. By Mr. Rd. Hopton. N° 334, p. 475.

The famous boiling well at Broseley, near Wenlock, in the county of Salop, was discovered about June, 1711. It was first announced by a terrible noise in the night, about two nights after a remarkable day of thunder: the noise awaked several people in their beds, that lived hard by; who coming to a boggy place under a little hill, about 200 yards from the river Severn, perceived a surprising rumbling and shaking in the earth, and a little boiling up of water through the grass. They took a spade, and digging up some part of the earth, immediately the water flew up a great height, and a candle that was in their hand set it on fire. To prevent the spring being destroyed, an iron cistern is placed about it, with a cover to be locked, and a hole in the middle, through which the water may be viewed. If a lighted candle, or any thing of fire be put to this hole, the water immediately takes fire, and burns like spirit of wine, or brandy, and continues so long as the air is kept from it; but by taking up the cover of the cistern, it quickly goes out. The heat of this fire much exceeds the heat of any fire I ever saw, and seems to have more than ordinary fierceness in it.

Some people out of curiosity, after they have set the water on fire, have put a kettle of water over the cistern, and in it green peas, or a joint of meat, and boiled it much sooner than over any artificial fire that can be made. If there be put green boughs, or any thing else that will burn, upon it, it presently consumes them to ashes. The water of itself is as cold as any water I ever felt; and what is remarkable, as soon as ever the fire is out, if hands be put into it, it feels as cold as if there had been no such thing as fire near it.

Observations on the Subterraneous Trees in Dagenham, and other Marshes, bordering on the River Thames, in the County of Essex. By the Rev. Mr. W. Derham, F. R. S. N° 335, p. 478.

Between 4 and 5 years ago, there happened an inundation at Dagenham and Havering marshes, in Essex, by a breach in the Thames wall, at an extraordinarily high tide; and by means of the great violence of the water, a large channel was torn up, or passage for the water, of 100 yards wide, and 20 feet deep in some places; in some more, some less. By which means a great number of trees were laid bare, that had been buried there many ages before.

The trees were all of one sort, excepting only one, which was a large oak, with the greatest part of its bark on, and some of its head and roots. The rest of the trees are by most persons taken to be yew; but a very ingenious gentleman convinced me they might more probably be some other wood, as alder, which grows plentifully by our fresh water brooks, or else hornbeam; but I rather incline to the opinion of its being alder; the grain of the wood, and manner in which the boughs grow, &c. much resembling that of alder, more than hornbeam.

By lying so long under ground, the trees are become black and hard, and their fibres are so tough, that one may as easily break a wire of the same size, as any of them. They maintain this toughness, if the wood be kept dry. But by drying, the trees become cracked, and very flawy within, but look sound outwardly, and with difficulty yield to wedges. But for the trees lying in the marshes, which are covered every flood, and laid bare every ebb, in a short time they became very rotten.

There is no doubt but those trees grew in the place where they now lie; and that in vast multitudes, as they lie so thick upon, or near each other, that in many places I could step from one to another. And there is great reason to think, that not only the marshes, which are now overflown, which are about 1000 acres, are stored with those subterraneous trees, but also all the marshes along by the river side, for several miles: for we discover these trees all along the Thames side, over against Rainham, Wennington, Purfleet, and other places: and in the breach that happened at West-Thorrock, about 21 years ago, they were washed out in as great numbers and of the same kind of wood, as those found lately in Dagenham and Havering Levels.

These last trees are of different sizes; some above a foot diameter, some less. As I was rowed in a boat along the channel, I met with two of the less sort, standing upright, in the same posture in which they grew; their tops just above low-water, and their bottoms, at least the bottom of the channel, at 16 feet deep. We endeavoured to draw them out, but could not do it with all our strength. They seemed to be about 2 inches diameter in their trunk, had some of their boughs on, were dead, and probably, being young and light, escaped the force of what threw the other more large and unwieldy ones down. Most of the trees, that I met with, had their roots on, and many of them their boughs, and some a part of their bark. There was only one that I perceived had any signs of the ax, and its head had been lopped off.

As I passed the channel which the water had torn up, I could see all along the shores vast numbers of the stumps of those subterraneous trees, remaining in the very same posture in which they grew, with their roots running some down, some branching and spreading about in the earth, as trees growing in the earth commonly do. Some of those stumps I thought had signs of the ax, and most of them were flat at top, as if cut off at the surface of the earth: but being rotten, and battered, I could not fully satisfy myself, whether the trees had been cut or broken off.

The soil in which all those trees grew, was a black, oozy earth, full of the roots of reed; on the surface of which oozy earth the trees lay prostrate, and over them a covering of grey mould, of the same colour and consistence with the dry sediment, or mud, which the water leaves behind it at this day. This covering of grey earth is about 7 or 8 feet thick, in some places 12 feet or more, in some less; at which depths the trees generally lie. The trees lay in no kind of order, but some this way, some that, and many of them across: only in one or two places I observed they lay more orderly, with their heads for the most part towards the north, as if they had been blown down by a southerly wind, which exerts a pretty force on that shore.

As to the age in which those trees were interred, it is hard to determine. Many think they have lain in that state ever since Noah's flood. But though I have not the least doubt but that at this day there are many remains of the spoils of that deluge, even in the highest mountains, yet I rather think these trees to be the ruins of some later age, occasioned by some extraordinary inundations of the river Thames, or by some storms, which blow sharply on this shore. As for extraordinary inundations of the Thames, there is at this day a mark, which if occasioned by an inundation, must have been that of a prodigious one, beyond any ever known to have happened in that river; which is a bed of shells, if not of a kind of marble too, lying across the highway on the descent near Stifford-bridge, going from S. Okendon. Below this bed of shells, at above 50 or 60 yards distance, in the bottom of the valley, runs a brook, that empties itself into the Thames at Purfleet, about 3 miles from thence; which brook ebbs and flows with the Thames, but not at any certain height, by reason of mills standing on it; but above a pretty high-water in the brook, the surface of the bed of shells lies above 20 feet perpendicular. Consequently if this bed of shells was reposited in that place by an inundation of the Thames, it must be such as would have drowned a vast deal of the adjacent country, and have overtopped the trees near the river, in West-Thorrock, Dagenham, and the other marshes, and probably by that means over-turn them.

For had these trees been left there by that deluge, we should not find the bed of earth, in which they grew, so entire and undisturbed, as it manifestly is at this day, a spongy, light, oozy soil, full of reed-roots, and of much less specific gravity than the stratum above it. Whereas I can from experiments affirm that in three places where I have tried it, the strata are in a surprising manner, gradually specifically heavier and heavier, the lower and lower they lie.

As to the manner how these trees came to be interred, I take it to be from the gradual increase of the mud, or sediment, which every tide of the Thames left behind it. I presume those trees might be thrown down before the walls or banks were made, that now keep the Thames out of the marshes; and then they were covered every tide. And as they lay thick, and near each other on the ground, they would soon gather a great deal of the sediment, and be soon covered with it. And after the Thames-walls were made, every breach in them, and inundation, would leave great quantities of sediment behind it; as I by a troublesome experiment found, in going over some of the marshes, soon after the late breach, where I found the mud, generally above my shoes, and in many places above my knees. And it is a practice among us, of which we have divers instances, that where a breach would cost more to stop, than the lands overflown will countervail, there to leave the lands to the mercy of the Thames; which by gradually growing higher and higher, by the additions of sediment, will in time shut out the water of the river, all except the highest tides. And these lands they call saltings, when covered with grass; or else they become reed-grounds, &c.

That it was the sediment of the Thames, that buried those trees, is further manifest from what was said before, of the likeness of the earth above them, in all respects, to the sediment the river now lets fall, when dry: which may be observed to consist of many distinct layers; some $\frac{2}{10}$ of an inch thick, some less, and some scarcely $\frac{1}{30}$ of an inch; all which several layers are doubtless the several quantities which every tide left behind it. This sediment, when dried by the sun and wind, becomes tough and hard, and looks like a grey lapis scissilis, or slate, divisible into many plates or layers. And perhaps we may ascribe the conformation of slate, Muscovy-glass, and other such laminated concretions, to a like work of nature, by adding new layers of such petrifactions, and particles, as the fossil is formed of.

I presume there will be no doubt, but that the subterraneous wood receives its blackness from vitriolic juices in the earth. If any should doubt it, I have tried the experiment, and find that alder-wood, whether green or old, becomes blackish, much of the same colour as the wood above-mentioned, in a solution of copperas. Which is not only an argument, that the blackness of the wood is owing to vitriol, but also that the wood is alder, or some such like wood, that will become black with vitriol: for I am informed that all subterraneous wood is not black, particularly fir. I have also tried hornbeam since, after the same manner, and find that also becomes black, as the alder does.

Experiments and Observations on the Effects of several sorts of Poisons on Animals; made at Montpellier in 1678 *and* 1679, *by the late William Courten, Esq. Communicated by Dr. Hans Sloane, R. S. Sec. Translated from the Latin MS.* N° 335, p. 485.

In the month of July, Anno 1678, we gave a dog a piece of bread steeped in 2 oz. of the juice of solanum Batavicum, Dutch night-shade, expressed from the green plant, and mixed with cheese. He did not seem to receive any injury from it. The same dose of the juice of the leaves of cicuta, or hemlock, had no more effect. We gave also the same dog a pretty large root of aconitum pardalianches, wolfs-bane, with the leaves and flowers of the same plant, bruised and mixed with flesh; which did him no hurt.

Two drachms of helleborus albus, white hellebore, very much disordered him, and caused retchings, suffocations, vomiting, and voiding of excrements. This dog, as we afterwards often observed in others that had taken the like corrosive medicines, whether because he was not able to endure the pain, or by reason of any other uneasiness, often scratched the ground with his feet: however he recovered, and got well again. He swallowed also five roots of colchicum ephemerum, meadow-saffron, dug fresh out of the earth: with which he was violently tortured, but did not die. At last he took 2 drachms of opium, which threw him into a deep sleep; but after vomiting and voiding fetid excrements, he recovered by degrees his former briskness.

Some weeks after, when the same dog had recovered his former vigour, we tried on him a much stronger poison. We caused him to be bit 3 or 4 times on the belly, a little below the navel, by an enraged viper. There arose immediately little black bladders, containing a liquid blackish sort of corruption; they were flaccid and tremulous, like the gall-bladder when it is about half full; and a livid colour by degrees spread over all the neighbouring parts. The venom propagated itself with surprising quickness, and weakened all, but more especially the animal functions: for notwithstanding the diaphragm still performed its office pretty strongly, though with some disorder, and the heart continued beating, though faintly and irregular; yet they seemed to fare much better than the brain, the strength of which was so weakened, that it could perform the functions of sense and motion but very faintly; so that the dog lay without any strength or sensation, as if he had been seized with a lethargy, or apoplexy: which kind of stupidity we also observed in different degrees in all other dogs bit by a viper. Being willing to save this dog, though we had found by many experiments, that much slighter wounds made by a viper had occasioned death, we had recourse to several remedies; and therefore cupped and scarified the part that was wounded, and applied theriaca, Ven. treacle. We let him rest for about two hours: but his sleepiness increasing more and more, and his vital and animal functions sinking, we were obliged to have recourse to another method of cure. Wherefore, to dispel his sleepiness, we forced into his throat half a drachm of volatile salt of hartshorn, mixed in broth; a little after which his eyes, which before looked dead, began to revive, and he was able to stand on his feet and walk. We then repeated the same dose of the volatile salt, by which he was freed from his sleepiness, and the strength of his heart recovered; and notwithstanding he remained weak for 3 days, yet he sensibly recovered strength, though he would not eat all that time: but he drank water very plentifully, and greedily; and on the second day did not refuse cold broth: after the 3d day he began to eat solid meats, and seemed now out of danger; only some large foul ulcers remained on that part of his belly that was bitten, of which he would scarcely have died, had he not been killed by another dog; which prevented us from seeing the event of this experiment.

But to try more fully the force of the above-mentioned poison, it is necessary to make several experiments of it: for though the bite of a viper, if it be but slight, may kill some dogs; yet in the month following, a large strong dog, that was bitten in the tongue, which is a very dangerous part, recovered without any medicines. His tongue indeed turned black, and swelled so much, that it could scarcely be contained in his mouth : he was stupid, as is usual from the venom of a viper, but not so much, but that he could stand on his feet. A few hours after, his sleepiness decreased; and the next day he endeavoured to lap water, but the size of his tongue prevented him. On the third day he threatened to bite any one that disturbed him, and had recovered so much strength, as to be able to escape out of the place where he was kept: and two days after, he was seen in the streets; but what became of him afterwards we could not learn.

October the 17th, we gave a dog 15 grains of the dried root of napellus, monk's-hood, powdered, and mixed with flesh and broth. As soon as taken, the dog was seized with a difficulty of swallowing, or rather seemed as if he was like to be strangled. He immediately grew faint and restless, and dug the ground with his feet; but he soon desisted, by reason of a fainting fit, as we imagined from the dull colour of his eyes, and a weakness over all his body. This fainting was presently succeeded by a violent vomiting, in which he threw up the flesh he had eaten, very little altered. His fainting soon returning again, he laid himself on the ground; but being seized with terrible convulsions of the abdomen, diaphragm, and of almost the whole body, he ran from place to place, and vomited so great a quantity of frothy matter, that he was like to have been suffocated. His vomiting increased, with a kind of crying and sobbing, like broken sighs, as if he had endeavoured to bark at those that stood by. In this manner he was miserably tormented for the space of an hour; when all his symptoms remitted, and by degrees he recovered. In the preceding summer, we gave a little dog a drachm of the said root of monk's-hood. He was soon after seized with the same symptoms, but they were longer and more violent; and he also recovered. In both these dogs we particularly noticed these broken and interrupted sighs, or kind of sobbing; because we did not observe the like to be occasioned by any other poison we had tried.

An ounce of the leaves, flowers, and seed of the monk's-hood when green, being bruised and given to a dog, scarcely disordered him any more than if he had eaten so much grass. About the same time we made trial of the nux vomica on another dog, that we might see its effects on his body when dead. The dog accordingly dying in a short time, we found his stomach and small guts very red; and judged that this redness and inflammation were caused by the corrosiveness of the medicine.

October the 20th, we injected warm into the jugular vein, of a strong lusty dog, 1 oz. of emetic wine: for $\frac{1}{4}$ of an hour, after the operation was over, and he was let loose, he continued pretty well, unless that he seemed somewhat dejected; but afterwards he began to grow ill, and an unusual agitation was manifest about the diaphragm; this was followed by a continual vomiting, and a little after by an evacuation of some hard excrements. By these evacuations he seemed to be somewhat relieved; but he soon grew uneasy, moved from place to place, and vomited again. After this he laid himself down on the ground pretty quietly; but his vomiting returning again, disturbed his rest, and abated his strength, which grew weaker and weaker; for in the space of an hour he vomited 12 times or more, and sometimes voided some liquid fæces, in small quantity; having frequent inclinations to go to stool, but in vain, as in a tenesmus. An hour and a half after the operation, he being so weak that he could not stand, his eyes dull, and looking as if he were half dead, we gave him some warm broth through a funnel. With this he was exceedingly refreshed at once, and soon after could look about, stand on his legs, and walk; but by reason of his weakness, reeled as if he had been drunk. We left him alone in a warm room, where he remained cold, and lay as if he had been dying; and in an hour after, we forced him to take some more broth, which revived him again: but in a little time, after some agitation of his body, he vomited, made urine very plentifully, howled miserably, and died convulsed. Next day, in viewing his viscera, we found two things very observable, but neither of them occasioned by the liquor injected; one in the heart, the other in the œsophagus. In the heart there were two polypuses: that which possessed the right ventricle, stretched itself into the vena cava and pulmonary artery; and that in the left ventricle, sent branches into the adjoining vessels, and was less than that in the right ventricle. The substance of the polypus was pretty firm, of a flesh colour, somewhat pellucid, and, being cut through the middle, was altogether of the same colour and consistence as on the surface. To the œsophagus there grew a remarkable gland, which was hard, callous and foul, and opened with a small, round, fleshy orifice, into the inside of the stomach, where, on pressing it, a little purulent matter came forth. On opening this gland or tubercle, we found in it a great many little worms, wrapped and entangled together, and moistened with a purulent matter. Some of these worms were above 4 inches long, others less. We found afterwards the like glands, full of worms, in other dogs, and in most we opened, but not so much corrupted as in this. We observed also the like foul glands in the aorta descendens, but in one only found a worm like these, which was almost got out of it, through an orifice, into the cavity of the thorax. After this we likewise observed more polypuses in dogs.

October the 27th we injected warm into the jugular vein of a dog a drachm and a half of sal ammoniac, dissolved in an ounce and a half of water. The liquor had scarcely arrived at the heart, when the dog presently fell into deadly convulsions over his whole body: on which we let him loose, but he died immediately.

November the 18th, we caused a whelp to be bitten in the lower lip by a cæcilia, or blind worm, so that the blood appeared in the wound. The whelp died indeed the same day, but because we had committed him to the care of another person, we could not be certain whether he died of the wound or not; and what increased our suspicion was, that there did not appear on the part that was bitten any livid colour.—December 12 we injected into the jugular vein of a dog a drachm of salt of tartar, dissolved in an ounce of warm water; he died crying, and in convulsions, almost immediately.—December 15 we found a polypus in both the ventricles of the heart of a dog, each polypus stretching itself with a double root into the vessels of the ventricle it possessed. Afterwards we often observed the like polypuses in other dogs.

December 20, we injected warm into the jugular vein of a dog an ounce of urine, made by a man fasting. The dog was uneasy during the injection, and while the liquor passed to the heart; but he was not seized with any convulsions or other ill symptoms; and being let loose, eat bread very greedily.—The same day we made a gentle decoction of 2 drachms of white hellebore, well powdered, in spring water, and evaporated it away to 9½ drachms; and the next day injected all the decoction, strongly pressed out and turbid, into the jugular vein of a dog. At first some few drops only passed to the heart, as some concreted blood obstructed the passage; but those drops very much affected the dog, as he was immediately seized with convulsive motions; but soon after, when the liquor had removed what lay in its way, and had entered the heart, it killed the dog as suddenly as if he had been shot through the heart with a bullet; for having loosened him presently, to see if any life remained, he was quite dead and flaccid, and hung like a fleece in the hand of the person that held him.

January 2, 1679, vinegar was injected warm into the jugular vein of a dog, without doing him any manifest harm.—The same day we caused a whelp to be stung in the tongue by several scorpions, but the wounds made by the scorpions, by reason of their weakness, being but slight, and not penetrating deep, we made a small incision on the abdomen, and drawing aside the skin, let the scorpions make several wounds on it; but without any effect, though we often forced the sting into the wounds, and pressed the bladder that is supposed to contain the venom.—In like manner a pigeon, being several times stung by a scorpion, remained unhurt.

January the 3d, 2 drachms of sugar dissolved in an ounce of water, were injected into the jugular vein of a dog, he received no harm from the injection, but continued well for the 3 days after, that we kept him.—January 4th, a drachm and half of spirit of salt, diluted in an ounce and half of water, and injected into the jugular vein of a dog, killed him immediately. In the right ventricle of his heart we found the blood partly grumous and concreted into harder clots than ordinary, and partly frothy. In the same dog that gland that contains worms, and is frequently found in the œsophagus, opened with two orifices into the cavity of that part, and in the sinuses of it there lay several small worms.

January the 5th, we gave a dog 12 small caterpillars of the pine-tree, or pityocampæ, vel erucæ pini, weighing half a drachm, which we bruised alive, and mixed with flesh. The dog, though he was but young, received no other hurt, than that now and then he seemed as if he endeavoured to swallow something, or was troubled with an inclination to vomit, from whence we judged the stomach and œsophagus to be only lightly affected; but these symptoms vanished in a few hours, and the dog continued brisk, and greedy of meat, all the rest of the day.—The same day we included a rat in a large glass with a scorpion, but the scorpion, being dull and benumbed with the extreme coldness of the weather, was able to wound the rat but very weakly; with which however the rat being provoked, set upon the scorpion, and gnawed off and devoured part of him, keeping his eyes shut all the while, that he might not be hurt by his claws or sting. The same fate happened to another scorpion, which we added to the former; but the rat notwithstanding remained unhurt.

January the 6th, we killed a dog almost in a moment, by injecting into his jugular vein an ounce of spirit of wine, in which there was dissolved a drachm of camphire.—The same day we injected warm into the crural vein of a cat, 50 grains of opium, dissolved in an ounce of water. The cat, presently after the injection, seemed very much dejected, but did not cry; only made a low, interrupted, complaining noise. After this followed tremblings of her limbs, convulsive motions of her eyes, ears, lips, and almost of all parts of her body, with violent convulsions of her breast; sometimes she would raise up her head, and seem to look about her, but her eyes were very dull and deadish; and though she was let loose, and had nothing tied about her head or neck, yet her mouth was so filled with foam or froth, that she was like to be strangled. At last, her convulsive motions continuing, and being seized with a stretching of her limbs, she died within a quarter of an hour. On opening her body, we did not find the blood much altered from its natural state.

February the 7th, we injected into the crural vein of a lusty strong dog, a drachm and half of opium, dissolved in an ounce and half of water. The dog immediately showed the great pain he endured, by a violent struggling of his whole body, a loud noise that he made, notwithstanding his jaws were tied, a great difficulty of breathing and palpitation of the heart, with convulsive motions of almost all parts of his body; in a little time all these remitted, and he was seized with a profound sleep, as if he had been in a lethargy or apoplexy. Having let him loose, he lay upon the ground, without moving or making any noise, in so deep a sleep, that he would not move with beating. About half an hour after, if we beat him, he would move a little, but presently lay down again. After an hour, if we beat him, he would move a little more; and by degrees his sleepiness a little decreasing, in an hour and half or 2 hours time, when he was beat he would make a noise, and walk a little, but seemed very heavy and stupified, and reeled as he went; but as soon as we left off beating him, as if he had forgot every thing that had passed, he presently laid himself down again, and fell asleep. Next day when we viewed the place where he lay, we found a great quantity of fetid excrements, like corrupted blood, or the diluted opium that he had taken; but still his drowsiness continued, and though we beat him with whips, that he ran crying about the room, yet he presently forgot it, and immediately fell asleep again. In this sleepy condition he continued three days, refusing whatever was offered him to eat, or rather not minding that or any thing else. On the fourth day we found him dead: but perhaps he would not have died of the stupifying quality of the opium, if, considering the extreme coldness of the weather, we had put him in a warmer place, and had forced him to have taken some broth.

February the 8th, we found in the bladder of a tortoise, adhering to its coat, a flat porous stone, about twice as large as a lentil.—February the 9th, a drachm and half of common salt, dissolved in an ounce and half of water, was injected into the jugular vein of a dog. After the injection, he was thirsty, and drank water greedily, but in other respects he seemed to be pretty well, and the next day was quite recovered.

February the 20th, 1679, we injected into the crural vein of a little dog, half an ounce of warm oil of olives, which we did with a great deal of difficulty, and very slowly, by reason of the smallness of the vein and thickness of the liquor. For half a quarter of an hour, that we were injecting the liquor, the dog did not seem to be uneasy, or out of order; but after that, he barked, cried, looked dejected, and fell presently into a deep apoplexy; so that his limbs were deprived of sense and motion, and were flexible any way at pleasure; his respiration still continuing very strong, with a snorting and wheezing, and a thick watery humour flowing in great quantity out of his mouth, which was sometimes mixed with blood. He lost all external sense: his eyes, though they continued open, were not sensible of any objects that were put to them;

and we touched and rubbed the cornea, as sensible a part as it is, without any more sign of his being sensible of it, than if he had been dead. Yet his eyelids had a convulsive motion; his hearing was quite lost; and his feeling, though at first he seemed to have some small sense of it when we touched his wound, yet afterwards it was so dull, that we pinched his claws and flesh with pincers, and bored holes through his ears, without his moving, or seeming to be the least sensible of it. It is worth observing, that in the midst of his sleep, being sometimes seized with a convulsive motion of his diaphragm, and other muscles that help respiration, he would bark strongly as if he were awake, and in a little time would be quiet again; so that in less than a quarter of an hour his rest would be disturbed three or four times, with this violent barking. But considering this more attentively, we found that at the very time he barked, he was as void of sense as before; for we could neither make him bark, nor leave off barking, by either beating or pricking him; but in a little time he would leave off of himself, and return to it again some time after. Thus in 3 hours after the injection, spent in sleeping and barking, he died; and having opened his body after he was dead, we found the bronchia of the lungs filled with a thick froth.

A few days after, we injected a larger quantity, viz. an ounce of oil of olives, into the jugular vein of a dog, which suffocated him the same moment.—Afterwards, the same quantity of oil of olives, being injected into the jugular vein of a dog, killed him in an hour's time. He was seized with a great sleepiness, snorting, and wheezing, and a bloody water ran plentifully out of his mouth. In this dog, though he did not die immediately, we did not observe the barking, as in the former, but in all that were suffocated by oil, we found their lungs filled with a very thick froth.

February the 27th, we injected 10 drachms of highly rectified spirit of wine into the crural vein of a dog. The dog died in a very little time very quietly, and as it were with pleasure, licking his jaws with his tongue, and breathing quick, but easily, without barking, crying, or any convulsive motion. In the vena cava, and right ventricle of the heart, the blood was concreted into a great many little hard clots; which appeared yet more conspicuous and harder in some blood that flowed back from the vein into the syringe. In this dog we found the emulgent artery of the left side to be double.

March the 2d, we injected three drachms of rectified spirit of wine into the crural vein of a small dog; which made him apoplectic, and as he were half dead. In a little time he recovered from his apoplexy, but became giddy; and when he endeavoured to go, reeled and fell down. Though his strength increased by degrees, yet his drunkenness still continued, his eyes were red and fiery, and his sight so dull, that he did not seem to take notice of any thing; and when beaten he would scarcely move. However, in 4 hours time he grew better, and would eat bread. The next day he was brisker, and seemed past all danger. In dissecting the same dog some time after, we found in the small guts 2 flat-worms, one of them about 6 spans long, and the other about 5. They had perforated the gut, and one of them was got half out of it into the cavity of the abdomen. About the same time we found in two dogs a worm, of near a foot in length, out of the intestines, in the cavity of the abdomen, the intestines being nowise perforated, but remaining sound and whole. That we might be more certain of this, we separated them from the mesentery, and viewed them very carefully. But in both these dogs the omentum was of a bad colour, and putrefied; from whence we conjectured, that these worms were bred from the putrefaction of the omentum.

We injected into the crural vein of a dog 5 ounces of a strong white wine, which made him very drunk, and little different from what a less quantity of spirit of wine would have done; but in a few hours his drunkenness abated, and he recovered.—The same month of March, we injected into the crural vein of a dog, an ounce of a strong decoction of tobacco. He was seized immediately with strange convulsions of his whole body. At first his eyes looked wild and distorted, his jaws trembled, and in a little time he died terribly convulsed. This experiment we repeated several times after, and always with the same success.—Ten drops of distilled oil of sage, mixed with half a drachm of sugar, and dissolved in an ounce of water, being injected into the crural vein of a dog, did him no harm.—In a castrated dog, we observed the processes of the peritonæum and spermatic vessels covered with fat, and scarcely to be seen; and that he did not smell so rank and strong as other dogs that had not been castrated.

A yellow streaked lizard, or lacerta chalcidica, which had been kept all the winter in a glass with bran, being exposed to the sun to refresh it, on the contrary died in a few hours. We have also often found, that scorpions exposed to the hot sun, especially in the summer, died in a short time.—A drachm of purified white vitriol, injected into the crural vein of a dog, killed him immediately.—Fifteen grains of salt of urine, dissolved in an ounce of water, and injected into the crural vein of a dog, cast him into such violent convulsions, that we were afraid he would die under them. When he had recovered himself a little, we repeated the injection with the same quantity; but the dog got the better of it, though with a great deal of difficulty, and perfectly recovered.

April the 27th, we made a decoction of 2 drachms of senna in water, and injected warm 3 ounces of it into the crural vein of a very fat, large, and strong dog. During the operation he continued pretty quiet, without any sign of pain or uneasiness; and when it was over we let him loose, expecting the event of it. He was melancholy and dejected, but easy and without any sensible commotion, for the space of an hour. After that, his respiration grew quicker, he had a murmuring noise in his belly, with violent commotions of the muscles of his abdomen, diaphragm, stomach and intestines, and vomited plentifully a bilious matter. After his vomiting he grew faint, and in a little time his vomiting returned again; so that in an hour and a half he vomited 4 times. His strength and appetite were very weak, and he would eat nothing for 3 days. But on the 3d day his appetite, strength, and former briskness returned, and he recovered.

Two dogs, which had their recurrent nerves cut, lost their barking and voice. But doubting whether the wound or scar might not affect and hurt the motion of the muscles, we performed the same operation on another dog, but without cutting the nerves; and when the wound was healed, he barked as freely as before.—A dog that had the nerves of the par vagum cut asunder, presently grew dejected and faint. He breathed very slowly, and with sighs; for when he had drawn in his breath leisurely and insensibly, it came forth again immediately very forcibly, and with a sigh, as if it had been retained a long time in the lungs. The muscles of the abdomen and the diaphragm laboured hard, as if they were to supply the defect of the lungs, which were grown almost useless, by being denied an influx of spirits by the pneumonic nerves. The dog refused all kind of meat; sometimes he vomited, or had an inclination to vomit; and at last, in 2 days time, he died.

Another dog, that had the nerves of the par vagum only tied, lived 10 days. He vomited frequently, and would not eat, unless clandestinely: he breathed with sighs, and was very faint.—A dog, that had the trunk of the aorta descendens tied hard, a little above the diaphragm, immediately lost the use of his hind legs; for when he stood on his fore legs, he would draw after him his hinder legs, as if they had been dead: he grew weaker by degrees, and died in 5 hours.—July the 12th, a mole being stung in the side by a scorpion, died immediately convulsed. On this occasion we observed, that the intestinum cæcum is wanting in moles.

Observations on Natural History and Antiquities, in Travels through Wales. By Mr. Edw. Lhwyd. N° 335, p. 500.

In a steep rock, called Craig y park, and others in the parish of Ystrad Dyvodog, we observed divers veins of coal, exposed to sight as naked as the rock; and found a flint axe, somewhat like those used by the Americans. At Goldclif, in Monmouthshire, we had some variety of formed stones: but what pleased me most was an asteria, or column star-stone, beset with sprigs its whole length, issuing from the commissures of the plates. This county abounds with entrochi; one of which I saw in a rock at the isle of Barry, above 15 inches in length; and another about 10 inches long, but as thick as a cane. We took their figures and dimensions, but could not get off the stones without breaking.

At Kaerphily Castle the people showed us an inscription, as they supposed, on one of the steps of the tower. I must confess I am not fully satisfied whether it were ever designed for reading, or for some kind of antique ornament; but rather incline to the latter. The stone was not designed for a step, there being none of the same kind in the whole staircase. The marks were mostly worn out by treading; and it is possible they might be once more uniform. Were it the old Celtic character, which Cæsar says was like the Greek, it would be a noble discovery: but I fear our ancestors, if ever they had any writing, have left us none upon stones.

Observations on the Antiquities and Natural History of Ireland. By Mr. Edw. Lhwyd, in his Travels through that Kingdom. N° 335, p. 503.

The most remarkable curiosity we saw by the way towards the Giant's Causeway, was a stately mount, at a place called New Grange, near Drogheda; having a number of huge stones pitched on end round about it, and a single one on the top. The gentleman of the village, Mr. Charles Campbel, observing that under the green turf this mount was wholly composed of stones, and having occasion for some, employed his servants to carry off a considerable parcel of them; till at last they came to a very broad flat stone, rudely carved, and placed edgewise at the bottom of the mount. This they discovered to be the door of a cave, which had a long entry leading into it. At the first entering we were obliged to creep; but still as we went on, the pillars on each side of us became higher and higher; and coming into the cave, we found it about 20 feet high. In this cave, on each hand of us, was a cell or apartment, and another proceeded straight forward, opposite to the entry. In those on each hand was a very broad shallow basin of stone, situated at the edge. The basin in the right hand apartment stood in another; that on the left hand was single; and in the apartment straight forward there was none at all. We observed water drop into the right hand basin, though it had rained but little in many days; and suspected that the lower basin was intended to preserve the superfluous liquor of the upper, (whether this water were sacred, or whether it was for blood in sacrifice) that none might come to the ground. The great pillars round this cave, supporting the mount, were not at all hewn or wrought: but were such rude stones as those of Abury in Wiltshire, and rather more rude than those of Stonehenge: but those about the basins, and some elsewhere, had such barbarous sculpture (viz. spiral like a snake, but without distinction of head and tail) as the fore-mentioned stone at the entry of the cave. There was no flagging nor floor to this entry or the cave; but any sort of loose stones every where under feet. Several bones were found in the cave, and part of a stag's or else elk's head; and some other things, which I omit, because the labourers differed in their accounts of them. A gold coin, of the Emperor Valentinian, being found near the top of this mount, might bespeak it Roman; but that the rude carving at the entry, and in the cave, seems to denote it a barbarous monument. So, the coin proving it more ancient than any invasion of the Ostmans or Danes; and the carving and rude sculpture, barbarous; it should follow, that it was some place of sacrifice, or burial, of the ancient Irish.

The Giant's Causeway is so well described in the Phil. Trans. N° 212 and 241, that nothing can be added to that account of it. We have the same stone on the top of Cader Idris, one of the highest mountains of North Wales; but ours is less elegant, and does not at all break off in joints; nor could I satisfy myself that there are set joints (as in the Entrochus and Asteria) in the Basaltes of Ireland; but that it is the nature of the stone to break off in such a convex form. However, we could perceive no seams in these pillars, excepting on those sides that were exposed to the weather.

Another remarkable curiosity we met with, was a copper trumpet, like a sow-gelder's horn; having the hole for sounding near the middle, and two rings at the smaller end, about two feet long. Three of these were found in an old karn, i. e. a large heap of stones, at Balle Niwr, near Carreg Fergus.—We could make nothing of the petrifying quality of Loch Neach; but that they sometimes find stones there, having the grain of wood.—We met with some Irish inscriptions there, and others here; which none of the critics in that language, we conversed with, could interpret.

Near Larne, in Antrim, we met with one Eoin Agniw, whose ancestors had been hereditary poets, for many generations, to the family of the O'Neals; but the lands they thereby held being taken away from his father, he had forsaken the muses, and betaken himself to the plough; so we made an easy purchase of about a dozen ancient manuscripts on parchment.—As to your queries: the mackinboy, is the tithymalus hibernicus, or latifolius sylvaticus, Cat. Hort. Oxon. Their shamrug is the common clover. The potato is not indigenous of Ireland. The arbutus is the same with the common. I have the account of the living fossil muscles attested, and signed, by the four persons present at the finding them; so that nothing but its being a singular instance makes me scruple the relation: but the labourers have such a character for veracity, that I rather incline to believe it.

Experiments showing the Proportion of the Power of the Loadstone at different Distances. By Mr. Fr. Hauksbee, F.R.S. N° 335, p. 506.

I took a quadrant of 4 feet radius; and having fixed it to the floor, in the position of the needle, whose south point directed itself to no degrees, I then fixed a board likewise on the floor, in a direct angle from the same; the graduations on the board being 3 inches distant from each other. The needle was suspended on a point arising from the centre of the quadrant, from whence the several stations of the magnet were measured. The magnet was laid on a thin piece of board; under which to one side was nailed a narrow slip of wood, to slide it along the side of the graduated board, by which the stone might be always kept in the same direction to the needle. The stone used was about 6 pounds weight; it was rough, and of an irregular figure; yet I could discover no inconveniency thence arising in the experiment, it being, and acting at all distances in the same position, as first placed on the board: and I have several times observed, that the proportions of its power will be regular, and agreeable to the several distances: for when the stone has been differently posited on the forementioned thin board, different angles of the needle would ensue at the same stations; yet their proportions to each other would be nearly the same. My meaning is this: suppose the stone was so placed, as at 3 inches from the needle it would give the needle an angle of 90 degrees, the stone being continued in the same direction at the several stations, the proportions of its power to each other would be much the same, as if the angle of the needle at the first beginning made only 87, or even but 80 degrees, on the quadrant; for on a small alteration of the poles of the stone, such diversity of angles will arise.

In these experiments I made use of two needles; one of a radius of 6 inches, the other but of one inch: which last, after abundance of trials, I found to be most accurate; besides the advantage it gave in beginning the experiment 6 inches nearer the stone, than the other: and from 2 feet distance from the same, it became nearly agreeable to the angles made by the long needle, to all the farther distances; as you will find by the following tables, which were made with the several needles in the same direction of the stone. I measured the angles by a silk thread strained directly over the needle to that part of the quadrant to which it was directed; which was the best way I could contrive, to come nearest the truth.

It may be observed, from the following tables, that the long needle at 9 inches from the stone, made somewhat a larger angle than the short needle at 3 inches distance from the same; that the short needle at the distance of 9 inches, made an angle of 9 degrees less than the long one at the same place. But this odds will easily be accounted for, if we consider the disproportions of the needles lengths; for the point of the long needle at 9 inches, was brought within an inch as near the stone, as the point of the short needle was, when but 3 inches distant from the same: the point of the short needle at 9 inches from the stone, was 5 inches further from it, than the long one at the same station. These disproportions being considered, it is no wonder such difference of angles should ensue, by using the several needles near the stone; for at 2 feet, and the farther distances, they become nearly agreeable, as before said. In speaking of distances from the needle, I always mean from its centre. It is further observable, that the stone at 5 feet distance from the needle, made an angle of 2 degrees with the one, and with the other of 2 and a half; yet on the absence of the stone, they would return to no degrees, as at first: which plainly shows, that the influence of the stone extended farther; though observations, at remoter stations, could not easily be determined.

Experiments with both the Needles.

Distances of the Loadstone from the Needle in Inches.	The several Angles of the short Needle at several Distances.	The several Angles of the long Needle at the several Distances.
3	87° 00′	
6	84 00	
9	78 30	87° 30′
12	69 00	81 45
15	56 45	72 15
18	43 30	53 20
21	33 00	35 00
24	24 00	24 10
27	18 00	17 50
30	13 30	13 10
33	11 00	10 10
36	8 45	8 00
39	7 00	6 30
42	5 30	5 15
45	4 30	4 10
48	3 50	3 30
51	3 20	3 00
54	3 00	2 35
57	2 45	2 15
60	2 30	2 00

At greater distances, and even the more remote in these tables, the power of the stone is so weak, and the measuring the angles at all times exactly, so difficult, that it is well if we come sometimes within 10 or 20 minutes of the truth.

This load-stone weighed exactly 6lb. $1\frac{1}{4}$ oz. avoirdupois. Its breadth at the north pole was 4 inches; at the south pole 5 inches. The length of the shortest side was $6\frac{1}{2}$ inches, of the longest side $7\frac{1}{2}$ inches. Its thickness at the north pole was one inch and a half, and at the south pole one inch.

The Specific Gravities of several Metalline Cubes, in comparison with their like Bulks of Water. By Mr. Fr. Hauksbee, F.R.S. N° 335, p. 511.

These cubes were supposed to be extraordinary in their several kinds, except the gold. Their workmanship was very accurate; and they were exactly of a size, though they differed a little from our measure. Six of them being laid on a rule, side by side, measured about a tenth more than 6 inches; and if their sides were changed, they still made the same measure. And it further appeared that they were exact, by their agreeableness in the weight of their respective bulks of water, as may be observed by comparing them in the following table.

Troy Weight.

	Weight of the several Cubes in Air.			Weight of the several Cubes in Water.			In proportion to their like bulk of Water.
	Oz.	dwts.	gr.	Oz.	dwts.	gr.	as … to 1.
Gold	9	11	8	9	0	$6\frac{1}{2}$	17.3
Silver	5	14	10	5	3	$10\frac{1}{2}$	10.421
Copper	4	16	8	4	5	$10\frac{1}{2}$	8.841
Brass	4	5	7	3	14	9	7.81
Lead	6	2	12	5	11	17	11.35
Iron	4	4	17	3	13	20	7.777

An Account of what appeared on dissecting the Body of Mr. Dove. By the late Mr. William Cowper, F.R.S. N° 335, p. 512.

Mr. Dove's body in divers parts appeared of a black, blue, livid, and various colours, before I made any incision into it; particularly the back, where the blood was settled, had a cadaverous blackness; where the cuticula was here and there vesicated, or distended with serum. Of this there was no appearance before death. The muscles of the abdomen had a mortified appearance, being of a blackish green colour. The liver was entirely sphacelated. The spleen had large mortified spots on its surface: both these parts were specifically lighter than in the natural state; insomuch, that portions of each of them swam on the surface of water, and seemed to have more air in them, than we commonly find in the lungs in their natural state. The rest of the viscera, in this lower cavity, were not in so bad a state; though the guts had here and there blackish spots on them.

The pectoral muscles were in little better state, than those of the abdomen; nor were the intercostal muscles like those of the limbs. I am apt to think all the muscles employed in respiration, had more or less of this blackish appearance. The right lobes of the lungs were diseased; and the same side of the thorax had a small quantity of serum in it. The lungs on the other side were in no bad condition. The heart was very flaccid and large: the right ventricle and vena cava had a small polypus in them. The vena pulmonaris was exceedingly dilated next the basis of the heart. The left ventricle of the heart had a small polypus, and had a great quantity of grumous blood. The great artery was very thin, and appeared not a little extended, and had some cartilaginous bodies interspersed in its membranes.

In the head; the dura mater was found inseparable from the cranium in its upper part. A polypus was drawn out of the upper great vein of the brain, called sinus falcis superior. The carotid arteries were very thin, and much larger than they ought to be, before they entered the substance of the brain. In short, all the blood-vessels, which I examined, were very much dilated, and seemed to be charged with as much wind as blood.

An Account of a Hail Storm, near Rotherham in Yorkshire, June 7, 1711. By Mr. Ra. Thoresby, F.R.S. N° 335, p. 514.

The storm of hail, accompanied with terrible thunder and lightning, happened the 7th of June last (1711): it began about Rotherham, where it burnt a noted tree. About 1 o'clock it reached Wentworth Woodhouse. The hailstones were from 3 to 5 inches in circumference, and some say larger, which killed several pigeons; but the chief damage done here was to the glass windows. In Washfield, about two miles from thence, it did vast damage. Some part of it escaped, and the barley received no damage; but the generality of the wheat was cut off, about half a yard from the ground, and the rye about two feet. The stubble, though green at first, turned white, so that it looked like a field newly shorn. Great quantities of twigs and small boughs were beaten off the trees, which being of less moment are omitted; but the damage in the corn was severe on the poorer sort of inhabitants.

Microscopical Observations on the Contexture of the Skin of Elephants. By Mr. Leuwenhoeck, F.R.S. N° 36, p. 518.

On observing the roughness of the skin of an elephant, and being told by the keeper that it fell off every year, I prevailed on him to scrape off a little of the said skin on a paper. On viewing the particles, I always imagined that the most part of it was a protruded matter, which had not nourishment enough to turn it all into hair; and that what became hair was very short and thin, in proportion to the size of so large a body; and the hair which is on the tail of the elephant, is much thicker than that on the other parts of the body. But as I more nicely viewed the scraped off particles of the skin, I discovered in some of them short hairs, the roots of which were sticking outwards in that part which is joined to the skin.

Observations of the Eclipse of the Moon, on Jan. 12, 1711-12. By the Rev. Wm. Derham, F. R. S. In a Letter to Rich. Waller, Esq. R. S. Secr. N° 336, p. 522.

Upminster, Jan. 14, 171½. Saturday evening being clear, I had a good opportunity of observing the lunar eclipse. The times are very accurate, and the observations made with an excellent six-foot telescope, as follow:

6ʰ 15ᵐ A duskishness on the N. east side of the moon.
6 36 A thick penumbra on the moon.
6 37 The penumbra so dense, that it may be taken for the beginning of the eclipse.
6 39 The eclipse undoubtedly is begun.
6 41 The shadow so dark, that it nearly hid the moon's N. easterly limb.
7 21 Moon's diameter by the micrometer, equal to 31′ 25″.
7 25 The distance of the shadow from the opposite luminous limb of the moon, was 1025 parts of the micrometer, equal to 20 minutes.
8 31 End of the eclipse is very near.
8 32 End of the eclipse.
8 32¾ The eclipse is undoubtedly ended.
8 36 A penumbra is left.

Further Observations relating to the Antiquities and Natural History of Ireland. By Mr. Edward Lhwyd. Dated Pensance, Cornwall, Aug. 25, 1700. N° 336, p. 524.

Ireland affords no great variety of antiquities. I have in divers parts of the kingdom picked up about 20 or 30 Irish MSS. on parchment: but the ignorance of their critics is such, that though I consulted the chief of them, as O'Flaherty, author of the Ogygia, and several others, they could scarcely interpret one page of all my MSS.; and this is occasioned by the want of a dictionary, which it seems none of their nation ever took the trouble to compose. What I most value among them, are their old laws, which might give some light to the curious, as to many of their national customs, and some of their old poems: but all are of use to any that would compose a dictionary of their language; which was anciently, considering the narrowness of their knowledge as to arts and sciences, doubtless very copious.

I saw no coins found there, besides the Roman gold coin of Valentinian, jun. formerly mentioned; several of our old English since the conquest; and one cast brass piece, inscribed with Runic letters, which I take to have been a Danish amulet. Several of our old British monuments, called Kaer, Karn, Cromlech, &c. we met with; and find that they distinguish them by the same names. What were peculiar to themselves, were their high round towers for belfrys; their round entrenchments, commonly called danes rathes; and the elf-arrow heads of flint.

About Sligo and Bali Shany we had good success as to figured stones; where we met with variety of astropodia and astrorrhizæ, or modioli, not yet figured or described, with other curiosities of that kind. In the same neighbourhood, on the mountains of Ben Bulben and Ben Buishgen, we met with a number of the rare mountain plants of England and Wales, and three or four not yet discovered in Britain. Mr. Heaton's chamædrys alpina is a common plant on those hills, as also on divers other mountains and heathy grounds in Connaught and Munster.

In the isle of Aran (near Galloway) we found plenty of the adianthum verum, and a sort of matted campion with a white flower. In most of the mountains of Galloway and Mayo grows an elegant sort of heath, bearing large thyme-leaves, a spike of fair purple flowers like some campanulæ,* and viscous stalks. I know not whether it be any thing related to the cisti ladaniferæ. In the same places pinguicula flore carneo minore is a common plant, and a sort of ros solis, which I take to be undescribed.

Sedum serratum foliis pediculis oblongis insidentibus is exceedingly common on all the mountainous tracts of Mayo, Galloway, and Keri. On the mountains of Keri, sanicula guttata grows in abundance; together with some other rare plants, as the arbutus, cotyledon hirsuta, cirsium humile montanum cynoglossi folio polyanthemum R. Syn. Alchemilla alpina pentaphyllos; sanicula aizoides inter guttatam et sedum serratum ambigens; veronica procumbens maxima, an N. D? &c.

Pentaphylloides fruticosa† we found plentifully among lime-stone rocks, on the banks of Loch Crib, in the county of Galloway; and Dr. Merret's vaccinia rubra foliis myrtinis crispis, a very beautiful plant, we found to be no rarity in this kingdom.

* Andromeda polifolia. Linn. † Potentilla fruticosa. Linn.

Extract of a Letter from the late Mr. Edw. Lhwyd to Dr. Tancred Robinson; giving an Account of some uncommon Plants growing about Pensance and St. Ives in Cornwall. Dated Pensance, Sept. 22, 1700. N° 336, p. 527.

I have hitherto met with no birds or fish here, which I consider as undescribed: only two or three stellæ, and some other exanguia marina have occurred, which I have not seen before on our British coasts. We have also met with the capillus veneris verus in abundance on the sea clifts about St. Ives. 2. Dr. Sherard's scrophularia scorodoniæ folio. 3. Blattaria lutea; an lutea minor Park.? But the leaves of ours are not jagged: also all the plants mentioned by Mr. Ray to grow here; excepting the gnaphalium marinum, which should grow near this town; and two or three more, which being at some distance, we have not looked for. We have also found some fuci, which perhaps may be new: and I am told the fishermen sometimes take up the corallina marina reticulata by their hooks; but I have not yet seen one of them.

Account of a Storm of Thunder and Lightning at Samford-Courtney in Devonshire, Oct. 7, 1711. By John Chamberlayne, Esq. F. R. S. N° 336, p. 528.

In the parish of Samford-Courtney, near Oakhampton in Devon, on the 7th of October, about 3 or 4 o'clock in the afternoon, there was a great darkness as the minister was catechising the children, so that he could hardly see with spectacles: several people being in the church porch, of a sudden a great fire-ball fell in among them, and threw them some one way, some another; but no one was hurt. The ringers in the belfry said, they never knew the bells go so heavy, and were obliged to leave off: and being very weary, and looking out of the belfry into the church, they saw 4 fire-balls a little larger than a man's fist, which of a sudden broke to pieces; so that the church was full of fire and smoke. One man received a blow in the neck, which caused him to bleed both at nose and mouth. He says, that the fire and smoke went up into the tower, and broke a large beam on which one of the bells hung, and the gudgeon breaking, the bell fell on the floor. It likewise carried away one of the pinacles of the tower next the town, and threw some of the stones near a barn door at a pretty distance from the church, and has done some damage to the barn at one end. The chimney of the house was removed in such a manner by the thunder and lightning, that the people were surprised that it continued to stand.

Microscopical Observations on Muscles, and the manner of their Production. In a Letter from Mr. Leuwenhoeck, F. R. S. N° 336, p. 529.

I have long observed that the shell fish, called muscles, lay their eggs on the outside of their shells; and that so regularly one by an other, that they may be compared to a string or band. These eggs, or imperfect muscles, lying on the shells, continually increase in strength, till at last they come to be perfect muscles; but then you may see remaining on the shells a part of the egg-shell, which sticks fast to the said shell, till the skin or membrane, with which the muscles are encompassed, is changed.

In August 1710, I procured some muscles, and dissected them according to the best of my power; and found within the shells on both sides of the muscle, against the parts of the fish, very thin and weak membranes; which I have taken often out of the muscles, and placing them before a microscope, I have seen a vast number of motions in the said membranes. And I observed those appearances, not only in one, but all the muscles I dissected, especially in such as had not been long out of the water.

I placed a small particle of the membrane before a microscope: and though that particle was not a hundredth part so large as a common grain of sand, yet I discovered an incredible number of motions in it: and those motions were not only circular, but I could discover at least 50 slender particles in the length; which appeared through the microscope equal to 6 hairs breadth, and the thickness of one hair, as they appear to the naked eye. And these motions continued so long, till our eyes were weary of viewing them; but as soon as the moisture of the muscle, in which the said particle lay, was evaporated, the motions were ended. The minute particles that lay round about, were put into such a motion, that one would have taken them for little living creatures.

I have endeavoured for several years to discover the ovarium, or egg-nest, in the muscles; and now lately, Nov. the 18th, there was a present made me of exceedingly fine and well tasted muscles; some of which had placed their eggs in part on their shells. This occasioned me to dissect several muscles again; when at last I discovered the ovarium of the muscle, and in a great many of them could see the unborn muscles as perfect as we could see them with our naked eyes; lying with their sharp end fastened to the string, or vessels, by which they receive their nourishment.

A few days after some muscles were brought me, which were very lean, like some of the first muscles; and among them I observed about 25 that had not yet placed their eggs on their shells, but most of them were still shut up in the ovarium, from which I took a great number of eggs; which even through the microscope appeared so small, that I could but just discover the figure of them. In some other muscles the eggs were larger: and though the first unborn muscles, which I judged to be so perfect, as to be ready to be placed on the shell, were of a brownish colour, mixed with little specks, yet the very small eggs were clear and transparent; but in the larger, one might discover some of the parts of the little fish within. I took out of the ovarium of one of the muscles some particles, as large as an unborn muscle, and which were somewhat longer than broad; being very white, and some of them of a particular figure: at which I was amazed, and began to consider whether these might not be some of those creatures, which are so prejudicial to such as eat muscles.

Turning my thoughts to the consideration of the excrements, or food of the muscle as it lay in the guts, I observed a gut, which had its beginning, or rather its ending, in the thinnest part of the fish, where the shell opens when the muscle is in the water; and which gut was very near the extreme part of the fish, and ran into that part where the stomach is. I have often separated this gut from the fish; and squeezing the matter out of it, I always observed that the earthy matter, which was in the gut, was mingled with a great number of grains of sand of different magnitudes; so that I judged there was above 1000 grains of sand in one gut; some of which were as large as the sand on the sea shore; but others again so small, that 1000 of them were not equal to one of the other large grains. I took a second gut out of the muscle, which lay deeper in it; and therein I also discovered as great a quantity of sand. I have likewise squeezed the matter out of the guts of some muscles, in which I found but few grains of sand.

In the matter taken out of the stomach, I observed several minute animalcula swimming; and had not till now perceived the stomach so full of food, nor of so thin a substance as this was. I have observed, that in all the muscles I have opened, there were ovaria or egg-nests; and I have taken the eggs out of them: and of those opened latest, I observed that the eggs were larger than before. So that I concluded that all muscles brought forth young ones; and that the eggs that were found on the outside of the shell were not all laid by the muscle itself; but that other muscles did also lay their eggs on each others shells; and accordingly I have observed some shells that were covered all over with eggs.

During the motion of those parts above-mentioned, which I shall here call the beard of the muscle, I have observed several times two or three animalcula swimming; and the small parts that lay round about were put into such a motion, that one would be apt also to take them for animalcula. And in my judgment, after several observations, most if not all, of the shell fish, bring forth young without the help of males: so likewise I believe it is in oysters; and I am also of opinion, that that exceeding number of small particles, which I discovered in them, and which I took for animalcula, are merely the parts put into a violent motion.

An Account of what appeared on opening the Body of Mr. St. John, who died of an Asthma, July the 2d, 1705, aged 72. By the late Mr. William Cowper, F. R. S. N° 336, p. 534.

It was remarkable, before the body was removed from the bed, whereon it lay some hours after death, that the blister in the neck had discharged not less than a quart or 3 pints of serum, before I began the dissection.

In the abdomen there was a small quantity of water, such as is usual in those who die of chronical diseases. The parts here were in a natural state, excepting the kidneys, of which the right was very much contracted, even to a third part of its natural size, and had two large hydatides, or bladders of clear water, on its surface. The left kidney was also lessened, but not so much as the right; its surface was also unequal, but had no hydatides on it. The ureter of this left kidney was very much contorted, at its rise from the pelvis, where its sides were petrified; insomuch that its canal was rendered almost impervious for the passage of the urine.

Nothing was found in the bladder of urine but divers stones of unusual figures, as if they had been pieces of a large stone broken to bits, in the centre of which a nucleus had been lodged. The gall-bladder was filled with gall-stones.

The stomach, which he complained of, i. e. in want of appetite, was no otherwise disordered; but a little redder, having more blood in its vessels than is usual; its muscular fibres being stronger than generally on the stomachs of healthful persons.

The cavity of the thorax, or chest, was filled with water on both sides, so that the lungs were not above a third part of their natural magnitude. The pleura, or membrane that lines the two cavities of the thorax, was very much thickened by the serum of water; from whence it descended by the muscles of the back into his legs. The valves of the left ventricle of the heart were petrified in several places, especially those called mitrales. Some stony bodies were found on the bronchia, at and near their rise from the lungs.

Concerning large Stones voided per Urethram. By Mr. Ra. Thoresby, F. R. S. N° 336, p. 536.

One Joshua Spurrit, on the Quarry Hill, near Leeds, having been long afflicted with the stone, voided 3 stones, which are of a great size to pass the penis; and 5 more he could not get rid of without the assistance of Mr. S. Pollard, an ingenious surgeon; who by an incision made way for them, as they came severally near the glans. Whenever one of these large stones broke out, there was a crack within his body, as if the sphincter muscle, or bladder itself, was rent. This patient being opened after death, there were found in the top of his bladder, which was contracted like a purse, two prodigious large stones, one especially which I measured, was rather more than 5½ inches one way, and 4 the other; it weighed 2 ounces, wanting 3 drachms; the other seems lighter, and weighs but 1 drachm above an ounce. There were two very odd stones taken out of the right kidney, the left was wholly degenerated into a kind of mucilage; and between the neck of the bladder and the end of the penis, which was mortified thereby, were lodged no less than half a dozen stones. There was little moisture left in the bladder, the ureters being broken off, and almost wholly consumed.

*Concerning the Ascent of Water between two Glass Planes. By Mr. Brook Taylor.** N° 336, p. 538.

I fastened together two pieces of glass, as flat as I could get: so that they were inclined in an angle of about 2½ degrees. I then set them in water, with the contiguous edges perpendicular. The upper part of the water, by rising between them, made the hyperbola, fig. 10, pl. 17, as I copied it from the glass. I have examined it as well as I can, and it seems to approach very near to the common hyperbola. But my apparatus was not nice enough to discover this exactly. The perpendicular assymptote was exactly determined by the edge of the glass; but the horizontal one I could not so well discover.

Bifrons, near Canterbury, June 25, 1712.

* Brook Taylor, a very able mathematician, and secretary of the Royal Society, was born at Edmonton, in Middlesex, in 1685. In 1701 he entered St. John's College, Cambridge; and in 1708, wrote his tract on the Centre of Oscillation. In 1709 he took the degree of Bachelor of Laws, and in 1712 was elected into the Royal Society, of which he was chosen secretary two years after, and the same year took his degree of LL.D. Dr. Taylor had many excellent papers on philosophy and mathematics, inserted in the Philos. Trans. from vol. 27 to vol. 32, inclusively; besides which, he published some other excellent works, viz. Methodus Incrementorum, in 4to. 1715, containing many excellent tracts, particularly a curious theorem on the manner of expressing a variable quantity by all the orders of its differentials or fluxions; also the problem of the vibrations of a tense cord, of which he gave the first solution. The same year also came out his Principles of Linear Perspective, first establishing the true practice of that art, on principles which have been ever since followed by all succeeding authors. Dr. Taylor was a profound and elegant mathematician of the old school of Newton, Jones, Cotes, &c. and one of the chief writers in the disputes with the Bernoullis and other eminent mathematicians on the Continent. But died at an early age, 46, in the year 1731.

Account of the Experiment on the Ascent of Water between two Glass Planes, in an Hyperbolic Figure. By Mr. Francis Hauksbee, F.R.S. N° 336, p. 539.

I took two glass planes, each somewhat more than 20 inches long, of the truest surfaces I could procure. These being held close together at one of their ends, the other ends were opened exactly to an angle of 20 minutes. In this form they were edgewise put into a trough of tinged water, which immediately rose between them in the form of fig. 12, pl. 17. At another time the planes were opened to an angle of 40 minutes: then the water appeared between them, as in the scheme, fig. 11. By these schemes the proportions of the power of attraction are in some measure evident to the eye; for there may be seen at the several distances, how many lines, which are 12ths of inches, the water is elevated, and the prodigious increase of them near the touching ends. I hope the tables are pretty accurate; for after many trials, I find the successes to be much the same, according to the different angles. This experiment was first made by Mr. Brook Taylor, as appears by his letter above.

A Table according to the Scheme of the Planes opened to an Angle of 40 minutes, in fig. 11.

Distances in Inches and Parts of Inches from the touching Ends.	Number of Lines elevated at the several Distances.
9	1
6	2
4¾	3
3	4¾
2½	6
2	7½
1½	10
1¼	12
1	15
0¾	19
0½	28
0¼	50

A Table according to the Scheme of the Planes opened to an Angle of 20 minutes, in fig. 12.

Distances in Inches and Parts of Inches from the touching Ends.	Number of Lines elevated at the several Distances.
13	1
9	2
7	3
6	3¾
5	5
4	6¾
3	9
2½	12
2	15½
1¾	18
1½	21½
1¼	27½
1	35
0¾	50
0½	76

A Description of the several Strata of Earth, Stone, Coal, &c. found in a Coal Pit at the West End of Dudley in Staffordshire. By Mr. Fettiplace Bellers, F. R. S. To which is added a Table of the Specific Gravity of each Stratum. By Mr. Fr. Hauksbee, F. R. S. N° 336 p. 541.

1, A yellowish clay, immediately under the turf; 2, a bluish clay; 3, a bluish hard clay, called by the miners clunch. This is one of the certain signs of coal. It has in it mineral plants; 4, a bluish soft clay; 5, a fine-grained grey stone; it lies next the former, and is found in some pits only; 6, a clay almost like the first, only whiter; 7, a hard grey rock: with something like the impressions of vegetables, but none distinct; 8, a blue clunch, like N° 3, with mineral plants in it; 8, +, this stratum, which is the same with N° 13, was not taken; 9, coal, called bench-coal; 10, coal, less black and shining than the former, called slipper-coal; 11, coal, more black and shining, called spin-coal; 12, a coal like cannal-coal, by the miners called stone-coal. These strata of coal have between each of them a bat, of about the thickness of a crown piece; 13, a black substance, called the dun-row-bat; 14, a hard grey iron ore, called the dun-row-iron-stone; 15, a bluish bat, in which the following iron-stone lies, called the white-row; 16, a hard blackish iron ore, lying in small nodules, having between them a white substance, and from thence by the miners called the white-row-grains, or iron-stone; 17, a hard grey iron ore, with some white spots in it, called the mid-row grains; 18, a black fissile substance, called the gublin-bat; 19, a hard blackish iron ore, with white spots in it, called the gublin-iron-stone; 20, a bat, in substance much like that of N° 18; 21, a hard grey iron ore, called the cannoc, or cannot-iron-stone; 22, a bat, somewhat harder than N° 20; 23, a dark, grey, hard iron ore, called the rubble-iron-stone; 24, the table bat, next under the rubble-iron-stone; 25, a coarse sort of coal, called the foot-coal; 26, a black, brittle, shining bat; 27, the heathen-coal; 28, a substance like a coarse coal, but by the miners called a bat; perhaps because it does not burn well; 29, the bench-coal; 30, a bat under the last, and is as low, viz. 188½ feet, as they generally dig, though there is a coarse coal under this.

Note, Those substances, which divide the strata of coals and iron ores from each other, are called bats by the miners; they are generally black, consisting of a matter peculiar to themselves, and are of a texture most like marl; though some of them are fissile, and others have a substance not unlike coal mixed with them.

A Table of the Thickness of each Stratum, and its Proportion to Water, or Specific Gravity.

No. of the strata.	Thickness of each stratum. ft.	inc.	Proportion to water.		or Specific gravity.
1	4	0	as 385	to 192	as 200 to 100
2	5	0	296	168	176
3	24	0	23	9	256
4	9	0	209	106	197
5	4	0	583	237	246
6	21	0	401	192	209
7	75	0	683	259	243
8	5	0	223	88	253
8+	1	0	..	.. ..	..
9	3	0	7	5	140
10	3	0	106	72	147
11	4	0	147	114	129
12	4	0	185	143	130
13	1	0	408	198	206
14	0	1	204	67	303
15	0	3	183	72	254

No. of the strata.	Thickn. of each stratum. ft.	inc.	Proportion to water.		or Specific gravity.
16	1	3	as 325	to 232	as 334 to 100
17	0	2	781	244	320
18	2	0	305	129	236
19	0	9	920	266	346
20	1	6	192	76	253
21	0	6	675	216½	313
22	1	0	428	165	290
23	0	6	828	231	358
24	2	0	333	153	218
25	1	0	198	154	128
26	6	0	238	141	169
27	6	0	298	236½	126
28	0	1	267	186	144
29	2	0	314	240	131
30	0	6	244	133	183

By which it is evident, that the gravities of the several strata are in no manner of order, but purely casual, as if mixed by chance.

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END OF VOLUME FIFTH.

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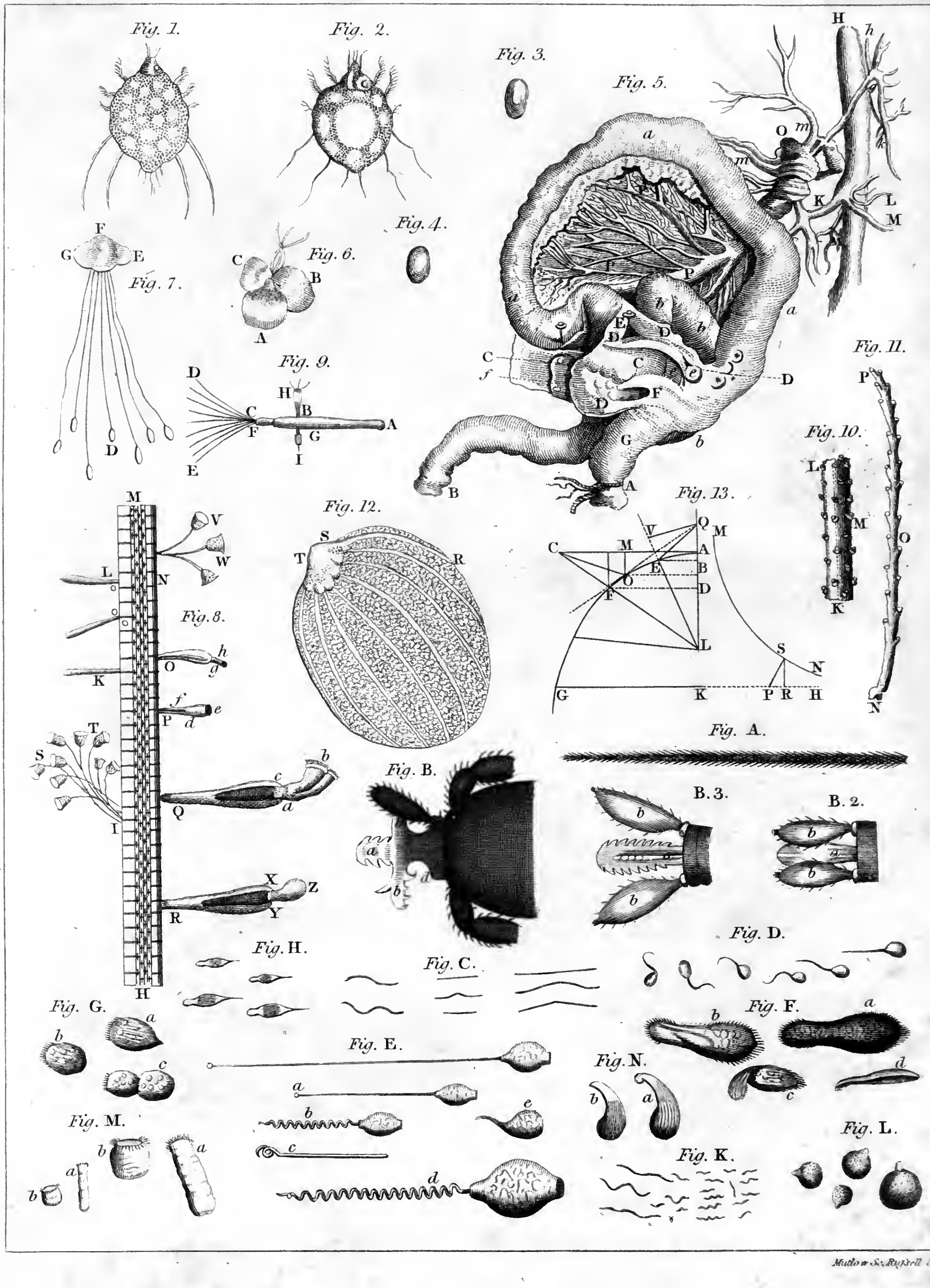

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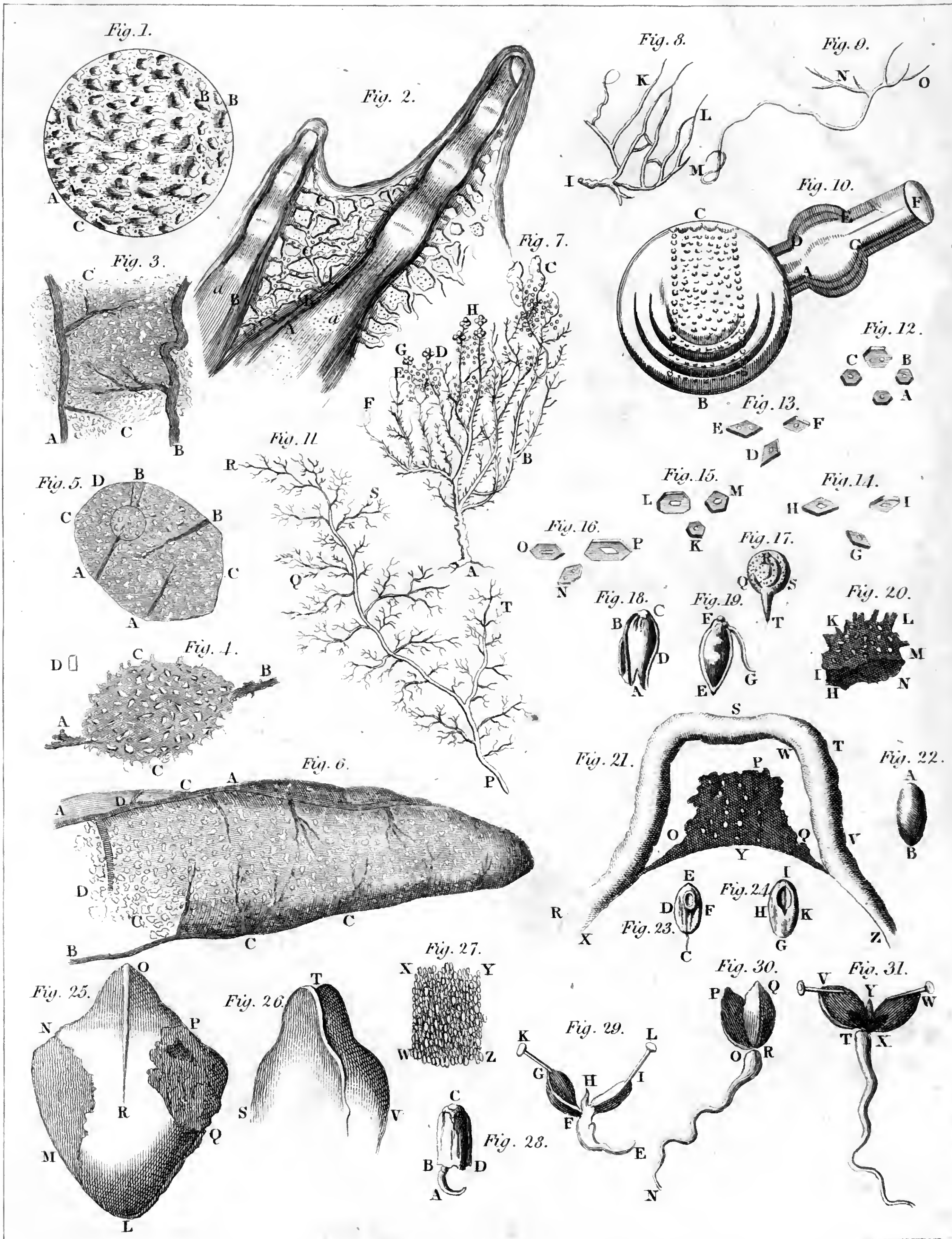

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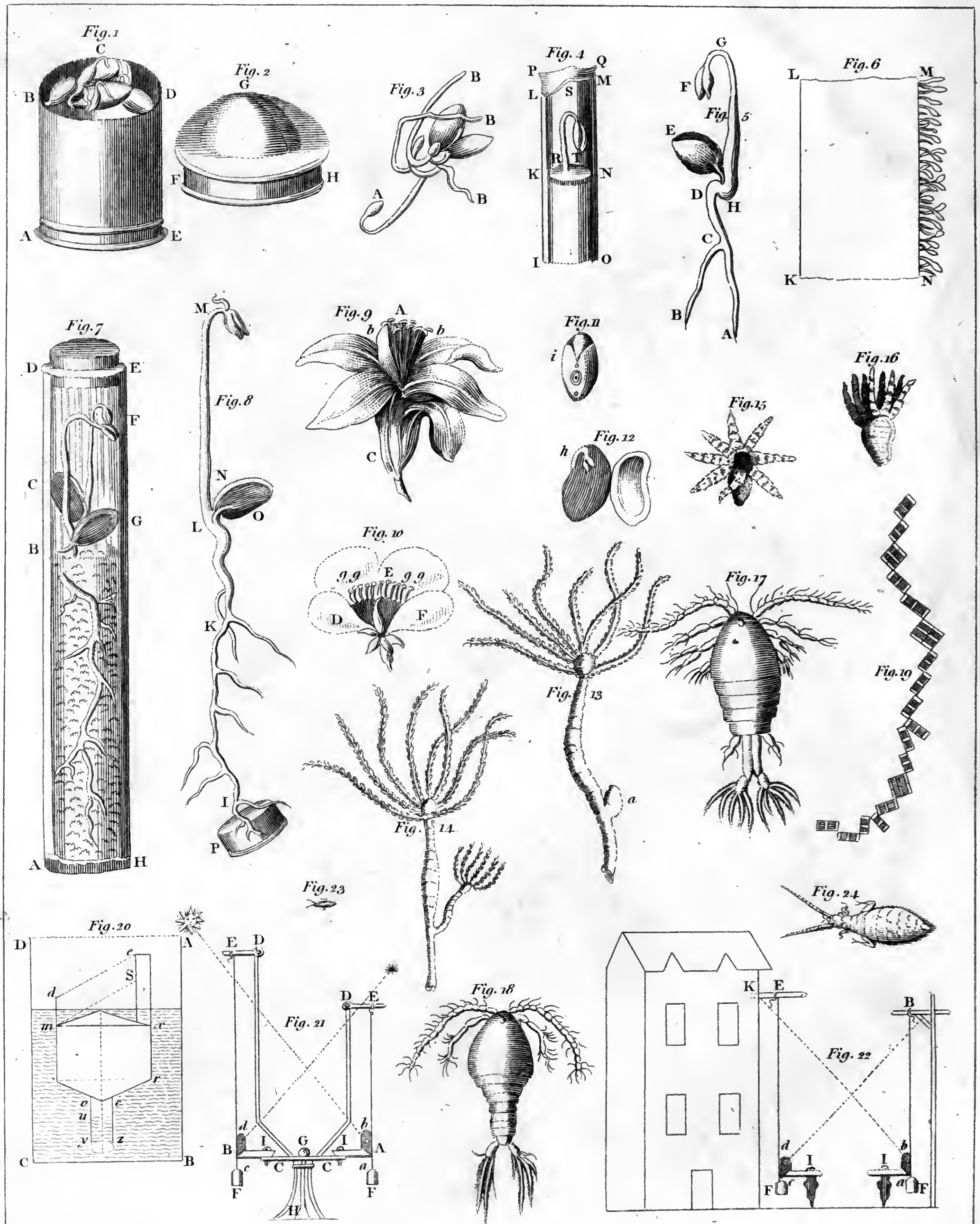

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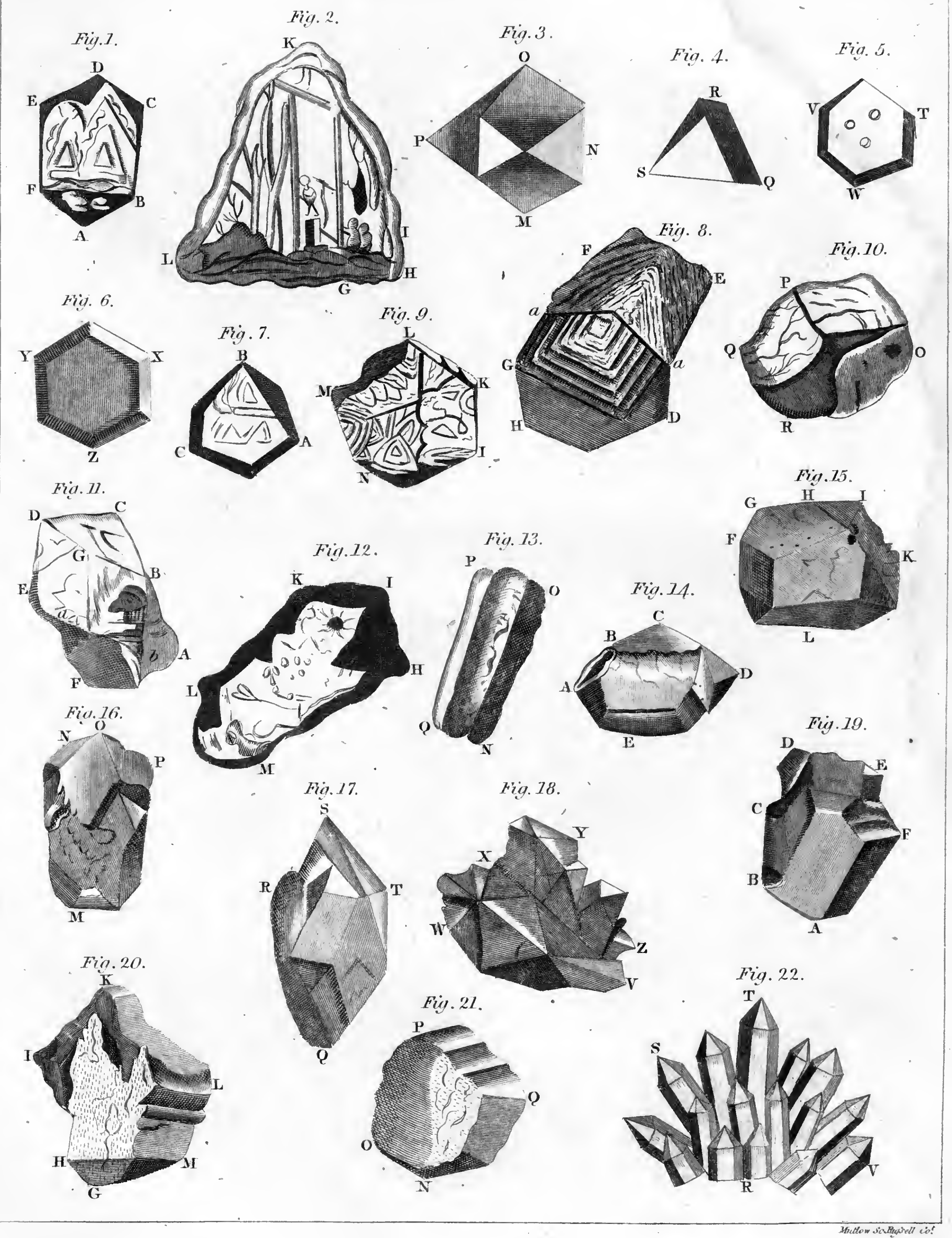

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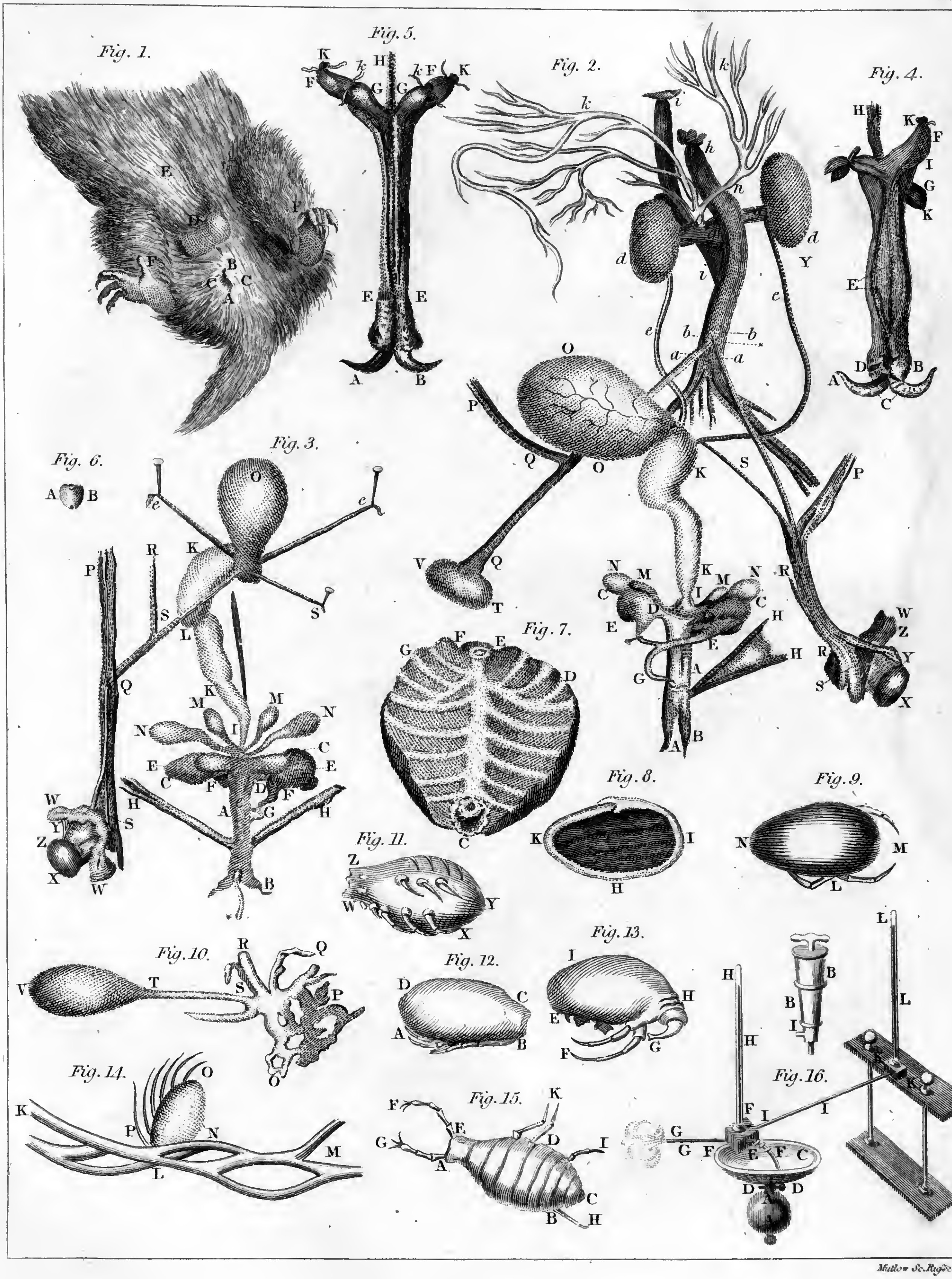

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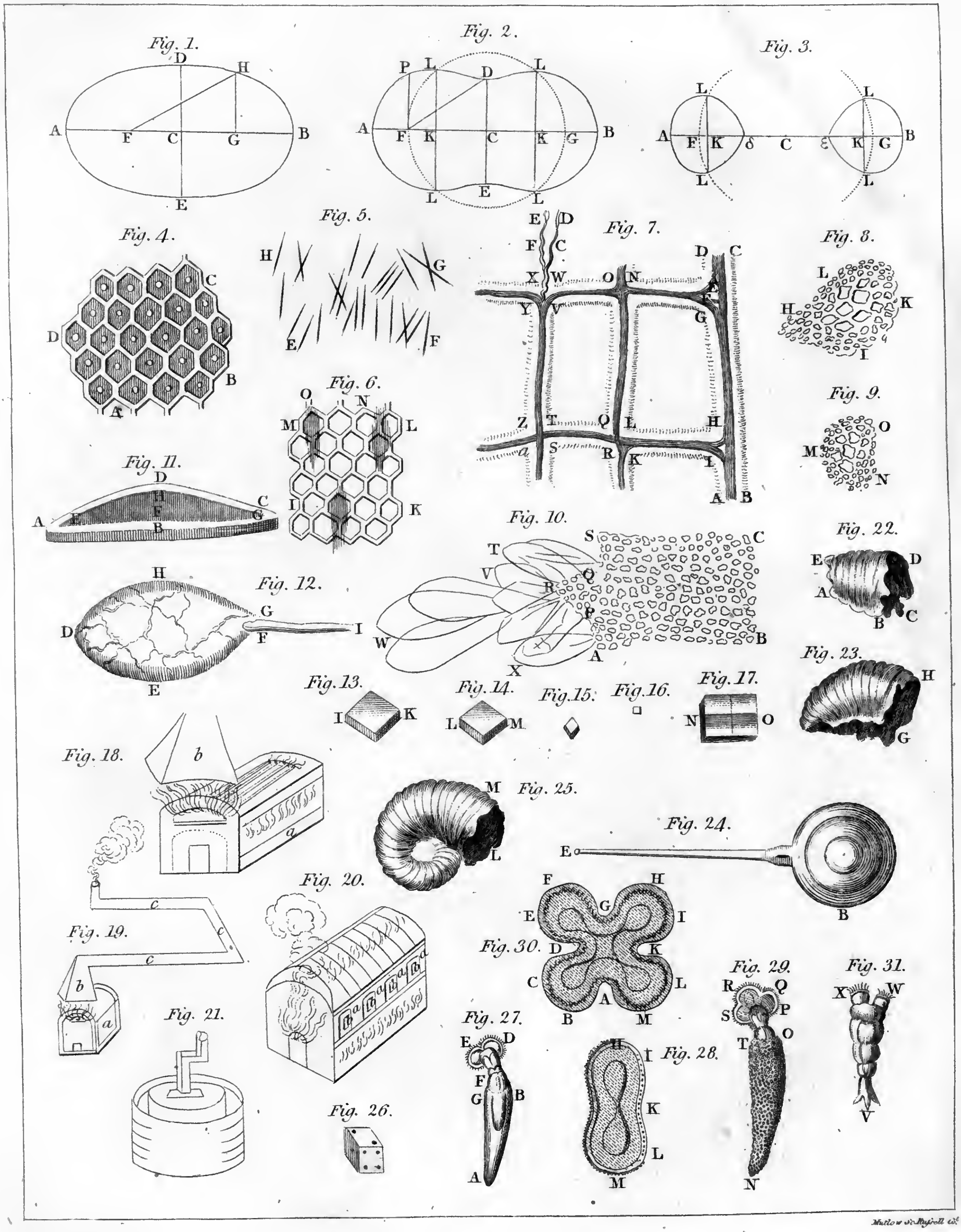

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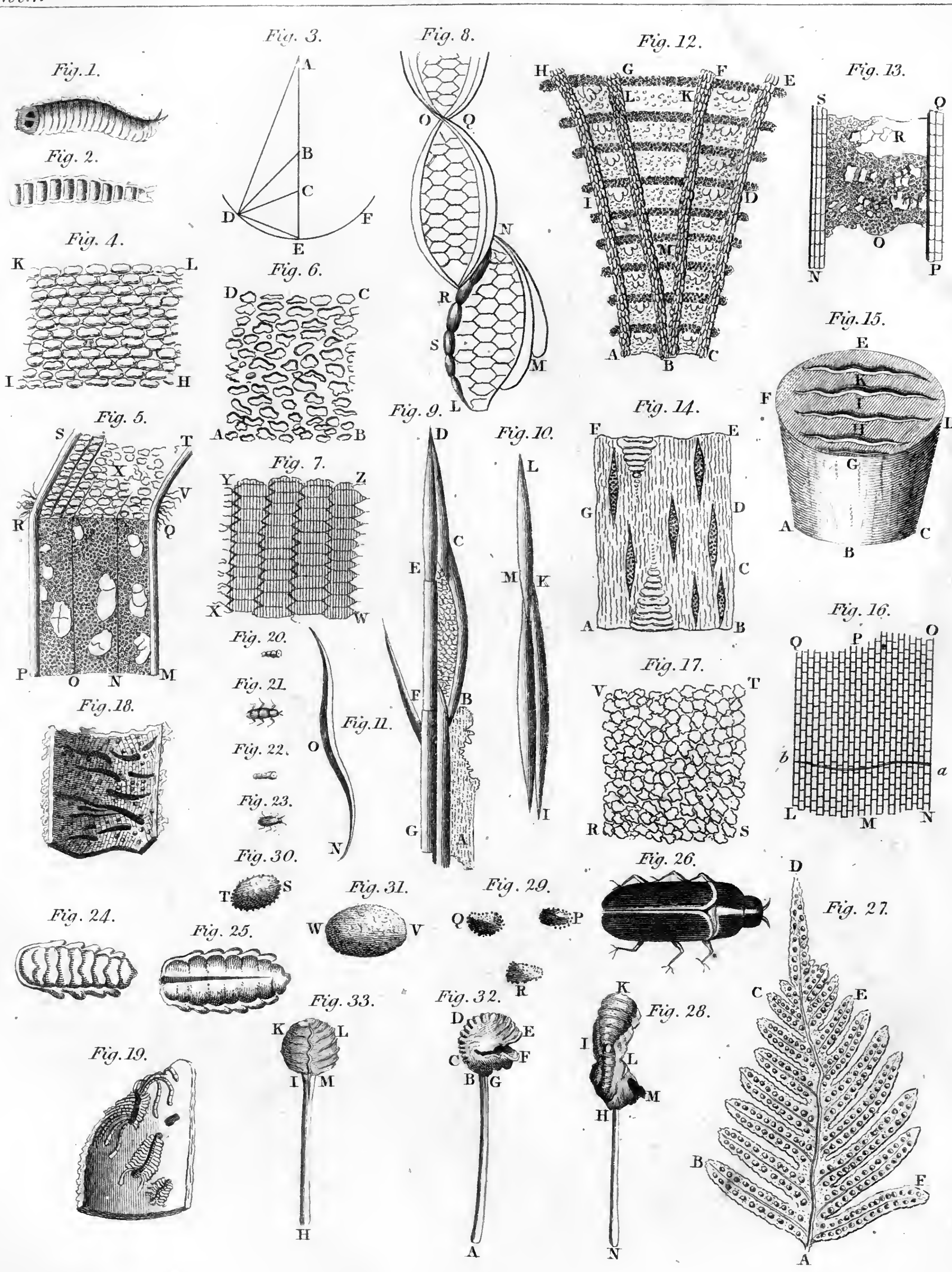

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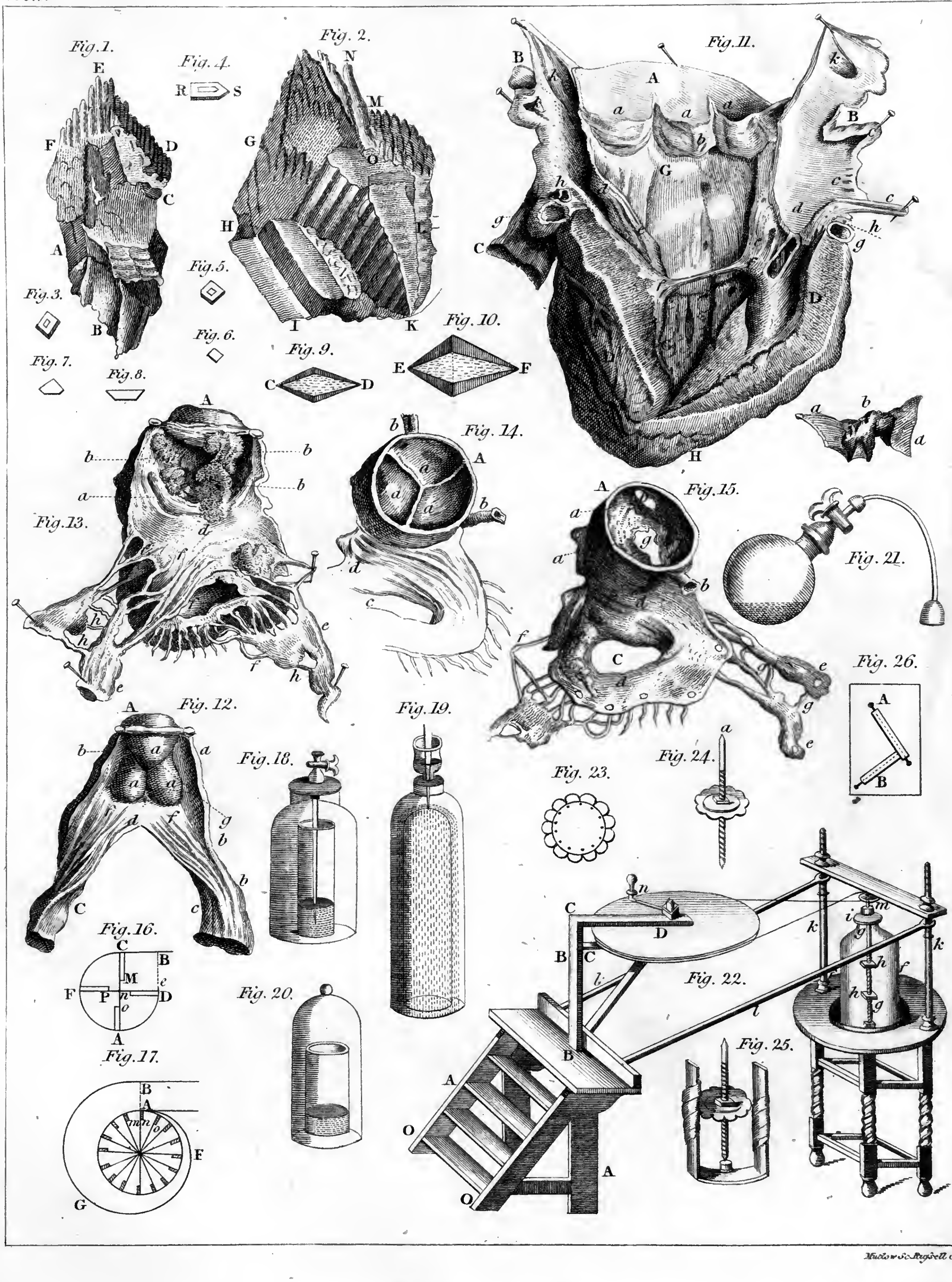

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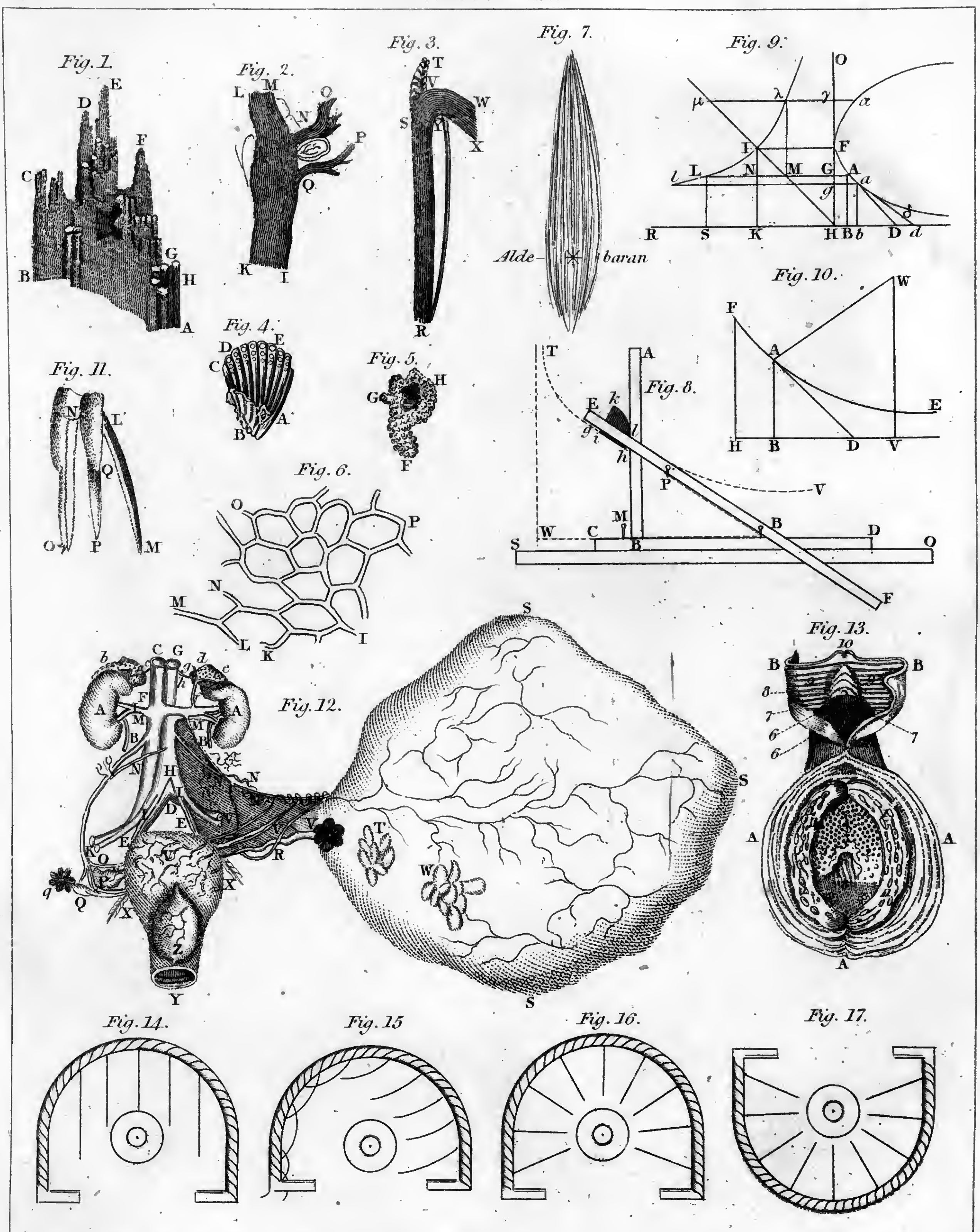

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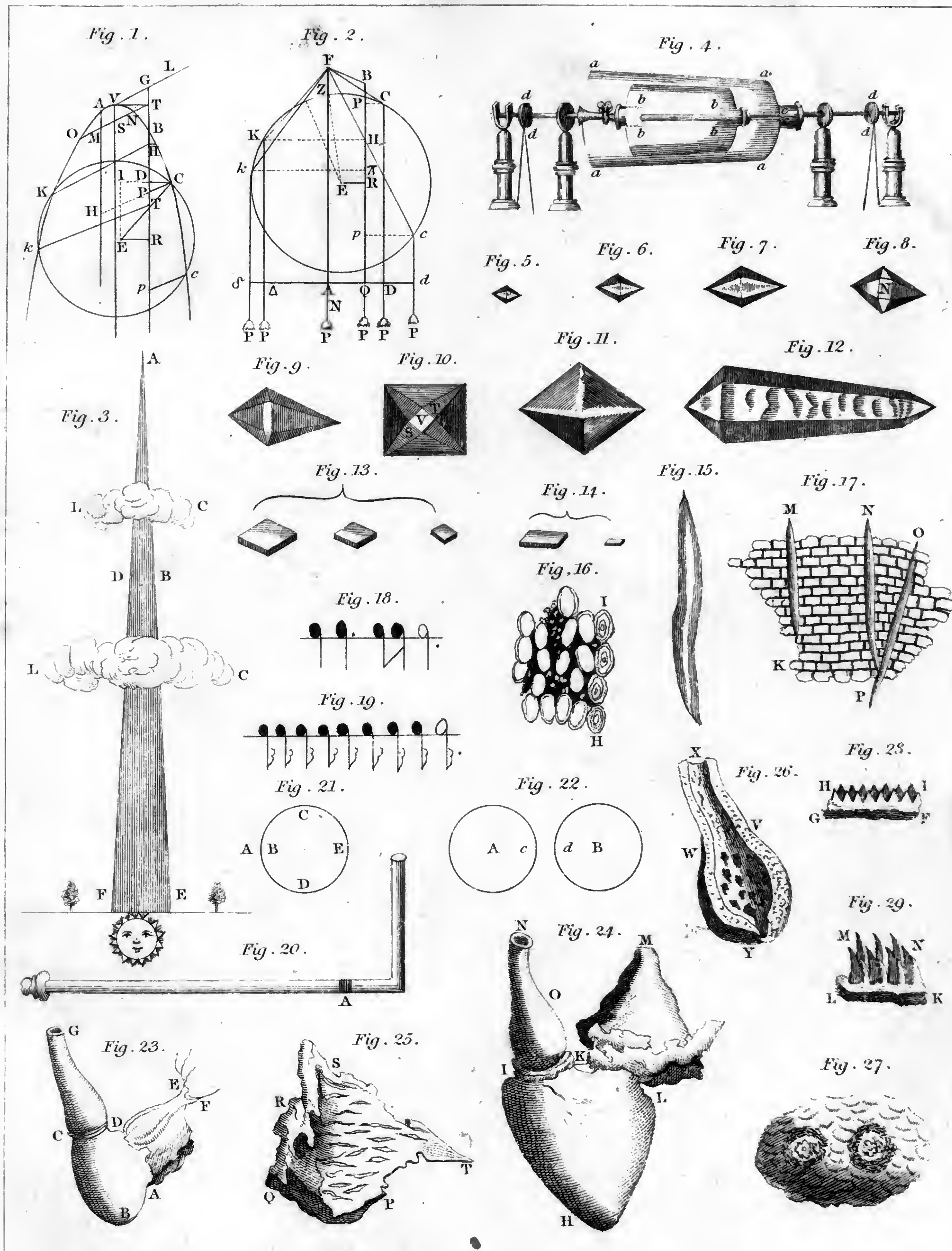

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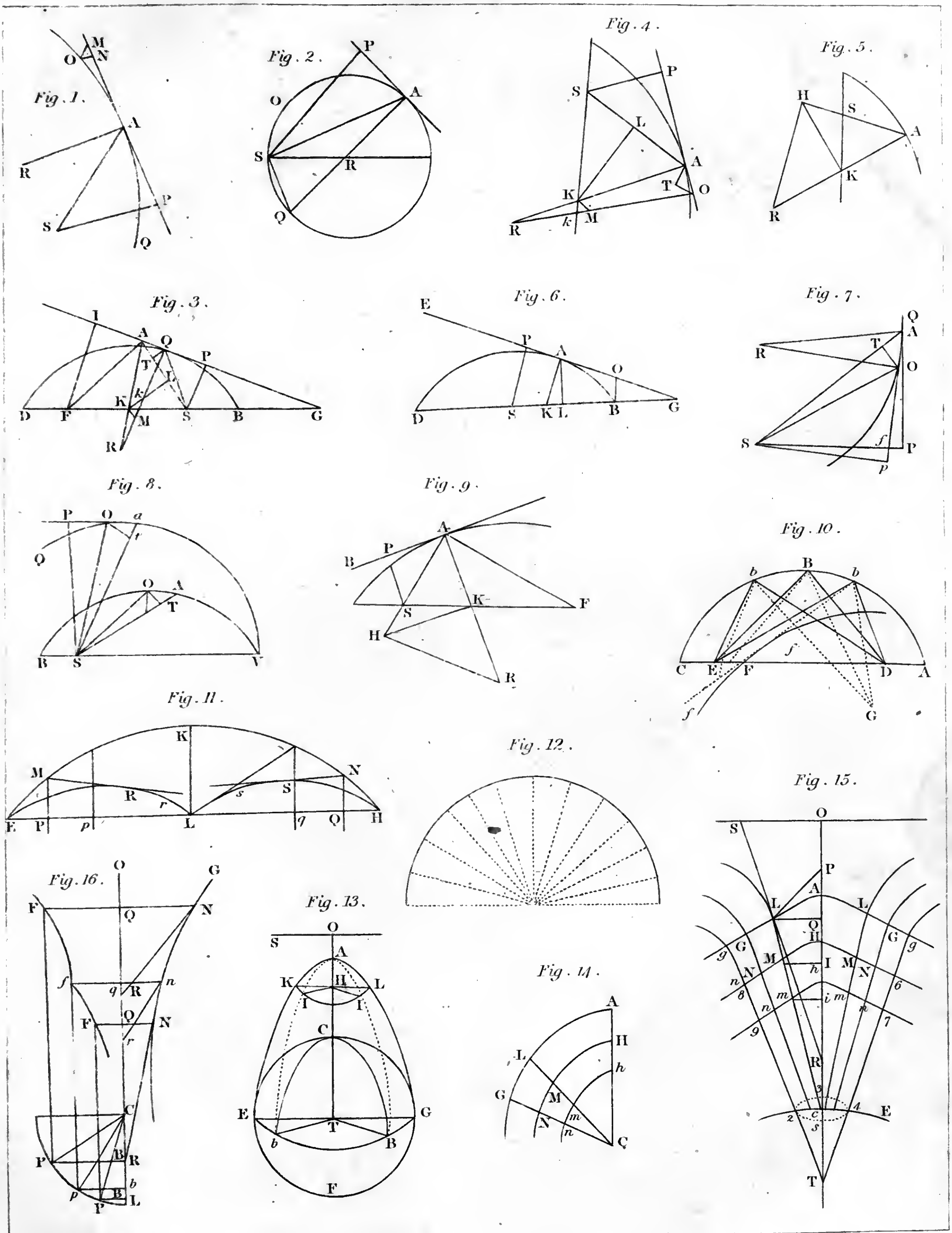

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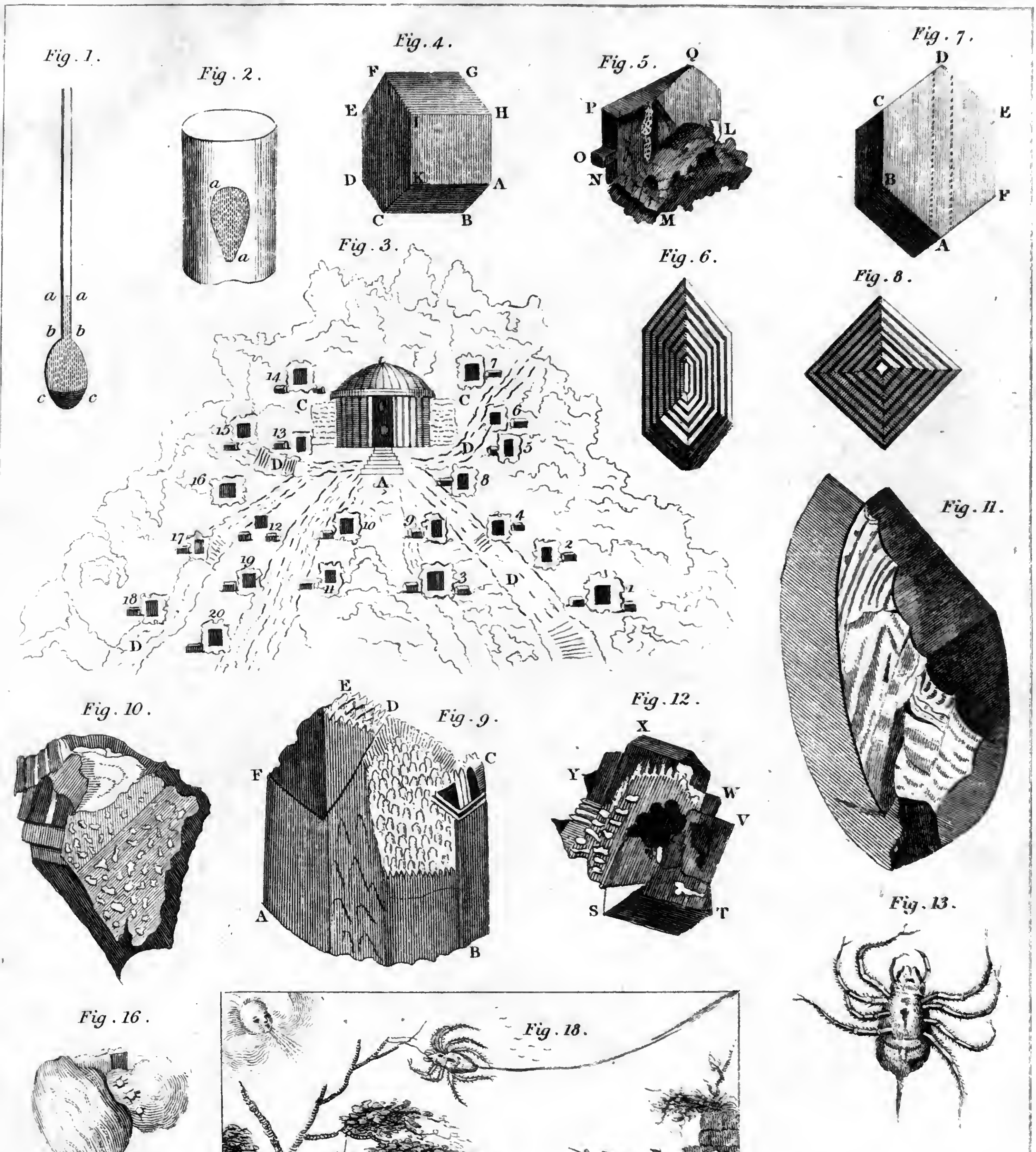

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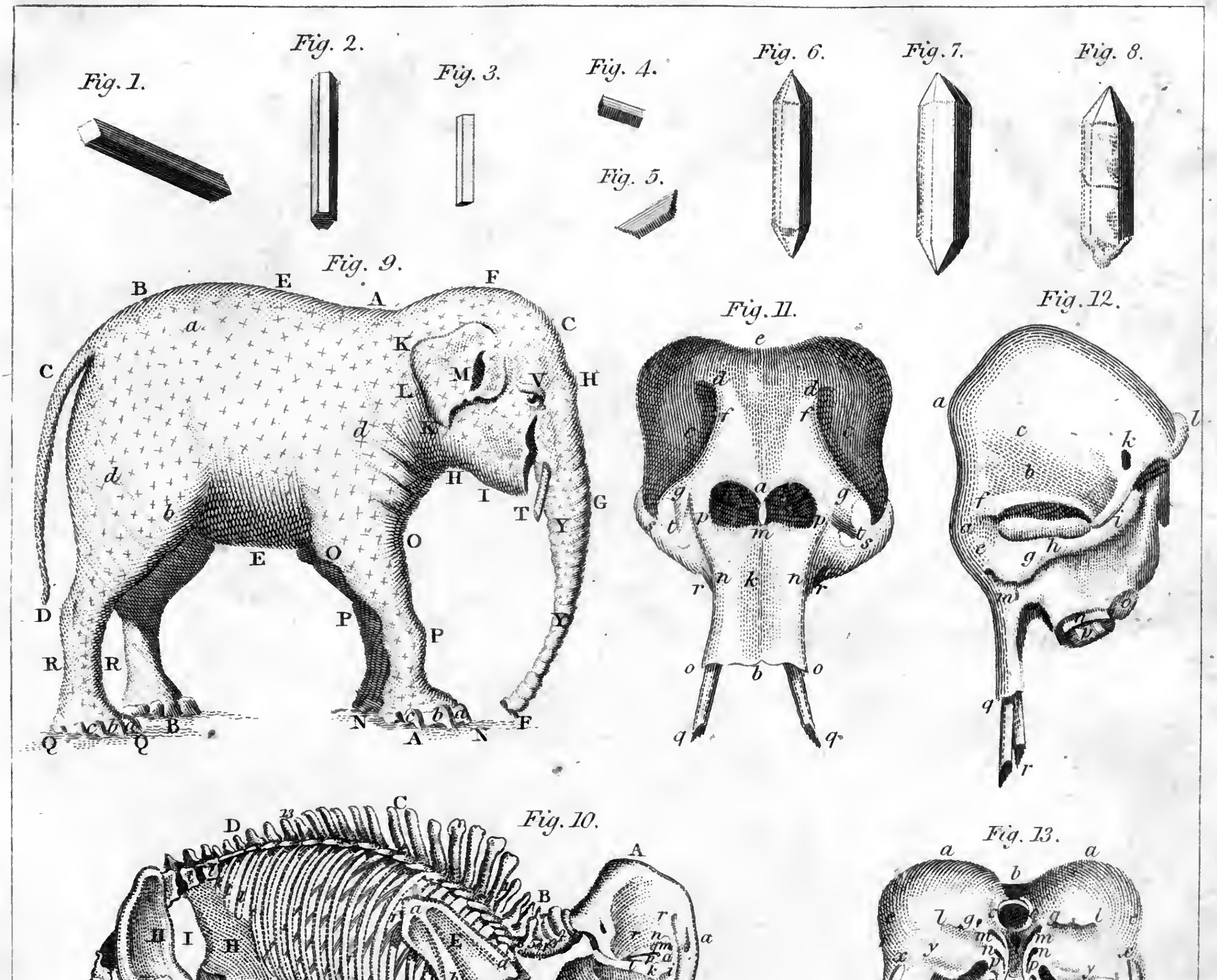

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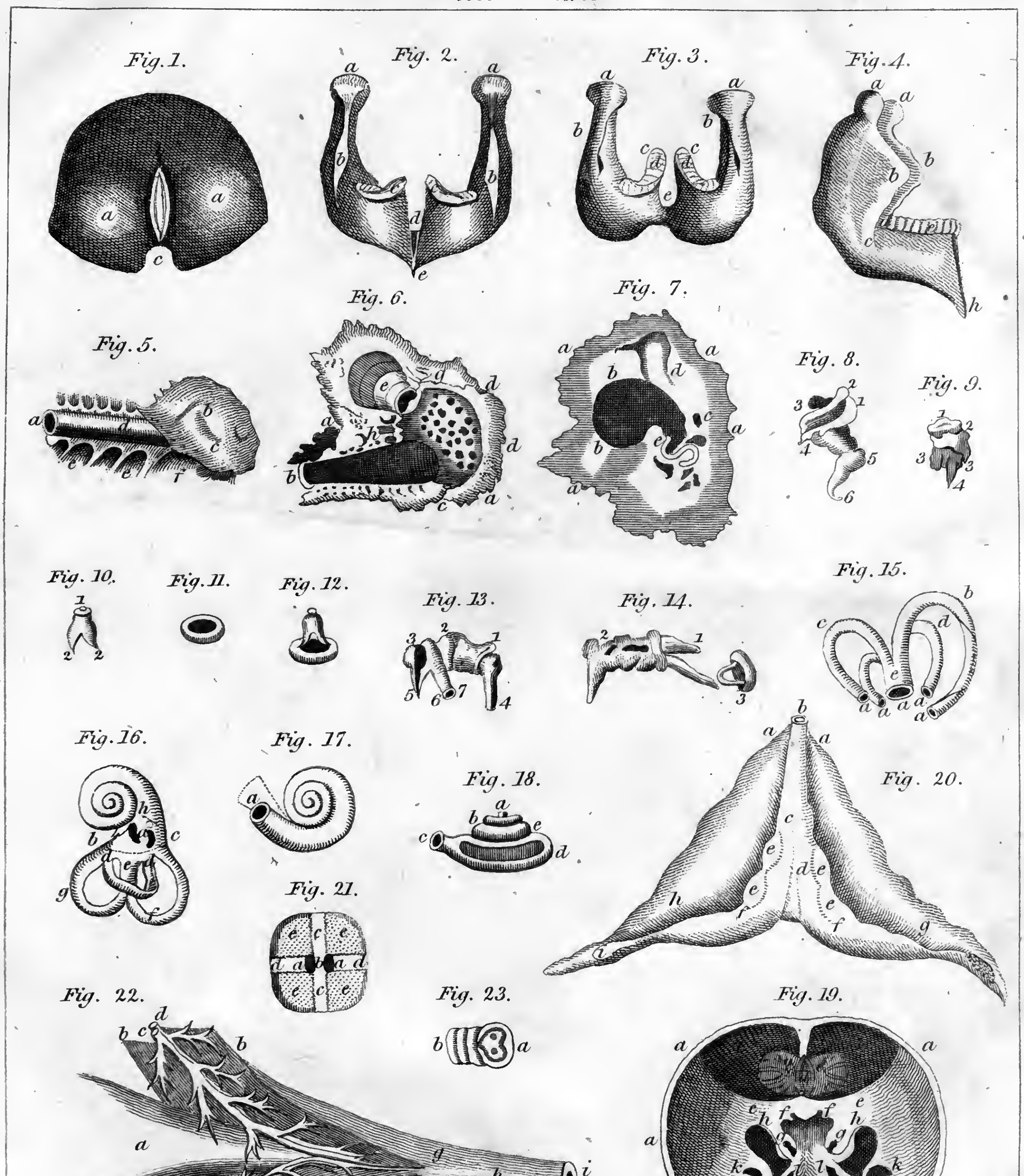

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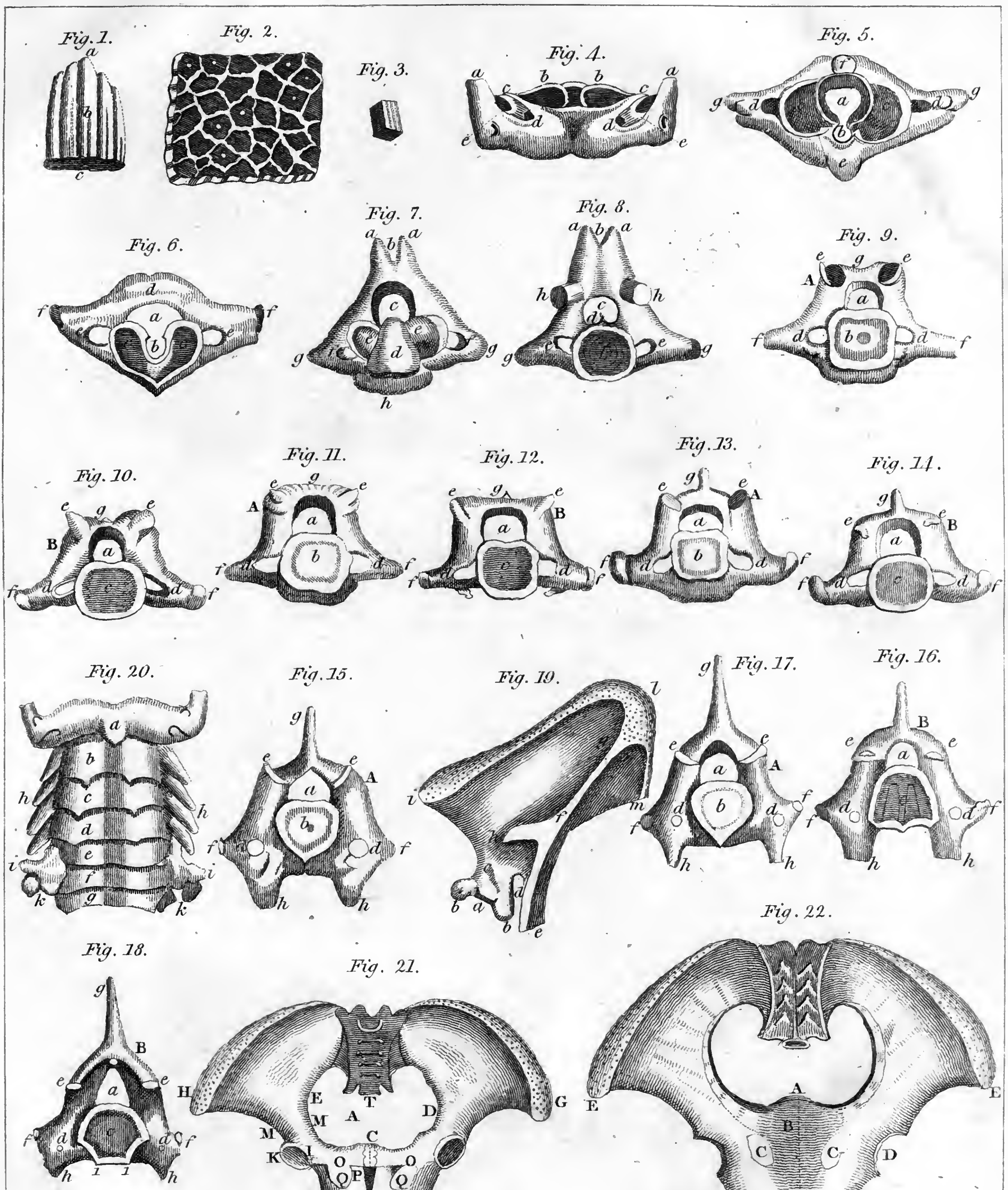

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Fig. 1.

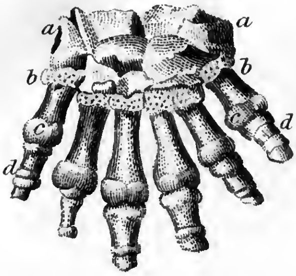
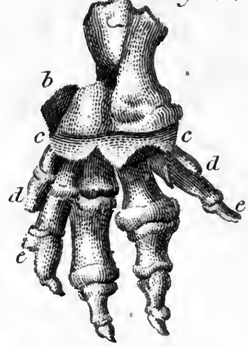


Fig. 2.



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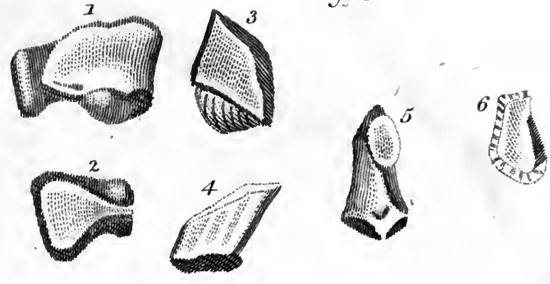

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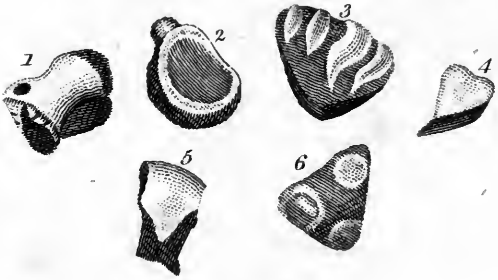

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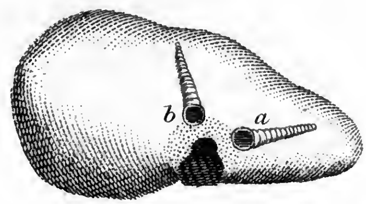

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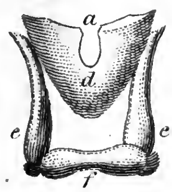

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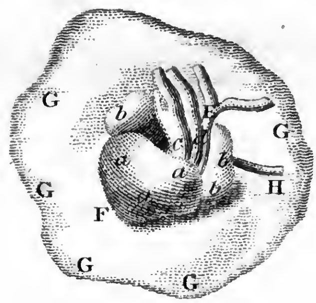

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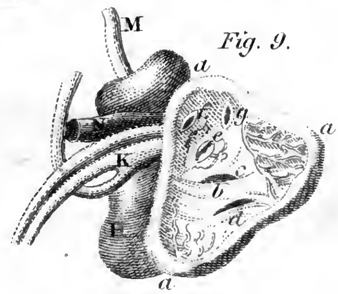

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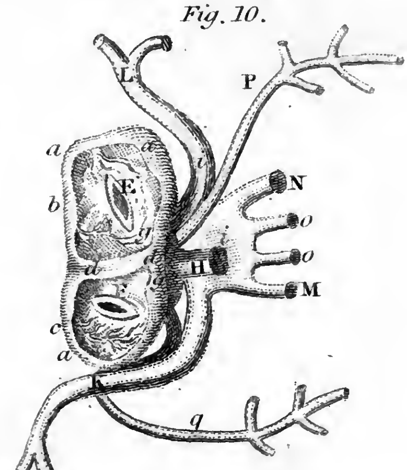

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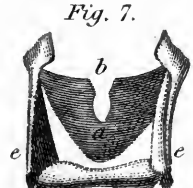

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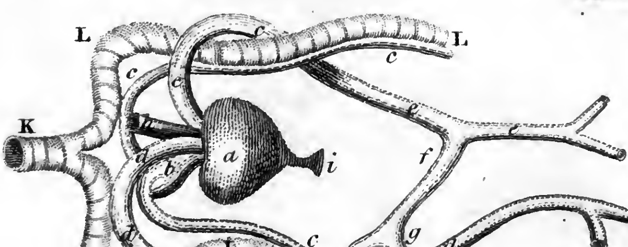

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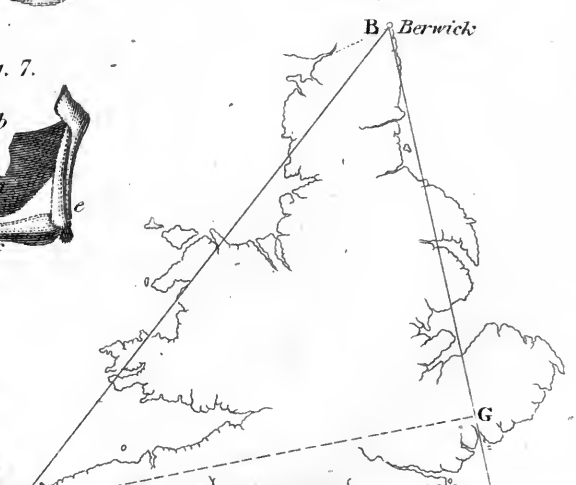

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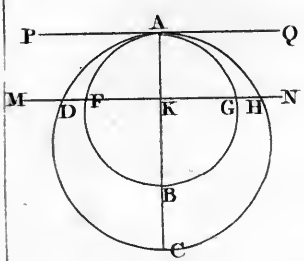
Fig. 1.

Fig. 6. Fig. 2. Fig. 7.

Fig. 4. Fig. 5.

Fig. 8.



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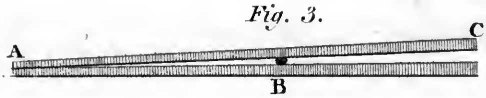
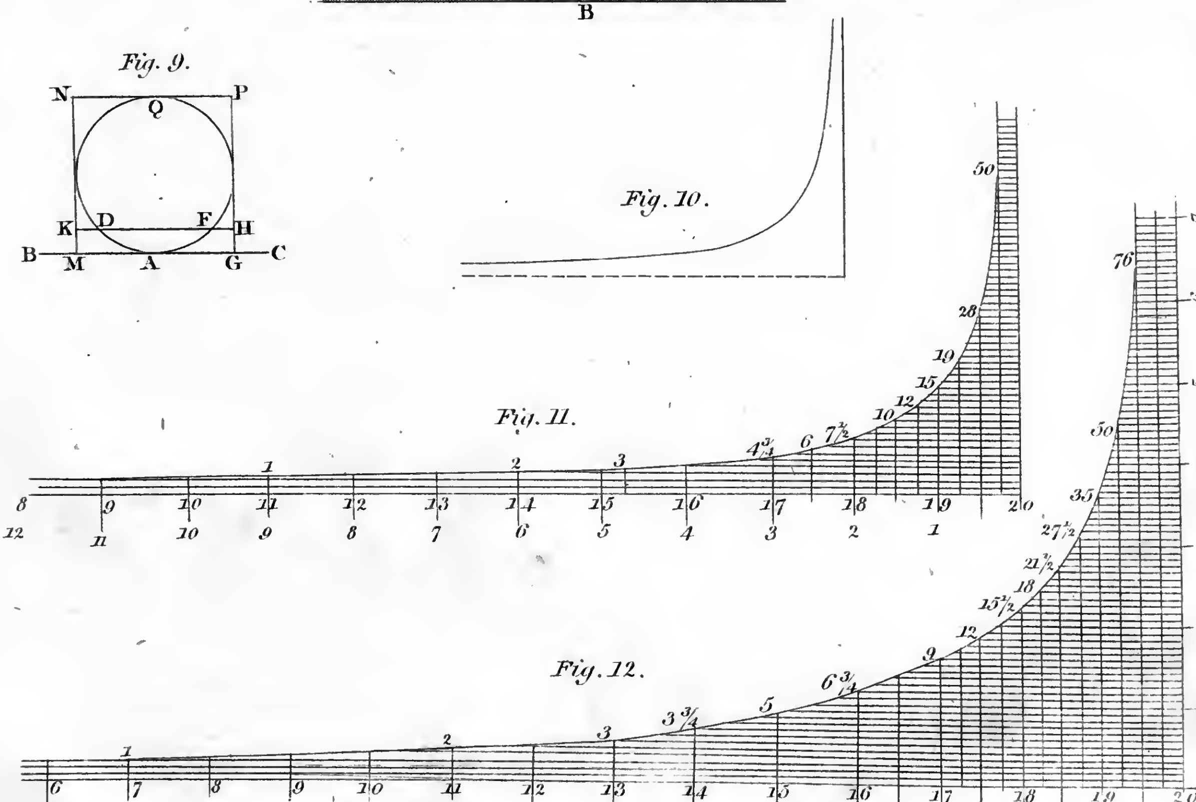


Fig. 9. Fig. 10. Fig. 11. Fig. 12.



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