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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THE

PHILOSOPHICAL TRANSACTIONS

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ABRIDGED.

An Account of the City of Prusa in Bithynia, and further Observations relating to Constantinople. By Thomas Smith, D. D. F. R. S. N° 155, p. 431. *Vol. XIV.*

So many accounts of the Ottoman empire are extant, that the reprinting of this paper is deemed wholly unnecessary.

A Letter from M. Lister, M. D. &c. in Answer to one of Mr. H. Oldenburg's, desiring an Explanation of a Paragraph, concerning the Use of the Intestinum Cæcum, Published in the Philosophical Transactions, N° 95, *Anno* 1673: *as follows: The use of the Intestinum Cæcum, subservient to that of the Colon and Rectum, manifest in such Animals where Nature intends a certain and determinate Figure to the Excrements.* N° 155, p. 455.

I did not think of explaining my sense of the use of the cæcum, until I had the leisure and opportunity of purposely examining the intestines of most kinds of animals. But because I am much mistaken by the person who raised the scruples you sent me, I shall be forced to tell you, what I presume may prove as near the truth as any one of the many conjectures extant in authors, about the unknown use of this part.

I understand by determinate figure; 1. The excrements divided into many small parts of a like shape, such as sheep, deer, conies, hares, rats, mice, horses, caterpillars, some snails, &c. do naturally void. 2. In a greater latitude I oppose figured excrements to liquid: thus the dung of pigeons, geese, and men, cats, dogs, &c. may be said to be figured. Now the cæcum in my opinion is subservient in some measure to the figuration of both, but most manifest in the first kind. Probably the use of the cæcum is to keep the excrements, which shall pass into its cavity, so long, that they are sufficiently drained, baked, hardened, or of a due consistence, as clay is tempered for the mould, to receive the figure to be given it from the colon and rectum. This use of the cæcum, seems to me to be much more manifest in such animals as have figured excrements of the first kind. In rats, whose excrements are constantly alike figured, the cæcum is very large, more capacious than the stomach itself. But its use in receiving the excrements or exhausted chyle is not more apparent from its large capacity, than that of farther drawing and tempering them to a stiffness, for the service of the colon, from the admirable contrivance and structure of this latter gut; it is to be noted, that immediately under the valve of that gut in this animal are certain spiral fibres, which make a kind of screw; now it seems to me, that the excrements, after they are brought to a due consistence, by the necessary stay they make in the cæcum, and being carried out thence into the spiral foldings or screw of the colon, cannot descend in a perpendicular, as formerly, through the small guts, but still gently glide, and that very leisurely by the winding of the screw; whence arises the figure.

And I am apt to believe, that if the cæcum of a rat, or any of the first kind of animals mentioned, was tied up, or otherwise hindered from its receipt, the animals would unavoidably fall into a diarrhœa, there being no reason why the yet liquid excrements or exhausted chyle, such as we constantly find it, even at the very bottom of the small gut, should stop at the entrance of the colon, and not speedily glide through the screw, in a downright descent, that is, elude the device of nature, and make the configuration of that so curiously contrived part useless; we, I say, supposing the experiment to have taken away the necessary diverticulum and repository of the unprepared excrements in tying up the cæcum.

I know not whether the observation will hold good in general terms, because, I say, I have not yet purposely examined divers animals in nature, viz. that wherever there are elegantly figured excrements of the first kind, there is ever a capacious cæcum; and on the contrary the less accurately figured and more liquid the excrements of any animals are, the less the cæcum, or none at all. This is certainly true, that some animals, which are naturally loose, have no cæcum at all, or very little, as the talpa, the echinus terrestris, the gulo; and, amongst the birds, the woodpecker kind, the hawk kind, &c.

We forbear to offer any doubts concerning nature's end, in the necessary figuration of the excrements in some animals, as first to prevent diarrhœas; secondly to abide hunger the better; thus snails in the winter rest with full intestines; thirdly, and chiefly to heighten the fermentation and digestion in the stomach and small guts. Also, why to some animals liquidity of excrements is as necessary.

Directions for the Use of the following Tide Table, sent by J. F. (John Flamsteed) M.R. and R.S.S. N° 155, p. 458.

Having found my last year's tide table very agreeable to experience, I here present you with another for the year ensuing, made by the same theory and numbers, but in a more convenient form for your tracts. I must only desire those that use it to note, that when by reason of long droughts in summer, or continual hard frosts in winter, the fresh waters are low, as also when the winds blow hard at N. or N.W. the tides may hold up longer than the times shown in the table. But when the contrary winds blow hard, or by reason of great rains the freshes are increased, they hold not out so long, yet have I very rarely found the difference more than half an hour. I find by inquiry made amongst our most experienced seamen, that the high waters of our English and neighbouring ports, keep a good correspondence with these of the river Thames; having therefore instructed myself the best I could by conference with them, I have made the following table of reduction, whereby the tide table may be made to serve at the places underwritten, by adding,

	h.	m.
For Tinemouth-haven, Hartlepool, and Amsterdam	0	30
Brest	1	0
Scilly	1	45
Bridlington-pier and Humber	2	0
Penzans, Weymouth, Hamburgh, and Hull	3	30
Lanion and Foulnesse	4	10
Bridgewater, Landsend, and Texel	4	45
Portland, Harfleur, and without the Vlie	5	40
And subtracting,		
For Leith, Maes, and Gouries Gut	0	15
Gravesend, Rochester, and Rammikins	1	0
Buoy of the Nore, and Flushinghead	1	20
Shooe Beacon, Redsand, Portsmouth, Ostend	2	30
Spithead, Harwich, Dover, Calais, and Dublin	3	0
Orfordness, Gunfleet, Hastings, Shoreham, and Dieppe	4	0
Needles, and Yarmouth pier	4	40
St. Helens, and Havre-de-grace	5	30

On some formed Stones of Fossil, found at Hunton in Kent. By Griff. Hatley, M.D.* N° 155, p. 463.

At Hunton, 5 miles from this place, and a quarter of a mile from the river Medway, after the coping of a piece of ground was taken off, which was a clay about 3 feet deep, we came to a very good blue marl, which was 3½ feet deeper, then there appeared a hard floor or pavement, composed of shells, or shell-like stones, crouded closely together; the interstices of which were filled up with the same marl. This layer, which runs as the veins of flints do in chalky earth, was about an inch deep, and several yards over, on which we could walk as on a beach. Under this layer we came to marl again; I think about a foot deep. This ground has a pond on one side of it, which probably was sometime a marl pit, and it is almost surrounded with springs. I cannot on inquiry find that in the memory of any man thereabouts, any floods from the river have reached so far as this place.

These stones, for I take them to be lapides sui generis, are of that sort which is called conchites; and resemble sea-fish of the testaceous kind. Most of them are turbinated, or wreathed, the rest are of the bivalvular sort; but I have not found any of them with valves closed together, but single. The size of the turbinated is from a vetch, to a hasel-nut; they are all filled with a terra lapidosa, like the marl, and are of that colour, till they are washed and rubbed, and then they appear of the colour of bezoar, and of the same polish. After they have been boiled in water, they are whitish, and leave a chalkiness on the fingers, which when it is rubbed off, gives a view of very fine black striæ, thick set on the outside. These wreathed stones are all perfectly formed, and alike in figure, only some have their sides a little depressed: upon a few of them there adhered a little portion of a glittering mineral like iron. I put some of them into vinegar, where they made a strong and boiling effervescence.

The bivalvular are most of them no larger than a kidney bean, some less; a few as broad as the largest sort of beans, but the valve much thinner than any of that kind which had been the exuviæ of an animal; the gibbous part of the valve is smooth, and of the same colour with that of the turbinated. In a few there are some oblong lineations, bent circularly to the commissure of the valve: I have a piece of such a one by me consisting of several lamellæ, which has this further observable in it, that the gibbous part is of a most beautiful black shining colour, and the inner part of a shining pearl-coloured substance.

Of this bivalvular sort many of them seem to be in fieri, not as to their shape, but as to their hardness and thickness, there being in some only the prima

* Petrifactions.

stamina, and in others, the several steps and progresses toward a perfect figuration, which seems to me an unanswerable argument, for their never having been the spoils of animals. Some of these appeared in the inner side white, and it came off on the fingers like chalk, and seemed as if a depression had been first made in the bed, of the shape of a valve, and then the convex side rubbed with chalk or painted white.

The shelly matter having been decomposed and obliterated, nothing remained but the casts or hardened matter with which the shells had been filled.

The imperfect as well as the complete formation of some of the bivalvular kind (the valves being only found single, and both sorts in a ground never heretofore disturbed) are no light argument for their being stones. But by what means they receive this likeness to shells, is hard to determine. There can be no convincing argument given, why the salts of plants, or animal bodies, washed down with rains, and lodged under ground, should not there be disposed into such like figures, as well as above it: probably in some cases much better, as in a colder place; and where, therefore, the work not being done in a hurry, but more slowly, may be so much the more regular.

Raphaelis Fabretti Urbinatis de Aquis et Aquæductibus Veteris Romæ Dissertationes tres. In Quarto. Romæ, 1680. N° 155, p. 466.

This book describes the ancient aqueducts of Rome, and occasionally notices the relation between the ancient and the modern Roman measures of length, as the foot, the mile, &c.

On the Doctrine of Sounds. By Narcissus, Lord Bishop of Ferns and Leighlin. N° 156, p. 472.

There is perhaps no better way of showing the usefulness of the theory of sounds, than by making a comparison between the faculties of hearing and seeing, as to their improvements, &c. In order to which, it is to be observed, that vision is threefold, direct, refracted, and reflected; answerable to which, we have optics, dioptrics, and catoptrics. In like manner hearing may be divided into direct, refracted, and reflected; to which correspond the three parts of the docrine of acoustics; which are yet nameless, unless we call them acoustics, diacoustics, and catacoustics, or phonics, diaphonics, and cataphonics.

Direct vision has been improved two ways. 1. As to the object, by the arts of producing, conserving and imitating light and colours, which are the objects of vision. 2. With respect to the organ or medium, by making use of a tube, without glasses, or a man's closed hand, to look through. So likewise direct hearing partly has already received, and partly further may receive as great and notable improvements, both with respect to the object, and the organ or medium.

1. As to the object of hearing, which is sound, improvement has been and may be made, both as to the production, and as to the conveying or propagating of sounds. As to the production of sounds, the art of imitating any sound, whether by the voice, or any way by the mouth, or by any sort of instrument, or any other means, the voice of any animal, or any other sound whatever, is an art that has as much improved direct hearing, as a harmonious sound exceeds a single and rude one, that is, an immusical tone: which art is yet capable of further improvement. The conveying and propagating, which is a kind of conserving, of sounds, is much helped by duly placing the sonorous body, and also by the medium. For if the medium be thin and quiescent, and the sounding body placed conveniently, the sound will be easily and regularly propagated.

Hence in a still evening, or the dead of the night, a sound is better propagated, and to a greater distance. The sonorous body must be placed conveniently, as near a smooth wall, near water, or a plain, whose surface is even: near a smooth wall, either plane or arched: hence in a church, the nearer the preacher stands to the wall, the better is he heard, especially by those who stand near the wall also, though at a greater distance from the pulpit; those at the remotest end of the church, by laying their ears somewhat close to the wall, may hear him easier than those in the middle. Hence also arise whispering places; for the voice being applied to one end of an arch, easily rolls to the other. And indeed were the motion and propagation of sounds but rightly understood, it would be no hard matter to contrive whispering places of infinite variety and use. And perhaps there could be no better or more pleasant hearing a concert of music, then at such a place as this; where the sounds rolling along together, before they come to the ear, must consolidate into one; which becomes a true composition of sounds, and is the very life and soul of concert. If the sonorous body be placed near water, the sound will easily be conveyed, yet mollified; as experience teaches us from a ring of bells near a river and a great gun shot off at sea; which differ much in the strength, and yet softness and continuance or propagation of their sounds, from the same at land, where the sound is more harsh and more perishing, or much sooner decays. In a plain a voice may be heard at a far greater distance, than in uneven ground; because the sonorous air meeting with little or no resistance on a plane, much less on an arched smooth surface, easily rolls along it, without being hindered in its motion, which is the true cause of its preservation or progression; and falls much when the air passes over an uneven surface. But the smooth surface of the water conveys a sound more entire, and to a greater distance than the plane surface of a piece of ground, a wall, or any other solid body whatever.

The organ, which is the ear, is helped much by placing it near a wall, especially at one end of an arch, when the sound is produced at the other, or near the surface of water, or of the earth, along which the sounds are most easily and naturally conveyed. Indeed it is incredible, how far a sound made on the earth may be heard in a still night, if a man lay his ear close to the ground in a large plain. Here otacoustics help the ear; which may be so contrived, that the sound might enter the ear without any refraction.

As to refracted vision, it arises from the different density, figure and magnitude of the medium; which is somewhat altered also by the divers incidence of the visive rays: and so it is in refracted hearing, all these causes concur to its production, and some others to be hereafter considered. Now as any object, a man for instance, seen through a thickened air, by refraction appears larger than he really is: so likewise a sound, heard through the same thickened part of the atmosphere, will be considerably varied from what it would seem to be, if heard through a thinner medium. And this I call a refracted sound. Improvements of refracted vision have been made by artificial instruments, by grinding or blowing glasses into a certain figure, and placing them at due distances; whereby the object may be enabled to emit its rays more vigorously, and the visive faculty empowered the better to receive them. And thus also instruments may be contrived for assisting both the sonorous body, to send forth its sound more strongly, and the acoustic faculty to receive and discern it more easily and clearly. Thus, a fine glass bubble, filled with clear water, and placed before a burning candle or lamp, enables it to dart forth its rays to a prodigious length and brightness: so an instrument may be invented, which applied to the mouth, shall send forth the voice distinctly to as prodigious a distance and loudness.

As instruments have been invented to help the eye, so likewise are there some, and more such there may be, for the ear. For, as spectacles and other glasses are made to help the purblind and weak eyes to see at any competent distance: so there are otacoustics to help weak ears to hear at a reasonable distance also. Which would be as great a help to the infirmity of old age as the other invention of spectacles is, and perhaps greater; forasmuch as the hearing what is spoken is of more daily use and concern to such men, than to be able to read books, or to view pictures. Also, as perspective-glasses and telescopes help the eye to see objects at a very great distance, which otherwise would not be discernible; in like manner may a sort of otacoustics be so contrived, as that they shall receive in sounds made at a very great distance also; but with so much advantage, that the ear shall be able to hear them, which otherwise would have been inaudible. Again, as microscopes or magnifying glasses, help the eye to see near objects, that by reason of their smallness were invisible before: so microphones or micracoustics, that is, magnifying ear instruments, may be contrived so that they shall render the most minute sound in nature distinctly audible, by magnifying it to an inconceivable loudness; by the help of which we may hear the different cries and tones, as well as by microscopes see the divers shapes and figures of the smallest animals. Also, as by polyscopes or multiplying glasses, one thing is represented to the eye as many, whether in the same or different shapes; so by a polyphone or polyacoustic, well ordered, one sound may be heard as many, either of the same or a different note. Insomuch that who uses this instrument will at the sound of a single viol seem to hear a whole concert, and all true harmony. By which means this instrument has much the advantage of the polyscope.

Reflected vision has been improved by the invention of looking-glasses and polished metals, whether plane, concave, or convex. Thus also reflected audition, made by echoes, may be improved, by contriving several sorts of artificial echoes. For in general, any sound falling directly or obliquely on any dense body, of a smooth surface, is beat back again and reflected, echoes more or less. But if the sound be propagated parallel to the surface of the dense body, there will be no reflection of sound, no echo. Or if the surface of the opposing body be uneven, the air by reverberation will be put out of its regular motion, and the sound be thereby broken and extinguished: so that though in this case also the air be reflected, yet sound is not reflected, nor is there any echo. And if it does echo more or less, to show, that when all things are as is before described, there is still an echoing, though it be not always heard, either because the direct sound is too weak to be beaten quite back again to him that made it; or that it does return home to him, but so weak that without the help of a good otacoustic it cannot be discerned; or that he stands in a wrong place to receive the reflected sound, which passes over his head, under his feet, or to one side of him; which therefore may be heard by a man standing in that place, where the reflected sound will come, provided no interposed body does intercept it; but not by him that first made it.

I shall further make out the comparison between reflected vision and audition by these following propositions.

1. As a plane speculum reflects the object in its due dimensions and colours; allowing for their difference of appearance according to their distance: so a plane obstacle reflects the sound back in its due tone and loudness; if allowance

be likewise made for the proportionable decrease of the sound according to its distance. 2. As a convex speculum reflects the object less, but somewhat brighter or clearer: so a convex obstacle repels the sound smaller, but somewhat quicker, than otherwise it would be. 3. As a concave speculum reflects the object larger, but more obscure and inverted: so a concave obstacle echoes back the sound louder, slower, and also inverted; but never according to the order of words. 4. As a speculum takes in and reflects more of its object, when placed at a great distance from it than when nearer; because it reflects according to the apparent magnitude of the body at such a distance, which is less: so also the echoing body, being removed farther off, reflects more of the sound than when nearer. And this is the reason, why some echoes repeat but one syllable, some one word, and some many. 5. As speculums may be so placed, that reflecting one upon or into the other, either directly or obliquely, one object shall appear many; as in Sir Samuel Moreland's glass-room: after the same manner echoing bodies may be so contrived and placed, as that reflecting the sound from one to the other, either directly and mutually, or obliquely and by succession, out of one sound shall many echoes be produced. Moreover a multiple echo may be made, by so placing the echoing bodies, at unequal distances, that they reflect all one way, and not one on the other; by which means a manifold successive sound will be heard; one clap of the hands like many, one ha like a laughter, one single word like many of the same tone and accent, and so one viol like many of the same kind imitating each other. Further, as speculums may be so ordered, that by reflection they shall make one single thing appear many different things; as one single man to seem many men, differing as to shape and complexion, or a company of men, which I think Sir Samuel Moreland's contrivance does not: so may echoing bodies also be ordered, that from any one sound given, they shall produce many echoes, different both as to their tone and intention. By this means a musical room may be so contrived, that not only one instrument, played on in it, shall seem many of the same sort and size; but even a concert of different ones; only by placing certain echoing bodies so as that any note played shall be returned by them in 3ds, 5ths, and 8ths, which is possible to be done otherwise than was mentioned before in refracted audition.

For farther improving this noble science, the following problems are proposed:—

1. The first is, to make the least sound, by the help of instruments, as loud as the greatest; a whisper to become as loud as the shot of a cannon. By the help of this problem, the most minute sounds in nature may be clearly and distinctly heard.

2. The second is, to propagate any the least sound to the greatest distance.

By means of this problem, any sound may be conveyed to a very great distance and heard.

3. The third problem is, that a sound may be conveyed from one extreme to the other, or from one distant place to another, so as not to be heard in the middle. By the help of this problem, a man may talk to his friend at a very considerable distance, so that those in the middle space shall hear nothing of what passes between them.

*Certain Observations on the Midland Salt Springs of Worcestershire, Staffordshire and Cheshire. Of the Crude Salt, which grows from the Stone-powder dejected by the said Brines in Boiling. Of the Specific Difference betwixt Sea-Salt and common Salt. A way (which seems to be the true Method of Nature) of Distilling Sweet and Fresh Water from Sea Water, by the Breath of Sea Plants growing in it. That this Breath probably is the material Cause of the Trade or Tropic Winds. In a Letter to the Editor from Dr. Martin Lister.** N° 156, p. 489.

Notwithstanding the great affinity between the salt of the Midland brine-pits, which is common salt, and the sea salt, I must not omit, among others,† a specific difference, which is now first published, and which, in my opinion, makes the sea water a water of its own kind; and also shows that none of the productions of incinerated plants are truly a marine salt.

The angles of the crystals of common salt, boiled out of the midland brine-pits; as also of sal gem or rock salt, which I take to be one and the same, are entire, and so are all those lixiviated-marine salts, so called and described by Dr. Grew. But some of the angles of the crystals of true sea salt are always cut off into triangular planes, at least on one of the sides. And this I learnt by suffering a bottle of sea water, taken up on the coast at Scarborough, where no river enters it, to evaporate leisurely, placed in the shade, after it had been half boiled away: and here all the crystals, which were many, and of different magnitudes, agreed in a like figure, as is described. This experiment I repeated with the like success; and do not doubt but it will succeed with any sea water, brought from any other part of the world.

Probably the sea-water was the only element of water created at the beginning. And the congregation of the waters was called sea, Genesis 1; that is,

* There is nothing in Dr. Lister's observations on the salt springs of Worcestershire, Staffordshire and Cheshire, or in his conjectures respecting the cause of the trade-winds, worth preserving. The only parts of this paper entitled to notice are those which relate to the crystallization of sea salt, and to the distillation of sweet or fresh water from sea water.

† N. B. The inland brine pits yield not bittern, of which see Mr. Collins, p. 54.—Orig.

before any animal or vegetable was created, or the sun itself. But on the creation of these the fresh water had its rise accidentally, because it owes its being in great part (as I have elsewhere shown *) to the vapours of plants and the breath of animals and the exhalations raised by the sun, and by this means the rivers may be furnished from the sea by the breath of its own plants and animals, so as to make what the wise man says very intelligible, Eccles. 1, All the rivers run into the sea, and the sea is not full; into the place from whence the rivers come, thither they return again; that is by way of exhalation and vapours.

Now that the sea-water is made fresh by the breath † of plants growing in it, I have elsewhere demonstrated; ‡ thus I took a long glass body, and having filled it pretty full with sea water, taken up at Scarborough, I put into it common sea-weed, fresh and new gathered, some with the roots naked, and some growing on and adhering to stones: the glass body being full, I put on it a head with a beak, and adapted a receiver to it, all without any lute or closing the joints; from these plants distilled daily, though in a small quantity, a fresh, very sweet, and potable water, which has no empyreuma or unpleasant taste, as all those distilled by fire necessarily have.

I urge this experiment, as the most natural, most easy, and most safe way, of having sweet water from the sea, and which may be of greater use than perhaps some are apt at the first to fancy, even to supply the necessity of navigators.||

An Account of the Increase of Weight in Oil of Vitriol exposed to the Air. By W. G. N° 156, p. 496.

Since Mr. Boyle has made the air a subject of his observations, the learned world is sufficiently taught what share it has in producing effects unknown before; and if, from the hint thus obtained, we reflect on the infinite variety of steams constantly emitted from all sorts of bodies into the atmosphere, which are there dissolved as it were in a common menstruum, we have reason to expect therein particles enough of all shapes, sizes, and motions, fit and proper to alter the texture, diminish or increase the bulk and weight, of almost any body exposed to its action.

* De Font. Med. Angl.—Orig.

† By the breath of plants is here meant the humidity exhaled from them.

‡ De Font. Med. Angl.—Orig.

|| Respecting the methods of obtaining sweet water from sea-water, the reader is referred to vol. i, p. 549 of this Abridgment.

As to the increase of weight, we know how bodies deprived of some constituent parts by fire, (as quicklime and all calxes) slacken and imbibe something from the air;* and the like is observed in the caput mortuum of salt, nitre, alum and vitriol. Thus also, all fixed salts run into a fluid per deliquium; it is the air, that in 7 years fully reimpregnates the earth, heaped up in the shade, whence nitre was before extracted; it is the air that causes the efflorescence of marcasites and vitriol-stones; it is the air that by its acid turns the lead of old buildings into cerusse, which without doubt increases in weight, (as that made by fire does), which is asserted to be at the rate of 6 or 7 pounds in 100.

Thus the increase and attractive power of solids is already put beyond question; but that liquids, such as seem saturated with their own moisture should imbibe more from the air, is not mentioned by any author that I know of, except the aforesaid learned person, who in his tract on aërial magnets advises trials on the liquid preparations of vitriol: I have heard indeed some druggists have accidentally taken notice of this increase in oil of vitriol, but the observation never was prosecuted so as to ascertain how much the said increase was, and what the substance gained. The industrious chemist Mr. White, our university operator, having a phial of that liquor unstopped and constantly running over, first gave occasion to the following notes: but since from thence no true estimate of the just increase could be collected, I hope it may not prove altogether ungrateful to the curious to state what has occurred more particularly on this subject.

On the 9th of Nov. 1683, 3 drams of oil of vitriol, so far dephlegmated as to burn or corrode a strong packthread asunder, was exposed to the air in a marmalade glass of 3 inches diameter, and placed in a nice pair of scales, in a room where no fire nor sun came; its increase for 7 natural days, divided by less portions of time, was according to the following table.

* By "calxes" the author means metallic oxyds, as, by way of illustration, he afterwards mentions cerusse, an oxyd of lead; but between metallic oxyds and quicklime there is no proper analogy. In the production of metallic oxyds by the action of fire, it is true, as the author remarks, that something (oxygen) is absorbed from the air, whence there is an augmentation of weight; but in the burning of chalk or limestone (carbonate of lime) there is something (carbonic acid gas) driven off, the consequence of which is a diminution of weight. Thus the condition of the two products (a metallic oxyd and quicklime) *immediately after* being subjected to a due degree of heat, is directly opposite. If, however, the quicklime be subsequently exposed to the open air, it re-absorbs the principle (carbonic acid gas) which had been driven off by the fire; and in consequence of this re-absorption, it recovers, along with its former properties, its loss of weight.

THE TABLE.

D.	Hour.	Gain.	Space of Time.	Weather.	Wind.	Sum of gain.	Natural Day.
		Scr. Gr.	h.			Dr. Scr. Gr.	
9	5 p. m.	0 0	0		Southerly.		
	11 p. m.	0 19	6	Moist.		0 8	1st
10	8 a. m.	1 12	9	Moist and Windy.	N. Wester-		
	11 a. m.	0 8	3	Rainy Morning.	ly.		
	5 p. m.	0 9	6	Clear.			
	11 p. m.	0 18	6	Starlight, Cold.			
11	8 a. m.	1 7	9	Bright Morning.	N. W.	0 2 18	2d
	11 a. m.	0 4	3	Mild.			
	5 p. m.	0 9	6	Mild, dry Weather.			
	11 p. m.	0 10	6	Mild, dry.	N. W.		
12	8 a. m.	0 17	9	Clear Morning.	North.	0 1 19	3d
	11 a. m.	0 5	3	Frosty.	N. more W.		
	5 p. m.	0 7	6	Overcast.			
	11 p. m.	0 6	6	Cloudy Rain.	Westerly.	0 1 3½	4th
13	8 a. m.	0 9	9				
	11 a. m.	0 3	3	Cloudy, mild.	South-West.		
	5 p. m.	0 5½	6				
	11 p. m.	0 6	6	Cloudy, moist.	South-East.	0 0 18	5th
14	8 a. m.	0 8	9	Cloudy, misty.	Southerly.		
	11 a. m.	0 2½	3	Misty.			
	5 p. m.	0 1½	6	Very Warm.			
	11 p. m.	0 2	6	Cloudy, unusually warm.	More South.		
15	8 a. m	0 6	9	Cloudy.	South-East.	0 0 15	6th
	11 a. m.	0 3	3	Cloudy, moist.	More South.		
	5 p. m.	0 4	6	Clear, coldish.	Easterly.		
	11 p. m.	0 4½	6	Dry, Starlight.	Easterly.		
16	9 a. m.	0 9	10	Cold, Cloudy, but Cold.		0 0 17½	7th
	11 a. m.	0 2	2	Cloudy, Windy.	South-East.		
	5 a. m.	0 2	6	Cloudy, very mild.			

From the 16th, in the successive spaces of 24 hours, each gained one of the number of grains following; as the 8th natural day gained 13½, the next 12, 9, 7, 6, 5, 5, 4½, 3, 3, 3, 3, 4, 3, (December) 4, 4½, 4, 3, 3, &c. still irregularly decreasing till the liquor was saturated. The diary was continued to Jan. 4, 168¾, when the increase in 24 hours amounted scarcely to half a grain, and probably had the weather been then dry, it might have been none at all, or rather the liquor might have lost what before it had gained. Hence it is obvious, that the more the liquor was saturated, the less was its daily increase, though not gradually less by an even descent each day, but sometimes two or more natural days together it was exactly the same, a day or two after less, and then again more the next day following, according as the liquor stood affected by the heat or cold, dryness or moisture of the weather, the differing time of the day and quarter of the wind. Thus on the view of the whole diary, of almost two months, it appeared that the increase was more in a moist, rainy, misty, and snowy season, but less in a frosty, clear, and dry one, as also more in a cold than in a warm air. When the wind was northerly, or easterly, the gain was less, cæteris paribus, than when southerly or westerly, and less in the day than in the night. The primary cause of this phenomenon seems to be the moisture of the air, which our liquor greedily imbibes. And we may safely conclude that moisture is the chief and only cause of the increase of weight in oil of vitriol, since in dry clear weather it constantly increases less than in moist and cloudy, the circumstance of heat or cold remaining the same in both.

As to the quantity of the whole increase, it cannot be determined by any general rule, since it varies according to the different strength of the oil of vitriol: for it appears by the table, that the more diluted the liquor, the less attractive it proved. This here employed, which was highly phlegmated, gave a triple and more than $\frac{1}{6}$ of its first weight, amounting in all from 3 to 9 drams, and 30 grains, before it came to a stand. Which proportion of increase I found confirmed in less quantities also; as 3 grains increased to more than 9 grains; and 1 grain gave the weight of something more than 3 grains. But besides the strength of the liquor, there are other circumstances; as the season of the year, and position of the place, which will certainly cause some alteration: thus, our liquor will gain more in winter than in summer; more in a cellar and sunless room, than in a room not so qualified.

All these circumstances, which relate to the quantity, will also influence very much the time of the increase; but what makes the most peculiar and principal variation in this point, is the proportion of the surface to the bulk of the liquor; for the greater or less the surface is, the quicker or slower is the increase. Thus, 3 grains dropped and diffused to nearly $\frac{3}{4}$ of an inch in breadth on a piece of glass, gained 3 grains in 6 hours, 1 grain in 6 more, 1 grain and $\frac{1}{2}$ in 12 hours more, in the next 12 hours it gained $\frac{1}{2}$ a grain, and in the last 12 hours it gained very little observable. So that in less than 48 hours, having more than triple its first weight, it was for sometime fully saturated, till rainy weather added something more. But to discover more nicely what influence the proportion of surface has in promoting or retarding the increase of weight, I exposed in the same room, and to the same temper of the air, 3 drams of the same oil of vitriol, in an open flat glass, 1 inch broad, being only $\frac{1}{3}$ of the diameter of that glass used at first with the like quantity. The result was, that whereas the other surface, of 3 inches diameter, gained, as in the table, near 19 grains the first 6 hours, this less surface gained a very little perceivably more than 2 grains in the same time. Now since the areas of circles are to one another as the squares of their diameters; as 1 the square of the less, is to 9 the square of the greater diameter; so was the weight of a little more than 2 grains, gained in the narrower glass, to near 19 grains, gained in the broader; so that the time of increasing bears, as near as can be expected, an exact proportion to the surface of the liquor exposed, and the liquor in the smaller glass having but $\frac{1}{9}$ part of the surface of the greater, could not be saturated under 9 times as many days as the greater.

The only use of it I can at present find will be to estimate moisture and dryness in the air, which is evidently suggested by this following observation: that when the oil of vitriol is saturated in the moistest weather, it afterward retains or loses its acquired weight, as the air proves more or less moist. Thus, the one grain above-mentioned, after its full increase, often varied its equilibrium, viz. in dry weather; the weights, in moist, the liquor constantly preponderated, and that so sensibly, that the tongue of the balance, of $1\frac{1}{2}$ inch long, described an arch of variation to $\frac{1}{3}$ of an inch compass, even with that little quantity of liquor; so that if more liquor expanded under a large surface be used, the minutest alteration of weather must needs much more affect it, and a bare pair of scales will afford a hygroscope as nice perhaps as any yet known.—This balance may be contrived in two ways; either such that the pin should be in the middle of the beam, with a very slender tapering tongue, of a foot, or a foot and half long, pointing to the divisions on a broad arched plate fixed above; or else the scale with the liquor may be hung to a point of the beam very near the pin, and the other extreme made so long as to mark a large arch on a board, placed conveniently for that purpose. The scale in either may be a concave glass of 4 or 5 inches diameter. Lastly, on the division of the arches should be inscribed the different temperature of the air shown by the liquor. And it is probable that oil of sulphur per campanam, as also oil of tartar per deliquium, and the liquor of fixed nitre, &c. may succeed as well as the oil of vitriol.*

A hygroscope might also be made of a viol-string running over pulleys, and suspending a bullet, connected to the shorter end of an index, the other extremity being so long as to describe a long arch, by the rising and falling of the bullet, by the shrinking and stretching of the string; and this would be still more nice, by fastening the index to the centre of the last pulley.

Ricreatione dell' Occhio é della Mente nell' Osservation' delle Chiocciole dal P. Filippo Buonanni, &c. in Roma, per il Varese, 1681. N° 156, p. 507.

A work on testacea or shell fish, which the author divides into univalves,

* There is much ingenuity in this author's application of the property which oil of vitriol or concentrated sulphuric acid possesses of attracting moisture from the air, to hygrometrical purposes. This mode, however, of measuring the varying humidity of the atmosphere, is superseded by the

bivalves, and turbinated. The descriptions of the different shells are illustrated by engravings.

Three Papers of Dr. Martin Lister, 1st on the Nature of Earthquakes. N° 157, p. 512.

The breath of the pyrites, as I have before shown, is sulphur, and it naturally takes fire of itself. The material cause of thunder and lightning, and of earthquakes, is one and the same, viz. the inflammable breath of the pyrites: the difference is, that one is fired in the air, the other under ground. For, what is burned with lightning, smells of very brimstone; and of earthquakes, the sulphurous stink of waters smelt before, and of the very air itself after them. They also agree in the manner of the noise, which is to be carried on, as in a train fired; the one rolling and rattling through the air, taking fire, as the vapours chance to drive; as the other fired under ground, in like manner moves with desultory noise. That the earth is more or less hollow, is made probable by what is found every where in the mountains, viz. Natural cavities or chambers, which the miners of the north call self-opens. These they frequently meet with, some very great, and others less, running in small sinuses. Many of them open to the day, discovering themselves without digging, as Pool's Hole, and Oaky Hole. Again, the great and small streams, which arise from under mountains, evince their hollowness. And that these subterraneous cavities are at certain times and in certain seasons full of inflammable vapours, the damps in mines sufficiently witness; which fired, act just as in an earthquake, save in a less degree.

Now that the pyrites alone, of all the known minerals, yields this inflammable vapour, I think it highly probable, for these reasons. 1. Because no mineral or ore whatever is sulphurous, but as it is wholly or in part a pyrites: and most of the English fossils which contain brimstone, are also found to hold iron. 2. Because there is but one species of brimstone that I know of, at least here in England: and since the pyrites naturally and only yields it, it is but reasonable wherever brimstone is found, though in the air, or under ground in vapour, to think that that also proceeds from it. For the sulphur vivum or natural brimstone, which is found in and about the burning mountains, is certainly the effect of sublimation; and those great quantities of it said to be found about the skirts of the volcanos, is only an argument of the long duration and vehemency of those fires. And though sulphur vivum, or rough brimstone, as

simple and still more accurate contrivances of modern philosophers; of which a description will be given in some of the future volumes of this Abridgment.

they call it, had from Hecla and Italy is opaque, and agrees not with the transparent and amber-like sulphur vivum of the ancients; yet it does not follow, that that also was not produced by sublimation, any more than that the stalactites, or water-wrought stone, is not so made, because some of it is found opaque, and some crystalline.

But possibly the pyrites of the volcanos may be more sulphurous than ours: and indeed it is plain that some of ours in England are very lean, and hold but little sulphur; others again a great deal. And this may be one reason why England is so little troubled with earthquakes; and Italy, and almost all round the Mediterranean sea, so very much. Another reason is the paucity of pyrites in England; besides the subterraneous cavities in England are small and few, compared to the vast vaults in those parts of the world; which is evident from the sudden appearance of whole mountains and islands.

Second, on the Spontaneous Firing of the Pyrites.—It may be objected, that no body is kindled by itself: but it seems to be apparently otherwise; for vegetables will heat and take fire of themselves, as in the frequent instance of wet hay; and animals are naturally on fire, and men also sufficiently evince it, when in a fever; and among minerals, the pyrites, both in gross and in vapour, is actually fired of its own accord; and damps naturally take fire of themselves. Also the volcanoes all the world over argue as much; for it is very probable that they are mountains consisting in great part of pyrites, as appears by the quantities of sulphur thence sublimed, and the application of the loadstone to the ejected cinders.

Third, concerning Thunder and Lightning being from the Pyrites.—There are two sorts of instances, besides the arguments before urged, which very much favour the opinion I lately offered, that thunder and lightning owe their matter to the sole breath of the pyrites: and although I am as loath, and as backward as any man to give credit to such instances, which seem rather prodigies, than the phænomena of nature; yet because they often occur in history, it is at least fitting to bring them under further inquiry and examination, that if they can be confuted as false, so much may be done for posterity; and that we at least may not leave on our registers assertions not true, if they can be fairly set aside.

The first sort of them are those which tell us of iron having fallen in great masses, and also in powder after the manner of rain, out of the air. In a part of Italy it rained iron in such a year, and in Germany a great body of iron-stone fell at such a time; the like Avicenna affirms. Julius Scaliger says he had by him a piece of iron, which was rained in Savoy, where it fell in divers places. Cardan reports that 1200 stones have fallen from heaven, and one of them weighed 120lb. some of them 30lb. some 40lb. very hard, and of the colour of iron.*

Now this ferrum or æs nubigenum, if there was ever any such, was concreted of the breath of the pyrites, which we have elsewhere shown to be the pyrites ex tota substantia. The other instance, which I say is recorded in our registers, is of lightning being magnetic: (See Philos. Trans. N° 127.) This I am sure of, I have a petrified piece of ash, which is magnetic; that is, the pyrites in succo; which makes it probable it may be magnetic also in vapour.

The Effect of Thunder on the Compass of a Ship. Communicated by Sir R. S. N° 157, p. 520.

July 24, 1681, the ship called the Albemarle, being 100 leagues from Cape Cod, in latitude 48°, about 3 o'clock afternoon, met with a thunder storm. The lightning burned the main-topsail, split the main cap in pieces, rent the mast quite throughout; and there was in particular one dreadful clap of thunder, with which something from the clouds fell on the stern of the boat, which broke in many small parts, and split one of the pumps, the other pump being also much hurt. It was a bituminous matter, smelling much like fired gun-powder: it continued burning in the stern of the boat; and it could not be extinguished till all the matter was consumed.

When night came, observing by the stars, they perceived that their compasses were changed; the north point of the compass in the binacle was turned clear south. There were two other compasses unhung in the locker in the cabin, in one of which the north point stood south, like that in the binacle; in the other, the north point stood west; so that they sailed 1000 leagues by a needle, whose polarity was quite changed. As for the compass wherein the lightning had made the needle to point westward, since it was brought to New England, the glass being broken, it has, by means of the air coming to it, wholly lost its virtue.

An Abstract of a Treatise on the Calculus Humanus; in Answer to several Queries proposed by Sir John Hoskins. By Fred. Slare, M.D. F.R.S. N° 157, p. 523.

After combating the various opinions of other authors concerning the composition of the human calculus, Dr. Slare concludes—This concrete seems rather referable to bone than to any other consistent or fluid part of the body,

* On this subject, the falling of stones from the clouds, several interesting papers have appeared in some of the late volumes of the Philosophical Transactions.

as I concluded by comparing chemical products, which I only very briefly relate. Having cleared the bone of marrow and fat by boiling it in water, I distilled it, and obtained about 2 drams and a half, from an ounce of bone, of a volatile liquor impregnated with salt, that smelt very different from spirit of urine, and nearer that of hartshorn: I found the caput mortuum, as to weight, very consonant; and also could extract no manner of salt from it. For which reason refiners make their cupels of calcined bones, as they are forced to dulcify (which they call washing out the salts of) other ashes, before they can make cupels of them. In this also it agrees with the calculus humanus, vulgarly so termed, that few acids will dissolve it, excepting those that are nitrous, nor do these work on it very vigorously. They must however be allowed to differ in their specific gravity, the calculus not having so close and compact a texture as the bones have. For bones I have found twice as heavy as their bulk of water.*

An Index of some Experiments made in this short Treatise.—Several stones of the bladder and kidneys were distilled; and all afforded volatile urinous salts, which ferment on any acids. Bones were distilled, and found to be of similar principles.—Petrified water affords only fresh and clear water on distillation.—Calculi examined hydrostatically, were found in proportion to their bulk of water, as 5 to 4.—We weighed flint, crystal, petrified water, Welsh diamonds, petrified wood in water; and found them all very near of a specific gravity, and almost as heavy again as our calculous matter.—We weighed bones hydrostatically; and found them twice as heavy as their bulk of water.—Bones not easily wrought on by common acids, only by nitrous ones, and that without ebullition.—Various unsuccessful attempts made to dissolve the callus, by acid and acrimonious menstruums, some were vegetable and some mineral, as spirit of salt, of vinegar, of venus, oil of vitriol, &c. also with alkalisate acria, as sal fraxini, which corrodes glass, lapis infernalis; but none would touch it except nitrous acid.—The coagulation of spirit of wine has no place here.

Experiments to be made, relating to the Therapeutic Part.—To discover innocent menstruums that may dissolve the stone.—To examine the condition of the urine sometime before a paroxysm, both as to its specific gravity and contents.—And also during the paroxysm, to regard the painful water sometimes

* But Dr. Slare found (as he mentions in a former part of this paper) human calculi to be "only as heavy as their bulk of water and a fourth part more." It was not till within these few years that the constituent parts of the urinary calculus were fully determined. It has been ascertained by modern chemical analyses that phosphate of lime (earth of bones) is a frequent ingredient in human calculi. Thus far has Dr. Slare's conjecture been verified. It is however but one of the ingredients, and that not always, of these morbid concretions. Oxalate of lime is another ingredient, and the most frequent of all, uric acid and urate of ammonia.

transmitted.—To inquire into the nature of nephritic medicines.—To examine lithontriptics, so as to exclude those from that class and character, which have no relative virtue that way: and to lessen the catalogue of those mistaken specifics.—To inquire into the nature of the hop, which is so much and perhaps innocently condemned for its aptness to generate the stone.—To explain the manner of the operations of some medicines, which though they are not lithontriptics, yet may be good nephritics.

A Postscript to the Editor, containing a short Account of two Human Calculi, of unusual Form and Size, from the same F. S. M. D.—I here send you the figure of a stone, fig. 1, pl. 1, of a prodigious size, and as rare a shape, somewhat resembling indeed the kidney, for that was quite worn away, and this stone filled up the place. It weighed somewhat more when taken out of the body, than it does now, for it then weighed 7 ounces and a half. There is no account of a stone generated in the kidney that nearly equals this. Without breaking it asunder, I can find it consists of several laminæ laid over each other, as that of the bladder does: the circumference measures 7 inches.

That taken out of the body of the late Duke of Norfolk's father, fig. 2, was brought not long since to the Royal Society, by Sir Theodore de Vaux, which is branched, and seems to have spread some branches into great vessels, whether arteries, veins, or into the ureter, I cannot determine, though these as well as the pelvis seem to have been filled up by this large stone: yet this comes far short of that before-mentioned, since it weighs but 4 ounces and a half: a stone indeed of an incredible size to be found in the kidney. The measure lengthwise, from one extreme to the other, was 4 inches; the extension of the branches, from one to the other, measured crosswise or transversely $3\frac{1}{2}$ inches.

Account of a Spot seen in the Sun from the 25th of April to the 8th of May, 1684; with the Line of its Course predicted, if it make a second Return. By J. Flamsteed, Astron. Reg. N° 157, p. 535.

April 25, as I was measuring the distance of the planet ♀ from the sun, about an hour before noon, I discovered a large spot entered within his disk, a little distant from his following limb. These appearances, however frequent in the days of Scheiner and Galileo, have been so rare of late, that this is the only one I have seen in the sun's face since December 1676. Wherefore I thought an account of it might not be unacceptable. By the observed meridional distances of it and the sun's southern limb, from the vertex at noon, I found it to have 3′ 40″ more north declination than the sun's centre, and at 3h. 35 m. afternoon I measured its distance from his next limb, 40″.

Next morning, April 26, I saw it more remote from his limb, and by the observations then made, at 8 in the morning, determined its longitude from the sun's axis $66\frac{3}{4}$° and its declination from the solar equator 9° 24′ south. Whence, supposing the revolution of any point of the sun, to the same fixed star, to be performed in 25 days 6 hours, the angle of his equator and our ecliptic 7 degrees, and the longitude of his northern pole ♍ 16°, I designed the line of its way or trace over the sun, and the points in it where the spot would appear every morning after, at the same hour, till its egress on the 8th of May, which I found altogether confirmed by such observations as I made till then; so that I had no reason to doubt of the theory.

When the spot was near the middle of the sun, it appeared very broad and almost square, the nucleus of the figure about 40″ diameter; but when it was near the limb, much narrower, and almost oval. It seemed to have consistence enough to endure a second return; if it shall, it will enter the visible disk of the sun on the 21st of May in the evening, and in its passage over him describe a line nearly straight, with greater latitude from the ecliptic.

Account of a Polypus found in the Heart of a Person that died epileptical, at Oxon. By W. Gould, M. B. Fellow of Wadham College, and F. R. S. N° 157, p. 537.

The person whose body was the subject of dissection, was a poor labouring man, a stranger in the town, and destitute of relations. He died in the street suddenly; so that there cannot be expected so particular a relation of the symptoms he laboured under as could be wished. However some things material to our purpose, as far as we could learn from the vulgar, who conversed with him in his illness, were such as these, viz. That he was of a swarthy, lurid complexion; that he was afflicted with fits of the falling sickness; an obstinate quartan ague of above a year's continuance; a deep jaundice, even to that degree which is called the black, with its constant consequent an universal settled ill habit of body; a sensation of a hard load and pressure at his stomach, meaning perhaps his breast, or the upper part of the region of the liver. He complained much of very great shortness of breath, being almost constantly apprehensive of choking; far-fetched involuntary sighings, and prodigious palpitations of his heart, were the continued mischiefs that attended his miserable life, a great while before death relieved him. He used to swoon very often; and at length died; according to the judgment of the by-standers, in the shivering fit of his ague, with the convulsions of an epilepsy, not without foaming and frothing at his mouth.

On opening the body, the liver upon deep incisions appeared bloodless, stuffed throughout like a bag of sand with a yellow gritty substance; the gall-bladder was furnished with the like, but of a darker hue. The spleen was very large, and of too soft and loose a texture, but not much discoloured. The omentum rotten and wasted. The membrane of the stomach very flaccid and thin, appearing black and mortified, and upon taking it out within 24 hours after death, though tied close at both ends, sent forth such an intolerable sour rancid scent, that the strongest double aquafortis, to which it might best be compared, could not prove so troublesome and offensive to the smell. The lungs were distended to the uttermost with a purulent froth. The heart much stretched beyond its natural magnitude, and of a very flat figure; the veins of the whole body were of an unusual and extraordinary size, especially the internal jugulars were strained to above $\frac{3}{4}$ of an inch diameter; polypous concretions also were found in the larger veins of the arms, legs, and other parts; but what most engaged our attention and wonder, was that which is represented by fig. 4, pl. 1, found in the right ventricle of the heart, and towards its apex or tip firmly radicated, so that no small stress was required for the separation. The part by which it was fixed was nearly an inch and half in diameter when fresh taken out, irregularly rough at the bottom, insinuating many roots into the lacunæ or little cavities of the ventricle, which again by less fibres were fastened to the inner membrane of the heart. The great branch B, which ran out into the right auricle, was nearly 2 inches diameter at the largest extreme, and reached no farther than the insertion of the vena cava. As for those branches marked G, G, tending to the arms, how far they grew I cannot assert, not knowing whether they were broken off or not; but the branches H, H, H, H, &c. tending toward the head, could not be drawn out without some force, and it is very likely they were broken off at the diverticula or two round sinuses where the jugulars enter the skull; for the like concretions were found in the vessels of the brain, to which probably these might be adjoined. The substance of the whole was plainly fibrous, resembling a nerve, and tough while moist, though upon drying brittle, the colour white, and was clothed with a thin coat including, in that part which filled the right jugular vein, two little black specks (h, h) of blood, as we suppose, a long while there coagulated. As for other circumstances of the shape, extent, and size of the polypus, the reader may recur to the engraven figure drawn rather less than half the dimensions of the thing itself.*

* In the concluding part of this paper the author endeavours to refute the opinion maintained by Kerkingius, (and others) that polypous concretions, such as those here described, never exist during life, but are produced by coagulation immediately after death. Remarks are offered on the causes and symptoms of polypi, extracted from various writers; in which there is nothing that claims particular notice.

The Explanation of the Figure of the Polypus, as it appeared when fresh expanded on a Board. Fig. 4, pl. 1.—A that part of the polypus which was firmly rooted in the right ventricle of the heart. B the branch terminated in the right auricle. CDDD the part tending toward the lungs. EE the branch running out of the ventricle into the pulmonary artery. eeeee, &c. The several lesser ramifications, distributed according to the several divisions of the pulmonary artery. FFF the branch belonging to the descending vena cava. GG the branches begun in the axillary veins. HHHHH, &c. the two branches that ran up the internal jugulars, even to their entrance into the skull. hh Two little black specks of concreted blood, contained within the coat of the polypus.

An Account of a Book, viz. The Geometrical Key, or Construction of all Equations, Linear, Quadratic, Cubic and Biquadratic, by a Circle and one only Parabola. By Mr. Thomas Baker, Rector of Bishop Nympton, in Devonshire. N° 157, p. 549.

The analysis which the ancients used for constructing problems geometrically, or by lines, has been highly advanced by Descartes's method; that part of this method which concerns local problems has been well explained by De Wit, but the other and more principal part of constructing equations has been lately cleared by De la Hire. Yet neither Descartes nor De la Hire do it without the trouble of preparing the equation by taking away the second term. To avoid this trouble our author here shows how to construct all affected equations, not exceeding the 4th power, by the intersection of a circle and parabola, without omitting or changing any of the terms. And although by the method of Descartes we may find not only any parabola, but also ellipses and hyperbolas, to construct these equations, yet of all lines of the first kind, a circle and parabola being the most simple, it follows that the way which our author has chosen is the best.

*A Letter from Mr. Wm. Molyneux, Secretary to the Society of Dublin, to Wm. Musgrave, L.L.B. Fellow of New College, and Secretary to the Philosophical Society of Oxford, for the Advancement of natural Knowledge; concerning Lough Neagh in Ireland, and its petrifying Qualities.** N° 158, p. 552.

It is generally agreed by all the inhabitants thereabouts, that Lough Neagh has a petrifying quality; yet I have a letter by me from a gentleman (unknown

* On the petrifying quality of this lake, further observations occur in some of the subsequent volumes of the Transactions; among which, those by Mr. Simon in the 44th volume deserve particular notice.

to me, and therefore I will not promise for his credit or the fidelity of his inquiry) who positively denies that there is any such thing; but asserts, that the stones that are brought to us as petrified wood, are found deep in sand hills in the country adjoining to the Lough, alleging as an experiment, that a gentleman of his acquaintance stuck an oak stake in the Lough 20 years ago, which there remains unaltered. But I conceive this assertion to be groundless, and the experiment falsely made.—For 1st, it is agreed by all, that no wood will petrify in this Lough, except holly;* so that his applying an oak stake was improper; 2dly, as to their being found in sand hills, they may easily be supposed in process of time to have been brought thither, and left there: for I do not find he asserts, that they are found so deep in those hills that have not been dug up; and 3dly, it is with some probability asserted, that the earth about Lough Neagh has this petrifying quality, and we may well imagine that these sand hills especially are not destitute of it. For I am certainly informed, that a gentleman of the country about this Lough, a little before the rebellion, cut down some timber for building, and amongst others cut down a large holly tree; but being diverted by the rebellion from building, his timber lay on the ground in the place where it was felled, on the banks of the Lough, all the time of the war; till at last, the kingdom being settled, the gentleman went to look for his timber, and found the other timber overgrown with moss, and the holly petrified, though the water of the Lough had never reached it.

I query whether the holly itself, that grows upon the banks of this Lough, may not be more apt to be petrified, than the same wood growing elsewhere, and brought thither, and put into the Lough; for certainly if the ground has this quality, this is very likely to follow.

That what we call Lough Neagh stone was once wood, is most probable on these accounts: 1st, it will not stir with acids, which is a property observed by Dr. Grew on some petrified woods, in the Museum of the Royal Society; though the doctor there makes it an argument for his suspecting they are lapides sui generis; 2dly, the Lough Neagh stone will burn and flame; and the smoke of it smells like the smoke of wood; 3dly, when burnt it betrays the very grain of wood, with the other vessels belonging to vegetables. But what confirms me above all, that these stones were once wood, is, that I have many of them by me of various degrees of petrification, I suppose according to the time they remained in the water, which I could never hear

* Some sorts of wood, on account of the closeness of their grain, must be less liable than others to be penetrated by the petrifying water; but the assertion that holly is the only wood petrifiable in this lake is contradicted by the observations of Mr. Simon. Among these petrifactions, he found some which resembled the wood of the ash, the fir, and the oak, as well as the holly.

justly determined; some that have clearly lost the colour of wood, and are become perfectly black, and very hard; others that are not so black nor hard; but one more especially was sent me about a year ago, which is a parallelopiped of about 4 inches long and an inch thick, cut I suppose whilst wood into that shape purposely, whose outward coat is very black and smooth, but this is merely superficial, for being cleft lengthwise through the middle, I there discovered the whole body perfectly of the colour and grain of holly, as I can scrape it with my nail; but what was most surprising in it, was the discovery of the pith, as plainly and as perfectly distinct in colour and texture from the rest (but it also was petrified) as it could possibly have been seen in the natural wood.

It is observed, that this petrifying quality is not equally diffused throughout the whole Lough, which is about 15 or 16 miles long, and 8 or 9 miles broad in all places; but is most strong about that part where the black water, (a river so called) empties itself into this Lough, that is, about the south-west corner; as likewise it is said to be stronger about the edges of the Lough, than further into the water.

It was queried a while ago by an ingenious and learned man of the Royal Society, Mr. Halley, whether Lough Neagh stone were not magnetical, for he was told it was; but upon trial I find it is not; for it will not stir a needle, or steel filings, neither will it apply to the magnet, in powder or calcined.*

On a Strange Well, and some Antiquities found at Kirkbythore, in Westmoreland. By Mr. Thomas Machel. N° 158, p. 555.

A well was lately discovered in the street of Kirkbythore, by the Bridge-end, on the north-side of the river Trout-Beck, and is about 10 yards distant from it, in the common road, and the same distance from the great Roman causey, which leads to Carlisle. It was covered over with a plank of wood, about 9 inches thick; but decayed and macerated to the colour and consistence of a peat or turf; above this was gravel and pavement about a yard thick, which being relaxed by the late thaw and swelling of the spring, easily gave way to a horse's foot.

Instead of walls there were two large wooden vessels, one on another, like

* In a letter dated Nov. 1684, and inserted in No. 166 in this Vol. Mr. Molyneux retracts his assertion, that the Lough Neagh stone is not attracted by the magnet, either in its crude state or when calcined. "That it will not do so *crude*, (to quote his own words) I still affirm; but that it does not apply *calcined*, I must retract; for I find by further trial, that it applies calcined most briskly, and in great quantities, to the magnet: the occasion of my former error being, that I did not calcine it long enough."

hogsheads, or wine-pipes, with bung-holes in them, about three inches diameter. They were made of fir, above an inch thick; each of them 6 feet deep, and at the heads 8 feet in circumference; and the compass in the middle 10 feet 4 inches. The wood was sound, but saturated with water.

At the bottom about 5 yards deep, were 4 planks of wood in a quadrangular posture, supported with a stone at every corner, to bear up the fabric, and let in water through the gravel and sand, which lay loose in the bottom about a yard deeper, as was tried by a spit, and these also sound, though the cover was rotten, because less exposed to the air.

There was only found in the bottom Fabritian plate, old earthen vessels, with pieces of urns, one piece of a drinking glass, and several sandals, which may serve to show the antiquity of it.

The earthen vessels were of very fine texture, of a brick colour, and in several forms; but most of them were like a basin or posset-cup; the bowl was semiglobular, the foot a ring: some were in diameter about 8 inches, and in depth on the inside, more than 3½, some more, some less. They were for the most part, very finely embossed; but three more especially, viz. 1. One with a vine branch, having a figure in every turning; and first, a naked man standing on his left foot, the other leg a-cross, and holding his left hand down towards his back, his right toward his belly, with a branch of laurel of three sprigs in it, one of which turns up towards his face, over the crook or bending of his arm; and at his feet is a branch of laurel, and a blossom or flower: in the next is a vine-leaf, two blossoms at the bottom; and at the top two peacocks looking backward: in the next is Victory, as I take it, viz. an angel or genius holding in its right hand a garland of laurel, the arm stretched out, and the face looking towards the man; in the left a sprig of the same tree, two sprigs likewise at the foot, with flowers or blossoms, and one flower in the middle between the garland and one of those sprigs. In the next is a vine-leaf the same as before, then after that a victory, and so by turns, till it ends with a vine-leaf next the man. Upon it are also some Gothic characters of the smaller sort, but dim and obscure. Another of these pots is adorned with circles and semicircles; in one of these circles is the figure of a man, sitting on a plinth or square stone; in all the rest are fluttering genii. In some of the semicircles are lions and goats, all single, and running: and near the bottom are stags in course and greyhounds pursuing, with an inscription, in Gothic characters of the larger sort, which seem to be the word Paulini. An inscription, in the like characters, is to be seen at Burrowbridge, and published by Dr. Lister. And the owners have been careful in preserving these; for this and some others, having been broken, are soldered with lead.

The third sort is still far more beautiful than any of the rest, being adorned with greyhounds very well moulded, and in full pursuit of stags and hinds, and the wild boar; with the same inscription as in the others. There were several other broken inscriptions; and one on the inside of the bottom of a plain dish or platter.

As to the glass, there was but a very small fragment of it, so that I can scarcely guess the figure, but I think it a flute glass, made like a tunnel or spire inverted; and it is remarkable that it hath been as thick as a barley corn.

The urns were of a leaden colour, inclining to black; one had been large, 2 inches thick in the side of the pot, but how large I know not, for there was a sherd only brought to me like half a shield: I have now the top also, being in diameter from outside to outside 7 inches, of which the roll is 2 inches, and the mouth 3 inches. The neck still straiter, and only one-third of an inch thick. And many such are found at this town, some of which have ears or handles as thick as my arm-wrist, and their heads and mouths much of this size, but thicker and stronger, into which I could not thrust my hand. But the other was almost entire, though a very small one; in depth $8\frac{1}{2}$ inches, the diameter of it $6\frac{1}{2}$ inches, or in circumference $19\frac{1}{2}$; in the mouth almost $3\frac{1}{2}$; in the neck somewhat more than $2\frac{1}{2}$ diameter: the bottom near the size of the top, excepting the ring; and the body the fifth part of an inch in thickness: but thicker somewhat at the top and the bottom.

Of the sandals, some were for men, some for women, and some for children; all shaped by their feet, spreading more to the outside than to the inner; some were very large, and some very crooked. The leather was fresh of which they were made, but very tender when it came to be spread on a last. Each consisted of three principal parts; an upper leather, or rather heel piece, with 2 tabs or latchets on each side; an inner sole $11\frac{1}{4}$ inches long, $3\frac{3}{4}$ broad, consisting of 3 or 4 soles stitched together with leather thongs, and an outer sole of 2, stuck full of nails with little round heads, so decayed and rotten that I could scarcely discern them to be iron, plated on the inside; and fixed to the upper leather (3 fold in the heel piece) between them, and sowed with leather, or rather tacked, which the iron nails help to defend. Yet I think some women's, of the better sort, had no nails at all. And of these there is one well worthy observation, of leather like Spanish, and curious workmanship, being exactly stitched down round about the fore piece $2\frac{1}{3}$ inches long with a pretty label, of the same piece, hanging from it, for fashion sake surely; since it is too slender to be of any use. As small in the string as the 15th part of an inch; the tongue at the end half an inch broad, one inch long between the two flourishes, which is the length of the small string, 8 parts in 10 of an inch, the 2 flourishes take up the rest. They are very different from the brogues of Ireland, and the Highlands of Scotland, for these latter have only 1 sole, but the former have 6.

Abstract of a Letter from John Evelyn, Esq. concerning the Damage done to his Gardens by the preceding Winter. N° 158, p. 559.

The past winter has been most severe in my gardens and grounds, and where it could attack the more defensible and such as were inclosed, it has ravaged all that lay open and were abroad, without any mercy.

As to timber trees, I have not many here of any considerable age or stature, except a few elms; but these have not suffered as the great oaks have done, nor do I find among many of the former, which I have planted, and that are now about 25 and 30 years standing, any of them touched; the same I observe of limes, walnuts, ash, beech, hornbeams, birch, chesnuts, and other foresters; but it seems the rifting so much complained of has happened chiefly among the large trees, especially oaks; Lords Weymouth, Chesterfield, Ferrers, Sir William Fermor, and others concerned in the same calamity, have all complained, because of their distant habitations. But if rightly I remember, one of these noble persons lately told me, that since the thaw, the trees which were exceedingly split, were come together and closed again; but that they are really as solid as before I doubt will not appear, when they shall come to be examined by the axe, and converted to use; nor has this accident happened only to standing timber, but to that which has been felled and seasoned. So much for our indigenæ. As for exotics, I fear my cork trees will hardly recover, but the spring is yet so very backward, that I cannot pronounce any thing positively, especially of such whose bark is very thick and rugged, such as is the cork, enzina, and divers of the resinous trees. The Constantinopolitan or horse chesnut is turgid with buds, and ready to unfold its leaf. My cedars I think are lost; the ilex and scarlet oak not so; the arbutus doubtful, and so are bays, but some will escape, and most of them repullulate and spring afresh, if cut down near the earth at the latter end of the month; the Scotch fir, spruce, and white Spanish, have received no damage this winter; I cannot say the same of the pine which bears the greater cone, but other Norways and pinasters are fresh; laurel is only discoloured, and some of the woody branches mortified, which being cut to the quick will soon put forth again, it being a succulent plant. Among our shrubs, rosemary is entirely lost, and to my great sorrow; because I had not only beautiful hedges of it, but sufficient to afford me flowers for making a considerable quantity of hungary water; so universal, I fear, is the destruction of this plant, not only over England but the neighbouring countries more southward, that we must raise our next hopes from the seed. Atriplex or sea purslane, of which I had a hedge, has also perished, and so another of French furzes; the cypresses are most of them destroyed, especially such as were kept shorn in pyramids; but amongst many some will escape, after they are well switched, that is, with a tough hazel or other wand, to beat off their dead and dusty leaves, which growing much closer than other shrubs, hinder the air and dews from refreshing the interior parts. This discipline I use to all my tonsile shrubs with good success, as often as a winter parches them. The berry bearing savine has not suffered in the least; it perfectly resembles the cypress, and grows very tall and thick. I think the arbor thuya is alive, and so is the American acacia, acanthus, paliurus, pomegranate; my laurustinus looks suspiciously; some large and old alaternuses are killed, especially such as were more exposed to the sun, whereas those that grow in the shade escape. Most of these will yet revive at the root, being cut close to the ground; the phyllireas angusti, et serratifolios have hardly been sensible of the least impression, more than tarnishing of their leaves; no more have the Spanish jasmines, and Persian. I would in the mean time advise such as have suffered detriment in their green-houses not to despair, when they see the leaves off of their myrtles, oranges, oleanders, jasmines, and other precious shrubs, russet or altogether shrivelled and falling, but to cut them to the quick, plaster the wounds, and plunge their cases and pots, trimmed with fresh mould, &c. in a warm bed, carefully refreshed, shaded, aired, and treated as sick patients, and as the prudent gardener best knows how. But above all, that he be sure not to expose them till these eastern winds be qualified; for they are deadly to all our plants abroad, and frequently do us more prejudice than the most churlish winters, as commonly finishing the destruction of what the frosts have spared, nor are we to be flattered with a warm day or two, which are apt to tempt gardeners to set out their plants before the end of April, or that we find the mulberry put forth, which is certainly the most faithful monitor, nor should we indeed cut or transplant any of the perennials, till of themselves they begin to sprout.

I need say nothing of holly, yew, box, juniper, &c. hardy and indigenous, and yet I find that a holly standard of near 100 years old is drooping and of doubtful aspect, and a very beautiful hedge, clipped about Michaelmas, is mortified near a foot beneath the top, and in some places to the very ground. This hedge also grows against the south, and is very russet, while the contrary side is as fresh and green as ever; and in all other places of my plantations that are shaded, the unshorn hollies maintain their verdure.

Among the fruit-trees and murals none seem to have suffered except figs;

but they being cut down, will spring again at the root. The vines have escaped, and of the esculent plants and sallads most, except artichokes, which are universally lost; and my samphire is all rotted to the very root; how to repair my loss I know not; for I could never make any of the seed which came from the rock samphire to grow.

The arborescent and other sedums, aloes, &c. though housed, perished with me; but the yucca and opuntia escaped; tulips many are lost, and so the Constantinople narcissus, and such tuberosæ as were not kept in the chimney corner, where was continual fire: some anemonies appear, but I believe many are rotted.

My tortoise, which by his constant burying himself in the earth at approach of winter, I reckon a kind of plant-animal; happening to be obstructed by a vine-root from mining to the depth he was usually wont to inter, is found quite dead, after having many years escaped the severest winter. Of fish I have lost very few; and the nightingale, which being a short winged bird, and exceedingly fat, at the time of the year we commonly suppose them to change the climate, whereas indeed they are then hardly able to fly 100 yards, they are as brisk as ever, nor do I think they alter their summer stations, whatever become of them all winter. I know not yet of any body who has given tolerable satisfaction in this particular amongst our ornithologists.

A Conjecture on under Currents at the Straits Mouth and other Places. By Tho. Smith, D.D. F.R.S. N° 158, p. 564.

In the Offing, between the North and South Foreland, it runs tide and half tide, that is, it is either ebbing water or flood on the shore, in that part of the Downs 3 hours, which is, grossly speaking, the time of half a tide, before it is so off at sea. The reason of this diversity of tides I take to be from the meeting of the 2 seas in that narrow strait. Often when the wind has blown hard at N.E. or at W. or W. and by S. there has happened an alteration of the tides in the Thames, which ignorant people have mistakingly reckoned a prodigy. And, it is a most certain observation, that where it flows tide and half tide, though the tide of flood runs aloft, yet the tide of ebb runs under foot, that is, close by the ground; and so at the tide of ebb, it will flow under foot.

Now, as to the Straits, there is a vast draught of water poured continually out of the Atlantic into the Mediterranean; the mouth or entrance of which between Cape Spartel or Sprat, as the seamen call it, and Cape Trafalgar, may be near 7 leagues wide, the current setting strong into it, and not losing its force till it runs as far as Malaga, which is about 20 leagues within the Straits. By the benefit of this current, though the wind be contrary, if it does not overblow, ships easily turn into the Gut, as they term the narrow passage, which is about 20 miles in length. At the end of which are 2 towns, Gibraltar on the coast of Spain, which gives denomination to the strait, and Ceuta on the Barbary coast; at which places Hercules is supposed to have set up his pillars. What becomes of this great quantity of water poured in this way, and of that which runs from the Euxine into the Bosphorus and Propontis, and carried at last through the Hellespont into the Ægean or Archipelago, is a curious speculation, and has exercised the ingenuity of philosophers and navigators. For there is no sensible rising of the water all along the Barbary coast, even down to Alexandria, the land beyond Tripoli, and that of Egypt lying very low, and easily to be overflowed. They observe, indeed, that the water rises 3 feet or 3 feet and $\frac{1}{2}$ in the gulf of Venice, and as much, or very near as much, all along the river of Genoa, as far as the river Arno; but this rather adds to the wonder.

I here omit to speak at large of the several hypotheses which have been invented to solve this difficulty; such as subterraneous vents, cavities, and indraughts, exhalations by the sun beams, the running out of the water on the African side, as if there were a kind of circular motion of the water, and that it only flowed in upon the Christian shore, which latter I consider as a mere fancy, and contrary to all observation.

My conjecture is, that there is an under-current, by which as great a quantity of water is carried out as comes flowing in. To confirm which, besides what I have said above about the difference of tides in the offing, and at the shore in the Downs, which necessarily supposes an under-current, I shall present you with an instance of the like nature in the Baltic sound, as I received it from an able seaman, who was at the making of the trial. He told me, that being there in one of the king's frigates, they went with their pinnace into the mid stream, and were carried violently by the current; that soon after they sunk a bucket with a large cannon ball, to a certain depth of water, which gave check to the boat's motion, and sinking it still lower and lower, the boat was driven ahead to windward against the upper current: the current aloft, as he added, not being 4 or 5 fathom deep, and that the lower the bucket was let fall, they found the under current the stronger.

The Longitudes, Latitudes, Right Ascensions, and Declinations of the chief Fixed Stars, according to the best Observers; in a Letter from Mr. Edward Bernard to the Rev. Dr. Rob. Huntington, Provost of Trinity College, near Dublin, in Ireland. N° 158, p. 567.

These tables of the places of some of the principal fixed stars, collected out of the tables and writings of many of the most noted observers, of different ages, are omitted, as of little or no use now, in the present improved state of the astronomical science and observations.

An Abstract of a Letter from Liege, concerning a strange Preservation of 4 Men in a Mine 24 Days without any Food, April 9, 1684. N° 158, p. 577.

This accident happened by a large collection of water bursting into the works, which filled all the lower parts, and prevented these men, standing on an ascent, from escaping, till after 24 days; when the water had been drawn off again, the men were found living, having had nothing to take all that time except water.

Extract of a Letter of Mr. Heathcot to Mr. Flamsteed from Cabo Corso Castle on the Coast of Guinea, of Dec. 14, 1683, *concerning the Tide on that Coast, Variation of the Needle, &c.* N° 158, p. 578.

The sea runs here along the shore continually to the eastward, at a very great rate, except at full and change, for then it runs to westward, or at least slackens very much. November 24th, I took the time of the high-water at the castle, as near as I could, at 3h. 30m. p.m. it flowed about 6 feet. The magnetical variation here is 3° 49′ from the north to the westward.

Martini Lister, M. D. e S.R. Lond. de Fontibus Medicatis Angliæ Exercitatio altera. Lond. in 8*vo.* 1684. N° 158, p. 579.

A continuation of a work formerly noticed. This second part abounds in physiological and pathological speculations, little worth the attention of medical men of the present days.

Nouvelle Maniere de fortifier les Places, par M. Blondel, &c. a la Haye, 1684. N° 158, p. 586.

This author having considered the art of fortification as it is at present practised, found it not to have received improvements proportionable to the art of attacking. This appeared by the success of the king of France, who usually made himself master of those towns in 10 days, which formerly were held capable of resisting as many months. There is therefore here laid down a new way of fortification, which may take away from the besiegers the advantages they draw from their number of men and cannon, and give the besieged room for making use of more cannon, and supplying the want of men. This is principally done by a vast enlargement of the flancks and demigorges, so as to be capable of 3 very large batteries, one over another, and sometimes a cavalier at top, all which in some figures may hold 100 or 120 cannon, sufficient to dismount any battery that may be raised on the counterscarp.

Two remarkable Cases relating to Vision, communicated by Wm. Briggs, M.D. Fellow of the College of Physicians, and Physician of St. Thomas's Hospital. N° 159, p. 559.

I received some time since an account of a remarkable case of vision, from Dr. Parham, physician in Norwich; in 2 letters, of which the following are abstracts.

First letter: ' I was lately in Suffolk, and there met with a curious and rare case relating to vision. A gentleman gave me the history of it, and his servant who was the subject of it, being then in the house, I had an opportunity of fully informing myself.

The young man may be near 20 years of age, and during the day time has as good a sight as any of the family, if not better; he distinguishes any object at as great a distance; sees either near at hand or far off, or at any intermediate distance, as well as any person; but when twilight once sets in, he is as blind as a beetle; sees nothing; runs against gates, posts or rails, and cannot without great difficulty direct himself. Neither does this seem to be from the want of any vigorous influence from the rays of the sun; for when he is at home and in the house, he tumbles over stools, runs his head against doors, and can hardly see or direct himself either by the greater or less lights of the fire and candle; but commits pleasant mistakes, is apt to compliment your feet as readily as present you steadily with a glass of wine, runs his head against his master's foot when he is to pull off his boots, &c.

There is no disease in the organ that can be observed; no vertigo or distemper in the head, to interrupt or any way intercept the spirits in their motions; but to all appearance the fabric of the organ is very true and exact. I observed the eye both by day and night, but could perceive no fault in any part; neither do I think the suspicion well grounded, that he sees better than others by daylight, because of the vigour and plenty of rays, for then he might have the same benefit from good fires and candles to invigorate the organ, and contract the pupil; but it seems rather to depend on some of the humours, though I cannot tell at present on which, having not had time since to consider of it. I tried him with spectacles for a variety of sights, but they did him no service by either lights."

In the second letter are contained answers to certain questions, to the following effect. He was in this same state from the first time he was able to take notice of things, and it came on without distempers. That it comes gradually upon him as day-light declines. The various aspects of the moon cause no difference. He feels no pain by fire or candle-light. He finds no difference between winter and summer. He never observed any difference by taking cold. He sweats much at work, but finds no inconvenience, neither observes any difference as to his sight, on those days when he works hard, or not.

The case now mentioned, though indeed in a different sense from that of Hippocrates, is called by later writers nyctalopia, or nocturna cæcitas, and is accordingly described, with the remedies for it, by Galen, Pliny, Forestus, Sennertus, and Joel. Celsus mentions it under the title of imbecilitas oculorum; but none of all these have given so exact and full a history of it as in the foregoing relation.*

To this unusual case I shall subjoin another relating to the same subject, which I lately had the opportunity of observing here in St. Thomas's Hospital, together with my friend Dr. William Dawkins, in a patient we had for some time under our hands.

The case was this; Daniel Wright, aged about 19 years, of a sanguine and plethoric constitution, about the end of the year 1683, was seized with a dizziness and pain in the upper part of the head, which he could impute to nothing but the excessive cold weather at that time. Having applied to an ignorant pretender to physic, a plaster for his head was only ordered at that time, without any evacuations. The patient upon this grew much worse, the pains of his head more fixed and girding, to which succeeded convulsive fits, which were accompanied afterwards with a tremor in his arms and legs; after which, all objects appeared double to him. After this poor young man had been thus tortured by this empiric and his distemper for about 3 months, he was taken into our hospital.

In this difficult and extraordinary case, where the patient had been so long afflicted, and the fits grew daily worse upon him, we endeavoured by all ways we could to relieve him. Accordingly we ordered the cephalic pills, and an electuary which we use in the hospital in epileptic cases, from which he received much benefit: he was also at intervals bled in the jugulars, and in the hæmorrhoids, and by leeches, which also relieved him; his head was shaved, blisters applied to his neck, and a seton made sometime after, &c. But about two

* This affection, night-blindness, is common in the tropical climates. It sometimes recurs periodically, of which a remarkable instance has been recorded by the late Dr. Heberden.

months after he had been under our care, a gutta serena seized on his right eye, so that he could not see at all on that side; but then the duplicity ceased, and he saw all objects single again as before.

In the mean time the left eye being still in danger, we further ordered, besides the continuance of the former electuary, a fontanel about the meeting of the sagittal and lambdoid sutures, and cupping-glasses to his neck and shoulders, that nothing might be left unattempted that we thought requisite; but it seems these last were omitted, and whilst we were thus solicitous about preserving his other eye, a severe fit seized him, soon after which he died.

The Description and Use of the Pores in the Skin of the Hands and Feet. By Neh. Grew, M. D. Fellow of the College of Physicians, and F. R. S. N° 159, p. 566.

The pores in the hands and feet are very remarkable; both in respect of their position and their amplitude. For if any one will but take the pains, with an indifferent glass, to survey the palm of his hand very well washed with a ball; he may perceive innumerable little ridges, of equal size and distance, and every where running parallel to each other. And especially on the ends and first joints of the fingers and thumb, on the top of the ball, and near the root of the thumb a little above the wrist. In all which places, they are very regularly disposed into spherical triangles, and ellipses; as in fig. 3, pl. 1. On these ridges stand the pores, all in even rows, and of such a magnitude, as to be visible to a good eye, without a glass. But being viewed with one, every pore looks like a little fountain, and the sweat may be seen to stand therein, as clear as rock water, and as often as it is wiped off, to spring up within them again.

What nature intends in the position of these ridges, is, that they may the better suit with the use and motion of the hand: those of the lower side of every triangle, to the bending in or clutching of the fingers; and those of the other two sides, and of the ellipses, to the pressure of the hand or fingers ends against any body, requiring them to yield to the right and left. On these ridges the pores are very providently placed, and not in the furrows which lie between them; that so their structure might be the stronger, and less liable to be depraved by compression; whereby only the furrows are dilated or contracted, the ridges constantly maintaining themselves, and so the pores unaltered. For the same reason, the pores are also very large, that they may still be the better preserved, though the skin be ever so much compressed and condensed, by the constant use and labour of the hand. So likewise those of the feet, notwithstanding the compression of the skin by the weight of the whole body.

These pores being thus made and secured, form a very convenient and open passage for the discharge of the more noxious and perspirable parts of the blood, which by the continual use of the hands and feet, are plentifully brought into them. Whence it is that the sweat of the feet, in many people, is much more offensive than that of any other part of the body. And that many hypochondriacal men, and hysterical women, have almost a continual burning in the soles of their feet, and the palms of their hands.

Abstract of a Letter from Mr. Anthony Leuwenhoeck, at Delft, dated Sept. 17, 1683: *Containing some Microscopical Observations, about Animals in the Scurf of the Teeth, the substance called Worms in the Nose, and the Cuticula consisting of Scales.* N° 159, p. 568.

Though my teeth are kept usually very clean, yet when I view them in a magnifying glass, I find growing between them a little white matter, as thick as wetted flour: in this substance though I could not perceive any motion, I judged there might probably be living creatures. I therefore took some of this flour, and mixed it either with pure rain water wherein were no animals, or else with some of my spittle, having no animals nor air bubbles to cause a motion in it; and then to my great surprize perceived that the aforesaid matter contained very many small living animals, which moved themselves very strangely. The largest sort were not numerous, but their motion strong and nimble, darting themselves through the water or spittle, as a jack or pike does through the water. The 2d sort spun about like a top, and were more in number than the first. In the 3d sort I could not well distinguish the figure, for sometimes it seemed to be an oval, and other times a circle: these were exceedingly small, and so swift, that I can compare them to nothing better than a swarm of flies or gnats, flying and turning among each other in a small space. Of this sort I believe there might be many thousands in a quantity of water no larger than a sand, though the flour were but the 9th part of the water or spittle containing it. Besides these animals, there were a great quantity of streaks or threads of different lengths, but of like thickness, lying confusedly together, some bent, and others straight. These had no motion or life in them.

I observed the spittle of two several women, whose teeth were kept clean, and there were no animals in the spittle; but the meal between the teeth, being mixed with water, as before, I found the animals above described, as also the long particles. The spittle of a child of 8 years old had no living creatures in it; but the meal between the teeth had a great many of the animals above described, as also the streaks. The spittle of an old man that had lived soberly, had no animals in it; but the substance upon and between his teeth, had a great many living creatures swimming nimbler than I had hitherto seen. The largest sort were numerous, and as they moved bent themselves. The other sorts of animals were in great numbers, insomuch that though the meal were little, yet the water it was mixed with seemed to be all alive: there were also the long threads above-mentioned. The spittle of another old man, a toper, was like the former, but the animals in the scurf on the teeth were not all killed by his continual taking brandy, wine, and tobacco; for I found a few living animals of the 3d sort, and in the scurf between the teeth I found many more small animals of the two smaller sorts.

I took in my mouth some very strong wine-vinegar; then closing my teeth, I gargled and rinsed them very well with the vinegar; and afterwards I washed them very well with fair water; but there were innumerable quantities of animals still remaining in the scurf on the teeth, but most in that between the teeth, and very few animals of the first sort. I took a very little wine-vinegar, and mixed it with the water in which the scurf was dissolved; upon which the animals died presently. From hence I conclude, that the vinegar with which I washed my teeth, killed only those animals which were on the outside of the scurf, but did not pass through the whole substance of it.

The number of these animals in the scurf of a man's teeth are so many, that I believe they exceed the number of men in a kingdom. For on the examination of a small parcel of it, no thicker than a horse-hair, I found so many living animals in it, that I guess there might have been 1000 in a quantity of matter no larger than the $\frac{1}{100}$ part of a sand.

A certain man being said to have worms taken out of his face, I took a quantity of these imagined worms, which I laid upon a clean glass, that I might view them at my leisure. I found them not to differ from what I gave an account of in my letter of the 4th of November, 1681; unless it were that some of the hairs in these supposed worms were so tender, that they broke in two on the least touch. Other worms seemed to be a bundle of hairs, but when I went to separate them, it was just as if I had touched a soft fat body. I squeezed some black specks out of the thick of my own nose, which I saw to be bundles of hairs, I then took out hairs from one of them to the number of 36. I took the worms out of the noses of 2 other persons, and I found the number of hairs in a bundle, to be from 3, 4, 5, 6, and 9 to 25, and 30. When the worms lay deepest in the nose, they seldom contained any hairs, unless the person they came from were very black, and then the hairs were more easily perceivable. In the pressing out of worms, I could tell whether there were hairs in them or not;

for if the substance came out straight, then there were always hairs; but if bended none.

In the year 1674, I asserted that the cuticula, or upper skin of a body, consists of round particles or scales. I then saw by a common microscope the parts of the scales appearing to the eye as if they were round, lying close in order, and so small that a sand would cover 200 or 250 of them. But examining them since by a glass which magnifies more, I am satisfied that they are not made out of the grosser part of the moisture or substance which is evaporated out of the body, as I formerly thought, but are mere scales, such as grow on the outward skin of a fish, and called fish-scales. These scales lie on our body just as they do upon fishes, the most part of them are five-sided, and are very thin, for I judge their breadth is above 25 times more than their thickness. They lie three deep on the body, every part being covered with 3 scales successively, though not above $\frac{1}{3}$ part of a scale discovers itself to the eye, the other $\frac{2}{3}$ parts being hid by the other scales.

The scales of fishes also appear but in part to the eye; but it is very remarkable, that though fishes never change their scales, yet men do often; particularly I instance in myself at this time, being the 1st of September, that the scales came off me not one by one, but 1000 in a cluster. When I pluck off a scale from my body which sticks fast, and perhaps is but newly grown, there comes blood after it, or at least there remains a red spot.

It is easy to conceive how a louse, flea, or other insect may thrust his sting or snout into the skin; for they need not do it through the scales, but between the plates or mails. From hence also may be perceived, that there are no pores in the cuticula, for the conveying out of sweat, because that may ouze out from between the scales, though they stick never so close together, without supposing that there are channels made for its passage. Let us only reckon how many vacuities a scale has, whereby it is nourished so as to grow, and that in the space of $\frac{1}{3}$ part of a scale there may be 100 such vacuities, through which the humours of the body may pass, and that 200 such parts of a scale may be covered with a sand. It will follow then, that the body may exhale out of 20,000 places in a quantity no larger then what a sand will cover.

A Letter from Mr. John Collins, to Dr. John Wallis, Savilian Professor of Geometry in the University of Oxford, respecting some Defects in Algebra. N° 159, p. 575.

To describe the locus of a cubic equation.

A Cardanic equation convenient for the purpose, viz. such as shall have the dioristic limits rational, have the coefficient of the roots to be the triple of a square number, such is $a^3 - 48\,a = N$.

Assume a rank of roots in Arithmetical progression, and raise resolvends to them. Thus:

Roots	a^2 −	$48a$	=	N. or resolvends.
1 ..	1 −	48	= −	47
2 ..	8 −	96	= −	88
3 ..	27 −	144	= −	117
4 ..	64 −	192	= −	128
5 ..	125 −	240	= −	115
6 ..	216 −	288	= −	72
7 ..	343 −	336	= +	7
8 ..	512 −	384	= +	128
9 ..	729 −	432	= +	297

Draw a base line, and a perpendicular to it; then from O in the base line set off the negative resolvends downwards, and the affirmative ones upwards, and raise their roots upon them as ordinates; then a curve passing through the same, is one half of the curve or locus on the right hand, for affirmative roots; and the other half on the left hand is described in the same manner, by assuming a rank of negative roots, and raising resolvends to them. See the curve fig. 1, pl. 2.

And 16, the third part of the coefficient of the roots, cubed, is equal to the square of 64, half the resolvend, or dioristic limit. Which, in composing Cardan's canon, is always subtracted from the square of half the absolute, as in the example following:

If I were to find the root belonging to the resolvend	297.
The square of its half is	$22052\frac{1}{4}$
The square of 64 half the dioristic limit	4096
The difference is	$17956\frac{1}{4}$

And the rule is $148\frac{1}{2} + \sqrt{17956\frac{1}{4}}$,
$148\frac{1}{2} - \sqrt{17956\frac{1}{4}}$.

That is, in a quadratic equation, if 297 were the sum of the two roots, and 64 the root of the rectangle: then if from the square of half the sum, the rectangle be subducted, there remains the square of half the difference of the roots; and giving them an universal cube root, it is $\sqrt[3]{148\frac{1}{2} + \sqrt{17956\frac{1}{4}}} + \sqrt[3]{148\frac{1}{2} - \sqrt{179856\frac{1}{4}}} = 9$ the root sought.

In the scheme, QB and QP may denote the roots of Cardan's binomials, that run infinitely upward, and terminate at Q.

As to Cardan's Rules.—The limits are of two kinds; viz. either the base limits when the resolvend is 0, and the equation falls a degree lower: or the dioristic limits, by which a pair of roots gain or lose their possibility.

Cardan's canons are but the sum of the roots of a solid quadratic equation, arising out of half the dioristic limit as the root of the rectangle and the resolvend as the sum.

If the roots of those binomials are separately set off, as ordinates on their resolvends, they produce curves infinitely continued upward, and meeting in a point bisecting the root that is equal to a pair of equal roots, when the equation is just limited, or dioristic, in the figure at Q.

If these binomials are laid off as ordinates to their resolvends, Mr. Newton upon sudden thoughts, supposed they may describe both sides of a hyperbola.

If these binomial curves be continued downward, and separately found, should always added make the root of a cubic equation capable of 3 roots; then Cardan's impossible or negative roots are proved possible, and we only in ignorance how to extract them.

Assume any root within the limits of 3 possible roots, and raise a resolvend to it; and when you have done, by Cardan's rules improved, you may find that root, and, with a little varying the same, both the other roots (for every number or magnitude capable of a cube root, is capable of two more) as follows:

After having obtained the cube roots of Cardan's binomials, according to Van Schooten, in Descartes, or Kersey, if you change the signs of the rational parts of those roots, as also the signs of the radical parts, and multiply these last parts by 3, the results are also roots of the cubic equation first proposed.

Example.—Of the equation $aaa - 21a - 20 = 0$, the cube roots of the binomials are $+ 2\frac{1}{2} + \sqrt{- \frac{3}{4}}$, and $+ 2\frac{1}{2} - \sqrt{- \frac{3}{4}}$; and their sum is the root sought $= + 5$. Also, the other two roots are $- 2\frac{1}{2} + \sqrt{2\frac{1}{4}}$, and $- 2\frac{1}{2} - \sqrt{2\frac{1}{4}}$.

Also, in this equation $a^3 - 60\,a - 32 = 0$, the binomial roots are $+ 4 + \sqrt{- 4}$, and $+ 4 - \sqrt{- 4}$. Hence the root sought is $+ 8$; and the other two roots are $- 4 + \sqrt{+ 12}$, and $- 4 - \sqrt{+ 12}$.

If the roots in the former section be assumed in arithmetical progression, and the equation with its several resolvends be depressed, there will come out a regular series of quadratic equations; whence an easy method will arise of writing down such ranks as, multiplied by an arithmetical progression, shall always produce the same cubic equation, the resolvend only varying.

Let the roots of this series of quadratics be found as usual in binomials; let these binomials be cubed; and then let it be observed, whether the results are constant portions of the square of the resolvend and of the dioristic limit; and if so, Cardan's rules will have their defect supplied.

If the binomial roots of a series of quadratics be squaredly squared, and those results be constant portions of the cube of the resolvend, and the dioristic limit; it will be certain there may be general surd canons for equations of the 4th dimension; and Monsieur Cavalerius, (now in London) positively asserts, he has a general method to obtain them for all dimensions.

As Cardan's are surd canons derived from the resolvend and dioristic limit, so it may be worthy consideration, whether other surd canons do not arise out of the limits of those particular cases and equations, and whether the glimpse of a general method might thence be derived, for all other equations, though encumbered with negative quantities? which Mr. Gregory, a little before his death, said he had attained.

The learned Dr. Pell has often asserted, that after the limits of an equation are once obtained, then it is easy to find all the roots to any resolvend proposed.

Suppose I should propound two cubic or biquadratic equations, in both of which all the signs are +. It is propounded out of these two, to derive a third equation, whose root shall be the sum, difference, or rectangle of the roots of the two equations propounded. This Mr. Gregory, a little before his death, wrote that he had obtained, and in the following series for finding the half of an hyperbolic logarithm I suppose made use of.

From a number proposed subtract an unit, and let that be numerator; also to it add an unit, and let that be denominator: and call that fraction N.

Then $\frac{1}{1}N + \frac{1}{3}N^3 + \frac{1}{5}N^5 + \frac{1}{7}N^7 + \frac{1}{9}N^9 + \frac{1}{11}N^{11} + \frac{1}{13}N^{13}$, &c. is equal to half the hyperbolic logarithm sought.

Example in the Number 2.

		N	
The fraction is $\frac{1}{3}$.	1	3333333 =	3333333
	3	370370 =	123456
	5	41152 =	8230
The rank N is easily	7	4572 =	653
made by dividing every	9	508 =	56
preceding number by 9,	11	56 =	5
	13	6 =	0
			3465733
			2
			6931466

which is the hyperbolic logarithm of 2 sought.

A Cardanic equation, that is a cubic one wanting the second term, may be multiplied or divided by a rank of continual proportionals, so as to render the coefficient of the roots canonic, that is, to make it the same with the equations of the table; then find the sine, tangent, or secant of the third part of that arch to which any sine, tangent, or secant is propounded, and so finding the roots in the table, those sought are thence obtained by multiplication or division. Also, the coefficient of the roots may in like manner be rendered an unit, and then the resolvends sought in a table of the sums or differences of the cubes of numbers and their roots, shall help to such roots, as multiplied or divided as aforesaid shall be the true roots sought.

Where equations have all their terms affected with the same sign, both + or —, Mr. Newton, and Mr. Gregory deceased, have affirmed they are all reduceable to some pure high power, which is of singular use in the infinite series. And where this cannot be done, a learned German has asserted, that they may be reduced to a higher power with a variable coefficient, which is the root sought with a common addend or subducend. And even this would render an easy tentative logarithmetical way for attaining the root.

Lastly, as to constructions for equations, the following problem seems to be universal. Any two analytic curves, viz. such as have the relation between the base and ordinate expressed by an equation, being given in magnitude and position, and from the points of their intersection ordinates let fall to the axis of either figure, or upon parallels to the said axis, the enquiry is of what equation those ordinates are the roots? Dr. Barrow liked the proposition as well grounded, and left a discourse about doing it in the conic sections, in which there are 3 cases, either the axes are parallel; or being produced concur, beyond the vertexes of the figures, without; or otherwise intersect within the figures. Mr. Gregory entered on the same contemplation, but death deprived us of the benefit of his thoughts.

Of analytic, or geometric curves, there are innumerable sorts, of which I shall mention one or two kinds. Between an arithmetical progression, and its squares, or between its squares and its cubes, or its cubes and biquadrats, there may be interposed as many arithmetical or geometrical means as you please: and thence loci or curves derived; which some call paraboloids or parabolasters; see Gregory's Geometriæ pars universalis.

On the Bridge of St. Esprit, in France. By Mr. Tancred Robinson. Nº 160, p. 584.

This famous Roman bridge is very crooked, bending in several places, and forming many unequal angles, and most so in those places where the stream runs strongest. The arches are very wide, and have their feet secured by 2 pedestals that encompass them. Both these pedestals have their several degrees or ranks of juttings out, like so many rows of stairs, the lowermost order projecting out most, the others being less, and going gradually more in; the second or uppermost pedestal is much less than the first or lowermost, being built a little within its lines of circumference. Between the great arches there are windows, or, as it were, small arches, that come down to the very plane of the second, or uppermost pedestal, dividing the feet of the great arches. From this rude description it appears that the Romans have here contrived every means to break gradually the mighty force of the Rhone, and to render its passage easy and inoffensive to the feet of the great arches; for here we see as many several palisadoes and sluices as may be sufficient to defend this wonderful fabric against all attacks of the torrent. The several ranks of stairs jutting from the pedestals, which are for the most part triangularly built, and well faced with free-stone, oppose and break the stream severally, or after each other, by reason of their various inequalities in jutting out, in case the flood should swell so high, as it frequently does, as to cover both the pedestals, then the small arches dividing the feet of the great ones help to convey the water through, which otherwise might endanger the great arches.

Dr. Lister adds the following remark, viz. What seems to be the foot of the arch is a horizontal arch gradually contracted, every stone being of vast length and wedge-like, laid level with the water.

Abstract of a Letter from Mr. A. Leuwenhoeck of Delft, concerning Scales within the Mouth, the Scaly Child that was shown, the Anatomy of the Slime within the Guts, and the Use of it. N° 160, p. 586.

In a former letter, I said that our skin was covered with scales, I have since that time examined the cuticula of the inside of the mouth, and chiefly that of the under lip, which I find to be covered with scales, larger than those on the body, but thinner. As the scales of our body lying over each other, so as to be 3 deep, are the cause of the skin appearing white; for diaphanous particles laid upon each other, and not too close, make a white; for which reason we see that paper, spittle, beaten glass, and snow, are white; so the scales of our mouth lying but a little over the sides of each other, suffer the redness of the flesh and blood to appear through them, and for this cause the lips and mouth are red.

I have often wondered what should be the cause that a single hair lying on the skin caused so great a tickling. But if the skin be covered with scales, and the outward bark of the hair very rugged, it will not be strange that a hair lying over the edges of one or more scales, should cause a motion in them, which being communicated to the skin, may cause that sensation.

There has been exhibited in this country a child about 10 years old, whose body, as they said, was all covered with fish scales. Having heard much of this wonder, I went to see it, but found it much different from the report of it. For there appeared to my naked eye and microscope no part of the body which I could say was covered with fish scales, but rather with a thick callus, and more especially within the hands and under the feet, upon some parts of the body also there were excrescences, like ridges of warts.

In the room I found a cluster, which my repeated observations confirm to be nothing but natural and ordinary scales, such as bodies are generally covered with. I afterwards put it in water, and let it lie some hours, till the parts would separate with the least touch into 1000 small scales.

I have been a long time desirous to examine the slimy matter which lines the inside of the guts; and so much the rather, becanse it is generally esteemed as a superfluous part, and fit to be removed; whereas on the contrary it appears to me, to be a part instrumental and necessary for the uses of the bowels. I took then this woolly substance, and having cleared it from the excrement as much as I could, I found a great number of very thin blood-vessels, branched out and lying so thick together, that the space of half the diameter of a hair was not void between the branches. Besides the blood-vessels there were also other vessels that had no distinguishable colour, which I suspected to be limphatics or lacteals; I could not discern any membrane that encompassed them, but all about them lay a glutinous clear slime, beset with small globules, which slime and globules I took to be the excrements lying upon the guts, but when I went to scrape gently this slime away, I found that I not only wounded the blood-vessels, but tore away many blood-vessels and other vessels together.

These blood-vessels do not spread their branches on all sides, like the blood-vessels in other parts of the body, but as they lie in a bow, send all their branches inward and none outwards; they also lie so close, that I imagine 10,000 of them may be in the space of an inch square.

I have been doubtful whether the arteries and veins were not in this place joined together; for among all the experience that I have had of the blood-vessels, I never perceived such a probability of anastomosis. For in other places the arteries being variously disseminated for the nourishment of the parts, the veins are so likewise, for carrying the blood back again into the heart; but in this place the arteries going no further than the hollow of the guts, seem to have no other business than to empty themselves into the veins. These observations also make me more than ever reject the opinion, that the extremities of the lacteal and lymphatic vessels have mouths or openings, whereby they receive and take in the chyle out of the guts; for I am persuaded that the extremities of the lacteæ in the guts are as well covered with their coat or membrane in that place, as in other parts of the body, which nevertheless will not hinder the food from passing out of the guts. And as the arteries, whose coats are made up of a thready substance, can strain the blood through them, so the nutritive juice also may pass through the thready coats of the blood, water, and milk-vessels, and in the same manner, the small branches of the veins may take up substances out of the bowels and carry them to the heart. This will not seem strange when we consider, that if a milk, water, or blood-vessel, be 1000 times less than a hair of one's head, the coats of them must needs be very thin, and the threads whereof the coats are made yet thinner. How easy must moisture pass through the sides of such vessels, especially when the matter which is to enter into the vessels is thinner than that which is already contained in them.

It has been objected, that while the passage into the vessels is so open, a quantity of air and wind may also get into them. Now that we may see how the moisture may pass out of the bowels into the veins, and the wind not pass, I made this following trial: I took an ox bladder, blew it up, and let it dry, as fig. 9, pl. 2, ABCD, I then took a piece of a hog's gut made clean, about the length of a span, and tied it up at E; then I put into it water, till the gut was about a quarter full; afterwards I forced in 3 quarters more of air, binding it fast at F. The gut then lay upon the bladder as EF; I then hung them in a chimney, where was made but little fire, and I found that the water in the gut not only moistened the bladder where it touched it, but ran down in 2 channels by the side of the bladder; so that in the space of 16 hours all the moisture in the gut was run out without the least air, nay the gut seemed as stiff as when it was first blown. Let us now compare the guts to the bladder, and the chyle and wind in the guts to the wind and water in the hog's gut, then shall the guts let the moisture pass through them, but not the air.

Among the aforesaid blood-vessels, and other vessels lying within the guts, I saw a matter seeming at first to consist of globules; afterwards it appeared like little guts within the great ones: at length it proved to be short threads, one end of which was partly covered with the aforesaid vessels, and the other end was fastened to a skin or membrane, probably the same with that called by the anatomists the innermost coat of the bowels. This woolly substance I conceive may be of great use; for the threads must be longer and lie closer together, and have little moisture between them, while the gut is empty and crumpled; but when the gut is full and distended with victuals, the threads must be shorter and lie not so close together as when the gut is contracted. By which means the blood-vessels, lacteals, and lymphatics come more easily to receive their liquors into them.

On the Projection of the Threads of Spiders; and on Bees breeding in Cases made of Leaves; also a Viviparous Fly, &c. By Dr. M. Lister. N° 160, p. 592.

I take the forking of some threads to be merely accidental, as it is to our hair, and not designedly done by the animal; for I have observed them exceedingly sleek and smooth. There is indeed a dividing in the projection of the threads of many sorts of spiders, and especially among those which we distinguish by the name of lupi; which tribe is most frequent and particularly delights in sailing, yet this division is of a different nature from forking. These lupi will dart a whole stamen or sheaf at once, consisting of many filaments, yet all of one length, all distinct and divided from each other, until some chance either snap them off or entangle them; but for the most part you may observe, that the longer they grow the more they spread, and appear to a diligent observer like the numerous rays in the tail of a blazing star. As for that which carries them away in the air so swift, it is as formerly hinted, partly their sudden leap, and partly the length and number of the threads projected, the stream of the air and wind beating more forcibly upon them, and partly the posture and managment of their feet, which, at least by some species, I have observed to have been used very like wings or oars, the several legs being sometimes close joined, and other times opened, again bent or extended, &c. according to the several necessities and will of the animal. They cannot strictly be said to fly, as they are carried into the air by external force; but they can, in case the wind suffer them, steer their course, and perhaps mount and descend at pleasure; and as to the rowing themselves along the air, it is observable that they always take their flight backwards, that is, their head looking a contrary way, like a sculler on the river. It is scarcely credible to what height they will mount, which yet is precisely true, and a thing easily to be observed by one that shall fix his eye some time on any part of the heavens, the white webs at a vast distance very distinctly appearing from the azure sky; but this is in autumn only, and that in very fair and calm weather. Divers sorts have divers ways and particularities in performing this surprising phenomenon.

The account given of the bees breeding in cases made of leaves, Philosophical Transactions, N° 65, exactly agrees with what I have observed. I add, that they are not very scrupulous in the choice of those leaves, but will make use even of exotic plants, such as the blue pipe or syringe-tree. There is a very strange economy of nature yet unsolved; the furthermost bee, says Mr. Willughby, makes her way out along the channel through all the intermediate cartridges, and according as these channels run upwards or downwards in the body of the tree, the maggot bee at the farther or upper end of each channel is first laid, and it should seem both hatched and perfected first, and must either wait until the rest be so too, or of necessity, by working through their cases, destroy them. If it be so, it is very strange. But I take it otherwise, and perhaps it will be found by diligent observers hereafter, that that bee which is nearest day, although it be last laid, is yet the first hatched; and I ground my conjecture upon this, that it is probable the eggs in the mother are all fit for laying, or equally ripe and forward, at the time that the first of them was laid, but are not therefore all laid by the dam until she has provided them with meat and a house, each separately, as is the nature of bees; and yet in recompence, the warmth of her body, or rather the daily increasing heat of the summer season, to which the mother bee is continually exposed, whilst the first laid eggs are sheltered in their deep channels, hastens their vitality so much, that they are hatched worms, and begin to feed before the first laid, and consequently are first perfected into bees. But this is conjecture only, and not observation, and to this purpose let me observe to you, that we are not always without our viviparous flies, although in a much colder region than Italy. The first time I took notice of them was the 2d year of the sickness raging in Cambridge, 1666. I have of this sort some by me at present, which I do not find cut or described either in Aldrovandus or Mouffet, though this sort of fly be very frequent with us.* And this was not the only strange phenomenon that I observed among insects, besides other things of nature, particularly that year; for being in harvest-time at Bessenburn in Cambridgeshire, at the house of Mr. W. A. he invited me along with him into the fields, where, says he, "I will show you a wonder;" which, indeed, was so to me, for lifting up the barley cocks with his cane, there appeared millions of maggots on the corn lands, and in their barns too, the floor would be covered with them that fell from the carts. The maggots were about half an inch long, no thicker than a pigeon's feather, of a

* Some few flies are known to be viviparous, the ova hatching internally before the larvæ or maggots are excluded. The most remarkable of these flies is the *musca carnaria*, Linn. Syst. Nat. edit. 12. It is described and accurately figured both by Reaumur and Degeer, and greatly resembles the large blue flesh-fly or *musca vomitoria* of Linnæus, but is of a greyer colour, with red eyes. It deposits its larvæ on meat, &c. like that species, and belongs to the second Linnæan division of the genus, viz. *muscæ pilosæ antennis plumatis.* Degeer describes a smaller species of viviparous fly, so nearly resembling the *musca carnaria* in every thing but size, that the same description as to form and colours might serve for both.

white colour, somewhat shaded with an Isabella or faint yellowish stripes, the length of the worm; they had 14 feet, after the manner of many caterpillars, and I was almost confident would have produced some sort of moth. I took up about a score of them, and put them into a box, but they immediately offended me with an ungrateful and strong stink, which yet is not usual to the caterpillar kind. However I kept them 2 days, but by reason of some apprehensions and fear the ladies had of them, where I then was, and on their intreaties I rid myself of them; I only observed, that the excrements which they voided, were little hard pellets of pure white flour, like that of barley. These and other things might be arguments of the power hot weather has in the hasty quickening the births of insects, as well as producing them; but I conceive it less useful to philosophy to dispute them, than to deliver faithfully matters of fact.

A Letter written to Mr. H. O. concerning some very aged Persons in the North of England. By Dr. M. L. N° 160, p. 597.

This is a short notice of the names and ages of several very old persons, either living or lately deceased, chiefly in the counties of York and Lincoln. They are as follow:

Robert Montgomery, residing in Skipton, aged 126 years.

Mary Allison, of Thorlby, died 1668, aged 108.

J. Sagar, of Burnley, died 1668, aged 112.

Thomas Wiggen, of Carlton, died 1670, aged 108.

Frances Woodworth, of Carlton, died 1662, aged 102.

Two men (their names omitted), father and son, then living, the one 100, and the other 140 years of age.

Account of a Monstrous Child. By M. Chr. Krate. N° 160, p. 599.

Feb. 29, 1684, at a village called Heisaggar, near Hattersleben, in South-Jutland, a soldier's wife was delivered of this monstrous child, fig. 2, pl. 2. It is supposed she had seen some wounded or disfigured body in the same manner, as this child appeared. At the left leg, 1, was to be seen an oblong round piece of flesh, of a brown and blue colour, somewhat sharpened at the extremities, which was joined to the calf of the leg, 2, and could be moved from 1 to 3; the other piece of flesh 4, was of the same colour, but fastened to the leg, so that it could not be displaced. On the right foot it had 6 toes; 7 was like a pistol bullet, which hung loosely to the leg; 8 another bullet somewhat larger. The face looked pretty old, as if it had been of 35 or 40 years of age: 5 and 6 at the forehead there were excrescences like artificial lace. It looked fiercely with the left eye, keeping the other close. Behind the head there was a shape like a hood, or other ornament, which women commonly wear. His arm was figured as the scheme, with several knots or joints. The tail, which was strangely grown out of the back-part, 9, was a quarter of a Zealandish ell long. The mother of this child, about 40 years of age, has had formerly two other sons, now of 7 and 9 years of age, which are very well shaped, and still living. This monster, after it had cried out twice or thrice presently expired.

Præclarissimo et Eruditissimo Viro D. Spon, M. D. et Lugdunensi Anatomico accuratissimo Marcellus Malpighi, S. P. Extracted and translated from the Latin. N° 160, p. 601.

This letter was written in consequence of a notice sent by Dr. Spon to the author, that a new edition of his (Malpighi's) works was about to be printed, wherein the publisher would be glad to insert any additional observations the author might wish to make.

The letter itself relates to two subjects, viz. (1) A preternatural horny excrescence which grew and hung down from the neck of an ox, in the place where the yoke is fastened; and (2) certain morbid appearances observed in the human kidneys.

He describes with much minuteness the aforesaid horn, the structure and appearances of which, he thinks, throw great light on the formation and growth of the hoofs of quadrupeds; a subject on which he had before offered some reflections in his treatise on the organ of touch. He supposes that hoofs and horns are produced by the elongation and subsequent consolidation of the papillæ of the skin invested with the cuticle and corpus reticulare, and that they are appendages or helps to the sense of touch. The spurs of cocks, he adds, have a similar origin and formation.

Then follows an account of certain morbid appearances observed on opening the body of a young gentleman, named Anthony Francis Davia, which serve to illustrate (he thinks) in a very striking manner the description he formerly gave of the structure of the kidneys. The external or cortical part of this person's left kidney was composed of a great number of glandular folliculi, resembling a bunch of grapes. These folliculi were not furnished with those excretory vessels of which the fleshy part of the kidneys is usually composed; but had a direct communication with the pelvis, or at least were connected with it by a very short duct. The right kidney was much larger than the left, and several glands of a considerable size, resembling vesicles distended with urine, were seen on its surface. Moreover, the congeries of excretory vessels, of which the fleshy part of the kidneys is composed, was more bulky than usual. Of the large glands or vesicles appendant to the excretory vessels some were filled with dark-coloured, corrupted blood; others with urine, or gravel, or tartareous matter. From this simple though morbid structure, it evidently appears (the author thinks) that the chief parts of the kidneys, besides the arteries and veins, are the glands and pelvis; which last being lengthened out into ureters, receives the urine from the glands by their excretory vessels, and gradually transmits it to the bladder. The preternatural enlargement and appearance of the glands (in this case) were owing to the urinary secretion being prevented from getting into the pelvis: whence it collected and remained stagnant within them. The same thing happens (the author observes) in other glandular parts, when diseased; and particularly in the liver, the glandular acini of which are not unfrequently so distended with bile, serum, or tartar, as to put on a vesicular appearance.*

A Letter from Mr. Charles Leigh, of Brazen-Nose College in Oxford, to Dr. Robert Plot, Sec. R. S. N° 160, p. 609.

The specimens of the water of natron and Nitrian nitre† you gave me, I have found to agree exactly with the descriptions which the ancients give of it. For the better understanding of the writings of the ancients concerning it, I shall in the 1st place show whence nitre has its denomination. In the 2d, the different names which ancient authors ascribe to it. In the 3d, the different places whence it comes. In the 4th, a description of it as it is when a compositum. In the 5th, the number of its principles when chemically resolved. In the 6th, the rise of them. In the 7th, its separation from the water of natron. In the 8th, its use in physic. In the 9th, in agriculture and mechanics. In the 10th, wherein it differs from sal ammoniac. In the 11th, from saltpetre.

All nitre took its name from a town in Egypt called Nitria. It is variously named by the ancients sal Ægypti, nitrum rubrum, spuma nitri, halmiraga, &c. &c.

It is found in Armenia, Africa,‡ Ægypt, &c.

* The reader is referred to volume i, pp. 322-324 of this Abridgment, for a detail of Malpighi's observations on the structure of the liver and kidneys.

† By Nitrian nitre is here understood the mineral alkali combined with the carbonic acid (carbonate of soda) and mixed with various impurities, not the neutral salt nitre (nitrate of potash.)

‡ That of Tripoli in Barbary, of which a description was given by the late Dr. Donald Monro, in the 61st volume of the Philosophical Transactions, is remarkably pure. It is also met with in considerable quantities in Hungary, and in some of the provinces of Russia.

The natron may be described thus: it is an alkaline salt perforated like a sponge, and of a lixivial taste, and thus I find it described by Pliny, Mathiolus, and Agricola.

Its principles I take to be chiefly two, viz. a sal marine,* and a urinous salt. (vol. alkali.)

The Nitrian water is of a blushy colour, and makes a brisk fermentation with an acid. I suppose that the water of Natron receives its redness from a red clammy substance.†

The next thing to be considered, is its separation from the water of Natron, of which the learned Dr. Huntington, who was at Nitria, gives the following account:

There is a town in Egypt called Nitria, which gives name to the Nitrian desert, where there is a lake called Natron,‡ taking up an area of 6 or 7 acres, situate about 30 miles west and by south from Terana, a town lower upon the Nile than Grand Cairo, and about the same distance north-west from the pyramids; from the bottom of this lake this sort of nitre called Natron arises to the top (as they apprehend) and there by the heat of the sun condenses into this kind of substance.§

The next thing to be considered is its use in physic: by Pliny it is commended in ulcers, inflammations, palsy in the tongue, consumptions, colic, hemorrhages, &c. &c.

Some authors assert it to be of wonderful efficacy in the stone.||

The next thing to be considered is its use in agriculture.

Mr. de la Cambre says, that plants grow in Egypt in such abundance, that they would choke each other, if they were not hindered by throwing sand upon

* This author was not acquainted with the true nature of this salt. Native natron (carbonate of soda) is not pure, but generally contains an admixture of sea salt (muriate of soda) calcareous earth, (carbonate of lime) and sand; sometimes clay also. The purified natron is a compound of soda and carbonic acid.

† This red clammy substance, which colours both the water and the salt itself, appears from the experiments of M. Berthollet not to be a mineral substance, but a vegeto-animal matter, susceptible of combustion, during which it gives out ammoniacal vapours.

‡ There are six natron lakes in the desert or valley here mentioned. A description of them by General Andreossy is inserted in the Memoires sur l'Egypte, tom. 1. The water of these lakes analysed by M. Berthollet was found to contain sea salt (muriate of soda) mild mineral alkali (carbonate of soda) and a little Glauber's salt, (sulphate of soda). See Observations sur le Natron par M. Berthollet, in the last mentioned memoirs.

§ That is (as the author afterwards remarks) when the water is evaporated to a certain degree by the heat of the sun, a film forms on the surface, and the salt shoots into crystals.

|| This assertion respecting the virtues of natron (carbonate of soda) in calculous cases, is confirmed by the experience of modern practitioners.

the fields, insomuch that the Egyptians must take as much pains to lessen the fatness of their land, as other nations do to encrease the fatness of theirs.*

In mechanics we have this account of it: it is said by Pliny, cap. de Vitri Inventione, that a company of merchants being thrown upon a shore where there were not any stones to be found, were forced to take great pieces of Egyptian nitre [natron] out of their ships, and make walls, upon which they hung their boiling kettle; the nitre being heated by the fire, mixed with the sand, and ran into several streams of glass, which afterwards hinted the way of making glass. It is likewise of use in dyeing; for Pliny and Vitruvius affirm, that by the help of this, the true azure is made, and that without this, there cannot be a true shadow.

In the last place I come to consider wherein it differs from saltpetre, and sal ammoniac. It may be distinguished from saltpetre, first by its fermenting: it will ferment with any acid; but saltpetre will not: I found that it would ferment with vinegar, as the old commentators observe in their comments upon Jeremiah and the Proverbs, but saltpetre will not: which gave occasion to some, in those texts, to alter the word nitre. 2dly, It may be distinguished from saltpetre† in its taste; for natron has a lixivial taste; but the other not. 3dly, By the volatile spirit which it affords: for from the one comes over a volatile alkali;‡ but from the other a corrosive acid. 4thly, The natron affords a red clammy substance, insipid; but the other not. This clammy substance, if I mistake not, is that which by Pliny is called ærugo salis. This it has from the earth; and therefore it is again said by Pliny, sunt ibi nitrariæ in quibus et rufum exit a colore terræ.§ 5thly, Like saltpetre it will not crystallize.|| 6thly, In the fire it makes no detonation.

From sal ammoniac it may be distinguished, 1st by its colour; for the natron is reddish, the other not; 2dly, by the texture of its parts; in sal ammoniac the parts seem close and firmly knit together, but the natron is spongy and perforated; 3dly, if mixed with sal ammoniac, this emits the same spirit as when it is mixed with quicklime.¶

* It is not easy to see what proof this observation affords of the use of natron in agriculture.

† Saltpetre is formed of nitric acid and potash; this salt (natron) of carbonic acid and soda.

‡ The volatile alkali here mentioned is an extraneous additament.

§ What this red clammy substance is, has been mentioned in one of the preceding notes.

|| It crystallizes, but the crystals differ in their form from those of nitre.

¶ Sal ammoniac is formed of muriatic acid and ammonia; natron is formed of carbonic acid and soda. By trituration with this carbonate of soda (natron) a decomposition of the muriate of ammonia (sal ammoniac) is effected; the muriatic acid leaving the ammonia to combine with the soda, to which it has a greater affinity. The ammonia (vol. alkali) thus disengaged rises up in the form of

Tuta ac efficax Luis Venereæ, sæpe absque Mercurio, ac semper sine Salivatione mercuriali, curandæ Methodus. Authore Davide Abercromby, M. D. Lond. 8vo. 1684. N° 160, p. 620.

Concerning the Porphyry Pillars in Egypt. In a Letter from Dublin, to the Editor. N° 161, p. 624.

There are now extant so many accounts of Egypt, in which these pillars and all other curiosities natural and artificial in that country are described, that the reprinting of this paper is deemed wholly unnecessary.

Præclarissimo et Eruditissimo Viro D. Jacobo Sponio Medicinæ Doctori, et Lugdunensi Anatomico accuratissimo. Marcellus Malpighius, S. P. Translated and abridged from the Latin. N° 161, p. 630.

In this very long letter the author gives a minute description of the uterus of a cow, which, he thinks, may serve to illustrate the structure of this viscus in the human subject; the various parts of which he observes are so contracted and confused in the unimpregnated state, as to be scarcely distinguishable.

He describes first the thick outer membrane of the uterus; then the fleshy fibres which run, some longitudinally, others horizontally; some in other directions, being crossed and intermixed towards the [Fallopian] tubes; and near the ovaria they are formed into fasciculi:* the lymphatic vessels next appear (in the order of dissection) turgid with their contained fluid; beneath these, the veins and arteries, forming an elegant net-work, each artery being generally accompanied with two veins. Among these are distributed the nerves, which pervade the whole substance of the uterus. "I have (says the author) discovered, especially during pregnancy, other vessels or ducts, which are peculiar to the uterus, and are large and conspicuous. Their situation and course are various; some of them emerging out of the substance of the uterus, others lying buried within it; so that to trace their progress has given me no little trouble. There are two sets of them, one of which goes to the sides of the uterus and vagina, especially in that part where the uterus faces and touches the bladder. And

pungent, odorous effluvia or vapours (which this author calls "spirit") according to the degree of temperature, in which the commixture or trituration is made.

* To trace the arrangement of the fleshy or muscular fibres of the uterus is extremely difficult. The late Dr. W. Hunter states, that he took particular pains in this investigation; but that, except upon the inner surface of this viscus, he observed nothing but irregularity and confusion. In an examination made 7 days after delivery, the muscular fasciculi were clearly seen in the inner surface, freed from the decidua. In the body of the uterus the fibres were circular. The fundus was made up of 2 concentric circular planes of fibres. See this celebrated anatomist's description of the uterus hereafter quoted.

although they are continued from the extremity of the vagina to the extremities of the cornua uteri, yet are they not all exactly of the same shape and appearance, nor of an equal size, nor running under one and the same plane. For the inferior set or portion of them, terminating not far from the orifice of the urethra, runs directly upwards towards the os uteri. This portion sometimes scarcely admits a probe, at other times it is wide enough to admit the little finger, opening into the vagina above the orifice of the urethra by a large and conspicuous hiatus. Near the os uteri these ducts seem to be obliterated, and nothing but very minute pores, terminating in one continued vessel, are to be found there. This portion (the most difficult to be traced) of these vessels, which takes its rise from an evident duct, but is buried within the fleshy fibres, stretches upwards along the sides of the uterus, until it comes to the cervix, where it emerges and is continued along the surface of the uterus. Of this portion of these vessels the appearances are various, so that they cannot be distinguished without a close and careful examination; for they lie buried within the substance of the uterus, and are often so minute as to be scarcely perceptible. Sometimes, indeed, they are collected into numerous and conspicuous clusters, at other times the fluid with which they are distended serves to point out their course.—One portion of these ducts emerging from the cervix uteri, is distributed over the body of the uterus and its cornua, till it loses itself in the extremities of the [Fallopian] tubes."—The appearances of these vessels (he observes) are greatly modified by their involucrum or investing ligament (ab ambiente involucro seu ligamento) as well as by their internal structure. In some places they are sinuous, in others straight; some of them have orifices or roundish glandular folliculi, which open in their insides; others are furnished with a sort of valves or cœcal appendices, resembling that of the colon. Near the orifice of the urethra these vessels become larger, opening by a winding duct into the vagina, in the manner before described.—The superior set or portion of these vessels, which runs along the sides of the os uteri, is invested with a cartilaginous involucrum, so as to be rendered varicous, and to have the appearance of being furnished with globular appendices. This portion, where it emerges from the cervix uteri, is as large as a quill; afterwards it becomes invested with a sort of spiral ligament, so as to exhibit a most elegant appearance. It continues its course until it reaches the cornua uteri, where it is divided into branches, which go to the extreme parts of the womb.—During pregnancy, these vessels, together with the uterus itself, are so much elongated and at the same time distended, that in cows they exceed the length of an arm, and are then so turgid with their contained fluid as to be very conspicuous. This fluid is of a different consistence in different portions of these vessels; in some places it is mucaginous, or half concreted, like pottage; in other places it resembles turpentine, both in colour and consistence. On exposure to the fire, it yields an amber-coloured coagulum (crassamentum carabis instar)—After subjoining the manner in which the uterus should be dissected for the detection of these vessels, the author asks, Whether they are the processuses or vasa deferentia of Laurentius, Riolan and others; or the lacunæ of De Graaf; or lastly the prostatæ fœminarum of Caspar Bartholine; or an apparatus of vessels never before noticed?*

The author next proceeds to a description of the substance of the uterus, and of the membrane with which its inner surface is lined.—This membrane is furnished with innumerable small orifices, from which there exudes a glutinous and mucous humor, serving to keep the uterus and vagina constantly moist. By macerating this membrane in water, the orifices of these excretory vessels are easily seen, especially in sheep. Moreover, during pregnancy the whole interior surface of the uterus and of its cornua is beset with tumors of unequal size, which appear to be so many appendices uteri seu vaginularum congeries, whence they have been named cotyledons. They receive from the chorion connecting vessels (which the author terms radices) in such manner that there is formed by their junction a perfect gland, by means of which nourishment is secreted, for the fœtus. That part of this glandular body which is attached to the uterus, is of an ash-colour, the other part is of a reddish colour. It is composed of a congeries of vaginulæ and sinuses, as is proved by injecting it with ink. It resembles the villous coat which lines the stomach and intestines of ruminating animals, abounding in roundish appendiculæ, and yielding on pressure a quantity of thick whitish mucaginous fluid (copiosum succum ptisanæ persimilem).—It is supplied with blood vessels, into which ink injected by the arteria uteri readily passes.† The other part of the gland, which is connected with the chorion, is composed of a number of radiculæ, which shoot into the above described vaginulæ. When this part of the gland is stripped of its vagina, and macerated in water, it forms a beautiful object for the microscope; its radiculæ and capillamenta become separated and raised up, so as to exhibit an elegant representation of a forest.—Then follow some observations on the appearances of the human uterus in dissections made either after parturition, or about the 7th month of gestation. To the interior surface of the uterus at such times are attached certain soft, mucous pelliculæ, which are connected with the chorion and placenta. They seem to form a sort of net work, composed of round, ash-coloured,

* The above description applies in part to the prostatæ mulierum of the older anatomical writers, as well as to the lacunæ of De Graaf; but it further seems to comprise the principal branches of the lymphatic vessels of the womb; which become so much enlarged, and are otherwise so much diversified in their appearance, during the different stages of gestation.

† The uterine fungi of quadrupeds.

friable* bodies, joined together by lateral branches (after the manner of a net) and resembling the omentum of fishes. These bodies are furnished with blood vessels, which communicate with the chorion and placenta. Whether these bodies, thus formed into a net-work, are muscular fibres, or nerves, or rather excretory vessels of the womb, the author does not pretend to determine.†—Next follow some remarks on the placenta, which the author considers as a contexture of the umbilical vessels held together or imbedded in a peculiar substance. These remarks are followed by others on the ovaria, and on the manner in which the germ or ovum gets from them (the ovaria) into the [Fallopian] tube. In cows he more than once met with a vesicular body, larger than a hen's egg, and filled with coagulable albumen, hanging from the ovarium. These vesiculæ are covered with a pretty strong membrane, on the inner surface of which numerous blood vessels are seen. In process of time a solid yellow body is produced, which appears as an excrescence from the ovarium, and when arrived at its full growth is as large as a cherry. It has an investing membrane, and is furnished with blood-vessels and nerves. This corpus luteum is not always of the same size, shape and appearance—Sometimes these corpora lutea are found effete, pervious by a sinuous duct, capable of admitting a probe. The cavity within its investing membrane is such as would contain a pea. From these facts it would appear (the author thinks) that the corpus luteum is designed by nature not merely as a nidus or lodging place for the ovulum, until the time of its ejection; but that it perhaps contributes also to its production, and may therefore be regarded in the light of a gland.—Hence it may be conjectured that the vesiculæ (the appearances and contents of which have been already described) which are at all times found in the ovaria, are not really the ovula themselves, to be afterwards fecundated, but the substance or matter from which the yellow and glandular body is formed. For it does not appear certain that the yellow and glandular body is only to be seen post coitum, seminisque masculini affusionem; for in calves, soon after birth, the author asserts that he has detected a vesicle or two accompanied with a corpus luteum.‡ In cows too, especially at the time of impregnation, and at different periods of gestation, he found many of these corpora lutea in the ovaria, sometimes of the

* Friabilibus componitur corporibus, &c. are the author's words; but there is certainly an impropriety in calling soft mucous pelliculæ friable. Loosely coherent would seem to be a more apposite expression.

† As in the immediately preceding part of these observations, the author has described the uterine fungi of quadrupeds; so here he has given an imperfect sketch of that uterine membrane, the decidua, which has since been so accurately figured and described in the works of the late Dr. Wm. Hunter. See his superb plates of the gravid uterus and the anatomical description of this viscus, published from his manuscripts, by Dr. Baillie.

‡ See also Blumenbach's observations on this subject, in the 9th vol. of the Comment. Reg. Soc. Gottingens. pp. 109—113.

size of a vetch, sometimes as large as a cherry, but without any suspicion of superfœtation. The same thing he observed in a woman about the 7th month of pregnancy. Moreover in one and the same ovarium, in different animals, several corpora lutea are found of unequal sizes, without a corresponding multiplicity of fœtuses. To this should be added, that in many ovaria (especially after they have been subjected to a boiling heat) the larger vessels are found turgid with a coagulated yellow juice. It may further be conjectured that a single albuminous vesicle may not suffice for the production of a single corpus luteum, but that several may be required; for where a perfect corpus luteum is formed, it not only occupies nearly the whole bed* of the ovarium, but commonly few albuminous vesiculæ are then found, although at other times they are in great numbers. Hence it may perhaps be inferred, that the production of this glandular body, is not the immediate consequence of the ovum contained within the ovarium being moistened with the seminal fluid, but that it is pre-existent to it (the ovum); and that the albuminous vesicles are not strictly ova, but the substance or matter from which is produced the gland, by means of which the ovum is secreted (separatur) cherished, and at a stated time excluded. The author is inclined to believe that the uterine vessels (as he terms them) which he has so minutely described in the former part of this paper, and whose contained fluid he supposes to answer the purposes of a semen fœmineum, or at least to supply the place of the liquor prostatarum, may possibly have some share in the business of fecundation; exerting, in conjunction with the semen masculinum, a stimulating and quickening energy upon the ovum. Now these vessels are continued to the extremities of the cornua uteri, and at the time of impregnation become turgid with a viscid liquor, the qualities of which he supposes to be considerably altered by the volatile particles of the semen masculinum; and that with this liquor the extremities of the cornua uteri where conception begins (ubi fit primæva conceptio) are moistened. In this manner (according to the author's views of impregnation) the ovulum dislodged from the ovarium becomes bathed with the uterine liquor, i. e. with a mixture of *both sorts* of seminal fluid—(Menstruo uterino, utriusque seminis volatilibus particulis, turgido aspergitur.)

* The author's expression is cavity (totam fere ovarii concavitatem) which is by no means proper, as the ovaria are not hollow bodies. Indeed many of Malpighi's terms are exceedingly exceptionable; thus he uses ichor (by which is commonly understood a thin acrid discharge from ulcers and wounds) to denote the mucaginous or lymphatic fluid contained in his so called vasa uteri propria; and by the words menstruum uterinum, quoted in the concluding part of this abridged account of his paper, he means, not the sanguineous menstrual evacuation, but his imaginary fecundating liquor. Of Malpighi's style it may further be remarked, that it is, on many occasions, too figurative; and that in describing the viscera, he frequently uses the term gland in a very wide and almost indefinite sense.

Concerning a Movement that measures Time after a peculiar Manner; being a Clock descendant on an inclined Plane. In a Letter from the Rev. Maurice Wheeler, M. A. N° 161, p. 647.

The exterior of this movement or clock, fig. 3, pl. 2, is a circular body of $3\frac{1}{2}$ inches diameter, consisting of 2 plates, measured by the same radius, and fixed in a parallel position to each other by the hoop h, the breadth of which is about an inch. This hoop and the two plates form the case of the movement; of which, the anterior plate is, towards the edge, inscribed with a horary circle, the divisions of which answer to the hours of a natural day. The deep shades within this circle are intended to represent a concave, of near half an inch deep; and the prominence g, in the middle of this concave, is a hemisphere of brass or silver, riding loosely on a pin, which lies hid, and is the axis of the movement. The upper half of this hemisphere is hollow, but the lower filled with lead; and the small winged figure that sits on it, with an erected finger performs the office of an index. But this being only for ornament, any other index may be substituted for it, as fig. 4, provided the axis on which it is supported, move freely in the hole H; and the lower part thereof, HI, so far preponderate over HP, as always to keep the index pendulous, with its point to the vertical hour.

The manner of its motion, as far as it appears outwardly, is thus: SE, fig. 3, represents a board or shelf, of a straight and even surface, about 6 feet long, and so thick as not to be apt to cast with change of weather, nor to grow camber under a small weight; on this is the movement placed, and hereon to perform its course, and therefore I call it the stage of the movement. This stage is raised at the end S, about 10° above the horizon, or line of level HE; but the angle of its declivity DEH, is variable. The two plates, which form the case of the movement, are to extend all round beyond the hoop h, $\frac{1}{8}$ of an inch, and the edges of them slightly indented; that while the movement descends upon the stage, it may turn only, and not slide.

The use thereof in measuring time, is as follows. The movement being placed as high as it can, near the point S, will move downward towards E, with that slowness, as to finish one entire revolution in 24 hours; and while it does so, the divisions on the horary circle, or dial-plate, successively culminating over the point of the index, will show the hours of the day and night. For in this movement, contrary to what is usual in others, the hour is shown by the access of the numbers to the index, which is always to keep the same position. Now when by several repeated revolutions it has measured out the length of its stage, it is to be replaced at S, as before; which may be done in less than half the time you are winding up a watch; and if the stage be 6 feet long, no oftener than once in a week.

The way of adjusting this motion to the exact measure of an hour, and rectifying its errors, is as follows: viz. by a screw inserted in the stage at S, by the turning of which the stage may be elevated or depressed, the movement will go faster or slower; faster if raised up; and slower if let down. And by making the horary circle moveable, and inserting several small bosses or buttons here and there upon its verge, with an easy touch of the finger it may be moved to the right or left, as there shall be occasion, till the just time be brought to the suspended index.

Now because it may seem at first view a little surprizing that a circular body should rest, or which in the present case is the same, move so imperceptibly slow, on a descending plane, having no visible impediment either to stop or retard the impetus of its own weight: therefore to explain this, let the circle LODN, fig. 5, represent any circular body, whose centres of gravity and magnitude are coincident at M; and let this circular body be placed on some level plain GG: then it is evident that the angle of its contact with that plane at a, will also be the point of its libration, and consequently it must rest there; because the momentum and obstacle are equal. Let DE represent a descending plane, making an angle of contact with this circular body at b, and here it is manifest it cannot rest; because the line of direction ra, which while it insisted on a level, divided the circular body by the centres of magnitude and gravity into parts equiponderate, is now removed to LD; which line LD falling without or beside the centre M, evidently destroys the equipoise of its parts, and therefore must leave it to tumble down towards E; for the momentum exceeds the obstacle. The reason therefore of its descent now being the overbalance of the parts LND to the remaining section LDO, it must necessarily follow, that if some weight, equal to the excess of LND above LOD, were affixed to the limb of the quadrant O a, as at P; then the circular body would rest as quietly at b, as it did before at a. The supposition cannot be denied, and the consequence is unavoidable, because $LDO + P = LND$, i. e. the obstacle is equal to the momentum. Nay this circular body will now resist a greater force, and maintain its point of libration b, more firmly on the declivity DE, than it could before, when it rested on the point a, in the level GG. The reason of which is evidently this, that by the addition of the counterpoise P to the quadrant Oa, the centre of gravity falls lower in the line of direction LD, and is removed from M to G, i. e. nearer to the point of libration b: and consequently will keep the circular body more steady in its present position. And the same principle which hitherto has been the cause of its rest, will become the cause of its motion. For let the numbers 1, 2, 3, 4, in fig. 6, represent a train of wheel-work, wherein there is no material difference from what is found in a common watch; only the numbers of the teeth on the wheels and pinions are to be so calculated, that the motion of the whole train may correspond to the assigned revolution of the body of the movement, which is to be once in 24 hours. It would be expedient also, that a spiral spring be applied to its balance, as is usual in the latter movement; but there is no need of a fuse; for the turns of the body of the movement, as it descends on the stage, answer all the purposes of a string or chain; and the contranitence of the weight P to the excess of LED above LQD, serves instead of a perpetual spring; and the movement wants only a perpetual descent, to make its motion so. And as the great wheel, in ordinary movements, is placed as near the edge of the framing plate ff, as it can be; in this case it must, with its axis or arbor M, possess the centre of the movement: because this wheel is to carry the weight or power P by the lever MP, and that weight P must always keep equidistant from the centre of the movement, that while the body of the movement performs its revolutions, the said weight P, and the great wheel, to which it is fixed, may, without any considerable variation, continue in or near the present position. Now suppose this weight P, with its lever MP, to be taken quite out of the movement, and then conceive the body of the movement to be placed on a horizontal plain HH, its point of contact in that plain is T, where it should, but cannot rest; because the weight of that part of the train marked with the numbers 2, 3, 4, removes the centre of gravity from M; and therefore on the opposite part of the movement, as about CQ, the inside of the hoop, which forms the case, is to be loaded with a thin lining of lead, which may be a counterpoise to that part of the train; that so the whole body of the movement, excepting only that P with its lever is now laid aside, may rest on that horizontal plane, or while it rides upon its own axis, indifferently in any point. This reducing of the movement to an equilibration of all its parts in the centre M, must be performed by trials, i. e. by rasping the lead at CQ, as much and in such places as is needful; which to an artificer of ordinary sagacity, will not be at all difficult.

The centre of gravity being thus reduced to M, the next thing is to replace the weight P, which, by the hole H, fig. 7, is to be set on the arbor of the central wheel M. Now let the body of the movement be placed on the declivity DE; and supposing $P + LQD = LDE$; then the body must needs rest there: but because the weight P is not now, as in fig. 5, fixed to any part of the quadrant QD, but hangs on the train of wheel-work 1, 2, 3, 4, it evidently follows that if its power be superior to the resistance of the train, then the whole body of the movement must needs descend towards E in fig. 3. By this it appears that there are two offices assigned to the weight or power represented by P. The first is, to be a counterpoise to the excess of the weight of LED, above LQD; the second is, that it be of force sufficient to put the train into a motion, so adjusted as may exactly comport with the time assigned for the revolution of the whole body. So that if there be any difficulty remaining, it consists in such an exact stating of the weight and power of P, that it may adequately serve both these intentions. And how easy this is, will appear from the following propositions.

1. That whatever the intrinsic weight of P shall be, as suppose it 4 ounces Troy, yet the power of that weight will be augmented or diminished, according to the different degrees of its elevation in the quadrant TQ. Thus, considering PM as a lever, its fulcrum is M, the point where it exerts its power on the train, is at V; then whatever power it has on the point V in its present elevation of 45 degrees, it will acquire a greater by being raised to 50 or 55, &c. and the greatest of all in 90 degrees, at Q: and on the contrary, let it sink to 40 or 35, &c. its power on the point V will still be diminished, insomuch that in T it will be quite extinguished; and therefore if P be of a competent weight to move the train at all, it will certainly move it in some degree of elevation or other in the quadrant QT. 2dly, if the weight P be considered as to its office of being a counterpoise to the body of the movement, it will perform this no less while it hangs by the lever MP, than if it were fast rivetted in the same place to the case of the movement: so in whatever point of the quadrant it moves the train, it may be also a counterpoise to the body of the movement. For, 1. At whatever point of the circle LETQ, fig. 6, the line of declivity DE makes an angle of contact; on the same point will the diameter SD fall at right angles with DE. 2. The line of direction LD will always fall on the point of contact D, making an angle with the diameter, as SDL. 3. The angle SDL will be always equal to DEH, in fig. 3, i. e. as great as is the elevation of the line of declivity DE above the horizontal EH, so great will be the angle of distance be between the diameter SD and the line of direction LD, fig. 6. 4. The greater the angle of declivity is, the less will be the section LQD; and so, on the contrary, the less that angle is, the greater the section. And therefore, 5. The excess of the weight of LED above LQD must be also greater, by raising up the stage with the screw at S; and that excess less, by screwing it down. 6. The lighter that part of the body is, which is represented by the

section LQD, the heavier ought the counterpoise P to be; and that either in its own intrinsic weight, in ounces, &c. or else in its potential weight, by being raised higher in the quadrant QT. 7. The screwing up the stage of the movement at S, will raise the counterpoise higher in the quadrant QT, by prop. 3, and therefore make it potentially heavier. From hence appears both the reason of the due adjustment of the motion of the train to the exact measure of an hour, and what weight is to be assigned to P, that moves it. Therefore having set the stage, by the help of the arched screw, at the elevation of about 10 degrees, place the movement thereon, and try what weight, hanging at the end of the lever MP, will stir the train; meanwhile holding the movement with the hand in such a position, as that the level may make an angle of about 30 degrees with the perpendicular MT: then let the movement loose, to undulate upon the stage; and when the vibration ceases, observe to what degree of the quadrant the lever points, and at the same time mind the pulses of the balance. If, at this observation, the weight lies low, as for instance between 25 and 35 degrees of the quadrant, and the beats of the balance are guessed to be not very different from their due time, the weight P is well enough proportioned; but if it chance to be much heavier than is absolutely needful, that excess will be moderated by screwing down the stage; and if it be not absolutely too light, its defect will be compensated, by screwing the stage higher. Therefore of these two extremes, chuse the former; for the fewer degrees P rises in the quadrant beyond what is absolutely necessary, the better.

Experiments to be made relating to Land Carriage. Proposed by Sir William Petty, Knt. N° 161, p. 666.

The water carriage of goods round the globe of the earth, is but about double the price of land carriage from Chester to London of the like goods. Land carriage by carts and waggons is cheaper than on horses backs, and this cheaper than by men. Therefore carriage by carts and waggons ought to be well understood and considered.

Land carriage by draught, is by wheel-barrows, straddles, carts of 2 wheels, sleds, waggons of 4 wheels, by cars on 2 high wheels, drays on 2 small wheels, Irish cars of 2 very small wheels. The present work is to design experiments whereby to know the difference and advantages of all the said several carriages, upon several ways and grounds. To which purpose it is proposed that the following experiments be made, viz.

Exper. 1.—The difference between what a man or horse can carry, and what they can draw up at a pulley.—2. The difference in what a man can carry for 1, 2, 3, 4, 5, &c. hours.—3. The difference of speed that a man can make under several burthens within the same time.—4. Let the difference between the weight of a common Irish car and the burthen which a horse can draw upon it be examined; as also the like difference between a cart for 5 horses and its burthen, and between a coach with a coachman with its burthen, and between the pack-saddle and of a pack-horse with its burthen.—5. Let the difference between a horse's draught on a small car, and a 5 horse's draught on a great cart and 4 wheeled waggon, be examined.—6. Let the same differences of horses draught at several distances from the carriage, and upon wheels of several heights, be examined.—7. Let the just weight of wheels be determined, to make them of the same strength, though of different diameters, and at what distance wheels of several heights should stand from each other.—8. What the difference is between iron and wooden axles, and of the friction made by them, within the boxes of their naves.—9. What is the true reason of the dishing out of wheels. —10. What is the true proportion of timber which ought to be in the nave, spokes, or rim of any wheel, in order to lightness, strength, and uprightness. —11. What is the difference between the high and low hanging of coaches, the distance of the standards, and of the difference between the hind and the fore wheels.

Other experiments.—Take a parallelopipedon of wood, suppose 4 inches square at the ends, and 8 inches long, with the weight thereof, and try as follows, viz.

1. How much weight less than that of itself, applied to convenient pulleys, will draw the said log over a smooth level table, of an assigned length, in an assigned time, and what weight will draw the same faster or slower, in any assigned proportion.—2. What difference there will be in the friction of the whole side of the said log on the plane table, and mounting the same on 2 small keels of $\frac{1}{4}$ of an inch thick.—3. What the difference between the last mentioned mounture, and setting the same upon 4 thick segments of circles, so as to touch the table but at 4 points, in imitation of dragging wheels, and whether it be material that the said segments should be of greater or less circles.—4. What the difference between the said mounture upon 4 such segments, or upon 4 wheels moving distinctly upon their axles, as also between 2 wheels or 1 segment, like a cart, or 1 wheel and 2 segments, like a wheel-barrow.—5. What the difference in draught will be in the aforementioned draughts, on the various inclinations of the said table, upwards and downwards, or on the said table covered with a blanket equally extended, or with a paste of clay of a certain thickness.—6. What the difference between the tenderest motion upon wheels and the draught through water.

Christiani Hugenii Astroscopia Compendiaria, Tubi Optici Molimine liberata; or, the Description of an Aërial Telescope. N° 161, p. 668.

This description of Huygen's aërial telescopes, of 100 or 200 feet in length, by the contrivance, without a continued tube, of suspending the object glass at the top of a long mast fixed in the ground, is described more fully in his own works. But it was never found to be of any use, as it could not be brought into practice, and has been superseded by the modern and powerful reflectors of short lengths.

A New and Easy Way of demonstrating some Propositions in Euclid. By Mr. ——— Ash, Member Phil. Soc. Dublin. N° 162, p. 672.

As a further instance of the evidence of mathematical theorems, I believe it not difficult to demonstrate any one of Euclid's, independently of the rest, without precedent lemmas or propositions; as an essay of which, I shall here subjoin some of the most useful, and on which the solution of most problems, especially algebraical ones, depend, and those also in the most various and different parts of geometry, viz. concerning the properties of angles, circles, triangles, squares, proportionals, and solids. The propositions which I shall endeavour to demonstrate thus independently, shall be these, the 32d and 47th of the 1st book; most of the 2d and 5th books; the 1st and 16th of the 6th, with their corollaries. In order to demonstrate the 32d, I suppose it known what is meant by an angle, triangle, circle, external angle, parallels, and that the measure of an angle is the arch of a circle intercepted between its sides, that a right angle is measured by a quadrant, and 2 right angles by a semicircle. I say then, in fig. 5, pl. 1, that in the triangle ABC, the external angle BCE is equal to the two opposite internal ones ABC, BAC. For let a circle be drawn, C being the centre, and BC the radius, and let CD be drawn parallel to AB; these two lines being always equidistant will both have the same inclination to any 3d line falling on them; that is, by the definition of angle, they will make equal angles with it; for if any part of CD, for instance, did incline more to BC than did AB, then they would not be parallel; it follows therefore that the angles ABC, BCD, are equal; also $BAC = DCE$, because AE falls upon two parallels; but the external angle $BCE = BCD + DCE$, which was before proved to be equal to ABC, BAC, *Q. E. D.*

Hence may be inferred as a corollary, that the three angles of every triangle are equal to two right ones; for the angles $ACB + BCE$ are measured by a semicircle, and therefore equal to two right angles. Corollaries also from hence are the 20th, 22d, and 31st, of the 3d book, which contain the properties of circles.

To demonstrate the 47th, I suppose the meaning of the terms made use of to be known, and that two angles or superficies are equal when one being put on the other, it neither exceeds nor is exceeded; this being allowed, I say the sides about the right angle are either equal or unequal; if equal, as in fig. 8, let all the squares be described; the whole figure exceeds the square of the hypothenuse BC by the two triangles M, V, and exceeds also the squares of the other two sides AB, AC, by the two triangles ABC and S; which excesses are equal, for M is equal to ABC, the two sides about the right angle being two sides of a square upon AB, by supposition equal to AC, and the third side equal to BC, therefore the whole triangles are equal; after the same manner S and V are proved to be equal, therefore the square of CB is equal to the square of the two other sides. *Q. E. D.*

But if the sides be unequal, as in fig. 7, let the squares be described, and the parallelogram LQ completed, the whole figure exceeds the square upon BC, by three triangles X, R, Z, and exceeds also the squares LA, AD, by the triangle ABC, and the parallelogram PQ, which excesses are equal; for Z is equal to ABC, the side OC = BC, AC = CD, the angle D = A, and OCD = ABC, which is manifest by taking the common angle ACO out of the two right angles BCO, ACD; therefore, by superimposition, the whole triangles are equal. In like manner X is proved equal to ABC, as also R; and the parallelogram PQ double of the triangle ABC; thus the excesses being proved equal, the remainders also will be equal, viz. the square of BC to the squares of AB, AC, *Q. E. D.* Manifest corollaries from hence are the 35th and 36th of the 3d book, also the 12th and 13th of the 2d.

The first ten propositions of the 2d book are evidently demonstrated, only by substituting species or letters instead of lines, and multiplying them according to the tenor of the proposition; thus to instance in one or two, call the whole line a, and its parts b and c, therefore $a = b + c$, and consequently $aa = bb + cc + 2\,bc$, which is the very sense of the 4th of the 2d book. Thus also, let a line be cut into equal parts e, e, and let another line d be added thereto, it is manifest that $4ee + 4de + 2dd = 2ee + 2ee + 2dd + 4de$, which is the 10th proposition of the same book.

Almost the whole doctrine of proportionals, viz. permutation, inversion, conversion, composition, division of ratios, and proportion ex æquo, and consequently the most useful propositions of the 5th book are clearly demonstrated by one definition, and that is of similar or like parts, which are said to be such as are after the same manner or equally contained in their wholes; thus the antecedents a and c are either equal to their consequents or greater or less; if equal the thing is manifest; if less, then by the definition of proportionals a and c are like parts of b and e, therefore what proportions the whole b and e have to one another, the same will a and c have, which is permutation; likewise $e : c :: b : a$, which is inversion. So also, if from proportionals we take like parts, the remainders will be proportional; whence conversion and division are demonstrated; and if to proportionals we add like parts, the wholes will still be proportional, which is composition, &c. If the antecedents be greater than the consequents, the consequents will be like parts of them, and the demonstration exactly the same with the former.

The first of the 6th book is proved by considering the generation of parallelograms, which are produced by drawing or multiplying the perpendicular upon the base, that is, taking it so often as there are parts and divisions in the base; therefore, in fig. 6, the same proportion that RX single has to NX single, the same has RX multiplied by XZ, that is, repeated a certain number of times, to NX multiplied to XZ, that is, repeated the same number of times, which is as much as to say, RX : NX :: the paral. RZ : the paral. NZ; now that this proportion also is true in oblique angled parallelograms is proved, because they are equal to rectangled ones on the same base and between the same parallels, as thus independently appears; the triangles RQX and MPZ are equal, for RX = MZ, QX = PZ, RM = QP, therefore adding MQ to both, RQ = MP; if therefore from these equal triangles we take what is common, viz. MLQ, the remainders will be equal, viz. RXLM = QLZP; to both of those add XLZ, then the whole parallelograms will be equal, viz. RZ = QZ, *Q. E. D.* That triangles also having a common base are in the proportion of their altitudes, hence follows, because they are the halves of parallelograms on the same base. And the demonstration is exactly the same in prisms, pyramids, cylinders, and cones, having the same base.

To prove the 16th of the 6th book, I suppose the 4 lines a, b, c, e, to be proportional; that is, granting a and c to be the less terms, the same way that a is contained in b so is c in e, and that d is the denominator of the ratio, it will follow then that b is made up of a multiplied by d, and e of c multiplied by d, so that $ad = b$ and $cd = e$, draw therefore the extremes upon one another, that is a on cd, and the means, that is, c on ad, the factors being the same, the products acd and cad are the same, and consequently equal *Q. E. D.*

An Account of the Course of the Tides at Tonquin; in a Letter from Mr. Francis Davenport. N° 162, p. 677.

The peculiar circumstances of the tides at Tonquin are sufficiently stated in the following paper by Dr. Halley, which renders the reprinting of this tedious description of them quite unnecessary.

A Theory of the Tides at the Bar of Tonquin. By Mr. Edm. Halley, F.R.S. N° 162, p. 681.

The effect of the moon on the waters in the production of the tides in the port of Tonquin is the more surprising, as it seems different in all its circumstances from the general rule, whereby the motion of the sea is regulated in all other parts of the world that I have yet heard of. For first, each flux is of about 12 hours duration, and its correspondent reflux as long; so that there is but one high water in 24 hours. Then there are in each month two intermissions of the tides, about 14 days asunder, when there is no sensible flood or rising of the waters to be observed, but the sea is in a manner stagnant. Thirdly, that the increase of the water has its 14 days period between the aforesaid intermissions; and at 7 days end makes the highest tides; from which time the water again gradually abates, and the flood is weaker till it comes to a stagnation, both increase and decrease observing the same rule in being exceedingly slow in their beginning and end, and swift in the middle. Lastly, and which is most strange, the rising moon in the one half of each month makes high water, and the setting moon in the other half.

These particulars considered, together with the tables showing the days of the water's stagnation in each month, gave me a light into the secret of this strange appearance, so as to be able to bring the hitherto unaccountable irregularity of these tides to a certain rule. And first it appears that the intermissions of the tides happen nearly on those days that the moon enters the signs of Aries and Libra, or passes the equinoctial, which divides the moon's course nearly into two equal parts, as well as the sun's; and from hence it follows, that the tropical moons in ♋ and ♑, are those which occasion the greatest flux and reflux. It also appears that the moon in northern signs brings in the flood, whilst she is above the horizon, so as to make high water at her setting, and on the contrary, that whilst she is in southern signs, it flows all the time the moon is below the horizon, and so makes high water at her rising. But it is to be observed, that though the moon pass swiftly from south to north when she is in or near ♈, and from north to south when in or near ♎, yet the motion of the sea, which is the cause of this tide, is scarcely discernible for 3 or 4 days, when the moon passes the said equinoctial points; whence it appears, that though the declination of the moon be that whereby these tides are regulated, yet the increase and decrease of the water is by no means proportionate to that of her declination, that changing swiftly, where the increase of the water is observed to be most slow. It seems therefore, and I propose it as a probable conjecture, that the increase of the waters should be always proportionate to the versed sines of the doubled distances of the moon from the equinoctial points; upon which hypothesis, fig. 8, pl. 2, will give an elegant synopsis of the whole matter. Let AB be the bottom of the bar of Tonquin, CD a perpendicular thereto, whereon to measure the several depths of the water; C♈ C♎ the mean depth, which is that whereat the water is stagnant on the moon's being on the equinoctial points, which is commonly about 15 feet; C♋ occid, the high water mark when the moon is in ♋ or ♑ being about 24 feet; C♑ occid the height of the low water mark when the moon is in ♋ or ♑, being about 6 feet; so that the greatest rise of the water on the tropical moons will be about 18 feet; then dividing ♈ ♋ and ♎ ♑ into two equal parts in E, F, on those two points, as centres, describe the two circles, each of whose radii are 4½ feet, which being kept between the compasses, naturally divide the said circles in the points ♉ ♊ ♋ ♌, &c. through which points if we draw lines parallel to the base AB, they shall cut the perpendicular CD, in the heights of the high and low water marks, which will be at the entrance of the moon into the said signs. So the greatest depth of the high water when the moon enters ♉, ♍, ♌, ♓, is but 17¼ feet, and the least at low water 12¾ feet; but when she enters ♊, ♌, ♐, ♒, the high water depth is 21¾ feet, the low water but 8¼ feet, as appears by the figure. And this hypothesis not only agrees with all that Mr. Davenport has observed himself or collected from the natives, but has been found to hold true since, in the year 1682, by the ingenious Captain Knox, in his voyage to this port, so that there is no room to doubt of the truth thereof. By this method then may the time and height of the tides be with sufficient certainty computed; but to philosophize thereon, and to attempt to assign a reason, why the moon should in so particular a manner influence the waters in this one place, is a task too hard for my undertaking, especially when I consider how little we have been able to establish a genuine and satisfactory theory of the tides, found upon our own coasts, of which we have had so long experience. It would be however a very acceptable thing if some curious navigators would inform us, what tides or currents are found at Macao, Quemoy, and other places on the coast of China, and on Formosa; it being most probable that this flood comes out of the north east, along the coast of China, for the northerly monsoon is found to occasion the highest spring tides. There is yet another thing well worth inquiry, viz. seeing that this motion of the sea is more or less, as the moon is farther from or nearer to the equinoctial, it is not unlikely that some years may have much higher spring tides than others, according to the various obliquity of the moon's orbit to the equinoctial; for when the ascending node is in ♈, as it was anno 1671, and will be anno 1690, the moon in ♋ and ♑ deviates from the equator full $28\frac{1}{2}°$, and but $18\frac{1}{2}°$ when the same node is in ♎, as it was anno 1680.

A Lunar Eclipse, June 17, in the Morning, 1684, observed at Greenwich. By Mr. Flamsteed, Math. Reg. N° 162, p. 689.

The eclipse began at 2h. 3m. in the morning, correct time. At 2h. $26\frac{1}{2}$m. the chord of the darkened periphery was 11′ 25″. The eclipse ended about 2h. 41m.

An Eclipse of the Sun, July 2, 1684, observed at Greenwich. By Mr. Flamsteed. N° 162, p. 691.

At 2h. 3m. 45s. afternoon, corrected time, the eclipse was not begun, the sun's limb being perfect; after that cloudy. At 2h. 12m. 17s. the eclipse had just begun on the lower limb. At 3h. 0m. 20s. the shadow touched the centre. The obscuration increased till 3h. 20m. from which, till 3h. 25m. the part of the diameter eclipsed was $19\frac{1}{2}$ parts out of 32. After which, it gradually decreased again; and at 3h. 45m. 35s. the digits eclipsed were only 16, or the centre emerged from the shadow. At 4h. 27m. 37s. exactly the eclipse ended, the whole time of the duration being 2h. $15\frac{1}{3}$m.

The same Eclipse of the Sun observed at Paris. By MM. Bulliald and Cassini. N° 162, p. 693.

At 2h. 25m. 30s. the eclipse began. At 3h. 10m. 5s. the centre was immerged, or 6 digits eclipsed, out of 12. At 3h. 35m. the greatest obscuration was $7\frac{7}{8}$ digits. At 4h. 4m. 10s. the centre emerged. And at 4h. 43m. 23s. the eclipse ended.

A Discourse concerning Digestion. By Mr. Charles Leigh, of Brasen-Nose College, Oxford. N° 162, p. 694.

It has been observed by Helmont, Meibonius, Tachenius, and Mr. Boyle, that meats, by being kept in an acid liquor, would look extremely white. But I do not find that by the help of a salt, that is merely an acid, there can be any chyle prepared from meats. Wedelius is of opinion that chyle is nothing but a mixture of oil and serum. Tilingius affirms, that it is made by a nitrous salt. Dr. Harvey by trituration. Dr. Willis by an acid and sulphur. Diemerbroeck and Sylvius by the saliva. Dr. Mayow by a nitro-aërial principle. Galen and Aristotle by heat. Others by a ferment spewed from the glands in the bottom of the stomach. Others by the relicts of the meat grown sour. So many different opinions I shall neither pretend to reconcile nor decide on; and shall therefore only lay down the phænomena afforded by an artificial digester, and from them draw inferences as probable conjectures. But shall in the first place premise a description of this digesting liquor, and see how far it may probably parallel the natural ferment in the stomach. It is prepared from spirit of sulphur, spirit of hartshorn, the chyle of a dog, and the saliva. The taste of it is like meat vomited out of a full stomach, something sour, but will not ferment with an alkali.

It is pellucid and without any smell: the salt that it shoots into is cubical.* Into a dram of this liquor I put a piece of veal, about the size of a nut, and set it on a digesting furnace. In two hours time there came from the meat a liquor having the colour and taste of chyle, and the meat afterwards was lighter, dry, and insipid. It afforded the same phænomena also in beef, mutton, or any other meat. And though it has been affirmed by some, that the same thing may be done by acid liquors only, yet in all the trials I have made upon them, I have not observed the like phænomena. Now since by the help of this menstruum, there came from the meats a liquor having the colour and taste of chyle, and since the taste of this menstruum is not distinguishable from the taste which is perceivable in meats vomited out of a full stomach; it may be conjectured, that by some such menstruum the meat is digested in the stomach.† I would not however be thought to affirm, that by a liquid menstruum alone the meats are digested, but that there are likewise required these further requisites, in some, or in most creatures. 1. That the stomach receive a gentle heat from the liver. 2. That it have a natural situation. 3. That it be assisted by the omentum. This may be inferred from those creatures which, having no caul, help concoction by doubling their hinder legs, and resting their bellies upon them, as hares and conies. 4. That the stomach have a tunica villosa: by which it is enabled to divide the meat into parcels, which must facilitate the operation of the natural ferment; as we see all menstruums sooner dissolve metals when these are filed into parcels, than when they continue in the lump; and without a tunica villosa, the tunica carnosa would be apt to be too much distended by our meat and drink, which would necessarily weaken the tonical motion of the stomach. 5. It is necessary that there be

* The saliva yields on analysis various phosphates, besides common salt (muriate of soda). The "cubical crystals" abovementioned, would consist of the last mentioned salt.

† The opinion of the food being digested in the stomach by the solvent action of a peculiar fluid or menstruum (the gastric juice), has been confirmed by the experiments and observations of succeeding physiologists.

windings of the intestines; for otherwise the digested meat would move too fast from the stomach, and so torment us with perpetual hunger.

The ingredients of the natural ferment I take to be these. The saliva, the succus of the glands of the stomach, and a nitro-aërial spirit of the nerves. That the saliva is an ingredient, may seem probable from these reasons. 1. Because that by the help of this, meats though impregnated with different principles, may be made to mix with a menstruum. 2. Since the saliva is impregnated with a volatile salt,* it is probable that that also may help digestion. The second ingredient is thought to be a liquor, that is separated by the glands in the bottom of the stomach. By these therefore it seems probable, that the glands in the tunica villosa separate a fermenting liquor;† and it is further observed, that those creatures which have the most of these glands, are the most voracious. Lastly, that the nitro-aërial spirits of the nerves are an ingredient of the stomachical ferment, seems reasonable from the arguments of Dr. Mayow, who argues thus: Now, since the animal spirits consist of nitro-aërial particles, it will not be difficult to conceive how the aforesaid effects are produced by them in the stomach. For though the nitro-aërial spirit be not acid, yet by it iron is corroded, vitriols are perfected, fixed salts are brought to a fluor, and the compages of things are dissolved as by a universal menstruum.

Abstract of a Journal of the Philosophical Society of Oxford, being an Account of some Experiments relating to Digestion; and of a large Bed of Glands observed in the Stomach of a Jack. Aug. 19th, 1684. By Mr. Musgrave. N° 162, p. 699.

Part of a mucous substance, taken out of the stomach of a jack, near the pylorus, and mixed with solution of sublimate, became much whiter than it was before. Another part of it, mixed with syrup of violets, turned green. Mr. Musgrave has observed like effects, by mixing a liquor, found in the stomach of a hedge-hog, with syrup of violets, and with solution of sublimate. These experiments are urged as an argument against the existence of an acid ferment in the stomach: it seems probable, that the great work of digestion proceeds from a volatile alkali.‡

* Phosphate of ammonia.

† The gastric juice. The terms "fermenting liquor" and "stomachical ferment," are extremely unapposite, since it is the nature of the gastric liquor to prevent fermentation. On this occasion we cannot but refer for a more accurate and complete view of the subject of this paper, to the late Dr. George Fordyce's Dissertation on the Digestion of Food.

‡ It is so difficult to obtain the gastric juice in a state of purity, that it is doubtful whether the alkali which showed itself in these experiments was derived from it, or from the alimentary substances dissolved in it. The latter conjecture is the most probable.

He also observed a large bed of glands, amounting to about $\frac{3}{8}$ of the inside of the stomach, and seated near the pylorus of a jack: the whole bed appears of a brownish red colour, and is divided into several ridges, which run parallel to one another, and the same way with the stomach: for the better contraction of that part, especially when empty; (at which time these glands, being fixed to the inmost coat, are, together with it, drawn up in wrinkles,) that edge of this bed of glands, which is nearest the head of the fish, is indented; the ridges breaking off suddenly; but at the other end, a little on this side the pylorus, they diminish almost insensibly.

By these glands, he supposes, at least a considerable share of the menstruum, the great efficacy of which make this fish a fit subject to illustrate the nature of digestion, is separated from the blood; for blood-vessels may be seen in great numbers, on the other side of the glands and inner tunic, by separating it and them from the middle, and muscular tunic. And, as a farther argument of this use of these glands, he has observed that part of the stomach where they are, is generally moister than the other part near the mouth; and that, in dissecting jacks whose stomachs have been filled with some large fish of the pinnaceous kind, which must enter with the head foremost, the head, and fore parts of the devoured fish have, as far as the glands reach, been either actually dissolved, or fairly turning into a mucilage; whereas, at the same time, the other and less bony part of the included fish, being not yet come within the power of the menstruum, has still retained its form and consistence.

Experiments and Considerations about the Porosity of Bodies, in two Essays. By the Hon. Robert Boyle, F. R. S. Lond. 8vo. 1684. N° 162, p. 702.

The author, continuing his endeavours to establish a solid and rational philosophy, in this book treats on the small pores of bodies. The book consists of two essays; the former of them treats of the porosity of animal bodies, and shows that the parts of animals, especially whilst living, are furnished with numerous pores; a fact which is deduced from many circumstances; as from the frame or constitution of the stable parts of the bodies of animals: from the nature of nutrition in animals: from the great plenty of matter daily carried off by sweat, and insensible transpiration: from the entrance of effluvia into animals; which is evident from the operation of some plasters and vegetable ointments; from the effects of pericarpia, or wrist-bands: and from cantharides used in blistering plasters: from the bringing off the matter of an apites by seige, or urine: from the staining ivory purple with a solution of gold in aqua regia; blackish with silver, and bluish with copper dissolved in aqua fortis. For proving the porosity of bones, he urges their specific levity; that blood vessels pass them; that they imbibe exhalations from the air, and emit them again; and that mercury has been found in the bones of some persons, who have taken of it.

The other essay is concerning the porosity of solid bodies, which the author proves, first, a priori, from the origin and formation of divers hard bodies; from the great disparity found in the specific gravities of such bodies as the eye does not perceive to be porous; from the frame and constitution of solid bodies; for even they consist of particles which cannot be supposed to touch one another so exactly, as not to leave any pore between them. 2dly, a posteriori, from some experiments and observations, arguing the porousness of wood, earthen vessels, stones, metals, and even glass itself.

Joh. Dolæi, M. D. Consiliarii ac Archiatri Hasso-Cassellani Encyclopædia Medicinæ Theoretico-Practicæ, &c. Francofurti ad Mœnum, An.* 1684, *4to.* N° 162, p. 704.

This book is divided into six parts: the 1st treats of the diseases of the head; the 2d, of those of the thorax; the 3d, of those of the abdomen; the 4th, of fevers; the 5th, of those incident to women; the 6th and the last, of those of infants and children. His practice is mostly chemical. He pays great deference to Ettmuller and Sydenham. He is much inclined to transfusion, and the injection of medical liquor into the veins; and quotes his own experience for it in madness and some other diseases of the brain.

Disquisitio de Magia divinatrice et operatrice, &c. Auctore Francisco Moncæio, 4to. Francofurti et Lipsiæ, 1683. N° 162, p. 706.

In this magic, the author gives the several divisions made use of by authors, different according to the divers conceptions they had of its parts. He makes it first, either human or diabolical, with their respective species. Secondly, natural, or transnatural, under which last he comprehends the transmutation of metals; under natural come all the surprizing effects of art, such as artificial fires, malleable glass, incombustible linen, cures of diseases by magnetism or transplantation, strange effects in chemistry and mathematics, &c.

After this general account, follows a very particular enumeration of its species,

* John Dolæus was a multifarious, and not always a judicious compiler, in the 17th century. He was physician to the Landgrave of Hesse Cassel, and died in 1707. Besides the abovementioned work, he wrote an Encyclopædia Chirurgica; an Encyclopædia Pharmaceutico-chemica; a Tract on the Gout, entitled Furia Podagræ Lacte mitigata; a Collection of Letters on Medical Subjects; and some other tracts.

to the number of about 69, alphabetically placed, together with the authors that treat of them; out of all which the author singles alectryomancy for the subject of this book.

A further Account of the Bridge at Pont St. Esprit, with a parallel History of some other Bridges at Rome: in a Letter from Mr. Tancred Robinson, to Dr. Martin Lister.* N° 163, p. 712.

Doubtless you recollect the stately ruins of the modern bridge at Avignon, which has yielded in many places to the extreme rapidity and violence of the Rhone. Its fall in my opinion may be ascribed to three defects: 1. it was not so multangular as that at St. Esprit: 2. it wanted, in 3 or 4 places, the little arches dividing the feet of the great ones; and in those parts it has suffered most: 3. the pedestals were not so geometrically and exactly laid, as those of Pont St. Esprit; their projections were few, and those not gradually contracted; so that the force of the stream must be the greater on the fabric.

Now that we are upon this sort of history, I shall venture to carry you from the Rhone to the Tyber, which, though not so swift as the Rhone, yet is subject to greater inundations, as many inscriptions assure us. No river ever had so many bridges built with such magnificence and art, as this; and though they were more pompous, and rich in rare stones, in sculpture, &c. than that I formerly sent you a draught of from Montpelier, yet they had the like provision for their security and preservation, and their design was much the same; as may be yet seen at Rome, in such old bridges as still remain there.

Description of a Bridge that may be built 70 Feet long without any Pillar under it. N° 163, p. 714.

A timber bridge may be built 70 feet long, or somewhat more, without any pillar under it, which may be useful in some places where pillars cannot conveniently be built: it may consist of two such arches of timber, as that which is represented in fig. 1, pl. 3; wherein *ac*, and *bo* are beams 28 feet long, and *ab* 32 feet long. Under the angles are set 2 large braces *el* and *sr*. At each end is a wall, on which are laid two beams *bh* and *ad*, each 20 feet long; and under these are 2 braces *de* and *rh*. There may also be braces at the extremities of the arches, crossing the bridge obliquely. It may be laid with planks, and railed; and beyond the walls causeways *fd* and *hn*. The length of the bridge *cmo* is 70 feet; and the height *km* 19 feet.

* See N° 160, in this volume.

*Observations of the Eclipse of the Sun on the 12th of July last, New Stile, made at the Observatory at Paris, 1684, and in several other Places.** N° 163, p. 715.

Eclipse observed	Beginning.			6 Digits.			Middle.			End.			Quantity.
	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	Dig.
At Paris, by Cassini and Sedilean	0	0	0	3	10	5	3	35	0	4	43	23	$7\frac{7}{8}$
By La Hire and Pothenot	2	25	24	3	12	6	3	36	27	4	43	27	$7\frac{1}{12}$
By Mr. Fontenay	0	0	0	3	12	40	3	38	0	0	0	0	$7\frac{3}{4}$
At Aix, by M. Gautier	2	54	30	0	0	0	0	0	0	5	9	9	$8\frac{1}{2}$
At Lyons, by M. Hoste	0	0	0	0	0	0	3	53	52	0	0	0	$8\frac{1}{4}$
At Bay of Roses, by Chasselles	2	40	0	3	25	0	0	0	0	5	1	30	9
At Honfleur, by M. de Clos	2	15	2	0	0	0	0	0	0	4	34	35	8
At Pau, by M. Richaud	0	0	0	0	0	0	3	15	0	4	45	0	10
At Avignon, by M. Bonfas	2	43	27	0	0	0	4	2	0	5	4	37	9

M. Cassini having compared together these observations, and made such reductions as the parallax requires, thence concluded the differences of meridians between the places of observation, as follows:

From Paris to Aix	14′ east	lat. 43°	30′
to Avignon	$8\frac{1}{2}$		
to Lyons	8 or 13		
to Roses	4	42	10
From Paris to Honfleur	7 west.		
to Pau	11	43	30

The Observations of the Ancients concerning the Obliquity of the Zodiac, in a Letter from Mr. Edward Bernard† to Mr. John Flamsteed, Math. Reg. Translated from the Latin. N° 163, p. 721.

Anno 230, before the birth of Christ, Eratosthenes found the obliquity of the ecliptic to be 23° 51′ 19″ 31‴. For according to him the distance of the

* See also N° 162, in this volume.

† The Rev. Edward Bernard was some time Savilian professor of astronomy at Oxford, having succeeded to that office in 1673, on the resignation of Sir Christopher Wren, who had been appointed surveyor general of the King's works. Mr. Bernard was an Oxford scholar, and was particularly skilled in the oriental languages; a talent which he very usefully employed, both in collecting together the scientific writings of the ancients, and in making extracts from them. Thus, in 1668, he went to Leyden to consult the Arabic copy of the 5th, 6th, and 7th books of Apollonius's Conics, which Golius had brought from the east, and of which the Greek text is lost; a transcript of which was there taken by Mr. Bernard and brought to Oxford, and was published by Dr. Halley in 1710. On his return to Oxford Mr. Bernard spent some time in examining and collating the most valuable manuscripts in the Bodleian library, with the view of publishing a collection of the works of the an-

tropics was $\frac{11}{83}$ of the meridian circle, or $47\frac{10}{83}$° (Ptolemy's Magna Syntaxis), which is only $\frac{3}{83}$ of a second less than Ptolemy's.—But Eratosthenes's number, according to Cleomedes, as derived by Riccioli, was 23° 46′. And the same, as corrected by Riccioli, is 23° 31′ 5″.

Hipparchus, anno 140, before Christ, has retained the obliquity of Eratosthenes, viz. 23° 51′ 19″ 31‴. Yet the Chovaresmic tables, composed 830 years after Christ, exhibit the canonic obliquity of the Alexandrians, according to a Latin manuscript of D. Hatton, as 23° 51′.

Pytheas Massiliensis, 324 years before Christ, in Riccioli, makes it 23° 52′ 41″.

Aristarchus, anno 280, before Christ, by Savil's calculation, 23° 51′ 20″; but as deduced by Riccioli 23° 30′.

Strabo, the geographer, anno 30, after Christ, makes it $\frac{4}{60}$ of the circle, or 24°. And the same is stated by Geminus in Christ's time; by Tatius, by Proclus, by the astrologers, according to Noddam the Arabian, by Abraham Abenesdra, &c. But Noddam, the astronomer, who flourished about anno Dom. 1200, remarks that the obliquity was never observed greater than 24°, nor less than 23° 33′, though it continually decreases.

Cl. Ptolemy, anno 140, after Christ, having often tried it with his ring and his table, always found it nearly the same with Eratosthenes, or 23° 51′ 20″;

cient mathematicians; a specimen of which he printed in Greek and Latin; he also drew up a synopsis or view of the whole, afterwards published by Dr. Smith, entitled, Veterum Mathematicorum Græcorum, Latinorum, et Arabum, Synopsis. In consequence of this plan, first enjoined by Sir Henry Savilie, Dr. Halley published his edition of the works of Apollonius, and Dr. D. Gregory those of Euclid. Mr. Bernard undertook also an edition of the Parva Syntaxis Alexandrina; in which, besides Euclid, are contained the small treatises of Theodosius, Menelaus, Aristarchus, and Hipsicles; but it was never published. In 1676 Mr. Bernard was sent to France as tutor to the Dukes of Grafton and Northumberland, sons of King Charles the 2d, by the Duchess of Cleveland, who then lived with her mother at Paris; but the simplicity of his manners not suiting the gaiety of her family, he soon returned to the pursuit of his studies at Oxford. In 1691, being presented to the rectory of Brightwell in Berkshire, he quitted his professorship at Oxford, in which he was succeeded by Dr. David Gregory, from Edinburgh.

Toward the latter end of his life, as often happens to studious men, he was much afflicted with the stone; yet notwithstanding this and other infirmities, he encountered another voyage to Holland to attend the sale of Golius's manuscripts, as he had once before done at the sale of Heinsius's library. On his return to England, he fell into a languishing consumption, which put a period to his life in January 1696, in the 58th year of his age.

Mr. Bernard seems to have been more distinguished by a talent for a laborious research among the intricacies of old books and writings, than for clearness of perception or skill in composition. Besides his works already mentioned, and two papers in the Philosophical Transactions, the one above printed on the obliquity of the ecliptic, and a former on the places of 23 fixed stars, he was author of many other compositions. As, 1. A Treatise on the Ancient Weights and Measures. 2. Chronologiæ Samaritanæ Synopsis, &c. with many manuscripts left at his death.

for the distance of the tropics was between $47\frac{2}{3}$ and $47\frac{3}{4}$. Of these he chose nearly the mean 47° 42′ 40″, the half of which gives 23° 51′ 20″. And the same in his hypothesis of the planets. But Theo, in his shorter tables, through brevity omits the seconds.

Pappus, 390 years after Christ, by Riccioli, makes it 23° 30′; but by Commandine 23° 50′.

Theo, anno 370, after Christ, 23° 51′ 20″. But elsewhere in round numbers 23° 51′.

Prince Almamou, anno Christi 825, and the Arabian philosophers in his time, make it 23° 35′. And in this number Alfraganus, in his astronomy, acquiesces.

Mohammed Ebn Gaber Al Bategnius, at Racca, about anno Dom. 880 or 890, makes it 23° 35′. In this Al Bategnius prefers his own observations before Ptolemy's; and says, that with a very long alhidade or paralactic ruler, like those of Ptolemy, he carefully took at Racca the distance of the tropics 47° 10′, that is 59° 36′ — 12° 26′; and therefore the latitude of Racca 35°, which yet Ulughbeg makes 36° 10′, and Riccioli 36°.

Thabet Ebn Corra, anno Dom. 1210, according to Riccioli, but rather in 901, found it 33° 30′.

Abul Hosein Ebn Suphi makes it 23° 35′.

Abul Wafi Albuziani, and Abn Hamed Saganiensis, a very ingenious man, anno Dom. 987, at Bagdad, found the obliquity very near 23° 35′. And so a Persian author in Selden's Archives gives 23° 35′. As also the Persic tables of Chryococca 23° 35′.

Al Batrun Abul Rihan, anno Dom. 995, or according to Abulfarag 1070, by using a quadrant of 15 cubits radius, makes it 23° 35′.

But Abu Jaafer Alchazan, anno Dom. 970, with his associate Abufald, at Edessa, and others of that age, found it rather less than 23° 35′.

Almæon, son of Almansor, anno Dom. 1140, made it 23° 33′ 30″ according to Riccioli, or 23° 33′ to Clavius and Mæstlin.

Ismael Abulfeda, prince of Hama, anno Dom. 1311, retains 23° 35′; perhaps on the authority of Almæmon.

Prophat, the Jew, anno Dom. 1300 or 1303, gives 23° 32′.

Abu Mahmud Al Chogandi, anno Dom. 992, with a sextant of 40 cubits radius, and its arc divided to seconds, made it 23° 32′ 21″. And hence the astronomer Noddam affirms, that the sun's greatest declination was hardly ever found less than 23° 33′.

Arzachel, of Spain, between anno Dom. 1070 and 1089, takes 23° 33′ 30″.

The Persian Chojah Nasiroddin, at Maraga, anno Dom. 1261 or 1269, observed the obliquity accurately, and found it to be 23° 30′.

Ebn Shatir Damascenus, anno Dom. 1363, says, that he corrected the obliquity, making it 23° 31′, not neglecting the sun's horizontal parallax, which he says he found of the extraordinary quantity of 2° 59′.

Prince Ulughbeg, anno Dom. 1437, with other astronomers, by using great care and the largest instruments, found it 23° 30′ 17″ or 23° 30′ 27″.

The learned Jew rabbi Moyses ben Maimon, anno Dom. 1174, found it 23° 30′.

I have not consulted half the oriental astronomers, whose writings are preserved in the libraries of the university of Oxford. But from the above observations, and some others which I reserve to myself, I conclude that the obliquity of the ecliptic has been always the same from the beginning of the world.* For the later ages, by the assistance of better instruments, have properly corrected the error and excess of the ancient astronomy.

Account of a Salt Spring in the Bed of the River Wear, in the County of Durham; in a Letter to the Editor from Mr. Hugh Todd, Fellow of University College, Oxon, and Chaplain to the Bishop of Carlisle. N° 163, p. 726.

When I was at Durham, being informed of an extraordinary salt spring, and another mineral spring, about a mile or a mile and half out of town; I went to see them the first leisure I had. The salt spring lies on the north-east side of the town, at a place called Salt-water Haugh, near Butterby. It rises in the middle of the River Wear, and is only to be seen and tasted in the summer time, when the river water is discharged all on one side of the channel; for in winter, when the river is high, it loses its salts in the fresh streams, so that they are not perceivable. The water does not rise in one or two places only, but seems to bubble up equally in all parts of the channel, for the space of 40 yards in length, and about 10 in breadth; for wherever in that space the stones and sand are removed, the water presently springs up. But what I admired most is, that the saltest of all the springs issues out of the middle of a rock. As to the degree of its saltness, it was as high as any brine can be, and though but little in quantity in comparison of the fresh river, yet of that force as to give a brackishness to the streams 100 yards below. Those that have boiled this brine say, that it affords a great quantity of bay salt, not so palatable, yet

* It is now well known to astronomers that this opinion of Mr. Bernard has no foundation, as the series of observations down to the present time have shown a continual decrease in the obliquity of the ecliptic, at the rate of about 35″ in 100 years, by which it has now descended below 23° 28′; and that by calculations on the universal theory of gravitation, it appears that it will continue to decrease only to a certain limit; after which it will increase again to another higher limit, then return, and so continue reciprocating between those two limits continually.

as useful as ordinary salt is. It tinges all the stones of a red colour. The original of this spring cannot be fetched any farther off than the rock out of which it issues; for the sea, which is the great treasure of salt water, is at too great a distance to dispense any streams to this place; being 8 miles off, where nearest.

Exercitatio Geometrica de Dimensione Figurarum, Authore Davide Gregorio, in Academia Edinburgensi Matheseos Professore. Edinburgi in 4to.* 1684. N° 163, p. 730.

In this work the author first takes notice of a treatise of his uncle, Mr.

* This David Gregory was the nephew of the celebrated James G. inventor of the telescope of his name, and a worthy inheritor of his mathematical genius; a genius which has run through the family of Anderson and Gregory for several generations. Indeed it is a singular fact that our author, David Gregory, and two of his brothers, James and Charles, were all mathematical professors at the same time in three British universities, viz. David at Oxford, James at Edinburgh, and Charles at St. Andrews. David, the subject of the present memoir, was first elected to the mathematical chair at Edinburgh in 1684, being then only 23 years of age, and in the same year he published the book above noticed. In 1691, on the resignation of Dr. Bernard, he was removed to the astronomical chair at Oxford, being recommended and befriended on that occasion by Sir I. Newton and Mr. Flamsteed, in opposition to their friend Mr. Halley; who was rejected, as was said, on account of his sceptical principles. In this situation Dr. Gregory rendered great service to the mathematical sciences by several important works. As, some valuable communications to the Royal Society, published in their Transactions, vols. 18, 19, 21, 24, 25. In 1695, he printed at Oxford his Catoptricæ et Dioptricæ Sphericæ Elementa, being the substance of some lectures read at Edinburgh; and the same was afterwards twice published in English, by Dr. Brown, and by Dr. Desaguliers, with the addition of an account of the Newtonian and Gregorian telescopes, &c. It is not unworthy of remark that, in the conclusion of this treatise is found an observation, which shows that the construction of achromatic telescopes, which Mr. Dollond has carried to such perfection, had occurred to the mind of David Gregory, from reflecting on the admirable contrivance of nature, in combining the different humours of the eye. The passage is as follows: "Perhaps it would be of service to make the object lens of a different medium, as we see done in the fabric of the eye; where the crystalline humour (whose power of reflecting the rays of light differs very little from that of glass) is by nature, which never does any thing in vain, joined with the aqueous and vitreous humours (not differing from water as to their power of refraction) in order that the image may be painted as distinct as possible on the bottom of the eye." The master-piece of Dr. Gregory, however, is his Astronomicæ Physicæ et Geometricæ Elementa, published in folio at Oxford, 1702. This work is founded on the Newtonian doctrines, and was esteemed by Newton himself as a most excellent explanation and defence of his philosophy. The following year Dr. Gregory gave to the world an edition, in folio, of the whole works of Euclid, in Greek and Latin; being done in prosecution of a design enjoined by the founder, Sir Harry Saville, of publishing the works of all the ancient mathematicians. In prosecution of the same plan, our author engaged soon after, with his colleague Dr. Halley, who had been elected the geometry professor, in the publication of the conics of Apollonius, when his useful labours were arrested by the hand of death, in 1710, being only the 49th year of his age.

Besides those works published in our author's life-time, as above-mentioned, he left also in manu-

James Gregory, printed at Padua, in the year 1667, entitled Vera Circuli et Hyperbolæ Quadratura, &c. Wherein he shows, that any sector of the circle, hyperbola, or ellipse, is the limit of a certain converging series; whose first two terms are a and b, of which a is a triangle, which, as to the circle or ellipse is inscribed, but as to the hyperbola, circumscribed to the said sector; and b, a trapezium, which contrarywise, as to the circle or ellipse is circumscribed, but as to the hyperbola, inscribed to the said sector; the two second terms, $\sqrt{ab}$ and $\frac{2ab}{a + \sqrt{ab}}$: the two thirds in like manner derived from the seconds as these are from the first. And so infinitely, with other things appertaining to the same, and to other such like approximations.

He then mentions another method, different from that former, published at London the year following, by Mr. Nicholas Mercator, in his Logarithmotechnia, for squaring the hyperbola by the help of infinite series. Approved also and demonstrated afterwards by Mr. James Gregory, apagogically. But, that a general method for such cases was yet wanting.

That about the beginning of the year 1670, he understood from Mr. John Collins, that Mr. Isaac Newton, professor of mathematics at Cambridge, had before that time a general method for such quadratures, and other like cases. Which, as an instance, Mr. Collins sent him an example of such an infinite series accommodated to a circular zone; namely, if the radius be r, and the breadth of the zone b, the zone is equal to $2\,br - \frac{b^3}{3\,r} - \frac{b^5}{20r^3} - \frac{b^7}{56r^5} - \frac{5\,b^9}{567r^7}$, &c.

That Mr. James Gregory was in pursuit of like methods of infinite series, but was prevented by death; and, except some particular examples, left nothing in his papers, yet found, that might declare his method and way of finding such examples.

That himself, Mr. David Gregory, in this treatise explains a method, which may suit such examples of his uncle. This he performs chiefly by the principles of indivisibles and the arithmetic of infinites, already known and received by geometricians as sufficiently demonstrated; applying them to particular cases, in parabolas, hyperbolas, ellipses, spirals, cycloids, conchoids, cissoids, &c.

Together with divers expedients or preparative observations, by division and extraction of roots in species, for reducing compound quantities into infinite series, thereby rendering them capable of having the method of infinites ap-

script, a short Treatise on the Nature and Arithmetic of Logarithms, which is printed at the end of Keill's translation of Commandine's Euclid; and a Treatise on Practical Geometry, which was afterwards translated and published in 1745, by the celebrated Mr. Maclaurin. He left also in manuscript a Commentary on Newton's Principia, which Newton valued and kept by him for many years after the death of the author.

plied to them. And concludes with expressing his hopes and expectations that Mr. Newton's methods to this purpose, long since contrived, but not yet published in print, will now shortly be made public.

L'Art de tailler, &c. The Art of pruning Fruit Trees, and a Tract on the Use of Fruits of Trees, for preserving us in Health, or for curing us when we are sick. Translated from the French Original of a Physician of Rochelle. London, in 8vo. 1685. N° 163, p. 733.

The author of this book having taken a particular delight in the cultivation of fruit trees, and in considering the use of their fruits, gives here the rules which many years experience has taught him on those subjects, viz. the cultivation and ordering of the trees, with the uses and management of the fruits.

Two Letters from that experienced Oculist, Dr. Turberville, of Salisbury, to Mr. William Musgrave, S.P.S. of Oxon, containing several remarkable Cases in Physic, relating chiefly to the Eyes. N° 164, p. 736.

The first Letter, dated London, Aug. 4, 1684.—The disease which, in some late discourse with you, I named bursa oculi, or the pouch of the eye, was a bag without matter in it, like an empty purse, on the white of the eye, under the upper lid, it hung flag about the length of a thumb nail. Another person had no visible disease in his eyes, but could not see at all, unless he squeezed his nose with his fingers, or saddled it with narrow spectacles, and then he saw very well; him I carried to Mr. Boyle as a fit subject for so great a philosopher to make his remarks on.

A maid, 22 or 23 years old, came to me from Banbury, who could see very well, but no colour beside black and white. She had such scintillations by night, with the appearances of bulls, bears, &c. as terrified her very much; she could see to read sometimes in the greatest darkness for almost a quarter of an hour.

The second Letter, dated Sarum, Oct. 5, 1684.—A saddler's daughter of Burford, had an impostume which broke in the corner of one of her eyes, out of which came about 30 stones, splendid, and as large as pearls.

A person in Salisbury had a piece of iron or steel stuck in the iris of the eye, which I endeavoured to push out with a small spatula, but could not; but on applying a loadstone it immediately jumped out.

Another person had for a long time been troubled with a great pain and convulsions in his cheek; you might cover the place where the pain was with a penny; the convulsions drew his mouth, face, and eye, aside: he had used many things prescribed him by physicians and surgeons, but to no purpose.—I applied a cupping-glass to the place with fired flax in it; then scarified, and cupped him again; after which I put on a plaster of diapalma, and he was perfectly cured.

I was consulted by a maid, who had a pustule broke in her eye, out of which there came fine small sand, like chalk, for many weeks together. By the use of purging, fumigation, and some topics, she recovered her sight in a very great measure.

An Ingenious Proposal for a new Sort of Maps of Countries, with Tables of Sands and Clays, such chiefly as are found in the North Parts of England, drawn up about Ten Years since, and delivered to the Royal Society March 12, 1683. *By Martin Lister, M.D.* N° 164, p. 739.

We shall be better able to judge of the formation of the earth, and of many phænomena relating to it, when we have duly examined it, as far as human art can possibly reach, beginning from the outside, downwards. As for its more inward and central parts, I think we shall never be able to confute Gilbert's opinion, that it is altogether iron.* And for this purpose it were adviseable that a soil or mineral map, as I may call it, were devised. The same map of England may, for want of a better, at present serve the turn. It might be distinguished into countries, with the rivers and some of the noted towns put in. The soil might either be coloured by a variety of lines or etchings; but great care must be taken very exactly to note on the map, where such and such soils are bounded. As for example, in Yorkshire, 1. the woods, chalk, flint, and pyrites, &c. 2. Blackmoor, moors, sandstone, &c. 3. Holderness, boggy, turf, clay, sand, &c. 4. Western mountains, moors, sandstone, coal, ironstone, lead ore, sand, clay, &c. Nottinghamshire, mostly gravel, pebble, clay, sandstone, hall plaster, or gypsum, &c. Now if it were noted how far these extended, and the limits of each soil appeared on a map, something more might be comprehended from the whole, and from every part, than I can possibly foresee, which would make such a labour very well worth the pains. For I am of opinion such upper soils, if natural, infallibly produce such under minerals, and for the most part in such order. But I leave this to the industry of future times.†

I shall entertain you at present with a scheme of sand, and another of clay,

* De Magn. lib. I, cap, 17. Tellus in interioribus partibus magneticam homogenicam naturam habet.—Orig.

† This is a very ingenious proposal.

such only as I have chanced to meet with in England. As for the sand, I have some reasons to think, that it was once the most exterior and general cover of the surface of the whole earth. Because all our northern mountains are more or less covered with it at this day, and the higher the mountains still the more and the coarser the sand; because the rivers arising in the mountains do yet daily bring it down in great quantities, and that it has been so in all probability in all ages, since the first rains fell upon the face of the earth; which seems to me to be truth like, in that the sea-shores or mouths of rivers are usually barred with it; besides the sandy sea-grounds in most places of the sea, and (which seems a clear evidence for the length of time) for that the low grounds near these rivers (which have been in all ages upon record, mosses) if you pierce so deep into them as to discover their bottom, you meet with this mountain sand in great quantities, and in some places a moss under that, and the same sand-beds under that. Now if we consider how long this moss or turf is in growing, it being mostly the leaves and roots of plants, we must allow very many ages for this purpose. And although Herodotus boldly conjectures that Egypt long before our times would be dammed up and useless, by the great plenty of mud yearly brought down that vast river; yet it does not appear that the country is much different from what it was in his time, so that the sand and mud are still carried to sea.

Another argument of the sands being the universal cover of the face of the earth is, from the great hardness, and consequently the durableness and unalterable quality of this mineral, above any other in nature. For though many things are called sand, from the smallness and little cohesion or dryness of the grains, yet this kind of mountain sand above all others keeps its natural and original magnitude, and is not made, as most sand is, by the attrition and wearing of one particle of stone against another; but is of a constant and durable figure; and therefore it seems to me for this reason to be the most fit for an outside or cover to the globe of the earth.

And if it shall be objected, that although we grant the high mountains of England and Europe are usually first bedded with sand-rocks, if not still covered in many places with loose sand, yet are there other mountains, as the high wolds all over England, not so, but their uppermost beds of stone are soft chalk, and on the smooth surface no appearance of any sand. This indeed is in part granted; but that there is nowhere any sand upon the chalk mountains, is not true; for to instance in those inland sand hills above Boulogne in Picardy, which sand is the very same with that on the sea-shore at Calais, and although this is not England, yet the sea has but accidentally divided us; for from Dunstable ex. gra. in England, even as far as the walls of Paris by Calais, is as it were a continued wold of chalk and flint. What difference there is between the wolds mountain sand, and that of the northern mountains, will best appear in the table. Now the nakedness of the wolds is from the smallness of its sand, which readily yielded not only to the rain that fell but to the wind also. Which is evident from that vast tract of sandy hills which bound the coasts of France, Flanders, and Holland, and which have made their coasts so shallow in respect of ours, as being in great part blown off the Yorkshire, Lincolnshire, Suffolk, or Essex, and Kentish wolds, and wrapped up upon their coasts; and the reason of this is partly from the more constant westerly winds blowing over from our coasts, and also of the meeting of the two tides, viz. that of the Channel, and that other of north flood upon their coasts.

I am well aware, that the finding of cockles or shells, as most writers are pleased to call them, upon mountains, and sand also there, is by the same Herodotus used as an argument of a great deluge or inundation of waters; but as I have elsewhere, I think, demonstrated, that the rock cochlites are no shells; so neither can I grant that the sand was adventitious to the mountains, but naturally originated there; for that it is there plainly to be found some loose and the rest in beds yet unloosened; as I could name very many places, for instance, Silden and Thorpe Fells, in Craven, this mountain sand is a white and transparent pebble, and some of it is small and easily swept and blown away, so is there much of it upon the high mountains mixed with white pebbles of greater size.

It is the character of this sand not to yield to fire, as flint will do; and though it agree with that and some other metals to strike fire from steel, yet it does not calcine, as flint will be brought to do. And therefore this sand is the true tarso of the Italian mountains, of which the fine Venetian glass is made; and for this reason, the flint glasses were here in England ill compounded, the foreigners mistaking the materials, which yet our country affords in plenty all over the northern, and, I doubt not, the western mountains too. I have seen from the Scotch mountains very excellent and large.

A Table of Sand, drawn up about Ten Years since; such chiefly as I have found in the Northern Parts of England.

Sharp or rag sand, composed of small transparent pebbles, naturally found upon the mountains, not calcinable.

FINE.—*White*; Stitneham Moor in the road washed up very white pebble. Flamborough head, of which the light house there is cemented. Calice sand, burns reddish, but falls not in water.—*Grey*; Seaton banks near Hartlepool or the Tees mouth, Escrick, in the gravel-pit there.—*Reddish*; The pillow sand in the Baltic.—*Brown*; In a spring at Heshington. The sand at the bath in Somersetshire.

COARSE.—*Grizly*; Acome near York drifted sand; Hutton Moor washed; Thorpe fells; Ouze at York; Nid at Mountain; dug up at Rawcliff, near Snath; Wharf at Ickly and Denton; Air at Carleton in Craven; Eure at Bolton.—*Brown*; Gauton; Santon in Lincolnshire; Bomeby common; Skipwith common.

Soft or smooth with flat particles.—From LIMESTONE; At . . . . . . in Yorkshire; A vein at Oswell beacon in Lincolnshire.

With mica of glittering particles, of Westmoreland; *Silver-like*; Sea sand about the Scilly islands; in Cleveland and about Scarborough; Ouze dust, or sediment at Rawcliff. *Gold-like*; A vein of mica in Heslington gravel pit; mica argentea in red sand rock near Rippon plentifully; mica aura of Cleveland.

Also I here give a scheme of clays, as well because it seems to be another coat of the terrestrial globe in the more depressed and hollow parts thereof, as because the mixture of sand and clay is not unusually called earth. Yet this term being too large, it will be convenient to limit it to such a mixture as we usually find on the surface of the ground, which has always in it, besides such sands and clays as either the soil naturally produces, or have by floods or winds or other accidents been brought thither, a great part of the rotten parts of plants and animals. And in this sense turf is earth, which is mostly where the erica or heath grows, because it is made up of the deciduous leaves of that plant, which being by the current of showers brought together, make up the moors, mosses, and fens, and in the mountains in hollow basins or depressures without vent, mosses of incredible depth, 1 or 2 fathoms ordinarily in the same kind of black earth, called peat or turf.

A Table of Clays.

PURE, that is, such as is soft like butter to the teeth, and has little or no grittiness in it.—*Greasy*, to be reckoned among the medicinal earths, or terræ sigillatæ. 1. Fullers earth; yellowish at Brickhill in Northamptonshire; at . . . . . . under the Yorkshire wolds; brown about Halifax; white in Derbyshire lead mines. 2. Boli in Cleveland; at Linton upon Wharfe. 3. Pale yellow in the marl pit at Ripley. 4. Cow-shot clay, or the soap scale lying in coal mines. 5. A dark blue clay or marl at Tolthrop

Harsh and *dusty*, when dry.—6. Creta properly so called, or the milk-white clay of the Isle of Wight. 7. The potters pale yellow clay of Wakefield Moor. 8. The blue clay of Bullingbrook pottery in Lincolnshire. 9. A blue clay in Bugthorp Beck, in which the astroites are found. 10. Yellow clay in the seams of the red sand rock at Bilbro. 11. Fine red clay in red sand rock, at Bilbro, at Rippon. 12. A soft chalky blue clay. 13. A soft chalky red clay, at Buttercrain.

Stony, when dry.—14. A red stone clay; 15. A blue stone clay, in the banks of Whitcar beck, near Leppington, and at Housam in the Milscar. 16. Clunch, a white stone clay in Cambridgeshire.

Mixed with round sand or pebble.—17. The yellow loam of Skipwith moor, Yorkshire. 18. A red sandy clay in the right hand bank of the road beyond Collingham, near the lime kilns going to ..... 19. A red sandy clay in the red sand rock near Rippon.

With flat or thin sand, glittering with mica.—20. Crouch white clay Derbyshire, of which the glass pots are made at Nottingham. 21. Grey or bluish tobacco pipe clay at Halifax. 22. A red clay in the red sand rock at Rotherham.

Observations of the Solar Eclipse July 2, 1684, at Oxford; in a Letter from Dr. Edw. Bernard, Astron. Prof. Oxon, to Mr. John Flamsteed, Astron. Reg. Also at Lisbon by Mr. Jacobs; at Dublin by Mr. Ash and Mr. Molyneaux; and at Tredagh by Mr. Osburn. N° 164, p. 747.

The phases of this eclipse were observed by Dr. Wallis; and the true times of the same were observed by Mr. Caswell and Dr. Rooke, by taking some altitudes of the sun to correct the clocks.

At 2^h	3^m	0^s	.. The eclipse began.
2	50	9	.. Middle of the sun's obscuration or 6 digits.
3	8	24	.. The greatest obscuration $7\frac{4}{10}$.
3	37	24	.. Again the central obscuration or 6 digits.
4	21	14	.. The end of the eclipse.

Mr. Jacobs at Lisbon, July 2, O. S. The beginning of the eclipse at 1h. 30m. exactly; the ending at 4h. 12m.

Mr. Ash and Mr. Molyneaux, at Dublin, though the day being much overcast hindered them from taking any thing accurately, yet gave some account of their observations, viz. that toward the middle of the eclipse, having a short view of the sun, they judged that about 8 digits were covered: at the ending also, having a faint view of it they assigned its end at 3h. 56m. P. M.

Extract of a letter from Mr. William Molyneaux, S.P.S. of Dublin, dated Sept. 2, 1684.—The same eclipse was observed by one Mr. Osburn, near Tredagh, in Ireland, lat. 53° 40′; the beginning 1h. 37m. 30s.; the end 3h. 56m. 20s.

Casp. Bartholini Thom. F. de Ductu Salivali hactenus non descripto, Observatio Anatomica. Translated and abridged from the Latin. N° 164, p. 749.

In the writings of Galen, Haly Abbas, Avicenna and Isaac, some obscure notice (the author observes) is to be found of the salivary vessels; but it is to Steno and Wharton, those celebrated anatomists of modern times, that we are indebted for an accurate anatomical description of them. The present communication offers some particulars not observed by them. The last mentioned anatomists have, each of them, described a duct (an upper and a lower one) leading from the parotid and maxillary glands into the cavity of the mouth; but to these two ducts this author adds a third; which arises from the sublingual gland, accompanies the ductus Whartonianus, and opens, by an equally manifest orifice, under the tongue, in the same place where that (the Whartonian duct) does.—The gland, indeed, from which this third vessel proceeds, is described by Steno, under the name of the sublingual gland; but he erroneously assigns to it several small excretory vessels, which belong to those minute clusters of glands lying near this sublingual gland, but distinct from it.—The author first discovered this duct in March 1682, as he was examining the head of a calf. Searching with a probe for Wharton's duct, he slipped it (the probe) accidentally into this duct, till then unknown.—This discovery was afterwards confirmed by observations made on other animals, such as sheep, the bear, and a lioness, to which last the accompanying engravings refer.—This communication concludes with some remarks on the structure of conglobate and conglomerate glands and their distinctive characters.

Explanation of the Figures from the Dissection of a Lioness.—Fig. 3, pl. 3, shows the inferior maxillary gland A, with the salivary duct of Wharton BB, and at the same time the adjacent sublingual gland C, with its salivary duct D (now first described) the various ramifications of which are seen dispersed through the whole gland.

Fig. 4, shows the two orifices, on each side, of the inferior salivary ducts; viz. that of Wharton's and that discovered by this author; their scite under the tongue is marked by the points of the probes aaaa, passing through the said orifices under the tongue b, which is here turned a little upwards from the lower jaw c, in order to give a clearer view of the parts.

Essays of natural Experiments made in the Academy del Cimento, under the Protection of the most Serene Prince Leopold of Tuscany. Translated by Richard Waller, Esq. F.R.S.* N° 164, p. 757.

This work was published in Italian, in the year 1667, and contains several

* The Academy del Cimento, that is, of Experiments, at Florence, was one of the most early established in Europe, having commenced in the year 1657, under the patronage of prince Leopold of Tuscany, afterwards Cardinal de Medicis, and brother to the grand duke Ferdinand the second, who was himself no mean philosopher and chemist, and the inventor of the thermometer, the con-

curious experiments, made in the Academy del Cimento, at Florence.—The Italian author prefixed a preface, explaining the design of the academy, to advance real knowledge, &c.

1. The experiments are comprised under 10 heads, treating of several subjects. Having described some of their instruments, as thermometers (of which they seem to bid fairest for the invention; the first having been brought from Florence, and shown here, though they were reduced to a standard, and received their perfection hence), the pendulum, and other instruments. They come then to their first head, the air's pressure; where most of the known mercurial experiments are tried and confirmed, objections answered, &c. with several experiments made in vacuo, both on animate and inanimate bodies.

2. Under the second head, on freezing, they are very curious in examining the expansion, force, and proceedure of freezing, both natural and artificial. To these are added, some experiments about the alteration of the capacity of vessels by heat and cold.

3. Experiments to show water incapable of compression.—4. Against positive levity.—5. Magnetical experiments.—6. On electric bodies.—7. About the change of colours of fluids by the mixture of other fluids; where it is observable, that water distilled in lead inturbidates all spring, river, and Bath waters. —8. About the motion of sounds made with several cannon, and musket shot. —9. Of projected bodies, their motion.

The 10th contains a miscellany of experiments not reducible to any of the former heads; as, the comparative weight of air and water; difference of weight of bodies hot and cold; force of heat in rarefaction. Of glass, whether penetrable: of light, and its quick motion: of burning glasses: of bodies affording light when broken to pieces. Experiments on the digestion of animals, &c.

struction and use of which may be seen in the essays above noticed, which are a collection of experiments made by the early members of this academy. Indeed the sole and professed object of the academy, was to make and record experiments in natural philosophy; a practice which had been introduced by Galileo, Torricelli, Aggiunti, and Viviani, who had thus prepared the way for this institution. Among its early and respectable members were, Paul del Buono, who in 1657 invented the instrument for trying the incompressibility of water, viz. a thick globular shell of gold, having its cavity filled with water; then the globe being compressed by a strong screw, the water came through the pores of the gold, rather than yield to the compression: also, Borelli, well known for his ingenious treatise De Motu Animalium, and other works; Candide del Buono, brother of Paul; Alex. Marsili, Vincent Viviani, Francis Redi, and the Count Laurence Magalotti, secretary of the academy, who collected and published the above volume of experiments. But, notwithstanding these hopeful beginnings, we have heard little or nothing more of the academy since that time.

A Letter from Mr. John Flamsteed, concerning the Eclipses of Jupiter's Satellites for the Year following 1685, *with a Catalogue, &c.* N° 165, p. 760.

This is another of those predictions of the eclipses of Jupiter's satellites, which Mr. Flamsteed was in the practice, for several years past, of announcing in this way after the manner of an almanack, containing a list of the times, nearly when they might be expected to happen in the year following; that astronomers might be apprised of the times when to prepare for observing them more accurately.

On the Effects of the great Frost on Trees and other Plants, Anno 1683. *By Mr. Jacob Bobart.* N° 165, p. 766.

Nothing seemed more surprising, though generally known to be true, than the cleaving or splitting of trees in the severe frost of 1683-4.—Not only oaks, but other trees were also cleft, as elms, and ash of considerable bulk and value; also walnut-trees in several places suffered by this calamity. Yet oaks were most of all affected, some being rent in such a manner as to be seen through, and with a noise like the report of a gun. Which clefts were not only to the same point of the compass, but sometimes on one side only, sometimes on two, three, or four several places, dividing or quartering the tree, and sometimes quite through. Also, not only in the bodies, but continued into the larger boughs and limbs of the tree, and sometimes descended into the superficial roots, but not to those very deep in the earth; the frost, though severe, not reaching considerably deep; but several shallow roots so knotted and knurled as not to be wrought upon with beetle and wedges, are known to be cleft by the frost. And it is much to be suspected, whether any of such cloven trees can be so perfectly sound timber, if proved by the saw and axe, as they ought to be; for if so, all might equally suffer, the air having alike access to one as well as the other. Whatever it was that gave occasion to some only, might prove a matter worthy of enquiry. A great part of the cause is supposed to be some imperfections in such trees, and the large sap-vessels and unnatural cavities therein, which some call wind shaken.

But by what means soever this may happen, it is certain that some trees are much more sound than others, and that some prove full of inbred diseases and cavities, before they are cut down, which cavities and stretched vessels being filled with too great a quantity of aqueous and undigested sap, are thereby rendered capable not only of condensation but freezing also; which being sufficiently known to employ more room in the state of ice than when formerly liquid, might probably cause these breaches.

It need not seem improbable, that ice should be able to tear the oaks or other trees, when we consider its great force and elastic power; as appears from many experiments of Mr. Boyle, in his history of cold; for strong vessels of several kinds of metals, being filled with water, close stopped and exposed to the cold, could not withstand the expansive force of the inclosed ice, but were found cleft and broken; as for instance, a strong barrel of a gun close stopped, with water in it, and frozen, has been rent longwise, and never across, just like the bodies of the trees.

Some trees and shrubs seem to have their vessels and passages so straightened, that they seem as it were shrunk with cold. Thus we see trees with their bark shrivelled, and their passages half stopped, whose sap now only squeezes, and with difficulty passes through the dried and narrow pores and passages of the body and branches. And sometimes this distemper is so prevalent, that whole branches of a tree are killed, when the other part is indifferent well. Some liquids, such as essential oils, do rather shrink than expand by freezing; and empyreumatic oils will hardly freeze at all, but waste; which may suggest what some trees are made of, or abound in; as firs, pines, &c. which are capable of enduring the cold of Norway and other such countries.

Yew and holly were in some places quite killed, and in many places they lost their leaves, and their bark was damaged. The furze in many places was quite killed. Common broom proves a degree hardier. In some places the sunny side of a juniper bush proves scorched between sun and cold, but that proves one of the most hardy of our native greens; so that it is hard to say what is winter-proof, even among our natives, except box and ivy, which stand in defiance of all.

In gardens, which generally are nurseries of exotics, this calamity principally bent its force against winter-greens, such as alaternus, commonly known by the name of phillyrea, and the true phillyrea also, which are generally killed, though some upon cutting down spring again. Also common bays in most places are killed, and laurel too in some places killed, or half dead. Rosemary, laurustine, halimus, arbutus, white jessamine, and others which seldom fail, are generally killed through the whole country. But if for the future, in such times of extremity, the surface of the ground, the bodies of such things as are here recited, and fig-trees, were well covered with straw, to keep off the frost, it might so preserve them to the following spring; though their tops, being too large and high to be capable of such covering, might lose their present leaves and beauty.

Several among those with deciduous leaves, have been sufferers; as arbor vitæ, the young plane trees, paliurus, the Aleppo ash, in some places the locust tree, and in most hedges the common large bramble, and some others, which upon cutting most of them spring out again. But such greens also as we receive from abroad, as oranges, lemons, myrtles, pomegranates, the perfuming jessamines, and various other rarities, have in many places suffered extremely, especially in houses of weaker defence.

Among plants, herbs, and flowers, there has been great destruction also, and many of common use, as most of the artichokes of England, and winter cauliflowers, sage, thyme, mastic, lavender, laven-cotton, and divers others were generally killed, except such as happened to be new planted that year, and so low that they had the enjoyment of the kind covering of a little snow, which proves the most natural feeding, and warm covering of any, so far is it from being cold and unkindly to them.

A Letter from Mr. Anthony Leewenhoeck, F. R. S. Dated April 14, 1684; *containing Observations on the Crystalline Humour of the Eye,* &c.* No. 165, p. 790.

In my letter of Sept. 7, 1674, I communicated my observations concerning the crystalline humour of the eye; which have been inserted in the Phil. Trans. N° 108, where the crystalline body or humour of the eye, is in hardness much like a preserved nutmeg; which with a razor I cut in two, and so observed it in parcels, and found it to consist of many orbicular scaly parts, lying over one another, which had their beginnings from the centre; all these parts again consisted of crystalline globules: and having suffered the said crystalline humour to dry for 3 days, it became so hard, that in the cutting, it broke in pieces, as if it had been hard rozin. And observing again these parts, I found therein not only the aforesaid orbicular scaly substance; but further that each scale was composed of other ring-like parts, and that these second were contrarily posited to the others. I compared the scaly parts to a globe, made out of a number of thin papers, laid upon each other; and that every paper or scale was again constituted out of so many parts, as there may be lines drawn upon a globe, reaching from one pole to the other.

I have sometime since made several more observations, concerning the eyes of oxen and cows; thinking that I had not yet discovered the make of the crystalline humour of the eye, so thoroughly as I might have done. I there-

* From a paper of Mr. Home's (inserted in the Phil. Trans. for 1794), stating some facts concerning the late Mr. J. Hunter's preparation for the Croonian lecture, it appears that, although Mr. Leewenhoeck was the first discoverer of the fibrous appearance in the crystalline humour of the eye; yet "the discovery of an eye (that of the cuttle fish) in which this structure of the crystalline humour is perfectly distinct, and in which all the circumstances of course and situation are determined, is due to Mr. John Hunter."

fore first examined the film or membrane which encloses it, and separates it from the other humours of the eye; and conceived that it was constituted of threads; but at other times, though great diligence was used, I could observe none at all.

In these inquiries, I have sometimes seen the impressions of the thread like substance of the outermost scales of the crystalline humour, in the forementioned membrane; whence I concluded, that one use of it might be, to fill up all the unevennesses caused by the threads in the superficies of the crystal, and so to constitute a perfect round. Another use of it may be, by compressing the crystalline humour to alter its figure, and make it part of a greater or less circle: and hence it may not improperly be called a muscle.

Although it did not plainly appear that the film is constituted of threads woven together, I make but little doubt of it; because formerly, in the thinnest membranes I have seen these thread-like appearances; and also having examined the parts thereabout, I found a great many streaks or lines, seeming to be lymphatic vessels, designed for the nourishment of the humours of the eye; these I traced till they entered into the coat of the crystalline humour, and then they grew so small that I lost sight of them.

I further observed that the crystalline body was compounded of thin scales, placed upon one another. These seemed about 2000 thick; for the axis, where it was longest, was $\frac{2}{3}$ of an inch, so that the distance from the centre to the circumference is as $\frac{1}{3}$ of an inch: now the length of an inch being 600 hairs breadth, $\frac{1}{3}$ must be 200 hairs breadth, which being multiplied by 10, the number of scales in the breadth of a hair, make 2000 scales, the thickness of this crystalline body. I have further observed, that each of these scales is constituted of threads, which in a very neat order lie by one another, so that each of these scales is the thickness of one of the threads. When we view the crystalline body with attention, as it comes fresh out of the eye, we find it excel in transparency the purest glass, though it is composed of so many thousand threads, and that they lie very close together, so that it might justly be wondered how the light can pass through them in right lines, which is absolutely necessary; for were it otherwise, the crystalline body would appear white, and not transparent. To amuse some curious persons, and to represent the crystalline body, yet plainer to their sight, I have taken a small tennis ball, and wound the same about with a very fine cord, having before stuck in many small pins, in the places where it was to be kept from slipping; then I smeered the ball over with strong glue, and when it was well dried, took out all the pins, and then the ball with the cord wound about it, represents the crystalline body of the eye.

I also took out of the eyes of sheep, hogs, dogs, &c. the crystalline bodies, and ordered them as I had done those of an ox; and have found not the least variation, either in the scaly parts, or in the course of the threads, composing those scales.

I have often endeavoured also to discover the nature of the vitreous humour of the eye; and which surrounds for the most part the crystalline humour; as I imagined it was no watery matter, but rather a transparent muscle. But notwithstanding all the means used, I could not make the least discovery thereof, as this matter always changed into a watery substance.

I further examined the crystalline body of the eyes of fishes, which are perfect globes; and found them also constituted of like thin scales, lying one over another, as in the former animals: and each scale also composed of threads; but these threads run not in the same manner as those of other animals; yet, notwithstanding all the industry used, I could never discover their true course; for when the threads approach the centre, they appear so thin, and are so close joined together, that the sight cannot trace them; and causing such a confusion, that I cannot be certain whether they end in the centre, or return again from thence.

I have also examined the crystalline bodies out of the eyes of birds, only to view how the threads of the scales, constituting also the crystalline body, run; and after many observations, I have discerned that the threads constituting the crystalline body of a turkey-cock, are extended like those in fish; but as the crystalline body of a fish's eye is perfectly globular; those of birds are a flattish round towards the cornea tunica. And when from without the crystalline body of the eye of a turkey-cock, I had with a very sharp knife taken off many of the scaly parts, to bring it to a smaller globe, it changed its figure, and became an oval; the threads being, where they meet, so thin and small, that at last they are not distinguishable. From whence we may conclude, that the threads of the scales, which lie nearest the centre, are in the midst thin, and thus make an oval figure: and that when the crystalline body increases in magnitude, the threads become then in the midst thicker; and thus constitute a flat round; as I have perceived it for the threads in the crystalline body of a turkey-cock, in their thickest part, were thicker than those in an ox, hog, sheep, &c.

Before concluding, I cannot but mention, that I have by several ways and means, seen with my naked eye, a thready substance, like that of the crystalline humour. I shall only mention two of them, viz. I take a clean wine glass, and hold the rim close against the pupil of one of my eyes, while the other eye is closed; and looking thus firmly through the rim of the glass, against the flame of a candle, or other light, I perceive the thread-like appearance above-mentioned; as if through a microscope, I had beheld a piece of a scale of the crystalline body of the eye. Or closing one eye, I hold the fingers of my hand before the other eye, so close together, that they leave but a small opening between them: this small space between the fingers, through which the eye receives the light of the candle, will represent a similar thread-like appearance, as in the former instance.

I have often been aware of a moisture lying on the outside of the pupil of the eye, containing some very few small globules; which, as often as we close our eye-lids, change place. From hence may be learnt the necessity of the eye-lids in us; and why fishes, which continually live under water, need them not. But should men and other animals, that live out of the water, not have them, they would soon be blind: for if the eye-lids by their closing did not constantly moisten the eye, its superficies would dry up and rumple; and especially in the sun, or before a hot fire. Also it is not improbable, that from the inner part of the eye a moisture continually issues through the cornea tunica, which by the eye-lids is cleansed off; for when I had examined several hogs eyes, which had been scalded with hot water, to get off the hair, I mostly observed, that a thin film, which was on the cornea tunica of the eye, was somewhat singed; whereby it was easily separated from that film, which was immediately under it; and when I then pressed the eye a little between my fingers, I perceived in several places, a thin watery matter to soak through the horny film, and lie like a watery damp on a glass; and when I continued this pressing for some time, this watery damp increased into small drops, and at length run like water in a stream. This ought not to seem strange, considering the parts of the horny coat to be made of hollow vessels, like veins; very thin, and spread about in branches.

I have lately taken the horny coat of an ox's eye, and have separated from it 7 films, extremely thin; in each of which, were a number of interwoven, very clear, and transparent streaks; which I judge to be, many of them, blood vessels; but so small, that they contain none of the globules which cause the redness in the blood. By the rubbing of our eyes with the hand, we may so press these blood vessels, until they become so stretched out, that some of these bloody globules may get in, and remain there for some time, which may cause that redness in our eyes which comes by thus rubbing them.

But to return again to the eye-lids. As our muscles and other parts of our bodies rest not, unless posited as they lie, when we were yet in the womb; in like manner, the eye-lids are not at rest, till the eyes are closed; and therefore we cannot long continue the eye-lids open, but with force; and, that the same might not be wearied, we often close them, although we mind it not. I have seen people, who listening with attention to a discourse have closed their eyelids, according to my calculation, 6000 times in an hour; whereas others standing by them closed them not above 2000 times in the same time.

Since I wrote to Mr. Oldenburg, in the year 1673, that the matter causing the redness of our blood was constituted of globules; I have examined the blood of oxen, sheep, and rabbits, and have observed no difference in magnitude between the globules of those animals and those of men; so that I conceived that the matter which in general made all blood red was globules. But after I had tried the blood of a salmon, a cod, of frogs, &c. and found that the matter which caused the redness therein was made up of parts oval and flattish, as before said, I examined the blood of several birds; and have also observed, that the matter causing the redness of their blood, was also composed of like flattish oval parts, with those of fishes. So that I now concluded, that all animals, whether birds, fish, or other creatures that live in the water, have the parts causing the redness of their blood, consisting of the said flattish oval parts, and if hereafter I chance to find the contrary, I will inform you of it.

On the rising and falling of the Quicksilver in the Barometer. By Dr. Martin Lister, F.R.S. N° 165, p. 790.

It is to be observed that the quicksilver is not affected with the weather, or very rarely, in St. Helena or the Barbadoes; and therefore probably not within the tropics, unless in a violent storm or hurricane. The first is affirmed by Mr. Halley, who kept a glass near two months in the island St. Helena, and the other of Barbadoes stands upon the credit of our registers.

In England in a violent storm, or when the quicksilver is at the very lowest, it then visibly breaks and emits small particles, which may be considered as a kind of fretting or fermenting: and consequently at all times in its descent, it is more or less on the fret. In this disorder of the quicksilver, I imagine its parts are contracted and brought closer together; which seems probable, for then the quicksilver emits fresh particles of air into the tube, which increasing the bulk of the air, and consequently its elasticity, the quicksilver is necessarily depressed thereby, that is, by an external force or power; and also the quicksilver must of itself come closer together in its own internal parts, and so descend for both reasons. And that much air is mixed with it appears from the application of a heated iron to the tube, as is practised in the purging of it that way; and also because polished iron will rust when immersed in it, as some philosophers have lately observed.

Now whenever the quicksilver rises in the tube, which it certainly does both in hot and frosty weather, it may then be said to be in a natural state, free, open, and expanded, which it seems it always is within the tropics, and with us only in very hot, and very frosty weather. But when it descends, it is then contracted, as it mostly is in our climate of England, and probably more or less so in all places on this side the tropics. Which contraction plainly appears from the concave figure of both surfaces, viz. of the quicksilver in the tube, and of that which stagnates in the cistern. The difficulty seems to lie in the reconciling the same effect of the quicksilver rising in the tube, from such seemingly differing causes, as great heat and intense frost; for those who willingly assent to us in one particular, and grant that warmth is a probable cause of its restitution to its nature, may yet doubt how great frost likewise should do the same. I answer, that salts liquefied will coagulate or crystallize, that is, will return to their own proper natures, both in cold and in heat; and therefore, though most men practise the setting them in a cool cellar for that purpose, yet some, as Zwelfer, advise, as the best means to have them speedily and fairly crystallised, the keeping them constantly in balneo. Thus also the lympha of the blood becomes a jelly, if set in a cool place, and it is likewise inspissated by warmth. Again, it is no new opinion that water is naturally ice. And Borrichius, the learned Dane, has said something for it; and I may venture to add, in confirmation of that doctrine, that salt is naturally rock, that is, naturally fossile, not liquid; and yet this is most like ice of any thing in nature; not only on account of its transparency, but also for its easy liquefaction, and the sudden impressions and changes the air makes upon it; so that it is scarcely to be preserved in its natural state of crystallization. Also salts of all sorts seem naturally to propagate themselves in a hard state, and to vegetate in a dry form. The like is to be observed in quicksilver, of its being a hard rock, and also from its willingness to embrace upon all occasions a more fixed state, as in its amalgamizing with almost all sorts of metals.

If therefore quicksilver and liquids are nearest their own natures, and suffer less violence in very cold and very hot seasons; the humours of our bodies, as liquids, must probably, in some measure, be affected accordingly. And that therefore cold is healthful, I argue from the vast number of old men and women to be found on the mountains of England, comparatively to what are found elsewhere. Again, the blood itself, or the vital liquor of animals equivalent to it, is in most kinds of animals sensibly cold; for the species of quadrupeds and fowls are not to be compared for number to fishes and insects; there being in all probability above 100 species of these latter creatures whose vital juice is cold, to one of the former; but because we most converse with those whose vital juice is hot, we are apt to think the same of all.

Again I have observed, which I offer as an argument of the little injury intense cold does to the nature of animals, I say, I have seen both hexapode worms (which I compare to the tender embryos of sanguineous animals, because such are in a middle state) and flies of divers sorts hard frozen in the winter, and I have taken them up from the snow, and if I cast them against the glass, they would endanger the breaking of it, and make it ring like so much hard ice; yet when I put the insects under the glass, and set them before the fire, they would after a short time nimbly creep about, and be gone, if the glass which I whelmed upon them had not secured them.

It has indeed been noted by a very wise philosopher, in contradiction to our English proverb, which says, that a green Christmas makes a fat church-yard;* that the last plague broke out here at London after a long and severe winter 1665. But I reply, that that was accidental only, for that disease is never bred among us, but comes to us by trade and infection. It is properly a disease of Asia, where it is epidemical. And therefore by the providence of God we are very secure from any such calamities as the natural effect of our climate.

And for the same reason, I judge the small pox, so much raging at present, not to be from the season or temperature of the year, but from infection wholly; that also being an exotic disease of the oriental people, and not known to Europe, or even Asia Minor, or Africa at all, till a spice trade was opened with the later princes of Egypt, to the remotest parts of the East Indies, whence† it originally came, and where it rages more cruelly at this day than with us.

The like I think of the griping of the guts (dysentery) that it is a peculiar disease of the West Indies, and yearly received from thence, for this reason, that it is none of the tormina ventris of the ancients, and therefore called by a new name by such as have written of it, and also because it is yet scarcely known in any part of the north of England, or the midland counties.‡ So that we are not to judge or prognosticate of the salubrity or sickliness of a year from foreign diseases, but by the raging of such as are natural to the people of our climate.

* It has been shown by Dr. Wm. Heberden (Phil. Trans. for 1796) that contrary to popular opinion, the mortality in London is greater in severe than in mild winters.

† From other accounts it would seem more probable that the small pox is of Abyssinian origin.

‡ Dysentery is so frequent in the southern parts of England, especially during the autumnal season, that it cannot be properly numbered among exotic or imported diseases. The same exciting causes which give rise to this disorder in the West Indies, produce it here; but in this climate these causes operate in a weaker degree, hence the complaint is neither so general nor so violent as in the tropical regions.

Scotia Illustrata, sive Prodromus Historiæ Naturalis, &c. Authore Roberto Sibbaldo, M.D. Equite Aurato, Medico et Geographo Regio, et Regii Medicorum Collegii apud Edinburgum Socio. Edinburgi, in fol. 1684. N° 165, p. 795.

This author, being the king's geographer for Scotland, undertook to publish an atlas of that country, in a set of maps; preparatory to which, he printed this introductory account of that country, containing the geographical and topographical descriptions of several parts; including the curiosities and peculiarities, the natural history, mines, soil, productions, people, animals, vegetables, &c.

Extract of a Letter from Dr. John Wallis to the Editor Dr. Robert Plot; concerning two very large Stone Chimney-pieces, with a peculiar Sort of Arch-work thereon. N° 166, p. 800.

At Edgecot, in Northamptonshire, at the house of Tobias Chancey, Esq. I have seen in an old kitchen, now disused, two very large chimneys of stone-work, with a peculiar arch-work in the front, whereby, without the advantage of a discharger of timber, as is usual, to defend the arch-work from being over burdened, an arch of massy stone in each of them sustains itself, at a great length, though almost on a flat, being very little raised in the middle. As this seemed to me somewhat extraordinary, I caused a draft of one of them to be designed, as in fig. 2, pl. 3. In which the lower arch-work, very little raised from a flat, is made of large stones, so locked into one another, as appears in the figure, that there might be the less danger of any one dropping out, or being forced down, to the destruction of the whole. Over this, after some walling interposed, there is another arch to defend the former, more raised from the flat, made of lesser stones, and with straight joints, not locked as the former. Over all which the walling was continued upwards, in an ordinary manner, forming, with the back, two vast tunnels of stone, and between them, over the middle of the chimney, a large window. The dimensions of the parts are here subjoined, viz.

AB the breadth between the jambs, from inside to inside, 18 feet; CD the depth of the stones in the lower arch 22 inches, locked into one another with a crooked joint, and the number of them as in the figure; DE the distance in walling between the arches, 2 feet 7 inches; EF the depth of the stones for the upper arch, 15 inches, with a straight joint; GH the place of the two tunnels; K a window between them.

The other chimney facing it, on the opposite side of the same kitchen, was much after the same manner, but somewhat less, and with some difference in the masonry.*

Some Queries for examining Mineral Waters. By Sir William Petty, Knight. N° 166, p. 802.

1. How much heavier than brandy? 2. How much will common water reduce its taste? 3. What quantity of salt on evaporation? 4. How much sugar, alum, vitriol, nitre, &c. will dissolve in a pint of it? 5. Whether any animalcula will breed in it, and in how long time? 6. Whether fish, viz. trouts, eels, &c. will live in it, and how long? 7. Whether it will hinder or promote the curdling of milk and fermentation of liquors, &c.? 8. Whether soap will mingle with it? 9. Whether it will extract the dissolvable parts of herbs, roots, seeds, more or less than other water? i. e. whether it be a more powerful menstruum? 10. How galls will change its colour? 11. How it will change the colour of syrup of violets? 12. How it differs from other waters in receiving the colour of cochineal, saffron, violets, &c.? 13. How it boils dry pease? 14. How it washes hands, boards, linen, &c.? 15. How it extracts malt in brewing? 16. How it quenches thirst with meat or otherwise? 17. Whether it purges? in what quantity of time and with what symptoms? 18. Whether it promote urine, sweat, or sleep, &c.

Philosophical Solution of the Case of the young Man who became blind in the Evening, in N° 159. *By Dr. Wm. Briggs. Abridged and translated from the Latin.* N° 166, p. 804.

It is universally known, that copious vapours, rarefied by the heat of the sun, ascend in the day-time, and descend again after sunset, being condensed by the cold; and hence the air near the surface of the earth must be thicker. So likewise, perhaps the humours of the eyes of the young man may in the evening become thicker and more turbid; like also as urine, by heat or cold, becomes clear or turbid. So that, from such thickness of the humours, the rays of light may be so refracted as hardly to reach the retina, or, if they do reach it, act with too weak an impulse.

Now, all the described phænomena agree very well with this hypothesis. For, in the first place, the young man has been always subject to this complaint from his infancy, without any apparent disease in the eyes, the humours being af-

* In the above-described structure, the upper arch is intended to act as a discharging piece, to take off the weight of the superincumbent wall, and prevent it from forcing down the flat under arch, by pushing out its jambs at A and B.

fected in the same manner as the air near the earth, by the vapours after sunset. In the next place, this blindness comes gradually on him, like the vapours gradually descending in the evening. Thirdly, the changes of the moon seem not to have any effect on them, as the rising and fall of the vapours do not depend on that planet. Fourthly, perhaps the crystalline and vitreous humours may be so viscid, that, though the aqueous humour and tunica cornea be clear, they cannot be dissipated. Fifthly, the blindness remains the same both in summer and winter; like as the vapours fall in all seasons, in a quantity sufficient to show the effect.

Another correspondent gives it as his opinion, that as there is no observable dimness in the person's eyes, the cause of the phenomenon is probably owing to the disposition of the optic nerve; its little tubes, while filled with the solar rays, easily admitting visible forms, as they are called; but when deprived of them, they become flaccid, and unfit for vision.

A Discourse on the Sepulchral Lamps of the Ancients, read before the Philosophical Society of Oxford, May 7, 1684. By Robert Plot, LL. D. Director of Experiments to the said Society. N° 166, p. 806.

At the last meeting, I showed that the downy part of the mineral called linum asbestinum, earth-flax, or salamander's wool, would perform the office of a wick tolerably well, it having burnt from about 9 in the morning, being still supplied with oil, till about 4 in the afternoon, without sensible diminution. yet it was objected that on other trials a friability had been found in it, which argued some sort of consumption; and that allowing it invincible in the fire, yet there might be some danger of its being clogged by the impurities of the best oil that could be got, or any way prepared. To the first of which objections it was then answered, that there was no such absolute necessity of making use of that wool, but that another wick might be contrived, against which that exception could not lie; such as a metalline wire, especially of the best refined gold, that will draw up oil as well as any other wick, and so make a perpetual flame, provided it be supplied with a perpetual oil. In consequence, I tried a parcel of wire made of annealed iron, of a suitable size for a wick, which could not be made to succeed, nor have I much reason to think it will, after the most mature deliberation, either in wire of silver or gold. If, therefore, it be necessary that we must have a perpetual wick for making such sepulchral lamps as were used by the ancients, I think we must stick to the first kind, made of linum asbestinum, or salamander's wool; which if it in any way can be supplied with a perpetual oil, I hope you will not judge me far from effecting the matter.

Now that there may be such a bitumen or inexhaustible oil, I need carry you no farther than Pitchford, in Shropshire, where there is a naphta or liquid bitumen, that constantly issues forth with a spring there, and floats on the water; this might be separated before it joins the water into a duct of its own, and so conveyed to some place convenient for such a lamp, into which it should as perpetually distill as it does now into the fountain; and thus we have an oil as everlasting as our wick, nor need we fear any extinction if inclosed in a tomb or vault under ground, in however damp or moist a place; it being the characteristic of a bitumen, to burn best where there is moisture, as is evident upon affusion of water upon sea-coal. And this is one way in which we might have a perpetual lamp, and that even without a wick, as the bitumen will burn without it, when once lighted. All the objection I can foresee that is likely to obtain against such an experiment, is that such a lamp as this would as likely burn in the open air as in an inclosed damp vault, whereas the lamps of the ancients nourished their flame best where there was most want of air, only in close vaults and tombs, and were presently extinguished on the least immission of external air, these being qualities necessary, and almost always asserted as concomitants of the ancient sepulchral lamps. To which I answer, that some of the lamps of the ancients burned as well in the open air as in close damp vaults, as several are mentioned by the ancients.

Another sort of perpetual lamp might be contrived in some coal mines, where the miners, when they have dug so deep that they begin to perceive a want of air, find a bluish flame begin to kindle in the fissures of the coal, which blazes, and moves up and down continually, and is a sort of fire that so little requires air for its continuance, that it burns best when there is least air, and is extinguished when disturbed by the motion of it, and which therefore must be the fire fittest for this sort of sepulchral lamps. But if it be thought that these lamps did not shine or burn the whole time they were inclosed in the tombs, but were only kindled by the admission of air when opened, I have thought of a way not at all liable to any of the defects or inconveniencies of the two former ways, whereby a glass of liquor inclosed in another, like the urns of Olybius, on immission of external air, shall certainly shine, though it did not so before. And it is this, take a small phial, into which put a little of the liquid phosphorus, which, if the phial be stopped, shines not at all, the external air being excluded from it; include this in another glass, as suppose the recipient of an air-pump, in which if the air be well exhausted, solid phosphorus itself will leave off shining in 10 hours time, though in the summer quarter; and the liquid in fewer, so that it shall shine no more than when the bottle containing it is stopped with a cork; and both of them will be extinguished proportionably in a yet less time, if the air be taken from them in the winter season. Now let such an exhausted recipient, with the included phosphorus, be placed in a tomb or vault, which are commonly dark, and if ever found, and the outer glass broken, as usually such things are by ignorant men employed in digging, possibly there will appear, upon admission of the air, as good a perpetual lamp as some that have been found in the sepulchres of the ancients, though in all probability of a different kind from all or most of them.*

That the Lacteals frequently convey Liquors that are not white. By Mr. Wm. Musgrave, Sec. R. S. N° 166, p. 812.

Exper. I.—I kept 2 dogs fasting, one 48 hours, the other 3 days, and then opened them; in both a considerable number of the lacteals appeared pellucid, like lymphatics, only not so full and turgid as those under the liver generally are, or as the lacteals themselves are sometimes seen. I cut several of them in each dissection, and immediately a transparent liquor flowed out of the orifice.

Exper. II.—A dog which had neither eaten nor drank for 3 days, was suffered to lap a quart of common water, an hour after which he was opened; and the lacteals appeared in great numbers all limpid, from the liquor contained in them, as in the former experiments; part of the water was supposed to be still in the stomach and small guts; for the quantity of water seen there was far greater than that in the primæ viæ of either of the dogs killed fasting.

Exper. III.—Another dog, after 3 days fasting, had a piece of fat meat given him; an hour and a half after which he lapped about a quart of common water, and half an hour after this was opened. I first tied the ductus thoracicus, then examined the lacteals, which I saw in as great a number, and as full as perhaps they were ever seen in this species of animals, 8 or 10 of them appeared perfectly white; very many of a faint diluted white; but most of them were pellucid, especially at the latter end of the dissection; by which time several which at first were either of a lively or of a fading white, were now grown transparent. That I might satisfy myself as to this difference in the colour of these vessels, I opened the intestinum jejunum and ileum in several places, and found the water was got as far as the cæcum, and had carried down divers little particles of the meat with it, by which means the liquor seen in the lacteals, at the first view of them, was either of a perfect or of a diluted white, or else pellucid, according to the mixture of the meat with the water in the guts.

* For many particulars relating to such lamps, with an historical account of several of them, and reflections on the possibility of them, see Montucla's Philosophical Recreations, translated by Dr. Hutton, vol. iv. p. 496, &c.

I ordered about 3 pints of broth to be given to a dog, which had been kept fasting 24 hours; and opening him 4 hours after this, I observed that the lacteals, beginning at the duodenum, were very much distended with the matter of the last meat: all the lacteals, that I saw at first, were of a perfect white colour; several of which I pressed between my fingers, drawing them from the circumference toward the centre of the mesentery, by which means I found, that the chyle, contained in these vessels, appeared white, when it ran in a shallow stream, as well as when it filled the lacteals: viewing the rest of these vessels along toward the cæcum, I observed that, near the middle of the intestinum ileum, they began to be of a more diluted white; and a little farther, they were really pellucid, and as turgid, to appearance, as those that were white; after which, turning back again toward the stomach, I saw the same vessels in as great numbers as at first, but the colour of most of them changed, being now all pellucid. The same thing happened in a dog kept fasting 2 days, and then opened 3 hours after he had lapped 3 pints of milk, part of which was seen in the stomach of the dog: about 15 of the lacteals, arising from the duodenum, were white; above 100, proceeding from the intestinum jejunum and ileum, were more or less transparent; as also were those of the duodenum at the latter end of the operation.

1. The 1st experiments sufficiently prove, that the lacteals convey not only chyle, but also another humour, separated from the blood, and now returning to it again; which may be seen purely by itself, without any mixture of chyle.

Now as far as I can judge at present, it seems not unreasonable to derive at least part of this liquor from the hollow of the intestines; if we consider that the pancreas, and glandularum plexus fragiformes, not to mention the liver, daily discharge a liquor into them. It seems probable, from the same experiments, that the lacteals are very seldom, or never, all empty at the same time; for though the chyle flows only in certain tides, or periods, pro ratione ingestorum; yet the refluent liquor, running in a more constant stream, does, when there is no chyle going in, keep the lacteals from being absolutely empty. And it is farther evident, from the same experiments, that this refluent liquor is, in its own nature, transparent; and passes such through the lacteals, after long fasting, when no chyle is mixed with it.

The experiments, both of the 2d and 3d kinds, seem to intimate, that a great part of the chyle itself is, in its passage through the lacteals, altogether limpid. Against which if it be objected, that some of the lacteals were in a like manner pellucid, in all the experiments of all the 3 kinds, and therefore it does not appear but that they may be filled with a refluent liquor in the last two cases, as well as they were in the first case. To this objection it may be replied, that in all the instances of the 2d and 3d cases, a considerable quantity of aliment was taken in by each dog, not long before his death; that some of this matter was seen in the primæ viæ of every one of them; an argument it was not all distributed; that there is no way certainly known, by which liquors are discharged the primæ viæ, in this species of animals, beside vomiting, siege, and by the lacteals; and since neither of the two former took place, it may not be unreasonable to suppose, that part of this matter was, at each dissection, in its way through the lacteals to the blood; all the operations being at such distances from the time of the matter being taken in, at which most liquid aliments are observed to swell up the lacteals.

But farther, whoever shall insist upon this argument, if he derives his liquor refluus from the musculous tunics of the intestines, is liable to the objection beforementioned against that opinion; if from the pancreas, glandularum plexus fragiformes, and liver, will find it no easy thing to account, why this liquor should be admitted the lacteals, and the water (taken in specie in the experiments of the 2d kind, as also in the first instance of the 3d kind, and included in the broth given in the second instance of the third kind) should be excluded; although in some of these instances it was seen to have almost reached the cæcum: especially since it is sufficiently proved, that decoctions of Indigo and stone blue have found their way into the lacteals.

Now if the liquor, seen in the pellucid lacteals of the last two kinds of experiments, did in a great measure, for I by no means exclude the refluent liquor, consist of the matter lately taken in before the dogs were opened; we may with good reason imagine, that water drank on an empty stomach, as it was in the second case, by several other quadrupeds, and men as well as dogs, will pass the lacteals, not under a white colour, but rather pellucid; and such cases are not uncommon; particularly, this seems to hold true in those who drink great quantities of diuretic mineral waters. And what is here said of water, is not unlikely to be true of several other liquors, as wine, beer, &c. at least so far as that they may not pass white through the lacteals. Again, if this principle be true, the third kind of these experiments will go yet farther, and argue, that the whole quantity of chyle, arising from some sorts of meat and drinks, taken either at, or near the same time; or from some sorts of meat taken alone, is not always white; for the lacteals, which appeared perfectly white in the several instances of the third kind, were far inferior in number to those that were pellucid in the same dissections: but the proportion of the white to the diluted and the pellucid chyle, depends upon so very many circumstances, as the quality of both meats and drinks, the distance between the time of their being taken, and the proportion of one to the other, that it seems almost impossible to determine it.

Hence there is some reason to induce us to think, that the lacteals frequently convey liquors, which are not white; and that chyle may not improperly be divided into 3 sorts at least, viz. white, pellucid, and intermediate to these; contrary to the opinion of those anatomists, who thought it to be always white, as that word is contradistinguished from pellucid; although by chyle they understood, as I likewise do, the effects of drink, as well as meat, concocted.

A Retractation of the 7th and last Paragraph of Mr. William Molyneux's Letter, in the Phil. Trans. N°. 158, *concerning Lough Neagh Stone, and its Non-application to the Magnet upon Calcination. Being an Abstract of a Letter from Mr. M. dated Dublin, Nov.* 25, 1684. N° 166.

The mistake acknowledged in this letter, has been duly rectified in the note added to the account of Mr. Molyneux's 1st. paper, p. 25, of this 3d vol. of our abridgement.

Directions for the Use of the following Tide-Table, sent by J. F. [J. Flamsteed] M. R. and R. S. S. to the Publisher. N° 166, p. 821.

This is the same kind of tide table, as Mr. Flamsteed has been in the habit of publishing annually, for several years past.

An Account of a sort of Paper made of Linum Asbestinum found in Wales, in a Letter to the Editor, from Edward Lloyd of Jesus Coll. Oxon. N° 166, p. 823.

The lapis amianthus or linum fossile asbestinum, abounds in the parish of Llan-Fair yng Hornwy, in the northern parts of Anglesey, where it runs in veins through a rock of stone, in hardness and colour not unlike flint. These veins are generally about $\frac{1}{4}$ of an inch deep; which is almost the extreme length of the amianthus. It is composed of a lanuginous matter exactly resembling that of pappous or downy plants; but so compact, that till you draw a pin, or some such sharp thing, across its grain, it appears only a shining stone; for not the least filament of lint is to be perceived in it. In its natural form some of it looks whitish, and some straw-coloured, but all shining: but when pounded in a mortar, the brightness disappears, and the whole becomes whitish. Above and beneath the veins there is a very thin septum of terrene matter, between the amianthus and the stone, to which it adheres. On putting a small quantity of the lint in the fire, it became red hot; but though it remained there $\frac{1}{4}$ of an hour, I could not perceive that it was at all consumed. Some of it being twisted in form of a wick, and dipped in oil, it gave as good a flame as other wicks, till the oil was consumed; the wick remained in the same proportion as at first. I pounded some quantity of it in a stone mortar, till it became a downy substance; and then sifted it through a fine searse, by which means I purged it pretty well of its terrene parts, the linum remaining. Having thus pounded and cleansed it, I brought it to the paper-mill; and putting it in water in a vessel, just capacious enough to make paper with such a quantity; I stirred it pretty much, and desired the workmen to proceed with it in their usual method of making paper, with their writing-paper mould; only to stir it about always before putting the mould in; considering it as a far more ponderous substance than what they used; and that consequently, if not immediately taken up after it was agitated, it would subside. The paper made of it proved but very coarse and brittle. But this being the first trial, I have some reasons to believe it may be much improved; nor did the workmen doubt, but in case it were pounded in one of their mortars for the space of 20 hours, it would make good writing-paper; which, when I shall receive a sufficient quantity of it, I design to try.

Specimen Universæ Rei Nummariæ Antiquæ; or, an Essay towards an Universal History of Ancient Coins and Medals, by Andreas Morellius. Paris, 8vo. 1683. N° 166, p. 825.

This ingenious Swiss has already delineated above 20,000 different and ancient coins; being a very considerable part of the sorts reserved in the cabinets of the princes of Europe. This specimen is divided into 14 tables, as into so many curiosities, and is prefaced with a short account of the more famous collectors of such antiquities. He distributes the whole into parts. 1. The coins of cities and kings before the times of the Cæsars, or at least without reference to them. 2. The consular coins. 3. Those of the Roman emperors. 4. The coins of the Hebrews, Phœnicians, Moors, Parthians, Arabs, Spaniards, Goths, and others, which either want inscriptions, or are not easy to be interpreted.

An Account of the reprinting of Johannes Goedartius de Insectis; cum Appendice ad Historiam Animalium Angliæ. By Martin Lister, M.D. and F. R. S. N° 166, p. 833.

It was desired that Goedartius, and the notes I had added, might be again printed in Latin; which for some reasons I suffered. I was willing to print with it a second edition of the appendix to the history of English animals, having some few things, with two plates of figures, to add, which are new, besides amendments. Also not knowing, but I might be prevented myself, by sickness or otherwise, I permitted to be printed off 4 plates more, which I had by me, very exquisitely designed, of the beetles of England; and having not the leisure to transcribe and perfect the tract to which they belonged, I neither numbered nor explained them, and therefore I call them mute tables: yet I am desirous thereby to encourage and recommend even bare and naked designs of the things of nature; that such persons as are wealthy and much at leisure, and are only willing to please themselves with elegancy of picture, may do good to posterity, in seeing that part of natural history well performed, which otherwise is scarcely in the power of private men; whose industry and studies are very often at a loss for want of that necessary help; which, if well done, might be referred to.

In the appendix, besides the additions, the whole genus of musculi fluviatiles is new described with more exactness. Also the pholas kind, that is, a certain sort of shell-fish, fast inclosed; and that naturally, in submarine rocks, is much enlarged, and the animals themselves are figured in some of them: by which a great objection is obviated, of those who assert equivocal generation; for these are inclosed in the rock, every one in a single hole of its own making, so that it was difficult to imagine how they could copulate; but by the figure we are eased of this doubt, for each animal has a long body, naturally exerted and extendable to a great length, as may be seen if they are put alive into water; so that we may well imagine they couple not unlike earth-worms, which come out of the ground for that purpose, and extend as much of their body as is necessary to meet a mate; which, if it happen to be near, their bodies are most within the ground, if farther distant, they are accordingly extended. And after this manner we must think of the solen kind; which are a sort of shell-fish deep bedded in sand, as the other is in the rock: these rise up at certain seasons, and by the like body extended, copulate.

END OF VOLUME FOURTEENTH OF THE ORIGINAL.

Some Experiments about Freezing; with the Difference between common fresh Water Ice, and that of Sea Water: also a probable Conjecture about the Origin of the Nitre of Egypt. By Dr. Lister, F.R.S. N° 167, p. 836. *Vol. XV.*

Dec. 3d, 1684, at night, I exposed 4 glass bottles in the open air on the ground to freeze; viz. one of the red natron water from Egypt; another of a strong solution of nitrum murarium in fair water; a third of sea water taken up at Scarborough, and more than half evaporated; and a fourth of the sulphur well at Knaresborough, that is, of natural brine evaporated to the same height with the sea water. The 4th, in the morning, the solution of nitrum murarium half of it ice; but not any of the rest. The 6th, in the morning, the bottle of nitrum murarium was most ice. The sulphur water had no ice that I could perceive at all in it: the natron had much ice at the bottom of the bottle; and the Scarborough sea water had some flakes of ice. The icicles of the natron were prettily figured, as is represented in fig. 5, pl. 3: the icicles of the sea water were also figured in oblong squares, as in fig. 6, and were brittle and transparent. I set the drained icicles of natron before the fire, when it readily enough dissolved into water again; this ice was both alike salt in ice and in water, much like in taste to the water out of which it was frozen. In like manner having drained the sea water ice, and exposed it before the fire; these icicles became soft and moist by degrees, but at length rather evaporated, than quite melted away; and having taken up a good thick lump of common ice, at least 100 times their thickness and bulk, this in a few moments at the same distance before the fire, grew always moisture, and dissolved into water; whereas the salt icicles, after 3 quarters of an hour lying before the fire, at length dried into a white powder, which was perfect salt, the moisture totally evaporating. Also the sea water icicles tasted very salt, when first taken out of the water. I repeated the same experiment of exposing to freeze the bottles of natural brine of Knaresborough sulphur, full half evaporated, and the same former Scarborough sea water, the 7th and 8th instant at night, and with the like success, viz. no icicles in the natural brine, but the same large ones as above described in the sea water, but not till after the 2d night's keen freezing. These salt icicles continued unthawed in the bottles, though they were brought into the house, and kept in a warm room, long after all other ice within doors was gone, viz. till the 12th instant at night, when the icicles also were dissolved. From which experiments we note, 1. That there may be salt ice from sea water frozen. 2. That there is a real difference between natural brine and sea water; as there is between the salts themselves, which they yield. 3. That the great floating mountains of ice in the northern seas, if on strict trial they shall be found to be salt, are not only the effects of many years freezing, but also much of their magnitude may be owing to the natural duration of that sort of ice. As to the nitre of Egypt, which the experiments made about it at Oxford plainly show to be little different from sal ammoniac;* considering that it rains little or nothing comparatively to the great heats in that country; and that the lakes there are only once a year furnished with fresh water from the overflowings of the Nile; also that vast tracts of land there, and all over Asia, are

* This conclusion is erroneous. See p. 51, of this vol. of these abridgements.

naturally covered with fossile salt; again, that those lakes are furnished with vast animals, as crocodiles, hippopotami, and doubtless great variety of other smaller animals; from all these things, well considered, it is easy to imagine, that in a year's time most of the salt water of those lakes has passed through the bodies of those animals, and consequently is become urinous or saline urinous, as is the nature and composition of factitious sal ammoniac.

Observations on the Practice of Physic. By Dr. Turberville, of Salisbury. N° 167, p. 839.

A gentlewoman was much troubled with the falling sickness; in her water I saw a great number of short worms, full of legs, and like millepedes; I gave her 2 or 3 purges, first with pil. agaric. and rhubarb; but still perceived in every water that was brought me, 8, 10, or more, of the worms. They appeared lively and full of motion; and the fits continued daily. At last I gave her ½ an ounce of oxymel helleboratum, in tansy water, which wrought well, and was successful; so that she had a complete cure.

About 6 or 7 years since, I had a gentleman (Mr. Oyliff) in cure for his eye, which was as large as my fist, black, fleshy, and full of bluish bladders; this I judged to be a cancer. After purging and bleeding, I cut out the ball and ulcered flesh, by many cuts, which were all insensible to him, till I came to the optic nerve; at the last cut he complained, and bled a little, the wound was healed in about a fortnight; he now wears a black patch over the place.

Not long after this, a young man had an eye as large as a hen's egg, very fair, without blemish, rheum, or redness, and his sight was pretty tolerable. I judged these symptoms to proceed from thin humours fallen on the eye, and distending its coats. I cured this distemper by applying drying medicines to the head and eyes, and making an issue in nucha. Appello morbum oculum bovinum, sive oculi hydropem.

Observations on the Cicindela Volans, or Flying Glow-worm. By Richard Waller, Esq. F.R.S. N° 167, p. 841.

The cicindela volans* is but rarely found in England, though I have happened twice to catch them at Northaw in Hertfordshire; they flew about the candle as soon as it grew dark; at both which times the weather was very hot, and perhaps it shines only at such seasons, though the animal be easy enough to be met with, all winged when it shines not, and without wings shining, which is

* The insect here described is not the common glow-worm, the female of which is wingless. It seems very nearly allied to the Italian glow-worm, *lampyris Italica* of Linnæus.

the common glow-worm. By Ulysses Aldrovandus, lib. 4, cap. 8, it is thus described: The winged glow-worm has its wings included in sheaths of a dusky colour; the head, on the upper part, is covered as if with a cowl, of the shape of an inverted flat shield, and of like colour as the sheaths; its little head, which is quite black, is parted into two tubercles or eyes, of the same colour; its tail consists of rings and is divided by several segments; in its extremity are two pellucid drops, like fire, but inclining from a shining fiery colour to a green or bluish one, like the flame of sulphur; and they are then best observed, when on compressing the tail that transparent humour tends to its extremity, &c.

It is much to the same purpose described by Moufet and Thomas Bartholin, in his treatise De Luce Animalium; save that they both I think mistake in allowing the male only to have wings, whereas they both fly alike, and there is no difference between them, except in size, the female being rather the larger. Its light was very vivid, so as to be seen plainly when a candle was in the room; but its vibrations were unequal, and the colour greenish, like that of the creeping glow-worm. The luminous part is two small specks on the under side of the tail, at its end. The shining continued for a little while after the tail was cut off, though it sensibly decayed, till at last it became quite extinct. Possibly the use of this light is to be a lantern to the insect in catching its prey, and to direct its course by in the night, which seems probable from its position on the under part of the tail, so that by bending it downward, it throws a light forward on its prey; while the luminous rays in the mean time do not at all incommode its sight, as they would have done had this torch been carried before it. This conjecture is also favoured by the placing of the eyes, which are on the under part of the head, not on the top. I observed also that it could and did, by some contrivance, cover its light, and make a kind of dark lantern.

Fig. 7, pl. 3, shows the insect on its feet, with the back upwards, where it appears to be of the beetle kind; it is of a dark brown colour, unpolished; when the case-wings are opened it extends two very large membranous wings, fastened to the upper part of the thorax; its head is covered as it were with a shield or broad-brimmed hat. Fig. 8, represents it laid on the back, to show the two eyes under its cowl or hat; they are black and very large, making up almost the whole head, there being little else to be seen; these are moveable, so that the animal can thrust them forward to the edge of its hat. From between these are discovered the two hairy feelers, or perhaps brushes to cleanse the eyes. Between these eyes and the thorax lies the mouth, on the thorax are 6 legs, almost all of a length. The tail consists of 7 shelly rings, on the last of which are visible the two shining points. Fig. 9, shows the insect on its back, as seen through a microscope when dead; where A A represents the two horns, feelers, or brushes, consisting of 10 roundish joints, besides the first, which is as long as two of the rest; they are all hairy, and like those of some butterflies. BB the cowl or hat over the head, which appeared of a speckled brown and yellowish colour, like tortoiseshell. CC the two eyes, composed of innumerable small glassy hemispheres in rows, as has been observed by Mr. Hook in his Micrography, to be the structure of insects' eyes, to supply their defect of motion, by the number of their pupillæ. DDDDDD the legs, of a shelly fabric, like lobsters, and jointed in the same manner, covered with many stiff hairs, though not so many as those of the blue-fly, represented by Mr. Hook in his Micrograph. schem. 26. The mechanism of the feet are much the same, only what is there called the pattens were here wanting, if not broken off, and their use supplied by the gibbous part, ddd. The talons, eeeeee, of the feet were shining, and very sharp-pointed. The legs consisted of two long joints, and the feet of four more, besides that which was armed with the talons. These seemed to be jointed into each other, and were all thick set with hairs or bristles. E the thorax, consisting of but one shell, of a polished copper colour, stuck full of tapering bristles, with a small dent in the shell where each grew. F the tail, consisting of 7 rings, of the same brownish colour, without hairs, except on their edges, which were set with them like a thin fringe, as the tails of lobsters, &c. are. These rings were of an unequal shining shell colour. ff the back, or upper part of 2 or 3 rings of the tail, turned up to show the work of the shell on that side. On the inside of the last of these was the light placed, though there was now nothing to be seen except that part being a little lighter coloured than the rest of the tail. GGG the membranous wings in every particular like those of the blue fly, with hairs on the veins or quilly parts. HH the insides of the case-wings, which were hairy, pointing all downwards. The outside of these cases was also very bristly.

On the Wurtemburg Siphon or Engine. By Mr. John Davis, Minister of Little Leak, in Nottinghamshire. Translated from the Latin. N° 167, p. 846.

This summer, 1684, a treatise fell into my hands on the Wurtemburg siphon, which is an inverted one, with legs of equal height, running backward and forward, in an uncommon manner. The author speaks in a wonderful manner of this machine, but apologises to the reader for his most serene patron, who wishes to reserve the structure of it to himself. On reading this, I considered how a siphon might be constructed, which should perform the same things as are ascribed to that of Wurtemburg. Having therefore in my hands a certain glass siphon, I erected it as nearly perpendicular as I could over two vessels;

and when fixed in that situation, I poured water into one of the vessels, till the orifice of the siphon was a little immersed in it, and presently, as I expected, the water ran out into the other vessel. Then that vessel being emptied into which I had first poured water, I poured it into the other, and immediately that water ran back into the first vessel. Though I would not venture to compare the artifice of this siphon of mine with that of Wurtemburg, yet I expect it may prove little less useful, especially if combined with another instrument of my contrivance.

Description of a Siphon performing the same Things as the Wurtemburg Siphon. By Dr. Papin, F.R.S. Translated from the Latin. N° 167, p. 847.

In the treatise on the Wurtemburg siphon, which was lately printed at Stutgard by M. Solomon Reisel, some wonderful effects are related of that new siphon, the characteristic properties of which are stated to this effect; 1. That the orifices of the two legs are placed on the same horizontal line within the brims of the vessels. 2. That though the orifices be but in part or the half filled with water, yet the water flows out drawn over a mountain. 3. Though the machine may have been at rest and dry for a long time, yet it will again produce its effect on the application of water. 4. That either of the orifices being opened, and the other remaining shut for hours, or a whole day, when this is afterwards opened, the water will flow out. 5. That though the orifices be both on a level, and the legs of an equal height, yet the water will flow out. 6. That the water rises and falls indifferently through either leg.

But as the author gives no explanation how the machine is made to produce these notable effects, the Royal Society desired me to construct a machine, which might exhibit the same phænomena as those above-mentioned. This I have effected after three several ways, the simplest and easiest of which seems to be the following.

AA, fig. 10, pl. 3, are two metallic vessels, into which the two extremities of the siphon are inserted. BCDEDCB is a siphon, having its orifices BB placed on the same horizontal line or level. F is a little tube soldered into the hole in the top of the siphon, and to be carefully stopped after the siphon is quite filled with water. Now it is plain that the water contained in the parts CD will prevent the admission of the external air to the upper part of the siphon, E. So that the siphon being always full of water, provided it do not exceed a certain height, it will certainly produce its effect, as soon as the water in the vessels AA shall fill some part of either orifice B. And both orifices being partly filled with water, when in each vessel A the surface of the water shall be in the same horizontal line or level, if ever so little water be poured into either vessel, part of it will be immediately conveyed through the siphon into the other vessel. And in like manner may be exhibited the other phænomena, as above-described in the book. The largest machine of this kind that has been hitherto made does not exceed 20 feet in height.*

A Miscellaneous Catalogue of common and cheap Experiments. By Sir William Petty, President of the Dublin Society. N° 167, p. 849.

1. The weight of a cubical foot of sea water, fresh river water, spring water, and rain water; as also of ale, wine, brandy, metheglin and Spanish wine, olive oil, rape oil, and train oil.—2. The weight of a cubical foot of wheat, barley, peas, oats, &c.—3. The quantity and weight of juice that 10 pounds of apples, pears, gooseberries, mulberries, peaches, and grapes, will yield, in order to make drink.—4. The weight of a cubical foot of oak, ash, elm, fir, willow, alder, birch, yew, pear-tree and box-timber, and the difference between the weight of the said timbers, extremely green and extremely dry.—5. The weight of the shell, white, and yolk of an egg, viz. of pigeons, hens, ducks, geese, and turkies, and the proportion between the weight of the egg and the hen, &c.—6. The weight of a cubical foot of wool, tow, feathers, hay, &c. pressed down with several weights.—7. The weight of several bottles and casks which are in common use.—8. The weight of a cubical foot of lime, sand, and mold.

With many other experiments proposed on a variety of practical subjects.

On a new Way of cleaving Rocks. By Mr. J. Beaumont. N° 167, p. 854.

About the year 1683, at the lead-mines in Mendip hills, the miners found out a new way of cleaving rocks with gunpowder, which is in the following manner: The first instrument they use is called the borer, expressed fig. 11, pl. 3, made of iron, and is 2 feet 2 inches in length; it is 1 inch square at the steeled end from a to b, and somewhat less in the other part; the use of this instrument is to make a hole in the rock deep enough to receive the powder. The second instrument, called the gun, fig. 12, is 6 inches in length, 1¼ diameter, and has a hole drilled through it to receive the priming powder. The first instrument is managed thus: one man holds it on the rock and turns it round, while another beats it down with a hammer of 5 or 6 lb. weight; when the hole is made somewhat deeper than the length of the gun, they dry it with

* M. Reisel afterwards acknowledged to the Royal Society, that the above-described siphon was exactly the same as that of Wurtemburg; as we shall find by a paper to be inserted from N° 178 of the Philosophical Transactions.

a rag, and put into it about 2 or 3 oz. of powder, over which they put a thin paper, and on it place the gun, which they bind firmly into the hole, by driving in against the flat side of the upper part of it, the third instrument, which is a little iron wedge, 4 inches in length, by the miners called a quionet, fig. 13; when this is done they pass down a wire through the hole drilled in the gun, and pierce the paper which covers the powder; then they prime the gun, and lay a train, retiring up out of the work before the powder takes fire. The paper is put at first over the powder, lest when the gun and quionet are driven down, the tools should strike fire and kindle the powder.

These instruments will be of great use to miners; for, besides what will be saved in timber in a year that is used in burning rocks, which is very considerable, we know that as soon as a man has fired his powder and split the rock, he may presently go to work again; whereas after a fire is laid in a shaft, a man can scarcely go to work in 24 hours after, the rocks being too hot to suffer him.

On the Advantages of High Wheels. By a Member of the Philosophical Society of Oxford. N° 167, p. 856.

Having read in the mechanics of Mersenne, Herigon, and Dr. Wallis, that the larger the wheels of a coach, &c. are, cæteris paribus, the more easily they may be drawn over a stone or such like obstacle; I was willing to try some experiments which I thought might convince some men better of the truth of it than a mathematical demonstration.

I therefore ordered a model of a part of a waggon to be made, consisting of 4 wheels, 2 axles, and a board nailed upon these. The smaller wheels were $4\frac{1}{2}$ inches high, and the larger wheels $5\frac{2}{3}$, viz. $\frac{1}{12}$ of the ordinary height of the wheels of a waggon, the weight of the model was almost a pound and half. I had also 2 other wheels made $5\frac{2}{3}$ inches high, to be put on instead of the smaller. The middles of the 2 axles were $6\frac{1}{4}$ inches asunder. All the wheels turned very easily on the axles.

A piece of lead $50\frac{3}{4}$ lb. avoirdupois was laid upon the model, so forward that the smaller wheels seemed to bear above $\frac{2}{3}$ parts of the weight. Then the model was drawn with a string laid over a pulley, the top of which was $\frac{1}{4}$ of an inch higher than the top of the hinder axles, and the middle of this pulley was $7\frac{1}{2}$ inches from the middle of the fore axle. The lesser wheels being put on, and the string fixed, tied to the top of their axle: then

1. 3 lb. drew the model on the smooth level table. 2. 20 lb. drew the lesser wheels over a squared rod $\frac{1}{4}$ of an inch thick. 3. 30 lb. drew them over a round

rod a little more than ½ an inch thick. 4. 31 lb. drew them over a square rod ½ an inch thick. 5. 12 lb. drew the hinder wheels over the larger square rod. The string being laid under the axle, viz. ⅝ of an inch lower than before; then, 6. 29 lb. drew the lesser wheels over the larger square rod. Then the 2 larger wheels being put on instead of the lesser, and the string lying over the axle, 7. 3 lb. drew the model on the table. 8. 25 lb. drew the fore wheels over the round rod. 9. 25 lb. drew them over the larger square rod. 10. The string lying under the axle, 16 lb. drew them over the least rod. 11. 23 lb. drew them over the round rod. 12. 23 lb. drew them over the larger square rod. 13. 13 lb. drew the hinder wheels over the larger square rod.

All these experiments, were tried twice at least, and most of them 3 or 4 times over. In all of them the lead was laid exactly on the same part of the board; yet, when the lesser wheels were taken off, the lead did not lean so much forward, so that the hinder wheels were somewhat more pressed than before.

By comparing the 2d, 3d, and 4th experiments, with the 10th, 11th, and 12th, it appears how much more easily a waggon, &c. might be drawn on rough roads, if the fore wheels were as high as the hinder, and if the thills were fixed under the axle. Such a waggon as this would also be drawn more easily were the wheels cut in clay, or sand, or any soft ground; besides, high wheels would not cut so deep as low ones. Some other advantages are mentioned by Dr. Wallis.

It is true, low wheels are better for turning in a narrow compass than high ones; but it seems probable that waggons with 4 high wheels might be so contrived, that there should be no great inconvenience in that respect; at least such waggons as seldom have occasion to turn short, as carriers' waggons and the like.

The difference observed in the 8th and 11th experiments, is agreeable to what is said by S. Stevinus and Dr. Wallis, viz. that if a coach, &c. must be drawn over rough uneven places, it is best to fix the traces to the coach lower than the height of the horses shoulders.

14. A table, 2½ feet long, was set with one end 8½ inches higher than the other end, and the model being loaded as before, less weight by 6 ozs. drew it up the table when the 4 larger wheels were on, than when 2 larger and 2 less were on. The reason of which, as given by Dr. Wallis and other writers on mechanics, is, because in the first case there was almost the same direction of the motion of the model and of the string that drew it; but not in the 2d case when the fore axle was so much lower than the top of the pulley.

An Extract from the Journal of the Philosophical Society of Oxford; giving an Account of a large preternatural glandulous Substance, found between the Heart and Pericardium of an Ox. N° 167, p. 860.

That lump of flesh taken out of an ox, and seen by this society, afforded the following observations: The weight of the whole substance, cleared from the little fat, &c. adjoining to it, amounted to 19¾ lb.

As to its figure, it so far resembled a heart, that it was a long time taken for nothing else; but it was something flatter than the heart is naturally, each of the flat sides making an equilateral triangle. The basis of this cone of flesh was 2 feet 7 inches in circumference; a thread drawn round it lengthwise, from the basis of the vertex, came to 2 feet 9 inches.

We then divided it, cutting from the vertex to the basis of the cone, and passing through both the ventricles, and mucro of the heart, by which means we saw the heart not to exceed the natural size; that which was extraordinary about it being a large glandulous substance encompassing the heart, unless where the vessels had their passage, and stretching the pericardium to the excess before mentioned; we saw no liquor in the pericardium, nor indeed was there room for any; this glandulous substance taking up all the space between the heart and pericardium, to both which it grew very fast.

This preternatural substance was thickest about the basis of the heart, where it covered the auriculæ, and was 3⅓ inches thick, it grew thinner on both sides gradually toward the mucro, where it was 1½ inch thick. In the septum cordis a gritty sabulous substance was found, half as large again as a walnut. In the lungs were several cystides, containing matter more or less fluid: one very large cystis held some ounces of a matter not unlike that of a steatoma.

The butcher who killed this ox, says, the lungs grew fast to the pleura, on both sides, which he affirms not to have found once in 40 times in the cattle killed by him. He says also, that the ox, though not overburthened with fat, complained much in travelling, which is easy to account for, there being not room for the heart to be distended, as it ought, in its diastole.

Nov. 4, 1684.

De Origine Fontium, Tentamen Philosophicum, in Prælectione habita coram Soc. Philos. nuper Oxon. instit. ad Scientiam Naturalem promovendam. Per Rob. Plot, LL. D. Custodiæ Musæi Ashmoleani Oxoniæ præpositum, et Reg. Soc. Sec. Oxon. 8vo. 1685. N° 167, p. 862.

The author of this treatise, de Origine Fontium, disliking the old way of handling this subject, as too general and remote, has chosen rather to argue for both parts of the question, from the history of springs; with intent more particularly to satisfy his reader, which springs they are, that wholly come from rains, mists, dews, &c. also which from the seas, and which from both. In order hereunto, he gives a scheme of the several species of springs, to which he thinks all sorts may be reduced: and then determines that such intermitting springs as are profluent after rains, and then gradually slacken, and at last are wholly dried up on summer heats, do certainly owe their birth to rains. And not only such intermitting ones, but some perennial springs also, such as we often find on the tops of mountains, which we may rather term weeping, than flowing or running springs; which seem to have their origin from the mists so frequently seen hanging on the tops of hills. Yet he cannot agree with several authors, that all springs owe their origin to rains, dews, &c. For he thinks not that temporary irregular fountains can possibly come from rains; much less the temporary regular ones, such as the fountains of the Lers in France, of Lamb's-bourn in England, of the Zirchnitzer sea in Carniola, and divers others. Much less still thinks he that such vast perennial springs, as those of Willowbridge in Staffordshire, of the Sorgue in France, can come from rains; since he finds on computation, that all the waters that fall near them in the space of a year in rains, dews, &c. will not comparably amount to what issues from them. For the better calculation of which, he shows to what height rain-water will rise in a year, in a conservatory fitted for that purpose, communibus annis; and how many hogsheads, &c. will flow out of a cubic inch bore, in 24 hours, or a year, &c. And then shows, that though it may be true, what an anonymous French author asserts, that more water falls in rains near the fountain head of the river Seine, than is requisite for the yearly expence of that river; yet it is not so at Willowbridge, or likely to be so with the fountain head of the river Sorgue, which, as Gassendus says, is navigable to the springs which are its original. Yet much less still can he imagine that all the rains, mists, dews, snows, &c. that fall on the surface of the whole earth for the space of a year, can supply the vast expence of all the rivers in the world for the same time. And as he judges that all fresh-water springs cannot come from rains, neither can he believe that either hot springs or salt springs are maintained thence. He then proceeds to prove, that there are subterranean communications between the seas and fountains, by which they are supplied; and that there are charybdes which swallow the sea, which happening sometimes to be stopped, the greatest rivers have ceased to run. This subterranean circulation of waters he further evinces from divers springs, that ebb and flow with the sea; and from divers lakes that have salt water and sea fish in them, yet have no superterranean communication with any sea. He also says it is further evident, that there are such passages from divers marine heterogeneous substances, that have been found in digging deep underground, such as shell-fish, &c. Where by the way he discourses of divers such indraughts there are in the sea, more particularly of the fluxus moschonicus, or Maalstroome, on the coast of Norway, and believes there must be some such vast charybdis, beside that on the coast of Sicily, in the mediterranean, which must swallow all the water which perpetually flows into it, otherwise it must needs overflow the low land of Egypt.*

Medicina Septentrionalis Collatitia, s. Rei medicæ nuperis annis à Medicis Anglis, Germanis et Danis emissæ Sylloge et Syntaxis. Opera Theophili Boneti.† *D.M. cum Indicibus et Figuris necessariis. Gen. fol.* 1685. N° 167, p. 866.

This author designing a body of the law of nature, in imitation of that of the civil law, to his two former volumes subjoins this, the intent of which is to show how much physic is indebted for its improvement to these northern nations. The instruments of which have been the Royal Society of England, afterward copied by our neighbouring nations; Paracelsus opened the way, and was followed by Helmont, Harvey, Lower, Bartholin, Malpighi, Wharton, Willis, Betts, Schneider, Steno, Sylvius and others. The book is divided according to the usual partition of the body of man, into three parts. The first contains the diseases incident to the head, under which he treats of the plica polonica, and brings instances of monstrous births, and afterwards considers the affections of the eyes.

The 2d book treats of the diseases of the mouth and breast, where among other things is a discourse concerning an infallible way of preserving a man from infection, though he converse constantly with all sorts of persons infected; the way is no more than forbearing to swallow the saliva.‡ He here treats largely of

* Dr. Halley has shown that the exhalations from the Mediterranean, are fully sufficient to carry off all the waters that run into it by the rivers.

† This learned and industrious physician (Bonet or Bonnet) was a native of Geneva, where he was born in 1620. For some years he was engaged in considerable practice; but was at length obliged to give it up in consequence of a loss of hearing. After this misfortune, he dedicated his leisure to the compilation of several valuable and extensive works, (besides that above reviewed) the chief of which are as follow: Labyrinthus Med. extricat. sive methodus vitand. errorum in praxi, 4to.; Sepulchretum sive Anatome practica, fol.; Mercurius Compilatitius sive Index Medico-practicus, folio. Of these, the Sepulchretum is by far the most valuable work. It was first published in 1679, and afterwards re-edited with considerable additions, by Manget, in 2 vols. fol. 1700. It has been only surpassed by a similar work of Morgagni's, (De Sedibus et Causis Morbor.) published more than half a century afterwards.

‡ That this alone does not always afford a sufficient security against infection, has been mentioned in a note at p. 493, vol. ii. of this Abridgment.

the nature and cure of a peripneumonia, and of its difference from a pleurisy; of all diseases of the lungs, as gangrene, schirrus, vomica and stones frequently found in them, a case of which latter is subjoined of one who expectorating many of different shapes and sizes, was eased from a dyspnœa, under which he almost mortally laboured. Among pleurisies, there is an account of a periodical one, returning once in 7 weeks. Among the celebrated remedies for it, are antimonium diaphoreticum; as also a bark in India very powerful, if not a specific in this case: it is called pawo de portada, of a yellowish colour, bitter taste, and a friable consistence; half an ounce in a glass of wine is the dose. In his histories of consumptions he relates an example of the great contagion of it, which was the infection of a senator's second wife, only by wearing the muff of his first wife, who died of one, though after the distance of above 12 months. He gives a particular account of the famous Cnæffelius's remedies in this, as he has done in other cases: instances how opium has been very unsuccessfully applied in pleurisies, death ensuing the stoppage of the spitting.

In his 3d book is contained what concerns the abdomen and its parts, of which the œsophagus being considered as the beginning, the preternatural cases of that are first handled; such as its being turned to a cartilage; being obstructed by different causes, and the like; and after the account of several ways deglutition has been hindered, by the histories of men swallowing strange substances, such as money, frogs, knives, &c. In short, there is comprized in this volume, whatever this age and these climates have produced curious in medicine taken from ephemerides and other tracts, and digested under proper heads.

Johannis Nicolai Pechlini Med. D. P. serenissimi Cimbriæ Principis Reg. Archiatri, Theophilus Bibaculus, sive de Potu Theæ Dialogus. Franc. 4to. 1684. N° 167, p. 870.

This tract, written by way of dialogue, gives an account of thee, or tsia, [tea] a shrub growing in most parts of China and Japan.

After an imperfect description of this shrub, the author proceeds to the consideration of its virtues, which is the main object of his treatise. He considers it to be of a bitter, astringent quality; its infusion gives a green colour; but on the mixture of the solution of vitriol, it turns black; whereas chamædrys, though bitter, is not astringent; and on such mixture grows green, rather than black. Veronica comes nearest to it, for it gives a good tolerable tincture; and though the taste be not bitter, yet it is very astringent; and it turns black also, like thee, when mixed with the solution of vitriol; neither do its effects come far short, since it cleanses the kidneys, and strengthens the head and stomach.—This plant he adjudges very suitable to our northern constitutions; and that it is a very agreeable bitter astringent, serving to prevent, if not to cure, most chronical distempers.

Notwithstanding all his encomiums of this exotic, the author is inclined to think we might receive as much benefit from some plants of our own growth; were people industrious to search after them; some of which are veronica, lingua cervina, marrubium, hepatica, cichoreum, &c.

On the Connought-Worm. By William Molyneux, Esq. Sec. of the Phil. Soc. of Dublin.* N° 168, p. 876.

The Connought-worm is said to be the only poisonous animal in Ireland: but whether it be really so, or not, I cannot assert on my own experience. Some of them are as thick as a man's thumb, and above 3 inches long, and some of them live so long as to have fine hair, thinly dispersed over their bodies. The most experienced people of the country agree, that the animal is poisonous, though no satisfactory experiment has proved it. The reasons for thinking the animal pernicious, if eaten by a beast, are these: first, the disease imputed to this animal seldom or never affects the cattle but in autumn, and then only this insect is to be found; secondly, it seldom or never affects any cattle but what feed in low marshy grounds, and there only this animal frequents; thirdly, cows which are greedy feeders, by great morsels, by reason of their chewing it afterwards in their cud, but especially swine that feed in low grounds, are the only creatures troubled by this worm; fourthly, the worm is very rare, and scarcely to be found in seven years, and so likewise is the distemper that proceeds from it, it being rare to have a beast affected by it. As to the symptoms that attend its venom, they are swelling in the head, and, as a peculiar characteristic, the swelling and procidentia ani, insomuch that the rectum will hang out above half a foot. The cure of this malady in black cattle, is a drench of the herb bear's-foot, rue, garlic, and butter and beer; but for swine, raddle pounded small and mixed with butter-milk. These are only used by the English husbandmen. But the Irish, as they certainly impute the malady to this insect, so they draw the remedy from the same; for they assert, that if a hole

* This insect is the larva or caterpillar of the *Spinx Elpenor.* Lin. or *Elephant Hawk-Moth.* In August it retires beneath the surface of the ground, and changes to a chrysalis, out of which in the succeeding May proceeds the complete insect. There can be little doubt, but that the poisonous qualities ascribed to the caterpillar in this paper, are entirely imaginary: many caterpillars, however, are of a slightly acrimonious nature; and some few are covered with a kind of brittle hair, which, when rubbed on the skin, produces an effect nearly similar to that of the substance called cowage or cowitch, viz. the down from the pods of the *Dolichos pruriens.* Lin.

be bored in a tree, and this worm be closed up therein, so as to starve and die, the leaves and bark of that tree ever after infused in water, and given as a drench, cures the disease. Another fancy, and as ill grounded, they have, that if a man bruise this worm between his hands, and let the expressed juice dry on them, ever after the water he first washes in, in the morning, given to the beast to drink, cures it.

But for myself, I am very apt to suspect that this worm is no more poison than other caterpillars, of which species it is; and Godartius, in his book of insects, calls it the elephant-caterpillar. But I verily believe that the ugliness of the worm, it being of a dark fuscous, and as they say, poisonous colour, together with its largeness beyond common caterpillars, has so wrought upon the fearful and ignorant vulgar, that they have given it the name of venomous.

Fig. 1, pl. 4, AB, is the worm lying on his belly, its length $2\frac{1}{2}$ inches almost; c its head; dd two variegated spots, mistaken for eyes; e a small protuberance towards its tail, from whence arises a part in shape of a horn, mistaken for a sting.

Fig. 2, represents the worm reclined almost on his back; f its mouth, formed like that of other caterpillars, as appeared in the microscope; gg, &c. six small horny feet or claws, 3 on each side, as in other caterpillars; hh, &c. eight papillæ, with which it fastens itself to what it goes or hangs on, as children's suckers are fastened to wet stones; ii two larger papillæ, with which it both sucks itself fast, and commonly grasps the stems of grass and herbs, to which it clings with the others.

Of a prodigious Os Frontis in the Medicine School, at Leyden. By Mr. Tho. Molyneux. N° 168, p. 880.

This os frontis is complete every way, and differs in no respect from that of a man's, but in its size; and since there is no creature, especially of the larger sort, that has this bone at all resembling the human, there is not the least question to be made, but that it formerly belonged to a man, and that of a most extraordinary large size. Its dimensions were as follow; from its juncture with the nasal bones, to the place where the sutura sagittalis terminated the convex way, $9\frac{1}{10}$ inches, transversely from side to side, still measuring the convex way it was $12\frac{2}{10}$ inches; in thickness about half an inch. I have measured this same bone in several ordinary skulls, according to all these dimensions, and find that one with another, they scarcely answer it in half proportion; for where it was $9\frac{1}{10}$ inches, they are but $4\frac{1}{2}$ inches; and where it was $12\frac{2}{10}$ inches, they are not above 6; and in thickness not above $\frac{1}{4}$ of an inch. So that, supposing this bone had the same proportion to his stature, which in other men it has to theirs, it must follow that he was more than twice as tall as men usually are, that is, 11 or 12 feet high. It is true, that some heads are very large in proportion to their body, yet generally such skulls want in thickness, as this does not, and are ill-shaped, and not proportionable.

A Stone grown to an Iron Bodkin, in the Bladder of a Boy; communicated by Dr. Lister, F.R.S. N° 168, p. 882.

Fig. 3, pl. 4, represents a stone, which was cut out of the bladder of a boy at Paris, by M. Colo: the iron bodkin, to which the stone grew, and which passes through the middle of it, had been thrust up into the bladder by the boy himself, about two years before the incision. The stone was presented by the above named surgeon to his late majesty of England, among whose rarities it is now preserved, and by whose permission I caused this draught to be made of it.

Abstract of a Letter from Mr. Anthony Leewenhoeck, on the Parts of the Brain of several Animals; also the Chalk Stones of the Gout; the Leprosy; and the Scales of Eels. N° 168, p. 883.

The cortical parts of the brain of a turkey cock, consist (a great number of blood vessels and small globules excepted) of a very clear crystalline oily matter, which for its transparency ought rather to be named vitreous than cortical. When I separated a little of this matter from the rest, there flowed from the place a little thin moisture, containing in it some extremely small globules, less than $\frac{1}{36}$ part of one of those which make the redness in the blood. This fluid matter, however, was chiefly in the brain of such turkeys, as had been dead for some time. Besides the abovementioned small globules, there were also some about the size of $\frac{1}{6}$ of a globule of our blood. Together with the abovementioned globules, there were some transparent irregular ones, as large or larger than a globule of our blood, which lay among the branches of the blood vessels, in a space no larger than a coarse sand. Though these blood vessels were so small, they had yet such a degree of colour, that I could discern the matter in them to be that which makes the blood red. And I was further confirmed in my judgment, by observing that other blood vessels, which were somewhat thicker, appeared proportionably higher coloured, and more inclining to red; and that the redness appeared more plain, when 3 or 4 vessels lay immediately one over the other, without any other matter intervening.

The cause of the brownish colour of the cortical parts, I take to be the great number of veins and arteries which run through the transparent substance, whereof these parts chiefly consist. To which may be added that there were several particles mixed, about as large as $\frac{1}{6}$ of a blood globule, which were not transparent. We may perceive that the cortical parts in many places enter deep into the medullary, but if we diligently search into them, we shall find them as full of blood-vessels as the outside. Besides these very thin blood-vessels, there are other vessels in the brain, so thin, that I conceive a round body, although divided into above 1000 parts, could not pass through them. And it is to be noted, that in these observations are mentioned only those blood-vessels, which are as thick or thicker than a hair of the head.

In the medullary parts of the brain were contained irregular globules of different sizes, some of them as large as a globule of our blood, and some larger, seeming to consist of a thin transparent oil-like substance; these are here in such numbers, chiefly where the medulla spinalis begins, that they seem to compose the greatest part of the brain. This great number of transparent globules causes a white colour, and they were yet so fast joined to each other, that some would be drawn out to twice their natural length, and then seemed to me as if they were kept together by net-like threads. This made me think, that the smallest branches of the blood-vessels might encompass these globules as the small horizontal vessels, in some kinds of woods give way to the great perpendicular vessels, and wind half about them. And I was confirmed herein, when I perceived many of the aforesaid vessels appeared clear in the middle, and brownish on both sides; but more brown when I had torn the globules asunder, and laid the thin thread-like parts by themselves. In fine, the medullary parts of the brain appeared often like a fisher's net, between each of whose meshes was placed a very pliable ball-like substance, which changed its figure into round or oval, according as the said meshes were stretched one way or other. Moreover the said medullary parts consisted of a very great multitude of very small globules, and some clear thin matter.

I observed also the brains of a sheep, and perceived in the cortical parts, a great multitude of extremely thin blood-vessels, containing some of the substance which makes the redness of the blood. The cortical part of the brain had also white streaks or lines, thinner than a hair of the head, which to the naked eye were invisible. I have also perceived brown streaks running through the medullary part of the brain, which were only caused, as I conceive, by there being but few or none of those transparent globules there placed.

Afterwards I carefully examined the brain of an ox, and satisfied myself that the vitreous and very transparent matter, which makes up most of the cortical part, consisted of nothing but extremely thin streaks or vessels, which were nearly joined together; but at another time I could not fully satisfy myself concerning the same. As to the remaining substance of the cortical parts, I could discern no difference from the brain of other animals; but that there was not such a quantity of fluid matter as in animals which had been longer dead. I found also the medullary substance of the brain to be such as I have before mentioned in a sheep; for it had very white streaks in the parts from whence the medullary spinalis takes its rise, this extreme whiteness was from several very transparent vessels, which lay close to each other, and seemed to me only made to carry the matter by which the medulla spinalis and nerves are partly nourished and maintained.

I have also examined the vitreous or transparent parts of the brain of several sparrows immediately after being killed, and have therein not only clearly discerned a great number of small blood vessels, as in a turkey, but as plainly as in an ox or sheep. Also the other parts of the brain of a sparrow were very near of the same size with those of an ox, there being no other difference, the great blood-vessels excepted, but that these consisted of a greater number of the same parts. There were also in the cortical parts an incredible number of extremely small vessels, lying so close to each other, that by reason of their transparency they looked like glass.

I have lately examined the crystalline humour of the eye of a man, that I might know whether the small threads of the scales wind about in the same manner as I have formerly described them in the eyes of beasts; but I could not discern the true constitution thereof, notwithstanding I have twice endeavoured it. This I observed, that the crystalline humour was not fine and clear, but yellowish, whereby it differed from the eyes which I had formerly seen of other animals.

A relation of mine, much troubled with the gout, has his heel spoiled by the great quantity of chalk that breaks out of it; this matter I examined in my method, and separated the same into three parts; the first was the driest and whitest, composed of small irregular parts, as if some small sands lay together, these through the microscope appeared very dark bodies, and each of them consisted of a great number of long transparent figures, which I can liken to nothing better than to cut horse-hair, something sharp at both ends. These figures I judged so thin, that more than 1000 of them lying close together would not equal the thickness of a head-hair. The 2d sort of this chalky matter not so white as the former, contained the forementioned irregular parts of long figures, in a very tough clear matter, mixed with blood-globules, and many of the small roundish parts. The 3d sort was, to the naked eye, somewhat reddish, caused by the many blood-globules, mixed throughout the slimy matter in the chalk; it was also constituted of the beforementioned roundish parts.

Of the Scales of Eels.—Among the fishes which are generated in our rivers and waters I know but of two sorts that are said to have no scales, the one sort is the eel and pælink, in some places not distinguished, but with us they are distinguished, the pælink being flatter and pleasanter in taste, and therefore sold dearer; the 2d sort is the eelpout; this last sort are short and thick, and scarcely to be met with; which two sorts of fish the Jews will not eat, as forbidden by the law of Moses.

I examined the skins of these fishes, after I had cleared them of their slime, and found them covered with scales as much as other river fish, for the scales, although very thin and small, lay orderly, and close fastened one over the other. Besides, these fishes are also provided with fins, like other fish; for they have on each side of the head a perfect fin, the whole hinder part or tail is, both above and under, furnished with a continued fin.

The Chemical Touch-stone of M. John Kunkel, Gentleman of the Bed chamber to the Elector of Brandenburg, De Acido, et Urinoso Sale Calido et Frigido, contra Doctor. Voightii Spirit. Vini vindicatum. Submitted to the Royal Society of London, &c. as to the High Judges of this Matter. Berlin, the 6th of July, 1684.* N° 168, p. 896.

This book sent to the Royal Society by favour of the Elector of Brandenburg, is written in high Dutch.

The author begins with an epistle dedicatory to the Royal Society, appealing to them as to impartial judges.

He then addresses to the reader an apology against some calumnies thrown upon him. After this, follows a short epistle of M. Kunkel's antagonist, Dr. Voight, to Dr. Martin Weisen; addressed to him as judge of the controversy.

M. Kunkel begins to lay down his opponent's propositions, which are three. 1. That there is no acid which of itself gives either heat or flame; for he says, that there is no acid of either the vegetable, mineral, or animal kingdom, which affords it. He instances in the acidum ventriculi et succi pancreatici; whereas on the contrary, the gall, which contains no acid but affords oil, gives also flame and heat. 2. He endeavours to prove spirit of wine to be an oil. 3. He obviates some objections.

M. Kunkel in answer says, that he never declared it as his opinion, that any body should produce heat, without the accession of a frigidum, which in

* The principal facts and arguments stated in this curious controversy it has been thought fit to preserve, as tending to show the progress which chemistry had made at the period above-mentioned, and what were the chemical theories which then prevailed.

other places he calls urinosum, nor that spirit of wine was a pure acid, but a spiritus duplicatus. He agrees that no pure acidum is inflammable, but that urinosum and terra must be added. He denies that there is a pure acidum in the world.

He examines 18 oz. of the gall of an ox, from which he obtains 16 oz. of clear water by distillation, of a stinking oil, and ½ oz. of a salt earth, in which he says there is a sal acidum: so that he would ascribe inflammability to belong rather to the acidum than oleosum.

M. Kunkel proceeds to give an account of Dr. Voight's opposite sentence; who instances in his common distillation of wine, viz. that after the spirit of wine per balneum mariæ is separated from the wine, that which remains in the vessel becomes sour, and will not burn, though perhaps somewhat of a subjugated oil may be mixed with it: but if there be separated any oil from the acid, it then burns per se.

M. Kunkel endeavours to prove sp. vini to be no oil; because oil will not intoxicate a man; and because acids, though of different families, do all unite and mix, and so also do oils.

M. Kunkel's opponent (Dr. Voight) proceeds to the regnum minerale, and absolutely denies a heat, flame, or light, to be properly inherent in any acid, but that some central oils, not easily separated from mineral acids, produce those incalescent effects. Rain water, he says, affords oil and volatile salt, but no inflammable acid. He says all acids, though ever so pure, if evaporated or distilled off, will always leave in the bottom of the retort, or other vessel, a materia pinguis et unctuosa, out of which an inflammable oil may be brought. He concludes saying, that it suffices to prove, that acidity does in nowise dispose a body to inflammability, because olea expressa et distillata burn and flame more readily, when they are purged of all mixture of acidity: as oleum lini, terebinthinæ, anisi, fœniculi, when distilled per cineres, and other alcalia that detain the acida.

M. Kunkel, in his answer, denies that in these mineral acids, particularly oil of vitriol, any oil is contained. Since Dr. Voight declares all heat to depend on bodies, on the score of their being oily, he (Mr. K.) demands that he will shew him an experiment, how to produce heat with the mixture of any oil and water; as M. Kunkel has often done with acids and water.

The adversary, Dr. Voight, proceeds to lay down several positions. 1. That oil of vitriol is the most fixed acid, and gives more heat than spirit of wine while flaming. 2. That fixed bodies are only respectively so. 3. That acids, by repeated distillations, may be brought to such a degree of subtlety (I suppose he means volatility,) as spirit of wine. 4. Any surmise about an acidum occultum signifies nothing, for even that may be easily parted. 5. All acida are volatile, because they yield a strong scent, which comes from their volatile salt. Now since we can find no inflammable acid in the world, we may properly infer spirit of wine to be no acid, because it takes fire and burns. He concludes with a definition of acidum. Quod acidum sit sal fluore potitum, vim habens astringendi et coagulandi.

M. Kunkel, in his answer, grants the first position. To the second he gives a check, blaming his not distinguishing between the acida regni mineralis et vegetabilis. The next position he denies absolutely. To the 4th position he answers, that any acidum may be concentrated in a body, and is there stronger, or has its parts extended and diffused, and so weaker, so that magis et minus vary the case much. To the last, which says all acida are volatilia, because of an odour or scent that rises from them: he denies that all do give an odour, and instances oleum vitrioli, which gives none; and by consequence no acida pura afford it, but such as are mixed with an urinous salt.

Dr. Voight goes on to prove, from his definition of salt, that spirit of wine is no acidum, viz. because it has no vim attenuandi et resolvendi; he means properly and in its own nature, which consists in its inflammability, so that spiritus vini, when it acts the part of a menstruum to dissolve bodies, does so because of some salia volatilia that are mixed with it: for the more you impregnate it with salt, the stronger menstruum is obtained. 2. He says sp. vini is so far from agreeing with his definition of acids, in having vim coagulandi, that it rather hinders it, e. g. acids coagulate milk: spirit of wine keeps it in a body together, and hinders that coagulation. 3. Spirit of wine corrects acida, by dulcifying spirit of salt and nitre, when mixed with them; which are great corroders of themselves. 4. Spirit of wine keeps bodies from degenerating into acids; thus, it preserves beer from souring. Also it prevents the fermentation of vinum mustum, and the working of beer. 5. Acida give a red tincture to syrup of violets; spirit of wine does not change it.

Thus far Dr. Voight. To the first proposition M. Kunkel replies, that since sp. vini, according to his concession, abounds with salt, and is therefore a menstruum, and can be sooner reduced to salt than oil, that for his own reasons it may better derive its properties from salt, than from his imaginary oil. He gives an experiment, viz. That sp. vini dissolves the crystals of silver; which it could not do, he says, unless it consisted of an acidum et urinosum; for no oleosa whatever can pretend to this effect. To his 2d proposition, that sp. vini hinders coagulation, and to his instance in common milk, he says the contrary; viz. That spirit of wine and milk in equal proportions will coagulate, as vinegar and milk do. To the 3d, which is only an instance that spirit of wine dulcifies acids, he denies it to be dulcis, but still very corrosive, and that it is diluted only as water will yet more effectually take off the pungency of the acid. To the 4th, where spirit of wine preserves bodies from turning to vinegar, and stum liquors, M. Kunkel says, that this is a plain proof of its being an acidum; or the sour vapour of accended brimstone produces the same effect: only with this difference, that the like acidum in spirit of wine is less copious, and for that reason you must use a much greater proportion of spirit. So that the induction is false, that there is no acid in spirit of wine, since acids do generally, if not always, hinder the fermenting of bodies. To the 5th he answers, that spirit of wine, digested with syrup of violets, will change the colour.

Dr. Voight having thus far been on the negative, by declaring what spirit of wine is not; he now asserts it to be an oil, which is ardent or inflammable, by no means on account of its volatile salt, for then salt of urine must burn; nor on account of any acid, which has been already refuted; much less on account of its water and earth, for that would be absurd: ergo ratione olei. He gives an instance how spirit of wine may be reduced to a true oil; for as it is a spirit, he says it has only lost the outward form and consistence of oil, being made more fine and subtile, yet notwithstanding he proposes a way to bring it to oil again: take a quantity of high spirit of wine, put it into a cucurbit, which is not very well closed, let it stand for 3 months, then endeavour to rectify it in a gentle balneo; you will find little spirit that will rise, but a good quantity of inflammable oil will remain at the bottom.

M. Kunkel's answer principally consists in denying the experiment, of reducing spirit of wine to oil, by the way and method proposed, or by any other: unless he perhaps first united some oil with his spirit, and then the air having sublimed the spirit, which was lighter than the oil or water, by this means perhaps, he obtained some little oil.

Dr. Voight examines an epistle of M. Kunkel's, sent to him for that purpose. The first presupposition of the author of the epistle is, that all vegetables contain an acidum et urinosum. The 2d, that all fermentations produce acids, which are separable by art. In answer to the first, Dr. Voight complains of his having omitted oleum, which is more certain, and constant in all vegetables, than acidum. For pepper, ginger, camphire, nutmeg, &c. have no acidum.

M. Kunkel will not allow his instances in pepper, &c. to be just, but on the contrary, that they contain a sal volatile et fixum, or an alcali salt.

Dr. Voight goes to the 2d supposition of M. K. that all fermentations produce acids, which are separable by distillation. This he refutes, as contrary to experience, since mead, and Canary wines, and even their Berlin beer itself, will ferment very high, without any acidity, but rather continue very pleasant and sweet. He owns that there are wines, both French and Rhenish, that after fermentation taste a little sourish or sharp, but that he says, follows fermentation ex accidenti ratione materiæ.

M. Kunkel replies here, that he has not fairly prosecuted the work of fermentation, which if he had done, out of all vegetables an acid might be made. And that the effect of fermentation may properly be called acetum or vinegar.

Dr. Voight proceeds; in the epistle he animadverts upon M. Kunkel's notion, viz. That salts, by virtue of their acidity, preserve bodies from corruption. This he says is against reason and experience: for salts are endowed with a power solvendi, incidendi, corrodendi, disuniendi, dividendi: the most corrosive menstruums, as aquafortis, regis, &c. justify it. As also the constitution of vegetables and animals, both which do soon corrupt and dissolve, on account of their abounding with this saline principle. Nay salt itself cannot resist the moisture of the air, being soon resolved in it. He instances yet farther, in the common way of powdering or pickling flesh, declaring against the ill quality of salt, for depriving flesh of its proper and genuine taste, bringing it to an acorem, and corrupting instead of conserving it, by making it unfit for nourishment, in producing diseases, &c.

M. Kunkel, in his reply, refers to the good housewifery, common custom, and universal experience, which justifies his salt, though an acid, to be a true preserver of bodies from corruption: he then dislikes his conceit, that flesh abounding with salt, should therefore corrupt; but on the contrary, being sweet, and wanting salt, does therefore the sooner putrify. This he proves by an instance, that a whole ox has very little salt in it; I suppose he means of fixed salt; he thinks a pickled herring, or powdered beef, very unjustly termed corrupted fish and flesh.

Dr. Voight takes up the author of the epistle, for urging an experiment, which he calls against reason and experience, viz. that a pound of rotten wood contains more acidum or alkali, than 5lb. of green wood: he pronounces the quite contrary for truth, proceeding from one degree of putrefaction, to what he calls a central corruption, or rottenness of wood, where he found that wood nowise tainted, afforded the most salt, and that in every degree of greater putrefaction, or the more the wood inclined to rottenness, the less salt it afforded, until he came to that which was quite rotten, which afforded none at all.

M. Kunkel answers as positively in his own behalf, that he is certain of the contrary, and could make it good.

Dr. Voight complains of the author's epistle, that he confounds salia alkalia et acida; for they must needs differ in their natures, which make so great an effervescence in their mixture, as sal tartari, et spiritus vitrioli do. He demands an experiment how to make an acidum ex sale alkali.

M. Kunkel answers, that while these salts are in their gross form, as he stiles it, they are very different: but, in a book he calls his Annotations, he shows a way how to reduce alkalia to acida, et vice versa: and that only by depriving one of its terrestreity, and giving it to the other. He instances in sal absinthii, out of which a spirit may be drawn to dissolve gold.

Dr. V. animadverts farther on the epistle, where he finds this assertion; "nobody can deny, where you have heat and light, you must needs have two contraries, viz. heat and cold, or an acidum and a urinosum." This the animadverter says, is contra rationem: for two contraries, non possunt immediate eundem effectum producere. He says acidum et urinosum may perhaps be external or accidental causes of heat, by promoting a sudden motion and conflict in some bodies, but must not be confounded with the matter that is inflammable. Thus, water and oil of vitriol mixed together, produce a much greater heat than if there be put into the oil of vitriol a quantity of urinous salt: but it is plain, that in the water there is an urinosum. Thus if you pour water upon calx viva (which is an alkali, and has nothing to do with acidity,) you will produce a great heat; yet we want here one of the two principles of heat, viz. the acidum. M. Kunkel thinks he has made it clear, that these two principles were demonstrated very plainly: to elucidate the matter, he gives some effects of sp. acidi (as for instance aquafortis,) and urinosi (as sp. urinæ itself;) these 2 in conjunction dissolve gold, make aurum fulminans, produce heat, flame, and even lightning. As to the experiment of water and oil of vitriol, he answers, that though he should still continue to deny, that water holds no urinosum, yet he must allow it to be a frigidum, which yet afforded this warming effect. As to the experiment of calx viva, he will not allow it to be without an acidum. He would confirm his opinion by an instance of the great phosphorus, called in England the solid, which he says has nothing either oleaginous or resinous, but holds salia acida et urinosa, and yet performs so many luminous effects.

Dr. Voight observes, that the author gives the reason why sp. of nitre and sp. of wine make so great an ebullition, but sp. of wine and oil of vitriol none at all: viz. because nitre is not so pure an acid as vitriol; for which reason they may stand the quieter, and without alteration together. To this reason the doctor gives no credit, for he says, sp. of nitre is as pure as that of vitriol, and that with sp. of wine, they make an alteration: and the higher they are, the more they heat; but if very highly rectified, they will produce flame on mixture.

M. Kunkel seems to be in nowise of his judgment, that the mentioned acid spirits are of an equal purity; he thinks in that of nitre there is something of an urinosum; in the oil of vitriol none at all. As to the experiment of flame, produced by oil of vitriol and sp. of wine, he tells him that he read it out of an author, but it never succeeded.

Since sp. of wine is an oily body, the author of the letter makes a question, how it comes to pass, that it makes no ebullition, and gives no colour to any spirit, except that of nitre? to which Dr. Voight answers, because it causes no more ebullitions, for that reason it approaches the nature of oils; for acidum et urinosum only excite those ebullitions. He answers further, that it does indeed make ebullitions, both with sp. of nitre, and of salt, but that it does not so by reason of any urinous salt it contains.

To this M. Kunkel thinks, he has copiously answered.

In the epistle, Dr. Voight answers to this objection, that if sp. of wine were an oleosum, why it would not mix with oil: he answers it by proving the contrary, that if you put into highly rectified sp. of wine, oil of fennel, or anniseeds, it will dissolve.

M. Kunkel replies, that it retains some few drops of this oil, by reason of that small quantity of acidum in the spirit; but if you impregnate it with more salts, it will take up and dissolve more oil. Also that it easily parts with this imbibed oil again: but true oils do all equally and thoroughly mix.

The author of the letter having put another query, about the causam ebrietatis, viz. whether it depended on a body, as oleaginous, or as sweet, urinous alkalisate, answers propter acidum? To which, Dr. Voight demands of him to show an acidum inflammabile taken out of sp. of wine. He proceeds then himself, to give an account of drunkenness: that wine affects the animal spirits, ratione olei, rather than salis acidi.

The author of the letter will not allow it to produce drunkenness as an oil: he quite divests oils of that quality, having never heard of any that got fuddled by drinking oils: but that oils have been taken into the body as preservatives against drunkenness. M. Kunkel concludes with his address to the Royal Society, excusing himself on the consideration of the great necessity of knowing the true nature of things, particularly of heat and cold: and that without a true account of the nature of salts, the harvest of chemistry will be very poor.

He selects an observation or two, wherein he has particularly observed, that a calidum et frigidum are to be found in a fulminating and combustible body. He will also be glad to be informed of the contrary by any chemist in Europe, if he can find out any body, that by a fair chemical examen, does not contain calidum et frigidum, except the true oil of vitriol, which, as already said, has only so much frigidum as it contains humidum or common water, which is so far overbalanced by the acidum, that we can discern no frigidum at all. In this menstruum all metals can be dissolved, excepting gold, which requires a frigidum to be joined with a calidum.

These terms of acidum, urinosum, and alkali, have so much confounded all men, that it has moved him to write particularly and copiously of them, in laboratorio suo experimentali. But in short, he finds all acids, excepting oleum vitrioli, to consist of salia composita, mixed with sal frigidum. Alkali salts are all much the same, if clean and pure, and nothing else but salia acida cum frigidis, and are much akin to common sea salt, when that is dispirited. The acids, if fixed by a mixture of earth, become alkali salts. But by joining pure oil of vitriol with such an earth as cornu cervi calcinatum, which carries in it no more of an acidum, or of an urinosum, this will not yet agree with other alkalies, unless by giving it a supply of an urinosum.

Salia frigida, if pure, are all homogeneous, of whatever vegetables or animals they may be made; from whence he infers, that nature only makes use of these two principal parts, which, since he came to understand it, has excused him from many a troublesome work or operation, and would also much lessen the processes of many, and lighten their labours, did they but know the true property of those bodies they work upon.

Since now he supposes himself to have laid this firm foundation, he will only hint at two or three well known materials, out of which heat and lightning may be brought, in which no oleum can be demonstrated to have been contained. This he needed not to have done, but with regard to the Oleisti, who would fain introduce some new thing, even against experience, and who would yet farther confound the confused world, through their opinionated philosophical arguments; whereas they had more need be led out of mistakes. He begins with aurum fulminans; to prepare this is so well known that it need not be described. The gold cannot be dissolved without a frigidum; but when it is once dissolved by it it must be precipitated by the same again, e. g. with spirit of urine; by this means you get a powder that gives a great report, a very clear light, and burns. If you cannot see the lightning, for it is very transient, put out the candle, and you will easily discern it. Here is nothing but an acidum and urinosum joined by an earth; from whence this thundering noise proceeds, of which, in some manuscripts that he has yet by him, he has more copiously treated. Perhaps it may be said, that if the matter were precipitated with salt of tartar, it would be one and the same thing, why does this make it heat as well, as if it were precipitated with an urinosum or frigidum? he answers in few words, that there is already in the solution as much of the urinosum as is necessary, so that a farther affusion of an urinosum will not well agree. Therefore it happens, that when the gold is precipitated by this alkali, so called, the urinosum may be so suppressed as not to make an explosion; now in case the precipitate should be dried, and imbibed sometimes with a spirit of urine, it will then recover its fulminating power again. So that we see, where heat and flame is, there must be two contrary principles.

In the next place, if a person take a good spirit of nitre, and drop upon it guttatim, spirit of wine, till it ceases to make an ebullition, the urinosum is then in a balance with the acidum. Only put this salt in a heated crucible, and it will go off with an explosion, only you have here no report, but heat and light; but if you join it with an earth, as aurum fulminans is, you shall then have a report. This spirit of nitre and calcined coral will do, if they are inspissated, or brought to be dry. If necessary, he could instance in that burning mixture ex oleo vitrioli, spiritu urinæ, et jove; in all these we have a calidum and frigidum, which is brought into motion by warmth, and may give heat, thunder, and lightning, as you please to manage it. Salt of tartar, sulphur, and nitre, make a fulminating powder; he will err here, that will ascribe the effect to sulphur, as if it produced it by virtue of its oil; it is very plain that it proceeds from the two contrary salts. The acidum alone produces heat, but no flame unless the frigidum be added; just as a red hot iron may burn or scorch a thing, that it cannot bring to flame, unless the frigidum should meet it. Oil of vitriol burns wood, which it could not do, unless it met its contrary there. He will pass by the terrible heat excited in the solution of Jupiter and Mars, although many such might be appealed to, which are well known to the experienced. He only mentions the wood-coal; take it, and let it be thoroughly kindled, and then go out of itself: kindle it thoroughly once more, and let it quench or go out in a crucible, or some place where the cold air may not too much annoy it: this may be done 10 or 20 times successively, and it will continue to give heat and light: if this came from oil, sure it must needs be a very fixed oil, and it is impossible it should perform such a work: but a sal duplicatum, which by virtue of its terrestreity is called an alkali, is to be found here.

A remarkable Case of Hydrophobia. By Dr. Roger Howman, Physician in Norwich. N° 169, p. 916.

Oct. 1, I was called to a patient in this city, who about 6 weeks before had been bitten by a mad fox, on the right hand; he began to be indisposed the Saturday before with shifting pains, which still increased, especially on his right hand, arm, shoulder and back, but not to confinement. He had been advised to take a dose of the common purging spirit of scurvy-grass, which gave him

7 or 8 stools, and made him very faint and weak. So I found him, and complaining that he could not use his right hand, it beginning to be paralytic, though his pains were much abated there, and where else they had been most troublesome; only on the lower parts, or small of his back, which soon after vanished also. He had bled freely at the wounds the fox had made, and they healed without any farther trouble; only at intervals a little pain on that hand and arm. Though the aquæ pavor, or dread of water, did not yet appear, his heat was much increased, and his pulse intermitted every 5th or 6th stroke, on the right side only: he also looked ghastly and thin, but his eyes sparkling and fiery. I prescribed the best temperate antispasmodic and antiparalytic remedies to be mixed with the specifics of common use in a hydrophobia. Next morning he complained his night had been restless, that then he had wholly lost the use of his right hand, and though the pains were more abated, yet he was very hot and uneasy: his pulse was then much stronger than over night, but intermitting on the right side only as before; his countenance was somewhat more ghastly, yet his veins very full as in the beginning, and increase of a fever; and no hydrophobia appearing, I advised him to bleed 6 or 7 ounces at the left arm, and the continuance of what I had prescribed before; he bled 8 ounces very freely, the blood well coloured, but very thick. In the afternoon, going into the country to visit some patients, to whom I was pre-engaged there, I could make no further observations till my return on Friday the 6th, at night; not many hours before he died. On Thursday after I left him, the great symptom appeared, and in my absence another was consulted, who gave him many remedies. At my return his heat was very great, his pulse very high, and intermitted then on both wrists, and if any thing were offered him to drink standing or sitting, he started as if his head would have fallen backwards off his shoulders; but when laid on his pillow, he could, though with great difficulty and uneasiness, at times get down a spoonful: he looked then very thin and ghastly, and seemed shy or afraid of every body that came suddenly near him, telling them they stifled him, or stopped or hindered his breath in coming so hastily to him. His reason was all along very good: his voice was broken and imperfect, as of those persons whose tongue and other organs of speech are turning paralytic. I saw him again at 10 that night, when all symptoms were growing worse; yet he could then walk out of one room into another, with very little help; but between 12 and 1 next morning he died, without any convulsive motions, sighs, or groans; as if in a moment there had been a total paralysis. From whence it is most observable, 1. That as the pains, which were like those in a rheumatism, abated, the paralysis and fever increased. 2. As the fever increased, the intermission of the pulse grew

more frequent, though it was much stronger; but why it intermitted first on but one side, is not easily accounted for. 3. That the imperfection of voice, as well as the difficulty of swallowing, were the effects of the paralysis, may probably be allowed, and be a satisfactory reason why the person Dr. Lister mentions, could not use the quill which was given him to suck with. 4. That his thin ghastly aspect, the defect of spirits and tonic vigour, was from a paralytic original, is not unreasonable to conjecture. 5. That the paralysis chiefly affected the muscles of the head and upper parts, may be partly collected from his inability to hold his head steady at the approach of any liquor; the fear thence rising, causing him to start, and his head so to fall backwards, as if it would fall off his shoulders. 6. And that his lower parts were less affected, is probable because 2 or 3 hours before he died he could walk out of one room into another, even when his voice was hardly intelligible.

Norwich, Jan. 27th, 1684.

Extract of a Letter from Senior Ciampini, to Dr. Croone;† concerning a late Comet seen at Rome. Abridged from the Latin.* N° 169, p. 920.

A new comet has lately been discovered at Rome, by the Abbot Blanchini,‡ the disciple of Gem. Montenari. It appeared but small, but regular in its orbit, of a thin light, and like a faint star. But through a telescope it was

* John Justin Ciampini, a learned Italian ecclesiastic, was born at Rome 1633, and died 1698. He was a zealous promoter and cultivator of learning. By his exertions were instituted two learned societies, viz. the academy of ecclesiastical history, and that of physics, and mathematics, at Rome. Ciampini was author of several pieces on antiquarian subjects.

† Dr. Wm. Croon, or Croone, was one of the most early and useful members of the Royal Society, being professor of rhetoric in Gresham college, at the time when the members of the society held their meetings there. At their first meeting, when formed into a regular body, Nov. 28, 1660, he was appointed their register, for taking the minutes of what passed at their meetings; which he continued to do till their charter was passed, by which Dr. Wilkins and Mr. Oldenburg were nominated joint secretaries. Dr. Croone died Oct. 1684, and left plans of two lectures to be founded, one in the college of physicians, on the nerves and the brain; the other an annual lecture at the Royal Society, on the nature and laws of muscular motion, which is still continued. Dr. Croone has been characterized as a general scholar, an accurate linguist, an acute mathematician, a well-read historian, and a profound philosopher.

‡ Francis Blanchini, or Bianchini, one of the most learned men of his time, was born at Verona 1662, and died in 1729, at 67 years of age. His taste for mathematics and physics induced him, at an early age, to found a mathematical academy in his native place. He repaired to Rome in 1684, where he became librarian to pope Alexander VIII. He afterwards obtained several ecclesiastical preferments from different popes, and was secretary to the conferences for the reformation of the calendar. Indeed Bianchini was greatly esteemed by all the learned, and was admitted a member of several academies. He was author of many learned works: a Universal History; an edition of Anastasius the librarian; and some dissertations on antiquities, &c.

more luminous. On June 30, new style, 1684, I first saw the comet, when its place was in libra, 9° some minutes, with 8° + north latitude. The first 17 days of July, its places by observation, were as below.

July. Day.	Longit.	Lat. North.	July. Day.	Longit.	Lat.
1	11° 18′ ♎	13° 12′	10	24° 42′ ♎ ..	39° 40′
2	13 16	17 57	11	25 44 ..	40 49
3	14 58	22 12	12	26 38 ..	41 58
4	16 45	25 40	13	27 23 ..	43 5
5	18 30	28 50	14	28 32 ..	44 9
6	19 50	31 34	15	29 49 ..	45 12
7	21 7	33 54	16	0 58 ♏ ..	46 20
8	22 20	36 6	17	2 8 ..	47 40
9	23 32	38 0			

The last 4 observations may require some better calculation, as an error of some minutes seems to have crept in. The observation on the 1st of July is very exact; for the comet appeared in the telescope along with the star in virgo marked Ω by Bayer, and fell under the girdle of the first northern part. But the most certain observation of all, is that on the 6th day, when arcturus was seen through the telescope along with the comet. Also, on the 14th day, the comet and the star χ in the hunting pole of Bootes, under his shoulder, were seen at one view in the telescope.

Observations on boiling Fountains and Subterraneous Steams. By Dr. Tancred Robinson, F. R. S. N° 169, p. 922.

The water of the noted boiling fountain at Peroul, near Montpelier, is observed to heave and boil up very furiously in small bubbles; which manifestly proceed from a vapour breaking out of the earth, and rushing through the water, so as to throw it up with noise, and in many bubbles; for upon digging any where near the ditch, and pouring other water on the dry place newly dug, the same boiling is immediately observed. The like bubbling of water is also found round about Peroul on the sea shore, and in the Etang itself. In order to discover the cause of this odd phenomenon, Dr. Robinson took some of the sand and earth out of the fountain and ditch, putting it into vessels, and pouring some of the same water upon it, there did not appear the least commotion or alteration; the surface of the water continuing very smooth, equal, and quiet. On further search, he discovered in several dry places of the ground thereabouts, many small venti-ducts, passages, or clefts, where the steam issued forth; at the mouths of these pipes, placing some light bodies, as feathers, small thin pieces of straws, leaves, &c. they were soon removed away. This vapour, on the application of a lighted candle or torch, did not flame or catch fire in the least, as the fumes running through a boiling spring near Wigan in Lancashire do, as noted in the Philos. Trans. N° 26; so that here we have two different sorts of steams causing these boilings, yet neither of the fountains are medicinal, nor so much as warm: the like is related by Varenius, near Culm, and by Dr. Plott in England. There are other boiling waters, of a quite contrary temper, being actually hot to several degrees, so as to boil eggs and many other things, put into them; as those near the Solfatara not far from Naples; as also on the top of Mount Zebio in the Duke of Modena's territories, not far from his villa near Sassolo; and in the source of the Emperor's bath at Aix la Chapelle, in the duchy of Juliers. Varenius tells us, that in Japan there bursts out a boiling spring, so hot that no water can be heated so much by the strongest fire; that it retains its heat three times longer than common water; and that it does not flow continually, but for two hours each day; and then the force and violence of the vapours are so great, that they remove large stones, and raise them to the height of 3 or 4 ells, with a noise like the explosion of a great gun.

From the foregoing history, we may take occasion to reflect a little on the variety of exhalations prepared in and flying out from the vast subterraneous magazines and repositories, as to their qualities and effects, some being cold and dry, resembling air or wind, as those near Peroul, and in the caverns of mountains, especially those of Æolus, and other hills of Italy, as also in mines; others are inflammable, and of a bituminous nature, though not actually warm, as those near Wigan in Lancashire; there are also many steams very hot, sulphureous, and saline, more especially those in the natural stoves, sweating vaults, grots, baths, and the volcanos near Naples, Bajæ, Cuma, and Puzzuolo, as also in some of the subterraneous works at Rome; others there are of an arsenical and such like noxious qualities, as in the Grotta del Cane, on the bank of the Lago Agnano; in several mines, and in poisonous springs and lakes. Now these various steams meeting with, and running through waters, must cause a great variety of phænomena and effects in them.

Whether this great diversity proceeds from the various breaths of the Pyrites and the lapis calcarius, whilst under their different states and changes, or from other effluvia, I dare not determine; however, I am convinced that sulphur is sublimed from the pyrites, especially that gathered on mount Ætna, Vesuvius, the solfatara, and in the stoves of S. Gennaro; for most of the stones and cinders thrown out of those mighty furnaces do manifestly contain iron, as appears by the magnet. As to the salt, taken by many writers to be a species of sal ammoniac, found together with the sulphur in those places, it appears to be a kind of nitrum calcarium; which seems to be confirmed from the great quantity of lime-stone round about Naples; many beds of it lying up and down the Terra di Lavoro, or Campania Felice.

Of the Weight of a Cubic Foot of several Bodies, tried in a Vessel of well-seasoned Oak, whose Concave was an exact Cubic Foot. By the Direction of the Philosophical Society of Oxford. N° 169, p. 926.

The following bodies were poured gently into the vessel, and those in the first 12 experiments were weighed in scales turning with 2 ounces, but the last 7 were weighed in scales turning with 1 ounce. The pounds and ounces here mentioned are avoirdupois.

	lb.	oz.
1. A foot of wheat, worth 6s. a bushel	47	8
2. Best wheat, worth 6s. 4d. a bushel..	48	4
3. The same sort measured again......	48	2
4. White oats of the last year	29	8
5. Blue pease, much worm-eaten......	49	12
6. White pease, of last year but one....	50	8
7. Barley of last year................	41	2
8. Malt, of two months old..........	30	4
9. Field beans, of last year but one	50	8
10. Wheaten meal, unsifted	31	0
11. Rye meal, unsifted	28	4
12. Pump water	62	8
13. Bay salt......................	54	1
14. White sea salt	43	12
15. Sand	85	4
16. Newcastle coals................	67	12
17. Pit coal from Wednesbury	63	0
18. Gravel	109	5
19. Wood-ashes	58	5

Another List of the specific Gravities of Bodies, which are in Proportion as the following Numbers.

Pump water	1000
Fir dry	546
Elm dry	600
Cedar dry	613
Walnut tree dry	631
Crab tree middling dry	765
Ash middling dry, and of the outside lax part of the tree	734
Ash more dry, and about the heart	845
Maple dry	755
Yew of a knot or root, 16 years old	760
Beech middling dry	854
Oak very dry, almost worm-eaten	753
Oak of the outside sappy part	870
Oak dry, but of a very sound close texture	929
The same tried another time	932
Logwood	913
Claret	993
Moil cider not clear	1017
Sea water settled clear	1028
College plain ale, the same	1028
Urine	1030
Milk	1031
Box	1031
Redwood	1031
Sack	1033
Beer vinegar	1034
Pitch	1150
Pit coal of Staffordshire	1240
Speckled wood of Virginia	1313
Lignum vitæ	1327
Stone bottle	1777
Ivory	1826
Alabaster	1872
Brick	1979
Heddington stone, the soft lax kind	2029
Burford stone, an old dry piece	2049
Paving stone, a hard sort from about Blaidon	2460

Flint	2542	Block tin	7321
Glass of a quart bottle	2666	Copper	8843
Black Italian marble	2704	Lead	11345
White Italian marble	2707	Quicksilver	14019
Another sort, closer texture	2718	Another sort	13593

The last experiment was tried with another quantity of quicksilver, which had been used in water in the preceding experiment, and this last is more to be depended on; as a small mistake, though here in the calculation allowed for, was found in the weight of the glass containing the quicksilver in the preceding trial.

The solids here mentioned were examined hydrostatically, by weighing them in air and water; but the fluids by weighing an equal portion of each in a glass holding about a quart. The numbers show the proportion of gravity of equal portions or bulk of these bodies. So that knowing by the former table, the weight of a cubic foot of water, and by this, the proportion in gravity between water and alabaster, we may by the rule of three find the weight of a cubic foot of alabaster; and the same of any other of these bodies; or we may know their magnitude by knowing their gravity. So that an irregular piece or quantity of these bodies being offered, by weighing them, we may know their just magnitude without further trouble.

A Letter from Dr. Robert Plott, of Oxford, to Dr. Martin Lister, F.R.S. concerning the Use which may be made of the following History of the Weather, made by him at Oxford throughout the Year 1684. N° 169, p. 930.

This register of the barometer is by a new and easy invention of observing the rise and fall of the mercury in the barometer, by parallel lines drawn from every decimal part of each month of its whole extent, marked by the wandering pricked line in the figure belonging to each month, like a map as invented by Dr. Lister. In a printed list for every day in the year, is set down the corresponding state of the weather, as to the quarters of the wind, E. W. N. and S. also as to the rain or fair weather, frost or snow, &c.

This practice is here recommended, as likely to be attended with many beneficial effects, observing that, by this means it is doubtless, that the learned Dr. Goad of London has arrived to such a pitch of knowledge in predicting weather, &c. i. e. by making such old almanacs as this for many years instead of new ones, which if once faithfully done in divers parts of the world, we should certainly obtain more real and useful knowledge in these matters in a few years than we have yet arrived to in many centuries; which was doubtless the opinion of the industrious Walter Merle, Fellow of Merton College, who thus observed the weather here at Oxford every day of the month, seven years together, viz. from Jan. 1337 to Jan. 1344; the MS. copy of which observations are yet remaining in the Bodleian library, and doubtless it was some such consideration as this that induced Erasmus Bartholin to make observations of the weather every day of the month through the whole year 1671, which are printed inter Acta Medica Tho. Bartholini.

Abstract of a Letter from Dr. Peirce of Bath, giving an Instance of the Effects of the Bath, in curing the Palsy and Barrenness. N° 169, p. 944.

Among the many diseases these waters have been noted for, the cure of palsies and barrenness are two: an instance of both in one person is the following. A gentlewoman, of about 30 or 32 years of age, having been married about 10 or 12 years, and never with child, was suddenly seized with a palsy on the left side; for which, after 8 or 10 months trial of other means to little purpose, she was brought to the bath, where, after the usual preparations, and some internal means, she continued that season, about 6 weeks. The winter coming on, she was forced to desist; but returned again early the next year, and continued with us the whole summer, and recovered, in great measure, the use of her arm, and hand, leg, and tongue, and also, in a few weeks after she returned to her husband, conceived with child, and had, at about a year and a half distance between them, 5 children following. She has had no return of a palsy, but is infirm, I think consumptive; she is now about 51 years of age.

Caroli Drelincurtii Experimenta Anatomica, quibus adjecta sunt plurima Curiosa super Semine Virili, Fœmineis Ovis, Utero, Uterique Tubis, atque Fœtu. Lugd. Bat.* 1684, 12*mo*. N° 169, p. 945.

The chief part of this book contains accounts of experiments on dogs, in which occur the following observations. A mastiff bled 5 lb. Troy weight, of

* Charles Drelincourt, who acquired great celebrity by his anatomical writings, was a native of Paris, where he was born in 1633. He studied at Montpelier, took his doctor's degree there, and was afterwards appointed chief physician to the forces under the command of Marshal Turenne, during the campaign in Flanders. He afterwards settled at Paris, where he got into considerable practice, which, however, he relinquished in 1668 for a professorship at Leyden. Besides the work above noticed, the following are among his principal tracts, viz. Diatribe de Partu Octimestri; De Conceptu adversaria; De Conceptu Conceptus; Libitinæ Tropœa; Apologia Medica; Præludia Anatomica; de Humani Fœt. Membran. These, with some other tracts, were republished collectively, about 30 years after his death, under the title of Opuscula Medica, 4to. 1727, with an account of the author's life, by Boerhaave, who had been one of his pupils, and who never mentioned him but

blood, within the space of half an hour, at several arteries.—The ribs of a dog were found more brittle near that end by which they are joined to the vertebræ than at the other end.—A needle being driven into the brain of a dog, between the first vertebræ and the os occipitis, the dog seemed as if struck with an epilepsy, and died in a little time.—The passage from the pelvis into the ureter of a dog being stopped, the kidney of the opposite side was found larger than ordinary.—Six veins being tied in a dog, viz the two crurales, the two axillares, and the external jugulars, the dog seemed choked in a very little time; the ligatures being loosened, and blood drawn, the beast moved again.—A procidentia ani has been caused in a dog to the length of a foot. A dog not much troubled at the pricking of the meninges, was concerned when the spinal marrow was pierced. Experiments have been tried by the author, with tallow injected into the veins of dogs.—The venæ mammariæ have been observed to communicate with the epigastricæ in a bitch; for blood was easily pressed from the former into the latter, and back again.—A phial which, when filled, contained 1 lb. of rain water, held 1 lb. and 1 oz. of venous blood, of arterious blood, and of serum, taken promiscuously from both sorts of blood; but it held 1 lb. 1 oz. and $\frac{1}{4}$ of grumous blood, cleared of the serum.—The author affirms that no lacteals arise either from the stomach or intestina crassa.—Valves are observed in the farther part of the ductus thoracicus, near its ending in the subclavian vein, contrary to what is affirmed by some anatomists.—The valves, lying in pairs in the ductus thoracicus, are generally an inch and half one pair from another. One kidney has been found 8 times larger and heavier in a dog than the other.—The author asserts that the lacteals impart no liquor to the pancreas asellii. Their origin has been traced as far as the glandulous tunic of the intestines.

There are several other observations concerning the lacteals, the ductus thoracicus, and the valve at the end of it. There follow also several queries de semine virili, de fœmineis ovis, vel in ovario, vel extra de utero; to which are subjoined some corollaries concerning the human fœtus, which he will not allow to have an allantois, or urachus.

in the highest terms of admiration and respect. He was much devoted to the ancient writers, and maintained that Hippocrates was acquainted with the circulation of the blood, and that the small pox is not a disease of modern times. He bestowed some pains upon comparative as well as human anatomy, having dissected various quadrupeds, of which an account is inserted in Blaise's Anatome Animalium. Dr. Drelincourt died in 1697. He was a profound scholar, and was particularly well versed in the Greek; but Haller, not without reason, criticises his latinity as turgid and metaphorical. He wrote with much acrimony against his contemporary Sylvius, and evinced a great contempt for the chemical sect of his days.

Of the Bogs and Loughs in Ireland. By Mr. William King, F.D.S.
N° 170, p. 948.

As to the origin of bogs, it is to be observed, that there are few places in our northern world, but have been noted for them, as well as Ireland; every barbarous ill-inhabited country has them. I take the loca palustria, or paludes, to be the very same we call bogs, the ancient Gauls, Germans, and Britons, retiring, when beaten, to the paludes, is just what we have experienced in the Irish, and we shall find those places in Italy that were barbarous, such as Liguria, were infested with them, so that the true cause of them seems to be want of industry. To show this, we are to consider, that Ireland abounds in springs; that these springs are mostly dry in the summer, and the grass and weeds grow thick about those places. In the winter they swell and run, and soften and loosen all the earth about them. Now that swerd or surface of the earth, which consists of the roots of grass, being lifted up and made fuzzy or spongy by the water in the winter, is dried in the spring, and does not fall together, but wither in a tuft, and new grass springs through it, which the next winter is again lifted up; and thus the spring is still more and more stopped, and the swerd grows thicker and thicker, till at first it make what is called a quaking bog, and as it rises and becomes drier, and the grass roots and other vegetables become more putrid, together with the mud and slime of the water, it acquires a blackness, and becomes what is called a turf bog. I believe when the vegetables rot, the saline particles are generally washed away with the water, in which they are dissolved; but the oily or sulphureous remain and float on the water; and this is that which gives turf its inflammability. To make this appear, it is to be observed, that in Ireland the highest mountains are covered with bogs as well as the plains, because the mountains abound much in springs. Now these being uninhabited, and no care being taken to clear the springs, whole mountains are thus over-run with bogs.

It is to be observed also, that Ireland abounds in moss more than probably any other country, insomuch that it is very apt to spoil fruit-trees and quicksets. This moss is of divers kinds, and that which grows in bogs is remarkable; for the light spongy turf is nothing but a congeries of the threads of this moss, before it be sufficiently rotten; and then the turf looks white, and is light. It is seen in such quantities and is so tough, that the turf-spades cannot cut it.—In the north of Ireland they call it old-wives tow, as it is not much unlike flax; the turf-holes in time grow up with it again, as well as all the little gutters in the bogs; and to it the red or turf bog is probably owing; and from it even the hardened turf, when broken, is stringy, though there plainly appear in it parts of other vegetables; and it is probable that the seed of this bog moss, when it falls on dry and parched ground, produces heath.

It is further to be observed, that the bottom of bogs is generally a kind of white clay, or rather sandy marl; that a little water makes it exceedingly soft; and when dry, it is all dust; so that the roots of the grass do not stick fast in it; but a little wet loosens them, and the water easily gets in between the surface of the earth and them, and lifts up the surface, as a dropsy doth the skin. Again, bogs are generally higher than the land about them, and highest in the middle; the chief springs that cause them being commonly about the middle, from whence they dilate themselves by degrees; and besides if a deep trench be cut through a bog, you will find the original spring, and vast quantities of water will be discharged, and the bog subside.

It must be allowed that there are quaking bogs otherwise produced. When a stream or spring runs through a flat, it fills with weeds in summer, and trees fall across and dam it up; then in winter the water stagnates more and more every year, till the whole flat is covered; then there grows up a coarse kind of grass peculiar to these bogs; this grass grows in tufts, and their roots consolidate together, and yearly grow higher, even to the height of a man; the grass rots in winter, and falls on the tufts, and the seed with it, which springs up next year, and so still makes an addition; sometimes the tops of flags and grass are interwoven on the surface of the water, and this gradually becomes thicker, till it lie like a cover on the water; then herbs take root in it, and by a plexus of the roots it becomes very strong, so as to bear a man. Some of these bogs will rise before and behind, and sink where a man stands to a considerable depth; underneath is clear water: even these in time will become red bogs; but may easily be turned into meadow by clearing a trench to let the water run off.

The inconveniences of these bogs are very great; a considerable part of the kingdom being rendered useless by them; they keep people at a distance from each other, and consequently interrupt them in their affairs. Generally, the land which should be our meadows, and the finest plains are covered with bogs; this is observed over all Connaught, but more especially in Longford and also in Westmeath, and in the north of Ireland. These bogs greatly obstruct the passing from place to place; and on this account the roads are very crooked, or they are made at vast expence through bogs. The bogs are a great destruction to cattle, the chief commodity of Ireland; for in the spring, when they are weak and hungry, the edges of the bogs have commonly grass, and the cattle venturing in to get it, fall into pits of sloughs, and are either drowned or hurt in the pulling out; the number of cattle lost this way is incredible. The bogs are a shelter and refuge to tories and thieves.

The fogs and vapours that arise from them are commonly putrid and stinking, and unwholesome: for the rain which falls on them will not sink, there being hardly any substance of its softness more impenetrable to rain than turf, and therefore rain-water stands on them, and in their pits, where it corrupts, and is exhaled all by the sun, very little of it running away, which must of necessity infect the air. The bogs also corrupt the water, both as to its colour and taste; for the colour of the water that stands in the pits, or lies on the surface of the bog, is tinctured by the reddish black colour of the turf; and when a shower comes that makes these pits overflow, the water that runs over tinctures all it meets, and gives both its colour and stink to many of the rivers.

The natives however had formerly some advantage from the woods and bogs; as by them they were preserved from the conquest of the English; and probably a little remembrance of this makes them still build near them: it was then an advantage to them to have their country unpassable, and the fewer strangers came near them, they lived the easier; for they had no inns, every house where you came was your inn; and you said no more, but put off your brogues and sat down by the fire; and still the natural Irish hate to mend highways, and will often shut them up, and change them, being unwilling strangers should come and burthen them. Though they are very inconvenient, yet they are of some use; for most persons have their fuel from them. Turf is accounted a tolerably sweet fire; and having very impoliticly destroyed our wood, and not as yet found stone coal, except in few places, we could hardly live without some bogs; when the turf is charred, it serves to work iron, and even to make it in a bloomery or iron-work: turf charred I reckon the sweetest and wholesomest fire that can be; fitter for a chamber, and for consumptive people, than either wood, stone-coal or charcoal.

Turf-bogs preserve things a long time: a corpse will lie entire in one for several years; also trees are found sound and entire in them, and even birch and alder that are very subject to rot; such trees burn very well, and serve for torches in the night.

All the inconveniencies of the bogs may be remedied, and may be made useful by draining them; and all or most of them have a sufficient fall for that purpose. The great objection against them is the expence, and it is commonly thought that it would cost much more than would purchase an equal piece of good ground: for an acre of good land in most parts of Ireland is about 4s. per annum, and the purchase 14 or 15 years, so that 3l. will purchase an acre of good land; and it is very doubtful whether that sum will reduce a bog; but this is far from the fact, as most bogs would well reward the expence of draining them.

As to loughs or lakes, the natural improvement of them, is first to drain them as low as possible; and then turn the residue of the water into fish-ponds; by planting a few trees about them, they may be made both useful and ornamental. As to those places called turloughs, quasi terreni lacus, or land-lakes; they answer the name very well, being lakes one part of the year of considerable depth, and level smooth fields the rest. There holes are in these, out of which the water rises in winter, and retires again in summer; many hundred acres being drowned by them, and those the most pleasant and profitable land in the country: the soil is commonly a marle, which, by its stiffness, hinders the water from turning it into a bog; and immediately when the water is gone, it hardens, and becomes an even grassy field; these, if they could be drained, would be fit for any use; they would make meadow; or bear any grain, but especially rape, which is very profitable. The lakes are chiefly in Connaught; and their cause is obvious enough, it being a stony hilly country; these hills have cavities in them, through which the water passes: it is common to have a rivulet sink on one side of a hill, and rise a mile or half a mile from the place: the brooks are generally dry in summer; the water sinking between the rocks, and running under ground; insomuch as that in some places where they are overflowed in winter, they are forced in summer to send their cattle many miles for water. There is one place on a hill near Tuam, between two of these turloughs, where there is a hole called the Devil's Mill, at which a great noise is heard, like that of a water under a bridge: when there is a flood in winter, one of the turloughs overflows, and vents itself into the hole, and the noise probably proceeds from a subterraneous stream; which in summer has room enough to vent all its water, but in winter, when rain falls, the passages between the rocks cannot discharge it, and therefore it regurgitates and covers the flats.

These turloughs are hard to drain; being often encompassed with hills, and then it is not to be accomplished: often they have a vent, by which they send out a considerable stream; and then it is only making that passage as low as the bottom of the flat, and that will prevent the overflowing: it sometimes happens that the flats are as low as the neighbouring rivulets, and probably they are filled by them; and then it is not only necessary to make the passage from the flat to the rivulet, but also to sink the rivulet, which is very troublesome, the passage to be cut being commonly rocky.

An Abstract of a Letter from Dr. Sam. Threapland, of Hallifax, to Dr. Plott, giving an Account of Stones voided by Siege. N° 170, p. 961.

A carpenter, about 40 years of age, of a strong habit of body, and very laborious in his calling, came to me about 3 weeks since, and made great complaint of the torture he had suffered by reason of two stones he had voided by stool last Christmas, there being about 14 days time between. He perceived no alteration or disturbance in his body, till within 5 or 6 days that the first came away; then he began to complain very much of a pain in the belly, much resembling the colic, and of a stoppage in the intestines, not much unlike that in a tenesmus, having frequent provocations to go to stool, but to no purpose on trial. He took little or no rest in all that time; his stomach retained scarcely any meat or drink it received; till in the conclusion one of the stones came into the intestinum rectum, where it lodged for about a day; then coming within the reach of his finger, he drew it out by force, and presently after he was well again; and so continued for a fortnight, till the other began to move; which occasioned a pain beyond the former in proportion to its bulk, being larger, and kept him in torment about 8 days; during which time, there was an absolute suppression of excrements; and when the stone came into the rectum, it continued near 2 days within the reach of his finger, with which he could not draw it out by any means; till at length he bent a small piece of iron into the form of a hook, with which rude instrument his servant drew it out with much ado, and not without wounding the rugous coat of that part. After that he soon recovered his former condition, and felt no further harm from this accident. About 7 years before, the very like case had befallen him, voiding 2 stones after the same manner, and about the same size.

Concerning the Salts of Wine and Vinegar, &c. By M. Anthony Leewenhoeck, F. R. S. N° 170, p. 963.

Having found my yearly provision of vinegar, which had been about 3 months in the cellar, to be more sour than ordinary, I left it open to the air during some hours, at which time I observed a great many particles, which I call the salt of the vinegar, as N° 1, fig. A, pl. 4, tapering towards each end, and having in the middle a long brownish figure: others of the same extent, as fig. B, being as clear as crystal; and these were the most numerous: others being long and brownish, having in the middle of them a bright clear substance, as fig. C. In another place were some few oval figures, within which were contained some lesser ovals, as fig. D. Under the aforesaid figures, A, B, C, I thought I saw many that had a hollowness within them, like that of a boat; sometimes one of the figures appeared, the one half brown, and the other part clear; sometimes one of the figures lay across another, as at E. Sometimes there were figures which seemed to have been cut in two, each of them representing but one half of A, B, or C, as F. Many of these figures were so small, as scarcely to be seen, but so numerous that I judged them to be many thousands in one small drop of vinegar. These particles I take to be the sharp pungent matter, which causes the sense in the tongue, which we term sour.

Having put into a glass about 2 inches wide, a little vinegar, which was suffered to stand on my table for 8 weeks. In this time I found swimming on the surface of the vinegar many particles, within which I perfectly discerned a hollowness like to that of the inside of a boat; for the figures were now increased in thickness. Those that had the cavity turned to the eye, were as fig. G; those whose sides were turned to the eye, which had only part of the cavity to be seen, were as fig. H. I have also described a full grown live eel, as fig. LM, of which there were many more in the vinegar. As also a full grown eel which I killed, that the designer might see it more distinctly, as fig. NO. This also may serve to show the size of the salt particles, compared with that of the eels.

I took several new glasses with vinegar, and put in them some crabs' eyes, split into small pieces, lest the grit that comes from them, when pounded, should hinder my sight: I found that the long sharp figures which might be likened to a weaver's shuttle, were now changed into figures, whose basis was oblong, rising up pyramidally, like a pointed diamond, as N° 2, fig. P. Others had their basis square as fig. Q. Others an irregular quadrilateral, as fig. R. But these last two figures, I supposed were accidental, for want of sufficient matter to complete, and perfect them on all sides. The number of particles was so great, that in a gross computation I judged them to be 6000 in a drop about the size of two barley-corns. But that which I most wondered at, was, that these salt particles here, were almost all of the same size: a thing I never observed in any other salt before.

I took some vinegar out of a glass, that had crabs' eyes in it, at a time before all the air-bubbles were ascended: but even then the basis of the salt particles was four-square, and not as in common vinegar. The liquor had quite lost its acidity. I took also white chalk, beaten to pieces, and put it in vinegar, where it caused as a great commotion and rising of air-bubbles, as the crabs' eyes had done: it produced also the same figures of the salt, and the same insipidness.

When blood has been sometime out of the veins, small salts then begin to go together, and appear, having the figures of common salt; and I am persuaded there is not a drop of blood in the body, which has not its share of them.

I examined lately my wine, which is very fine and well tasted, such as in France is called Vin de Damoisselle, though it is but an Orleans wine, brought down the Loire. In it I saw many very pretty figures of different sizes, and some very small, which I shall call the salt of the wine; many of these figures were of the same shape, as those of the salt of vinegar, abovementioned. In some of these figures I have not only seen a cavity, but found them increase to so great a size, by the wine standing 24 hours uncovered on the table, that they equalled the thick particles of salt seen in vinegar, as seen N° 3, fig. A. There were also some figures which had no sharp points, but were roundish at the ends, as fig. B. There were also several figures which grew tapering at one end, and at the other were round, as fig. C. Also some figures, which differed from fig. C, so that one of their ends was not round, but flat, as fig. D. Also some few figures were longish, representing a thin right angled parallelogram, as fig. E. There were also many figures having their two longer sides roundish, and the shorter sides straight, something resembling a barrel, as fig. F. Some few figures made a perfect square; others again were twice as long as they were broad, being largest in the middle, and inclining towards the shorter sides; not much unlike a flat bottomed boat, with the fore and after parts flat, as fig. G. While all these figures in great numbers, were driving together in the quantity of a drop of wine, it was pleasant to see so great a variety. These salts I conceive would be sour upon the tongue, if there were not a great many sweet particles in wine mixed with them. From hence I gather, that the pleasant relish of wine, consists in one sort tempering the other, so as to make a harmony on the tongue and palate. The same thing we experience daily, by mixing several things, which, if used simple, would be either too sweet, too flat, or too sour; as for instance, butter and vinegar melted together, make a very grateful sauce. Hence we may comprehend the several tastes that wines may have, though they grow in the same vineyard; for not only will the bunches that grow on the southside of the hill be sweeter, because the heat of the sun draws from them the superfluous moisture; but the sharp or salt parts in the wine, by taking away the more waterish substance, become more rigid. Also, we may conceive the reason, why wine, having stood some time in the open air, loses its savour: namely, that many small salt particles are joined together, to make a few great ones; whereby, as the number of the salts are lessened, the sense is not so agreeably excited, as if it were touched in more places, though the bodies that do it be never so small.

In contemplating some large flat particles, as also some sharp ones, which were imperfect, I was confirmed in the make of those bodies, namely, that all the sharp salt particles in wine and vinegar, how small soever they are, had at first flat thin bodies, which by being rolled up at the four corners, make the salts above described.

Having set some Mosel wine for a few hours uncovered on the table, I then saw swimming in it divers figures of salt, such as I had formerly seen in other wine and vinegar; but with this difference, that in many of them I could not only perceive a thickness and a hollowness, but also distinguish that each of them consisted of 7, 8, 9, or 10 plates, lying over one another, as here N° 4, fig. A. I saw also several figures, out of the top of which other half figures appeared, as fig. B. There was something like this in the Orleans wine, but not near so much as here. I saw also several salts, which had other particles thrust through them, as fig. C. There were also some flat figures, whose sides were rolled up, as fig. D. And some whose shorter sides were indented, as fig. E. Some appeared like half of A, as fig. F. A few others had their ends blunt, as G.

I observed in good Hock wine, of a year old, after it had stood 3 hours uncovered, that there were salt particles in it, which were sharp at both ends, having a height, or ridge upon them, like the sharp bottom of a boat turned upside down, though they were otherwise diaphanous, as N° 5, fig. A. Such a kind of figure has appeared in French wine; but when I let this wine stand for two days and nights, some of the salt figures were larger, having several circumferences, some 2, 3, 4, and others so many that they could not be counted, as they lay close together, some were so beautiful that no sea production, whether coral or shells, might be compared to them. As in fig. B. Some figures were blunt at both ends, and sometimes one end more than another, as fig. C. In another place I saw swimming in the wine, salts which had not only several circumferences, but steps or wrinkles across, as at D. Some little particles, of different sizes, resembled wine casks, as E. In observing some places, where the thinner part of the wine was evaporated away, I found several figures like branches, seeming to proceed from one salt particle; in viewing them exactly, I saw that the branches consisted of nothing but very small salts joined together, some whereof were very regular, and the largest were placed at the end of the branches; as at FGHI.

In Hock wine, 6 years old, I found at first very few salt particles; but after standing 3 or 4 days, there were many more, though in much less numbers, than in the same wine that was but of a year old. But I am persuaded that the largest of these consisted each of above 100 small ones, compacted together, as N° 6, fig. A. When the greatest salts were got together, the smaller particles, swimming in the wine, cleave to them. Now and then there was a figure, that seemed the half of the former, as B; also some small figures which were diaphanous, and whose points were not so sharp as the large ones, as fig. C. There were also some diaphanous particles, greater than the last mentioned, having a small figure in the middle, as D: and a few that were blunt at the ends, as E. There were also some resembling dried branches of a tree, as beforementioned in wine of a year old, which branch-like figures consisted of small salts, hanging together.

From these observations, I may guess what may be the cause that Rhenish wine, not only keeps good a great many years, in a well-stopped vessel, but also loses its sourish taste, and takes one that is sweeter and milder; namely, because the salt particles in the Rhenish wine adhere together, and then stick to the bottom and sides of the cask, forming tartar; and so the older the wine, the fewer the salts. But the nature of French wines is contrary; for the salts in a well-stopped vessel do not run together, and therefore they never get a milder or sweeter taste: but in wines that come from Nantes, though the salts run more together, yet the sweetness is presently lost.

I bought some wine or Rinco, very pleasant, and of the growth of 1683. But it proved to be of the Palatinate. At first I observed few salts in it; but when it had stood open 24 hours, I discovered many that were sharp at each end, as N° 7, fig. A, having a division running along them, but being otherwise diaphanous, and appearing by the microscope of the same size here drawn; but there were an innumerable quantity of a less sort, which were of the same figure. I perceived also some that resembled a wine vessel, but they proved to have two sides rolled up, as fig. B; so also may the salt figures, which I have described as blunt at one or both ends, be like C and D. I saw also figures, as A, which had both their sharp points rolled up together, as at C. Also figures with one end not rolled, as at D. Some few figures had a square base, with the sides rising up pyramidal, like a pointed diamond, as E. Sometimes one of these figures was in the middle of another, as at E, as well as in the former wines. Some salt figures had their sides rolled up, so as the ends did not touch one another; but left an opening in the middle, as at F. Whereas others, that were more shut, seemed only to have a line on the back of them. Sometimes there appeared figures long and slender, as G. But, above all, a sort of small soft particles was in greatest numbers, of a globular figure, the whole body of the wine, except the salt particles, seeming to consist of them, and the sweetness to take thence its rise.

I took a little Rinco wine, which had worked in the cask all the foregoing summer, and had been pierced some weeks since, when it was fine, and had a good taste; this wine, when it had stood upon the table about an hour, had many salt particles in it; but after it had stood 16 hours, the salts were thick, and had such a deep boat-like figure, as formerly mentioned in vinegar; as seen N° 8, fig. A. There were also several salts, having brown longish figures in the middle of them; and some that had two, three, and four circumferences, as fig. B. Some particles had a line or joint running through them. Others were altogether diaphanous, as fig. C. Others had one end sharp, and the other blunt, being not yet perfect. And some again were very diaphanous, as may be seen under the letter D. Some figures appeared as E; and when on looking on the places, where the wine lay thin, and was almost evaporated, I saw a great number of particles, mostly with two sharp ends, and were a thousand million of times smaller than a great sand. I saw also particles of salt swimming about, which had the true shape of a wine vessel; but they were very thin and clear, without any line or joint going through them. There were also several oblong particles, very thin and clear, and very small; though they be drawn large, as at fig. H. Because I was forced to use to F and H a less microscope, than to the other six letters A, B, C, D, E, G. Also when the wine lay thin, there were several branch-like figures, consisting of irregular salts, the shape of many of which could not well be expressed.

I examined Ceronce wine, and found the salts to be mostly, as N° 9, fig. A. Some of which appeared as if rolled up, others were thin and pellucid; and others, when the wine had stood long, were so thick, that they had a brown circumference about them, as fig. B. Several were like the half of A and B, as fig. C.

I examined also Coteau wine, and found several particles, as A, B, C, N° 9. And several whose sides were rolled up, as fig. D. Also flat figures, whose longest sides were straight, and both the ends circular, as fig. E. Also salts having a sharp point, as fig. F. And particles representing a flat-bottomed boat, turned upside down, as fig. G. Others of the same make I could look into, as into a cavity. There were also several very small and long particles, as fig. H, which I imagine, if they had more stuff, would have been as E. There were likewise some salts, like fig. I.

I also observed Tonsain wine, which was very thick and sweet; I found the salts to be the same, as those in Coteau wine, but in less numbers; only with this difference, that in the Tonsain wine several figures had as it were stairs or partitions in them, as fig. 9, letter K.

I took high country wine, of the deepest sort, and found swimming in it very few salt figures, though I let it stand 3 days and nights; but the salts were much larger than in the Coteau and fine Tonsain wine; and had the shape of A, B, D, G, in N° 9.

I took Rhenish wine tartar, beaten very small, and put it in fair rain-water, and when the water was settled, I saw in it many such figures as in the wine itself, viz. some which were very clear, and had two sharp ends, as N° 9, fig. L. But the most of them were very irregular, probably because no sweet or oily matter was mixed with them.

I took the tartar of French wine, and examining it in the same manner as the former, I found some salts, which agreed perfectly with those in the wine; but the rest of them were more irregular than in the tartar of rhenish wine.

I took Orleans wine, pure as it came out of the vessel, and put into every drop a piece of crab's eye, as broad as the knife, and when it had stood 3 hours I could find no such salt in it as I had seen in the wine that had no crabs' eyes. But there were many salts, whose basis were an oblong square, and the sides rose up pyramidal. Other salts were flat, as N° 10, fig. A; others were six sided, as fig. B; others had two slanting sides, as fig. C; some few quadrilaterals had four sided figures within them, as D; others again had the shortest sides something irregular. Some salts were like fig. E. In these last I could perceive no rising, perhaps because they were very small.

I likewise took wine, and put white chalk into it, in the same quantity as before, letting it stand about a quarter of an hour before I observed it, and then I found a great number of the salts; but they were not so large as those in the wine mixed with crabs' eyes; but when this wine and chalk had stood about 12 or 14 hours, I saw the salts not only larger, but the pipes likewise in several places rose from a point of chalk, in great quantities, as above in N° 10, fig. F. These pipes also were larger than the others, though they differed sometimes in size among themselves.

I put into Rinco wine, some pieces of crabs' eyes, and after 12 or 15 minutes, discovered a few salts in it; but when the wine had rested some hours, I found in it a very great number of figures, as N 10, ABCDEF, the salt figures first discovered, were now grown large, though none of them were like the salts in rinco wine, which had no crabs' eyes in it.

P.S. Since the writing of this letter, I have opened a bitch, and found in the womb, or rather in both the tubes, a great quantity of the male seed of a dog, concerning which I shall enlarge in my next.

Description de la Louisiane Nouvellement Decouverte au Sud Ouest de la Nouvelle France; par Louis Hennepin, Missionnaire Recollet, &c. A Par. 1683, 8*vo.* N° 170, p. 980.

The voyage here described was begun under the conduct of M. de la Salle, at the latter end of the year 1678, from the fort of Frontenac, in about 45° north latitude, being the most western plantation of the French in the river of St. Lawrence. It was continued behind most of the territories of the king of England on the continent of America, first through the lake of Frontenac or Ontorio, being 80 leagues long, and 25 or 30 leagues broad; next through the lake of Conty or Erie; then through the lake of Orteney or des Hurons; lastly through the lake Dauphin or Illinois. These last three lakes are near of an equal compass, being about 120 or 130 leagues long, and 40 or 50 leagues broad. At the further end of the lake Dauphin, in about 37° latitude, the voyagers went up a river south, till they came to a certain pass, of about a league and a half over land, leading to the river Illinois, or Segnelay, which, after about 200 leagues, falls into the river Mechacipy (Mississippi) or Colbert; and that, after about 120 or 130 leagues more, runs into the bay of Mexico. These last 130 leagues are taken from the relations of others, and were not travelled by the author; but instead of that, he makes amends with 500 leagues rowed up the river Mechacipy, in a canoe or bark against the stream.

The mouth of the river Seignelay is in about $36\frac{1}{2}$° latitude; the river Colbert there about, is a league broad, and in some places two.

The countries Illinois and Louisiana are described to have large meadows, plenty of trees for ship-building, vines and hemp growing naturally, and also to yield plumbs, cherries, citrons, apples, pears, walnuts, small nuts, gooseberries, Indian, and other wheat, turnips, melons, pumpkins, cabbage, and variety of pulse; to abound in Indian cows, deer, goats, beavers, otters, porcupines, tortoises, also wolves, bears, and wild cats; several sorts of fowls, as swans, turkeys, bustards, herons, crows, ducks, partridges, wild pigeons, parrots, &c. For fish, to have sturgeons, salmons, salmon trouts, pikes, carps, eels, turbots, and several other kinds, not known in our parts. There are also mines of coal, slate, and iron, small pieces of pure copper, a fountain of salt, and alum, and doubtless other things not discoverable by so slight a survey.

An Essay on the great Effects of even, languid, and unheeded Motions. London, 1685. N° 170, p. 982.

The author of this essay (Mr. Boyle) says, it was designed to facilitate the explaining the doctrine of occult qualities; and premises this postulatum in order to the demonstrations that follow, viz. that we are not to consider bodies as so many lumps of matter, that differ only in bulk and shape; but rather as bodies of peculiar internal textures, on account of which they must be considered as engines, whose operations being assisted by the mechanism of the body wrought on, a great part of the effect is due to the action of one part of the body itself that is wrought on, upon another, assisted by the concurrence of the neighbouring bodies. Hence he notices the chief causes on whose account men are used to overlook or undervalue the efficacy of local motions which are either unheeded, or thought languid. The first thing overlooked is the efficacy of the celerity of small bodies moving through a small space; how great this is, he illustrates by considering the powerful effects of bullets; the great incalescence caused by the brisk motion in turning of metals, as steel and brass, the fragments of which are often so heated, as not only to offend the eye-lids, but even to blister the hands of the workmen; vitrification itself being produced by the common striking fire between a flint and steel. Neither are fluid bodies incapable of making impressions on solid ones: witness sun-beams in the focus of a burning glass; the flame of a lamp; and even the air in a good wind-gun. Though we are in the second place too apt to think the softness of fluid bodies, and their insensible motion, may hinder them from those effects. But to show the contrary, besides the obvious instances of deluges and storms, he produces many of the strong operations of sounds upon distant and solid bodies. One of the most remarkable of which is the effect of an instrument, though small, by which an engineer could sink ships in a few minutes; the explosion being so great as to cause a kind of storm in the water round about, and rudely shake vessels that lay at no inconsiderable distance. He observes again that men undervalue the motions of bodies too small to be sensible, though the numerousness enables them to act in swarms: yet how little reason they have for it, he evinces by the operation of the wind in autumn; the solution of sugar in that water where amber, though lighter, sinks and remains entire, by the activity of the flame of spirit of wine; that of the animal spirits in large and bulky animals; the forcible motions produced by glaciation in liquors, &c.

The modification of the invisible motion of fluids, as to what they may perform on the disposed bodies of animals, is as little regarded; though it is not so despicable, if we may believe Scaliger's story of the sound of the bagpipe being too diuretical on a Knight of Gascony; several sorts of noises set the teeth on edge, and a domestic of the author's always bled at his gums, when he heard brown paper torn: add to this, the cure of the tarantula, and two verses of Lucan which seldom fail to put the author almost into the fit of an ague, &c.

Neither is efficacy confined to organical bodies, but extends also to inorganical ones; as is evident from the sympathy of strings; the ringing of a glass to such a note; as likewise some echoes answering only certain sounds; and the like. The mistake likewise is as great when men look upon divers bodies to have their parts in a state of absolute rest, when they are in a state of tension, or compression. Instances of this are the sudden cracking of glasses that seem to be well nealed, the scaling of well heated copper, the brittleness of mixtures of metals, all which and the like probably proceed from contraction. The last main cause why such motions are overlooked, is our being used to the sensible motions of solid bodies, where as many effects proceed from the intestine motions produced in and among the parts of the same body; such as tools being over-heated, and so losing their temper; the breaking of optic glasses in grinding; bodies becoming electrical and odorous by rubbing, and the like; from all which he concludes, that such local motions as are wont to be past by unobserved, may have a notable operation on such bodies as are peculiarly disposed to admit it, and so have a large share in natural productions.

In the discourse on some unheeded causes of the salubrity and insalubrity of the air, he confines himself to the impregnation it receives from subterraneous effluvia. Of these he makes two sorts, some constantly are sent up into the air, which he calls ordinary emissions; others ascend only at times, these are extraordinary emissions; these again are periodical or fortuitous. This doctrine he endeavours to illustrate by asserting, first, that in divers places the salubrity or insalubrity of the air considered in the general, may in good part be due to subterraneous expirations, especially to those he before called ordinary emissions, for this he appeals to experience, which finds some places more healthy than the manifest qualities would permit one to expect; this effect he therefore ascribes to the friendly effluvia from the earth, and argues from the observations in Hungary, and Bohemia, where the air is impregnated with mineral exhalations, suitable to the ore the earth contains under it. He affirms it also probable, that in divers places some endemical diseases do at least in part depend upon subterraneous steams, especially where the cause of the distemper cannot otherwise be accounted for, if together with this we consider the perviousness of human bodies, and the penetrating quality of mineral expirations, of which he gives many experiments.

Trichiasis admodum rara, &c. Lond. 1684. N° 170, p. 986.

This trichiasis, or flux of hair, continued for some years, both at a fistula in the abdomen, and by the anus, in the following manner. This woman having been gone some months with her second child, came to Dieppe, where carelessly going on board a vessel, she hurt her belly against a plank; this bruise was followed by a tumor which ended in a fistula; whence issued a great quantity of hair, attended with much purulent matter, till at length the viscera were corrupted, and the fæces came that way too, so that she died. Anatomy easily discovered the wonder of her sickness; the contiguous viscera were sphacelated; the right appendix of the womb not distinguishable into its known parts, but consisting of one long tumor, covered with a thick skin, which was opened; in it was found an oval bony substance, hairy on the upper part, hollow and rough on the lower; one end of which had some lines of a face; the other seemed designed for the insertion of the vertebræ of the neck. That this body was the source of that succession of hair, is plain from its agreeing with that on the bone; and likewise from the experiment of the author who tells us, the hair continued still to grow, though the bone had been kept a long time in spirit of wine. This conception was in the tuba, for the womb itself was found entire.

Account of the Sugar made from the Juice of the Maple, in Canada.*
N° 171, p. 988.

The savages of Canada, in the season when the sap rises in the maple, make an incision in the tree, by which it runs out; and after they have evaporated 8 pounds of the liquor, there remains 1 pound, as sweet and as much sugar, as that which is got out of the canes; part of the same sugar is sent to be refined at Rouen. The savages have practised this art longer than any, now living among them, can remember. There is made with this sugar a very good syrup of maiden hair, and other capillary plants.

A Periodical Evacuation of Blood, at the End of the Finger. Dec. 22, 1684.
[*In a Letter from Mr. Ash.*] N° 171, p. 989.

Walter Walsh, a temperate man of a sanguine complexion and pleasant

* The acer saccharinum Linn. Dr. Rush of Philadelphia, has given a full account of the method of extracting sugar from the juice of this tree, in the 3d vol. of the American Transactions. From 23 gallons 1 quart of the juice, 4 lbs. 13 oz. of good grained sugar have been obtained. It is asserted, that it is not inferior in quality to that which is made in the West-Indies from the sugar-cane.

humour, was in the 43d year of his age seized with a great pain over all his right arm; a great heat, and redness in his right hand; and a pricking in the point of the fore-finger, on which there appeared a small speck, as if a little thorn had run in; and supposing it such, he opened it, on which the blood spun out in a violent but small stream; after spending its violence, it would cease for a while, and only drop, and then spring out with violence again; continuing thus for 24 hours, till at last he fainted away, and then the blood staunched of itself, and his pains left him: from that time during his whole life, which continued 12 years, he was frequently troubled with like fits; seldom having a respite of 2 months, and they never returned oftener than in 3 weeks: he rarely bled less than a pottle at a time; the oftener the fit came, the less he bled; and the more rarely it assaulted him, he bled the more; whenever they endeavoured to staunch the blood, it raised most exquisite tortures in his arm; no remedies proved in the least effectual: he had no other distemper that troubled him; neither season nor weather affected him: he had no outward accident that at first brought on the bleeding: drinking more than ordinary made him more apt to bleed: these frequent fits brought him at last very low, insomuch, that towards his latter end he bled but little, and that too but like diluted water. He died at last of this distemper.

On the Changes of Weather from the Alterations in the Weight of the Atmosphere, &c. By Dr. Garden of Aberdeen. Communicated to the Phil. Soc. of Oxford, by the Rev. Dr. Middleton, Provost of the King's College in Aberdeen. N° 171, p. 991.

The air agrees with all other fluids in this, that it gravitates; and it has this peculiar property, which is not so much observed in other fluids, that its specific gravity is not always the same. Now, according to the rules of the equilibrium of fluids, every fluid specifically lighter than another, will ascend and emerge above it; and every fluid specifically heavier, will descend and subside below it. Now there is some certain proportion between the specific gravities of the fluid of air, and of that which ascends in vapours, and falls down again into rain; and if this proportion were always the same, it is like we should have no commixture of these fluids, but the vapours would either always float above or always remain below. But this proportion of their specific gravity is frequently changed; for it is known that water when warm is lighter than when it is cold; and yet perhaps the small particles of it, if figured according to Descartes' conjecture, are more capable of ascending in vapours in frost, than at another season, as being more extended, and so having a larger portion of the fluid of air to sustain them; and the daily observations of the different heights of the mercury in the barometer, show that the gravity of the atmosphere is not always the same. And now, from these known properties, may be easily deduced a statical account of the rising of vapours, of their floating in the air in clouds, and of their falling down again in rain.

For if we may be allowed to suppose that when the atmosphere is heaviest, there is some such proportion between its specific gravity, and that of the fluid of vapours, as there is between water and oil; the vapours, according to the known laws of fluids, must needs ascend; and so long as this proportion continues, they must needs float above in the air: but when the atmosphere's gravity is changed, the vapours must descend again.

Now let us see whether this accords with observation and experience. And first as to the ascent of vapours, I do not know any determinate instrument that will indicate their ascent, as certainly as the barometer does the change of the air's weight; for the common hygroscopes are not very exact; and besides, their change by moisture shows rather the falling than the rising of vapours; yet there are two or three observations which seem to be certain indications of their ascent: as first, if the horizon and the remote hills seem smoky and obscure, so that nothing can be seen at any distance, even though the heavens be not cloudy, but clear, and though there be no fog, nor any cap of clouds on the hills, which rather indicate the falling of vapours. Again, if on looking to any distant part of the country round about you, it appear all in an undulating motion, this seems to be a sign of the plentiful rising of vapours, for this is only occasioned by looking through an unequal waving medium, which makes frequent inflections of the beams of light; as you see any object seems to have a tremulous motion in all its parts, when viewed through smoke. Another indication of the ascent of vapours, seems to be the copious rising of steams above waters, marshy grounds, and fens, which is frequently seen in frosts and in cool nights in summer. To these may be added the redness of the sun, so as to be easily looked on; and of the moon, a considerable time before setting, or after their rising. Now since I have had occasion to make observations of the barometer, I have always noticed that when the mercury was rising, and consequently in the increase of the atmosphere's gravity, I have then frequently observed all the foregoing phænomena obscure. But on the contrary, when the mercury has been low, and so the atmosphere's gravity less, I have observed none of these effects; but the remote hills were clear and distinct, and no waving in the air, nor steams from the waters. I know not whether I may add here a conjecture about the great light and the capræ saltantes, the northern lights, which are some nights to be seen in the north. I have taken notice of them usually when the mercury has been high in the barometer, and then appearing in that quarter of the heavens where the sun is at that time below the horizon; this has given me occasion to think that perhaps the steams of vapours may have ascended so far in the atmosphere, as to be beyond the earth's shadow in that part of the horizon, and so, by refracting the beams of light towards us, to occasion that light and those capræ saltantes. It may be considered also whether the red skies in the evening, which denote fair weather, do not proceed from the height of the clouds, occasioned at that time by the increase of the specific gravity of the atmosphere.

Now as to the falling down again of the vapours, it is visible by their gathering into thick and dark clouds, by the falling down of clouds and mists on the tops of hills, and thick fogs in the air, and by their dropping down into rain, snow, &c. and that these do usually happen only when the mercury subsides a little, and consequently when the atmosphere's gravity is less, is the constant observation of those who have had occasion to take notice of the changes of the barometer.

Against this it may be objected, that it is observable many times that even when the mercury in the barometer is rising, there will be rain, and particularly sometimes in north-east winds. To this I answer, that the vapours will fall down in rain, not only when the specific gravity of the atmosphere is lessened, but also if the clouds have been carried for some while towards one quarter of the heavens by the winds, and then if the winds change suddenly into another quarter, these vapours which were formerly scattered in small particles, and so easily floated, are suddenly driven together into little drops, and so must needs fall down in rain, and therefore the falling of rain while the mercury is rising, is observable only on the sudden change of contrary winds.

In the next place, let us consider, whether those frequent commotions in the air, which we call winds, may not be accounted for on the same principles. As to fluids in general, that known definition of Archimedes is universally acknowledged, viz. that the parts less pressed, give way to those that are more pressed; so that if there be any portion of a fluid of a far less pressure and resistance than the rest, the whole fluid runs in a current thither, till all be reduced to an equilibrium. Now as to the fluid of air in particular, it is evident that its pressure is not always the same. And it is very probable that the air's gravity is not alike changed throughout the whole atmosphere in an instant; but that the mercury may have subsided in the barometer, and consequently the air become lighter, at London, for example, when there is no such change observed at Paris or Edinburgh. Now this supposition affords an intelligible account of winds from the known nature of fluids. For when the air becomes specifically lighter in one place, or its pressure is lessened, the neighbouring parts of the atmosphere, whose weight is not thus lessened, run thither in a current, till the atmosphere thereabouts be reduced to an equilibrium again; and according to the portion of air thus changed, and the lessened or acquired degrees of the weight and spring, the currents and winds are strong or weak, and of a long or short continuance.

Now observation and experience agree with this, the mercury being found to subside for the most part in the barometer at the rising of winds; at least it is observed to be in motion, and either rising or falling, and consequently there is a change in the atmosphere's pressure at that time. And thus we have an intelligible and aërostatical account, of the ascent of vapours, of their falling down again into rain, and of the currents and impetuosity of winds, from this known property, the variation of the atmosphere's gravity.

But I acknowledge the great difficulty still remains, how to account for the different changes of the specific gravity of the atmosphere; and of this there can hardly be expected a satisfactory account, till we come to know the cause of gravity in general, and that of the air's weight in particular; and therefore I shall only here offer 2 or 3 hints, which perhaps may incite others to consider it more narrowly. And 1st, it is now almost generally acknowledged, that there must needs be a fluid much more subtile than common air, and of a far greater pressure than it, which is the cause of the continuity and cohesion of all terrestrial bodies, and in which the air seems as it were to float, and to have the same relation to it, as the vapours have to the air; and therefore if we could reach its nature and properties, it might be considered what influence this may have on the change of the air's gravity. Or 2d, seeing the infusion of one liquor into another, in chemical preparations, will alter its specific gravity, so that the bodies which were formerly sustained in it, will fall down and be precipitated, as the particles of gold floating in aqua regis will be precipitated by the infusion of another chemical liquor, it may be considered whether plenty of nitrous steams, or some such mixture, may not alter the air's specific gravity. Or 3dly, we may possibly come to a nearer solution of this, by considering the influence which the heat and cold have upon the spring of the air. The air has this peculiar property, that it is endued with elasticity, as well as gravity; and therefore we are to consider what influence the change of its spring may have on that of its weight; and it seems evident that the increase of its spring diminishes its weight, and the lessening of its spring increases it: for on the increase of the air's spring, the air is rarefied, and so a less portion of it presses on the subjacent fluid; but when it is lessened, the air is condensed, and so a greater portion of it presses on the subjacent body. For example, let us suppose the springy particles of air to be like to the springy hairs of wool, or the spring of a watch; and that many millions of rows of them go to make up the cylinder of air, which from the top of the atmosphere presses on the mercury in the barometer, and keeps it suspended to the height of 30 inches; let us suppose this air rarefied, so that all its springy particles expand themselves, and therefore push off from this cylinder some thousands of those rows; this cylinder, now consisting of a far less number of those rows of particles, must needs have a less pressure on the mercury, so that it will subside perhaps to 29 inches. And thus it continues till the air's spring be weakened, and so the particles be crouded again into narrower room. Now if this be found to hold in the theory, experience seems very well to answer it; for I have hitherto observed that, in cold weather and sharp frosts the mercury rises highest in the barometer, and if the foreign measures agree with ours, it is usually higher here than in France or Italy.

I shall here after all subjoin two or three observations, which may serve to confirm what has been said. The first is of the course of the weather under or near the line. It is said in Purchas's Pilgrims, and elsewhere, that in Brasil and Guiana in America; in Guinea, Congo and Ethiopia in Africa; in the East Indies and the Maldive Islands; they have almost continual floods of rain, from about the beginning of May to the end of August, which they call their winter, and the rest of the months of the year fair and clear weather, which they call their summer; so that when the sun is nearest to them, they have constant rains; and when remotest, fair weather. And this, amongst other causes, may be owing to the extraordinary rarefaction of the air and lessening of its specific gravity there at that time, so that the vapours in the neighbouring parts of the air do all flow thither, and descend as it were in floods of rain. And as this is accounted the cause of the inundation of the Nile, and some other rivers, so perhaps this may be also the reason why those countries which are near them, and somewhat remoter from the line, such as Egypt and the like, have seldom or never any rain. The second observation is on the barometer, viz. that when the wind is north, north-east, or north-west, the mercury ever rises, and so the air is heavier; but when the wind comes from the south, south-east, or south-west, it falls, and so the air's gravity is less; by which we may see what influence the cold and heat have on the air's weight; and a cold wind is said to fill the sails of a ship much more forcibly than a warm. The 3d observation, is on an experiment of Mr. Boyle, as I find it set down in the Philosophical Transactions, N° 63. I made, says he, by distillation a blood red liquor, which chiefly consisted of such saline spirituous particles as may be obtained from the mass of blood in human bodies: this liquor is of such a nature, that if a glass phial about half filled with it be kept well stopped, the red liquor will rest as quietly as any ordinary one, without sending up any smoke. But if the phial be unstopped, so that the external air be admitted, in a quarter of a minute, or less, there will be elevated a copious white smoke, which will not only fill the upper part of the glass, but be plentifully discharged into the open air, till the phial be again stopped. And a little after he adds, if the unstopped phial were placed in our vacuum, it would not emit any visible steams at all, nor so much as appear in the upper part of the glass itself that held the liquor; whereas, on gradually restoring the air, the returning air would presently raise the fumes, first into the vacant part of the phial, whence they would ascend into the capacity of the receiver; and likewise when the air was pumped out, they also accompanied it, and the red spirit, though it remained unstopped, emitted no more fumes, till the new air was let in again: so far Mr. Boyle. Such then was the proportion between the gravity of the vapours of this red liquor and the air, that the air being in its ordinary degree of gravity, these vapours did ascend; but the air's gravity being much lessened in the receiver, by the pumping out a great deal of it, and so expanding the springs of the rest, it was incapable to elevate those vapours.

A Discourse concerning the Air's Gravity, observed in the Barometer, occasioned by that of Dr. Garden; presented to the Phil. Society of Oxford. By the Rev. Dr. Wallis, President of that Society. April 14, 1685. N° 171, p. 1002.

The discourse of Dr. Garden, read at our last meeting, concerning the different state of the air, in respect of its different gravity, has in it a great deal of very ingenious speculation. And what I then said of it, on the first reading, and what I am now saying again to the same purpose, is not to contradict it, or detract from it, but to add to it; as a notion which I have long since considered, and judge it capable of further improvement.

The notion of the air's weight and spring has been so well settled, by innumerable experiments, that hardly any considering person now doubts of it. And it has chased away the notion of fuga vacui, formerly received, by showing us an efficient cause of those effects, for which before we could only pretend to a final cause. The first occasion, that we know of, of introducing it, was from Galileo's discovery, that water by pumping, was not to be raised higher than about 3 or 4 and 30 feet of our English measure. Which was a certain argument, that the cause of those effects, commonly ascribed to fuga vacui, was but of a finite strength: whereas, if nature's shunning a vacuity had been the true cause, it was to have operated without limit. Upon this, that Lyncean philosopher, as he was called out of his great sagacity, happily guessed at the counterbalance of the air's weight, as the true cause. And that therefore air, which was before thought to be a light body, was but comparatively so, and had indeed a positive gravity, though less than that of other bodies, which we are conversant with.

This notion was happily pursued by Torricellio, a successor of Galileo, who rationally argued, that if the air's counterpoise was sufficient to raise and sustain water at that height, and only to that height; then must it be a just counterpoise to a lighter liquor of a greater height; but to a heavier liquor of a less height. And making a trial in quicksilver, he found it to succeed accordingly; and in a just proportion to the respective gravities of those fluids. And he has by this means made the experiment, commonly called the Torricellian experiment, much more manageable with quicksilver, in tubes of about 33 inches English measure, than before with water, in much longer tubes.

In pursuance of this notion, we find by several sorts of baroscopes, not only that the air has gravity, but that it has a different gravity at different times and places, according as its counterpoise is capable of sustaining quicksilver at different heights; sometimes a little lower than 28 inches, sometimes a little higher than 30 inches, and at other times at some middle height between these. Which different weight of the air may reasonably be supposed, partly to proceed from, and partly, as Dr. Garden properly intimates, to cause the difference in the weather and winds.

That there is in our air a body, more subtile than the fumes and vapours mixed with it in our lower region, and which with it make up that heterogeneous mixture which we commonly call air, seems to be very certain. But whether that subtile body be, as Dr. Garden seems to suppose, much heavier than our common air, I much doubt; and rather think it is not, not having hitherto had any cogent experiment, either to prove it heavy or elastic. But it may, for ought I know, be void as well of weight as spring; and what is found of either in our common air, may be attributed to the other mixtures in it.

The air being then of a different gravity at different times and places, it may therefore be considered as a fluid, whose parts are in some places heavier, and others lighter: and therefore much of a like nature as if they were different fluids, of different specific intensive gravity one from the other. Now when several fluids, or several parts of a fluid, are thus of different weights; they will, from the general nature of heavy fluids, when undisturbed, change places with each other, till the heavier becomes lowest, and the lighter uppermost. And this not only as to the minuter parts, as the sinking of sand, &c. but much more as to larger parcels; as when oil, wine, water, beer, or other the like liquors, are put together in the same vessel. And the same must happen, if some parts of the same liquor do accidentally acquire, by expansion or otherwise, a greater degree of lightness than the other parts, those lightened parts ascending, and the heavier subsiding; as when water, beer, or other thin fluids are gradually heated by a fire underneath; the lower parts being first warmed, ascending to the top, while the colder and heavier subside; whence we find in such cases, that bubbles arise, and the liquor at the top is warmer than that at the bottom. But in case what is warmed be of a thicker consistence, so that the parts cannot readily shift places, that at the bottom will be hotter.

From such considerations as these, Dr. Garden well observes, that some parts of the air being thus, by rarefaction, or increasing its spring, or otherwise, become lighter than others; these heavier parts rushing into the places of those lighter, may cause a wind as from such parts. And this I take to be very true, though such accidents happening very variously and uncertainly, will cause such confusion of motions, and disturbance of each other, that it will be hard to reduce them to a regular adjustment. But I add thereto, that the earth's diurnal motion, compounded with its annual, the one in some parts accelerating, in others retarding the other; and its difference in different times of the year, by reason of the obliquity of the zodiac to the equinoctial, and in different times of the month, because of the moon's different position, which is an appendance to the earth's motion, and thereby differently affects it, and according to the different place of the earth and moon, as to the aphelion or perihelion of the one, and the apogæum or perigæum of the other, seem to me greatly to influence, not only the tides, but the winds also; especially the breezes and trade-winds, which at certain times of the day or month, or of the year, are observed to blow constantly, or most frequently, from a particular point. And I am not sure that the body of earth and water, or terraqueous globe, is exactly spherical, allowing only for the small inequalities of hills and dales, which in a body of that magnitude are inconsiderable, but may have somewhat of an oblong spheroid, having a longer axis from pole to pole, then at the equator. And though this cannot be much, because of the earth's shadow in the moon's eclipse appearing circular, and the descent of heavy bodies being always, as to sense in a perpendicular to the horizon; yet, if it be but little, this, with the compound motions before-mentioned, will give the air a considerable disturbance. To which I may add also, that we are not sure that the seas and continents are so adequately adjusted to each other, as that its centre of gravity coincides with its centre of magnitude, by which the confusions of the air's motions would be yet greater.

From the comparative weight or lightness of the air at different times, Dr. Garden deduces also the rising or falling of vapours in it; and this is certainly to be admitted also. Only I add, that these static principles chiefly take place, when things are in other respects at rest; but when they are in commotion, it is often otherwise. And in such cases, we must, beside the respective gravity, take into consideration the force, impulse, or impetus, superadded to the respective gravity of the parts or matter. Thus, in a jetty of water, the water is thrown up into the air to a great height, not because it so becomes lighter than the air, but on account of the impressed force. And this I take to be the cause of fumes, vapours, and other like matters, which ascend in the air, not that they are lighter than it, but because impelled upward from the bowels of the earth, or from the superficial parts of it by an external force. And that there are such fumes, &c. projected upwards, from the bowels of the earth, and some of them with great violence, is undeniable, from the instance of earthquakes and other eruptions. And to such causes I principally attribute the origin of winds, and the ascent of most other things into the air.

There is yet another motion suggested, which is also very considerable as to this affair; which is, the weakening or strengthening the spring of the air. And that this spring of the air is sometimes stronger, and sometimes weaker, I think is undoubted also: and that the spring of the air is strengthened both by compression and by heat, but in a different manner. For, if the same quantity of air be compressed into a less room, the spring is certainly stronger; as is plainly seen in the wind-gun, and other compressing engines. Again, the same quantity of air included in a close vessel, will, by application of heat, have its spring strengthened; as is observable in thermometers of all sorts. If the spring be strengthened by compression, it is manifest that the intensive gravity must be thereby increased, because the same quantity of air, and consequently of weight, extensively taken, is now contracted into less room, which therefore must be intensively heavier, as being the same weight in a less bulk; now this may possibly, as a greater pressure or stronger spring, force up the vapours under it with a greater impetus, and so make them fly higher, but not so as to make them lighter, but rather the contrary, as pressing them closer together, much less to make them specifically lighter than the air itself.

If the spring be strengthened the other way, as by heat, this rather diminishes its intensive gravity by thrusting its parts further asunder, and so possessing a larger space. Now in case this air be, by a close vessel, so confined as not to expand upward, it will certainly press the stronger on the stagnant quicksilver below, and make that in the tube rise higher. But in case it be unconfined, as in the open air, it may as well expand itself upward, by making the atmosphere in this part so much higher. Nor is there any necessity, as to the subjacent parts, that the atmosphere should be every where of the same height. For the laws of statics, as to the subjacent parts, may be equally preserved without it, the greater altitude compensating for the levity of the parts, as when a portion of the sea is covered with a fleet of ships, the under parts are equally pressed, partly by water and partly by ships, though the tops of the ships over some parts be higher than the surface of the water over others. Only in such case the upper part of the atmosphere being fluid, may flow collaterally over the other parts on either side, if lower; and so at leisure reduce itself to an equal height in all parts.

But though the spring of the air, increased by heat, may thus expand itself upward, yet, because it presses every way, it must act in like manner downward also, and thereby press harder on what is under it; and because it will require time to work upward gradually, before the effect reach the top of the atmosphere; and because by such dilatation of its parts more room is left in the intervals to receive what is forced; it is reasonable to believe that in such cases, the pressed vapours, cæteris paribus, may rise more copiously than when the spring of the air, for want of heat, is less strong; the rather, because the same heat, which thus strengthens the spring of the air, also rarefies the vapours, and makes them lighter, and may also increase the subterraneous heat, or whatever else it is, that drives them upward. Notwithstanding all which, we have more rains in winter: which should argue, that more vapours do then rise to supply them.

But I suspect that, in this whole business of strengthening the spring, we may be under some mistake; and what we think to be produced on the open air, is indeed effected upon the quicksilver, or rather on the air latent therein. My meaning is this, we find that in very hot weather, and also in frosty weather, the quicksilver in the tube commonly stands very high, from whence we are apt to conclude, that therefore the outward air presses very hard on the stagnant quicksilver, without the tube. But in this I am not satisfied. For we are apt to consider, that in filling the tube with quicksilver before it is inverted, if great care be not used to cleanse it from air, many aërial particles will remain mixed with it, which, while their spring is weak, are easily pressed by the weight of the quicksilver, so close as hardly to be discerned otherwise than by the effect; but when, by the external heat, their spring is strengthened, they expand themselves, and cause the quicksilver to swell in bulk, without increasing its weight; and consequently to stand higher though not to press heavier. And the same solution perhaps may serve for its standing so high in frosty weather; for water, we know, though it contract with cold, yet when it freezes it expands itself; which makes ice lighter than water, and to swim on the top of it. Now whether this be purely of itself, or in part at least, from the particles of air lodged in it, may not perhaps be so easy to determine. However, if there be the like effects on air as on water, namely, that it expands with freezing, or if in the quicksilver there be lodged particles of water as well as of air; we have in either way an account of this phenomenon. For then the small particles, whether of air or water, lodged in the quicksilver, being thus expanded by freezing, will make the quicksilver swell and so stand higher, without increasing its weight, and consequently without arguing a greater weight of external air pressing on the stagnant quicksilver.

Description of a Stone of the Bladder, shown to the Royal Society, Feb. 25, 1684-5. N° 171, p. 1015.

This stone is said to have been taken out of the bladder of one Francis Dugood, of Auchenhove, in Aberdeen, who died at 50 years of age. The length of the stone was $5\frac{9}{10}$ inches, the diameter $3\frac{6}{10}$, the weight 2 lb. 3 oz. 6 dr.

Account of an Aqueduct designed for carrying the River Eure to Versailles. N° 171, p. 1016.

The aqueduct intended to be made near Maintenon, for conveying the river Eure to Versailles, will be in length 7000 fathoms, of which 462 will be 35 fathoms, 4 feet high; the rest will be lower according to the difference of the ground, but not less than 5 feet and 6 inches high. There will be to the said aqueduct 861 arches; which, where they are highest, will be 12 fathoms in breadth, and 8 fathoms in thickness, diminishing to 14 feet at the top. The other arches will be less in breadth as well as thickness, according to the nature of the ground. The said aqueduct will have 15 inches fall to every 1000 fathoms in length, so that for the 7000 fathoms there will be 8 feet 8 inches fall. There are 14000 soldiers that work at it, under the command of the Marquis d' Uxello, with 3 commissaries of war for their conduct, together with treasurers, pay-masters, and victuallers.

Account of an old Earthen Vessel, lately found near York. N° 171, p. 1017.

This earthen vessel, supposed by some to be an urn, by others to be a flower-pot, was found lately at the brick-kilns without Bowthant Bar, near York. The clay of it is of the colour of Halifax clay when burnt. On the middle of the side is a face of the size of a woman's, well performed, the face being bossed from within with the finger, when upon the wheel; having some streaks of red paint about the curls of the hair and eye-brows, and two red threads about the neck. On the back part of the vessel a leaf is drawn in red, which is still very fresh, but no glazing, neither on the clay nor red colour. The vessel is preserved in the Ashmolean Museum at Oxford.

Account of a Shell found in one of the Kidneys of a Woman. By Dr. Peirce, of Bath.* N° 171, p. 1018.

A gentlewoman of about 28 years of age, very fat and corpulent, after having been long troubled with frequent and sometimes violent vomitings, fell at length into a fever, which had no very ill symptoms at first, yet in a few days she died suddenly. For the satisfaction of her relations the body was opened; the lower region being first examined, I quickly found what might account for her long vomiting, and perhaps her fever and death too, viz. an ulcer in the pancreas, which had sphacelated some part of the stomach and bowels. Examining the state of the kidneys, they were found covered with a prodigious quantity of fat; which removing with my hand, and reaching one of the kidneys, I felt something prick my finger in the lower part, where the ureter is inserted: I presently concluded it to be a stone, and kept hold of it till I made my way to it with my knife, and took it out, with an abundance of mucous bloody matter about it. Opening the kidneys, I found not so much as gravel, much less any stone, in either of them: on further examination of this matter, supposed to be a stone, by washing off the mucus from about it, I found it to be a small shell, very finely wrought; in the hollow of it there was a mucous matter, not at all unlike the substance of a snail as to consistence;† but of a bloody colour.

Fig. 1, pl. 5, represents the shell in its true size. Fig. 2, shows the same shell somewhat magnified. Those indented checkers on it, are alternately a little depressed and raised, and are very exactly wrought. There are 6 or 7 spiral lines or rounds, in the turban.

Bath, April 11th, 1685.

Gualteri Charltoni‡ Inquisitio Physica de Causis Catameniorum, et Uteri Rheumatismo. Lond. 1685. 8*vo.* N° 171, p. 1020.

A treatise on the human uterus, its functions, diseases, &c. It is far from the best of Dr. Charlton's works.

* A calculus resembling a shell.

† If the patient had lived longer this "mucous matter, not unlike the substance of a snail," would probably have concreted to a stony hardness like the outer part, or supposed testaceous covering.

‡ Walther Charleton was a physician of considerable renown in the 17th century. He was a native of Somersetshire, and studied at Oxford, where he took his doctor's degree in 1642. He was an early member of the Royal Society, and was physician to Charles I. and II. He practised for many years in London, but at length retired to the Isle of Jersey. He died 1707. To his medical and physiological writings belong, in addition to the treatise abovementioned, the following: Spiritus Gorgoneus sive de causis, signis et sanatione lithiaseos; Exercitationes Pathologicæ; Anatomical Lectures; Tractatus de Scorbuto; and, the most celebrated of all, his Œconomia animalis.

Davidis Abercrombii, M. D. de Variatione ac Varietate Pulsus Observationes. Accessit ejusdem Authoris nova Medicinæ tum Speculativæ tum Practicæ Clavis, &c. Lond. 1685. 8*vo.* N° 171, p. 1023.

The author in this tract, among the various opinions of the cause and origin of the pulse, thinks it most probably proceeds from the joint motion of the spirits, arteries, and muscles.* And as to the variation of it, he assigns these for most certain causes, viz. The climate, season, temperament, age, diet, passion, disease, &c.

The climate alters the pulse. Hence the Frenchman's pulse is more equal and quick; the German's, Dutch, English, and Scot's, more uncertain;† which yet is something to be attributed to their irregular living; in general, the higher and nearer the sun is, the quicker; the lower and farther off, the slower is the pulse. And, he thinks, for the most part the systole is more quick than the diastole. In rainy seasons the pulse is more free and nimble, by reason of the less pressure of the atmosphere; it is more impetuous in the spring; more equal after a quiet sleep; weak and uncertain in men very intent upon business, &c. The temperament results from the animal spirits, and the contractive or dilative motion of the muscles and arteries, to which their fibres are even by nature disposed. Melancholy renders the pulse extremely inconstant, probably through the great thoughtfulness of such men. In bilious tempers it is high and strong; in the sanguine more equal and regular than in any; in the phlegmatic equal enough too, but more slow. In children, especially infants, the pulse is very small, but through the great quantity of lymph, as it were drowning and dulling the action of the spirits. In old men extremely uncertain. In gluttonous people, dull and slow, unless by drinking it be made as it were stumbling and vertiginous, which often foreruns sudden deaths. By too sparing a diet it becomes very small and slow, always abates upon long fasting. Of the passions, it is most altered by fear, joy, and anger; to which women are more subject than men. In fevers the pulse is varied according to the beginning, height, and declination. In scorbutic and hysteric persons, very uncertain. In icteric and hydropic, much stopped and interrupted by the stagnant humours. In the gout, free and expedite. In the plague, as in the asthma, mightily oppressed, unless freed by the hot fit.

* The pulse is occasioned by the heart driving the blood with impetus during its systole into the arteries. It consists therefore in a dilatation of the arteries, accompanied perhaps with some degree of displacement, in consequence of the suddenness and force with which such dilatation is made.

† These are mere assertions. In the whole of these remarks the author has given full scope to his imagination.

In general, any variation of the pulse certainly shows some alteration in the habit of body.

The pulse is unequal, either in respect of time or strength, that is, either it strikes quicker and slower, or else stronger and weaker. The first commonly in most acute distempers, and seldom betokens much danger. The latter both in chronical and acute is very dangerous. And often, sometimes 2 or 3 days or more, precedes death. It is interrupted when its strokes are much smaller than usual, or their intervals much greater. The first shows a great decay of strength. The latter, which is as it were a standing-still, foreruns swooning, palsies, apoplexies, &c. and sometimes death itself. The intense pulse is that whose stroke is very hard, or else this strength is made up by the frequency of less mications, as in the height of fevers. The remiss has strokes less quick or less strong, and in sickness shows more danger than the other. The superficial pulse shows an exact temperament of body, as also a free and merry temper of mind. The deep pulse shows a disposition to melancholy, asthmas, lethargies, &c. and is more frequent in the aged than the young. The leaping pulse often portends no great danger. The trembling shows great extremity, and very few ever recover after it. But the wandering pulse, which sometimes is felt at one place, sometimes at another, and sometimes no where, never but some few minutes precedes dissolution, which yet may perhaps from volatile spirits sometimes receive a short reprieve, but never a perfect restitution.*

The design of the other tract is to teach how to discern the virtue and quality of any plant or other body, without the particular knowledge of the species or name of it, only from the taste; which he says is either sour, as the sharp leaved dock or olus sylvestre: harsh, as the medlar: austere or rough, as the quince: sweet, as the fresh juice of ripe grapes: fat and oily, as the sesamum: bitter, as the wild cucumber: salt, as common salt: tart, as garlick: or lastly, insipid, as the gourd. All which sorts he treats of in particular.

Part of a Letter from Sir R. B. to Dr. L. concerning a new Sort of Calesh. N° 172, p. 1028.

Sir William Petty, Mr. Molyneux and I have spent this day in making experiments, with a new invented calesh, along with the inventor of it. It is in all points different from any machine I have ever seen: it goes on two wheels; carries one person; is light enough. Though it hangs not on braces, yet it is easier than the common coach, on all sorts of roads. A common coach will

* These distinctions of the pulse being for the most part fanciful, very little ues can be derived from them in practice.

overturn, if one wheel go on a superficies a foot and a half higher than that of the other, but this will admit of the difference of 3⅓ feet in height of the superficies, without danger of overturning, &c. No particular description however is given of this vehicle.

Account of a strange Sort of Bees in the West-Indies, communicated by M. I. N° 172, p. 1030.

M. de Villermont informs me, that he has received from Cayenne in America, a sort of honey comb, of a different make from the European, which is composed of small bottles, or bladders of wax, of a brownish colour, inclining to black; being as large, and shaped liked the Spanish olives. They hang together in clusters, almost like a bunch of grapes, and are so contrived, that each of them has an aperture during the time of work, but it is closed up, as soon as the vessel of wax is filled with honey; after which the bees go to work on another vessel.

They ordinarily lodge in a hollow tree, or the cavity of a rock, by the sea side; these being the fittest places to secure them from such animals as are greedy of their honey, and therefore likely to molest them; and they have the more need of this caution, because they are more liable to be disturbed than ordinary bees, as having no stings, and being incapable of hurting any thing. When the combs are removed, they must be carried gently, and in the same position they lay in.

The honey itself is as clear and liquid as rock-water, and hardly to be distinguished from it by the sight. To take it out, you must pierce every bottle with the thorn of a wild palm, or a pin, a little more than ⅓ from the bottom; for if you pierce it lower, there is a sediment, whose thickness hinders its running. This honey is one of the most agreeable liquors in the world. If a large glass, or about half a pint be drunk fasting, it will give 2 or 3 stools, in about 2 hours time, according to the temperament of the party; but if drank at meals, it does not purge at all. A group of these honey-bottles is represented, fig. 3, pl. 5.

Ambergris* is asserted to be but the wax, mixed with the honey, which falls into the sea, and is beaten about in the waves, between the tropics.

A New Hygroscope. By William Molyneux, Esq. Sec. of the Dublin Society. N° 172, p. 1032.

In fig. 4, pl. 5, AB is a whipcord, about 4 feet long, tied fast to the end of

* What ambergris is, has been shown at p. 94, vol. ii. of this Abridgment.

a hook A. At the end of this whipcord hangs the weight C, about a pound or something more; this weight is so fitted at the end as to receive and carry the index D; under these is placed a graduated circle on the board EF, fixed by a bracket against the wall. All things being thus adapted, the moisture of the air twists the cord, and gives a motion to the index over the divisions in the graduated circle; and again, as the air grows more dry, the cord untwists, and brings back the index by a contrary motion.

What first gave me the idea of this, was the observing all cords tied at both ends to be much more tight and stretched harder after rain has fallen on them, than before; I concluded that if I could, as it were, tie a cord at both ends, and yet give one end a liberty of circumvolution, it would perform the desired effect. Now the weight C hung at the cord does this; for it fixes, as it were, the end of the cord B, and yet it permits it to twist and untwist. And the reason of this is plain; for the little particles of moisture, insinuating and soaking into the cord, are like so many wedges, which must needs shorten it, as a bladder is shortened by being blown up, and will lift a great weight. But the easiest way for the cord AB to shorten, and raise up the weight C, is to do it as a screw; for itself is a screw, its strands being twisted, and each particular thread in it screw-wise, and consequently must give a circular motion to the index.

To make an experiment of this, I wetted a cord and hung it up with the weight at the end of it, and I perceived as it dried it untwisted, and that too very quick, so as to be perceived by the eye; after the cord had so far untwisted as I thought it had come to that degree of dryness that the present constitution of the air would permit, I took a basin of warm water, which emitted a steam and fume, and placed it under the cord; immediately the cord began to twist again very quick, and so continued till the water ceased fuming or was removed, and then immediately it began to untwist again. I then breathed on it gently with my breath, and found that 8 or 10 breathings would twist it 5 degrees of a circle. I then left it to the air only, and for these last three weeks have observed its motion as affected by its moisture and drought, and obeying its alterations very nicely, not the least shower falling, at which it does not presently twist; and when by rising clouds a fair day becomes overshadowed, the cord is immediately sensible thereof, and again as sensible of their vanishing and change to fair sunshine. So that I have seen it show alteration, when not the least could be collecting from the sweating of stones, cracking of wainscot, &c. And hence it seems to be the nicest hygrometer, that has ever yet been used.

Even by applying a candle or heated iron near the cord, it makes it twist very quick, contrary to Mr. Hook's oat-beard.

One of the grand defects of most former hygrometers is, that they grow weak with age, and do not so nicely obey the alterations of the air, when long kept, as when first made. Planks and boards grow more seasoned, and probably oat-beards will perish with time; but whether our present invention be subject to the same fault must be left to time to determine.

I observe that the alterations of the air may give this kind of hygroscope more than one turn; now this being inconvenient, and the duplication of the turn hard to be registered, as Mr. Hook proposes in his Micrography, concerning the beard of a wild oat, I have thought of the following way for remedying this.

The index D has, suppose, two complete turns, but the point A, being fixed, has no turn or motion at all, therefore the middle point G has but one turn; and consequently if I hang it up at the point G, or no longer than GD, half the former length, the index D will have but one turn. What is here said of two turns, and the middle point G, may be accommodated to any other number of turns and parts, and the corresponding points in the cord.

On the French Macreuse and the Scotch Bernacle; with a Continuation of the Account of Boyling and other Fountains. By Dr. Tancred Robinson, F.R.S. N° 172, p. 1036.

There are so many mistakes among naturalists and some learned men, concerning the bird at Paris called Macreuse, and in other parts of France, Macroul, or Diable de Mer, that it may be no improper subject of inquiry. The French eat it on fish days, and all Lent, accounting it a sort of fish, or a sea animal with cold blood, or else a bernacle generated either out of rotten wood floating on the sea, or out of certain fruits falling into the water, and there transformed into a bird; or else from a kind of sea-shells, adhering to old planks and ship-bottoms, called Conchæ Anatiferæ; whereas the bernacle, as also the macreuse itself, is oviparous, and of the goose kind; and the shells themselves contain a testaceous animal of their own species, as the oyster, cockle, and muscle do. Gesner was led into the first error by Gyraldus, Boethius, and Turner; Sir Robert Moray fell into the third and last mistake; Sir Robert Sybbald and M. Graindorge have indeed confuted these equivocal generations of the bernacle and macreuse; yet they both make them to be the same bird; whereas they are of different tribes, the bernacle of the goose, and the macreuse of the duck kind.* That the bernacle and macreuse are both oviparous is beyond all doubt, the anatomy of their parts serving for generation, their laying eggs, and some-

* The bernacle is the *anas erythropus* of Linnæus, and the macreuse is the *anas nigra* of that author.

times breeding among us, are all evident proofs thereof. M. Cattier, in his Traite de la Macreuse, affirms that the French macreuse is the greater coot of Bellonius; and Mr. Willughby, Ornitholog. p. 320, seems to be of the same opinon. Some learned men take it for the puffin of the Scillies and Isle of Man; others for a sort of colymbus or mergus, ducker or diver. But the French macreuse is of the duck kind, and is the scoter, or anas nigra minor described by Mr. Ray in Mr. Willughby's Ornitholog. p. 336.

The macreuse is frequently taken in nets placed under water, on the coasts of Normandy, Languedoc, and Provence; and I have seen it on the Laguna of Venice, at the mouths of the Breuta, Addesis, and the Po. A duck very like unto this, if not the same, I saw on the Mare Mortuum, and the lake Avernus, as also many other water-fowl feeding upon and flying over that water, though said by many to kill birds at a distance; I observed also several land fowl to fly over that lake without the least disturbance. But perhaps the poisonous steams, if there be any peculiar to that lake, sometimes vanish and return again, or else may be altered by new effluvia intermingled with them.

When I delivered my thoughts concerning boiling fountains, their varieties and causes, I had not then time enough to mention the burning ones, except only that near Wigan in Lancashire, with which those burning fountains, near Grenoble in Dauphine, near Cibinium or Hermanstadt in Transylvania, near Chermay, a village in Switzerland, in the Canton of Friburgh, and that not far from Cracovia in Poland, do agree in many particulars; as, in being actually cold, yet inflammable and taking fire at a distance, on the application of any lighted body; which the boiling springs near Peroul will not do; this ought to be understood of them in their sources, because when removed from thence, neither the waters, nor their earths will produce any such phænomena, as boiling or flaming.

It is related of the burning fountain in the palatinate of Cracow, that on evaporating the water, a dark or pitch-like substance may be extracted, which cures the most inveterate ulcers in a very short time; and that the mud itself is very powerful against rheumatic and gouty pains, palsies, scabs, &c. The inhabitants of an adjacent village, drinking much of this spring, do generally live to 100 or 150 years, which is attributed to the sanative virtue of the water.

On the French Macreuse. In a Letter from Mr. Ray, F. R. S. to Dr. Robinson. N° 172, p. 1041.

I had no sooner seen the cases of the male and female macreuse, which you sent me, but instantly I found that the macreuse was no stranger to me, though unknown by that name: I was very much pleased to be so suddenly rid of my long continued scruples about it, and not a little surprised when I found it to be another kind of bird than I imagined. A particular description of the cock, you may find in Mr. Willughby's Ornithology, page 366, of the English edition, among the sea-ducks, to which kind this bird belongs, and not to the divers or duckers, (Mergi or Colymbi,) as I falsely fancied to myself. The first knowledge of this bird we had from Mr. Jessop, who sent us the skins of this among others stuffed, from Sheffield in Yorkshire, by the name of Scoter, as it seems they call it thereabouts. Afterwards the cock of this kind was found in the market at Chester, by the then Bishop Wilkins's steward, who bought it; and brought it home to the palace, where I then happened to be, and saw and described it. Lastly, Sir Thomas Brown of Norwich sent, among many others, the picture of this duck; and Mr. Johnson of Brignal, near Greta-Bridge in Yorkshire, the description as I have related in the book, and page before quoted.

I observed in this bird, and in some others of the sea-ducks that are much under water, that they want that vessel, or ampulla, situate in the very angle of the divarication of the wind-pipe, which for want of a better and fitter name, we are wont to call the labyrinth of the trachea; which, though being common also to the colymbi, which of all birds dive most and continue longest under water, we may very probably from thence conclude, that the labyrinth does not serve them for a reservatory of air, to enable them to continue the longer under water, as I sometimes conjectured; but for the intending and modulating of the voice, seeing in the plash-duck the females want it: but I am somewhat to seek about the use of this vessel, and I think it were worth while to examine what sorts of birds have it, and what want it; whether the males only, or in some the females also. I observed it in the mergus cirratus longiroster major or the dun-diver, and that very large, and extended by very strong bones, and yet I thought myself to have sufficient reason to judge that bird to be the female of the merganser: but I dare not be confident that it is a female, because of this labyrinth.

And now because I am writing of birds, I propose it to your consideration, whether that sort of bird mentioned by the learned Dr. Plott to be often heard in Woodstock Park, and from the noise it commonly makes called the wood-cracker,* be not the lesser sort of picus martius varius; for since the publishing of Mr. Willughby's Ornithology, I have observed that bird sitting on the top of an oaken tree, making with her bill such a cracking or snapping noise,

* It is generally supposed to be the nuthatch or *sitta europœa*. Linn. which feeds in a similar manner with the woodpeckers.

as we heard a great way off; the several snaps or cracks succeeding each other with that extraordinary swiftness, that we could not but wonder at it: but how she made the noise, whether by the nimble agitation of her bill too and fro in a rift of the bough, or by the swift striking of the mandibles one against another, as the stork does, I could not clearly discern: but an intelligent gentleman, who was very diligent in observing the same bird, said it was the former way.

*Epistola Cl. Viri D^{ni} Gulielmi Cole, M. D. ad Honoratissimum D^{um} D^{um} Rob. Boyle, Armig. de Falsa Graviditate.** N° 172, p. 1045.

Matrona quædam, mente omnino constans, pia, fide dignissima, et circa prægnantes et puerperas olim versatissima, 79^{num} annum nunc agens, se fætum utero gestare jam diu credidit, creditque adhuc, imo quod magis mirabere (forsan et risu excipies,) per totos septem elapsos annos gestasse. Dum illac negotiorum causa quadriennio abhinc iter facerem, et à quibusdam, ipsa non minus credulis, rei famam acceperam, novitate permotus ad hoc miraculum nulla mora contendi, ut tam insolita scena oculos pascerem. Inveni ventrem multum tumentem, non autem, qualis esse solet hydropicorum tumor apparuit, sed sursum, more gravidarum, eminebat. Et dum plura sciscitarer, nec illam (ut nec ejus maritum, qui decennio minor uxore fuit) de ingravidatione amplius dubitantem comperissem, petii ut tantæ fiduciæ causam exponeret.

Illa haud gravate respondit, se olim decem liberos enixam, nec ab eo tempore per 28 annorum spatium mensium fluxum passam, in eum tandem satis copiosum incidisse, ex quo brevi post omnia conceptus signa apparuerunt; inde nauseam, et vomitiones subinde recurrentes, necnon et inordinatam quorundam præ cæteris ciborum appetentiam, ut prægnantium mos est, invasisse, et per plures menses perstitisse, ventre paulatim intumescente; postea solito tempore primos fætus motus se percepisse, et exinde, tumore indies aucto, motus etiam tanquam ejusdem locum sæpe variantis, nunc ab una nunc ab altera ventris parte, qui et tractu temporis invalescebant, sensisse; tandem (appellente usitato partus tempore) ipsos parturientis labores subiisse, ut obstetricem accersere necesse habuerit. Sed non adfuit Lucina; attamen licet dolores illi evanuerint, haud detumuit venter; quin sæpius præ dolorum recursu obstetrix (quæ et ipsa, prout à nonnullis accepi, in eandem cum domina sua sententiam propenderat) rursus accita est. Ab eo tempore motum illum, sed vegetiorem se persensisse asseruit, adeo ut vestes frequenter attolli ab adstantibus conspectæ fuerint, tumore, licet aliquantulum, haud tamen impense

* Reprinted in the original Latin, many circumstances in the narrative not being suited to appear in a translation.

aucto. Mammæ, quas et vidi et attrectavi, minime, prout vetulis solenne est, flaccidæ, sed amplæ et distentæ (at non supra modum) glandulisque, more prægnantium, distinctæ. Mihi quinetiam sponte affirmavit, obstetricem sibi asseruisse orificium uteri internum æque tenerum et molle fuisse, ac in quavis fæmina mox paritura. Cum porro percontarer utrum inter decumbendum, cum à latere ad latus se reclinaret, pondus ab uno in alterum devolvi perciperet prorsus negavit, meque de mola nil suspicari jussit; cum illam peritius dignosceret, quam ut hac in parte decipi posset.

Cis paucos dies illam revisi, eademque, ut retuli, denuo narrantem audivi, mammasque in eodem statu comperi, ventrem vero aliquanto magis intumuisse, de cujus tensione plurimum conquesta est. Motus autem magis quam antehac, vegetos se tum percipere dixit; atque ego, manu supra vestes admota, bis, dum pauculam illic moram facerem, ejusmodi motum nunc ex una nunc altera ventris parte sensi, qualem in vere prægnantibus observasse memini. Toto hoc gestationis (si ad ipsius mentem loqui liceat,) tempore nullum, saltem alicujus momenti, sanitatis dispendium passam se profitetur, nullis symptomatis laborasse, nisi quæ gravidis sunt familiaria, et quæ etiam, dum puerpera fuit, perpeti solebat. Cibos satis recte appetit, optimeque digerit: minime siticulosa est, prout hydropicis usui est, urinamque ad liquidorum assumptorum mensuram proportionatam reddit, ad morem autem gravidarum solito frequentius. Ædes hortumque satis valenter obambulat, nec baculi fulcimento indiget. Moderate dormit, sed petulca ventris sarcina matri suæ vix ultra diluculum quietem indulget, verum calcitratu cogit è lecto surgere; quo facto, et cibis assumptis, illa se iterum somno parat, saltem mitius sævit. Quoad habitum corporis carnosa est, vultusque nil morbidi (me judice,) intus latentis præ se fert. Nullum tibiarum pedumve toto decursu unquam passa est tumorem, nec vulgatum ullum hydropis cujuscunque, sive universalis, sive ipsius uteri, indicium, præter solum, quem dixi, abdominis tumorem colligere potui. Nec tamen fœtum utero includi quisquam, nisi qui famæ suæ prodigus est, asseruerit; cum et ætas, et temporis à prima affectus invasione decursi longitudo (quorum neutrum cuiquam accidisse, præterquam quod Saræ pro miraculo obtigisse Sacra Pagina testatur, ulla, quod sciam, historia fide digna propalavit,) in tam absonam sententiam insurgant. Iter Londinum (marito nuper vita functo,) propediem meditatur, quod superest vitæ apud filiam confectura; ubi (cum appulerit), ab ipsius ore, si locata opera dignum censueris, certior fias; nec enim in tam frequenti, novitatis avida, urbe diu latere potest.*

Jan. 28, 1671.

* Dr. Cole mentions in a note, that on the death of the patient, which happened 2 or 3 years

A Letter from Mr. Nich. Waite, Merchant of London, to Dr. Robert Plot. Concerning some Incombustible Cloth, lately exposed to the Fire before the Royal Society. Dated Sep. 10*th*, 1684. N° 172, p. 1049.

The great respect and honour I bear to the learned and ingenious gentlemen of the Royal Society, prevailed with me, within few days after my arrival in this city, to expose to their sight and examination a piece of linen cloth, which by their experiment consumed not in the fire: and you being then desirous I would give a short narrative of what substance, and in what parts it is said to be made; I here send you the same account I received of it, from one Conco, a natural Chinese, resident in the city of Batavia in the north-east parts of India. Who, by means of Keay-arear Sukradana, also a Chinese, and formerly chief customer to the old Sultan of Bantam, did after several years diligence, procure from a great Mandarin in Lanquin, a province of China, near ¾ of a yard of the said cloth; and declared that he was credibly informed that the princes of Tartary, &c. used it in burning their dead; and that it was said and believed by them, to be made of the underpart of the root of a tree growing in the province of Sutan; and was supposed, in like manner, to be made of the todda trees in India: and that, of the upper part of the said root, near the surface of the ground, was made a finer sort, which in three or four times burning I have seen diminish almost a half: they report also, that out of the said tree there distils a liquor, which not consuming, is used with a wick made of the same material with the cloth, to burn in their temples to posterity.

The handkerchief or pattern of this incombustible linen, which was shown to the Royal Society, being measured, was found in length 9 inches, between the fringe or tassels; the fringe at each end being 3 inches more; so that the whole was just a foot in length: and the breadth was just ½ foot.

There were two proofs of its resisting fire given at London: one before some of the members of the Royal Society, privately, Aug. 20, 1684; when oil was poured on it whilst red-hot, to enforce the violence of the fire. Before it was put into the fire this first trial, it weighed 1 oz. 6 drs. 16 grs. and lost in the burning 2 drs. 5 grs.

The second experiment was public, before the Society, Nov. 12 following, when it weighed, before it was put into the fire, 1 oz. 3drs. 18 grs. Being put into a clear charcoal fire, it was permitted to continue red-hot in it for several minutes: when taken out, though red-hot it did not consume a piece of white

after her removal to London, several physicians of eminence requested permission to open the body; but "viri cujusdam nimis pii suasu" this request was not complied with.

paper, on which it was laid: it was presently cool, and on weighing it again, was found to have lost 1 dr. 6 grs.

Dec. 3, Mr. Arthur Bayly, F. R. S. presented them with a piece of this linen, in the name of Mr. Waite. At the same time he presented Dr. Plot with another piece of it, which being brought to Oxford, the experiment was again repeated on it, Dec. 16, it being put into a strong charcoal fire in the Natural History School, in a full meeting of the Philosophical Society of that university; where after it had continued red-hot for some considerable time, it was taken forth again little altered when cold, saving that it seemed a little whiter and cleaner than before it was put in. Concerning which, Dr. Plot, being desired to offer his thoughts, drew up the following discourse, which was read before the said Society, June the 23d, An. 1685.

A Discourse concerning the Incombustible Cloth above mentioned. By Robert Plot, LL. D. N° 172, p. 1051.

The incombustible cloth was greatly esteemed by the ancients, among whom it was more common, and perhaps better known, than it is yet among us, equally precious with the best of pearls. Nor is it now of mean value, even in the country where made, a China covet, which is a piece 23¾ inches long, being worth 83 tale, that is, 36l. 13s. 4d. The reality of such a thing has been either doubted or denied by very good authors; who, though they allowed such a mineral as amianthus, out of the woolly part whereof this sort of linen was always anciently said to be made, yet questioned the possibility of its having been actually done: Dalecampius holding it very incredible, that it should be woven into cloth, by reason of its shortness; and Schildius, in his Commentary on Suetonius, absolutely denying it. Boxhornius indeed does not deny but that there might be such linen among the Indians, where the materials of it grow; of which they might make funeral shrouds for the bodies of their princes, and so preserve their ashes distinct from those of the pile in which they were burnt: but he is peremptory that the Romans never used any such; and so is Isaac Casaubon. The truth whereof I shall not dispute: but whether they did or not, I am sure they might, had they pleased; for Pliny says expressly, that he himself had seen napkins of it, which being taken foul from the board at a great feast, were cast into the fire, by which means they were better scoured, and looked fairer and cleaner, than if they had been washed in water. Now if they had such napkins, they might doubtless have had sheets of it too, and have put them to the use above-mentioned, had they thought it expedient, as it is said the Tartarian princes and others do at this very day.

That this linen was very well known to the ancients, beside that of Pliny, we have the further testimony of Cælius Rhodiginus, who places both the materials and manufacture of it in India; and Paulus Venetus more particularly in Tartary, the emperor of which, he says, sent a piece of it to Pope Alexander. It is also mentioned by Varro; and Turnebus in his commentary upon him, De Lingua Latina, as a thing inconsumable by fire. In these latter ages: George Agricola relates, that there was a mantle of this linen at Vereburg in Saxony; and Simon Majolus says, he saw another of it at Louvain exposed to the fire. Salmuth also acquaints us, that one Podocattarus, a Cyprian knight, showed it publicly at Venice, throwing it into the fire without scruple or hurt; and Mr. Lassells saw a piece of it in the curious cabinet of Manfred Septalla, canon of Milan. Mr. Ray was showed a purse of it, by the Prince Palatine at Heidelberg, which he saw put into a pan of burning charcoal, till it was thoroughly ignited, without receiving any harm; and we are told in the Burgundian philosophy, of a long rope of it, sent from Signor Bocconi to the French King, and kept by M. Marchand, in the king's gardens at Paris, which though steeped in oil and put in the fire, is not consumed. To which add, that we have now seen a piece of this linen pass the fiery trial, both at London and Oxford.

This substance has several names; as, 1. Amianthus, because the fire is so far from spoiling it, that it rather gives it a lusture. 2. Asbestos, or unextinguishable. 3. Salamander, or salamander's wool; I suppose from the thryallides or candle-wicks said to be anciently made of it. 4. From a pungent quality which Agricola says it has on the tongue, without astringency, it is otherwise called alumen, with the distinguishing epithet plumeum, taken from its downy filaments, to discriminate it from the other alums. 5. From the light gray colour of its lanuginous parts, it is called by some polia; by others corsoides; and from its likeness to the hoary fibres of some sort of matweed, spartopolia. 6. From the capacity it has of being spun into thread, it is also called linum, or flax, with some distinguishing epithet, taken either from its quality, such as asbestinum, or vivum; or from the place where found, it being called in general linum fossile, earth-flax, and in particular linum Indicum, Creticum, Cyprium, Carpasium, Carystium. But besides these places, which have given epithets to the threads made of it, it is also found in Tartary, at Namur, in the Low-countries, at Eisfeld in Thuringia; among the mines in the old Noricum; somewhere in Egypt, and in the mountains of Arcadia; at Puteoli, and in some mines of Italy. To which we may add our own country, it having been lately found in a small island, called Ynis Molroniad, in the parish of Llan-Fair yng Hornwy, in Anglesey in Wales.

Secondly, as to its natural principles, though it be commonly by the lithographers reckoned among stones, I should rather judge it a terra lapidosa, or middle substance between stone and earth; but whether the one or the other, it probably consists of a mixture of some salt, and a pure earth without sulphur, coagulated in the winter, and hardened to maturity by the summer heats: Which salt Johannes Hessus proves, by a very cogent argument, to be alumen liquidum, describing it, as Matthiolus also does, to be a whitish lacteous substance, somewhat inclining to yellow, that sweats out of the earth, and smells like rotten cheese; of which, having gathered a quantity at Puteoli, together with the other species of alum, and kept it a while by him, when he came to look on it again, he found it to have lost the smell, and a great part of it changed into alumen plumeum; the saline part I suppose shooting into threads, and the pure earth uniting them, as found in the places wherever generated, whether by sweating from the earth, as Pliny and Matthiolus would have it, or percolated through rocks, as found in Wales, the veins of it there running through a rock of stone, in hardness and colour not unlike flint. And yet it seems to be made of much such an alum as that of John Hessus, at Puteoli, was, some of it being straw-coloured, as if it still retained the yellowness that his liquid bitumen was said to have, which is a colour not assigned to it by any author, most of it being said to be white or cinereous; some of it red, and some of an iron-colour, as Agricola tells us; and I have some of the Cyprian by me, sent from Aleppo by my worthy friend Dr. Robert Huntington, part of which is of a light blue or pearl-colour, and some of it has a cast of sea-green. But however the whole mineral substances found at several places may differ in colour, yet I do not find but the woolly part of them all seems to be much the same, viz. of a white silver colour, the threads very fine and slender, yet very ponderous, the smallest particles of them, when thoroughly wet, sinking in water; whence it is probable, that it is not a vegetable but a mineral substance, although it be known that there are several woods, such as box, red wood, Persian wood, &c. that will sink in water.

Concerning the manufacture of it into thread, cloth, &c. Marcus Paulus Venetus acquaints us, in his book de Regionibus Orientalibus, how it is made in Tartary; where he says it is found in a certain mountain in the province of Chinchinthalas, and made into cloth, as he was informed by one Curficar, a Turk, superintendant of the mines in that country, after this manner. The lanuginous mineral or amianthus being first dried in the sun, is then pounded in a brass mortar, and the earthy part separated from the woolly, which is afterward washed from any impurities that may stick to it; being thus purged, it is then spun into thread like other wool, and after wove into cloth, which when foul or spotted they cleanse, by throwing it into the fire for an hour's time, whence it comes out unhurt, as white as snow. Which very method, as Strabo describes it, seems also to have been used in ordering the Cretan amianthus; with this addition, that after it was pounded, and the earthy part shook from the woolly, he says it was combed, and so also says Agricola, which argues there was some of a greater length than any I have yet seen; what the Cretan might be I cannot tell, but the Cyprian I am sure is short enough, as also the Welch, and so was all that was known in Pliny's time, who confesses that it was very difficult to weave, by reason of its shortness.

As to its uses, Pliny informs us, that shrouds of this linen were anciently used at the royal obsequies of kings, to wrap up their corpses in, so as that the ashes of their bodies might be preserved distinct from those of the wood which made the funeral pile, and the letter acquaints us that the princes of Tartary use it at this day for burning their dead; and though it must be acknowledged it diminishes every time it undergoes the violence of the fire, yet this hinders not but it may do that service several times before it be rendered quite useless. Cælius Calcagnanus says, that some of the ancients made themselves cloth of it; with whom Turnebus agrees in his Commentary on Varro; and Cælius Rhodiginus relates that the Indians made garments of it; but Hierocles restrains it to the brachmans only. Marco Antonio Castagna, who found this mineral somewhere in Italy, knows how to prepare and render it so tractable and soft, that it resembles well enough a very fine lamb's skin, which he can make thick or thin to what degree he pleases, and so to resemble either a very white skin or a very white paper. We have also made paper of the Welch amianthus lately here at Oxford, which will bear both fire and ink well enough, the ink only turning red by the violence of the fire.

Lastly, to show the reason whence it is, that this substance should be so strangely privileged by nature, as to be wholly put out of the power of fire, we must consider that the qualities and power of fire, according to Aristotle, are to separate things of a different, and unite those of a like nature; whence the subjects most apt to take fire, and be dissolved by it, are such heterogeneous bodies, in whose pores the most sulphurous, bituminous, and aqueous particles are lodged, which being seized by the fire are quickly put into motion, dilated, and separated, and being thus made capable of flying away, they are at last consumed, and the frame of those bodies are dissolved, whose parts before were united by them. When these are gone, the fire naturally goes out, as having nothing now left to work upon, nothing remaining but the salts and earth in the form of ashes; which in all compounds are the things that resist this element most, and will remain after the most exalted operation it can be forced to. Nor do the salts only of mixed bodies thus baffle the force of fire, but the simple ones much more, as being more homogeneous; as we see in the decrepitation of common salt, and exsiccation of vitriol; which, when the aqueous parts are once evaporated, are now a pure simple homogeneous body, no longer sensible of the fire, the decrepitation ceasing, and nothing remaining that can be dilated any further to break the corns of salt. Now whatever the fire cannot dilate it cannot separate, nor consequently destroy, or carry any thing from it; except what is heterogeneous, and accidentally adhering to the outside of it, which is perfectly the case of our incombustible linen, whose threads being altogether homogeneous, and nothing else but the pure striæ of liquid alum, holding nothing of sulphur, bitumen, or water, or any thing that is different or heterogeneous from itself, that can be dilated or separated, it is in no possibility of being liable to the fire, which may indeed pass through it, as we see it does when made red-hot, but can carry nothing from it.

Short Memoirs for the Natural Experimental History of Mineral Waters. By the Hon. Robert Boyle, F.R.S. London, 1684-5, *8vo.* N° 172, p. 1063.

The author divides this tract into 6 sections, the first whereof is only introductory, wherein remarking the imperfect ways in which such waters have hitherto been described, he therefore has thought fit to communicate these his memoirs, in order to a more full and methodical history of mineral waters; to the drawing up of which he thinks these three following observations necessary. 1st. That a man ought to take notice of those particulars that relate to it, whilst it is yet under ground, or in its native receptacles. 2dly. To examine the properties and other qualities of it, when it is drawn up by men at the spring head, or other receptacle. 3dly. He is to consider the operation and effects of it on human bodies, whether sick or sound, &c.

To the first of these he has subjoined a set of titles for the 1st part of the proposed work. He has given likewise a scheme of titles for the 2d part; but because the 2d part is that which he mainly designed, he has reserved to it two other sections, viz. the 4th and 5th. In the first of which he gives experimental remarks on the way of examining mineral waters by the help of galls. He cannot by any means think the infusion of galls to be of that use and certainty, that it is commonly presumed to have, inasmuch as it only discovers a liquor to be or not to be, either of a vitriolate or ferruginous nature; for there are divers metalline ores, and other mineral bodies which, not participating of iron, will not by this means be discovered, and yet may at the same time strongly impregnate the water. The decoction of arsenic, for example, changes no more upon galls than would common water. Moreover, unless iron be the only predominant mineral in the vitriolated water, the infusion of galls does not answer it, for on a strong solution of Roman vitriol, where copper is the predominant metal, the infusion of galls afforded neither a black nor blackish colour, only a thick and muddy one, that was not so much as purplish.

In his 5th section he observes, and brings his ocular demonstration for the life and motion of those creatures found in water wherein pepper has been infused. He observes likewise that the freshness and quickness of such ferruginous waters as are lighter than common water, are much lost in the removal. He dislikes the division of mineral waters into acidulæ et thermæ, having by several trials found, that there is not a manifest acidity in those waters that are not sulphureous or hot; neither does he think any of our purgative waters contain a salt that belongs to any one known sort of salts; but is either of a sort for which we have yet no name, or else is of a compounded nature, since two bodies, neither of which is cathartic, may by a change of texture compose a third body briskly purgative; of which he has given an example; he adds that those ferruginous waters, that are not heavier than common water, and in most drinkers prove but diuretics, afford very little caput mortuum, or dry substance, on the total evaporation of their liquor, whereas mineral waters that are purging, and manifestly more ponderous in species than common water, leave a considerable quantity of residence, ex. gr. 1 lb. of Barnet water, which is purgative, yielded 1 dram of white powder. 1 lb. of Tunbridge water yielded caput mortuum, about 1 dr. 25 gr. 1 lb. of German spa water gave but gr. 1½. Neither need so small a quantity seem inconsiderable, since upon trial one part of marcasite communicated a tincture to 61440 parts of the infusion of galls; and on computation 1 gr. of vitriolate substance might impregnate 6000 times its weight of common water, so as to make it fit to produce with galls a purple colour. To all which lastly is subjoined the 6th section, which the author says, consists only of a set of articles referable to the medicinal use of mineral waters.

Phænomena in Cadavere prænobilis cujusdam Fæminæ, Apoplexia peremtæ, inter dissecandum, Maii 12, 1679, *à Clariss. Medico Dno Dre Cole observata. Translated and abridged from the Latin.* N° 173, p. 1068.

The lady of rank, who is the subject of this narrative, was affected many years before her death with what are commonly called hypochondriacal and hysterical symptoms; and latterly in addition to these with bleedings from the nose, sometimes so profuse as to be extremely alarming. Among other remedies, recourse had been had to the lancet, which (as well as the hemorrhages) had frequently afforded relief, although the patient was much emaciated, not only in consequence of the attacks themselves, but also of her very great abstemiousness.* About a month before her death there happened one of these hemorrhages, to a degree which threatened to prove fatal; it was however with great difficulty stopped; after which there succeeded such an alleviation of her complaints, that she flattered herself with being entirely out of danger. The very day before she died there came on another of these hemorrhages, which was suppressed at the beginning by the remedies employed for that purpose; so that she again thought herself safe. But in the evening of the same day she was suddenly seized with a violent head ach, accompanied with some faltering of speech. A surgeon was sent for to bleed her, but before he arrived she expired, viz. within half an hour from the attack.

On opening the body no scirrhosity of the liver appeared (as had been suspected by Dr. Mayerne, and other celebrated physicians, who had attended her some years before) but this viscus was of a larger size than natural. There was no bile in the gall-bladder (which was much contracted); but it contained 14 calculi, most of which were equal to a pea, but 2 or 3 of them were rather larger. They were round and somewhat flatted, of a smooth surface, and dark colour, resembling bezoars, after they had been some time exposed to the air, but at first view looking like aloetic pills. They were yellowish in the inside, with some degree of hollowness in the centre, and they easily crumbled between the fingers. The spleen, like the liver, was larger than natural. No morbid appearances were observed in the stomach, pancreas, mesenteric glands, or omentum. The kidneys were rather less firm than usual. The uterus exhibited nothing preternatural.

In the right side of the thorax a portion of the lungs, 4 fingers breadth, adhered firmly to the pleura, and in various places, especially at the margins of the lobes, the lungs appeared of a black or livid hue, which showed that they approached to a state of sphacelation.

The heart was sound, and there was a considerable quantity of fat at its basis, although the rest of the body was greatly emaciated.

It was on opening the head that the cause of this sudden death became evident; for the blood-vessels that go to the membranes on the right lobe of the brain were found remarkably turgid; and on dissecting them (the membranes) from that side of the brain, which was the side of the head the patient complained of at the moment of her attack, there flowed out a large quantity serosi sanguinis, which being removed, and an incision made into the substance of the brain, there came into view a large clot of blood, which weighed about

* In a subsequent part of the narrative the author mentions, that the patient denied herself a sufficiency of food, "ne seraphicis fruitionibus poneretur obex."

$1\frac{1}{4}$ oz. and which must necessarily have formed a large cavity there.* But there was no extravasated blood in the ventricles, nor between the membranes in any other place. As for the left lobe, neither it nor its vessels exhibited any thing preternatural. The cerebrum and cerebellum, taken out of the cranium, and freed as much as possible from the superfluous blood (*superfluo*, quà licuit, sanguine liberatum) weighed 2 lb. 14 oz. avoirdupois.

Abstract of a Letter from Mr. Leewenhoeck, to the R. S. dated Jan. 23d, 1684-5; concerning the various Figures of the Salts contained in several Substances. N° 173, p. 1073.

I took some of the salt of Carduus Benedictus, such as is commonly sold in the apothecaries shops: it was rather moist, and its parts seemed to be so huddled up together, that they could not be distinguished from one another: I closed it up in a glass, to prevent the evaporation; and when it had stood stopped for some days, many of the particles were run together, on the side of the glass, forming some flat longish figures, of different magnitudes, the largest in length about the diameter of a hair of my beard; as N° 1, fig. F, pl. 6. In another place, these salts lay so, that I could easily discern their thickness, as fig. G. In another place, the thin flat salts lay over one another, as fig. H. I put this salt in water to dissolve it, and took of it about as much as two barley corns, spreading it thin before me: and when it was in motion, I not only observed the abovementioned figures, and shootings of the salt; but found several new figures, which were thin and long, and sharp towards both ends; as fig. I. Others that lay near them were broader, but not so long, and their ends not so sharp; as fig. K. I saw also some perfect four square figures, as fig. L; but they had no thickness that I could discover. Also there were some quadrilateral pyramidal figures, like those of common salt; as fig. M. These observations must be made before the water is evaporated, for when the water is almost gone, such a multitude of particles appear, and run together that they cause a confusion. On a further examination, of a more genuine sort, I perceived very plainly a number of figures tapering towards both ends, as abovementioned in fig. I. After about a day's time, I saw several flat figures; as F and H. But having dissolved the salt in rain water, and viewed it as it lay thin upon my plate, I found all the abovementioned figures; but those of K, L, and M exceeded in number all the rest; insomuch, that I con-

* Dr. Cole has not described, with the requisite degree of precision, in what part of the right lobe of the brain this large quantity of coagulated blood was found. How the extravasated blood could suddenly form for itself a cavity in the substance of the brain, it is difficult to conceive; and we cannot help suspecting (though the Dr. was persuaded to the contrary) that the extravasation of blood took place in one of the ventricles.

ceived I saw more in a quantity of water equal to the weight of a grain, than there are stars to be seen in the heavens by the naked eye.

Salt of Wormwood.—In salt of wormwood, after being dissolved in water, were presently discovered a great quantity of figures, sharp at both ends; as N° 2, fig. A. Again, there were lesser figures, as B, in innumerable quantities: some few had six sides, as C; a few were flat and square, as D; a very few were like triangles, which had the angles cut off; as E. Where the water had continued long, there were several six-sided figures, as large as a small sand; as, among the rest, F. Also some square pieces of salt, like a looking glass with a border about it, as fig. G.

Alum.—Having put some alum in rain water, it exhibited very small figures, with hexagonal bases, and sides rising up pyramidal, like a pointed hexangular diamond, as N° 3, A A A. They were of different magnitudes, and some seemed plain, without any rising, as B B. There were also six-sided figures irregular, as C and D. But as the water evaporated, there were seen several long blocks of alum, as fig. E; and the salts run together, as large as sands, growing unmeasurably, where the water had been thickest, so as thereby to be less distinct. There were also six-sided flat figures, as F, having in the middle other small six-sided figures, rising pyramidally.

Salt-Petre.—I put salt-petre in water, and observed swimming in it a few long particles, which seemed to have no thickness, as N° 4, fig. A. These increased visibly in size, though I could not perceive any particles near them that could cause it. As the water in any place began to be evaporated, I found many figures whose bases were square, and rising into pyramids. There were also a few triangular figures. Where the water lay thick, there were pretty figures like square sticks, as C D E. These last figures took up no greater space than might have been covered with a great sand; though there were clusters of them, that were 100 times less.

Vitriol of Cyprus, or Blue Vitriol.—Having dissolved blue vitriol of Cyprus in fair rain water, and viewed it in a microscope; I found swimming on the water clear pellucid figures, like crystal; these had no thickness to be discerned in them, because they were level with the surface of the water; all their ends were sloping, as N° 5, fig. A. In 2 or 3 minutes of time, they grew 100 times larger than before; though they continued to have the same shape, for they increased both in length and breadth; but in becoming bulky, they lost their pellucidness, and turned of a blue colour: other salts were shorter, and shaped like B, some of them so small, that by calculation they were above 28,000 times thinner than a hair of my head. As I spread the water very thin, the figures were inconceivably strange: it seemed that the vitriol separated from the water, and ran into round particles, just like oil when mixed with water. As these figures passed slowly through a great deal of water, they increased gradually; as for example, a 4-sided figure was raised higher by the accumulation of the vitriol, which made a border around it; and this not only once, but 15 or 16 times.

Salt or Oil of Tartar per Deliquium.—I mixed the liquor of tartar with water, and let it stand for some time, that the grosser foul parts might sink down. In this mixture I observed long slender particles, which through a common microscope were like the shavings of a man's beard; as N° 6, fig. A. These drove against the superficies of the water; and some of them grew in length, breadth, and thickness, as fig. B. Also some, as fig. C, having often two slope ends, and some only one, the other end being sharp, as fig. D. These salts in some places grew so large, that through a microscope they seemed as long as one's finger; but then they were oddly formed, especially when crowded together.

Of Muscovy Pot Ashes.—Having put Muscovy pot ashes in rain water, and let it stand a few hours, I observed in it longish figures, like weavers' small shuttles, as N° 7, fig. A. These particles were so small, that I judged them to be 1000 times thinner than a hair of my head; but as they became larger, they became like fig. B. I saw also a 6-sided figure, which rose pyramidally, like a 6-sided pointed diamond, as fig. C. But this sort was very rare, being scarcely one to 1000 of the former. I saw also several oblong figures, with 4 even or straight sides, as fig. D. And some few exactly square, as fig. E.

After a day's time, I put more pot ashes into the water, that the ley might be the stronger, and after having stood 10 hours to clear, there appeared in it a great number of extremely small 4-sided figures, E; also 6-sided ones, as F. But they were generally imperfect, and somewhat long: these last two sorts were each so small, as well as the first, and some few shaped like G, that they would not cover the $\frac{1}{1000000000}$ part of a coarse sand. When the ley was thicker in one place than another, there came out so many different figures, having all their dimensions, that they were scarcely to be counted, viz. square, oblong or parallelogram, cubical, triangular, hexangular, rhomboidal, and various others; which had sometimes their thickness equal to their length or breadth.

Of Camphor.—I observed some camphor as it is brought from the Indies, and found at first nothing remarkable; but on more narrowly examining it, I perceived the crystalline figures as they were clotted together. These figures, when not too close to one another, had 6 perfect sides; they were of different magnitudes, and generally longish; they had all their dimensions, their breadth and thickness equal, and less than their length, as fig. B, N° 8. Many of them, though they had 6 sides, were somewhat irregular; some shaped like the flint of a firelock, as fig. D; and though most of them were irregular, yet I judged if they had not lain too close together, their form would have been like those of fig. A; for when the parts of the camphor at first were small, and lay too close together, they might take from one another the means of increasing regularly every way; but when the parts are great, and come to apply their sides to one another, they then make very large and irregular salts.

Salt of the Ashes out of an Oven for the Foundery of Cannon.*—I mixed some of this with water, and let it stand till the grosser parts subsided, and the liquor was pretty clear, and then there appeared a very great number of small clear thin pipes, so extremely slender, that I could hardly see them; when these pipes were increased to about the size of the 25th part of a hair, their ends were sloped, as N° 9, fig. A. Of these salts there were several thousands in one drop of water. There were likewise floating about a few particles like fig. B; and though I could not discern the thickness in the first figures, yet in these last it was very plain, and small in comparison of the breadth: at another time there appeared salts like fig. C.

*Salt, or Ashes of a Tin or Lead Oven.**—Our Porcelain bakers use much tin or lead, which they calcine in their ovens. This work is so prejudicial to those that tend it, that a man is unable to stand before the mouth of the oven more than 24 hours at a time, and then he looks as if he were poisoned: so that every day a fresh man is employed to take care of the oven, and remove the scum from the surface of the lead. Hence I was induced to examine some of that greyish substance which sticks to the stones, on which the flame of the oven beats. Having dissolved it in water, and let it stand to settle, I found several oblong figures, as N° 10, fig. A: they were of different magnitudes, and some larger than fig. B; some were sharp at both ends, as fig. C. All these were generally without any discernible thickness, and were as transparent as the clearest water. I particularly observed that three figures, two of C and one of B, lay in a quantity of water not so large as a sand: while I caused some heat in this water, and continued to watch it, imagining the other salts it contained would increase the bulk of these three, I could find nothing but very small square salts, whose sides rose up pyramidally; these indeed grew larger, but the others were not altered: as the square salts grew larger, they became so much the more irregular, because the smaller salts were driven upon them or as it were attracted; for as they came near them, the foremost had as quick

* Furnace.

a motion, as it were forcibly impelled, till it united with them, making them more and more irregular; of such salt particles, there lay 50 together, and as many more by themselves, being all produced from one drop of water.

Further, I took at about 2 feet distance from the mouth of the tin and lead oven, out of the chimney which the flame did not reach, a black substance like soot, which I mingled with fair rain water, and let it stand till it was settled; and in this water I observed there were many irregular figures, whose irregularity I believe proceeded from the want of matter to perfect them, and from the too soon evaporating of the moisture; I saw likewise many neat, flat, transparent bodies, having each 4 corners sticking out, and 2 inward angles, as N° 11, fig. A: of this sort there were some that I could not see, but with my glasses that magnify most. There were also some figured like a rhomb, and others like a rhomboid, as at fig. B.

Salt in Quick Lime.—I put some of the quick lime, from Liege, into water; and observed an exceedingly great nnmber of salt particles, but so very small, that I cannot attribute to them any perfect figure, though seeming inclined to a square, and their thickness nearly equal to their length: together with these, were some small figures sharp at both ends, as N° 12, fig. A; others blunt at both ends, as fig. B; others had 6 complete sides, as fig. C; and among these, some were so very small, that a globule which makes the redness of the blood, would cover them; these last were as transparent as the finest glass, and as thin as can be imagined; even when 5 or 6 of these particles lay disorderly over one another, their united thickness was still inconsiderable. Also some few figures were shaped like D and E; and some few like F.

Salt in Lime of Fish Shells.—Of this lime, which is made of sea shells burnt, I mixed a good quantity with water, and letting it stand till it was settled, I took up a drop, which seemed to be as clear as crystal; but I soon perceived figures resembling thin boughs of a tree, without leaves; their number being so great, that they made the water white and troubled: these are composed of small salts, and are difficult to be described; so that I have represented only one small one, N° 13, fig. A B C D E, the breadth of which may be covered with a head hair: the particles of these branchy figures were hexagonal, and when seen edgewise, they showed like F; some other figures had their upper part quadrilateral; and others had the basis quadrilateral, and the sides running up like a pointed diamond, as common salt. Others were like I K L, and M; which, lying apart and separate from the rest, were more distinctly to be seen. Other figures were irregular, and could not well be described because of their smallness, and lying so thick and close together.

Among the innumerable quantity of small salts, contained in a little water, some are larger, and these either transparent or more obscure, of which last I shall describe two, as G and H, which appeared plainly to be compounded of other figures; for in one of the sides of G, I counted more than 30 figures, which number being cubed, comes to near 30,000: and yet the side of G seemed not to be the 40th part of the diameter of a coarse sand.

Salt of English Soda.—I took of this soda, (which is made of glass wort, and is much used by the potters in our city, for glazing their porcelain) and beat it small, and then put it into rain water; when it was settled, I observed in the water long thin figures, whose ends seemed sometimes to be straight lines, and other times circular, as N° 14, fig. A. As the figures increased in size, they became more straight at the ends, and not flat or plain, but generally raised, as fig. B. Some were raised, but their ends consisted of 3 sides; and some were even at one end, and had 3 sides at the other end, as C. There were also 6-sided small figures, as D; and of these I discovered a great number completely formed, very thin and small, and clear as crystal; whereas the other figures were obscure, as if they had been strewed with blackish sand; and so likewise were these, when they were viewed with a better glass. I saw also figures which were broad in the middle, and sharp at both ends; or rather a little flat, as E. Also small squares, as F; and this was the most general figure of this salt; for the bases were mostly square; running up into a pyramid, like common salt. I took some pains to find of what kind of parts B, C, D, and E, were compounded, and it seemed to me, that each of them consisted of parts like itself, yet I once observed that a six-sided figure like D had joined itself to one like E.

When the soda had lain long in the water, and this was become very strong, there appeared in it a great many transparent figures, like the fairest crystal; these had their sides perfect, their breadth and thickness equal, shaped as B, C, and D.

Salt of Soda of Brittany.—Having treated this soda as the former, I saw floating in the water an incredible number of small 6-sided figures, as N° 15, fig. A; being very thin, and many of them so little, that I could not have described them, if some of them had not been larger than the rest. In another place, I saw large 6-sided planes, made like hexagonal looking glasses, and having a small 6-sided figure on the surface, as B. I saw also a few figures formed like C, and some squares, as D, part of them plain, and others had the sides rising up pyramidally to an obtuse point, as E, or rather a pyramidal frustum, as F: for the top is the true figure of the salt, and the rest of the bulk is nothing but an addition of other quadrilateral salts. Likewise would the top of G, if magnified, be like to H.

Alicant Soda.—In this salt I could see some particles, flat, thin, 4-sided, and 6-sided figures, as N° 16, fig. A and B. Also oblong figures, as C, whose sides run up sharp: these salts were dissolved by the least moisture of the air.

Sal Ammoniac.—The figure of this salt dissolved in water generally appeared like the boughs of a tree, beset with irregular leaves, one larger than another, as is represented N° 17, fig. A E. In another place lay 5 or 6 branches like A, seeming to proceed from a common centre, as E. I saw also salt particles like B and C; and where there were no branches, the scattered salts looked like so many flints, differing from each other in size, but being never perfectly round, as fig. D.

Experiments at Woolwich, March 18, 1651, *for trying the Force of Great Guns. By Mr. Greaves.** N° 173, p. 1090.

At 200 yards distance from the platform for the great ordnance there were raised 3 butts, one behind another: the space between the 1st and the 2d butt was 14 yards, between the 2d and the 3d, 8. The thickness of each butt was 19 inches, whereof 13 was of beams of massy oak fastened into the ground, and set so close that they touched each other: on each side were planks of oak, each 3 inches thick, which were joined close, and fastened on both sides with iron bolts, and strong pins of wood; and on the back at the ends, and on the middle there were 3 braces of elm, 12 inches broad, and 5 inches thick.

The first experiment was with an iron demy cannon of 3500lb. weight, with a cylindrical bore; the powder 10lb., the bullet 32lb. of iron, which pierced through the first two butts, and stuck in the 3d, so as the ball was almost quite within, but the timber not shivered, nor scarcely split. The like effect happened with 9lb. and 8lb. of power.—The 2d experiment was with an iron demy cannon having a taper bore, and being 3600lb. in weight, and 4 inches longer than the former; the powder 7lb. the iron bullet 32lb. and which in 3 trials seemed to have the same force with the first. One of the shots piercing through the 2d butt, and lodging near the edge of the middle butt of elm, tore it, but by the yielding of it, the bullet glanced aside off the 3d butt, and

* This was probably Mr. John Greaves, a good mathematician and antiquary. After leaving the university (Oxford), he was some time professor of geometry in Gresham College. He afterwards visited several foreign parts, as Holland, France, Italy, Turkey, Egypt, &c. returning home well stored with manuscripts, gems, coins, and other antiquities. After his return he was appointed professor of astronomy at Oxford, but he was obliged to resign that professorship by the persecution of the parliamentary visitors. Mr. Greaves was born at Colemore in Hampshire, in 1602; and he died in 1652. He was the author of several learned works; as the Egyptian pyramids, the ancient weights and measures, and other antiquarian subjects.

entered the earth.—The 3d experiment was with a whole culverin of brass, of 5300lb. in weight, 11 feet one inch in length, with a taper bore, being intended for a chace piece to the frigate called the Speaker; the iron bullet was 18lb. in weight; the powder in the first trial 10lb. in the 2d 9lb. in the 3d 8lb: which last proportion did the best execution, and passed through the first two butts, entering a little into the 3d, which the former two just touched, but did not enter.—The 4th, experiment was with a whole culverin in brass, made at Amsterdam for the French, with this mark 3580, being 10 feet long, and not very thick in the breech: the first shot, with 9lb. powder, and 18lb. iron bullet, passed through the 3 butts, and entered one foot into the ground; it passed by the joints of the timber, 2 planks having been beaten down before. The 2d shot, with 8lb. powder, passed through 2 butts, and grazed between them. The 3, with 8lb. passed 2 butts, and 7 inches into the 3d, but the first butt was much battered before, where it entered. The 4th shot, with 8lb. powder, passed two butts, and in both butts through the midst of a massy strong beam, that had not been battered.—The 5th experiment was with an iron demy culverin, having 9lb. bullet in iron, and 4lb. powder; this passed one butt, which was torn before, and entered the 2d.—This ½ culverin was discharged 8 times, as fast as it could be loaded with powder and the iron bullet, and yet was but scarcely luke-warm at the breech, a little more in the midst, and most at the muzzle, yet this last scarcely so hot as my hand, though the gunners in charging her, wet not at all the scoop or sponge.—The 6th experiment was with a brass demy culverin; the breech diameter was 13 inches ⅛, the mouth 9⅛, The first shot, with 4lb. powder, 9lb. iron bullet, passed 2 butts. The 2d. shot, with 3lb. powder, passed almost 2 butts: this proved to be the best shot, because the timbers were the strongest.*

A new Way of raising Water. By Dr. Papin,† F.R.S. N° 173, p. 1093.

Having several times observed a practice, that after some new discovery, the inventor propounds it as a riddle, to stimulate those that are ingenious in the same kind of learning, and so occasion them sometimes to discover even better things than what is propounded; I thought I might do the same, concerning

* There can be little or no scientific knowledge derived from these experiments. The chief object in making them, seems to have been, to determine whether these pieces of ordnance would send their balls through such obstacles as the sides of ships, which they showed very completely.

† This was Denys Papin, the nephew of Nicholas, and cousin of Isaac, noticed p. 239, vol. 2. In that place our author was, by mistake, called Nicholas, instead of Denys. It seems that he had been settled some time in England, but was originally a French philosopher.

a way for raising water, which I conclude to be new, since it is not used on considerable occasions, where it might be of great advantage. It is as follows: See fig. 5, pl. 5.

A A is a large glass, made like a tumbler, but much larger, and set upon the chimney board B B—C C is the engine, like a small rock, that constantly spouts out water by the two holes D D: this rock is kept at a distance from the bottom of the glass A A; so that it may plainly be seen that it cannot receive any water by subterraneous tubes.—E E is a factitious coral, reaching from the centre of the rock C C, to the centre of the crown F F.—F F is a crown, bearing on the aperture of the glass A A, and holding the rock C C suspended at a considerable distance from the bottom.—G G a glass, open at both ends, applied to the rock C C, to keep the water on it from falling down.—H H two shells to receive the water from the jets. If the Royal Society will be pleased to appoint some persons that may come, and to watch it a whole day, it may be seen whether it will not run constantly, without losing any thing of its strength. I hope that the learned in hydrostatics, being by these means assured of the possibility of such a motion, will be the more ready to think of it, and find perhaps something better than this. But if no person do, I will myself within some months publish this contrivance, with the uses it may be applied to.*

According to the inventor's desire, the Royal Society ordered that the thing should be observed. Mr. Hook saw it for near half an hour, there being other persons to observe it longer, who watched it about 4 hours together, during which time there sprung out of the rock C C, above 100 times more water, than a vessel of the same size could hold: so that they went away not doubting but the water did circulate in the said engine, and might continue a great deal longer, since it ran still as constantly and as high as at their first coming: and Mr. Boyle, knowing the whole contrivance, assures that it may continue fo a whole day and more; and thinks it worthy to be left for some time to the inquiry of ingenious men.

Accounts of Books.

I. *A Treatise of Algebra, both Historical and Practical. By John Wallis, D. D. Professor of Geometry in the University of Oxford, F. R. S.* N° 173, p. 1095.

This work was published in folio, 1685, and is the first book professedly written on the history of the science of algebra. The history however, properly so called, is but a small part of the work, the great mass and bulk of it being made up of the practice of the art, being indeed a complete treatise on

* The nature of this machine will accordingly be found explained hereafter, viz. in N° 178.

the algebraic art, as it then was. In the history, Dr. Wallis describes, though in a very brief manner, the methods and works of some of the principal authors on this art. Then, in the practical part, he illustrates each author's method, by many pertinent examples, adding also many other things of his own, of considerable ingenuity and importance.

Apologia pro Circuitione Sanguinis; qua respondetur Æmylio Parisano, Medico Veneto, Authore Georgio Entio; Editio altera, auctior et accuratior. Lond. 1685, 8vo. N° 173, p. 1105.

A 2d edition of the learned author's defence of the circulation of the blood. In this epistle to Dr. Harvey, he shows how little truth there is in Father Paul's being the inventor of the circulation; forasmuch as the papers written by him on this subject, and found in his study after his death, were no more than notes taken at the reading of Dr. Harvey's book, which was lent him by a countryman of his lately returned to Venice from England, where he had been ambassador from that state;* and was presented by the author with one of these books; the truth of which appears from a letter written by Father Fulgentio to Dr. Harvey, expressing as much.

An Answer to some Queries proposed by Mr. William Molyneux, concerning Lough-Neagh. By Mr. Edward Smyth, F. of Trin. Col. Dublin. N° 174, p. 1108.

1. Whether Lough-Neagh has really the quality of petrifying wood? To this I answer, that no experiment is yet known, to prove the lough has this petrifying quality; or that the water any way promotes the petrification; but rather that two experiments made by a gentleman of worth and good credit, whose estate lies contiguous to the lough, prove the contrary. For about 19 years ago, he stuck two holly stakes in two several places of the lough, near that place where the upper band enters into it; and that part of the stake, which for so long time has been washed by the water, remains there without any alteration, or the least advance towards petrification; as for that part of the stake which is covered by the mud or earth, he has not yet looked on it, but promises to do it this summer,† taking advantage of the fall of the lough.

2. Whether this quality be equally diffused throughout the whole lough, or be more strong in any particular parts thereof? Because there have been no cer-

* This circumstance has been before noticed in the abstract from Dr. Clarck's letter, inserted in volume 1, pp. 247, 248 of this Abridgment.

† This experiment is inconclusive, as the whole of the stake was not examined.

tain experiments made upon all parts of the lough, and much time required to make this trial, we cannot expect a speedy resolution of this query; Dr. Boat, in his Natural History of Ireland, tells us that his brother informed him, who lived in those parts, that this virtue is found especially about those places, where the black water discharges itself into the lake, but confesses he never could find any person who himself had made the trial, and therefore had this information from report.*

3. What woods are petrified by the lough? or whether only holly? That not only holly, but also oak, and some other wood has been petrified about this lough, and in the soil adjacent, I have sufficient grounds to conjecture on this account; because some fishermen, being tenants of a gentleman from whom I had this relation, told him, they had found buried in the mud of this lough great trees, with all their roots and branches petrified, and some of such size, that they believe they could scarcely be drawn by a team of oxen. They broke off several branches as thick as a man's leg, and larger, but could not move the main trunk. If we may credit this relation, we must allow some other woods to be petrified besides holly, for holly never grows to that size.†

4. Whether the wood or holly, brought from other places, be as apt to be petrified as what grows in the grounds adjacent to the lough?

5. What time is requisite to petrify a piece of a determinate size? I heard of no experiment which can resolve this query.

6. Whether any one has seen the same body partly wood and partly stone? I was informed by two gentlemen of the north that this may be frequently seen, who alleged they themselves had seen the same body wood and stone. But the only reason for thinking so, being the diversity of colours, which might well enough proceed from several degrees of petrifaction, we may probably think them deceived, for they made no experiments on that part which they reputed wood.‡

7. Whether the bark has been seen petrified as well as the wood? The bark is never found petrified, as I am informed by a diligent inquirer, but often somewhat rotten about the stone answerable to the bark.

8. Whether any one has certainly made experiment of the lough's petrifying, by putting a piece of wood into it, and letting it lie there till it was petrified?

* It appears from the observations of Mr. Simon, inserted in the 44th vol. of the Transactions, that the petrifying quality is confined to particular parts of the lake, where the water which springs up from the bottom abounds in a calcareous impregnation.

† It has been already mentioned in a note on Mr. Molyneux's letter, at p. 24 of this vol. of the Abridgment, that petrifactions of other sorts of wood beside holly have been found in this lake.

‡ Mr. Simon's observations, before referred to, are very satisfactory respecting this point.

Several pieces of holly have been put into the lough, but none, that I ever heard of, were ever taken out anywise altered.

9. Whether there be any sand-pits near about the lough in which these pieces of wood, we esteem petrified, are found? I never could hear of any such sand-pits, nor that this petrifying virtue was stronger in any such places; there is a greater quantity of these stones found in the adjacent ground; and when ground is newly broke, usually turned up in ploughing.

10. Whether the earth or sand about this lough be indued with this quality? That this virtue is certainly, if not only, in the ground or soil, I judge for these reasons: that there are many stones turned up daily, especially at their breaking up new ground, which we cannot in any probability think were brought thither; they are often found at 2 miles distance from the lough, seldom further, in great numbers, and very deep in the ground; now for what use and reason they should be brought thither I cannot imagine; but because there may lie exceptions against this reason I shall produce another, which I believe will plainly prove this assertion, it being matter of fact. The gentleman on whose credit I received this information had occasion one day to survey a part of his own land, and at a small distance from the lough he saw a stump of a tree just dug out of the ground, which by handling of it he found petrified; his servant that dug it up, standing by him, told him he had just rooted it out of the ground; he assured me the roots and all were stone, and altogether like those stones that are usually found, and go by the name of Lough-neagh stones. This certainly proves the soil to have this petrifying virtue, which was never yet proved of the water. This gentleman was of opinion these were lapides sui generis, till this observation convinced him; and I believe the wood, which I before mentioned that was found by the fishermen petrified, owes its petrifaction to the soil, and not to the water. But that these stones were once wood, is I think very certain, for they show the plain vestigia of wood, they likewise burn and cleave; filings of this stone thrown in the fire emit a fragrant smell; they cut kindly with a knife, though not so easily, as other wood; but had they none of these properties, the instance now alleged, I think, is as convincing as demonstration.

Historiæ Convulsionum Periodicarum per Clariss. D^num^ Guil. Cole, M. D. descriptæ, et communicatæ. Convulsionis Quintanam observantis Periodum. Abridged and translated from the Latin. N° 174, p. 1113.

A lady, who had been the mother of several children, was taken in labour rather before her time; and after an alarming flooding was delivered of a dead child. This event was followed by convulsions, which attacked her at intervals for many months afterwards. At length she got better, but not so much so as to be entirely free from the convulsions. After this she experienced an aggravation of her complaints, the convulsions observing at first tertian, then quartan, and afterwards quintan periods, i. e. with an interval of 4 complete days between each paroxysm, and this with the utmost regularity. The convulsions lasted for an hour or two, and were preceded by some degree of rigor, and a copious discharge of limpid urine. The same kind of evacuation took place as the convulsive paroxysm ceased, when the patient fell into a sleep which continued for several hours. When she awoke, she was wholly unconscious of what had happened. For 3 days after each attack she remained so extremely weak, as scarcely to be able to raise herself up from her bed or chair, much less to walk across the room without the help of her attendants. The cure was at first attempted by testaceous medicines and Sp. C. C. (from a suspicion of acidity) but as these remedies proved to be of no service, it occurred to Dr. Cole, that it would be proper to resort to the treatment adopted in the case of intermittent fevers. Accordingly he had recourse to the Peruvian bark, by means of which (in conjunction with the testaceous and volatile medicines before mentioned) the paroxysms were completely removed.

2. *Historia Convulsivi Affectus octonarium Periodum à multis Annis observantis, per Clariss. D^{num} Guil. Cole, M. D. descripta et communicata. Abridged and translated from the Latin.* N° 174, p. 1115.

Dorothy Cook, a widow 60 years old, was seized with epileptic fits 36 years ago, 3 days after her marriage. These fits recurred frequently at first, but at no stated periods. After some months they came on about the full and new moon, at which periods she had several attacks in a day. Some months afterwards, instead of coming on once a fortnight, the fits seized her twice a week, viz. on Thursdays and Saturdays. Soon after her marriage she became pregnant, and was brought to bed, at the usual time, of a child, who died epileptic a short time afterwards. Her second child died in the same way; but although she bore several children afterwards, none of them were subject to attacks of this sort.

The fits had continued in the manner above-mentioned for about 3 years, when they were stopped for some months by a quack medicine. They then returned again in consequence of a fright, and the same remedy was again resorted to, but without success. By the assistance, however, of a celebrated practitioner, Dr. Johnson, the fits were a second time removed, and she continued to enjoy her health until the famous battle of Worcester, in 1651, when the scenes of horror and bloodshed consequent to the taking of the city of Worcester (where she lived) by Cromwell, threw her, as well as the rest of the inhabitants, into the utmost consternation, and brought on a return of the fits; which at first observed no regular periods, but afterwards attacked her in the order above-mentioned, resisting the remedies which had before proved successful. The fits continued to observe this type for 2 years, when in consequence of another fright they changed their day of attack, at first seizing the patient twice a week, and afterwards on Sundays only, and in this manner they have continued ever since. Wearied with repeated disappointment, she cannot now be prevailed upon to take any more medicine.

One remarkable circumstance must not be omitted, viz. for a long time past she has taken it into her head, that if she went out of doors she should be immediately seized with a fit; she therefore constantly confines herself to her own house. About 20 years since she accepted an invitation to spend the evening at a neighbour's; but no sooner had she entered the door, than she was attacked (though it was not the stated time for a paroxysm) with a violent fit, as she had apprehended, and was obliged to be carried home again. From that time to the present moment, she has never stirred from her own door, except when she was carried to her new house, 18 years ago; on which occasion (though it was not the stated day of an attack) she was seized with fits, which continued for 10 successive days, (2 fits each day) till they at length returned to the order they had before observed.

Notwithstanding the patient has been afflicted so many years with this disorder, yet, in the intervals between the paroxysms, her intellects do not appear to be at all impaired; and she manages her domestic concerns as well as if she had never been subject to such attacks.

Abstract of a Letter of Mr. Leewenhoeck, F. R. S. dated March 30, 1685; *concerning Generation by an Insect (Animalcule).* N° 174, p. 1120.

About the latter end of the summer of the year 1683, I took the semen masculum of a dog, which was of about a year and half old. This I put into a glass pipe, and wrapped it up in soft leather, because the nights were something cold. This semen I observed 4 days successively, and in the first, I found that several of its animals were dead. In the second and third day, there were yet more of them dead, but on the fourth, there were very few left alive; and so far I proceeded at that time. But in the beginning of October in the year 1684, I observed again the semen masculum of the same dog, who was then very strong and vigorous, and I found that after 7 days and nights, there were some few animals yet left alive, a very few whereof swam as briskly as if they had just come from the dog.

From hence I infer, that these animals would have lived a much longer time if they had been in the uterus; also that conception in females is not always made immediate post congressum, but sometimes 9 or 10, or more days after it, if one of the animals can then find the punctum or proper place for its nourishment: and lastly, that the uterus may not be fit for the reception of animals at one time, though before 2 or 3 days are passed, it may become perfectly capable.

I know my opinion about generation has been rejected by several persons, some whereof being skilled in anatomy have affirmed to me, that the semen masculum never comes into the uterus; that it is never to be seen in that place; and that it is nothing but a vapour which causes fruitfulness. But I am not of these men's minds; for it seems plain to me, that the parts are proportioned by nature for carrying the semen into the uterus. And therefore wolves, foxes, and dogs are fastened; and though the dog at first sheds a little thin clear moisture, yet that is not a true semen, for I could not find any animals in it. But as he is fastened, there drops very leisurely a little semen, which is white, by reason of the great number of animals contained in it: and this would not reach the uterus, by reason of its coming so slowly, and of the swelling of the penis, unless the dog were fastened. I remember that Dr. Grew, in a letter to me, used some words to this purpose: Harvey in his book de Gen. Animal. denies that he ever found in the uterus anatomised immediately after conception, the semen masculum. And Dr. De Graaf, in his book de Part. Muliebr. Gen. inservientibus, constantly asserts, that the semen masculum is nothing but a vehicle of a certain volatile salt, or such like spirit, conveying to the egg of the female a contactum vitalem. But though a late writer has reckoned up the authorities of 70 persons, who have asserted the same opinion, and that of the ovarium, yet I think they have been all mistaken. As will appear by the following trials.

A bitch was delivered to me after she had been once lined. The next day she was lined again at 8 A. M. in my presence, and again at 2 o'clock P. M. whereupon I caused her to be killed, by running an awl into the medulla spinalis near the head. As soon as she was dead, I bound her legs to a table, and opened the vagina where I found a white substance, which I took out, and viewed with my microscope, discovering it to be nothing but scales, of that sort which cover the inside of the vagina, lying in a clear thin liquor. Afterwards I opened the middle of the cornu on the left side, which was about 3½ inches long, and as thick as a good quill, as at K, pl. 5, fig. 6. Had I only looked with my naked eye, I should have said there was no semen of a dog in it, but when I made use of a very good microscope, I saw to my great satisfaction, a very great number of the living animals of that semen. I then opened the cornu towards the end, as at L, and saw there likewise a good quantity of the semen which was very lively. I opened the cornu on the right side, and found a like number of the living animals. Then I opened that part of the uterus which is between the vagina and the cornu, as at H, which part was $1\frac{2}{3}$ of an inch long: and here I found, in the matter lying on the inside of the uterus, a greater number of the living animals than before, and I cannot think they were less than 100 millions.

After 3 hours and $\frac{1}{2}$ I went to communicate this my discovery to an eminent physician and anatomist, who could not be of De Graaf's opinion about the descent of the eggs out of the ovarium, though he had been present at his dissections. In his presence I opened the uterus in another place, and showed him the substance contained therein, having in it a great number of the living animals of the semen masculum, though the weather was very cold and freezing. I likewise showed him one of the plexus commonly called the ovarium, wherein were several bloody spots, caused by the breaking of some of the water bladders, or glandulous particles, of which they consist: which broken water bladders were still included in a very strong membrane; the said physician remaining convinced that generation was more clearly explained after my way, than by eggs sucked out of the ovarium.

I opened the vagina uteri, and caused it to be so drawn, with the uterus, and ovarium, or testicles; the vagina and uterus are not exhibited of the full length, to avoid occupying too much place on the paper; but their breadth is in due proportion, as also the ovarium with the glandulous particles, and water bladders. ABCD is the pudendum of a bitch opened. DEFG is the vagina uteri opened, which was 5 inches long. This I first examined in the middle about F, to see whether there were any of the semen masculum in that place, but I found none at all, but instead thereof a great deal of scales, which probably belonged to the inward skin of the vagina, and were worn off; also several globules of that sort which makes the blood red, and a great many globules of a less sort. I then examined it about E, but found only the same substance.

Lastly, I examined it about G, close by the uterus, where I found very few animals; but the scales were there very plentiful, being at least 25 scales to one animal. These last animals I suppose were only such as were scattered in the way, when the penis of the dog was drawn out.

From these observations it may be concluded, that not the vagina, but the uterus itself, is made for the reception and inclosing of the semen masculum; lest the animals should swim back, and come into the sheath, which would be unprofitable ground; for the animals had swam forward in the uterus 5 inches and $\frac{1}{3}$, which is the distance from GHIK to L.

I opened likewise the whole length of the uterus, as PQ, seeking for the bodies which they say are sucked from the ovarium in generation; but I found none. For if it be granted, that the largest eggs are the ripest, as in all animals that are oviparous, I should not have had much trouble to discover them, if they had continued so great round bodies as those in the ovarium LMNO, or such bodies as are represented in the ovarium QRS.

The uterus of a bitch is, from IK to L, almost of an equal thickness; but at L it becomes immediately very small, contrary to the uterus of a sheep; and how is it to be comprehended, that such large round bodies as the eggs can pass through so narrow and wrinkled a passage, and not only one of them, but several together; as in a bitch, which being but once lined, brings several puppies. That I might satisfy myself herein, I separated the thinnest part of the tuba from the testicle, and bound a thread about the tuba where it grows thick; partly that in the mouth that sucks down the supposed eggs, (where I have put a great pin to show the cavity) I might pour quicksilver, for discovering how much the thin stiff tuba would stretch itself; and partly that I might know whether the round membrane which contains it, and reaches from L to O, as ON, had a hollowness.

In fig. 7, VWX is the thin end of the tuba. T is the thread bound about the thick part of the uterus. X is the opening where the quicksilver is put in. The tuba did not stretch itself any wider than is here represented, nor did it lose that crinkled form, which it had while it was in the ovarium, as represented at W. The uterus at TV stretched itself very much, by the quicksilver, and from thence I am convinced that it has no other passage than at X; for if it had, the quicksilver would have run out of it.

Hence will appear the impossibility that the great glandulous bodies in LMNO and QRS, in fig. 6, should go through the small passage XWV in fig. 7, till they come to the widening of the uterus; and I must confess I could not find any round particle in it, that was larger than a red-making globule of the blood.

I procured another bitch, which had been proud 3 days, in which time they said she had been lined thrice, and when she had been lined a 4th time in my presence, I opened the uterus in the same manner as I had done the other. But though I found a great quantity of the semen masculum, I could not find any body larger than a blood globule. Neither was there in the vagina uteri any thing except what I have formerly mentioned.

I understand that De Graaf, when he anatomised female rabbits, used to judge how many young would next have been brought forth, by the number of red specks in the ovarium, though none of the eggs were to be found in the uterus. He was also of opinion, that the eggs first came down in the uterus, 4 days after the female had taken the buck. But it is strange that rabbits should be matured in 30 days, and yet the eggs first be seen in $7\frac{1}{4}$ parts of the time.

In the year 1678 I sent to the Royal Society the draught of one of those numerous animals, which are in the semen of a dog; noting that they are so small that 1,000,000 are not equal to a coarse sand. I shall here again insert the same figure, that you may see how I imagine these animals came to be round in the uterus. I need not mention how several insects cast their skin, and how tadpoles, as they grow in size, lose their tails, and appear to be frogs.

In fig. 8, EFGH is one of the animals of the semen of a dog, as I saw it swimming. IKLM, fig. 9, is one of the animals when dead. I have often observed these last animals to change their figure, chiefly when the moisture about is exhaled. When one of these animals has got so far in the womb, that it finds a place fit to nourish it; then may it suddenly cast off its tail, or its whole outward covering, and remain of an oval form.

I believe that what is called frigidity in some men, is either a want in them of the animalia seminis, or a weakness in the animals, which renders them unable to live long in the womb. If this be true then, that a fœtus is an animal seminis masculi, we may reject the vulgar opinion, that a conception comes to be male or female according to the prevalency of one seed above the other, since the animalia seminis are themselves of both sexes.

In the beginning of February I received from a butcher, the uterus of a sheep, which had taken the ram they said 17 days before. I found in one of the cornua a small body wrapped up in membranes of a red fleshy colour. But being not able to distinguish its figure, though I put it in a glass tube about the thickness of a goose quill, together with oil of turpentine; I took it out of the glass again, and as it lay round, I gently stretched it out in length, and then I plainly saw the back bone, with blood vessels lying near it, and sending forth several branches, also in two distinct places the medulla spinalis lying in the back-bone. And in the head, I saw not only the maxillæ, but the muscles that covered them, and the vessels lying in them, though they were not yet red and looked like sinews. Likewise the brain, and the small blood vessels therein; the eyes about the size of a coarse sand, but as clear as crystal, I opened the belly, and took out several pieces of the bowels, which were transparent, and about the size of a small hair. I saw several strings of flesh, which I took to be the muscles: when I cast my eye on the breast, to see the ribs, I found the place so full of blood vessels, that there was nothing else to be seen.

This whole animal I judged to be no larger than $\frac{1}{8}$ part of a pea. And if so small a creature be furnished with all its parts, there can be no reason to doubt, that the same creature has them, though he be a 1000 times less, and the parts are not distinguishable by the sight. And if an insect has limbs and bowels, though it is not 1,000,000,000th part so large as a sand, then may the animalia seminis have likewise the same limbs and bowels, which the fœtus has when brought forth.

Further, I took out of the ovarium, which was on the same side where the fœtus lay, 2 round red globular bodies, commonly called eggs, each about the size of a pea, and consequently each egg about 8 times as large as the said whole animal. These supposed eggs, though I took them from between membranes, were each wrapped in a skin or membrane, having blood vessels running through it; and on opening the eggs, I found they consisted chiefly of a glandulous substance, and every glandule likewise consisted of other lesser glandules.

I bought a female rabbit, and let it take buck 3 times in my presence, and then killed it; but did not open the womb, till a quarter of an hour after. About an inch from the beginning of one of the horns, I found a little fluid matter, containing some few living animals of the male seed; but in the parts where the cornu joins to the vagina, the living animals were very numerous; in the middle of the cornu, and toward the further end, there were no animals at all, nor any thing to be seen, but a few blood globules swimming in a little fluid matter. The reason why the animalia seminis were not throughout the cornu, I believe may be, that the seed had not been long enough in the womb and therefore the animals had not time to disperse themselves all over it.

The other horn being yet untouched, I wrapped it up in a piece of paper, and put it in a small box which I carried in my pocket, the weather being cold, from 9 o'clock in the morning till 3 in the afternoon, at which time I opened it, but found it little different from the other horn, either in the number of the animals, or in their progress in it. I opened the womb a little below the horns, and found a greater number of the animalia seminis than I had yet seen; but in the outermost part of the vagina there were no animals at all. In both the horns laid open, I could find no round body larger than a blood globule.

I then endeavoured to discover the make of the animals, but it proved a work of great difficulty, yet I saw that their bodies are not oval but flattish. Fig. 10, ABC, is the body of a dead animal of the male seed of a rabbit, which I had taken out of the womb of the female. In the said bodies, there oftimes appeared 6 bright risings of a globular form, placed after the manner they are here drawn; but the bright rising or globule nearest the tail was generally larger than the rest, as it is represented at C. Though some of the bodies were without these globules, yet I could sometimes perceive that at the tail. CD is the tail lying in bends, as it usually is when the animal swims.

In any of the several seeds which I have observed, I cannot say that the animals are ever at rest, as long as there is any fluid matter left to move in; but their motion is continual, till it decreases gradually as they come to die. There are some animals which have also a small globular substance in the tail, but it is not to be discovered without great attention.

In fig. 11, EFG is the body of an animal, having only one globule at the tail, and another in the tail. GH is the tail of the animal.

I have said that the male seed of the rabbit was in the beginning of the horns, but not throughout, as in bitches; that I might be sure of this observation, I let a female rabbit take buck twice in half an hour's time, and then let her live 6 hours after, at which time I killed her, in the presence of a physician, who had been a companion of De Graaf in his dissections of rabbits. When I had taken out the womb, with the appertenances, I opened one of the horns in 3 several places, at the beginning, in the middle, and at the farther end; and took out of each place some of the matter, which I showed him in a microscope; satisfying him, that the living animals of seed were throughout the horns, and did not only move, but discover the distinct motion of their tail.

I showed him likewise the supposed ovarium, in which were no red spots, but a great many water bladders as large as a pin's head. I likewise showed him the matter contained in the water bladders, which was nothing but a tranparent moisture mixed with some bood globules. The other horn of the womb being not then opened, I laid it in a box on a moist paper for 16 hours, and then I found in it a great many living animals. But in one place there were 25 dead, for one alive; and in another place 100 dead, for one alive. After 5 hours more, I looked again, but then the animals were all dead.

I again put a rabbit to buck, and after 2 full days killed her, searching all the horns of the womb, and finding only a few animals, but nothing else remarkable, saving that there were no eggs; some round particles lay here and there, but they were 1000 times less than a sand. I saw likewise some blood vessels in the cornua, which were so small, that a blood globule could not pass through them, without being broke into 64 parts. When I looked on the ovarium with my naked eye, I thought I saw 8 or 9 red spots in it, but when

I looked with a common microscope, I saw they were water bladders, one redder than another, and containing some bloody matter, which consisted of small glandulous parts, joined together with membranes, having many globules of blood spread among them, by which one of them was become blood red.

Afterwards I killed a rabbit which had been bucked 6 days before; and when I opened one of the cornua uteri, I saw therein a round water bladder about the size of a barley corn, having a tender membrane, seeming to be composed of globular parts, and showing through a common microscope like the grain of shagrin; this I imagined to have come from an animal of the male seed, but when I opened it, and searched through all the contained watery matter, I could make nothing of it, but that I thought I saw something having the form of a rabbit, but 1000 times less than a sand. Yet I cannot be positive, since in the other horn of the womb, I found 2 such round bladders which had not the same appearances. When the watery matter began to evaporate, I saw a great number of 6-sided figures, whose sides rose up pyramidally, like those described in my last letter, N° 3, fig. 4. being clear as a polished diamond: about them lay beautiful brown globules. There were also some salts having a square basis, and others an oblong, but their number was few and scarcely worth noticing. I examined the ovarium, and found in it 2 or 3 water bladders which lay deep, and were half hid, and one that was reddish, the rest that stuck out more, being the greater number, were of an ash-colour, and consisted of glandulous parts. I looked also how the eggs might have a passage to come down, but could find none.

I received from a butcher the uterus of a sheep, which was said to have taken the ram 3 days before. As I opened each of the cornua, the moisture ran plentifully out, which I put separately in 2 dishes, searching with my common microscope, till I found a small body of about the size of a coarse sand. At first I could perceive no figure it had; but when I applied it to a good microscope, it proved to be a young lamb, with the head and back bent forward, but in very good order; the jaw-bone and eyes were very distinct, but the back bone much less.

From all the preceding observations I am more and more confirmed, that the supposed ovariums are only instruments for the disburthening the parts thereabout; for if they were true ovariums, the eggs would first be little when the animals are young, and become gradually larger against the time that the animals come to procreate, as it is in birds that lay eggs. That I might satisfy myself and others herein, I examined calves of 3, 4, 5, and 6 weeks old, and found the eggs as large as if they had been of a full grown cow, being like peas. I also found that the eggs in a sucking lamb were larger than in a full grown sheep, and the glandulous parts and water bladders as many. I have been often considering, whether the opening of the tuba might not be to give way for the evacuating some moisture, at the time the uterus is closed, and which cannot get through the vagina, for I have sometimes found in the thin part of the tuba near the opening a white matter, consisting of transparent globules, a little larger than a blood-globule.

There lately came to my hand a book called Collectanea Medico-Physica, in which, cent. 5, p. 8, are some words to this purpose. But it is most surprising what the learned Bontekoe relates from the curious Leewenhoeck, that the sperm of men is full of small children, and so the sperm of other animals, according to their kind. But this is a mistake, for I have only said it is full of animals, or worms with long tails, whose figure I have often shown. For as it is not proper to say, that worms swimming in the water are flying insects, though they come from them; or to say that the kernel of an apple is a tree, though a tree comes from it; so it is no less improper to say, that the worms in men's seed are children, though children come from them. It is often said that a tree may be seen in the kernel of an apple; but I must profess, that a seed, besides the radicle where the root springs, consists of roundish transparent globules, which when it grows, turn into cylindrical bodies, being vessels to carry the nourishment to the radicle; but when that is sufficient to draw its nourishment from the earth, the remaining part of the seed does no more service to the plant, but dries away. It is true, when we examine several seeds, we find in them two or more beginnings of leaves, as small as a small sand; these lie folded up, and show in a microscope like the beak of a sparrow; but when they are unfolded, we then see the beginnings of other leaves, which grow not till the root is larger. These last observations have been made on seeds, just taken out of an apple, or a little dried. But if you cut up a Turkey bean, though every so dry, the two leaves are visible to the naked eye; and if you use a microscope, you see the ribs or vessels of the leaves, for they are partly made to stiffen it, and partly to carry nourishment for its growth, &c.

Dissection of a monstrous double Cat. Read before the Dublin Society by Dr. Mullen. N° 174, p. 1135.

This monstrous kitten was double from the navel downwards, having 4 hind feet, 2 tails, 2 anuses, and 2 pudenda, for they were females. They were joined in one trunk at the navel, and were continued so upwards; but yet this monster had 2 pair of fore feet, one of them on the back and the other on the breast. The head, though single, had 2 pairs of ears, one naturally situated, and another at the hinder part of the head, between the processus mammillares, to which the vertebræ of both the necks were joined, for there were 2 back-bones continued all the way to the head. Though the 2 bodies seemed to be but one entire body above the navel. There was only one stomach under the liver in the right side, reaching under another liver in the left. The guts were single till within 6 or 7 inches of the anus, and then a division into 2 branches, one going to each fundament; below the division there were plainly to be seen 2 cæcums, each within about 3 inches of the anus. There were 2 livers, one much smaller than the other, that which was in the right side was the least, the other lay lower down in the left side. They were both entire, without any division or lobes. There was a vena umbilicalis inserted into each of them. There were 2 arteries inserted into the liver in the left side, both coming from the aorta, and these I shall call the cœliacæ. There was only one inserted into the liver placed in the right side. There was no vena cava below the livers. for all the veins coming from the lower parts entered the livers as the vena porta does naturally. There was a branch of a vein on each side, proceeding from the loins, inserted into the back parts of the liver, and besides these there was not a branch to be seen but what was inserted into the middle of the livers.

There were no vesiculæ felleæ that I could find, and perhaps the reason I could not discover any was, because of the tenderness of the livers, for they were putrefied before I got the kitten. There were 2 kidneys on each side, furnished with ureters. There was neither spleen nor pancreas in either side. There was a double diaphragm meeting in the middle, between the 2 back-bones, and making a membrane, which seemed to be a mediastinum, for it reached up to the thymus. There were 2 hearts in it, one placed above the other, and a little to the right side; it was much higher than ordinary, and had a vein coming to it from the little liver in the right side, which (together with 3 other small veins, one from each of the fore-feet and one from the head) furnished this heart with what blood was to be circulated by it. It had only one auricle and one ventricle, so that it seemed to be but half a heart. There was a pretty large passage into the arteria aorta, the contrivance of which was very singular; for above this heart it was made like an arch of a circle, into which there was a direct passage from the heart for the blood. When I further examined this artery, I found that it went down on each side on the vertebræ of the backs, between the kidneys, and divided itself on each side after the usual manner, after it had lent each kidney a branch, the liver in the right side one, and the liver in the left side two. Below the former, a little towards the left side of it, there was another half heart, having only one auricle and one ventricle, like the former. This received little blood, but what was transmitted from the large liver in the left side, by what is called the truncus ascendens of the vena cava. The artery carrying the blood from this heart was inserted into the artery lately described, as well as that of the other heart. So that if the blood circulated through either of them, the whole animal must necessarily be supplied with blood; a contrivance not unlike that of the arteries under the brain, where the arteriæ carotides and vertebrales empty themselves into one common channel, from which all parts of the brain may easily be supplied with blood. The head was joined to 2 necks about the processus mammillares. There were 4 orders of ribs, though the body was but one above the navel. That which outwardly seemed to be a back, was really the place where the ribs met, and which might be rather called the breast, though it wanted a sternum.

Account of Books.

1. *Michaelis Ettmulleri* Opera omnia Theoretica et Practica, &c. 4to. Lond.* 1683. N° 174, p. 1140.

In treating of any disease, this author notices, 1. The history of the distemper. 2. The part affected. 3. The causes. 4. The prognostics. 5. The method of cure.—He looks upon it to be of the greatest consequence to distinguish between the animal faculty, placed in the spirits and genus nervosum, and the vital, whose seat is in the blood and viscera. He commends Sylvius, Willis, Sennertus and Helmont. The work is divided into 3 parts, to the last of which are subjoined his Chirurgia Medica and Collegium Consultatorium; besides an Appendix, consisting of several tracts.

* This physician, who acquired great reputation as a teacher and an author, and who travelled in the pursuit of knowledge through the most enlightened parts of Europe, was born at Leipsic 1644, (not 1646.) He died when only 39 years of age, of a hectic fever, brought on by his unremitting exertions for the advancement of his profession. Ettmuller was much devoted to the chemical theories of his days, and his pathology, founded thereon, now ceases to be received. He bestowed much attention on pharmaceutical chemistry, and wrote commentaries on Schrœder and Ludovicus; but, although in some instances he animadverted upon certain compositions in the dispensatories of those authors, remarkable for the number and contrariety of ingredients; yet he concurred with them in praising many substances described in their materia medica, which every man of reflection must condemn as useless, disgusting, superstitious, absurd: such, for instance, as the calculus humanus, as a diuretic and sudorific; the blood of a hunted hare, as a remedy against epilepsy; broth made from the spleen of an ox, against suppression of the menses; and the internal use of the dung of various animals, in cases of jaundice, vertigo, colic, &c. We cannot therefore allow to Ettmuller the merit of having rendered the materia medica less redundant or more rational than he found it; nor of having contributed much to that which until within these few years was so greatly desired, the simplification of officinal and extemporaneous formulæ.—More complete editions of his works than that above noticed were afterwards published, one by the author's son in 3 vols. folio, 1708; and another by Cyrillus, professor of physic, at Naples, in 4 vols. folio, 1728 and 1736.

2. *Raymundi Vieussens,* D.M. Monspeliensis Neurographia Universalis, fol. Lug.* 1685. N°.174, p. 1144.

This author divides the brain into two parts, outward and inward. He begins with the crassa meninx, which he makes to be double. He denies the pia meninx to have any glands, is very particular in the distribution of the vessels of it, and will not allow the rete mirabile, on which he has a distinct chapter, to consist of any thing besides arteries. The brain is divided into the cinereous or glandulous part; and the medullary or fibrous, which from its figure and use he calls the oval centre of the brain; he considers the corpus callosum as the true fornix; and having given the description of all the internal parts of the brain, he gives likewise their use, founded on their structure, and so passes to the cerebellum, in which he describes both the processus vermiformes, and gives an account of the valvula major cerebri, &c.

After the anatomy of the brain, he discourses on the office of the brain and its parts, on the necessity and nature of the animal spirits, succus nerveus, &c. And treats briefly of the animal faculties, concluding the 1st book with a chapter on judgment and reason.

The 2d book treats of the medulla spinalis, which he considers as the prolongation of the brain, and as the foundation of sensation, motion, and nutrition, of the parts below the head.

The 3d book treats of the nerves; in the last chapter of which we meet with observations on muscular motion. The work is illustrated by plates.

On the Cause of the Winds and of the Change of Weather. By Dr. Garden of Aberdeen. N° 175, p. 1148.

There are continual easterly winds under the line called breezes; and there-

* In that difficult department of anatomy, the brain and nerves, Vieussens (who was physician to the hospital at Montpellier) acquired great and deserved celebrity. Many of his predecessors, as Haller observes, had in their investigation of these parts, been content with dissections of the quadruped race; but this anatomist justly conceived that in different animals the structure of these parts must vary considerably, and therefore that an accurate knowledge of them, in relation to ourselves, was only to be acquired by dissections of the human subject; from which he accordingly took all his descriptions and plates. Besides the Neurographia, above-noticed, he wrote several other treatises, some in Latin, others in French, viz. on fermentation and digestion, on the blood, on the structure of the viscera, on the heart, on the ear, &c.; but none of them add much to the fame which he acquired by his first publication, and some of them abound in fanciful and erroneous theories. A 4to vol. of his posthumous works came out in 1774, from which it appears that he had paid great attention to morbid as well as natural anatomy; as we find related therein, the histories of many patients whose bodies he had examined after death.

fore the Spaniards, in going to the West Indies, sail southwards from Spain, along the coast of Africa, till they pass beyond the tropic of Cancer, within 20° of the line, where they usually find an easterly wind, and then they sail on westward, with full winds, so that they scarcely need to shift their sails in the whole voyage; and this they give as the reason why the voyage from Spain to the West Indies is shorter, more easy, and more certain, than the return to Spain. In the South-sea also, going from New Spain or Peru, to the Philippines, or China, the voyage is easy, sailing always from east to west near the line, where the easterly winds blow right a-stern. Acosta relates, that in the year 1584, there went a ship from Calloa in Lima to the Philippines, which sailed 2700 leagues out of sight of land, and this in two months, with a constant breeze, their course being almost always under the line.

Now these continual easterly winds between the tropics, I suppose, proceed both from the motion of the earth and the vertical influences of the sun, in this manner. As the vast fluid and æther, in which the earth floats in its annual motion, moves forward with the earth in that motion, or rather carries the globe of the earth along with it; so the atmosphere, and a large vortex of æther beyond the moon, go round with the earth in its diurnal motion, which, though according as it is removed from the earth it may be proportionably slower in its motion, yet that portion of the atmosphere which is nearest the earth and surrounds it, may be supposed to keep equal pace with the earth in its motion; and if there were no changes in the atmosphere's gravity, I suppose it would always go along with the globe of the earth from west to east, in a uniform motion, which would be wholly insensible to us. But that portion of the atmosphere under the line being extremely rarefied, its spring expanded, and so its gravity and pressure much less than the neighbouring parts of the atmosphere, and consequently incapable of the uniform motion to the east, it must needs be pressed westward, and make that continual breeze from east to west between the tropics. Again, on this side the tropic, about 28 or 30°, there are to be found constant westerly winds; and therefore the Spanish fleets from the West Indies do not return the way they went, but both from Peru and New Spain they sail along the coast northward till they touch at the Havannah in Cuba, and rendezvousing there, they sail still higher without the tropics, where they find westerly winds, which serve them till they come in view of the Azores, and from thence to Seville. In like manner in the South-sea, those who return from the Philippines or China to Mexico, in order to have the western winds, they sail up a great height till they come right against the islands of Japan, and discovering California, they return by the coast of New Spain to the port of Acapulco, from whence they departed. So that though they sail easily from east to west in both seas within the tropics, because the eastern winds prevail there, yet returning from west to east they must seek the westerly winds without the tropics, in the latitude of 27°.

Now the reason of this seems to be clearly deducible from the former; for the pressure of the air between the tropics being continually less than that of the neighbouring parts of the atmosphere, and so consequently by them pressed westward; its motion from west to east is proportionably increased beyond that uniform motion it would have, if the whole atmosphere were of an equal density, and consequently there will blow a constant wind from west to east, for some degrees beyond the tropics.

Again, the easterly winds between the tropics it seems do not blow constantly from the same point, nor directly from the east; but for the one half of the year, viz. from about April till November, they come from the south-east, and for the other half of the year, viz. from November to April, they blow from the north-east. And these are called the monsoons and trade winds. Hence it is that those who sail from China, Japan, &c. to Bantam, must wait for the northerly monsoon, which falls between November and April; and those who return from Bantam must go back again when the southerly monsoon comes, which is between April and November. And the currents of the seas are said to observe the same motions and changes with the winds.

Now these monsoons may be easily accounted for from what has already been said about the cause of the continual easterly winds between the tropics; for since the diminution of the pressure of the air under the line, and that of the neighbouring parts of the atmosphere, cause these continual breezes, if the sun were constantly in the equinoctial, it is probable the wind would always blow directly from the east; but as he is the one half of the year on the one side of the line, and the other half on the other, there must necessarily follow a change of these breezes into stated monsoons. For if we imagine the atmosphere to be divided into two equal hemispheres by the equinoctial plane, and if the sun were always in the plane, there would be constantly an equal pressure from both these hemispheres on the air under the line, and the breeze would be directly from the east. But when the sun comes on the north side of the line as far as the tropic of Cancer, and back again, there is not an equal balance, but the pressure of the southern hemisphere of the air must needs be greatest, and consequently the breeze must blow all that season from the south-east; and when the sun returns again to the southward of the line, as far as Capricorn, and back again, the pressure of the northern hemishphere must preponderate, and cause the wind to blow all that half year from the north-east. And this seems to accord very well with experience, for their northern monsoons are in our winter season, when the sun is in the southern signs; and their southern ones in our summer, when he is in the northern signs.

The rivers Indus and Ganges, where they enter the ocean, contain between them a large peninsula, which is divided in the middle by a ridge of high hills, called the Gatæ, which run along from east to west, and quite through to Cape Comorin. On the one side is Malabar, and on the other Coromandel. On the Malabar side, between that ridge of mountains and the sea, it is what they call summer from September till April; in which time it is always a clear sky, with little or no rain. On the other side the hills, on the coast of Coromandel, it is at the same time their winter, every day and night yielding abundance of rain; and from April to September it is on the Malabar side their winter, but on the other side their summer; so that in little more than 20 leagues journey in some places, as where they cross the hills to St. Thomas, on the one side of the hill you ascend with a fair summer, on the other you descend with a stormy winter. The like is said to obtain at Cape Razalgate in Arabia. And Dr. Trapham relates the same of Jamaica, intimating that there is a ridge of hills running from east to west through the middle of the island, and that the plantations on the south side of these hills have from November to April a continual summer, whilst those on the north side, have as constant a winter, and on the contrary from April to November.

From these and such like accounts, it seems evident that a bare diminishing of the atmosphere's gravity will not cause rain, but that there is also requisite either a sudden change of winds, or a ridge of hills to meet the current of the air and vapours, whereby the particles of the vapours may be driven together, and so fall down in rain. And hence it happens, that whilst the wind blows from the north-east, viz. from November to April, there are continual rains in the northerly plantations of Jamaica, and on the side of Coromandel in the East Indies; because the winds beat against that side of the hills: and so there is fair weather on the other side, there being no winds to drive the vapours together. But in the southerly monsoon, viz. from April to November, Malabar and the southern plantations of Jamaica have floods of rain, the wind beating against that side of the hills; whilst in Coromandel and the other side of Jamaica there is fair and clear weather. The maps make those mountains of Gatæ run south and north; and if so, the monsoons must blow from other points, by reason of the neighbouring countries and islands, or else this is not the true cause of these seasons.

This serves also to clear up the singularity of seasons in Peru beyond any other parts of the earth, and seems to be assigned by Acosta as the cause of it.

Peru runs along from the line southwards about 1000 leagues. It is said to be divided into 3 parts, long and narrow, called Llanos, Sierras, and Andes; the Llanos or plains run along the South-sea coast; the Sierras are all hills with some valleys; and the Andes are steep and craggy mountains. The Llanos are about 10 leagues in breadth; the Sierras about 20 leagues, and the Andes the same; they run in length from north to south, and in breadth from east to west. These parts of the world are said to have these things remarkable. 1. All along the coast in the Llanos it blows continually with one only wind, which is south and south-west, contrary to that which usually blows under the torrid zone. 2. It never rains, thunders, snows nor hails in all this coast, though there falls sometimes a small dew. 3. On the Andes it rains almost continually, though it be sometimes more clear than at other times. 4. In the Sierras, which lie between both these extremes, it rains from September to April, but in the other seasons it is more clear, which is when the sun is farthest off, and the contrary when it is nearest. Now the reason of all seems to be this: the eastern breezes which blow constantly under the line being stopped in their course by the Sierras and Andes, and yet the same breezes being to be found in the South-sea beyond Peru, as appears by the easy voyages from Peru to the Philippines, a current of wind blows from the south on the plains of Peru, to supply the eastern breeze in the South-sea; and there being but one constant gale in these plains, and no contrary winds, nor hills to beat upon, this seems to be the reason why the vapours are never, or very seldom, driven into rain. And the Andes being as high perhaps in many places as the vapours ascend, in the highest degree of the atmosphere's gravity, this may probably be the reason why the eastern breeze, beating constantly against these hills, cause rains upon them at all seasons of the year. And the Sierras being it seems lower than the Andes, therefore from September to April, when the sun is nearest, and so the atmosphere's gravity less, and the vapours lower, they are driven against the Sierras and form rain.

The causes of those particular, various, uncertain and inconstant winds, which blow without the tropics, and that most frequently in mountainous countries, and more seldom in great plains, such as Poland, cannot easily be conjectured: but those general winds, which usually take place every where about the two equinoxes, seem to proceed from some general cause, and this I take to be the change of the monsoons and trade winds, about these seasons, between the tropics. For about these seasons, there must be a change of the balance of the atmosphere, which must produce strong winds over all the earth.

The Figures of some Antiquities; communicated by a Member of the Royal Society. N° 175, p. 1159.

In pl. 5, fig. 12, is a ring of Corinthian brass with a vizard of Silenus in a sardonix. Fig. 13, an amulet of the gnostics, cut in a chalcedony, the names of certain æones or intelligences. Fig. 14, a tessera of crystal, having on one side XIII, on the other 1 △. Fig. 15, a Fortune and a half Diana, in an onyx. Fig. 16, an ancient picture of the virgin in a chalcedony. Fig. 17, a cameo, with a mixture of several gods. Fig. 18, an onyx. Fig. 19, a glass lachrymatory.

A Letter of Dr. Lister's to Mr. Ray, concerning some Particulars that might be added to the Ornithology. N° 175, p. 1159.

I send you an abstract of my notes about birds, which may be added, if you think fit, to yours and Mr. Willughby's History of Birds.

Fringilla montana major. Sea larks nostris aucupibus appellantur. Sc. in agro Eboracensi sæviente bruma sat copiose capiuntur.

Rubeculæ excrementa semper liquida sunt; etiamsi illa ovorum luteis duriter coctis victitet; intestinis cæcis caret, aut ea valde exigua et inutilia habet. Muscarum autem cortices, et quæ ex id genus victu facile coqui et eliquari in ventriculo non possunt; figura rotunda, pisorum in modum rejicit.

Morinellus anglorum. Hæ aves bis in anno loca paludosa relinquunt et se in campos montosos gregatim recipiunt, Sc. Aprili et Augusto.

Transmigrationis autem causa ex victu pretenda est. Nam tum temporis aves quoddam cibi genus ibidem reperiendum appetunt; ut autem id certo scirem, plurium ventriculos dissecui, in quibus maximam partem scarabæos quosdam minutos et nigricantes inveni, at inter alia cochleas terrestres exiguas adverti.

Anserum ferorum 5 genera vulgo numerantur in agro Eboracensi satis frequentium. 1. The little Spanish goose, Sc. æque parvus ac brenta, sed figura et colore quodammodo ad anserem domesticum accedens, ab Hispania denominatur. 2. The barnicle, satis cognitus. 3. The Scotch goose, Sc. vulgatissimus ferus, à Scotia ad nos, exeunte Augusto, adveniens, è quibus fere constant innumeri illi greges qui in planis campis (the Wolds dictis) et alibi passim inveniuntur. 4. The whilk, anser maximus niger, ineunte bruma primum adveniens; raro alibi quam in pratis, pascuisque herba pascitur. 5. Anser palustris noster, grey lagg, dictus; huic magnitudo anseri domestico subpar. Caput ex fusco nigricat, et ad medium collum infuscatur; dorsum ex cinereo livescit; ipsæ autem alæ, et earum remiges nigricant; uropygium albescit, ejusque pennæ e x ternæ albidæ sunt: venter cinereus; is vero imus sensim fit niveus; rostrum à capite ad mediam fere partem nigrum, deinde subpurpureum, ipso ejus apice nigro: in superiore mandibulo non nisi unus denticulorum ordo, atque idem simplex ordo in inferiore; item huic lingua utrinque uno denticillorum ordine armatur. Pedes subpurpurei sive carnei coloris, ungues fere albidi, excepto medii digiti, qui ex majore parte nigricat. Pendet libras 7, et fere dimidium. In paludibus agri Eboracensis nidificant, ipsi et earum pulli mense Maio pinguescunt, et in deliciis habentur.

Ad lanios ea avicula referenda est, quam à Germanis silk-tail vulgo appellari audio; harum unam aut alteram sclopeti glande transfixam, Eboraci, exeunte Januario anni 1680 vidi. Sane perquam elegans avicula est, magnitudine turdum fere æquabat; huic in extremis alarum pennis 4 aut 5 apices parvi coccinei nudi, cornei, minime plumis vestiti; item in extrema cauda limbus latus, luteus instar corticis citri, cætera maximam partem lanium colore refert.

Hos lanios in Prussia frequentes esse aiunt mercatores nostri.

I will not affirm the grey lagg to be different from the common wild goose; your description and mine so well agree, save in the colour of the bill and legs. The design of the lanius I can send you done from the skin.

I have since viewed the head of the bird called silk-tail at Mr. Charlton's, and find it to want the notchs in the upper part of the bill proper to the lanius kind. It must therefore be removed, and put amongst the jays.*

Abstract of a Letter from Dr. Cole of Worcester, dated May the 13th, 1685: Concerning Stones voided per Penem. N° 175, p. 1162.

Two stones, of the size of fig. 20 and 21, pl. 5, were voided by the penis, from a man who for many years had been subject to great pain, first in the kidneys, and afterwards in the bladder.

Account of a Book.

Johannis Hevelii, Consulis Dantiscani, Annus Climactericus. Gedani 1685, in folio, wherein, among other things, he vindicates the justness of his Celestial Observations, against the exceptions by some made to the accuracy of them. N° 175, p. 1162.

This accurate astronomer laments the great calamity he suffered by fire, Sept. 26, 1679; when, in a few hours, his houses, 7 in number, with all they contained, his money, plate, gold, silver, and all his household goods, his

* It has however a small notch on each side the tip of the bill. It belongs to the genus *ampelis* of Linnæus, and is the *ampelis garrulus* of the Systema Naturæ.

printing-houses, with the furniture thereof, and great part of his library; also the remaining copies of all his printed works, published at his own expence, from the year 1647 to 1679, and particularly his dear Urania, and all its observatories, with all his instruments, astronomical and optical, and many other things of great worth, were in a manner wholly consumed, and reduced to ashes. He is thankful however that, before this calamity, he had, the same year, finished and published the latter part of his Machina Cœlestis, containing the observations of almost 50 years, and which are thereby preserved: that some of his papers, particularly his new catalogue of the fixed stars, were wonderfully preserved from the fire: and, that he has taken courage to resume his former studies, to rebuild his observatory, and furnish it with necessary instruments, though much inferior to those incomparable ones that perished by the fire; and to apply himself to deduce a-new, from their first originals, much of what was destroyed relating to his Prodromus Astronomiæ, his correction of the tables, his Uranographia, and his new celestial globes.

This present piece, the first published by him since that dreadful conflagration, he calls his Annus Climactericus, being the 49th year of his observations, and because of that great revolution of affairs which in that year befel him; the beginning of which, and the greatest part of it, being much to his content, but the end of it so sad and dismal.

Among the happinesses of that year, he reckons, first, his finishing and publishing the latter part of his Machina Cœlestis, containing the observations of 48 years. And then Mr. Halley's visit to him at Dantzic, for some months; and the great satisfaction it afforded him. Having before much desired that some such skilful person from the Royal Society at London, acquainted with the way of observing there, by telescopic sights, might repair to him at Dantzic, to witness the manner of his observing, by plain sights, and the naked eye. Who, having thus been an eye-witness of both ways, might satisfy that Society of the comparative merits of both; at least, that his way of observing was not so despicable as might by some be thought or pretended. And he particularly rejoices that the observations then made in Mr. Halley's presence, and with his assistance, were wonderfully preserved from the fire, by which so many other things perished.

For there had some years before happened a controversy between Mr. Hook and him, by letters, concerning the excellency of telescopic sights, which Mr. Hook much preferred before the plain sights, used by Tycho, by the Landgrave, by all observers heretofore, and by this author; as if it were not possible, with these sights, to make observations nearer than to 2 or 3 whole minutes: whereas he alleges, that himself with telescopic sights, by an instrument only of a span breadth, could make observations, 30 or 40, or 50, and even 60 times more accurate, than could be done the other way with the largest instruments. And, on the author's publishing that first part of his Machina Cœlestis, wherein all his instruments are accurately described, Mr. Hook published his animadversions thereon; with much more of bitterness and boasting, as this author thinks, and others also whom the author cites, than there was reason for: thus seeking to raise his own reputation by disparaging what is done by others, in things wherein himself does nothing.

The author thinking the credit of his observations to be herein much concerned; he complained hereof in his letters to several learned men, and particularly to the Royal Society, desiring that justice might be done him in this point. For that a difference of this nature was not to be determined by conjectural speculations, which a man may project to himself; but by practical experience, and trials actually made both ways; and these duly examined and compared by persons competent to judge of such matters.

In order to this, the author had often and earnestly desired that Mr. Hook would vouchsafe to give him at least these 8 distances, observed by his instruments, great or small as he pleased, to be compared with the same observed by this author: viz. 1. Lucidæ Arietis à Palilicio; 2. Hujus à Polluce; 3. Pollucis à Regulo; 4. Reguli à Spica Virginis; 5. Hujusque à superiori in manu Serpentarii; 6. Hujus ab Aquila; 7. Hujusque à Marcab; 8. Atque hujus demum à Lucida Arietis: but could never obtain from him either these or any other; though it could be no hard request to grant, if Mr. Hook could, as he says, perform according to his method 100, or 200 observations in one night.

He had from Bulliald, Buratin, Fullen, and others, who had been eye-witnesses of his observations, and assistant at them, great attestations of their accuracy: others who had not so been present, had yet a great esteem for them, notwithstanding the exceptions made to them by Mr. Hook; blaming his censure as undeserved and too severe: and others even of those who had a great opinion of telescopic sights, and did themselves make use of them, had yet a far other opinion of his instruments and observations, than what had been expressed by Mr. Hook; and thought at least this demand of his very reasonable, that it might be free for either to make use of such sights as themselves thought best, or were best acquainted with, without taking upon them to prescribe to others, or to reproach and vilify them because not of the same opinion with themselves.

In consequence then of his request, the Royal Society sent Mr. Halley to Dantzic, where he arrived May 26, 1679, and there continued with the author till July 18; being all that time a constant attendant at his observations; and had with him a very good instrument of his own, furnished with telescopic sights, the better to compare together the two ways of observation.

The same day that Mr. Halley arrived, he did in Mr. Halley's presence, as a specimen of his manner of observing, take, with his large brass sextant, the distance of Regulus and Spica; which he found to be 54° 1′ 55″, the same exactly, even to seconds, which he had six times before observed in the years 1658, 1661, 1671: at which Mr. Halley being much surprized, it was again for his greater satisfaction, that he might not think it only a piece of good luck, observed the same exactly, June 1, and again June 7.

Many more observations were made from day to day, during all the time of his abode there, such particularly as Mr. Halley directed. To which he attended constantly, and strictly examined them, that he might not by any mistake be imposed on. Which observations are all here particularly set down. And among the rest, a list of several observations made by the author, with a small instrument and common sights, and the same made by Mr. Halley, with a much larger, and telescopic sights: by which the author thinks it will appear to an indifferent judge, that those performed by the plain sights, though in a smaller instrument, are the more accurate. And he professes that he could not, with Mr. Halley's instrument, make observations with equal accuracy and quickness, as with his own.

Upon the whole matter, Mr. Halley finding every thing to his satisfaction, and beyond his expectation, thought fit to leave in writing his attestation thereof, in form of a letter to the author, dated July 8-18, 1679, declaring himself "abundantly satisfied of the use and certainty of these his instruments and observations. And whereas he had before been always doubtful, that his observations by naked sights might, as to some minutes, be uncertain; and had therefore wondered why he declined the use of telescopic sights; he had, partly to gratulate the author's publishing of his observations, and partly to satisfy his own scruples, undertaken that journey; which he now considers as no small happiness; and declares himself abundantly pleased with it. And offers himself a voluntary witness, of the almost incredible certainty of those instruments. As having seen, with his own eyes, not one or two, but a multitude of observations of the fixed stars, performed with his great sextant, even by divers observers, and by himself sometimes, though less expert therein, being often repeated, most accurately, and almost incredibly to agree; and never to differ more than by an inconsiderable part of a minute." With further expressions of joy and admiration; as wondering at nothing more, than to find them so accurate.

The author being, by these observations, confirmed in his resolution of adhering to bare sights, with his naked eye, without glasses; and having satisfied Mr. Halley as to the certainty and accuracy thereof (far beyond what Mr. Halley could expect;) thought fit in the first place (after his Machina Cœlestis) to publish these, for the satisfaction of others, to preserve the just reputation of his observations before published, which Mr. Hook had endeavoured to render suspected.

He therefore gives, first, his obversations of the year 1679, beginning from Jan. 8, N. S. where his Machina Cœlestis ended; and so onwards till May 26, when Mr. Halley arrived. And therein, among others, the transit of some stars, and the occultation of some others, by the moon: March 25, March 30. From thence, to June 18, is an account of all his observations made with Mr. Halley, with their success. And thence to Sept. 26, the fatal day when his observatories, with all their furniture, were destroyed; which concludes the observations of that year.

After these observations, of the year 1679, are 27 letters which had passed between the author and several other learned men, relating to the controversy between him and Mr. Hook, about the use of telescopic sights; in which letters are contained the whole particulars of this notable controversy.

With regard to the subject of diagonal divisions, he observes, that the division of an angle into equal parts by straight diagonals, obliquely cutting concentric arches in the limb, would require, in mathematical rigour, that the concentric circles be set at somewhat unequal distances; and in small instruments, where the breadth of the divided limb is a considerable part of the radius, as $\frac{1}{5}$, or $\frac{1}{10}$ of it, and the angle to be so divided, of a considerable size, suppose 10 minutes or more, it may require some little difference of intervals. But where the instrument is large, as here, 6, 8, or 10 feet radius, and the breadth of the limb to be divided, but narrow, as here about half an inch, and (as here) the angle to be so divided but 5 minutes; the true intervals according to mathematical rigour, are not to be distinguished by the clearest sight from equal distances; for no sense can distinguish the one from the other. And if there had been any difference; the author had sufficiently provided for it, by performing the same divisions by straight lines from the centre also. The computation of Dr. Wallis, to which the letter refers, is to be seen at large in the Philosophical Transactions, N° 111.* On this principle the author makes a calculation, by which it appears that the differences are far too small to be in any way distinguished.

* Volume 2, p. 189, of this Abridgment.

Having dispatched what properly concerns his Annus Climactericus, and the letters relating to it, the author gives the continuation of his observations since that time: relating to several astronomical phænomena, as new comets, solar and lunar eclipses, occultations of stars by the moon, &c.

A course of Chemistry. By Nicholas Lemery, M. D. Translated from the 5th Edition in French. By Walter Harris, M.D. Fellow of the College of Physicians. Lond.* 1686. N° 175, p. 1183.

This edition is not only adorned with several tables of figures, representing the supellex chymica; but is also enlarged by the addition of divers operations; as particularly the pulverisation of tin, by casting it, when melted in a crucible, into a round wooden box, which has been whitened with chalk on all sides within; then covering the box, and presently shaking it about, until the tin is become cold, and converted into a grey powder; in which form it easily mixes with salts, and other matters. It also teaches the making of flowers of jupiter; which consist of tin volatilized, and raised in the form of meal, &c. and the preparation of an oil of mercury, by dissolving corrosive sublimate in spirit of wine; which may be done, although that spirit is not able to dissolve quicksilver, nor mercurius dulcis. A caustic oil of antimony is taught to be made by dissolving antimony in the acid spirits of salt and vitriol.

* This chemist, who was born at Rouen in 1645, after receiving some instructions in pharmacy, in his native town, travelled to Paris (where Glaser at that time gave lectures), and afterwards to Montpelier. At this place he remained 3 years in the house of an apothecary, where he had the best opportunities of making chemical experiments. He returned to Paris in 1672. Here he eagerly engaged in the prosecution of that branch of experimental inquiry to which he was so much attached. He connected himself at first with the Prince of Condé's apothecary, and consequently had the use of that nobleman's laboratory; but afterwards he established a laboratory of his own, where he delivered a course of lectures to a large concourse of pupils; and acquired considerable sums not only by them, but also by the sale of his chemical preparations, which were in very great request. In 1675 he published his Cours de Chimie, which soon went through many editions, and was translated into various languages. When he came over to England, in consequence of the persecutions exercised against the protestants in France, he presented the work just mentioned to Charles II, and had the honour of kissing his Majesty's hand on the occasion. In 1697 appeared his Pharmacopée Universelle, and his Traité des Drogues; and in 1707 his Traité de l'Antimoine, a work which exhibited its author in a very conspicuous point of view as a chemical experimenter, and added not a little to the knowledge which had been before acquired concerning the properties of this metallic body, and the compounds produced from it. In particular he discovered an improved method of obtaining what was termed kermes mineralis; an antimonial preparation which has been much celebrated for its medicinal virtues. Lemery died in 1715, leaving a son who became physician to Lewis XV, and held the professorship of chemistry in the King's garden.

A method is given for drawing an oil and spirit of paper. An account of the Peruvian bark is inserted; with the manner of drawing a tincture, and making an extract of it. An account is given of sugar, and the spirit of it, and of phosphorus.

Collegii Experimentalis, sive Curiosi, Pars Secunda; per J. Christ. Sturmium. Alt. 4to. 1685. N° 175, p. 1184.

The author, in this second part of his collection, describes many new experiments and philosophical instruments; as a glass diving bell; some amendments in the air-pump, particularly of that kind which is portable; some experiments tried on the baroscope, polished planes, &c.; some hygrostatical experiments, together with the description of the instruments by which they were performed; of long siphons, and their use in conveying water; of glass bubbles, and the reason of their flying into little pieces on breaking; of water pots; a sort of lamps; and of stentoreophonic tubes; the invention of which he justly ascribes to Sir Samuel Morland; he speaks of a new sort of thermoscope; of the Magdeburg hemispheres, mentioned by Guerik, and the difficulty of separating one from the other, when the air included in them is rarefied. He treats of the force of breath blown into a bladder, and raising a considerable weight; and explaining muscular motion by this principle, after Dr. Croon's manner, proposing that by these means a statue of a man may be made to move in imitation of nature. He examines the experiment of the hydria helmontiana, urged by Dr. More, as an argument for his hylarchic principle. He describes an aerometer, consisting of an hygrometer, thermometer, and barometer. Speaking of magnetical experiments, he affirms, that he has a large needle, exceeding a rhinland foot, and 7 inches, both ends of which apply to either pole of the magnet.

To this treatise the author subjoins an epistle to Dr. More of Cambridge, concerning his hylarchic principle; where he examines the Doctor's demonstrations of that principle, and answers the arguments against the elasticity of the air.

Account of a strangely self-moving Liquor. By Mr. Boyle, F. R. S. N° 176, p. 1188.

An ingenious teacher of mathematics, having occasion to make a composition for a new fire-engine, mixed several ingredients in an earthen pot over kindled coals; but incautiously suffered the matter to take fire: having stifled the flame as hastily as he could, and removed the vessel from the fire, and suffered it to grow cold; when afterwards he came to look upon it, to see if what remained might be of any use to him, he was surprized to find it variously and briskly moved. Wherefore having set it aside, to be certain that it should be thoroughly cold, he after some hours visited it again, and found it move as before. And having cast a quantity of seeds upon it, to see if the liquor would move them also, the bituminous part of it connected them into a kind of thick scum, that covered most of the superficies; but yet left some intervals, in which the liquor appeared, and discovered that it continued its motions. Two days after, the engineer told me of this odd accident. And though it was then a dark night and bad weather, my diffidence or my curiosity made me engage him to send for the pot as it was; partly to be sure of the matter of fact, and partly to try if the knowledge I had of the ingredients, which he had before told me, would afford any hint of the cause of so odd an effect; alike to which in kind, though not in degree, I had many years before devised, and successfully practised, the way of producing.

The vessel being come, though the hasty transportation of it seemed to have sufficiently disturbed it, there appeared manifest signs of such a motion, as the engineer had ascribed to it; and therefore having set it aside in a laboratory, where some furnaces kept the air constantly warm, and did there and elsewhere, at distant times, look heedfully upon it, now and then displacing, or quite taking off, some of the thick scum; and by this means I had the opportunity to take notice of several phænomena, whereof these are the chief.

First I observed, that the motion of this liquor was not only brisk, but very various; so that having loosened some small portions of the scum from the rest, one of them would be carried towards the right hand, for instance, and another towards the left, at the same time.—2. Where the liquor first came out from under the scum, it seemed to move the most briskly, flowing almost like a stream, whose motions upwards had been checked, and as it were reverberated, by that incumbent obstacle.—3. Several motions in this liquor were the more easy to be observed; because though it were dark, yet it was not uniform, consisting in part of oily and bituminous ingredients, which though they seemed to have but one common superficies with the rest of the liquor, yet by their colours and power of vigorously reflecting the light, they were easily enough distinguishable from the rest. And I often observed, that some of these unctuous portions of the matter, emerging to the surface of the liquor, though perhaps at first one of them would not appear larger than a pin's head; yet in moving forwards it would at the same time diffuse itself circularly, and make as it were a great halo, adorned with the colours of the rainbow, and so very vivid, as afforded a very pleasant and surprizing spectacle; these phantasms often nimbly succeeding one another, and lasting till they lost themselves against or under the thick scum.—4. The motions of this odd liquor were not only various, but frequently vortical; to be satisfied of which, I sometimes put short bits of straw, &c. on the discovered part of the surface of the liquor, by which they were carried towards very distant, if not opposite, parts of the vessel at the same time. But to make the vortical motion more evident, I several times detached pieces of the thick scum from the rest of the body, and had the pleasure to see them move both with a progressive motion in crooked lines, and with a motion about their own middlemost parts. All this while the liquor, whose parts were thus briskly moved, was actually cold, as to sense.—5. To observe what the presence, or absence, of the free air would do to this liquor; I caused many spoonfuls of it, with some of the scum, to be put into a cylindrical glass, which though large itself, had a neck belonging to it, that was but about the size of one's thumb, that it might be well stopped with a cork. But having by this means kept the free air from having a full and immediate contact with the whole surface of the mixture, as it had when that mixture lay in the wide-mouthed vessel; I could not perceive the liquor to move to and fro, even though the orifice of the neck were left open: whereas, having at the same time poured some of the liquor into a very shallow and wide-mouthed vessel, it moved rather more nimbly and variously than in the great earthern pot, and showed many of those vivid and self-dilating circles before-mentioned. And these, by the fineness of their colours, and the quickness with which they succeeded each other, afforded a delightful spectacle as long as I staid to observe it.—6. Though the motions of the hitherto mentioned liquor did not seem to be always equally brisk, yet they appeared to continue manifest and various in some diversities of weather, as to cold and heat; and equally the same by candle light, as by day light. And thus the motion continued in the liquor as long as it was observed, which was at several times for 5 months after, when any longer observations were prevented by the vessel being accidentally overturned, and the liquor wasted.

On the Collection of Secants, and the true Division of the Meridians in the Sea-Chart. By Dr. Wallis, F. R. S.* N° 176, p. 1193.

An old inquiry, about the sum or aggregate of secants, having been lately renewed, I have thought fit to trace it from its original: with such solution as seems proper to it: beginning first with the general preparation; and then applying it to the particular case.

* On this subject, see Vol, I. p. 69. Also Gregory's Exercitationes Geometricæ.

General Preparation.—Because curves are not so easily managed as straight lines; the ancients, when they contemplated figures terminated, at least on one side, by a curve line, either convex or concave, as A F K E, fig. 1, 2, pl. 7, they often made use of some such expedient as this following, viz. By parallel straight lines, as A F, B G, C H, &c. at equal or unequal distances, as there was occasion, they divided it into as many segments as they thought fit; or supposed it to be so divided: which segments were less by one than the number of those parallels. To each of these parallels, wanting one, they fitted parallelograms, of breadths equal to the intervals, between them and the next following: which thus formed an adscribed figure made up of those parallelograms. Then, if they began with the greatest, and therefore neglected the least, such figure was circumscribed, as fig. 1, and therefore larger than the curvilinear figure proposed. If with the least, neglecting the greatest, the figure was inscribed, as fig. 2, and therefore less than that proposed. But, as the number of segments was increased, and thereby their breadths diminished; the difference of the circumscribed from the inscribed figure, and therefore of either from that proposed, continually decreased, so as at last to be less than any assigned. On which they grounded their method of exhaustions. In cases wherein the breadth of the parallelograms, or intervals of the parallels, is not to be considered, but their length only; or, which is much the same, where the intervals are all the same, and each reputed = 1: Archimedes, instead of inscribed and circumscribed figures, used to say, All except the greatest, and All except the least. As Prop. 11. Lin. Spiral.

The particular Case.—Though it be well known, that in the terrestrial globe all the meridians meet at the pole, as E P, E P, fig. 3, whereby the parallels to the equator, as they be nearer to the pole, continually decrease: and hence a degree of longitude in such parallels is less than a degree of longitude in the equator, or than a degree of latitude; and that, in such proportion, as is the cosine of latitude, which is the semidiameter of such parallel, to the radius of the globe, or of the equator: yet it has been thought fit, for certain reasons, to represent these meridians in the sea-chart, by parallel straight lines; as E p, E p. Whereby each parallel to the equator, as L A, was represented in the sea-chart, by l a, as equal to the equator E E; and a degree of longitude therein, as large as in the equator. By this means, each degree of longitude in such parallels, was increased beyond its just proportion, at such rate as the equator, or its radius, is greater than such parallel, or its radius. But in the old sea-charts, the degrees of latitude were still represented, as they are in themselves, equal to each other, and to those of the equator. By which, among many other inconveniences, as Mr. Wright observes, in his correction of errors in navigation, the representation of places remote from the equator, was greatly distorted in those charts. For rectifying this in some measure, Mr. Wright advises, that (the meridians remaining parallel, as before) the degrees of latitude, remote from the equator, should, at each parallel, be protracted in like proportion with those of longitude; that is, as the cosine of latitude, which is the semidiameter of the parallel, to the radius of the globe, which is that of the equator, so should a degree of latitude, which is every where equal to a degree of longitude in the equator, be to such degree of latitude so protracted, at such distance from the equator; and so to be represented in the chart: that is, every where in such proportion, as is the respective secant, for such latitude, to the radius: for, as the cosine is to the radius, so is the radius to the secant of the same arch or angle; as fig. 4, $C : R :: R : \sigma$. So that, by this means, the position of each parallel in the chart should be at such distance from the equator, compared with so many equinoctial degrees or minutes, (as are those of latitude,) as are all the secants, taken at equal distances in the arch, to so many times the radius. Which is equivalent, as Mr. Wright observes, to a projection of the spherical surface (supposing the eye at the centre) on the concave surface of a cylinder, at right angles to the plane of the equator; and the division of meridians, represented by the surface of a cylinder erected on the arch of latitude at right angles to the plane of the meridian, or a portion of it: the altitude of such projection, or portion of such cylindric surface, being, at each point of such circular base, equal to the secant of latitude answering to such point; as fig. 5. This projection, or portion of the cylindric surface, if expanded into a plane, will be the same with a plane figure, whose base is equal to a quadrantal arch extended, or a portion thereof, on which, as ordinates, are erected perpendiculars equal to the secants, answering to the respective points of the arch so extended: the least of which, answering to the equinoctial, is equal to the radius; and the rest continually increasing till, at the pole, it be infinite; as at fig. 6. So that, as E R S L, a figure of secants erected at right angles on E L, the arch of latitude extended, is to E R R L, a rectangle on the same base, whose altitude E R is equal to the radius; so is E L, an arch of the equator equal to that of latitude, to the distance of such parallel, in the chart, from the equator. For finding this distance, answering to each degree and minute of latitude, Mr. Wright, as the most obvious way, adds all the secants, as they are found calculated in the trigonometrical canon, from the beginning to the degree or minute of latitude proposed. The sum of all which except the greatest, answering to the figure inscribed, is too little; but the sum of all except the least, answering to the circumscribed, is too great, which is what he follows: and it would be nearer the truth than either, if, omitting all these, we take the intermediates; for $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. min. or the doubles of these, 1, 3, 5, 7, &c. min. Which yet, because on the convex side of the curve, would be somewhat too little. But any of these ways are exact enough for the use intended, as causing no sensible difference in the chart. If we would be more exact, Mr. Oughtred directs, and so had Mr. Wright done before him, to divide the arch into parts yet smaller than minutes, and calculate secants adapted to them. Since the arithmetic of infinites has been introduced, and in pursuance thereof the doctrine of infinite series (for such cases as would not, without them, come to a determinate proportion), methods have been found for squaring some such figures, and in particular the exterior hyperbola, by way of continual approach, by the help of an infinite series. As, in the Philos. Transact. N° 38,* and my book De Motu, cap. 5. prop. 31. In imitation of which, it has been desired by some, that a like quadrature for this figure of secants, by a proper infinite series, might be found.

In order to which, put r for the radius of a circle, s for the right sine of an arch or angle, v for the versed sine, $c = r - v = \surd : r^2 - s^2$: for the co-sine or sine of the complement; $r - v = \sigma$ for the secant; t for the tangent. Then is $c : r :: r : \sigma$, or $\sigma = \frac{rr}{c}$ the secant; and $c : s :: r : t$, or $t = \frac{rs}{c}$ the tangent. Now, if we suppose the radius C P, fig. 7, divided into equal parts, and each of them $= \frac{1}{8} r$; and on these to be erected the co-sines of latitude L A: then are the sines of latitude in arithmetical progression; and the secants answering to them, $LS = \frac{rr}{c}$. But these secants, answering to right sines in arithmetical progression, are not those that stand at equal distances on the quadrantal arch extended, as fig. 6, but standing at unequal distances on the same extended arch; viz. on those points thereof, whose right sines, whilst it was a curve, are in arithmetical progression, as fig. 8.

To find therefore the magnitude of R E L S, fig. 6, which is the same with that of fig. 8, supposing E L of the same length in both, however the number of secants therein may be unequal; we are to consider the secants, though at unequal distances, fig. 8, to be the same with those at equal distances in fig. 7, answering to sines in arithmetical progression. Now these intervals, or portions of the base, in fig. 8, are the same with the intercepted arches, or portions of the arch, in fig. 7; for this base is only that arch extended. And these arches, in parts infinitely small, are to be reputed equivalent to the portions of their respective tangents, intercepted between the same ordinates; as in fig. 7, 9; that is, equivalent to the portions of the tangents of latitude: and these

* Vol. I. p. 273, of this abridgment

portions of tangents, are to the equal intervals in the base, as the tangent of latitude is to its sine.

To find therefore the true magnitude of the parallelograms, or segments of the figure, we must either protract the equal segments of the base, fig. 7, in such proportion as is the respective tangent to the sine, to make them equal to those of fig. 8; or else, which is equivalent, retaining the equal intervals of fig. 7, protract the secants in the same proportion: for, either way, the intercepted rectangles or parallelograms will be equally increased: as LM, fig. 9; viz. as the sine of latitude is to its tangent, so is the secant, to a fourth; which is to stand on the radius equally divided, instead of that secant: that is, as $s : \frac{sr}{c}$ or $c : r :: \frac{rr}{c} : \frac{r^3}{c^2 = r^2 - s^2}$ = LM, fig. 9.

And this, because $c^2 = r^2 - s^2$, and the sines s in arithmetical progression, is reduced by division into this infinite series, $r + \frac{s^2}{r} + \frac{s^4}{r^3} + \frac{s^6}{r^5}$, &c. That is, putting $r = 1$, the series $1 + s^2 + s^4 + s^6 +$, &c.

Then, according to the arithmetic of infinites, we are to interpret s, successively, by $1s$, $2s$, $3s$, &c. till we come to s, the greatest, which therefore represents the number of all. And because the first member represents a series of equals; the second, of secundans; the third, of quartans, &c: therefore the first member is to be multiplied by s; the second by $\frac{1}{3}s$; the third by $\frac{1}{5}s$; the fourth by $\frac{1}{7}s$, &c: which makes the aggregate, $s + \frac{1}{3}s^3 + \frac{1}{5}s^5 + \frac{1}{7}s^7 + \frac{1}{9}s^9 +$ &c. = ECLM, fig. 9. And this, because s is always less than r or 1, may be so far continued, till some power of s become so small, as that it, and all which follow it, may be safely neglected.

Now, to adapt this to the sea-chart, according to Mr. Wright's design: having the proposed parallel of latitude given; we are to find, by the triogonometrical canon, the sine of such latitude, and equal to it, take CL = s; and by this find the magnitude of ECLM, fig. 9; that is, of RELS, fig. 8, or of RELS, fig. 6. And then, as RRLE, or so many times the radius, is to RELS, the aggregate of all the secants; so must a like arch of the equator equal to the latitude proposed, be to the distance of such parallel, representing the latitude in the chart, from the equator: which is the thing required.

The same may be obtained, in like manner, by taking the versed sines in arithmetical progression. For if the right sines, as here, beginning at the equator, be in arithmetical progression, as 1, 2, 3, &c.; then will the versed sines, beginning at the pole, as being their complements to the radius, be so likewise.

The Collection of Tangents.—The same may in like manner be applied to the aggregate of Tangents, answering to the arch divided into equal parts. For, hose answering to the radius so divided, are, $\frac{rs}{c}$, taking s in arithmetical progression. And then, enlarging the base, as in fig. 8, or the tangent as in fig. 9. in the proportion of the tangent to the sine; as $s : \frac{rs}{c} :: c : r :: \frac{rs}{c} : \frac{r^2s}{c^2} = \frac{r^2s}{r^2-s^2}$. Hence, by division, we have this series, $s + \frac{s^3}{r^2} + \frac{s^5}{r^4} + \frac{s^7}{r^6} +$, &c. That is, putting $r = 1$, the series $s + s^3 + s^5 + s^7 + s^9$, &c: which multiplying the respective members by $\frac{1}{2}s$, $\frac{1}{4}s$, $\frac{1}{6}s$, $\frac{1}{8}s$, $\frac{1}{10}s$, &c. becomes $\frac{1}{2}s^2 + \frac{1}{4}s^4 + \frac{1}{6}s^6 + \frac{1}{8}s^8 + \frac{1}{10}s^{10}$, &c. which is the aggregate of tangents, to the arch whose right sine is s. And this method may be a pattern for the like process in other cases of similar nature.

A Letter from Mr. S. George Ash, Sec. of the Dublin Society, to one of the Secretaries of the Royal Society; concerning a Girl in Ireland, who has several Horns growing on her Body. N° 176, p. 1202.

This infirmity did not show itself on the subject of this letter, viz. Ann Jackson, till she was about 3 years old, after which the mother concealed her out of shame, and brought her up privately; but she soon dying, and the father becoming exceedingly poor, the child was left as a charge upon the parish. She is now between 13 and 14 years of age, yet can scarcely go, and is so little in stature, that I have seen children of 5 years old taller; she is very silly, speaks but little, and that not plainly, but hastily, and with difficulty; her voice is low and rough: her complexion and face well enough, except her eyes, which look very dead, and seem to have a film or horn growing over them, so that she can hardly now perceive the difference of colours.

The horns abound chiefly about the joints and flexures, and not on the brawny fleshy parts of her body; they are fastened to the skin like warts, and about the roots resemble them much in substance, though toward the extremities they grow more hard and horny; at the ends of each finger and toe grows one as long as the finger or toe, not straight forward, but rising a little between the nail and the flesh (for near the roots of these excrescences is something like a nail) and bending again like a turkey's claw, which also it much resembles in colour; on the other joints of her fingers and toes are smaller ones, which sometimes fall off, others growing in their places. The whole skin of her feet, legs, and arms, is very hard and callous, and grows always more and more so; on her knees and elbows, and round about the joints, are many horns; two more remarkable at the point of each elbow, which twist like rams' horns; that on the left arm is above ½ inch broad, and 4 inches long; on her buttocks grow a great number, which are flat by frequent sitting; at her arm-pits and the nipples of her breasts small hard substances shoot out, much slenderer and whiter than the rest; at each ear also grows a horn; the skin of her neck of late has begun to turn callous and horny, like that of her hands and feet. She eats and drinks heartily, sleeps soundly, and performs all the offices of nature like other healthy people, except that she never had the evacuation proper to her sex.

Historia Ulceris in Inguine dextro, Intestinorum Fæces emittentis: à Dno Gui. Earnshaw, Medico Alcestrensi, descripta et communicata. Translated from the Latin. N° 176, p. 1204.

A woman who lived at Alcester, in Warwickshire, and who was about 40 years old, returning home from a neighbouring town was suddenly seized with a violent pain in the right groin, followed by hickup; and half an hour afterwards a tumour was perceived there as large as a nutmeg: this tumour became gradually hard, and at length black. The patient was so much harassed by fever and pain, as to be deprived of her recollection and understanding; and she was publickly prayed for, as one whose life was despaired of. At length, in consequence of the application of a cataplasm, the inguinal tumour broke, and from its aperture there was voided, for the most part unchanged, whatever she eat or drank, within a quarter of an hour or half an hour afterwards, without any pain, either in the ulcer itself or in the intestines. Thus one day while she was taking some boiled milk, there issued from the ulcer first some of the milk unchanged, then some that was curdled, with a frothing and crepitus (tanquam ab ano); another time, when she had eaten some chicken and parsley, they were both voided by the ulcer. On being called in, I found her in an emaciated hectic state, but having at stated times evacuations by urine and stool, without pain. The ulcer was 3 or 4 fingers in length, and 1 finger in breadth, but of no great depth. An opening ptisan was prescribed, the first dose of which came away by the ulcer, without producing any evacuation per anum; the next day a purgative bolus was given, part of which was discharged in the same way half an hour afterwards. Stools however succeeded, and less came away through the ulcer. The next day the same bolus was repeated, producing 3 copious stools, containing many scybala. After this, exsiccating traumatic drinks were given, with occasional repetitions of the aforesaid bolus; by persisting in the use of which for about a fortnight, the patient got perfectly well.

Part of a Letter, containing a further Account of the Aqueduct near Versailles, &c.* N° 176, p. 1206.

Labourers are every where at work about Versailles, making magazines for the waters to be conducted thither; as, one upon the mountain of Montboron already cut, which will have 2200 perches of surface, each perch being 18 French feet; and 12 feet in depth. In another place, much lower, will be another magazine, to receive the waters of many pools. In the valley of Buc will be an aqueduct, the middle of which will be raised 22 fathoms high, for conveying the pools of Sarle. This aqueduct is 300 fathoms long, and passes through two mountains, which have been cut through for that purpose. The valley also on both sides of the aqueduct is raised 11 fathoms high, to make passages.

An aqueduct also is making near the tower of stone, where the mills raise the water, which now will pass without force to the top of the mountain, and be there part of it distributed into several very large cisterns, which are making above Marli for that place.

The elevation of the aqueduct of Mantenon is now set forth at only 2560 fathoms, whereas it was designed to be carried on more than 8000 fathoms, and the remainder to be made of earth, which must be brought thither. These 2560 fathoms contain 242 arcades, the aperture of which is 6½ fathoms, and the face of each pillar, sustaining the arches, 4 fathoms; there will be then on the side of Mantenon 33 single arches, afterwards 71 double ones, being one arch upon another, then 46 treble ones, which will generally be 216½ feet high, viz. up to the floor of the channel; afterwards 72 double ones; then 20 single, reaching to the mound of earth which is to be 50 feet high.

From the ground up to the second arcade are 16 fathoms, from the second to the third, or upper arcade, are 14 fathoms, which arcades are double in number to those they stand upon, and 6½ fathoms more to the floor of the channel, which will at least be 6 feet high, besides the parapet.

The pillars at the ground are 8 fathoms thick, but what with the slopes and shortenings which are made in every story; the top where the channel goes will be but 20 feet broad: there will be also at each pillar a buttress jetting out 1 fathom, and 2 fathoms wide.

Account of a Book.

Frederici Hoffmanni Fred. Fil. M. D. Exercitatio Medico-Chymica de Cinnabari Antimonii. Lugd. Batav. 1685, *8vo.* N° 176, p. 1208.

The author of this treatise gives an exact account of the manner of preparing

* See vol. II.

the so called cinnabar of antimony.* He examines its constituent principles; explains the phænomena which occur in its preparation; and describes its medicinal virtues.

Some remarkable Effects of a great Storm of Thunder and Lightning at Portsmouth, Oct. 23, 1685. N° 177, p. 1212.

On board his Majesty's ship the Royal James, a flash of lightning and thunder together struck the mast, which was put into her for careening, being a made mast, and bound with iron hoops from one end to the other, and shivered it down to the deck, breaking one of the iron hoops in the body of the mast, so that splinters are forced out of the middle of the mast a foot and half long; so that the mast is wholly unserviceable, and must be taken out. At the same time a ball of fire ran about the deck.

On board the Coronation a great ball of fire came into the gun-room ports, throwing a boy out of one of them, by which he was drowned; and several workmen on board were struck down, and remained senseless for some time; the fire-ball ran up and struck on the starboard side of the wardrobe, and left a place scorched round on the side, and between the two ring-bolts, as if it had been a shot; and beat the wainscot over to the other side, all scorched as if with fire; it also ran up against and struck the doors and hinges; and divided into several fire-balls on the deck among the men; some part of it also broke in at the windows of the round-house, shivering off a great deal of the wainscot, broke the glasses of a prospective, and made a hole through a letter that lay in the window eight double, the size of a musket bullet, and no more; it also shivered the timber that holds the ensign staff on the poop. Portsmouth, Oct. 24, 1685.

On the tendency of the Needle to a Piece of Iron, held perpendicular, in several Climates. By a Master of a Ship crossing the Equinoctial Line, Anno 1684; *and communicated by Mr. Arthur Bayley, F. R. S.* N° 177, p. 1213.

All the way from England to 10° north latitude, the north point of the

* What was formerly called cinnabar of antimony is a compound of sulphur and quicksilver. The process for obtaining it consisted in mixing together and subjecting to sublimation well pulverised antimony (sulphuret of antimony) and corrosive sublimate (oxymuriate of quicksilver). On the application of heat the muriatic acid seized the antimony (with which it has a greater affinity) and formed with it what was called butter of antimony (oxymuriate of antimony) while the disengaged sulphur (the other component part of the crude antimony) united with the quicksilver, forming with it th compound termed cinnabar (sulphuret of quicksilver). But cinnabar so prepared is not different from common cinnabar, and the title cinnabar of antimony has only served to produce confusion.

needle tended to the upper end of the iron, and the south point to the lower end, very strongly.—In lat. 9° 42′ north, and meridian distance from the Lizard 9° 32′ west, the south point of the needle strongly tended to the lower end of the iron, but the north point did not so strongly tend to the upper end as before. —Lat. 4° 33′ north, and meridian distance 5° 18′ west, from the Lizard, the north point of the needle began to decline from the upper end of the iron, and the south point to incline more strongly to the lower end.—Lat. 52′ south, and meridian distance 11° 52′ west, from the Lizard, the north point of the needle neither tended to the upper end of the iron nor the lower end, but the south point still inclined to the lower end, though not so strongly.—Lat. 5° 17′ south, and meridian distance 15° 9′ west, from the Lizard, the south point of the needle would turn to the lower end of the iron, about 2 points, but by removing the iron any further, it would fly away from it, and tend to the poles again; but it would not tend to the upper end at all, neither would the north point incline to either; but when the iron lay horizontal, and the ends of the iron made to respect the poles of the world, the north point of the needle would turn to the south end of the iron, and contrarily the south point of the needle would turn to the north end of the iron, and alter its respect to the poles 5 or 6 points, and no further; but holding the iron perpendicular, and its middle to the needle, it would still respect the poles.—Lat. 8° 17′ south, and meridian distance from the Lizard 17° 35′ west, the north point of the needle would not respect the upper end of the iron, but rather fly from it; but the south point would still somewhat respect the lower end, and alter its true position about 2 points: but laying the iron aslope over the compass, so that the upper end was towards the south pole, and the lower end to the north, then the north point would respect the lower end and follow it; but if you point the upper end to the north, and the lower end to the south, the north point will forsake it. But laying it horizontal, it would act as in the foregoing observations.—Lat. 15° south, and 20° west, from the Lizard, the south point of the needle began to respect the upper end of the iron, and the north point the lower end, and followed it about 1 point; but laying the iron horizontal, the north point respected the south end of the iron, and contrarywise, &c.—Lat. 20° 20′ south, and 19° 20′ west, from the Lizard, the south point of the needle respected the upper end of the iron, and the north point the lower end pretty strongly, and followed it 3 or 4 points; but laying it horizontal, it would act as before.—Lat. 29° 25′ and 13° 10′ west, from the meridian of the Lizard, the south point of the needle respected the upper end of the iron, and the north point the lower end strongly.

Abstract of a Letter from Mr. J. Flamsteed, Math. Reg. and F. R. S. giving an Account of the Eclipses of Jupiter's Satellites, &c. Anno 1686. N° 177, p. 1215.

You have here the eclipses of Jupiter's satellites for the next year 1686, deduced from the same numbers by which I calculated those of the years 1684 and 1685. I shall not need to repeat here again, how to find within what limits on our earth any of them, not visible with us, will be observable; but shall only observe that all my observations of them since made, have fully verified my assertions concerning them, and that I have not found my numbers at any time since to err above one minute in the eclipses of the first or innermost satellites: so that if a way were found of managing a tube of 8, 10, or 12 feet long at sea, and observing the time as exactly as we can at land, observations of these eclipses would certainly give us the difference of longitude, as exactly as the latitudes are pretended to be observed by our more knowing seamen, and certainly much better than the observations of lunar eclipses. How the differences of longitude are gained by them, you will see by the following examples, the latter of which I have purposely produced to show the good agreement of my numbers with the heavens.

Anno 1680, Oct. 23, O. S. Signor Joseph Ponthia and Marco Antonio Cellio, with a telescope of 25 palms long, observed the total immersion of the first satellite into Jupiter's shadow at Rome, at 10h. 7m. 53s. p. m. which in our observatory here I noted at 9h. 15m. 41s. the difference is the difference of our meridians = 52m. 12s. or 13° 3'.—Again, Jan. 28, 1685, Signor Francis Blanchini, having received my catalogue of the eclipses of the satellites for that year, observed the total immersion of the first at Rome at 11h. 19¾m. which I saw not here, but my numbers give it 10h. 27½m. therefore the difference of meridians is 52½m. agreeing with the former observation, and showing the error of the tables to be insensible.

Tycho therefore judged not amiss, when he placed Uraniburg and Rome under the same meridian; for by several observations of satellite eclipses, it is evident, that the difference of meridians between Uraniburg and our observatory is 51m. 10s. of time, so that Rome lies only 1m. of time, or ¼ of a degree to the east of Uraniburg.

Some ingenious persons having often wished that such tables of the satellites' motions were published, as might serve to find their appearances at any given time, though I think it not fit, because they are capable of much correction and great improvement, to publish mine at present; yet for the sake of the diligent observer, I have contrived a small instrument, whereby with the sole help of the usual catalogue, and the table of the parallaxes of Jupiter's orbit, their distances from the axis of Jupiter may be found, to any given time, within the compass of the year, and for any future year by the like tables.

To make the catalogue of eclipses, as also the table of the parallaxes of Jupiter, it was necessary first to make a table of Jupiter's heliocentric places, to which the parallaxes being applied, give the geocentric. The common ephemerides are very faulty in this planet.

The Solutions of three Chorographic Problems. By a Member of the Phil. Soc. of Oxford. N° 177, p. 1231.

The three following problems may occur at sea, in finding the distance and position of rocks, sands, &c. from the shore; or in surveying the sea-coast; when only two objects, whose distance from each is known, can be seen at one station: but especially they may be useful to one, who would make a map of a country, by a series of triangles, derived from one or more measured bases; which is the most exact way of finding the bearing and distance of places from each other, and thence their true longitude and latitude; and may consequently occur to one who would in that manner measure a degree on the earth.

Prob. 1. There are two objects, B and C, fig. 10, pl. 7, whose distance BC is known; and there are two stations, at A and E, where the objects B, C, being visible, and the stations from each other, the angles BAC, BAE, AEB, AEC, are known by observation; to find the distances or lines AB, AC, AE, EC.

Construction.—In each of the triangles BAE, CAE, two angles at A, E, being known, the third is also known: then take any line *ae* at pleasure, fig. 11, on which constitute the triangles *bae*, *aec*, respectively equiangular to the triangles BAE, AEC; join *bc*. Then upon BC constitute the triangles BCA, BCE equiangular to the correspondent triangles *bca*, *bce*, join AE, and the thing is manifestly done.

Calculation.—Assuming *a e* of any number of parts; in the triangles *a b e*, *ace*, the angles being given, the sides *a b*, *a c*, *e b*, *e c*, may be found by trigonometry: then in the triangle *b a c*, having the angle *b a c*, and the legs *a b*, *a c*, we may find *b c*. Then *b c* : B C :: *ba* : BA :: *b e* : B E :: *c a* : CA :: *c e* : CE.

Prob. 2.—Three objects, B, C, D, fig. 12, are given, or, which is the same, the sides, and consequently the angles of the triangle B C D are given; also there are two points or stations A, E, such, that at A may be seen the three points B, C, E, but not D; and at the station E, may be seen A, C, D, but not B; that is, the angles B A C, B A E, A E C, A E D, and consequently E A C, A E C, are known by observation: to find the lines A B, A C, A E, E C, E D.

Construction.—Take any line *a e* at pleasure, fig. 13, and at its extremities make the angles *e a c*, *e a b*, *a e c*, *a e d*, equal to the corresponding observed angles E A C, E A B, A E C, A E D. Produce *b a*, *d e*, till they meet in *f*, and join *c f*; then upon C B describe (according to 33, 3, Eucl.) a segment of a circle, that may contain an angle = *c f b*: and upon C D describe a segment of a circle capable of an angle = *c f d*; suppose F the common section of these 2 circles; join F B, F C, F D; then from the point C, draw the lines CA, CE, so that the angle F C A may be = *f c a*, and F C E = *f c e*; then A, E, the common sections of C A, C E, with F B, F D, will be the points required, from whence the rest is easily deduced.

Calculation. Assuming *a e* of any number of parts; in the triangles *a c e*, *a f e*, all the angles being given, with the side *a e* assumed, the sides *a c*, *e c*, *a f*, *e f*, will be found; then in the triangle *c a f*, the angle *c a f* with the legs *a c*, *a f* being known, the angles *a f c*, *a c f* with the side *f c* will be known: then for the rest of the work in the other figure, the triangle B C D having all its sides and angles known, and the angles B F C, B F D, being equal to the found *b f c*, *b f d*; how to find F B, F C, F D by calculation, and also protraction, has been shown by Mr. Collins, in Phil. Transactions, N° 69, p. 563, vol. 1, as to all its cases.

But here it must be noted, that if the sum of the observed angles, B A E, A E D, be 180 degrees, then A B and E D cannot meet, because they are parallel, and consequently the given solution cannot take place; for which reason another is here subjoined.

Another Solution.—Upon B C, fig. 14, describe a segment B A C of a circle, so that the angle of the segment may be equal to the observed angle *b a c*, (which, as above quoted, is shown 33, 3, Euclid); and upon C D describe a segment C E D of a circle, capable of an angle equal to the observed C E D; from C draw the diameters of these circles C G, C H; then upon C G describe a segment of a circle G F C, capable of an angle equal to the observed angle A E C; likewise upon C H describe a circle's segment CFH, capable of an angle equal to the observed C A E: suppose F the common section of the two last circles H F C, G F C, join F H, cutting the circle H E C in E; join also F G, cutting the circle G A C in A: then A, E, are the points required.

Demonstration.—For the angle B A C is = *bac* by construction of the segment, also the angles C E H, C A G, are right, because each is in a semicircle: therefore a circle being described on C F as a diameter, will pass through E, A. Therefore the angle C A E = C F E = C F H = (by construction) to the observed angle *c a e*. In like manner the ∠ C E A = C F A = CFG = the observed angle *c e a*.

If the stations A, E fall in a right line with the point C; the lines G A, H E, being parallel, cannot meet: but in this case the problem is indeterminate, and capable of infinite solutions. For, as before, on C G describe a segment of a circle capable of the observed angle *cea*; and upon C H, describe a segment capable of the observed *cae*: then through C, draw a line any way cutting the circles in A, E, these points will answer the question.

Prob. 3.—Four points, B C D F, (fig. 15), or the 4 sides of a quadrilateral, with the angles comprehended, are given; also there are 2 stations A and E such, that at A, only B, C, E are visible, and at E only A, D, F, that is, the angles B A C, B A E, A E D, D E F are given: to find the places of the two points A, E; and consequently, the lengths of the lines A B, A C, A E, E D, E F.

Construction.—Upon B C describe a segment of a circle, that may contain an angle equal to the observed angle B A C; then from C draw the chord C M, or a line cutting the circle in M, so that the angle B C M may be equal to the supplement of the observed angle B A E, *i. e.* its residue to 180 degrees. In like manner, on D F describe a segment of a circle, capable of an angle equal to the observed D E F; and from D draw the chord D N, so that the angle F D N may be equal to the supplement of the observed angle A E F; join M N, cutting the 2 circles in A, E: then A, E, are the two points required.

Demonstration.—Join A B, A C, E D, E F; then is the angle M A B = B C M (by 21, 3, Eucl.) = supplement of the observed B A E by construction; therefore the constructed angle B A E is equal to that which was observed. Also the angle B A C of the segment is, by construction of the segment, equal to the observed angle B A C. In like manner the constructed angles, A E F and D E F, are equal to the correspondent observed angles, A E F, D E F; therefore A, E are the points required.

Calculation.—In the triangle B C M, the ∠ B C M (= supplement of B A E) and ∠ B M C (= B A C) are given, with the side B C; thence M C may be found; in like manner D N, in the △ D N F, may be found. But the ∠ M C D (= B C D − B C M) is known, with its legs M C, C D, therefore its base M D and ∠ M D C may be known. Therefore the ∠ M D N (= C D F − C D M − F D N) is known, with its legs M D, D N; thence M N, with the angles D M N, D N M will be known. Then the ∠ C M A (= ∠ D M C + D M N) is known, with the ∠ M A C (= M A B + B A C) and M C before found; therefore M A and A C will be known. In like manner, in the triangle E D N, the angles E, N, with the side D N, being known, the sides E N, E D will be known; therefore A E (= M N − M A − E N) is known. Also in the triangle A B C, the ∠ A, with its sides B C, C A being known, the side A B will be known, with the ∠BCA; so in the triangle EFD, the ∠E, with the sides, ED, DF being known, EF will be found, with the ∠EDF. Lastly, in the triangle ACD, the ∠ACD (= BCD − BCA), with its legs AC, CD, being known the side AD will be known; and in like manner EC, in the triangle EDC.

Note that, in this problem, as also in the first and second, if the two stations fall in a right line with either of the given objects: the locus of A or E being a circle, the particular point of A or E cannot be determined from the things given. As to the other cases of this third problem, wherein A and E may shift places, i. e. only D, F, E, may be visible at A, and only A, B, C, at E; or wherein B, D, E, may be visible at A, and only C, F, A, at E; or wherein A may be on one side of the quadrilateral, and E on the other; or one of the stations within the quadrilateral, and the other without it; for brevity's sake these may be here omitted, as the surveyor will easily direct himself in those cases, by what has been said above. The solution of this third problem is general, and serves also for both the preceding ones. For suppose C, D the same point in the last figure, and it gives the solution of the second problem: but if B, C, be supposed the same points with D, F, by proceeding as in the last, you may directly solve the first problem.

On the Circulation of the blood, as seen by the help of a Microscope, in the Lacerta Aquatica. By Wm. Molyneux, Esq. N° 177, p. 1236.

Dublin, Oct. 27, 1685.—Our Society lately received transcripts of two of Dr. Garden's letters, the first dated from Aberdeen, July 17, 1685, to Dr. Middleton; the other Sep. 4, 1685, to Dr. Plot. To both these letters I have something to say.

In the first he gives an account of the visible circulation of the blood in the water-neut, or lacerta aquatica; and I am heartily glad, that this learned and ingenious doctor has hit upon this experiment. It is now above two years and a half since I first discovered this surprising appearance, and wrote a large account of it, May 12, 1683, as also of the whole anatomy of this animal, to my brother, who was then at Leyden. And I have since that showed it frequently, both on the outside without dissection, and in the inward vessels also, to several curious physicians and philosophers, to their great satisfaction and admiration; particularly I exposed it first to our Society, May 26, 1684, as appears by the following minute taken from our registry. "May 26, 1684, Mr. Molineux opened before the company a water-neut, which he takes to be the salamander or lacerta aquatica; in the body of this animal there are two long sacculi aerei, on which the blood vessels are curiously ramified; to these blood vessels applying a miscroscope, he showed the circulation of the blood ad oculum, as plainly as water running in a river, and more rapidly than any common stream." The same experiment I repeated again before them on the 2d of June following, and to those that had good observing eyes, the circulation was as visible outwardly on the hands and toes, as in the vessels within. But certainly the appearance in the vessels on the forementioned sacculi, with the beating, emptying and filling of the heart, is most surprising to the beholder. This creature seems wonderfully adapted by nature for this experiment; for besides the transparency of its skin and vessels, I have had them live 9 hours after they have been expanded, and all their viscera laid open.

To Dr. Garden's 2d letter I have only this: he there endeavours to explain the trade winds within the tropics, from the different gravity of the atmosphere at divers times of the year. And yet it is asserted in N° 165 of the Philos. Trans. that the mercury is not affected with the weather, or very rarely, whether it be cloudy, rainy, windy, or serene, in St. Helena, or the Barbadoes, and therefore probably not within the tropics, unless in a violent storm or hurricane. Now if the mercury move little or nothing in the baroscope, it is likely there is little or no change in the gravity of the atmosphere within the tropics.

On Dr. Papin's Way of Raising Water. By Dr. Nath. Vincent, F. R. S. N° 177, p. 1238.

I have inquired into Dr. Papin's problematic engine for raising water, in N° 173, (p. 193 of this vol.), and conclude it may be solved after this manner. Within his rock C C, (fig. 5, pl. 5,) there may be a vessel placed, which shall be made like the body of a pair of bellows, or those puffs heretofore used by barbers, which being filled with water, a piece of clock-work put under it may produce the jets; the water being received into the shell H H, and running thence into the hollow of the coral E E, may be thereby conveyed into the follicular cavity, in the same quantity as it is ejected from the two emerging tubes; and it shall circulate 100 or 1000 times, according to the going of the clock-work. If some such account as this will unriddle the engine, the contrivance of it is owing to a theory communicated by myself, &c.

On the same Subject. By Mr. R. A. N° 177, p. 1239.

I conceive that the air is forced into the outer glass at the bottom of M. Papin's machine. That it then passes up between the two glasses. That the outer glass or case being close luted at the head or crown, to which the inner glass is hung by the coral, the air is forced into the mouth of the inner glass. That the air, so forced, pressing on the surface of the water that covers the rock, forces the water to rise through those two extreme parts that are not at all clogged, or covered with water.

Observations on a French Paper, concerning a perpetual Motion. By Dr. Papin, F. R. S. N° 177, p. 1240.

The paper printed in French, and containing a contrivance for a perpetual motion, being expressed in such a manner, that can hardly be understood but by those who are well acquainted with such descriptions, I have endeavoured to explain it as follows.

Let D E F, fig. 16, pl. 7, be a pair of bellows, 40 inches long, which may be opened by removing the part F from E: let them be exactly shut every where but at the aperture E; and let a pipe E G, 20 or 22 inches long, be soldered to the aperture E, having its other end in a vessel G, full of mercury, and placed near the middle of the bellows. A is an axis for the bellows to turn upon. B is a counterpoise fastened to the lower end of the bellows. C, a weight with a clasp to keep the bellows upright.

Now if we suppose the bellows opened only to $\frac{1}{3}$ or $\frac{1}{4}$, standing upright as in the figure, and full of mercury, it is plain that the mercury, being 40 inches high, must fall, as in the Torricellian experiment, to the height of about 29 inches, and consequently the bellows must open towards F, and leave a vacuity there: this vacuity must be filled with the mercury ascending from G, through the pipe G E, the pipe being but 22 inches long: by this means the bellows must be opened more and more till the mercury, continuing to ascend, makes the upper part of the bellows so heavy, that the lower part must get loose from the clasp C, and the bellows turn quite upside down; but the vessel G, being set in a convenient place, keeps them horizontal, as fig. 17, pl. 7, and the part F engages there in another clasp C; then the mercury by its weight runs out from the bellows into the vessel G, through the pipe E G, and the bellows must shut closer and closer, till the part E F comes to be so light, that the counterpoise B is capable of freeing the part F from the clasp C; then the bellows come to be upright again, as fig. 16; the mercury left in them falls again to the height of 29 inches, and consequently all the other effects will follow as before, and the motion continue for ever. Thus much the French author.

Upon this it is to be observed, that the bellows can never be opened by the internal pressure, unless the said pressure be stronger than the external: now in this case, the weight of the atmosphere freely presses on the outward part of the bellows, but it cannot come at the inward part except through the pipe G E which, containing 22 perpendicular inches of mercury, counterpoise so much of the weight of the atmosphere; so that this being supposed to be 29 inches of mercury, it can only press the inward part of the bellows with a weight equivalent to 7 perpendicular inches of mercury. From this we may conclude, that the pressure of the atmosphere being weakened within the bellows more than it can be assisted by the mercury contained in the same, as may easily be computed; the bellows standing upright, as fig. 16, must rather shut than open. Thus, without losing any labour and charges in trying, we may be certain that the project can never succeed.

Account of Books.

I. *Edvardus Bernardus de Mensuris et Ponderibus* κατ' ἐπιτομήν. *Oxonii è Theatro Seldonio, A. D.* 1685. N° 177, p. 1242.

This compendious treatise contains a comparison of the ancient weights and measures with the modern, and deduces the length of several of the former from the measurement of some ancient buildings; as, the pantheon at Rome, Minerva's temple in her own city, the numerous antiquities of Constantinople, the admirable ruin of Chilmenar, and the perpetual pyramids of Egypt. From these and several other monuments is asserted the equality of the English foot to that of the Hebrews, Babylonians, Greeks, Chinese, Castilians, and those of Lisbon and of Lyons. And farther, that dividing the English foot into 1000 parts, the Parisian will contain 1066, the universal foot of Sir Jonas Moor 1089, the old Roman 970, that of Villalpandus, derived from Vespasian's Congius, 986, the Rhinland foot of Snellius 1033, the Venetian 1140, the Bononian of Mr. Auzout 1140, &c. Thus from the foot, passing through the various measures of antiquity, this treatise at last gives for one degree, or the 360th part of the circuit of the whole earth, $73\frac{4}{10}$ English miles, of 5000 feet in a mile; $67\frac{4}{10}$ universal miles, $66\frac{2}{3}$ Arabic miles, exactly according with the old observations of Hasan, Nodham, Masudy, and other Arabs; and little differing from the late experiments of Mr. Norwood in England, and Mr. Picart in France. As for ancient weights, the true Hebrew sekel, or siclus argenteus, inscribed with Samaritan characters, is equal to 3 shillings English, or to the tetradrachm of Thasus and the Greek islands. The Attic tetradrachm makes 2s. 9d. English, or 4 denarii Gordianici. The denarius consularis Romanorum makes $7\frac{1}{2}$d. English. The denarius Tiberianus $7\frac{1}{4}$d. And lastly, the denarius Vespasianicus $6\frac{1}{4}$d, Hence also is found the value of the Hebrew talent in silver to be 450l. sterling, and that of gold to rise to 5400l. sterling. And so for other ancient weights and measures of capacity.

II. *Ant. Nuck* de Ductu Salivali Novo, Saliva, Ductibus Aquosis, et Humore Aqueo. Ludg. Bat.* 8vo. N° 177, p. 1244.

This ingenious anatomist divides this little tract into two parts, The first gives his discovery of a new gland, with its proper ductus; his observations on the saliva; with an historical account of the ductus salivalis when disordered. This gland is seated in the orbit of the eye, just between the musculus abducens and the uppermost part of the os jugale; it is of no certain figure, being sometimes oblong, sometimes almost round or oval, and sometimes triangular; it exceeds not the size of a small nut, or the weight of ½ a drachm, or 2 scruples at most; it has 4 sorts of vessels besides that peculiar to it; an artery from the carotid; a vein from the jugular, and its nerve springs from the third pair, called motorii oculorum; and he does not question but it has lymphatics also. Many small sprigs rise from this gland, which meeting together join all in one duct; which, crossing the ductus stenonianus, passes directly downward on the outside of the os maxillare, and opens on the last tooth but one of the dentes molares of the upper jaw. Our author gives it the name of ductus salivalis superior alter, in opposition to that of Steno.

In the second part of his book he gives an account of certain ducts which convey water to the eyes, having the first time accidentally hit upon one in the Gadus, he traced it along on the outside of the tunica sclerotis, till it entered that part of it called the cornea: it easily admitted a probe, but nothing issued on withdrawing the probe, because, as he supposes, the extremity of the duct serves as a valve, and discharges its matter after the same manner as the ductus communis, the ureters, &c. do into their respective cavities without admitting regurgitation. He has found in dogs and birds 2 in each eye, exactly opposite to each other; in some sheep's eyes 5, in others 6: but he can by no means discover their rise, being able to trace them as far as the optic nerve, but very little farther; he assures us that they are also present in the human subject.

The General Bill of Christenings and Burials, according to the Report made to his Majesty by the Company of Parish Clerks of London, 1685. N° 177, p. 1245.

Christened.		Buried.	
Males	7484	Males	11891
Females	7246	Females	11331
In all	14730	In all	23222.

* This expert Dutch anatomist, who flourished in the 17th century, bestowed great pains on the glands and lymphatic vessels. He was very successful in tracing and demonstrating the course of the last-mentioned vessels by quicksilver injections. Besides the treatise above-noticed (which was several times reprinted with additions under the title Sialographia) he wrote the following: Defensio ductuum aquosor. 1691, and Adenographia, 1691; both which, with the first mentioned work, and some other tracts, were republished together in 2 vols. under the title of Opera Omnia, 1733.

Account of Books.

I. *Jo. Con. Peyeri,* Merycologia, sive de Ruminantibus et Ruminatione Commentarius. Basil. 4to.* N° 177, p. 1246.

This treatise consists of three books; in the first of which, after a division of animals into ruminantia et non ruminantia, he reckons up those of the former kind, some of which, he says, may more properly be said to chew the cud, others imitate that action, and are here called ruminantia spuria, as the mole-cricket, bee, beetle, crab, lobster, mullet, and several birds; but the creatures which ruminate in a more genuine sense are, among quadrupeds, the ox, deer, sheep, and goat kind, the camel, hare, and the squirrel; also some men, of whom he gives several instances.

In the second book, he treats of the organs subservient to rumination, as of the several stomachs belonging to some of the ruminantia legitima, and of them first in general; then in particular of the paunch, reticulus (κεκρύφαλος) the feck, the reed; of the single stomach in hares and rabbits; all which are described with great exactness, and after a very satisfactory manner.

As to the stomachs of the ruminantia spuria, he affirms they all have spiral muscular fibres, by means of which they, as it were, grind their meat up and down, after a manner somewhat analogous to rumination; for the better affecting of which, in some of them the stomach is extremely rough in the inside, as in the mullet; in others it is very hard and callous, as in geese, hens, &c. in the stomach of others, as of the cricket and lobster, there are teeth.

After this he passes to the œsophagus, as it is in the ruminantia genuina; where, rejecting the account which Petrus Aponensis, Æmylianus, Aquapendent, and even Fallopius, have given of that part, he describes it, according to Steno, to consist chiefly of 2 spiral muscles, formed like screws, crossing one another.

From the œsophagus he proceeds to the mouth, and then to those parts which more remotely assist in rumination, such as the pectoral and abdominal

* John Conrad Peyer, who acquired at a very early period of life much celebrity by his anatomical writings, was a native of Schaffhausen in Switzerland. Before his Merycologia appeared he published his Exercitat. de Glandulis Intestinor. 1677; his Pararga Anatomica, 1681; and Pæonis et Pythagoræ (nempe Peyeri et Harderi) Exercitationes Anatomico-medicæ, 1682. Besides these, and one or two other anatomical tracts, several communications from him were inserted in the Eph. Nat. Curios. Peyer's merits as an anatomist are very considerable; and although he was not the first discoverer of the muciparous glands of the intestines;* yet has he described these glands more fully and accurately than others had done, and at the same time shown in a clearer point of view the consequences which result from their action, both in their natural and diseased state. His Merycologia, and some others of his works, prove that he was well versed in many parts of comparative anatomy.

* See the account of Pechlin at p. 321, vol. 2, of this Abridgment.

muscles, and the diaphragm. He subjoins cuts, representing the 4 stomachs of the ruminantia bisulca, the single stomach of the rabbit, and the œsophagus of an ox. The third book consists of reflections and speculations.

II. *Castorologia à Jo. Mario, aucta à Jo. Franco. Aug. Vindel,* 1685, 8vo. N° 177, p. 1249.

A treatise, wherein is delivered the natural history of the beaver, with an account of the medicinal uses of that peculiar odorate substance, named castor, which is contained in little bags or follicles situated near the anus.

On the Magnetical Variation at Nuremberg, in the Year 1685. *By M. G. C. Eimart, &c. Abridged from the Latin.* N° 178, p. 1253.

In the beginning of August, 1685, having taken all possible care to be certain of our meridians, we tried several magnetic needles, both those old ones we had employed in observing about 5 years before, and many new ones, of a middling length, the longest not above 6 inches, but which were more slender and active; and, what is strange, we found the declination of the needle not to vary one minute, but exactly agreeing with the former in every meridian, being again just 5° 5′ to the west. Whether it has gone farther in the mean time, or its deviation be now retrograde, which might by chance be the case, is not certain; for I am not willing to assert that, being rather inclined to think it has been stationary at that point, its motion doubtless being circular.*

On Dr. Papin's new Water Engine. Br W. Tenon. N° 178, p. 1254.

Reading lately in the Transactions a description of the effects of a water engine, invented by Dr. Papin, and propounded as a problem to find out the contrivance, the following thought occurred to my mind. I do not think it is the same with his, nor so good, but such as it is I send you, because variety of fancies gives hints many times to better new inventions.

In pl. 8, fig. 1, A B D E represents a cylindrical vessel closed on every side, and divided into two rooms or parts by the floor or partition E F. And G L M H is another cylindrical vessel, within the upper room, cemented with its mouth downwards to the floor, and full of water up to the surface I K; the upper part thereof, G I K H, being full of air. Q O, R P, two pipes open above and below,

* This doubt has probably been owing to some inaccuracy in the observations, as the variation has certainly been gradually increasing westward in Europe, since about the year 1666, and is now near 23° west; but whether at present stationary seems to be uncertain, owing to the want of observations on this curious phenomenon.

and let through the upper room into this vessel, and reaching almost down to the floor E F. V W, a pipe open above and below, and let into the upper room. These pipes must be close joined round about them, to the floors C D and G H. X, Y are two little hemispherical bladders, prepared with oil or some oily substance, against water, and cemented with their mouths upward to the floor E F underneath. α, β, two valves, opening out of the upper room into the bladders. γ, δ, two other valves opening out of the bladders into the inner vessel above. N Z, a pendulum playing on the centre N, and having two battle-door arms a, b, to squeeze alternately the bladders which rest upon them.

Let the upper room be filled with water at the pipe VW, and if the pendulum be made to play by clock-work, the bladders will perpetually pump it thence into the inner vessel, and the compressed air G I K H in the upper part of that vessel, pressing on the surface of the water I K, will force it thence into the pipes O Q, P R, out of which spouting with a perpetual even stream into the spoons S, T, it will run down by the pipe W V, into the upper room again. The pendulum will play most easily when the upper room is filled to the top of the pipe W V. Instead of the bladders, may be other contrivances, as of suckers or little organ bellows, playing alternately with two leaves about an axis in the middle. Sept. 16, 1685.

A Total Eclipse of the Moon, Anno 1685, *Dec.* 10, *N. S. observed at Dantzic, by M. Hevelius. Abridged from the Latin.* N° 178, p. 1256.

It required no small skill and labour to manage our tubes, though they were but short, viz. of 5, 6, and 7 feet, and to keep them sufficiently steady to the moon, to distinguish exactly the penumbra, and mark out all the phases, through what spots they passed, or what part they arrived at in every particular instant of time. These things I have performed as well as I was able, and as the severe season and floating clouds would permit. It may be observed that a very dense penumbra preceded the true eclipse, so that I could scarcely, if at all, distinguish the precise beginning. As to the colour, it is particularly remarkable in this eclipse; for I have very rarely found so great a diversity in colour. Sometimes it was rusty, or of the colour of a weasel, but when the total obscuration came on, the moon's limb was in parts rather livid, and partly brightish and ruddy. But in the middle of the moon appeared an obscure and dense little cloud, so that we could not well distinguish the moon's spots. This dark cloud advanced gradually towards the right hand, and to the Palus Mæotis; so that about the beginning of the recovery of light, the whole true shadow appeared very black, and about the last phasis noticed, the remaining part of the moon still obscured, or its limb could not be seen at all. The times of the principal phases were as follow.

	Correct times.	
At	$9^h\ 32^m\ 25^s$....	the penumbra.
	9 50 10....	beginning of the eclipse.
	10 58 25....	the total immersion.
	12 43 0....	beginning of the emersion.

Description and Uses of an Instrument for finding the Distances of Jupiter's Satellites from his Axis. By Mr. J. Flamsteed, Math. Reg. and F. R. S. N° 178, p. 1262.

In pl. 8, fig. 2, the small circle in the middle represents the planet Jupiter, the 4 concentric circles the proper orbits of his 4 satellites, duly proportioned to the breadth of his body; the distances between the parallel lines intersecting them, being each equal to one of his semidiameters. The 4 divided circles next without these, are distinguished into as many parts as there are days and hours in each satellite's revolution; the innermost of them serving for the first or innermost satellite, that next it for the 2d, that next without this for the 3d, and the outermost for the 4th; above which is a small divided arch of 15°.

By this instrument to find the distances of the satellites from Jupiter's axis to a proposed time. 1. Find the parallax of Jupiter's orbit to the time proposed, and note whether it be to be added or subtracted. 2. Extend the thread from the centre of the instrument over the parallax numbered in the small arch; it cuts off in the 4 divided circles so many hours, as each satellite takes up in passing from the axis of the shadow to the axis of Jupiter, viewed from the earth; these I call the simple parallactic intervals, which, if the parallax was to be added, are also additional, if to be subtracted, they are to be subducted. 3. To these parallactic intervals, add the times of half the duration of the eclipse of each satellite; which, for the first, may be assumed 1h. 10m. for the 2d 1h. 30m. greater exactness being needless; but for the 3d and 4th, when eclipsed, (their immersion into the shadow and emersion from it being commonly given in the tables) take half the difference of these times at the next eclipse to the time proposed, for the half duration, and add them to the simple parallactic intervals, so you have them augmented. But note that as often as the 4th satellite is not eclipsed, (which is 2 years in every 6) its interval needs no augmentation, the tables showing the very time when it passes the axis of the shadow. 4. Find in the tables the times of the eclipses of each satellite next preceding the time proposed, and when the 4th is not eclipsed, find the time of its passing the axis of the shadow; to which, if the parallactic intervals augmented were additional, add them to; if subductive, subtract them from, each the time of its proper satellite's eclipse; so you have very nearly the apparent times when each satellite last past over the axis of Jupiter, viewed from the earth. 5. Subtract each of the times thus obtained from the time proposed, the remainders are the intervals of the motion of each satellite from Jupiter's axis. 6. Extend the thread from the centre over each of these intervals of motion, numbered severally in the divided circles belonging each to its proper satellite, where it cuts the proper orbit of that satellite, whose interval was numbered in its peculiar circle; it shows among the parallels how many semidiameters of Jupiter that satellite is distant from him, and on which side of him it is posited.

Note further, that the thread, as it lay extended over the parallax of the orb numbered in the small arch, where it cut the several proper orbits of each satellite, showed among the parallels how many semidiameters of Jupiter the centre of the shadow was distant from the centre of Jupiter viewed from the earth. And that if the parallax of the orb were additive, the shadow lies on the right hand from Jupiter; if subductive, on the left.

To explain these precepts, I shall give two brief examples. Let it be then proposed to know how far each satellite appears distant from Jupiter on the 26th of December, 1685, at 16h. 52m. p. m. when the 3d satellite falls into the shadow; also on July 16, 1686, at 10h. p. m. when there is no eclipse.

Anno 1685, Dec. 26, 16h. 52m. p. m. the parallax of the orb, is 9° 20′ additive. Therefore,

	1.			2.			3.			4.		
	d.	h.	m.	d.	h.	m.	d.	h.	m.	d.	h.	m.
The simple parallactic intervals add	0	1	5..	0	2	10..	0	4	25..	0	10	20
The half duration of the eclipses to be added	0	1	10..	0	1	30..	0	1	18..	0	0	0
The parallactic intervals augmented	0	2	15..	0	3	40..	0	5	43..	0	0	0
Last immersions and conjunctions, Dec.	25	9	37..	25	5	47..	19	12	58..	10	0	30
Times of last passing Jupiter's axis, Dec.	25	11	52 .	25	9	27..	19	18	41..	10	10	50
Subtracted from the time proposed, Dec.	26	16	52..	26	16	52..	26	16	52..	26	16	52
Leaves the intervals of motion	1	5	0..	1	7	25..	6	22	11..	16	6	2
Over which, numbered in their peculiar circles, the thread being severally laid, cuts the proper orbit of each at their visible distances from Jupiter.	semid. 5 right			semid. 6½ left			semid. 3 right			semid. 4½ right.		

See pl. 8, fig. 3.

Again, 1686, July 16d. 10h. p. m. the parallax of the orbit is 10° 46′ subductive. Hence,

	1.			2.			3.			4.		
The simple parallactic intervals subd.	0	1	12..	0	2	35..	0	5	19..	0	12	0
Half duration of the eclipses add	0	1	10..	0	1	30..	0	0	0..	0	0	0
The parallactic intervals augmented	0	2	22..	0	4	5..	0	0	0..	0	0	0
The next last emersions and passing the axis of the shadow, July	15	5	55..	15	22	2..	15	9	19..	15	17	52

Time of last passing the visible axis of Jupiter...	15 3 33..	15 17 57..	15 4 9..	15 5 52
The time proposed..........................	16 10 0..	16 10 0..	16 10 0..	16 10 0
Intervals of motion...........................	1 6 27..	0 16 3..	1 0 51..	1 4 8
	semid.	semid.	semid.	semid
Therefore distances from Jupiter's axis...........	$5\frac{1}{3}$ right	$8\frac{1}{5}$ left	$12\frac{1}{2}$ left	$10\frac{1}{3}$ left.

See fig. 4, pl. 8.

And the satellites stand at the two proposed times, as in the two figures 3 and 4, pl. 8.

A Human Liver appearing glandulous to the Eye. By Mr. John Brown, Surgeon of St. Thomas's Hospital in Southwark. Dec. 15, 1685. N° 178, p. 1266.

The size of this liver was not extraordinary, which rather seemed less than usual, but what was very remarkable, it consisted, in its concave, convex, and inward parts, of glands, which, with the vessels, made up the whole substance of it. These glands contained a yellowish ichor, like so many pustules, and was probably part of the bilious humour lodged in them, though otherwise the liver between the gland was of its usual reddish colour. In the gall bladder was found a soft friable stone.

Explanation of fig. 7, pl. 8—A A A are the glands in the concave part of the lesser lobe of the liver. B B B the glands in the concave part of the greater lobe of the liver; which were of different magnitudes; though in general they were much less in the lesser, than in the greater. C C C C the inward part of the greater lobe of the liver, as it was divided. D D D D D several black specks, that appeared inserted in those glands, which were probably from the divarications of the vessels, being divided on opening this lobe. E the gall bladder, which was of a greenish colour. F the vena portæ tied up, with the ductus biliarius, &c. G a particular set of glands, lodged between the former and the vena cava. H the vena cava. I part of the ligamentum suspensorium.

The convex part of the liver was, in every respect, the same with the concave part of both lobes, as to the glands here described.

On the Strength of Memory. By Dr. Wallis. N° 178, p. 1269.

Upon occasion of a discourse at a meeting of the Philosophical Society at Oxford, March 24, 1684-5, concerning the advantage which those may have, as to memory and its application, who want their sight, Dr. Wallis confirmed it by this consideration, that even we, who have our eye-sight, can with more advantage apply our memory, in matters of close attention, by night in the dark, when all things are quiet, than by day, when sights and noises are apt to divert our thoughts. And gave instance in the application of his own memory by night, in performing arithmetical operations in great numbers, better than by day he could have done; and even by day we may better do it with our eyes shut, than open. And at their next meeting, March 31, 1685, he gave this further account of it. Having had the curiosity to try how far the strength of memory would suffice, to perform some arithmetical operations, as multiplication, division, extraction of roots, &c. without the assistance of pen and ink, or any thing equivalent; and finding it to succeed well, for instance, in extracting the square root, from numbers of 8, 10, 12, or more places; he then tried it with success in numbers of 20, 30, or 40 places; particularly in the square root of the number 3, which he found to be 1,73205,08075,68877,29353; also, at the request of a foreigner, he took another number at random, consisting of 53 places of figures, and in the dark of the night extracted its root to 27 places, by the usual process; both which he retained in his mind, till he afterwards wrote them down by day light. By which he was sufficiently satisfied, that a tolerably good memory, fixed with close attention, is capable of being charged with more than at first may be imagined.

On a large Stone voided by Urine. By Dr. Wallis. N° 178, p. 1271.

A model, or pattern of a stone, which came from the bladder of Mrs. E. V. of Wallingford, at the age of 63 years, Aug. 7, 1685, was communicated to the society of Oxford. The compass of it was 5¼ inches; its length 4¾ inches, and weight 3 ounces avoird. This stone was drawn out by the hand, without instruments. Since its discharge, she has been troubled with urinæ incontinentia.

On the Wurtemburg Siphon. By Dr. Salomon Reisel, Chief Physician to the Duke of Wurtemburg. Abridged from the Latin. N° 178, p. 1272.

That I may not long disappoint the wishes of the Royal Society, I must confess that the siphon described by the excellent Dr. Papin, is the very same as the Wurtemberg siphon, which I proposed, also made with a recurvation of the lower ends. Nor is there any other mystery at the top, as the inventor has described, than that it must be filled with a funnel, as without being so filled, it cannot run away. This shall soon be confirmed through the press, as it would be too tedious to set down all the particulars in this place.

A full Description, with the Use of the new Contrivance for raising Water, propounded in the Philos. Trans. N° 173. *By Dr. Papin, F. R. S.* N° 178, p. 1274.

In pl. 8, fig. 5, AA is the great tumbler, which must have some small hole in the bottom, as I. I L L a slender pipe, hidden by the chimney-board B B, by which the tumbler A A communicates with the pump or bellows M M. Which pump or bellows is well shut; and having no other aperture, except through the pipe I L L. These are put in some secret place, where a person may play them, and not be seen. N N a slender pipe, making a communication between the glass A A, and the crown F F; this pipe reaches near to the cover of the crown, that the water contained in it may not run down by that aperture. E E the factitious coral, hollow within, but shut at the bottom, and open at the top. D D D D two crooked pipes, soldered to the sides of the coral E E, so that the water running down the coral may spout out through the holes D D. O O a pipe concealed in the coral E E, passing through the bottom, where it must be well soldered, and reaching near to the bottom of the rock C C. P P a pipe to convey the water from the glass G G into the rock C C; and is well soldered to the cover of the rock. Q a valve working by a spring at the bottom of the pipe P P, to keep the water, that gets in that way, from returning back. R another valve at the top of the pipe O O, that the water getting up that way, may not fall through the same.

Now it is plain, that the rock C C being filled partly with water, and partly with air; if we open the bellows M M, the air from the crown F F must run through the pipe N N, into the tumbler A A; and thence through the pipe I L L, into M M, to fill the vacuity made there: the air in the crown F F being thus rarefied, it gives liberty to the air in the rock C C to rarefy also, by driving the water through the pipe O O: the water, being got up into the crown F F, runs down the coral E E, and through the crooked pipes D D D, spouting out at their upper apertures, and from the shells H H, falls upon the rock C C.—If we afterwards shut the bellows M M, the air in their vacuity must run back into the tumbler A A, and press upon the water at the top of the rock C C: but the air in the rock having been rarefied, its spring is not sufficient to resist this pressure, so that the water is forced into the rock through the pipe P P. And by thus opening and shutting the bellows M M, the water must constantly circulate as above described.

As for the uses which this way of raising water may be applied to: the glasses, being merely to conceal the secret, must be left out; and there may be made several receptacles above one another, to receive the water raised; and there should be as many bellows, all to communicate with one receptacle: these bellows should be moved by an axis, so that when the first is open, the second should be shut; the third open, the fourth shut; and so alternately: by this means, the first or lowest receptacle would give the necessary supply of water to the second, the second to the third, the third to the fourth, &c. till the water would be raised to the intended height: such receptacles might easily be set 12 or 15 feet above one another, and so a few of them might raise water to a considerable height, as well as ordinary pumps do; but this new way would have this advantage, that whereas in the ordinary pumps, the strength to be applied lies near the water to be raised, but by this contrivance the stream of a river may be applied to draw water out of a mine far distant from it: by the same way the stream of the Thames might keep constant water-works in Windsor Castle, almost as easily as in the lowest fields. It is also to be observed, that those bellows that are open, have the air in them very much rarefied, so that the outward air lies heavy upon them to shut them, by which means the motion of the engine must be helped in lifting up the opposite bellows, to be opened.

As a further Explanation of the Use of this Contrivance, in fig. 6, *pl.* 8.—AB, AB, are several receptacles set above one another, which must be well shut and soldered every where. CDD, CDD, are two slender pipes, by which the first and third receptacles communicate with the pump HH. EFF, EFF, two other slender pipes, by which the second and fourth receptacles have a communication with the pump II. HH, II, two pumps, whose plugs are so moved by the axis LL, that when one descends the other ascends. MM, a wheel fastened to the axis LL, that it may be moved by the stream of a river. NO, PQ, NO, PQ, are large pipes for the water to ascend, from a lower to a higher receptacle. O, Q, O, Q, are valves fitted to the top of the said pipes, to prevent the water from descending.

Now it is plain that when the plug in the pump HH ascends, the air comes in through the pipes CDD, by which it is rarefied in the first and third receptacles, marked AA: by which means the water may be driven up into the receptacles through the pipes NO; because at the same time the plug in the pump II descends, it causes the air to return to its ordinary pressure in the second and fourth receptacles, that it may be capable to force up the water through the said pipes NO, and the lowest pipe draws the water that lies open to the air. For the same reason, when the plug in the pump II ascends, the air must come in through the pipes EFF; and so be rarefied in the second and fourth receptacles marked BB; by which means the water is forced up into the said receptacles through the pipes PQ, PQ; because at the same time the plug in the pump HH descends, it causes the air to return to its ordinary pressure in the first and third receptacles, so that it is enabled to force up the water through the said pipes PQ.

*Observations on the Purple Fish.** *By Mr. Wm. Cole, of Bristol.*
N° 178, p. 1278.

Mr. Cole having been informed that a certain person, living by the sea-side in some port or creek in Ireland, made considerable profit, by marking with a delicate durable crimson colour, fine linen of ladies or gentlemen, sent from many parts of that island, with their names, or otherwise as they pleased: he made experiments on all the shell-fish taken on the coasts near him, till at length he obtained that delicate colour.

The whole process, discovered by often repeated experiments, was as follows: These shells being harder than most of other kinds, are to be broken with the smart stroke of a hammer, on a plate of iron, or firm piece of timber, with their mouths downwards, so as not to crush the body of the fish within; the broken pieces being picked off, there appears a white vein, lying transversely in a little furrow or cleft, next to the head of the fish, which must be digged out with the stiff point of the horse-hair pencil made short and tapering. The letters, figures, or what else shall be traced on linen or silk, as much forced in, as it can be by the pencil, will presently appear of a pleasant light green colour; and if placed in the sun, it will change into the following colours; i. e. if in winter, about noon, if in the summer, an hour or two after sun rising, and so much before setting; for in the heat of the day, in summer, the colours will come on so fast, that the succession of each colour will scarcely be distinguished, viz. it will change from a light green to a deep green; and then in a few minutes to a full sea-green; after which, in a few minutes more, it will become of a Watchet blue; and from that, in a little time more, it will be of a purplish red: after which lying an hour or two more, supposing the sun still shining, it will turn to a very deep purple red, beyond which the power of the sun cannot further alter it. And note that these changes are made faster or slower, according to the degrees of the sun's heat. But the last and most beautiful colour, after washing in scalding water and soap, after again exposing the matter to the sun or wind to dry, will be a different colour from all those before-mentioned, i. e. of a fair bright crimson, or nearly of the prince's colour, which, though no styptic be used to bind the colour, will continue the same, if well managed; as has been found in handkerchiefs, that have been washed more than 40 times; only it will

* The animals of several shells of the Linnæan genera of *buccinum* and *murex,* afford a purple dye, and were used by the ancients in the preparation of their most valuable vestments. The species here described is the *buccinum lapillus* of Linnæus, a very common shell on most of the European coasts. It is unnecessary to add, that the introduction of cochineal has long superseded the use of these kind of dyes.

be somewhat allayed, from what it was after the first washing. He made large letters on so many pieces of cloth, as there are distinct colours, and put them in a book, to keep them from the air; then several months after, showed the various colours remained distinct as before; yet by often opening the book, and so exposing them to the air, all the colours, excepting the last two, before washing either, would fade; but all the colours being washed, will become one and the same. While the cloth, so written on, lay in the sun, it would yield a very strong fetid smell, like garlic and asafœtida mixed together.

The shells are of various colours, but most of them white; some are red, when newly taken off the rocks; some yellow; others of both these colours; some a blackish brown; many of a sandy colour; and some few striped with white and brown parallel lines.

It seems to be a kind of amphibious animal, alternately living in both elements every tide: being out of their native place, and in want of such viscissitudes, they take this course to find the air; when any of them are put into a vessel of sea-water, for in fresh they soon expire, after they have lain for some time on the bottom of the vessel, they creep up to the surface of the water, and by extending a kind of lip, with their opercula, they cling to the side of the vessel or pan, with about half that part above the water, sometimes creeping down under it, and returning again to their station, between wind and water.

There are many sorts of this purple fish, differing in size, structure, and colour of the shell, according to the nature of the sea-grounds, depth or shallowness of water, rocks, gravel, and mud, as also according to the latitude where they are found; they differ also in the variety of the colours of the tinging juice in their veins, as black, livid, violet, deep sea-green, light and deep red, amethystine, &c. But the best of all were found in the Tyrian seas, near that island on which the famous city of Tyre was built; this was celebrated and prized above all the rest for its rich colour, called in former ages by divers names, as Ostrum Sarranum, Pelagium, Venenum Tyrium, Purpurissum, Flores Tyriani, &c. Almost all authors agree, that the juice lies in a certain vein in the fish, and some mention it to be white and viscous, as this of ours is. It were to little purpose, to give the history at large of all the purpuræ; and when and how first discovered by Phœnix the son of Agenor, 2d king of the Phœnicians, by means of a shepherd's dog devouring one of the fishes, and colouring one of his lips with that excellent dye: by which its antiquity appears. In succeeding ages it received improvements, to the time of Pliny, when it seems to have arrived at its highest perfection; when the Roman artists, in preparations of that tinging juice, for dying the robes and other vestments of emperors, senators, &c. strove to excel each other in new fashioned purples, to gratify the luxury of the great men of those times. This colour sold then at very high prices; the fine double dyed purple of Tyre, called diabapha, yielded 1000 Roman denarii the pound, which is computed to be more than 30l. sterling; and this of ours being of so excellent a colour, without other preparation or addition of any thing to it, may now, or at least hereafter, by farther improvement, perhaps vie with the Tyrian purple. Johnston out of Aristotle, mentions a species of these fishes, under the name of littorales quæ parvæ, et flore sunt rubro; this agrees with ours, which may be named purpura littoralis, sive Teniensis parva turbinata.

Pl. 8, fig. 9, 10, 11, 12, are several shells of the purpura, in their natural size; fig. 13 the operculum.

On a Runic Inscription at Beaucastle. By Mr. Wm. Nicolson.

N° 178, p. 1287.

This inscription is on one entire free-stone, of about 5 yards in height, washed over, as the font at Bridekirk, with a white oily cement, to preserve it the better from the injuries of time and weather. The figure of it inclines to a square pyramid; each side being near 2 feet broad at the bottom, but upwards more tapering. On the west side of the stone, are 3 fair draughts, which evidently manifest the monument to be Christian. The lowest of these represents the portraiture of a layman, with a hawk or eagle perched on his arm. Over his head are the ruins of Lord Howard's inscription. Next to these, the picture of some apostle, saint, or other holy man, in a sacerdotal habit, with a glory round his head. On the top stands the effigies of the B. V. with the Babe in her arms; and both their heads encircled with glories as before. On the north appear a great deal of checquer-work; subscribed with ancient Runic characters, fairly legible, though unintelligible. On the east side appears nothing but a few flourishes, draughts of birds, grapes, and other fruits: all which are probably no more than the statuary's fancy. On the south are flourishes and conceits, as before; and towards the bottom another decayed inscription, the defects in which are sufficient to discourage us from attempting to expound it.

On a Runic Inscription on the Fount at Bridekirk. By Mr. Nicholson.

N° 178, p. 1291.

The fabric of this monument seems fair enough, to evince that it is Christian; and that it is now used to the same purpose for which it was at first designed. Mr. Cambden, though not acquainted with the characters of the inscription, yet seems to fancy as much: and, for proof of his opinion, brings a quotation out of St. Paulinus's epistles. But he needed not to have sent us so far off for a voucher, if he had taken good notice of the imagery on the east side of this stone. We have there fairly represented, a person in a long sacerdotal habit, dipping a child into the water; and a dove hovering over the infant.

On the south-side of the stone is an inscription in Runic characters, which it is well enough known, since Ol. Wormius's great industry in making us acquainted with the Literatura Runica, have been chiefly used by the pagan inhabitants of Denmark, Sweden, and the other northern nations; and the Danes are said to have swarmed mostly in these parts of our island: which two considerations make it probable that the fount is a Danish monument. But then, on the other hand, we are sufficiently assured, that the heathen Saxons also made use of these Runæ; as is plain from the frequent mention of Runcꞃæꝼꞇıᵹen and Runꞅꞇaꝼaꞅ in many of the monuments of that nation, both in print and manuscript still to be met with. Besides, we must not forget that both Danes and Saxons are indebted to this kingdom for this Christianity: and therefore thus far their pretentions to a Runic (Christian) monument may be thought equal. Indeed some of the letters seem purely Saxon: being not to be met with among Wormius's many alphabets: and the words themselves seem to come nearer to the ancient Saxon dialect than the Danish. However let the inscription speak for itself: and I doubt not but it will convince any competent and judicious reader that it is Danish. Thus therefore I have ventured to read and explain it:

Er Ekard han men egrocten, and to dis men red wer: Taner men brogten, i. e. Here Ekard was converted; and to this man's example were the Danes brought.

The language of the whole seems a mixture of the Danish and Saxon tongues: but that can be no other than the natural effect of the two nations being mixed together in this part of the world. Our borderers, to this day, speak a triple of languages (British, Saxon, and Danish) in one; and it is hard to determine which of those three nations has the greatest share in the motley breed.

On the Latitude of Constantinople and Rhodes. By Mr. John Greaves, some time Professor of Astronomy in the University of Oxford. N° 178, p. 1295.

The received latitude of Byzantium (now Constantinople) by Appianus, Mercator, Ortelius, Maginnus, and some others, is 43° 5′. This also we find in the Basle edition of Ptolemy's geography procured by Erasmus out of a Greek

MS. of Pettichius. It is confirmed too by another choice MS. in Greek, of the learned and judicious Mr. Selden. And as much is expressed in the late edition of Ptolemy by Bertius, compared and corrected by Sylburgius, with a manuscript out of the Palatine Library. So that it cannot be doubted that Ptolemy assigned to Byzantium, as our best modern geoprapher have done, the latitude of 43° 5′. And this will farther appear, not only out of his geography, where it is often expressed, but also out of his Almagest, as the Arabians term it, where describing the parallel passing through it, he assigns to it 43° 5′. What was the opinion concerning Byzantium of Strabo, preceding Ptolemy, or of Hipparchus preceding Strabo, or of Eratosthenes still earlier, and perhaps more accurate than any of them, their writings not being now extant, unless those of Strabo, cannot be determined; and as for Strabo, we can expect but little satisfaction; for his description of places, having more of the historian and philosopher, than the exactness of a mathematician, who strictly respects the position of places, without inquiry after their nature, qualities, and inhabitants, (though the best geography, would be a mixture of them all, as Abulfeda, an Arabian Prince, in his rectification of countries above 300 years since has done ;) for these reasons we can expect little satisfaction from Strabo, and less may we hope for from Dionysius Aser, Arrianus, Stephanus Byzantinus, and others. Wherefore next having recourse to the Arabians, who in geography deserve the second place after the Grecians, we find in Nassir Eddin the latitude of Byzantium, which he terms Buzantiya, and Constantiniya, to be 45°; and in Ulug Beg's astronomical tables the same is expressed. Abulfeda also, who chiefly follows four principal authors as his guides, in compiling his geopraphical tables, viz. Alfaras, Albiruny, Hon Saiid Almagraby, and Ptolemy, says that all these place Byzantium in 45° of latitude. And here it may justly be wondered how this difference should arise between the Greek copies of Ptolemy, and those translated into Arabic by the command of Almamon, the learned Calif of Babylon; for Abulfeda expressly relates, that Ptolemy was first interpreted in his time, that is, in the computation of Almecinus in Erpenius's edition, and of Emir Cond a Persian historiographer, more than 800 years since: concerning which Abulfeda writes thus, "This book (Ptolemy's Geography) was translated out of the Grecian language into the Arabic, for Almamon:" And in this I find, by three fair MSS. of Abulfeda, Byzantium to be constantly placed in 45°, and as constantly in the Greek copies in 43° 5′. But in the πρόχειροι κανόνες of Chrysococca, out of the Persian tables, made about the year 1346 in Scaliger's calculation, it is placed in 45°. To reconcile the difference between the Greeks and Arabians may seem impossible, since the Greek copies agree among themselves, and the Arabic copies amongst themselves. The best way to end the dispute, will be, to give credit concerning the latitude of Byzantium neither to the Greeks, nor Arabians. And that I have reason for this assertion, appears by several observations of mine at Constantinople, with a brass sextant of above 4 feet radius. Where, at the summer solstice, taking the meridian altitude of the sun, I found the latitude to be 41° 6′.* And in this latitude in my chart I have placed Byzantium, and not in that either of the Greeks or Arabians. From which observation, being of singular use in the rectification of geography, it will follow, that all maps for the north-east of Europe, and of Asia, adjoining on the Bosphorus Thracius, the Pontus Euxinus, and much farther, are to be corrected; and consequently the situation of most cities in Asia, properly so called, are to be brought more southerly than those of Ptolemy, by almost 2 whole degrees, and than those of the Arabians, by almost 4.

Concerning Rhodes, it may be presumed, that, having been the mother and nurse of so many eminent mathematicians, and having long flourished in navigation, by the direction of these, and by the vicinity of the Phœnicians, they could not be ignorant of the precise latitude of their country, and that from them Ptolemy might receive a true information; though it cannot be denied, but that in places more remote from Alexandria, he has much erred.

Now, according to Ptolemy, the parallel passing through Rhodes, is in 36 degrees of latitude. But Abulfeda, in some copies, places the island Rhodes, in the latitude of 37° 40′: and the geography of Said Ibn Aly Algiorgany, in 37 degrees, if it be not by a transposition in the MS. of the numerical letters in Arabic 37 for 36, which by reason of their similitude are often confounded in Arabic MSS. By my observations, however, under the walls of the city Rhodes, with a fair brass astrolabe of Gemma Frisius, of 14 inches diameter, I found the latitude to be 37° 50′. A larger instrument I durst not venture to carry on shore in a place of so much jealousy. And this latitude in the chart I have assigned to the city Rhodes, better agreeing with the Arabians, than with Ptolemy, whom I know not how to excuse.

Francisci Willughbeii Armig. de Historia Piscium Libri quatuor, Jussu et Sumptibus Societatis Regiæ Lond. edit. Totum Opus recognovit, coaptavit, supplevit, Librum etiam primum et secundum integros adjecit Johannes Raius è Societate Regia, Oxonii è Theatro Sheldoniano, 1686. N° 178, p. 1301.

The work is divided into 4 books. The first treats of fishes in general: the

* The most modern and correct accounts now make the latitude of Constantinople to be 41° 1′ 24″, and its longitude 28° 55′ 49″ east of Greenwich

2d of the cetaceous or whale kind: the 3d of cartilaginous fishes, which have gristles instead of bones: the 4th and last of those called spinose fishes, that have small sharp bones in their flesh; after which is annexed a large appendix.

In the first book are considered both the internal and external parts of fishes. Next to the parts of fishes, and their uses, many things are observed concerning their hearing, their respiration, their motions, generation, food, growth, age, and their division. Thus, it is noted, concerning the eyes of fishes, that they are flatter or more depressed than those of quadrupeds, but the crystalline humour rounder, indeed either exactly spherical, or very nearly so. They want eye-lids, and it is shown that they are not necessary to them. Concerning the hearing of fishes; it is remarked, that in no fishes, besides the cetaceous kind, have hitherto been found any auditory passages or ear-holes, and whether they hear or not, is a question not yet fully decided, notwithstanding all the experiments alleged to prove the affirmative.* Concerning the gills of fishes, it is shown, that they are only as it were inverted lungs, and are of the same use to fishes as lungs to quadrupeds: that the whole mass of blood in every circulation passes through the gills, as in quadrupeds it does through the lungs; only it returns not again to the heart, and therefore in fishes, both the vessels which carry it to the gills, and those that bring it back, are arteries. It is also proved, that fishes have a kind of respiration, as well as land animals, and that the use of the air is as necessary to those as to these; so that if it be inclosed, or made any ways unfit for life, they cannot live; as is clearly evidenced by great variety of undoubted experiments. The fins serve not so much for progressive motion, as for keeping the body erect, and for moving it to and fro, upwards and downwards in the water: the progression is performed principally by the motion of the tail, by the sudden extension whereof, after being inflected, the fish shoots itself forward with great force and velocity. Vid. Phil. Trans. N° 115. The swimming-bladders, found in most fishes, serve to bring the body to an equilibrium with the element in which they swim; that so they may with more facility be impelled or moved any way. That this is one use of it, is demonstrated from experience, for upon breaking that bladder, the fish is no longer able to support itself in the water, but presently sinks down, and constantly lies groveling at the bottom. From this air-bladder there is a channel or passage in most fishes, leading to the mouth of the stomach, but in some

* That fishes are endowed with the sense of hearing is now no longer a matter of doubt with physiologists. The organs of this sense have been detected by modern anatomists in various other fishes besides those which belong to the cetaceous order; as may be seen by referring to Mr. John Hunter's observations, inserted in the 72d vol. of these Transactions, and to the works of Professors Camper and Monro.

few to the bottom of it, and there terminated: which probably serves for the emission and reception of air at pleasure, to balance the body, according as either its gravity, or that of the medium is altered. The received opinion that fishes have neither reins nor bladder for urine, is from experience contradicted, it being found by dissection, that few fishes want a bladder, none reins. The situation of the appendices or blind-guts is observed to be different in fishes, from what it is in other animals; for in these they are situated at the end of the guts, at least of the small guts; in those at the beginning, next the stomach.

Next to the parts of fishes and their uses, many things are said concerning the generation of fishes; which is shown to be threefold, according to the three chief kinds of fishes. For, 1, the cetaceous kind (which are rightly called by Latines belluæ marinæ, sea-beasts) generate exactly after the manner of viviparous quadrupeds. 2. The cartilaginous kind somewhat resemble birds in their manner of generation, for they breed large eggs, with distinction of yolk and white; only they do not lay them, but cherish and hatch them in their wombs, and so bring forth live young ones; as vipers, and perhaps some other sorts of serpents do. 3. The spinose kind, (under which name are comprehended all other fishes beside the fore-mentioned,) conceive an innumerable number of small eggs called spawn, which probably answer to the cicatriculæ in birds' eggs, and that (as is conceived) without any coition or copulation with the male; on which spawn, as soon as it is cast by the female, the male or milter presently comes and scatters his milt or seed.

In the last place is delivered a general division of fishes, according to the characteristic notes of nature. As, 1, into the cetaceous kind, which in most of their internal parts, the conformation of their brain, their manner of respiration and generation, agree exactly with viviparous quadrupeds. 2. The cartilaginous kind, by which name are not to be understood all sorts of fishes that have gristles instead of bones, but those which Aristotle calls σελάχη, which are, as he phrases it, inwardly oviparous, breeding great eggs like to those of birds, or rather serpents, but outwardly viviparous, laying and hatching those eggs in their own bellies, and so bringing forth live young. 3. The spinose kind, under which name are comprehended all fishes that are oviparous, or cast their spawn, indeed all besides those belonging to the two foregoing genera. Those of the cetaceous kind being but few, are not subdivided in this work. The cartilaginous kind are divided into those that are long and round bodied, called sharks and dogs; and those that are flat and broad, which are again subdivided into suborbinate genera. The spinose fishes, which are the most numerous, are divided into, 1, the flat kind, which swim lying on one side; as the sole, plaise, &c. 2. The anguilli-formes, or eel-like fishes, that are long, slimy, and lubricous, having either no scales or very small ones; many of which want the belly fins, or lower pair of fins; such are the conger, common eel, sea serpent, eelpout, &c. 3. Fishes of a shorter, more contracted, and thicker body, that want the second or lower pair of fins. Of this kind are the globe-fishes, either prickly or smooth; the triangular and quadrangular fishes, the file-fishes, hippocampi, &c. The rest of this kind, which have two pairs of fins, are divided, according to the number and quality of their back-fins, into, 4. Such as have three fins on their backs, which are only those of the cod-kind. 5. Such as have two fins on their back, and 1, such as have them both with soft and flexible rays or nerves. 2. Such as have the foremost with stiff and spinose rays, the hindermost with soft and flexile. 6. Such as have but one single fin on the back, and 1, such as have all the rays of the said fin soft, flexile, and nervose. 2. Such as have the foremost rays thereof stiff and spinose, the hindermost soft and flexile. In the first book is also exhibited a catalogue of English fishes, as many as have come to the knowledge of the authors; as well such as are found in salt, as in fresh waters.

The 2d book, which treats of cetaceous fishes, gives, first, general notes of this kind; 2dly, particular descriptions of the several species. The 3d book contains the cartilaginous kind. These have gills in common with the spinose tribe; but, instead of single apertures, they have five oblong holes on each side. These all want scales and the swimming bladder; they have gristles instead of bones, as their name imports, and have their mouths in the prone or underside of their bodies; the males have two penis-like appendices annexed to the fins encompassing the vent, and generate as is before declared. Many of these fishes are very voracious, and of speedy concoction, as the sharks; yet no acid humour is to be perceived by taste in their stomachs, of which there is a very memorable example. The 4th book, which comprehends all the spinose and oviparous fishes, is divided into many sections or subordinate genera, the titles of which are already enumerated.

Godefridi Bidloo, M.D. Anatomia Humani Corporis. Amstel. fol.* 1685.
N° 178, p. 1309.

This anatomy is perfectly demonstrative, or iconographical, consisting of 125

* The above-mentioned author, Godfrey Bidloo, was a native of Amsterdam, and professor of anatomy and surgery at Leyden. Being appointed physician to King William III, he accompanied him to England. On the death of this Prince, of whose last moments he published an account in the Dutch language, he returned to Holland and resumed his professorship at Leyden, where he died 1713, aged 64. Besides his Anat. Corp. Hum., he wrote De Animalculis Hepatis Ovilli; Exercitat. Anatomico-chirurg. and two controversial tracts, one against Ruysch, viz. Vindiciæ quarund. De-

tables of large and most elegant cuts, with explications of them; the draughts were taken by the curious Gerard de Lairess, and are said to be all originals. The method observed is, 1. Of the head. 2. Of the breast. 3. Of the abdomen. 4. Of the womb, as it is in pregnant women; and of the fœtus, &c. 5. Of the muscles of the limbs. 6. The osteology. It is a beautiful work, which, without engaging in controversies concerning the uses of parts, represents to the eye the whole anatomy of man.

lineationum Anatom. and another against Cowper, entitled Gul. Cowper Criminis Literarii citatus coram Tribunali Soc. Anglicanæ. In this last tract he accuses Cowper, or his bookseller, of having purchased from the Dutch proprietors the copper-plates engraved for his (Bidloo's) Anat. Corp. Hum. and of having used them in his own (Cowper's) work, after altering the explanations, &c. without once acknowledging the quarter from whence they were derived. The engravings belonging to Bidloo's anatomy do credit, as Haller observes, to the artist; and many of the parts, and especially the viscera, are not unfaithfully represented. It is otherwise, however, with the muscles and some of the blood-vessels, where we find much inaccuracy, for which Bidloo and not the engraver is censurable; and with regard to his microscopic representations, they are in a great measure imaginary.

END OF VOLUME FIFTEENTH OF THE ORIGINAL.

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*A Discourse concerning Gravity and its Properties; together with the Solution of a Problem of great Use in Gunnery. By E. Halley.** N° 179, p. 3. *Vol. XVI.*

Nature, amidst the great variety of problems wherewith she exercises the wits of philosophical men, scarcely affords any one wherein the effect is more visible, and the cause more concealed, than in those of the phænomena of gravity. Before we can go alone, we must learn to defend ourselves from the violence of its impulse, by not trusting the centre of gravity of our bodies beyond our reach; and yet the acutest philosophers, and the subtilest inquirers into the original of this motion, have been so far from satisfying their readers, that they themselves seem little to have understood the consequences of their own hypotheses.

The notion of Descartes seems to be quite incomprehensible; he would have the particles of his celestial matter, by being reflected from the surface of the earth, and so ascending from it, to drive down into their places those terrestrial bodies they find above them: this is as near as I can gather the scope of the 20, 21, 22, and 23 sections of the last book of his Principia

* It would seem that the numbers of this 16th volnme of the Transactions were collected and prepared by Dr. Halley, as the volume is addressed in his name, to the earl of Carberg, the president.

Philosophiæ; yet neither he, nor any of his followers, can show how a body suspended in libero æthere shall be carried downwards by a continual impulse tending upwards.

Vossius and others assert the cause of the descent of heavy bodies, to be the diurnal rotation of the earth upon its axis; not considering that, according to the doctrine of motion, all bodies moved in a circle, recede from the centre of their motion; by which an effect contrary to gravity would follow, and all loose bodies would be thrown into the air in a tangent to the parallel of latitude without the intervention of some other principle to keep them fast, such as that of gravity. Besides, the effect of this principle is found throughout the whole surface of the globe nearly equal; and certain experiments seem to argue it rather less near the equator, than towards the poles; which could not be the case, if the diurnal rotation of the earth on its axis were the cause of gravity; for where the motion is swiftest, there the effect would be most considerable.

Others assign the pressure of the atmosphere, as the cause of this tendency towards the centre of the earth; but unhappily they have mistaken the effect for the cause, it being plain from undoubted principles, that the atmosphere has no other pressure but what it derives from its gravity; and that the weight of the upper parts of the air, pressing on the lower, do so far bend the springs of that elastic body, as to give it a force equal to the weight that compressed it, having of itself no force at all: and supposing it had, it will be very hard to explain the modus, how that pressure should occasion the descent of a body circumscribed by it, and pressed equally above and below, without some other force to draw or push it downwards. But to demonstrate the contrary of this opinion, an experiment was long since shown before the Royal Society, by which it appeared, that the atmosphere was so far from being the cause of gravity, that its effects are much more vigorous where the pressure of the atmosphere is removed; for a long glass receiver, having a light down-feather included, being evacuated of air, the feather, which in the air would hardly sink, did in vacuo descend with nearly the same velocity as a stone.

Some think to illustrate this descent of heavy bodies, by comparing it with the virtue of the loadstone. But, setting aside the difference in the manner of their attractions, the loadstone attracting only in and about its poles, but the earth almost equally in all parts of its surface, this comparison avails no more than to explain unknown things by another equally so.

Others assign as the cause, a certain sympathetical attraction between the earth and its parts; whereby they have, as it were, a desire to be united. But this is so far from explaining the modus, that it is little more than telling us in other terms, that heavy bodies descend, because they descend.

But though the efficient cause of gravity be so obscure, yet its final cause is clear enough; for it is by this single principle that the earth and all the celestial bodies are kept from dissolution; the least of their particles not being suffered to recede far from their surfaces, without being immediately brought down again by virtue of this natural tendency; which, for their preservation, the infinite wisdom of their Creator has ordained to be towards each of their centres; nor can the globes of the sun and planets be otherwise destroyed, than by depriving them of this power of keeping their parts united.

The affections or properties of gravity, and its manner of acting on falling bodies, have been in a great measure discovered, and most of them made out by mathematical demonstration, by the accurate diligence of Galileo, Torricelli, Huygens, and others, and now lately by our worthy countryman Mr. Isaac Newton, who has an incomparable Treatise on Motion almost ready for the press. Of these properties, the first is, that by this principle of gravitation, all bodies descend towards a point, which either is, or else is very near to the centre of magnitude of the earth and sea, about which the sea forms itself exactly into a spherical surface, and the prominences of the land, considering the bulk of the whole, differ but insensibly from it. 2. This point, or centre of gravitation, is fixed within the earth, or at least has been so, ever since we have any authentic history: for a consequence of its change, though never so little, would be the overflowing of the low lands on that side of the globe towards which it approached, and the leaving new islands bare on the opposite side, from which it receded; but for these 2000 years it appears, that the low islands of the Mediterranean Sea, near which the more ancient writers lived, have continued much at the same height above the water, as they now are found; and no inundations or recesses of the sea, arguing any such change, are recorded in history; excepting the universal deluge, which can no better way be accounted for, than by supposing the centre of gravitation removed for a time towards the middle of the then inhabited parts of the world; and a change of its place, but the 2000th part of the radius of this globe, would be sufficient to bury the tops of the highest hills under water. 3. That in all parts of the surface of the earth, or rather in all points equidistant from its centre, the force of gravity is nearly equal; so that the length of the pendulum, vibrating seconds of time, is found in all parts of the world to be nearly the same. It is true that at St. Helena, in the latitude of 16° south, I found that the pendulum of my clock, which vibrated seconds, required to be made shorter than it had been in England by a very sensible space, before it would keep time; and since that, the like observations have been made by the French observers near the equator: yet I dare not affirm, that in mine it proceeded from any other cause, than the great height of my place of observation above the surface of the sea, by which the gravity being diminished, the length of the pendulum vibrating seconds is proportionably shortened.* 4. That gravity equally affects all bodies, without regard either to their matter, bulk, or figure; so that, the resistance of the medium being removed, the most compact and the loosest, the greatest and smallest bodies, would descend the same spaces in equal times; the truth of which appears from the experiment before cited. In these last two particulars, is shown the great difference between gravity and magnetism; the one affecting iron only, and that towards its poles, the other all bodies alike in every part. From hence it will follow, as a corollary, that there is no such thing as positive levity; those things that appear light, being only comparatively so; and whereas several things rise and float in fluids, it is because, bulk for bulk, they are not so heavy as those fluids; nor is there any reason why cork, for instance, should be said to be light because it swims on water, any more than iron because it swims on mercury. 5. That this power increases as you descend to, and decreases as we ascend from, the centre, and that in the proportion of the squares of the distances from it reciprocally, so as at a double distance to have but a quarter of the force; a principle on which Mr. Newton has made out all the phænomena of the celestial motions, in so easy and natural a manner, that its truth is past dispute. Besides, it is highly rational, that the attractive or gravitating power should exert itself more vigorously in a small sphere, and weaker in a greater, in proportion as it is contracted or expanded; and if so, seeing that the surfaces of spheres are as the squares of their radii, this power at several distances will be as the squares of those distances reciprocally; and then its whole action on each spherical surface, be it great or small, will be always equal. And this is evidently the rule of gravitation towards the centres of the Sun, Jupiter, Saturn, and the Earth; and thence is reasonably inferred, to be the general principle observed by nature in all the other celestial bodies.

These are the principal affections of gravity, from which the rules for the fall of bodies, and the motion of projects are mathematically deducible. Mr. Isaac Newton has shown how to define the spaces of the descent of a body, let fall from any given height, down to the centre, supposing the gravitation to increase, as in the fifth property; but considering the smallness of height, to which any projectile can be made to ascend, and over how small an arch of the globe it can be thrown by any of our engines, we may well enough suppose

* Hence it would appear, that Dr. Halley was not then aware that the slowness of the vibrations, which required the shortening of the pendulum, was owing to the decrease of gravitation, consequent on the oblate figure of the earth, and the centrifugal force from the rotation.

the gravity to be equal throughout: and the descents of projectiles to be in parallel lines, which in reality are towards the centre, the difference being so small, as by no means to be discovered in practice. The opposition of the air, it is true, is considerable against all light bodies moving through it, as likewise against small ones; but in large and ponderous shot, this impediment is found by experience but very small, and may safely be neglected.*

Propositions concerning the Descent of heavy Bodies, and the Motion of Projects.

Prop. I. The velocities of falling bodies are proportional to the times, from the beginning of their falls.—For the action of gravity being continual, in every space of time the falling body receives a new impulse, equal to what it had before, in the same space of time, received from the same power; for instance, in the 1st second of time, the falling body has acquired a velocity, which in that time would carry it to a certain distance, suppose 32 feet, and if there were no new force, it would descend at that rate with an equable motion; but in the next second of time, the same power of gravity continually acting on it, superadds a new velocity equal to the former; so that at the end of 2 seconds, the velocity is double to what it was at the end of the first: and after the same manner may it be proved to be triple at the end of the 3d second, and so on. Therefore the velocities of falling bodies are proportional to the times of their falls. Q. E. D.

Prop. II. The spaces described by the fall of a body, are as the squares of the times, from the beginning of the fall.—Let AB (fig. 1, pl. 9,) represent the time of the fall of a body; BC, perpendicular to AB, the velocity acquired at the end of the fall; and draw the line AC; then divide the line AB, representing the time, into as many equal parts as you please, as b, b, b, b, &c. and through these points draw the lines bc, bc, bc, bc, &c. parallel to BC. It is manifest that the several lines, bc, represent the several velocities of the falling body, in such parts of the time as Ab is of AB, by the former proposition. It is likewise evident, that the area ABC is the sum of all the lines bc, being taken, according to the method of indivisibles, infinitely many; so that the area ABC represents the sum of all the velocities, between none and BC, supposed infinitely many; which sum is the space descended in the time represented by

* How erroneous this notion is, we shall have occasion to notice hereafter, when we arrive at those parts of the Transactions containing some of Mr. Robins's papers relating to this subject; where it is found that the effect of the air's resistance, even on heavy iron cannon balls, instead of being inconsiderable, is in fact enormously great; so much so indeed that without that resistance, they would in many cases range 10 times, or 20 times as far, as they are now found to do; so that, instead of an extreme range of a mile or two, as is now the case, they would be capable of ranging to the distance of 10, 20, or 30 miles!

AB. And, by the same reason, the areas Abc will represent the spaces descended in the times Ab; so then the spaces descended in the times AB, Ab, are as the areas of the triangles ABC, Abc, which by the 20th of the 6 of Euclid, are as the squares of their homologous sides AB, Ab, that is, of the times. Therefore the descents of falling bodies, are as the squares of the times of their fall, Q. E. D.

Prop. III. The velocity, which a falling body acquires in any space of time, is double to that, with which it would have moved the space descended by an equable motion, in the same time.—For, draw the line EC parallel to AB, and AE parallel to BC, in the same fig. 1, and compleat the parallelogram ABCE: it is evident that its area may represent the space, a body moved equably with the velocity BC, would describe in the time AB; and the triangle ABC represents the space described by the fall of a body, in the same time AB, by the 2d proposition. Now the triangle ABC, is half the parallelogram ABCE, and consequently the space described by the fall is half what would have been described by an equable motion with the velocity BC, in the same time; therefore the velocity BC, at the end of the fall, is double to that velocity, which in the time AB would have described the space fallen, represented by the triangle ABC, with an equable motion, Q. E. D.

Prop. IV. All bodies on or near the surface of the earth, in their fall, descend in such a manner, as at the end of the 1st second of time, to have described 16 feet and 1 inch, London measure, and acquired the velocity of 32 feet 2 inches in a second.—This is made out from the 25th prop. of the 2d part of that excellent treatise of M. Huygens de Horologio Oscillatorio; where he demonstrates the time of the least vibrations of a pendulum to be to the time of the fall of a body, from the height of half the length of the pendulum, as the circumference of a circle is to its diameter; whence, as a corollary, it follows, that as the square of the diameter is to the square of the circumference, so is half the length of the pendulum, vibrating seconds, to the space described by the fall of a body in a second of time: and the length of the pendulum, vibrating seconds, being found $39\frac{1}{8}$ inches, the descent in a second will be found by the aforesaid analogy to be 16 feet 1 inch: and by the third prop. the velocity will be double thereto; and thus nearly it has been found by several experiments, which, by reason of the swiftness of the fall, cannot so exactly determine its quantity.

From these four propositions, all questions concerning the perpendicular fall of bodies are easily solved; and either the time, height, or velocity being assigned, the other two may be readily found. From them likewise is the doctrine of projectiles deducible, assuming the two following axioms, viz. That a body, put in motion, will move on continually in a right line with an equable motion, unless some other force or impediment intervene, by which it is accelerated, or retarded, or deflected. 2dly. That a body being agitated by two motions at a time, does by their compounded forces pass through the same points as it would do, if the two motions were divided and acted successively. As, for instance, suppose a body moved in the line GF, (fig. 2, pl. 9.) from G to R, and there stopping, by another impulse suppose it moved, in a space of time equal to the former, from R towards K, to V. I say, the body shall pass through the point V, though these two several forces, acted both at the same time.

Prop. V. The motion of all projects is in the curve of a parabola. For let the line GRF, in fig. 2, be the line in which the projectile is directed, and in which, by the first axiom, it would move equal spaces in equal times, were it not deflected downwards by the force of gravity. Let GB be the horizontal line, and GC perpendicular to it. Then the line GRF being divided into equal parts, answering to equal spaces of time, let the descents of the projectile be laid down in lines parallel to GC, proportional to the squares of the lines GS, GR, GL, GF, or as the squares of the times, as from S to T, from R to V, from L to X, and from F to B, and draw the lines TH, VD, XY, BC parallel to GF; I say the points T, V, X, B, are points in the curve described by the projectile, and that the curve is a parabola. By the second axiom, they are points in the curve, and the parts of the descent GH, GD, GY, GC, = to ST, RV, LX, FB, being as the squares of the times, by the 2d prop. that is, as the squares of the ordinates, HT, DV, YX, BC, equal to GS, GR, GL, GF, the spaces measured in those times; and there being no other curve but the parabola, whose parts of the diameter are as the squares of the ordinates, it follows that the curve described by a project, can be no other than a parabola: and saying, as RV the descent in any time is to GR or DV the direct motion in the same time, so is DV to a third proportional; that third will be the line called by all writers of conics the parameter of the parabola to the diameter GC, which is always the same in projects cast with the same velocity; and the velocity being defined by the number of feet moved in a second of time, the parameter will be found by dividing the square of the velocity by 16 feet 1 inch, the fall of a body in the same time.

Lemma. The sine of the double of any arch is equal to twice the sine of that arch into its cosine, divided by radius; and the versed sine of the double of any arc, is equal to the square of its sine divided by radius. For, let the arch BC, in fig. 3, be double the arch BF, and A the centre; draw the radii AB, AF, AC, and the chord BDC, and let fall BE perpendicular to AC: then the angle EBC will be equal to the angle BAD, and the triangle BCE will be similar to the triangle ABD; therefore it will be as AB to AD, so BC, or twice BD, to BE, that is, as radius to cosine, so twice sine to sine of the double arch. And as AB to BD, so twice BD or BC to EC; that is, as radius to sine, so twice that sine to the versed sine of the double arch; which two analogies resolved into equations are the propositions contained in the lemma to be proved.

Prop. VI. The horizontal distances of projections made with the same velocity, at several elevations of the line of direction, are as the sines of the doubled angles of elevation.—Let GB, fig. 2, the horizontal distance be $= z$, the sine of the angle of elevation, FGB, be $= s$, its cosine $= c$, radius $= r$, and the parameter $= p$. It will be as c to s, so z to $\frac{sz}{c} =$ FB = GC; and by reason of the parabola, $\frac{psz}{c} =$ to the square of CB, or of GF. Now as c to r, so is z to $\frac{rz}{c} =$ GF, and its square $\frac{rrzz}{cc}$ will be therefore $=$ to $\frac{psz}{c}$; which equation reduced, will be $\frac{psc}{rr} = z$. But, by the former lemma, $\frac{2sc}{r}$ is equal to the sine of the double angle of which s is the sine, therefore it will be as radius to sine of double the angle FGB, so is half the parameter to the horizontal range or distance sought; and at the several elevations, the ranges are as the sines of the double angles of elevation. *Q. E. D.*

Corol. Hence it follows, that half the parameter is the greatest random, and which happens at the elevation of 45°, the sine of its double being radius. Also, that the ranges equally distant above and below 45 are equal, as are the sines of all doubled arches to the sines of their doubled complements.

Prop. VII. The altitudes of projections made with the same velocity at several elevations, are as the versed sines of the doubled angles of elevation. For, as c is to s :: so is $\frac{psc}{rr} =$ GB, to $\frac{pss}{rr} =$ BF, and VK $=$ VR $= \frac{1}{4}$ BF, the altitude of the projection $= \frac{pss}{4rr}$. Now, by the foregoing lemma, $\frac{2ss}{r} =$ to the versed sine of the double angle, and therefore it will be as radius to versed sine of double the angle FGB, so is the height of the parameter, to the height of the projection VK; and so these heights, at several elevations, are as the said versed sine. *Q. E. D.*

Corol. From hence it is plain, that the greatest altitude of the perpendicular projection is a 4th of the parameter, or half the greatest horizontal range; the versed sine of 180° being $= 2r$.

Prop. VIII. The lines GF, or times of the flight of a project, cast with the same degree of velocity, at different elevations, are as the sines of the elevations. For, as c is to r :: so is $\frac{psc}{rr}$ = GB, by the 6 prop. to $\frac{ps}{r}$ = GF, that is, as radius to sine of elevation, so is the parameter to the line GF; so that the lines GF are as the sines of elevation, and the times are proportional to those lines; therefore the times are as the sines of elevation.—Ergo, &c.

Prop. IX. Problem 1. A projection being made at pleasure; having the distance and altitude or descent of an object, through which the project passes, together with the angle of elevation of the line of direction; to find the parameter and velocity; that is, in fig. 2, having the angle FGB, GM, and MX.—*Solution.* As radius to secant of FGB, so GM the distance given, to GL; and as radius to tangent of FGB, so GM to LM. Then LM − MX in heights, or + MX in descents, or else MX − ML, if the direction be below the horizontal line, is the fall in the time that the direct impulse given at G, would have carried the project from G to L, = LX = GY; then, by reason of the parabola, as LX or GY is to GL or YX, :: so is GL to the parameter sought. To find the velocity of the impulse by prop. 2 and 4, find the time in seconds that a body would fall through the space LX, and by that dividing the line GL, the quotient will be the velocity, or space moved in a second sought, which is always a mean proportional between the parameter and 16 feet 1 inch.

Prop. X. Prob. 2. Having the parameter, the horizontal distance, and height or descent of an object; to find the elevations of the line of direction necessary to hit the given object; that is, having given GM, MX, and the greatest random equal to half the parameter; to find the angles FGB.—Let the tangent of the angle sought be = t, the horizontal distance GM = b, the altitude of the object MX = h, the parameter = p, and radius = r; then it will be, as r to t so b to $\frac{tb}{r}$ = ML; and $\frac{tb}{r} \mp h$ $\left\{\begin{matrix}\text{in ascents}\\ \text{in descents}\end{matrix}\right\}$ = LX, and $\frac{ptb}{r} \mp ph$ = $GL^2 = XY^2$ by the parabola; but $bb + \frac{ttbb}{rr} = GL^2$ (47, 1 Euclid). Therefore $\frac{ptb}{r} \mp ph = bb + \frac{ttbb}{rr}$; which equation transposed, is $\frac{ttbb}{rr} = \frac{ptb}{r} \mp ph - bb$; and divided by bb, is $\frac{tt}{rr} = \frac{pt}{br} \mp \frac{ph}{bb} - 1$. this equation shows that the question has two answers, and its roots are $\frac{t}{r} = \frac{p}{2b} \mp \sqrt{\frac{pp \mp 4ph}{4bb} - 1}$; from which the following rule is derived; divide half the parameter by the horizontal distance, and reserve the quotient, viz. $\frac{p}{2b}$; then say, as square of the distance given is to the half parameter, so is the half parameter ∓ double $\left\{\begin{matrix}\text{height}\\ \text{descent}\end{matrix}\right\}$ to the square of a secant = $\frac{pp \mp 4ph}{4bb}$, the tangent answering to that secant will be $\sqrt{\frac{pp \mp 4ph}{4bb} - 1}$ or rr: so then the sum and difference of the quotient above found, and this tangent will be the roots of the equation, and the tangents of the elevations sought.

Note, That in descents, if the tangent exceed the quotient, as it does when ph is more than bb, the direction of the lower elevation will be below the horizon; and if $ph = bb$, it must be directed horizontal, and the tangent of the upper elevation will be $\frac{pr}{b}$. Note also, that if $4\,bb + 4ph$ in ascents, or $4\,bb - 4ph$ in descents, be equal to pp, there is but one elevation that can hit the object, and its tangent is $\frac{pr}{2b}$; and if $4\,bb + 4\,ph$ in ascents, or $4\,bb - 4\,p\,h$ in descents, exceed pp, the object is without the reach of a project thrown with that velocity, and then the thing is impossible.

From this equation, $4\,bb \mp 4\,ph = pp$, are determined the utmost limits of the reach of any project, and the figure assigned, wherein are all the heights on each horizontal distance, beyond which it cannot pass; for by reduction of that equation h will be found $= \frac{1}{4}p - \frac{bb}{p}$ in heights, and $\frac{bb}{p} - \frac{1}{4}p$ in descents; from whence it follows, that all the points h are in the curve of the parabola, whose focus is the point from whence the project is cast, and whose latus rectum os parameter to the axis is $= p$. Likewise, from the same equation may the least parameter or velocity be found, capable to reach the object proposed; for $bb = \frac{1}{4}pp \mp ph$ being reduced, $\frac{1}{2}p$ will be $= \sqrt{bb + hh} \pm h \left\{ \begin{array}{l} \text{in ascents} \\ \text{in descents} \end{array} \right\}$, which is the horizontal range at 45 degrees, that would just reach the object, and the elevation requisite will be easily had; for dividing the semiparameter so found by the given horizontal distance b, the quotient into radius will be the tangent of the elevation sought. This rule may be of good use to all bombardiers and gunners, not only that they may use no more powder than is necessary to throw their bombs into the place assigned, but that they may shoot with much more certainty, because a small error committed in the elevation of the piece will produce no sensible difference in the fall of the shot; for which reasons, the French engineers, in their late sieges, have used mortar pieces inclined constantly to the elevation of 45°, proportioning their charge of powder according to the distance of the object they intend to strike on the horizon.

And this is all that need to be said concerning this problem, of shooting upon heights and descents. But if a geometrical construction of it be required, I think the following is as easy as any can be expected, which I deduce from the foregoing analytical solution, viz. $\frac{t}{r} = \frac{p}{2b} \pm \sqrt{\frac{\frac{1}{4}pp \pm ph - bb}{bb}}$: Having made the right angle LDA, pl. 9, fig. 4, make DA, DF $= p$, or greatest range, DG $= b$ the horizontal distance, and DB, DC $= h$, the perpendicular height of the object; draw GB, and make DE = to it. Then with the radius AC and centre E sweep an arch, which if the thing be possible, will intersect the line AD in H; and the line DH, being laid both ways from F, will give the points K and L, to which draw the lines GL, GK; I say the angles LGD, KGD are the elevations requisite to strike the object B. But note, that if B be below the horizon, its descent DC = DB must be laid from A, so as to have AC = to AD + DC. Note also, that if in descents DH be greater than FD, and so K fall below D, the angle KGD shall be the depression below the horizon. And this construction so naturally follows from the equation, that I shall need say no more about it.

Prop. XI. To determine the force or velocity of a project, in every point of the curve it describes.—To do this, we need no other præcognita than only the third proposition, viz. that the velocity of falling bodies is double to that which in the same time would have described the space fallen by an equable motion: for the velocity of a project is compounded of the constant equal velocity of the impressed motion, and the velocity of the fall, under a given angle, viz. the complement of the elevation; for instance, in fig. 2, in the time in which a project would move from G to L, it descends from L to X, and by the third proposition has acquired a velocity, which in that time would have carried it by an equable motion from L to Z, or twice the descent LX; and drawing the line GZ, I say, the velocity in the point X, compounded of the velocities GL and LZ, under the angle GLZ, is to the velocity impressed in the point G, as GZ is to GL; this follows from the second axiom, and by the 20th and 21st prop. lib. 1, conic, Midorgii, XO parallel and equal to GZ will touch the parabola in the point X. So that the velocities, in the several points, are as the lengths of the tangents to the parabola in those points, intercepted between any two diameters; and these again are as the secants of the angles, which those tangents continued make with the horizontal line GB. From what is here laid down, may the comparative force of a shot, in any two points of the curve, be either geometrically or arithmetically discovered.

Corol. From hence it follows, that the force of a shot is always least at V, the vertex of the parabola; and that at equal distances from thence, as at T and X, G and B, its force is always equal; and that the least force in V is to that in G and B, as radius to the secant of the angle of elevation FGB.

The 10th proposition contains a problem, untouched by Torricelli, which is of the greatest use in gunnery, and for the sake of which this discourse was principally intended; it was first solved by Mr. Anderson, in his book of the genuine use and effects of the gun, printed in 1674; but his solution required so much calculation, that it put me on finding out whether it might not be done more easily; and accordingly, in 1678, I discovered the rule I now publish, and from it the geometrical construction.

Now these rules would be rigidly true, were it not for the resistance of the medium, by which not only the direct impressed motion is continually retarded, but likewise the increase of the velocity of the fall, so that the spaces described thence are not exactly as the squares of the times; but what this resistance of the air is, against several velocities, bulks, and weights, is not so easy to determine. It is certain, that the weight of air to that of water is nearly as 1 to 800; whence its weight to that of any project is given. It is very likely, that to the same velocity and magnitude, but of different matter, the resistance will be reciprocally as the weights of the shot; as also, that to shot of the same velocity and matter, but of different sizes, it should be as the diameters reciprocally; whence generally, the resistance to shot with the same velocity, but of differing diameters and materials, should be as their specific gravities into their diameters reciprocally; but whether the opposition to differing velocities of the same shot be as the squares of those velocities, or as the velocities themselves, or otherwise, is yet a more difficult question. However it be, it is certain that in large shot of metal, whose weight many thousand times surpasses that of the air, and whose force is very great in proportion to the surface; this resistance is scarcely discernible, for by several experiments, made with all care and circumspection, with a mortarpiece extraordinary well fixed to the earth on purpose, which carried a solid brass shot of $4\frac{1}{2}$ inches diameter, and of about 14 lb. weight, the ranges above and below 45 degrees were found nearly equal; if there were any difference, the under ranges went rather the farthest; but those differences were usually less than the errors committed in ordinary practice, by the unequal goodness and dryness of the same sort of powder, by the unfitness of the shot to the bore, and by the looseness of the carriage.

In a smaller brass shot, of about $1\frac{1}{4}$ inch diameter, cast by a cross bow, which ranged it at most about 400 feet, the force being much more equal than in the mortarpiece, this difference was found more curiously, and constantly, and most evidently the under ranges exceeded the upper. From which trials I conclude, that although in small and light shot, the opposition of the air ought and must be accounted for; yet in shooting of great and weighty bombs, there need be very little or no allowance made; so that these rules may be put in practice to all intents and purposes, as if this impediment were absolutely removed.*

* The experiment mentioned in the preceding lines, as well as in those that follow, appear to have been made with too little force, or charge of powder, to determine the point in question. For with

An Experiment of Shooting by the Rarefaction of the Air. By Dr. D. Papin, R. S. S. N° 179, p. 21.

Ordinary wind-guns produce their effect by the compression of the air; but Ottho Guerick found out a new sort that shoots by rarefaction, and he has published that device at large in his book on Pneumatic Experiments. I have therefore had the curiosity to try it myself by another contrivance, which I take to be better than his; first, because I can make a rarefaction much more perfect than he could do. 2dly, because his device could only be used for guns of a small bore; whereas my way may be applied to the largest bores. So that one might by this means throw up vast weights to a great distance.

A A pl. 9, fig. 5, is a pipe, very equal from one end to the other. B B a small pipe soldered to a hole near the end of the pipe A A, and applied to the plate of the pneumatic engine. C C some kind of stool, to bear up the hinder part of the pipe A A. D a piece of lead fitted to the bore of the pipe A A.

The pipe A A is to be shut at both ends by valves outwardly applied, and so the said pipe A A, though never so large, may be exhausted of air by means of the pneumatic engine; which done, the valve towards D must be suddenly opened, that the whole pressure of the atmosphere acting on the lead D may drive it along the pipe A A with such rapidity, that it will be able to carry it to a great distance. And because such a valve shutting a great hole would prove very difficult to be opened, when the pipe A A is of a large bore, the aperture towards D may be left much smaller than the pipe; the velocity of the air being so great, that even through a pretty small aperture it presses the lead D as freely almost as if the whole bore was quite open.

Having prepared a barrel carrying a lead of 2 ounces, the experiment was shown before the Royal Society; and the effect was found very considerable, the force being little less than that of the wind-gun by compression; the same experiment being afterwards repeated with a longer barrel, it was found that the length in this way of shooting has very little, if any advantage.

Part of a Letter from Dr. Salomon Reisel, chief Physician to the Duke of Wirtemburg, concerning an Extraordinary Tincture given to a Stone. Stutg. Febr. 12mo. 1686. N° 179, p. 22. *Translated and extracted from the Latin.*

In this letter it is stated that a certain jeweller of Stutgard, named Christo-

small charges or velocities the defect of range is not very great; and besides, at elevations equally above and below 45° the effects would be nearly equal, and therefore little or no difference would appear; only the lower elevation must range the farther, because passing through a less quantity of resisting medium. So that on the whole it appears that Mr. Halley had deceived himself by means of an inadequate experiment.

pher Muller, having prepared a precipitate of gold from a solution of this metal in aqua regis, by means of oil of tartar, and having triturated in a chalcedony-saucer the said precipitate (moistened with water) with some red glass, to be afterwards fused for the purpose of enamelling (according to the directions given by Neri in the 6th book of his Ars Vitraria) he found, after repeating the trituration a 3d time, that this red powder (which had remained in the vessel for several days) had stained not only the chalcedony-saucer itself (which before was of a pellucid onyx colour throughout) in various places, but the pestle also, and had penetrated so deep (though the substance of the chalcedony was so hard as not to be touched by a file) that the stain could not be got out either by pure water, by lixivia, or by any other liquors; at the same time the fine polish of the chalcedony was not at all impaired. But when this experiment was repeated with another chalcedony vessel, it did not become coloured like the former.

A Catalogue of Simple and Mixed Colours. By R. Waller, F. R. S. N° 179, p. 24.

Having sometime since seen a table of the simple colours made use of in limning and painting, I have here endeavoured to give a more philosophical and useful one by the addition of some mixed colours. Not that I pretend to give the shades of all the mixed colours, which are indeed infinite, as the compositions and proportions of them may be unlimited; but I have mixed each of the simple yellows and reds with each of the simple blues, and these mixtures give most of the medium colours, viz. greens, purples, &c. To know what each of these mixed colours is compounded of, you need but look to the top of the table directly over the colour inquired after, where you may find the one ingredient, and at the side, in the same row, the other. I expect that this table will be of some use and advantage in the describing of the colours of natural bodies. Thus to describe a plant, it may be seen which of the simple or mixed colours comes nearest to it, and then the word affixed to that colour may be made use of.

A short Description of the Simple Colours specified in this Table.—1. Spanish white, made of chalk and alum burnt together. 2. I take the lapis Armenius to be the blue bice sold in the shops, for it is light and friable; formerly it was brought out of Armenia, but now from the silver mines of Germany, called Malachites, in high Dutch Bergblaw. 3. Ultramarine is made of the bluest lapis lazuli, which is freest from gold-veins by calcination. 4. Smalt is made of zaffer and pot-ashes, calcined together in a glass-furnace. 5. Litmase, or litmus, I suppose the juice of a plant. 6. Indigo, said by Pliny to be brought from India: a kind of mud adhering to the froth about reeds, and which when tried with a coal, the true burns with a purple-flame, and smells of the sea: Linschoten says, it is called anil, that it grows in Camaia, and is a plant like rosemary, which is gathered and dried, then wetted with fair water, and beaten to a mud; this operation being repeated, it is dried and fitted for use.* 7. Indian ink; its use known to Pliny, though not its composition; which is yet undiscovered, except it should be burned rice, as has been thought. The yellows and reds made use of, are these that follow.

1. Cerusse is the rust of lead, made by a vaporous calcination; Pliny writes thus of it in the 34 lib. cap. 18. Cerusse, or psmythian, is made in the plummers' shops of small plates of lead laid upon each other on a vessel of very strong vinegar; what falls into the vinegar is taken out, and dried in the sun: and he says it was made at Rome of burned marble flint, quenched in vinegar. 2. Masticot is a kind of improper calx of tin.† 3. Gutta gambæ, or gambodia, the inspissated juice of a plant, not well known;‡ it comes from the [East] Indies. Some think it the juice of euphorbium; others scammony, or tithimal; others ricinus; others refer it to the greater cataputia, esula, or the flowers of the Indian ricinus, and will have it coloured with turmeric. 4. Ochre, a kind of natural earth. There are two sorts; the one native, formerly brought out of Attica, now from Dacia and Hungaria, and from many places of England, especially in the forest of Dean; the other a factitious substance of lead, burned and quenched in vinegar. In Pliny's time it was made of rubrica, or reddle burned. 5. Orpiment, a fat inflammable mineral, justly ranked among poisons, for its extreme corrosive quality. Pliny says it was dug up in Syria on the surface of the earth; and that the Emperor Caligula had hopes of getting gold out of it; hence he caused 14 pounds of it to be tried, which afforded him very good gold, but in so small a proportion, that he lost by the trial.§ 6. Umber is a native earth. 7. Red-lead, a colour unknown to the ancients, made of litharge or burned lead, by a reverberatory calcination, or of cerusse put in a platter over the fire; which must be continually stirred till it has acquired a red-lead colour. 8. Burned ochre is the common yellow ochre burned in the open fire. 9. Cinnabar or vermillion. There are two sorts; native, or the minium of the ancients, which is the mineral that yields quicksilver; of which and of sulphur it chiefly consists. It is found in the mines of Istria. This colour was among the ancient Romans used to sacred purposes; and on festivals, Jupiter's

* Indigo is now well known to be a sort of fecula extracted from the plant termed indigofera tinctoria.

† Masticot is a calx of lead.

‡ Gamboge is a gum-resin obtained from a tree denominated by botanists stalagmites cambogioides.

§ Orpiment is a compound of sulphur and arsenic.

face was painted with it, and the bodies of those that entered in triumph. The factitious cinnabar is that which we now use; and is made by a sublimation of mercury and sulphur. 10. Carmine, made of cochineal. 11. Lake, thought to be an Arabic word: it is made of flocks died, or shavings of scarlet-cloth, or of the cochineal insect, or else of kermes-berries, their tincture being extracted with a lye of pot-ashes, and then precipitated with a solution of rock-alum. After the same manner a lake may be made of any plant or flower. There is also another sort of lake made of gum-lac, by extracting its tincture with urine. 12. Sanguis draconis, (dragon's blood) is the gum of a tree, which looks like dried blood; it is brought out of several places in the East Indies; and the tree which produces it is well described in the Hortus Malabaricus. 13. English reddle or ruddle, is found in many places of England; among the rest, near Witney in Oxfordshire. 14. Lamp-black, by Pliny thus described: it is made of the soot of rosin or pitch, burned, houses being built on purpose for it, that keep in the smoke. Its use is in writing-books, lib. 35, cap. 6.

Accounts of Books.

I. An Essay towards the recovery of the Jewish Weights and Measures, comprehending their Money, by help of Ancient Standards compared with ours of England. By Richard Cumberland, D.D. 8vo. Lond. 1686. N° 179, p. 33.

The learned author of this treatise has here collected the several testimonies, both ancient and modern, sacred and profane, that may give any light into the discovery of the ancient Jewish weights and measures. For this purpose, depending chiefly on the experience of Mr. Greaves, whose integrity was never yet questioned, and who with his own hands compared our English standard foot with the several foreign measures our author has occasion to use. The book consists of four chapters; the first gives an account of the method proper to be used in this inquiry. The second proves by many arguments the probability that the Jewish ammab or cubit, was the same with the present Egyptian cubit; for that the usual rise of the Nile, necessary for the fertilizing of Egypt, was in the days of Herodotus, as well as now, about 16 cubits; whence he concludes, that the old cubit of Egypt is not altered, but the divisions on the Nilometre are the same as in all antiquity: also that the constant necessity of surveying their lands, by reason the annual overflowing effaces the land-marks, obliged them to observe a constant standard to avoid confusion. Next he alleges that this cubit has not been altered by any conquest; the Babylonian cubit of 5 palms being shorter, and that of 6 being the same; that their next conquerors, the Greeks and Romans, have their cubit considerably shorter; and that the Turks, their present masters, have not introduced theirs, which is much longer, as appears by Mr. Greaves. Lastly, he shows, after the same, that the side of the great pyramid, and the length of the tomb within it, are measured by an even number of such Egyptian cubits; whence he concludes they were so designed at first, viz. the side of the pyramid to be 380 cubits, and the length of the tomb just 4; which carries with it a great show of probability. This done he proves the Jewish cubit the same with the Egyptian cubit, by several probable arguments; among which the chief seems to be, that the whole nation of the Jews had been for so many years subjects of Egypt, and carried undoubtedly away with them their weights and measures: and there is no testimony or reason to prove that the Jews have since altered them. Hence he concludes the old scripture cubit $21\frac{9}{10}$ inches English nearly, equal to what Mr. Greaves found the modern Egyptian cubit; and from hence makes a table of all the other measures whose proportions to the cubit are agreed upon.

The third chapter treats of the epha, and the other measures of capacity, endeavouring to prove the content of that measure equal to $\frac{1}{6}$ of the ardub, or cube of the Egyptian cubit; that is 7 gallons and a half and half a pint nearly, or very near the cube of the English foot, and containing just 1000 ounces avoirdupois of water; and hence he gives a table of the contents of all the other scripture-measures of capacity, having a known relation to the epha.

The fourth chapter treats of the weights and coins mentioned in scripture; and having by trial, as well as authority, found the weight of the shekel just half the Roman ounce, equal to the half ounce avoirdupois, he determines its value 2s. $4\frac{1}{4}$d. and thence derives the value of the gold and silver talent, weighing 3000 shekels. Lastly, he recommends for a universal standard, the length of the pendulum vibrating seconds, to be the horary yard, which he says is 3 feet $3\frac{1}{4}$ inches English, following in this Mr. Huygens, the first proposer of it. By the way he takes notice of the harmony that is between the measures and weights thus stated; for having the Egyptian cubit given, the sixth part of its cube is the epha, the tenth of it the homer, the tenth of that cotyla, the tenth of which is an ounce avoirdupois in water, whose half is exactly the weight of the shekel. The whole book being made up of very rare remarks, is well worth the perusal of the curious reader.

Note, That the learned Dr. Edward Bernard, in his late account of weights and measures, agrees nearly in the capacity of the epha, with what Dr. Cumberland has determined; for he makes the cube of the English foot to contain 76lb. Troy of spring-water, and the epha, or rather bath, to contain 75 such pounds; so that it is a pound less than the cube of a foot English, which Dr. Cumberland has stated about 12 ounces of water more than the said cube.

II. Ephemeris ad Annum a Nativitate Domini 1686, *ad Longitudinem Urbis Londinensis; ex Novis Hypothesibus exactissime supputata, et Regiæ Societati dicata, Londini in* 8*vo. Impensis Gulielmi Cooper.* N° 179, p. 35.

There being at this time a great want of ephemerides of any tolerable exactness, several of our astronomers were persuaded to undertake the calculation of one for this present year, which they have done from tables of their own, whose numbers, by many years observation, have been found to answer with great preciseness to the celestial motions, the moon only excepted; whose motion, by reason of her manifold inequalities, not being yet reduced to the nicety of the rest of the planets, it was thought needless to do any more than reduce her tychonic place in Argol, to our meridian, and compute the true latitudes. The several persons concerned have promised a continuation thereof for some years to come, which will make it valuable to all lovers of astronomy.

Account of a Journey of the Emperor of China into the Eastern Tartary, Anno 1682. *By Father Verbiest, Missionary to Pekin.* N° 180, p. 39.

The Emperor of China made a journey into the Eastern Tartary in the beginning of this year 1682, after having quelled a revolt, by the death of three rebellious kings, formed in some provinces of the empire: one of these princes was strangled in the province of which he had made himself master: the second, being brought to Pekin, with the principal heads of his faction, was cut to pieces in the sight of the whole court: the most considerable Mandarines acting with their own hands in this sad execution, to revenge upon this rebel the death of their relations, whom he had caused to be cruelly murdered. The third, who was the most considerable, and indeed the head of the revolt, had, by a voluntary death, prevented his deserved punishment, and so put an end to a war, which had lasted for 7 years.

Peace having been settled, by the re-establishing in the empire, and all the provinces, the peaceable enjoyment of their ancient liberties, the emperor departed the 23d of March, to go into the province of Leao-tum, the country of his ancestors, with a design of visiting their sepulchres, and, after honouring them with the usual ceremonies, of prosecuting his journey into the Eastern Tartary, a journey of about 1100 miles from Pekin.

The Emperor took with him his eldest son, a young prince of 10 years of age, who already had been declared heir of the empire. The three principal queens went also this journey, each in her gilded chariot; as also the principal kings which compose this empire, accompanied by all the grandees of the court, and the most considerable Mandarines of all the orders; who having all a great train of attendants, and splendid equipage, made a court about the emperor of more than 70,000 persons.

It was his will that I should accompany him in this journey, and that I should be always near his person, that I might make in his presence, the observations necessary for knowing the disposition of the heavens, the elevation of the pole, the magnetical declinations of every place, and for measuring with mathematical instruments the height of the mountains, and the distances of places: he was well pleased also to be informed of what concerned meteors, and many other physical and mathematical matters. Insomuch that he gave orders to an officer to carry upon horses such instruments as I should have occasion to make use of, and recommended me to the prince his uncle, who is also his father-in-law, and the second person of the state, being called by a Chinese name, which signifies an associate in the empire. In this journey we always proceeded towards the north-east. From Pekin to the province of Leao-tum, the road, being about 300 miles, is pretty equal: in the province of Leao-tum itself it is about 400 miles, but much more unequal by reason of the mountains: from the frontiers of this province to the city of Ula, or the river which the Tartars call Songoro, and the Chinese Sumhoa, the way, which is about 400 miles, is very difficult, being sometimes crossed by mountains extremely steep, sometimes by valleys of extraordinary depth, and through desert plains, for two or three days. The mountains of this country are covered on the east side with large oaks and old forests, which have not been cut for ages.

All the country beyond the province of Leao-tum is exceedingly desert, where nothing is to be seen on all sides but mountains and valleys, and dens of bears, tigers, and other beasts of prey; there is scarcely a house to be seen, but only some poor reed-huts, on the sides of some brooks and streams. All the cities and towns in the province of Leao-tum, and which are in very great numbers, are entirely ruinous; for the petty king of the Tartars, who kindled this war, having but a very small army, caused the inhabitants of those places to take arms, and destroyed the towns, that he might deprive his soldiers of the hopes of ever returning again to their own homes.

The capital of Leao-tum, called Xin-Yam, is a fair city and pretty entire, and has still the remains of an ancient palace. Its latitude is about 41° 56′; and there is no declination of the magnetic needle. The city of Ula, which was almost the extremity of our journey, lies in 44° 20′; and there the needle declines from the south to the west 1° 40′.

From Pekin to this place towards the east there is made a new road, by which the Emperor can commodiously pass with his horse, and the queens in their chariots. This road is about 10 feet broad, and as smooth and straight as could possibly be made; it extends above 1000 miles, and has a little rising on each side, of about a foot high, every where equal and perfectly parallel to one another; and this way was as neat, especially when the weather was fair, as a threshing floor, there being certain persons all along this way, to take care to smooth and cleanse it. There was made for their return, a road like the former planing or levelling the mountains, and raising bridges over the rivers; and for ornamenting them, they had extended on each side of them a sort of mats, on which were painted divers figures of animals, which had the same effect as tapestry hangings. The emperor seldom made use of this road, being mostly occupied in hunting: and when he accompanied the queens, he only rode by the side of it, that the great numbers of horse in his train might not spoil it. He usually marched at the head of this kind of army. The queens followed immediately, but at some distance, in their chariots, with their train and equipage; after these marched the kings, the grandees of the court and the mandarines, every one according to his rank; behind these an infinity of attendants, and other people on horseback, brought up the rear.

As there was not one city upon all this way, that could either lodge so great a multitude, or furnish them with provisions, they were obliged to carry along with them all necessaries for the journey, and even provisions for 3 months. On this account there were sent before, by the roads made on each side of the emperor's road, a great number of waggons, camels, horses, and mules, for carrying the baggage: besides these, the emperor, the kings, and almost all the grandees of the court, had great numbers of led horses, and droves of beeves, sheep, and other cattle; and though they marched by a road at a good distance from that of the emperor, yet they raised such clouds of dust, that it was difficult to distinguish any one at 15 or 20 paces. The march was so well regulated, that this army encamped every night by the sides of some river or brook; and for this reason the tents and baggage necessary for this encamping set out very early in the morning; and the quarter-masters, on their first arrival, marked the ground most proper for tents of the emperor, queens, kings, grandees of the court, and of the mandarines, each according to his dignity, and according to the rank he had in the Chinese militia, which is divided into 8 orders or standards.

In the space of 3 months, we marched about 1000 miles, advancing towards the north-east, and about as many in our return: in fine, we arrived at Kam-Hay, which is a fort situated between the South-sea and the mountains of the north: it is here the celebrated wall begins, which separates the province of Leao-tum from that of Pekely; from whence it extends very far northwards, over the tops of the highest mountains. On entering this province, the emperor, the kings, and the grandees of the court, quitted the great road, to take that of the mountains of the north, which are extended without interruption towards the north-east: there, some days were passed in hunting, which was performed in this manner: the emperor chose 3000 men of his life-guard, armed with arrows and javelins, and disposed them so, that they occupied a great circuit about the mountains, which they environed on all parts, forming a kind of circle of at least 3000 paces in diameter; then drawing nearer together with equal progress, and without quitting their range, whatever might be the obstacles in their way, they reduced this great circle to another much less, of about 300 paces in diameter: so that all the beasts which had been enclosed within the first, found themselves taken in this last as in a net: for the men stood locked together so closely, that they left no meshing place for the beasts to make their escape by. Then they pursued them so vigorously in this small place, that the poor animals, tired with the violence of their coursing, came and fell down at the feet of their chasers, and suffered themselves to be taken without trouble. I saw taken in this manner 2 or 300 hares in less than one day, besides a great number of wolves and foxes. I have seen more than 1000 deer so pent up by these sorts of nets, which came to cast themselves into the hands of the hunters, having found no passage to save themselves by: they killed also bears, boars, and more than 60 tigers, but these by other means, and with other weapons.

After having travelled about 400 miles in hunting daily after this manner, we arrived at last at Xyn-Yam, the capital city of the province, where we remained 4 days. During which time, the emperor with the queens went to visit the sepulchres of his ancestors, which are not far distant, from whence he sent back the queens to Xyn-Yam, and continued his own journey into the Eastern Tartary. After several days marching and hunting, he arrived at Kirin, 400 miles from Xyn-Yam: this city is built along the great river Songoro, which has its source in the mountain Cham-pe, 400 miles to the south: this mountain, so famous in the east for having been the ancient seat of the Tartars, is always covered with snow, from whence its name Cham-pe, signifying the white mountain. As soon as the emperor saw it, he alighted from his horse, and fell on his knees on the bank of the river, and bowed himself three times to the ground to salute it: after which, he caused himself to be carried on a glorious throne of gold, and so made his entry into the city: all the people ran in throngs before him, testifying by their acclamations the joy they had to see him. This prince took great pleasure in those testimonies of their affection; and that he might give them some marks of his being very sensible of it, he was pleased to suffer himself to be seen by all, and forbid his guards to hinder the people from approaching him, as they used to do at Pekin. In this city they make barks of a very particular construction: the inhabitants keep always a great number of them ready fitted to repulse the Muscovites, who come often into this river, to dispute the fishing of pearls. The emperor reposed himself two days, after which he descended on the river, accompanied with more than 100 boats, till he arrived at the city of Ula, the fairest in all this country, and which formerly was the seat of the empire of the Tartars. A little below this city, which is at most about 32 miles from Kirin, the river abounds with a certain fish, nearly resembling the plaice of Europe: and it was chiefly for taking the divertisement of fishing, that the emperor went to Ula; but the rains coming on so suddenly, swelled the river so much, that all their nets were broken and carried away.

In our return to Pekin, we suffered infinite fatigues, the roads having been spoiled by the rains, and rendered almost impassable, meeting in many places large collections of mud and water; the bridges were either overturned by the violence of the currents, or covered all over by the overflowing of the waters.

In this manner, after infinite fatigue and dangers, I arrived at Pekin in perfect health the 9th day of June very late; though many others were detained in the way by distempers, or were returned much hurt and lamed.

I shall just add, that I understood from the inhabitants of Ula, that Nincrita, a place much renowned in those parts, is distant from Ula 700 Chinese stadia, each of which is 360 geometrical paces; and that embarking at Nincrita on the great river Helum, into which the Songoro and some other more considerable rivers are discharged, and following the course of the river, which runs towards the north-east, or somewhat more to the north; they arrive in 40 days at the Eastern Sea, which is probably the Straits of Anien.

A Journey of the Emperor of China, into the Western Tartary, in the Year 1683. *By Father Verbiest, Missionary to Pekin.* N° 180, p. 52.

The emperor this year made a journey into the Western Tartary, with the queen his grandmother, called the queen mother. He set out the 16th of July, with more than 60,000 men, and 100,000 horses. He positively resolved, that I, with one of the two fathers that were at the court of Pekin, the choice of which he left to me, should follow him. I chose father Philip Grimaldi; because he is the most known, and because he perfectly understood the mathematics. Several reasons prevailed with the emperor to undertake this journey. The first was, to keep his militia, both in peace and war, in continual exercise; and for this reason it was, that after he had established a firm peace in all parts of this vast empire, he recalled his best troops from every province, and resolved in his council to make every year expeditions of this kind in several seasons, that by hunting of deer, boars, bears, and tigers, they might learn to overcome the enemies of the empire, or at least to prevent the cooling of their courage, or the degenerating from their pristine valour by the luxury of China in a long course of peace. In effect, these kinds of hunting had more of the show of a military expedition than of diversion, the men being all armed with arrows and cymeters, divided into companies, and marching in battle array after their standards, with the sound of drums and trumpets; during their hunting they entirely invested the mountains and forests, as if they had been cities which they designed to besiege; imitating in this the manner of hunting of the Eastern Tartars. This army had its van and rear-guards, its main body, its right and left wing, commanded by so many generals and petty kings. More than 70 days were spent before they set out on their march, in bringing together the ammunition for the army. For in all the Western Tartary, so called, not in respect of China, which is itself westward of it, but in respect of the Eastern Tartary, there is nothing to be found but mountains, rocks, and valleys, there being neither cities, towns, nor villages, nor even single houses. The inhabitants lodge under tents pitched in the open fields. They are mostly shepherds, and remove their tents from one valley to another, as they find pasture, living by hunting and on the produce of their cattle, and being subject as slaves to the will of their masters, and their lamas or priests, for whom they have a great veneration. This part of Tartary lies without the prodigious Chinese wall about 1000 stadia, or 300 European miles, and extends from the north-east towards the north. The emperor rides on horseback at the head of his army through these desert steep mountains, far from great roads, exposed all day to the scorchings of the sun, to the rains, and to all the injuries of the weather.

The 2d reason for undertaking this journey, was to keep the Western Tartars in their allegiance, and to prevent any pernicious designs forming against the state. It was for this reason that he entered their country with so great an army, and with such warlike preparations; and that he often caused great guns to be discharged, to strike terror into the route. Besides this great retinue, he would also be accompanied with all the marks of grandeur of the court of Pekin, viz. with a multitude of drums, trumpets, timbrels, and other musical instruments, which formed concerts during his sitting at table, when he entered the palace, or when he went out; by this outward pomp that he might astonish these barbarous people, and impress them with a fear and respect of his imperial majesty. For the empire of China never had any enemies more to be feared than these Western Tartars, who beginning on the east of China, encompass it with an almost infinity of people, and keep it as it were continually beleagured on the north and west sides; and it was to make a bulwark against their incursions that a Chinese emperor in ancient times caused this great wall to be built, which separates China from their country. I have passed it 4 times, and have considered it very attentively; and I can say, without exaggeration, that all the seven wonders of the world put together are not comparable to this work, and all that fame has spread concerning it is far short of the truth. But the monarch which in our days has re-united the Chinese and the Tartars under one and the same government, has done some things which are more for the security of China than the Chinese emperor that built the long wall. For after having reduced the Western Tartars, partly by artifice, partly by force of arms, he has obliged them to remove to 300 miles distance from the wall of China, where he distributes to them lands and pastures, whilst he has given their country to other Tartars, his subjects; notwithstanding which, these Western Tartars are so powerful, that if they should agree together, they might make themselves masters of all China and of the Eastern Tartary.

One of the first cares of this emperor was to engage to his interest, by his royal bounties, and by demonstrations of a singular affection, the lamas or priests; for these men being in great repute among their own nation, easily persuaded them to submit to the government of so great a prince; and it is in consideration of this service, that the present emperor looks upon these lamas with a favourable eye, that he bestows presents on them, and that he makes use of them to keep the Tartars in their obedience.

He has divided this vast country into 48 provinces, which have submitted and are tributary to him. From whence it is that the present emperor may justly be called the greatest and most powerful monarch of Asia. For after he had charged himself with the government, he did not at all intrust the care to any of the Colaos, or any of the great men of his court. He chastises with wonderful equity both high and low; he deprives them of their charges, and degrades them from the rank they held, proportioning always the punishment to the heinousness of the fault. He takes cognizance of the affairs transacted in the royal council, and the other tribunals, even to the causing them to render to him an exact account of the judgments there given. In short, he disposes and orders all things of himself, and it is on account of the absolute authority he has thus acquired, that the greatest lords of the court, and persons of the highest quality in the empire, even the princes of the blood, never appear in his presence but with the profoundest respect.

The 3d reason which the emperor had for making this journey, was for his health; for he knew by experience, that when he is too long at Pekin without going abroad, he cannot avoid being attacked by several distempers, which he prevents by means of these long progresses. For during the whole journey he never sees any woman; and what is more surprising, there appears not any one in all this great army, except those which are of the retinue of the queen mother; and it is only once before that she has accompanied the king, when he took with him the three queens as far as the capital city of the province of Leao-tum, to visit the sepulchres of their ancestors. Another reason for this journey, was to avoid the excessive heats of Pekin in the summer. For in this part of Tartary there reigns, during the months of July and August, so cold a wind, especially in the night, that it is necessary to put on thick clothes and furs. And this extraordinary cold may be owing to the great height and mountainous nature of the country, and to the saltpetre in which it abounds; the cold here being so intense, as to penetrate 3 or 4 feet depth in the earth.

As to what relates to the particularities of our journey, they are similar to those which happened to us the last year, in the journey to the Eastern Tartary, described in my last letter, our treatment being just the same. For more than 600 miles, which we had travelled in going and returning, for we did not return by the same road, the emperor caused to be made a great highway, across the mountains and valleys for the queen mother, who rode in a chariot; he caused also an infinite number of bridges to be built over torrents, and rocks to be cut through, with incredible labour and expense.

Some Observations and Conjectures concerning the Chinese Characters. Made by R. H. R. S. S.* N° 180, p. 63.

Whether there ever was any natural language, I do not dispute; but that there have been, still are, and may be more artificial languages invented, is not difficult to prove. The Chinese court language is said to be of this kind, invented and spoken by the literati and Mandarins throughout the whole empire of China, and differing from all the other languages spoken in it. This I conjecture to be no other than the names of the characters by which they write and express their meaning, arbitrarily imposed by them, as we in Europe give names to arithmetical figures, and not as we pronounce words written with a literal character. This I judge, by comparing the characters with the names, monosyllables, or words, they pronounce and read them by. Nor do they ascend above a monosyllabical name, though the character be composed of many single characters, each of which has its proper sense and monosyllabical name, and though

* Probably Dr. Robert Hook.

the meaning of each character be an ingredient in the notion of that compounded character.

I might give an instance also in the artificial language invented by the late Rev. Bp. of Chester, Dr. Wilkins, which in all the accomplishments of language excels every one yet extant; to which is also annexed a real character, legible into that or any other language spoken. By which language the character and every additional mark are effable, and yet the character is not literal but real, which is more curious and useful than the Chinese way.

But whatever we may judge of language, it is past dispute that writing was ever artificial, how anciently soever it were used, and was the invention of some thinking and studious men. It is also evident that there have been various ways thought of for expressing significancy, according to the several geniuses of the inventors; as may be guessed from the Egyptian hieroglyphics, the Chinese characters, the Mexican chronology, and the literal characters of several nations; all of which seem to proceed by different methods, and from different turns of invention.

Which of these methods is the most ancient, may be hard to prove. The Egyptian mummies and obelisks prove a great antiquity of the hieroglyphics; yet the Chinese chronology, if it may be credited, exceeds the Egyptian in that respect. For the Chinese make Fohi, the first king of China, to be the inventor of their character, and account him to have lived 2950 years before Christ; during all which time they pretend to have a written and authentic history; but their account of the times preceding that they esteem more fabulous, as chiefly depending on fiction and oral tradition; as may easily be seen from the number of years they reckon since the creation to the present year 1686, viz. no less than 88,640,102 solar years, there having elapsed since the creation 8864 ven of years, every ven containing 10,000 such years, and of the present ven this year 1686 is the 102d. Which account is abundantly more extravagant than the Egyptian; but this need not invalidate their history since Fohi; by which it appears that their character was invented before Moses, about 1400 years, and even before Menes, the first king of Egypt, about 500 years. So that the Chinese character seems to be the most ancient of the kind; and the book Yekim, said to be written by Fohi, the oldest book.

For Paulus Jovius affirms, that the first occasion of the art of printing in Germany, was owing to a German merchant, who, returning from China to his own country, related what he had observed concerning the practice of it as used in that country. And though the Chinese way be wholly different, as to the method of composing from what was invented and perfected here in Europe, yet such an intimation was sufficient to an ingenious artist to improve the first contrivance, and accommodate it more to the literal way of writing with us: and as our way is now possibly brought to the greatest perfection for exactness and expedition, so doubtless must their way of printing, since they can engrave their stamps for a sheet as soon as one of our compositors can set and correct a sheet of our literal character; and since one man alone will print off 1500 sheets in one day. And though it is generally believed to be much the same with our wooden cuts for printing, yet from some observations, hereafter to be noticed, it seems to be quite different.

By a Chinese manuscript, having found that the pronunciation had no affinity with the strokes of the character, I conceived it was either a numeral character consisting of numbers, or else a real character, but not a literal, unless it were a literal character of some other language than that by which it was pronounced, whose pronunciation is lost, though the significancy be retained. Since that time I procured from China a dictionary of the court language; but this whole book consisted only of the Chinese characters, without any interpretation or pronunciation; however, by the help of the pictures of that and a Chinese almanack, I soon found out their characters for numbers, and their way of numeration, with the figure and use of their abacus or counting board, for performing the operations of arithmetic; which I find pretty nearly to agree with that of the ancient Romans, save only, that instead of pins and sliding grooves of the Romans, the Chinese abacus has strings or wires, and beads to slide upon them; and that instead of 4 pins for digits or units, the Chinese has 5 beads; so that it seems the Chinese abacus was designed for a duodecimal progression, whereas that of the Romans was designed for the decimal. One thing is remarkable in the Chinese, that the places in the abacus lie horizontal, and the first place is next the left hand, which probably was also the first in their old way of reading. Now as the Chinese and Roman abacus do much agree, save only that they proceed contrary ways, so also does their way of expressing numbers by letters or marks, one stroke or line signifying 1; two lines, 2; three lines, 3; a cross, 10; two crosses, 20; three crosses, 30; and so on to 100, which they express by a square mark; and a cross with a stroke added for 1000. And though the characters are not all the same, yet the order and method of one agrees very nearly with that of the other; especially if it be allowed, that the primitive way of writing and reading with the Chinese was horizontal, like the European way.

Having thus discovered their characters for numbers, and their way of numeration, I next applied myself to understand something of their language and character. But upon perusing all the accounts I could meet with in books, I found very little satisfaction as to my principal inquiry, which was, first concerning the method of their character, whether it consisted of a certain number of marks methodically disposed like letters in a literal, or like numbers in a numeral, or like radicals in composite and decomposite derivations. This character is said to be legible into a great many languages, considerably different from each other; but how this is effected, is not related, only it is said that the marks are of the nature of our arithmetical figures, which are become almost universal, at least here in Europe. Secondly, as to the number of these characters, I found as little satisfaction; for, some accounts making them 120000, others 80000, and others 60000; and that a man must be able to remember, and to write, and read at lest 8000, or 10000, before he can express his meaning by them, and that it is the business of a man's whole life to be thoroughly intelligent in the whole character; which seems to intimate that the characters are immethodical, and there are as many primitive characters as words. Others tell us of various kinds of characters, which have been in use in several ages: the first, they say, were hieroglyphical, like the Egyptian or Mexican, consisting of pictures of animals and vegetables. But the last are made up of lines and points, there being no such thing as letters or syllables, but every distinct word and notion has a distinct character, and they are all primitive or in composite. Which accounts seem to insinuate, that this character is the most difficult, and the most perplexed piece of learning in the world, and depends wholly on the strength of the memory in retaining their form and signification. But from my own observations, there seems reason to doubt of the accuracy of these accounts. For, in the first place I observed, that every one of their characters, whether consisting of more or fewer strokes, was comprised within a certain square space, which is proportioned according to the size or manner of writing they design to make use of; not that the whole square is filled with every character, but that no part of that character exceeds the limits of that square; so that though the character have but one stroke, it takes as much room in the line as another that has 20 or 30 several marks; and thus their characters are exactly ranged in rank and file, not unlike our numbers in arithmetic.

Yet they vary the size of the character on several occasions; as in the titles of books, chapters, or sections, &c. I have also met with three several kinds of characters: the most usual is the fixed, or set square form; the second sort is the running hand, in which the orders of the courts are written, of which I have seen 3 or 4 kinds, in which the pencil is never taken off till the whole character be finished; and sometimes 2 or 3 are all written without break: the third seems to be somewhat like the flourishing great letters used by scriveners at the beginning of deeds, and by the Germans at the beginning of chapters and sections. They are compounded of the same strokes as the set character, but modulated and shaped a little different, to make them appear the more beautiful and regular. This third is used for epitaphs, and other inscriptions on buildings or monuments. These 3 sorts may be called the three general kinds of writing; but there is to be found an almost infinite variety of forms, as will appear by considering that the printed characters are exactly the same with the written; insomuch that every variety in each stroke, line, or point, made with the pencil, is perfectly expressed in the impression; and the form, mode, or hand of every writer, is exhibited so curiously, that it seems hardly possible to be performed after the way of wooden cuts, as authors affirm it is, but it must be done after the method of our copper cuts, printed by a rolling-press. Their paper is generally very thin and fine, and very transparent, but brown; so that whatever is written or printed on it, is almost as legible on the back as on the foreside; which is of great use in the cutting of their stamps. And thence they never write or print on both sides of the same leaf, but only on one side; and to make the leaf appear printed on both sides, they double the sheet, with the printed sides outwards, and putting the folded part forward, they sew, bind or stitch together, all these sheets by the cut edges, and upon whole sheets, instead of single leaves. They begin the book on the top of the right hand side of the page, and they read downwards to the bottom; then they begin the next line towards the left hand at the top, and so read to the bottom; and thus they proceed to the end of the book. The title of the book is set first, on a whole leaf, usually of a thicker paper, and some title is likewise written on the folding or edge of every sheet, where is set also the number of the book, and that of the sheet, half of which appears on one side, and half on the other side of the fold.

As to the character itself, I find that each one is made up of a certain number of strokes, lines, or marks, which are very distinct from each other, in their shape and position; and because these are single strokes, they may be called the letters, elements, or particles, out of which the more compounded characters are constructed or made up. These are the first kind, and of which there are but a very few. Two, three, four, or more of these joined together in a certain order and contexture (in the doing of which there is a great regularity and order observed, and all within the regular square space) seem to make syllables or primitive radical characters, each of which has a primitive, single, or distinct notion or signification, as well as sound, which is made much use of in the more compounded characters or words. Of this kind the figures of the numbers seem to be. Of this kind I understand there are about 500. The third sort of characters is a decompounded kind, made up of two, three, or more of those of the second sort, diminished proportionably in their size, either as to their length or breadth, or both, from what they have in the same writing, when they are single, and fill up the whole letter square or word's square. This method alone of crowding together all the characters of a decompounded one into one square, which is of the same size both for the most simple and the most compound, seems to be the great singularity, by which the Chinese characters differ from those of all the rest of the world. And this probably is the reason why they are taken for a real, and not a literal character: for if the primitive language, or pronunciation of the characters, be lost, or not understood, then the whole characters become a real, and not a literal character. But I conceive it might be at first either a literal character, and so the whole square character be composed of so many distinct letters or syllables, which composed the word intended; and so there might be a regular order of placing these letters in the character; that is, that the whole square being divided into so many parts, there was a rule for finding which was the 1st, 2d, 3d, and 4th place; so that the several letters, that made up the word, being placed in those, according to the order they had in the word, it was easy, by that rule, to decipher the said character, and thence to find the word and its signification, as regularly as if the letters had been written one after another, like all other literal characters. Or secondly, it might be a real character, consisting of several marks or letters, that expressed so many simple notions, several of which, joined together, might make up the more compounded characters.

The present use of this character seems to differ from what it was at first, both as to the position of writing and reading it, and as to its expression and pronunciation. For the way of writing and reading it might at first be exactly the same with the European; that is, they might begin at the top of the page towards the left hand, and so proceed towards the right in the horizontal line to the end of it; then beginning at the left end of the next line under the first, and proceeding with that in the same manner; and so with the next under that, and all the remaining, till the whole discourse was completed, joining leaf to leaf, one under another, after the same manner as the rolls are at present written, and as the Volumina of the ancients were. And that the parts of the volume might be the more easily come at, without the trouble of rolling and unrolling, they contrived to fold them like a fan, forwards and backwards, stitching them together so as that the written sides might lie outwards, and open freely from each other, and the fair sides meeting together; it came to make the present form of their books, which being laid, as we generally place our books before us, they seem to begin at the top of the page on the right-hand, and to proceed to the bottom, and then at the top of the next line towards the left hand, and descend as in the former; proceeding in this order with all the rest.

Secondly, as to the pronunciation of this character, by the court language, or by any other now used, I conceive it to be wholly different from that of a literal character, that is, from being pronounced or spoken according to its marks or figures, whether simple or compounded. The reason of which different pronunciation may have proceeded partly from the loss of the primitive language, for which it was designed, partly from an affectation of monosyllabical words in this court language; to help the poverty of which, they are obliged to make one syllable to signify many different notions, to do which, they have introduced a kind of musical toning or accenting of each of them, and that not single, but compound of two or three tones to each signification of every one of these monosyllables; arising partly from this way of writing by divers nations of different languages; who minding only the figure and signification, read it into their own mother tongues, as we in Europe arithmetical figures; and partly also from the omission of most grammatical distinctions, the same character serving for substantive and adjective, singular and plural in all cases, for the verb in all tenses and numbers, &c. for the abstract and the concrete signification: partly also from their syntax, it being necessary to consider the whole sentence, to discover which part of speech each character is of in that sentence; wherein the order and positions of the characters to each other, for which they have rules, have their signification: and lastly, from the loss of the very notion of a literal character; whence for the expressing of proper names, they are obliged to make use of several characters, whose sounds or words come nearest to the sounds of the syllables of that name, as tam, jo, van, for Adam Jovan.

Now, though I conceive this character is not effable, properly as a literal character, by any of their present languages; and though possibly it might be at first a real character, that is, each of them compounded of such strokes or marks, as by their figures, positions, and numbers in the square, denoted the several philosophical ingredients, that made up the notion of the whole character, as the book Ye Kim seems to show, by giving rules for the order and significancy of places in the square, &c. Yet I think it not difficult to make it a literal, or at least a syllabical character, and legible into a language, somewhat after the manner of the universal character invented by Bishop Wilkins; in which the single characters might be monosyllables, and the compounded dissyllables, trissyllables, &c. according to the number and order of simple characters in the square of the compounded.

Account of an Engine that consumes Smoke. By. Mr. Justel, R. S. S.
N° 181, p. 78.

M. Dalesme, engineer, has found out a machine, which, though very small and portable, consumes all the smoke of all sorts of wood whatever; and that in such a manner, that the most curious eye cannot discover it in the room, nor the nicest nose smell it, although the fire be perfectly open. This engine is made after the manner represented in fig. 6, pl. 9, and is composed of several hoops of hammered iron, of about 4 or 5 inches diameter, which shut into each other. It stands upright in the middle of the room, on a sort of trevet made on purpose. A is the place where the fire is made, where if you put little pieces of wood, it will not make the least smoke, neither at A nor B, over which you cannot hold your hand within half a foot, because of the great heat. If you take one of these pieces of wood out of the fire, at A, it smokes presently, but ceases immediately, as soon as it is cast in the fire again. The most fœtid things in this machine make not the least ill scent; on the other side, all perfumes are lost in it, and incense makes no smell at all, when burned in it; which only happens when the fire at A is well kindled, and the tunnel B D very hot; so that the air that feeds the fire cannot come that way, but must all press in upon the open fire; by which the smoke and flame are all forced inwards, and must pass through the heap of burning coals in the furnace A; in which passage its parts are so dispersed and refined, that they become inoffensive, both to the eye and nose.

The two New Interior Satellites of Saturn discovered. By M. Cassini, at the Royal Observatory at Paris. N° 181, p. 79.

The first or innermost satellite of Saturn, by the observations hitherto made, is never distant from his ring above two thirds of the apparent length of the same ring, which is taken for the measure of the distances of these satellites: and it makes one revolution about him in one day, 21 hours and 19 minutes. Therefore, in less than 2 days it makes two conjunctions with Saturn, the one in the upper part of his orbit, and the other in the lower; and the ring taking up the greatest part of the diameter of the orbit, in which this satellite makes its revolution, these conjunctions are of a long continuance, in respect of the whole revolution, it being 8 hours and a half, and sometimes more in passing the length of the ring. This happens particularly, when the position of the ring, in respect of the earth, being very oblique, it appears very narrow; and the orbit of this satellite being nearly in the same plane with it, they appear very close together. But when the ring and the orbits of these satellites are more open, there is a greater distance in latitude between this satellite and the ring, and it may be seen both above and below the ansæ.

The second satellite of Saturn, according to the observations hitherto made, is but three quarters of the length of his ring distant from it, and makes its revolution about him in 2 days 17 hours and 43 minutes. After a great number of choice observations, it was concluded that the proportion of the digression of the second, to that of the first, counting both from the centre of Saturn, is as 22 to 17. And the time of the revolution of the second satellite to that of the first, as 24$\frac{2}{7}$ to 17; which is that very same proportion which Kepler observes between the distances and periods of the primary planets, and which we have found to hold among the other satellites of Saturn, and is also verified in the satellites of Jupiter: which clearly shows the admirable harmony of the particular systems, with the great system of the world.

Of all the satellites, there are none so near their primary planet, as these two satellites of Saturn, and which, taken both together, make so great a number of conjunctions with their primary in the same space of time; for there are no less than 653 in a year; whereas the first two satellites of Jupiter make together, only 617.

These two satellites were first of all seen March 1684, by means of two excellent object glasses, of 100 and 136 feet, and afterwards by two others, of 90 and 70 feet, all made by Signior Campani, after the discovery of the third and fifth satellites, which had been made by others of his glasses, of 47 and 34 feet. We made use of them without tubes, by a more simple contrivance than those proposed either before or since. We have since seen all these satellites with that of 34 feet, and continued to observe them with glasses of M. Borelli of 40 and 70 feet, and by those which Mr. Artouquel has lately made, of 80, 155 and 220 feet. It was easy to see these two satellites by these different sorts of glasses, after having found out the rules of their motion, by which the eye might with more particular attention be directed to the places where they ought to appear.

The Radices or Epochæ of their Motions, were these: viz.—The first satellite was observed 45 degrees distant from its perigee, moving towards the west, March 11, 1686, N.S. at 10h. 40min. at night; and returned to the same position on the 14th of April at the same hour. The second was 36 degrees distant from the perigee to the west, the 30th of March 1686, N.S. at 8 o'clock in the evening.

By a Comparison of the Revolutions of Saturn's Satellites with those of Jupiter.—It appears that the former, in the same order, perform their revolutions in less time than those of Jupiter answering to them; except the first, as may be seen in the following table.

Periods of the Satellites' Revolutions.

	d.	h.	m.		d.	h.	m.
The first of Jupiter in . .	1	18	29	The third of Jupiter in . .	7	4	0
The first of Saturn in . .	1	21	19	The fourth of Saturn in	15	23	15
The second of Saturn in	2	17	43	The fourth of Jupiter in	16	18	5
The second of Jupiter in	3	13	19	The fifth of Saturn in . .	79	21	0
The third of Saturn in . .	4	12	27				

By these discoveries, the admirable analogy and uniformity of the parts of the universe are most evident, and the infinite wisdom and power of the Creator is demonstrative to the contemplative mind. The discoverer considering that the ancient astronomers had given the names of their heroes to the stars; and that Galileo, after their example, had honoured the house of the Medici with the discovery of the satellites of Jupiter, which he made under the protection of Cosmus II, calling them Sidera Medicea; so in like manner M. Cassini concludes that the satellites of Saturn are not unworthy to bear the name of Louis le Grand, under whose reign and in whose observatory the same have been detected, which therefore he calls Sidera Lodoicea.

Two Observations of the Eclipses of the Planet Jupiter by the Moon. By Mr. Hook, Mr. Halley, and Mr. Haines. N° 181, p. 85.

The first was observed at Gresham College, London, by MM. Hook and Halley, viz. on March 31, 1686, at night. At 9h. 33m. as near as could be guessed, was the time of the central immersion, which was very difficult to be observed, by reason of the asperity of the moon's limb, which undulated and sparkled very much, as it appeared through the vapours near the horizon, so that the contact of the limbs could with no certainty be determined: the ingress happened much about the length of the spot, called by Hevelius Palus Mareotis, to the north of the said spot, or about the 124th degree of the outer limb of his Selenography, nearly in the same latitude with the moon's centre.—At 10h. 30m. the western edge of Jupiter began to emerge out of the dark limb of the moon.—10h. 31m. 20s. the whole disk of Jupiter was entire; so that he was about a minute and a third in coming out from behind the moon; whence the diameter of this planet may be determined.—The emersion was exactly in a right line with the moon's centre, and the northern part of Palus Mæotis, or about the 324th degree of the inner limb of the Selenographic Table of Hevelius.

The other occultation happened May the 28th in the morning, or astronomically, the 27th after midnight.

The immersion was seen at Totteridge (which place is about 9 miles from London, and nearly 25s. of time to the westward of it) by Mr. Edw. Haines, a member of the Royal Society: who through a gap of the clouds observed the contact of the moon's limb and Jupiter's, at 15h. 3½m. the clouds closing again permitted him to observe no more; however from this we may conclude the central immersion at London to have been 15h. 4½m.—The emersion was observed at London by E. Halley, at 15h. 49m.; for at 15h. 50m. Jupiter was all out, and the limbs so little separated, that he judged that a minute before, the centre of Jupiter had been on the moon's edge. The point of the emersion was opposite to the southern part of the spot called, by Hevelius, Insula Macra, or at the 342d division of the inner limb of his map of the moon.

On the Cause why Bodies dissolved in Menstrua, specifically lighter than themselves, swim in them. By Mr. Wm. Molyneux, of Dublin, F.R.S.* N° 181, p. 88.

Why bodies, dissolved, float in liquors lighter than themselves; as for instance, why mercury, dissolved in strong spirit of nitre, swims in it, though each small particle of mercury be far heavier than so much of the liquor whose place it occupies, is a problem, which Mr. Molyneux thinks cannot be solved by the prime law of hydrostatics, which is, that a body, specifically heavier than a like quantity of liquor, sinks in that liquor; thus a cubic inch of iron, being heavier than a cubic inch of aqua-fortis, and being put into it, should sink; and yet we find that iron, being dissolved in a convenient quantity of aqua-fortis, floats in it, and does not fall to the bottom. The reason which my brother gives for this is, that the internal motion of the parts of the liquor

* Mr. Wm. Molyneux was a good mathematician and philosopher, and a useful member of the Royal Society. Besides a number of papers in the volumes of the Philos. Trans. he was also author of some ingenious books; as his Sciothericum Telescoplum, in 1656; also a Treatise on Optics, in 1692, in 4to; and The Case of Ireland Stated, in relation to its being bound by acts of parliament made in England. Mr. M. being born at Dublin 1656, studied at that university; after which he went to London, and was entered of the Middle Temple. Afterwards returning to Dublin, by his endeavours the Philosophical Society was established there in 1683, of which he became the first secretary. The year following he was appointed chief engineer, and surveyor general of his majesty's works. In 1685 he was elected F. R. S. And in 1689 he left Ireland, and settled with his family at Chester. But in 1692 returned again to his native country, when he was chosen representative in parliament for the city of Dublin, and in 1695 for the university there. He died in 1698, by a fit of the stone, at only 42 years of age.

His son, Samuel Molyneux, born at Chester 1689, like the father, was a good scholar, and particularly skilled in optics and astronomy. He applied himself much in improving the telescope; and by means of his apparatus, and with his assistance, it was that Dr. Bradley conducted the astronomical observations on the fixed stars, which led to the celebrated discovery of the aberration of the rays of light. Mr. S. M. bequeathed his scientific papers to Dr. Smith, professor of astronomy at Cambridge, who published them in his noted treatise on optics.

sustains the particles of the dissolved solid; for being so very minute, they are moveable by the least force imaginable, and the action of the particles of the menstruum is sufficient to drive the atoms of the dissolved solid body from one place to another; and consequently, notwithstanding their gravity, they do not sink in the liquor though lighter than themselves. As a proof of this, he offers a known experiment in chemistry, viz. that a menstruum over a digesting fire, will dissolve a greater quantity of a body, than when it is off the fire; and if suffered to cool, a great proportion of that which was dissolved will precipitate. For, says he, the particles of the menstruum acquire a more violent agitation by the fire, and are thereby able to sustain a greater quantity of the dissolved body, or to resist a greater gravity. Against this notion it has been objected, that the common experiment of precipitation, by mixing an alcali with an acid, seems to contradict this; for thereby the fluidity of the menstruum is not destroyed, and consequently the internal agitation of its parts is not diminished, and yet the particles of the dissolved body precipitate all to the bottom. To this he answers, that all mixtures of different liquors introduce in each a different conformation of pores, and therefore the infusion of a new liquor drives the insensible parts of the dissolved body from their places, and forces them to strike against each other, and cling together, and so becoming larger and heavier than formerly, the internal agitation of the liquor is no longer able to move and sustain them, and consequently they fall to the bottom.

But I conceive another account may be given of this appearance, and that the aforesaid law of hydrostatics is a little defective. It is true indeed, if we consider only the specific gravity of a liquor, and the specific gravity of a solid particle floating in it, the forementioned rule is exact; but in sinking there is requisite a separation of the parts of the liquor by the sinking body; and there being a natural inclination in the parts of all liquors to union, arising from an agreement or congruity of their parts, they resist any thing that endeavours to separate them. Now unless a body have weight sufficient to overcome this congruity or union of parts, such a body will float in a liquor specifically lighter than itself. But that a heavy body, as mercury or iron, may have its parts reduced to that minuteness, as that their gravity or tendency downwards is not strong enough to separate the cohesion or union of the parts of a liquor, will be manifest by considering, that the resistance made by the medium to a falling body, is in proportion to the surface of the body; but as the body decreases in bulk, its surface does not proportionably decrease; thus a sphere of an inch diameter has not 8 times less surface than a sphere of 2 inches diameter, though it has 8 times less bulk; and consequently passing through a medium, as suppose air or water, the sphere of 1 inch diameter is proportionably to its bulk more resisted than a sphere of 2 inches diameter in proportion to its bulk; and hence it will happen, that at last a body may be reduced to that minuteness, that its gravity pressing downwards, which is in proportion to its bulk, may be less than the resistance of the medium which operates on the surface of the body; seeing, as was said, the surfaces of bodies do not decrease so fast as their bulks, these decreasing in a triplicate, but those in a duplicate ratio of the diameters of the bodies.

But because it was said that the forementioned law of hydrostatics is a little defective, I desire to explain myself a little further on that point. In weights falling through the air, were gravity only considered, the proportions of their descents would be exactly as Galileo has demonstrated; but it is allowed by all, that the resistance of the air not being considered in those demonstrations, they are not mathematically true in practice, but that there is really some deviation from that proportion on account of the air's resistance. Now, what is this less than to say, that the resistance of the air takes off some of the action of gravity, by its opposition. And if so, what shall we say were an iron sphere let through a medium of water? surely the proportions of its descents would be much more disturbed in it, as water is much more solid and difficult to be separated or passed through than air, and consequently more of the operation of gravity is taken off or resisted by this opposition of the water than that of the air. And if so, surely there may be a certain degree of gravity, that may be quite taken off by the resistance of the water; were a pistol bullet let fall through the air it would descend nearly according to the proportions assigned by Galileo; but were a single grain of sand so let fall, it would be much hindered in its course, and the half of this grain would be still more obstructed; what shall we then say of the 10,000th part, or of a part the 10,000,000,000th of this, and again of the infinite subdivisions of that, till at last we come to a part that would be wholly resisted or sustained; such as the minute particles of a body dissolved in a menstruum are?

On this account it is, that the above-mentioned principles of hydrostatics is a little defective; for it does not consider the natural congruity of the parts of a liquor which they unite and keep together; and therefore liquors have an innate power of resisting a certain degree of force that would separate them; such as I suppose the degree of gravity in the most minute particles of a body dissolved in a menstruum.

But I would not be thought wholly to reject my brother's solution of this problem, for that motion in a menstruum which is able to dissolve such a solid body as iron, that is, which can disturb the close and strong cohesion of the parts of iron, may very well be supposed sufficient to sustain those parts in the vessel, wherein the solution was made; and certainly no better account can possibly be given of such solution than by supposing such an internal motion in the parts of the menstruum insinuating themselves into the solid body, and loosening its parts.

Some Reflections on the foregoing Paper by Mr. T. M.—What my brother has laid down in this discourse I think undeniably evinces that the received law of hydrostatics is somewhat defective. For liquors, though they are fluid, yet they are bodies, and therefore consist of parts united; which union, though it be easily destroyed, yet it requires some degree of force for the effecting it; however, I imagine this property ought not to be considered as the sole cause of this appearance; nay perhaps does not so much as contribute the least in producing this effect; for whatever is of sufficient power to raise the minute particles of a heavy body in a light fluid, is certainly sufficient to keep them in that state; now my supposition may give some account of this; what my brother says, never can: for he must necessarily suppose them first raised; and then he gives the reason of their not sinking; whereas it is not to be questioned but that that force which raised them is the same that keeps them from falling to the bottom.

A Letter from Dr. Sigismond Konig, dated Bern, the last day of Feb. 1686, *to the Royal Society, being a Continuation of the History of his Patient, Margaret Lower; an Account whereof is given in the third Philosophical Collection,* of Dec.* 10, 1681. *Translated and abridged from the Latin.* N° 181, p. 94.

From the time, viz. the autumn of 1681, to which the history of the case was brought down in the former communication, the patient continued tolerably well until the 18th of August, 1682, when she began to be afflicted with nausea and hickup, but without vomiting. These symptoms (after some alleviation procured by a cordial medicine with dulcified spirit of nitre) were followed on the 29th of the last-mentioned month by violent pains in the stomach and abdomen, difficult respiration, hysterical paroxysms, palpitation of the heart, &c.; to which succeeded convulsions, loss of speech, and pains like those of a person in labour (accompanied with contractions of the limbs) in which the patient voided by stool on the following day (the 30th) a stone (represented in fig. 11, pl. 9,) covered with blood. She afterwards voided two more of a smaller size, accompanied with a hæmorrhoidal flux, in consequence of a laceration of the vessels. In a few weeks she got tolerably well again.

Since that time she has voided about every 3 or 4 weeks calculi (of a harder texture than before and of an angular figure) by stool, but none by vomiting. Previous to their expulsion, the bowels, at other times open, become constipated, and a day or two after this constipation takes place a stone is voided. The urine is scanty, not in proportion to the quantity of drink, and generally

* Vol. ii. p. 510, of this Abridgment.

of a thick consistence and of a turbid appearance; it is rarely suppressed, but whenever this happens, an angular calculus as large as a bean is voided per urethram; and what before happened only occasionally, now takes place every day, viz. in the morning, dum urinandi motus urget, part of the urine first comes away per urethram, and almost immediately afterwards another quantity (to the amount of 3 or 4 oz.) is discharged by vomiting, of the same colour, consistence, smell, and (according to the patient's account) of the same taste (a taste creating much nausea) with that discharged from the bladder. The abdomen continues tumid; and there is not only in the left hypochondrium (as before) a hardness, accompanied with a sound as of stones rubbing one against another, but the same sound is perceived in the whole right hypochondrium also; and the patient moreover complains of great pain about the hypogastrium. She has a tolerable appetite; her usual drink consists of a decoction of the radix glycyrrhizæ et graminis with barley, to which is occasionally added a small quantity of some light wine. Last year, on the 12th of Dec. a gangrene, as large as the palm of the hand, showed itself on the right leg. This gave way to the remedies employed against it. She has since been affected with angina notha, accompanied with a hemorrhage from the fauces; for the removal of which bleeding in the foot and clysters were resorted to. On the 20th of Feb. she had natural evacuations downwards; but on the 23d she had not only evacuations by stool, but also brought away part of the oily clyster by vomiting, but without any calculi. [Here the author terminates the history of this case for the present.]

In the remaining part of this communication, after mentioning various instances of the formation of calculi in other parts of the body, besides the kidneys and bladder, the author details the result of his chemical examination fo these stones; and concludes with reflections on the manner of their production, and with a rationale of the concomitant symptoms.

With regard to his chemical examination,* Dr. Konig states that, 1. In trying to dissolve them, he found that on pouring upon them sp. sulphuris, vitrioli, acetique, some effervescence was produced, especially in those which had been discharged from the stomach, and which were of a looser and friable texture; but that this soon ceased, and that no solution could be effected. Sp. salis ammoniaci had no action whatever upon them; but they were completely dissolved by the sp. nitri acidus.

2. By distillation there was obtained from those calculi which had been voided by the œsophagus a small quantity of volatile salt, volatile spirit and water

* A further examination of these calculi by Dr. Slare is inserted in the following N° of these Transactions.

(phlegma) a good deal of earth, but scarcely any fixed salt; from those which were voided from the intestines and bladder, a larger quantity of volatile salt with a little sub-acid phlegm and a strong urinous spirit, some fixed salt, and a large proportion of earth; 6 oz. of these calculi yielded 5 oz. 2 dr. of caput mortuum, about 1½ scr. salis lixiviosi, and of water (phlegma) impregnated with volatile spirit and volatile salt 5½ dr. besides a small portion adhering to the sides of the receiver. This liquor being collected and mixed together with an addition of sp. vini alcoholiast. and being afterwards subjected to distillation in an alembic, there sublimed from it in the head of the alembic 2½ scr. of volatile urinous salt.

3. On the addition of spirit of vitriol to the distilled liquor, it became of a red colour, and at length turbid, with some deposition. When the spirit of vitriol was poured upon the caput mortuum which remained after the distillation, or upon the salt lixiviated from it, a violent effervescence was produced, like that which takes place when the same spirit is mixed with salt or oil of tartar.

Hence Dr. Konig concludes that these calculi consist of a large proportion of earth, a small quantity of volatile salt, and a little acid, obtunded and edulcorated by an urinous salt and spirit.*

On the height of the Mercury in the Barometer at different Elevations above the Surface of the Earth; and on the Rising and Falling of the Mercury on the Change of Weather. By Mr. Edm. Halley. N° 181, p. 104.

The elastic property of the air has been long since made out by experiments before the Royal Society, and elsewhere; and the resistance of its spring is found to be nearly equal to the weight or force that compresses it, as also, that the spaces the same air occupies under differing pressures, are reciprocally as those pressures; it has been likewise shown by undoubted experiment, that the specific gravity of the air, near the earth's surface, to that of water was once as 1 to 840, again as 1 to 852, and a third time, in a very large vessel holding 10 gallons, as 1 to 860; all which, considering the difficulty of the experiment, agree well enough, the mercury standing at all those times about 29¾ inches;

* By comparing these experiments, as well as those made by Dr. Slare, and inserted in the following N° of these Transactions, with the recent analyses of Dr. Hyde Wollaston, (Philosophical Transactions for 1797) and other modern experimenters, it will be seen how little the chemists of the 17th century were acquainted with the real nature of these animal concretions; and to what great perfection have at length been brought the methods of separating and determining their constituent parts. Nevertheless these experiments cannot but be interesting to those who trace the origin and progress of discoveries; inasmuch as they demonstrate the compound nature of human calculi, their insolubility in the vitriolic acid, their solubility in the nitric acid (and according to Dr. Slare in the muriatic acid also) their resolution into fixed and volatile parts, and the extraction of an acid as well as an ammoniacal and urinous product from them, by distillation.

but because it was summer weather, and consequently the air rarefied, when these experiments were made, we may, without sensible error, say in round numbers, that the barometer standing at 30 inches, and in a mean state of heat and cold, the specific gravity of the air to water is as 1 to 800. By the like trials, the weight of mercury to water is as 13½ to 1, or very near it; so that the weight of mercury to air is as 10,800 to 1, and a cylinder of air of 10,800 inches or 900 feet is equal to an inch of mercury; and were the air of an equal density like water, the whole atmosphere would be no more than 5,1 miles high; and in the ascent of every 900 feet the barometer would sink an inch. But the expansion of the air increasing in the same proportion as the incumbent weight of the atmosphere decreases, that is, as the mercury in the barometer sinks, the upper parts of the air are much more rarefied than the lower, and each space, answering to an inch of quicksilver, grows still larger; so that the atmosphere must be extended to a much greater height. Now on these principles, to determine the height of the mercury at any assigned height in the air, and e contra, having the height of the mercury given to find the height of the place where the barometer stands, are problems not more difficult than curious, and which I thus resolve.

The expansions of the air being reciprocally as the heights of the mercury, it is evident, that by means of the curve of the hyperbola, and its asymptotes, the said expansions may be expounded to any given height of the mercury; for by the 65th prop. lib. 2, Conic. Mydorgii, the rectangles A B C E, A K G E, A L D E, &c. (in fig. 7, pl. 9) are always equal, and consequently the sides C B, G K, L D, &c. are reciprocally as the sides, A B, A K, A L, &c. If then the lines A B, A K, A L, be supposed equal to the heights of the mercury, or the pressures of the atmosphere, the lines C B, K G, L D, answering thereto, will be as the expansions of the air under those pressures, or the spaces that the same quantity of air will occupy; which expansions being taken infinitely many and infinitely small, according to the method of indivisibles, their sum will give the spaces of air between the several heights of the barometer; that is the sum of all the lines between C B and K G, or the area C B K G, will be proportional to the distance or space intercepted between the levels of two places in the air, where the mercury would stand at the heights represented by the lines A B, A K; so then the spaces of air answering to equal parts of mercury in the barometer are as the areas C B K G, G K L D, D L F M, &c. These areas again are, by the demonstration of Gregory of St. Vincent, proportional to the logarithms of the numbers expressing the ratios of A K to A B, of A L to A K, of A M to A L, &c. So then by the common tables of logarithms, the height of any place in the atmosphere, having any assigned height of the mercury, may most easily be found; for the line C B in the hyperbola, whose areas denote the tabular logarithms, being 0,0144765, it will be as 0,0144765 to the difference of the logarithms of 30 and any other lesser number, so 900 feet, or the space answering to an inch of mercury, if the air were equally pressed with 30 inches of mercury, and every where alike, to the height of the barometer in the air, where it will stand at that lesser number of inches: and by the converse of this proportion may the height of the mercury be found, having the altitude of the place given. From these rules I derived the following tables.

A table showing the altitude to given heights of the mercury.

Inch.		Feet.
30	....	0
29	....	915
28	....	1862
27	....	2844
26	....	3863
25	....	4922
20	....	10947
15	....	18715
10	....	29662
5	....	48378
1	....	91831
0,5	....	110547
0,25	....	129262
0,1	...... 29 mil. or	154000
0,01	...... 41 mil.	216169
0,001	.... 53 mil.	278338

A table showing the heights of the mercury at given altitudes.

Feet.		Inch.
0	....	30.00
1000	....	28.91
2000	....	27.86
3000	....	26.85
4000	....	25.87
5000	....	24.93
1 mile	....	24.67
2	....	20.29
3	....	16.68
4	....	13.72
5	....	11.28
10	....	4.24
15	....	1.60
20	....	0.95
25	....	0.23
30	....	0.08
40	....	0.012

On these suppositions it appears, that at the height of 41 miles, the air is so rarefied as to take up 3000 times the space it occupies here at the earth's surface, and at 53 miles high, it would be expanded above 30000 times; but it is probable that the utmost power of its spring cannot exert itself to so great an extension, and that no part of the atmosphere reaches above 45 miles from the surface of the earth.

This seems confirmed from the observations of the crepusculum, which is observed commonly to begin and end when the sun is about 18 degrees below the horizon; for supposing the air to reflect light from its most rarefied parts, and that as long as the sun illuminates any of its atoms, they are visible to an eye not intercepted by the curvity of the earth; it will follow, from fig. 8, that the proportion of the height of the whole air, to the semidiameter of the earth, is much about as 1 to 90, or as the excess of the secant of about $8\frac{1}{2}$ degrees to radius: for if E be the eye of the observer, S a place where the sun sets at the end of twilight in E, and the angle ECS, or TCA be found 18 degrees, the excess of the secant of its half ECH, would be the height of the air, viz. GH: but the sun's ray ASH, and the visual ray EH, do each of them suffer a refraction of about 32 or 33 minutes, by which being bent inwards from H towards G, the height of the air need not be so great as if they proceeded straight on; and having from the angle ECS taken the double refraction of the horizontal ray, the half of the remainder will be $8\frac{1}{2}$ degrees, whose secant being 10 111, it follows that as 10000 to 111, so is the semidiameter of the earth, supposed 4000 miles, to 44,4 miles; which will be the height of the whole air, if the places E, S, whose visible portions of the atmosphere, ERZH and SHKB, just touch each other, be 18 degrees asunder.

At this height the air is expanded into above 300 times the space it occupies below; and we have seen the experiment of condensing it into the 60th part of the same space; so that it should seem that the air is a substance capable of being compressed into the 180000 part of the space it would naturally take up, when free from pressure. Now what texture or composition of parts shall be capable of this great expansion and contraction, seems a very hard question; and which is scarce sufficiently accounted for, by the comparing it to wool, cotton, and the like springy bodies.

Hitherto I have only considered the air and atmosphere as one unaltered body, as having constantly at the earth's surface the 800th part of the weight of water, and being capable of rarefaction and condensation in infinitum; neither of which is rigidly true: for the weight of the whole atmosphere is various, being counterpoised sometimes by $28\frac{1}{2}$ inches of mercury, and at other times by no less than $30\frac{1}{2}$; so that the under parts being pressed by about a 15th part less weight, the specific gravity of the air on that account will sometimes be a 15th part lighter than another. Besides, heat and cold very considerably dilate and contract the air, and consequently alter its gravity; to which add the mixture of effluvia or streams rising from almost all bodies, which, assimilating into the form of air, are kept suspended therein, as salts dissolved in liquors, or metals in corroding menstruums; which bodies being much heavier than air, their particles, by their admixture, must needs increase the weight of that air, with which they lie incorporated, after the same manner as melted salts augment the specific gravity of water. The other consideration is, that the rarefaction and condensation of the air is not precisely according to the proportion here laid down; for though experiment very nearly agrees with it, as may be seen in the 58th chap. of Mr. Hook's Micrographia, yet are the condensations not possible beyond certain degrees; for air being compressed into an 800th part of its space, it would be equally dense with water, which yields not to any force whatever, as has been found by several experiments. Nor can the rarefaction proceed in infinitum; for, supposing the spring, by which it dilates itself, to proceed from what texture of parts you please, yet there must be a determinate magnitude of the natural state of each particle, which they cannot exceed, when freed from all manner of pressure.

Though these objections affect the strict accuracy of these conclusions, drawn from the specific gravity of the air, observed at any time; yet the method, here shown, will by a like calculation give the heights of the quicksilver, and the refractions of the air, from any assigned height of the barometer at the earth's surface, and any specific gravity given. As to the condensation and rarefaction by heat and cold, and the various mixture of aqueous and other vapours, these two objections seem generally to compensate each other; for when the air is rarefied by heat, the vapours are raised most copiously; so that though the air, properly so called, be expanded, and consequently lighter, yet its interstices being crouded full of vapours of much heavier matters, bulk for bulk, the weight of the composition may continue much the same.

Having mentioned the difference there is between the height of the mercury at one time, from that at another, it may not be unacceptable to offer some reasons for the said difference, which seem to have some appearance of truth. First then, it is demonstrable that the height of the cylinder of mercury, is equal to the weight of the whole incumbent air; and consequently that difference in the height of the mercury must be owing either to a decrease or increase of new matter to the air. That hypothesis therefore that shows how the air shall be increased or diminished in any particular place, will give a reason for the greater and lesser height of the mercury in the baroscope. But to direct us in the choice of the several causes, which may be assigned for the increase and decrease of the air, it will not be unnecessary to enumerate some of the principal observations made on the barometer.

The first is, that in calm weather, when the air is inclined to rain, the mercury is commonly low. 2. That in serene good settled weather, the mercury is generally high. 3. That on very great winds, though they be not accompanied with rain, the mercury sinks lowest of all, with respect to the point of the compass the wind blows upon. 4. That, cæteris paribus, the greatest heights of the mercury are found on easterly and north-easterly winds. 5. That in calm frosty weather the mercury generally stands high. 6. That after very great storms of wind, when the quicksilver has been low, it generally rises again very fast. 7. That the more northerly places have greater alterations of the baroscope, than the more southerly. 8. That within the tropics, and near them, those accounts we have had from others, and my own observation at St.

Helena, make very little or no variation of the height of the mercury, in all weathers.

I conceive then, that the principal cause of the rise and fall of the mercury, is from the variable winds which are found in the temperate zones. A second cause is the uncertain rising and falling of the vapours lodging in the air, whence it comes to be at one time much more crowded than at another, and consequently heavier; but this latter in a great measure depends on the former. Now from these principles, I shall endeavour to explain the several phænomena of the barometer. As,

1. Why in calm weather, the air being inclined to rain, the mercury is commonly low? I answer that, the mercury's being low, inclines it to rain; for the air being light, the vapours are no longer supported by it, becoming specifically heavier; so that they descend towards the earth, and in their fall meeting with other aqueous particles, they incorporate together, and form little drops of rain; but the mercury's being at one time lower than at another, is the effect of two contrary winds, blowing from the place where the barometer stands; by which the air of that place is carried both ways from it, and consequently the incumbent cylinder of air is diminished, and accordingly the mercury sinks. As for instance, if in the German Ocean it should blow a gale of westerly wind, and at the same time an easterly wind in the Irish sea; or if in France it should blow a southerly wind, and in Scotland a northern; it must be granted, that that part of the atmosphere over England, would thereby be attenuated, and the mercury would subside; and the vapours, which before floated in those parts of the air, of equal gravity with themselves, would sink to the earth.

2. Why in serene settled weather, the mercury is generally high? To this I answer, that the greater height of the barometer is occasioned by two contrary winds blowing towards the place of observation, by which the air of other places is brought thither and accumulated; so that the incumbent cylinder of air being increased both in height and weight, the mercury pressed by it must necessarily rise; and then the air being specifically heavier, the vapours are better kept suspended.

3. Why upon very great winds or storms, though accompanied with no rain, the mercury sinks lowest of all, with relation to the point of the compass on which the wind blows? This is caused by the very rapid motion of the air in these storms; for the tract of the earth's surface where these winds rage not extending all round the globe, that stagnant air which is left behind, as also that on the sides, cannot come in so fast as to supply the evacuation made by so swift a current; so that the air must necessarily be attenuated, more or less, according to the violence of those winds. To which add, that the horizontal

motion of the air, being so quick as it is, may probably take off some part of its perpendicular pressure: and the great agitation of its particles is the reason why the vapours are dissipated, and do not condense into drops, so as to form rain, which otherwise is the natural consequence of the rarefaction of the air.

4. Why, cæteris paribus, the mercury stands highest on an easterly or north-easterly wind? This happens, because that in the great Atlantic Ocean, on this side the 35th degree of north latitude, the westerly and south-westerly winds blow almost always trade, so that whenever the wind comes up here at east and north-east, it is sure to be checked by a contrary gale, as soon as it reaches the ocean; on which account the air must needs be heaped over this island, and consequently the mercury stand high, as often as these winds blow. This holds true in this country, but is not a general rule for other places, where the winds are under different circumstances.

5. Why in calm frosty weather the mercury generally stands high? The cause of this is, that it seldom freezes, but when the winds come out of the northern and north-eastern quarters; or at least, unless those winds blow at no great distance off; for the northern parts of Germany, Denmark, Sweden, Norway, and all that tract from whence north-eastern winds blow, are subject to almost continual frost all the winter; by which the lower air is very much condensed, and in that state it is brought hither; and being accumulated by the opposition of the westerly wind blowing in the ocean, the mercury must needs be pressed to a more than ordinary height.

6. Why, after very great storms of wind, when the mercury has been very low, it generally rises again very fast? This seems to be caused by the sudden accession of new air, to supply the great evacuation made by such continued storms, and by the recoil of the air, after the ceasing of the force that impelled it; and the reason why the mercury rises so fast, is because the air being very much rarefied beyond its mean density, the neighbouring air runs in the more swiftly, to bring it to an equilibrium, as we see water runs the faster for having a great declivity.

7. Why, in more northerly places, the variations of the baroscope are greater than in the more southerly? The truth of this is proved from observations made at Clermont and Paris, compared with others made at Stockholm. And the reason seems to be, that the more northerly parts have usually greater storms of wind than the more southerly, by which the mercury should sink lower in that extreme; and then the northerly winds bringing the condensed and ponderous air from the neighbourhood of the pole, and that again being checked by a southerly wind, at no great distance, and so heaped, must needs make the mercury in such case stand higher, in the other extreme.

8. And lastly, Why, near the equinoctial, as at Barbadoes and St. Helena, there is very little or no variation of the height of the barometer? This remark, above all others, confirms the hypothesis of the variable winds being the cause of these variations of the height of the mercury; for in the places abovementioned, there is always an easy gale of wind blowing nearly on the same point, viz. E.N.E. at Barbadoes, and E.S.E. at St. Helena, so that there being no contrary currents of the air, to exhaust or accumulate it, the atmosphere continues much in the same state: however, on hurricanes, the most violent of storms, the mercury has been observed very low; though this is but for once in two or three years, and it soon recovers its settled state of about $29\frac{1}{2}$ inches.

One principal objection against this doctrine may be, that it supposes the air sometimes to move from those parts where it is already evacuated below the equilibrium, and sometimes again towards those parts where it is condensed and crouded above the mean state; which may be thought contradictory to the laws of statics, and the rules of the equilibrium of fluids. But if we consider, that when once an impetus is given to a fluid body, it is capable of mounting above its level, and checking others, that have a contrary tendency to descend by their own gravity, this will no longer be regarded as a material objection; but shall rather conclude, that the great analogy between the rising and falling of the water on the flux and reflux of the sea, and this of the accumulating and attenuating of the air, is a great argument for the truth of this hypothesis. For as the sea, opposite the coast of Essex, rises and swells by the meeting of the two contrary tides of flood, of which the one comes from the S. W. along the English channel, and the other from the north; and on the contrary, sinks below its level on the retreat of the water both ways, in the time of ebb; so it is very probable, that the air may ebb and flow after the same manner; but by reason of the diversity of causes, by which the air may be put in motion, the times of these fluxes and refluxes are purely casual, and not reducible to any rule, as are the motions of the sea, depending wholly on the regular course of the moon.

A free Inquiry into the vulgarly received Notion of Nature. By the Hon. R. Boyle, Esq. Anno 1686, *8vo.* N° 181, p. 116.

This work is divided into 8 sections. In the 1st section, after having premised something of the manner of conception in the rational soul, the author answers two objections, showing, that it is not blamable to oppugn nature after this manner; and that there is sometimes a necessity to recede from the common opinion of men. The 2d section reckons up the several common acceptations of the word nature; and then substitutes in their stead other words and expressions more suitable to the true notion of nature. The 3d examines the Aristotelian definition of nature, and proves it obscure and intricate; the author then states the reason, why he endeavours to avoid the frequent use of this word nature. The 4th section first examines several axioms concerning nature, by which she is described after the vulgar apprehension; and then lays down a much better description of her. Here the author distinguishes nature into general, which he calls cosmical mechanism, and particular, which he names individual mechanism. In the conclusion is shown the origin of Polytheism, and how nature came to be made a goddess by the ancients. The 5th proposes the reasons by which the author was persuaded to reject the received notion of nature. And the 6th discusses the arguments in behalf of nature, drawn from the common consent of mankind; from the endeavour observable in bodies to maintain their natural state; from the distinction of motion into natural and violent; from the crisis of diseases, &c. The 7th section expounds the several received axioms or attributes of nature; among others, these two, natura vacuum horret, and natura est morborum medicatrix, are largely and accurately handled. The 8th and last section shows, that nature, according to the vulgar acceptation, is not a real, but an imaginary being, &c.

Account of a Book.

Traité du Mouvement des Eaux, et des autres Corps fluides, par feu Mr. Mariotte, a Paris. An. 1686, 8vo. N° 181, p. 119.

This work is divided into 5 parts, and each part again into discourses or chapters. The first part contains 3 discourses; the 1st of which is concerning the several properties of fluid bodies, as their glaciation, evaporation, dilatation by heat, and admission of the air into their pores, &c. The 2d is on the origin of fountains; which the author deduces from the rains that fall, and sink into the earth, till they meet with a clayey or rocky soil, which being not able to pass, they run along till they find their way out into the air, where they become fountains. And to prove that the quantity of rain water is sufficient to furnish the rivers, he shows by experiment, that there falls in the countries about the fountains of the Seine, at least 7 times as much water as that river discharges. The 3d is about the origin and cause of winds, where he assigns 3 general and 4 particular causes. The first of the general causes, is the diurnal motion of the earth; the second is the condensation and rarefaction of the air, caused by the heat of the sun; and the third is from the moon's respect to her apogæon or perigæon, by which she sometimes rises from, and at other times descends towards the earth. The particular causes are, 1st. The extraordinary rising of the vapours and exhalations out of certain places of the earth. 2d. The fall of great rains and hails. 3d. The great quantity of nitrous and sulphureous exhalations in earthquakes. 4th. The sudden melting of snow in the high mountains.

The second part treats of the equilibrium of fluids. The 1st discourse demonstrates, from the principles of mechanics, how fluids counterpoise each other's weight, and gives the rules for the doctrine of floating bodies. The 2d discourse shows the nature of the elasticity of air and flame, and how their spring is counterpoised by weight. The 3d discourse treats of the equipollence of a fluid body to a stroke or shock; showing the rules for the force of jets d'eau, from several heights of the reservatory, and different diameters of the bore of the pipe; giving also an account of the comparative force of wind and water-mills with the manner of computing them.

The third part treats of the measure of running and spouting waters. In the 1st discourse, are produced several experiments to find the quantity of water passing through a bore of an inch diameter, just under the surface of the water. Notice is here also taken of the length of the pendulum vibrating seconds, in parts near the equinoctial, having been found at Cayenne a tenth, and at the isle of Goree, near Cape Verde, an eighth of an inch shorter, than at Paris; the cause of which is stated to proceed from the diurnal motion of the earth.

The second discourse shows by experiment, that the quantity of water expended by a jet d'eau, of the same diameter of bore, but at different heights of the reservatory, are in a subduple proportion of these heights. The third discourse shows, that the quantity evacuated by different bores, at the same height of the reservatory, are as the squares of the diameters of the bores. The fourth discourse shows, the manner of finding the quantity of water which a river or an aqueduct furnishes, illustrated by the example of the Seine at Paris.

The fourth part treats of the height to which the water of fountains rises. And its first discourse shows, that the jets d'eau never rise so high as their reservatories, but always fall short, by spaces which are in duplicate proportion of the heights they rise to; which is proved by several experiments. The next inquiry is, the best sort of ajutages or spouts for jets d'eau; affirming from experiment, that an even polished round hole in the end of the pipe, gives a higher jet than either a cylindric or conical ajutage, of which yet the latter is the better. A second discourse of this part treats on the amplitudes or distances of oblique jets, according to the doctrine of Galileo and Torricelli, and concludes with a geometrical way of finding the height of the reservatory by the horizontal stream issuing out of a hole bored in the side of the pipe.

The fifth and last part treats of the pipes that are to convey water, and of their necessary strength. It consists of three discourses: the first shows the size of pipes requisite for the several expenses of water; proving that in small pipes emptying the same water, the water, running faster, has more friction and is consequently more retarded; to avoid which, this rule is given, that the squares of the diameters of the pipes be as the quantity of water to be expended; in which case the water will run in all alike fast, and the friction be the same; and when a great pipe branches into several smaller, distributed to different jets, the square of the diameter of the main pipe must be proportioned to the sum of all the expenses of its branches. The second discourse treats of the strength of pipes requisite for bearing the pressure of the water; where are several pretty experiments of the resistance of solids. The last gives a method of distributing water by pipes into a city; and shows how those pipes are to be cleansed from mud, by leaving apertures to let out the water in those places where the pipes lie lowest; and from air, by the like apertures left on the tops of those eminences where the pipes pass.

An Essay towards an Universal Alphabet. By Mr. Francis Lodwick, R. S. S. N° 182, p. 126.

Having observed a great difficulty in truly writing what is pronounced, or truly pronouncing what is written, either in our own, or foreign languages, by the common alphabets now in use, arising either from the want of some letters, or the different pronunciation of the same character, or letter, in different languages, and the irregularities of its various sounds in any one language; I saw a necessity of some such expedient as is here attempted, viz. An Universal Alphabet, which should contain an enumeration of all such single sounds, or letters, as are used in any language. The benefits of such a collection are the following. 1. Children, being at first taught and accustomed to the true expression of all these single sounds, or letters, will without difficulty be brought to pronounce truly and readily any language whatever; for the difficulty some persons have to pronounce some letters, is owing to their not being accustomed to pronounce them, either single or in conjunction with others: a difficulty which chiefly takes place in persons come to age. 2. It will enable any one, accustomed to the true pronunciation of this alphabet, truly to describe the pronunciation of any language whatever; and although he had never before heard this language pronounced, he shall at first sight of such writing, be able truly to sound it. 3. It will also be useful to perpetuate the true sounds of any language, and serve as a standard of it to after-ages. For if all the single sounds be here characterized, and that no one character have more than one sound, nor any one sound be expressed by more than one character, it cannot happen that any character should be falsely pronounced, without being soon discovered; for this false sound must be the true sound of some other letter of this alphabet.

In this collection I proceed according to these rules.—1. That no true single sound can be justly described or expressed by the conjunction of any two or more other single sounds, viz. if a vowel, by the conjunction of other single vowels, or if a consonant, by the conjunction of other single consonants. 2. That whatever sound cannot be expressed or described, except by the conjunction of two or more single sounds, it is no single but a compounded sound. 3. That in every composition of single sounds, the particular single sounds which make up that composition ought to be truly and clearly discerned in the sound of the composition, otherwise it cannot be truly said to be a composition and composed of such single sounds.

The single sounds, commonly named letters, are usually distinguished into vowels and consonants. Vowels are such as are singly expressible, as a, e, o, &c. Consonants are such as cannot singly be expressed without the conjunction of a vowel, as, b, d, f, g, &c.

The whole number of vowels are these 14 following; to which, for the better distinguishing their sounds, are annexed as many words in which they are expressed, all English except three, viz. 7, 8, 12, because no English words occurred in which they are expressed.

1	a as	tall	8	ui as	muis, Low Dutch
2	a ..	tallow	9	y ..	tile
3	a ..	tale	10	o ..	tone
4	e ..	tell	11	u ..	tun
5	ea..	teal	12	u ..	une, French
6	i ..	till	13	oo..	tool
7	u ..	dure, French	14	ou..	tould.

These vowels have each a long and a short sound. Short, as in the words, God, man, sin; and long, as in ball, demand, seen, &c.

Of dipthongs.—A diphthong, in the ordinary use of the word, signifies a compound of two vowels, but those commonly so named are most of them nothing but single vowels, as ea, oo, ou, eo, ai, in the words teal, tool, tould, people, main, &c. That these are only single sounds will appear if we consider the sounds of the vowels singly, which make those supposed compositions; and then whether those sounds in composition will make out the true sound required, so as both of them may be clearly discerned in these pretended compounds. For instance, as ea in teal, consider the sound of e in the word sent or in the word scene; and a in the word ball, or in the word and, or in the word tale; and then whether e, in either of the two sounds going before, and a in either of the three sounds following, joined together, will make out the true sound of ea in the word teal; if not, then is it a single sound. If you thus proceed to examine all the others, you will I doubt not find the same event; and I believe the true diphthongs and triphthongs of the Greeks were no other than a true expression of the single vowels they joined together, but in so short a time, as that both or all three were expressed in the time that usually one single vowel is expressed in.

Of Consonants.—The whole number of consonants are ranged in the following table, in 6 ranks and 11 files.

	1	2	3	4	5
1	B bond	D dark	J. Jest	G game	=
2	P pond	T tart	Ch. Chest.	K came	=
3	M mind	N name	gn Seignior } Fr.	ng song	=
4	=	dh this	J. Jean } Fr.	g gaen } l. d.	V valley
5	=	th thing	sh shall	ch dach } l. d.	F folly
6		n danse Fr.			

	6	7	8	9	10	11
1	=	L lane	H hand	Y yarn	R rand	W wand
2	=					
3	=					
4	Z zeal	lh W				
5	S seal					

The first file contains 3 consonants, the second 6; the third and fourth 10; the fifth and sixth 4; the seventh 2; the remaining four, each one; in all 29 consonants. The second rank in each file contains derivatives, in relation to the first rank, or their primitives, all alike in kind. So also all the derivatives in the third, fourth, and fifth ranks, whence their sounds will be the better comprehended.

Those places filled by two strokes, (=), signify that sounds may be expressed by the same position of the mouth with their primitives, answering in kind to those in the same rank in which they stand; but they would be so alike in pronunciation to some others in the table, that the difference would be too nice for common discernment. As those of the fourth and fifth ranks, in the first file, are like those of the fourth and fifth ranks in the fifth file; and those of the first, second and third ranks in the fifth file, are like those of the same ranks in the first file; so those of the first, second, and third ranks in the 6th file, are like those of the same ranks in the second file.

Some of these 29 single consonants, are commonly supposed compounded; as th, ch, sh, gn, ng, &c. But if we consider the sound of each single consonant in the composition a-part, and then the conjunction of them in that order, so as the single sounds may be clearly discerned in the composition, we shall never make the sounds required; and if neither by this, nor by any other conjunction, the required sound can be made out, it must be a single, and no compound sound.

All single sounds ought to have single and distinct characters. But it will be impossible, in the use of the present characters or alphabets, to add those that are wanting, and to correct and limit the sound of others in use, thence to constitute a perfect alphabet; because people so long accustomed to such corrupt and different expressions of the present characters, will be always subject, on the sight of the old, to give them those sounds they have been used to, and to spell words according to their old and corrupt custom, whatever rules shall be given to the contrary; and therefore there will be a necessity of a whole new set of characters, both vowels and consonants. Such a set of literal characters is proposed in fig. 9 and 10, pl. 9. The first rank in every file are the radical characters; the other succeeding ranks have each a distinct characteristical addition, to distinguish them from each other, which causes some complication; but yet I judged it necessary to express the same in the character, the more regularly to sort them into classes, and to express the derivation of letters of the same organ, the one from the other.

In writing, the vowels are to be placed over the consonants, which they follow in expression; and whereas some syllables begin with a vowel, place the 12th consonantal character, answering to the Hebrew, aleph; and over the same, place the vowel beginning such a syllable. To distinguish the long vowel from the short, add a dot to the vocal character. The 9, 11, 12, 13, 14th vocal characters, are, for want of single strokes, compounded of the first and second. The diphthongs, truly such, may be made by the conjunction of the single vocal characters in the order as they follow, and they will be easily distinguished from the 5 preceding compounded characters of the single vowels, because there will not readily occur any diphthongs compounded of the first two vowels. The accent may be a cross line under the syllable to be accented. The characters signifying the various modes of expression, may be these following, and ought to be placed at the beginning and end of every sentence, requiring it:—[ ] Explication, ( ) Parenthesis, ¡ ¡ Emphasis, ? ? Interrogation, ! ! Wonder, ¿ ¿ Irony.

An Essay concerning the Universal Primer. By Mr. Lodwick.
N° 182, p. 134.

As the present alphabets are imperfect, so are also the primers, or first books, by which children and others are taught to spell and read. First, in not having a perfect alphabet; secondly, in not being digested in such a method, as is fit and proper to teach them; for the usual way of teaching to spell, is to dismember every syllable, of more than one letter, into many syllables, by expressing every letter a-part, and syllabically, and the consonants with such a vowel as they are usually named with; and then requiring them to join all these syllables into one word. But how preposterous this method is, one instance will show; suppose the monosyllable brand be to be spelled; they teach them thus to dismember it, bee, er, a, en, dee, and then require them to join these into one syllable, which it is impossible to do; and they must be necessitated, as they have began, to express this one syllable by five syllables; whereas they should be taught to express every syllable entire at first sight, without dismembering it. And to do this, they must proceed gradually, first beginning with the most simple syllables, and so by degrees proceeding to the more difficult and compounded, till they can readily pronounce a whole syllable at first sight, even the most difficult.

To that end let all the primers be thus contrived: at the top of the leaf let all the vowels be placed singly in order as they follow in one rank; and under the same, place syllables; first, of one vowel and one consonant, following it throughout all the variations; then of one consonant and one vowel following; 2dly, of two consonants before, and one vowel following, throughout the variations; 3dly, of one vowel and three or four consonants following, and of three consonants going before, and one vowel following; 4thly, of one, two, or three consonants going before a vowel; and one, two, three, or four consonants following: 5thly, of some syllables with diphthongs or triphthongs. For instance,

a	e	i	o	u	&c.
ab	eb	ib	ob	ub	&c.
ad	ed	id	od	ud	&c.
ba	be	bi	bo	bu	&c.
ald	eld	ild	old	uld	&c.
dra	dre	dri	dro	dru	&c.
balm	belm	bilm	bolm	bulm	&c.

After this, place a number of words of two, three, or four syllables, from the more easy to the most difficult expressions, without regarding their significations. Further, let there follow some words of several syllables, with the accent variously placed, as on the first, second, third, &c. Let there be also two or three small discourses written in this alphabet, in so many several languages, with the accent rightly placed, and truly distinguished by their pauses. And thus you have a perfect primer.

Of teaching with this Primer.—Begin with the true sound of all the vowels singly; then proceed to the following single syllables, beginning with the easiest of expression, and so on, gradually to the most difficult; then to words of more syllables; and lastly, to the use of the accent and pauses. When the learner has passed all these, he may be exercised in reading the discourses, and therein let him exactly observe the accent and the pauses. When he can read and utter exactly whatever is written in this character, in what language soever, then teach him to write truly what he hears distinctly expressed, according to this alphabet, proceeding therein gradually as before; and observing rightly to place the accent and pauses, and also the use of the signs of the different modes of speaking.

In teaching observe these necessary rules:—1. Proceed leisurely and orderly. Suffer not to pass by any mispronunciation uncorrected. 2. In spelling, let no syllable be dismembered by repeating the letters singly, but pronounced whole as they are found. This new primer will, without change, except in the title, be the same for all nations and languages.

Some further Remarks on the Instrument proposed by an anonymous French Author, for effecting a perpetual Motion, an Account whereof is given in N° 177 *of these Transactions. By Dr. Papin, M.D. R. S. S.* N° 182, p. 183.

Having seen in the Journal des Sçavans of May 13th, and in the Nouvelles de la Republique des Lettres of the month of June, that the author of the perpetual motion is not satisfied, but endeavours to answer the objection that I propounded against his contrivance, in the Philosophical Transactions of the month of December, 1685, I find I must explain myself more at large than I did in that paper.

I am fully enough convinced, that the mercury in his engine must follow the laws of the equilibrium of fluid bodies: but the consequence which he draws from that principle, seems to me very groundless; for although the lowermost part of the bellows be pressed by the weight of 40 inches of mercury, it does not follow that all the parts which are situated higher must bare the same pressure. On the contrary, it is plain that the upper part, having no mercury above it, bears none at all; the parts that lie in the middle, near the axes of the bellows, bear but 20 inches; and so all the rest must bear more or less, according as they lie higher or lower: it is evident therefore, that there are as many parts that bear less than 20 inches, as there are that bear more; and the increase of pressure following an arithmetical progression, it is undeniable, that all these pressures added together will do no more than one uniform pressure, that would be equal to 20 inches every where.

Having thus found the quantity of pressure caused by the mercury within the bellows, we must remember that the pressure of the atmosphere within the same bellows is equivalent but to 5 inches, as I observed in my first paper; so that we find that the inward pressure is equivalent only to 25 inches of mercury in all. Now the pressure of the atmosphere on the outside is every where equal to 27 inches (French); from whence it appears that the pressure without is stronger than the pressure within. So that I had reason to say, that the bellows standing upright must rather shut than open.

Although I might observe some other things in his description, which will increase the difficulty of opening the bellows, I forbear to speak of them; noticing only what is most material, and makes his perpetual motion to be altogether impossible. As for the argument the author draws from comparing his engine to an ordinary siphon; I beseech him to consider what a difference there is between a siphon from which the water runs down at the bottom, and his engine, which ought to draw up the heavy liquor into the highest part of the instrument, and I doubt not but he will acknowledge the weakness of his argument.

A short Examen of the Stones sent to the R. S. from Bern, of which an Account is given in the last Transaction. By Frederick Slare, M. D. R. S. Soc. N° 182, p. 140.

Those who have made experiments in hydrostatics find all pure metals to have specific and peculiar gravities. From this hint Dr. S. formerly endeavoured to discover the nature of the calculus humanus, which he found to have no attributes that are proper to a real stone; and bringing them to a hydrostatic test, he found them very different in their specific gravity, and very remote from an equal proportion to their bulk of common stone, when weighed in water. There is a standard of gravity so competent to all real stones, that where they deviate from this standard, we have good reason to question those concretions, whether they are stones or not. The standard of gravity for real stones he found to be generally about 2 to 1 * of the common fluid.

* Common stone however, is to water, more accurately as $2\frac{1}{2}$ to 1.

This stone sent from Bern for 13 drs. must either have been avoirdupois, or else is wasted something, for he found it only to weigh in the air 12 dr. 36 gr. in water 6 dr. 48 gr. The difference thereof is 5 dr. 48 gr.

The proportion between this concrete and water proves to be as 756 to 348, or as 2 and somewhat more than a 6th to 1.

This matter, compared with common chalk, was found specifically lighter, bearing only the proportion to water of 521 to 290, considerably short of that of 2 to 1. Shells and testaceous bodies very nearly agree with this matter; which takes off the former opinion that this patient had perhaps devoured wall-lime, and such like testaceous matter, from whence the stone might receive its original: for this being broken into pieces, will not so easily cement again into so compact a body as it was formed of, as we see in whiting, which is lighter than chalk: Hence, this being vastly heavier than chalk, can scarcely be thought a concretion of such a matter.

Compared with petrified water, i. e. an icicle broken off a grotto, where the petrifying spring furnished enough; it came very near the gravity of our stone and the usual weight of ordinary stones; a piece that weighed 5 drams out of the water, discovered its weight to bear the proportion of 403 to 184, or 756 to 345, to that of water. This anomalous substance being so near the weight of our petrified water, would almost incline a man to believe it real stone, and the rather because we are informed the patient drank much water. The following experiments also seem to prove that it is rather of the ordinary stony constitution, than of that which is proper to animal concretions. For instance, ordinary vinegar was poured upon it, it presently wrought upon it with a hissing noise, as it did on the petrified water when powdered. When spirit of vitriol was added, it also dissolved it, but let it fall again, as aquafortis does tin when it has corroded it; which is agreeable to the relator's account. But it does not appear that he used spirit of salt; for this wrought upon it very vigorously, and presently dissolved it, and kept it so without any precipitation.

All these experiments distinguish this concrete from the ordinary animal ones, as the stone in the bladder, kidney, the tophi, &c. for these will not be dissolved, or in the least corroded by any of the mentioned acids: though spirit of nitre be a general menstruum, which dissolves them all readily.* Dr. Slare adds, that he failed in these trials to extract a volatile and fixed salt from it.†

* The assertion that uninary and other animal calculi are not corroded or dissolved by any acid except spirit of nitre (nitric acid) is erroneous. Many of them are soluble in the muriatic acid also.

† In his "further trial" however he says that he distilled from it a liquor that looked and smelled like spirit of hartshorn.

A further Trial of the said Stones by chemical Distillation. By the same. N° 182, p. 145.

This stone was brought to a gross powder, and conveyed into a coated retort, which was kept for some hours in a naked fire, so hot that the glass melted. The quantity put into the retort amounted to $\frac{1}{4}$ oz. 20 grs. The liquor that came over scarcely afforded 3 or 4 drops, which looked like spirit of hartshorn rectified, and smelled much like the same: which plainly discovered it to be an animal substance, though it afforded much less than the [ordinary] calculus humanus does: and by consequence gave a much larger proportion of caput mortuum or residuum in the retort: all which is very consentaneous to the nature of the stone, for its specific gravity was much heavier than the stones are we usually find in the human body; and therefore the parts may be supposed more fixed, or consisting of fewer volatile parts, such as are carried over by distillation.

The remainder, weighed in the retort, came to 3 drs. 50 grs.; 10 grs. of which hung about the neck of the retort in the form of a dirty hard baked oil. The other 20 gr. partly went off in vapour through the lute, and what was found in the receiver in a liquid form.

On applying a magnet to this caput mortuum, no particles of iron were found to adhere to it.

Two Observations of the Eclipse, November 30th last, made at Nuremburg; the one by Mr. G. C. Eimmart, the other by Mr. J. P. Wurtzelbaur. Communicated by Mr. Theodore Haak, R.S.S. N° 182, p. 146.

This eclipse of the moon was the more remarkable, as it fell out very near the apogeon of the moon, and was nearly central; so that the duration was as great as possible. But neither at London nor Greenwich, nor Paris, could it be seen, for thick clouds, which intercepted the sight of the moon the whole time: the only account we have received is already published, from letters of M. Hevelius of Dantzic, in N° 178 of these Transactions: and now these two from Nuremburg, made by the industrious observers Mr. Eimmart and Mr. Wurtzelbaur.

The times of the principal phases, by both the observers, were as below, by

	Eimmart.			Wurtzelbaur.		
Beginning of the eclipse	9ʰ	21ᵐ	30ˢ	9ʰ	23ᵐ	30ˢ
The total immersion	10	23	30	10	25	20
Beginning of emersion	12	13	10	12	11	30
End of the eclipse	13	14	0	13	14	30

By these observations the middle of the eclipse ought to have been about 11h. 19m. P.M. at Nuremberg: the duration will be 3h. 51m.; and the total darkness 1h. 46m. The longitude of Nuremburg has been formerly stated at 11 degrees from London, and since found to be so, by observations of the last eclipse of the sun July 2, 1684, which made it 44½m. of time. So that the middle of this eclipse at London should have been 10h. 34½m. which from the observation of Mr. Hevelius had been formerly concluded 10h. 35m.

Account of an extraordinary Swarm of Grasshoppers near Avignon, in Languedoc. Communicated by Mr. Justell, R.S.S.* N° 182, p. 147.

These insects are undoubtedly of a peculiar species, although they appear in nothing different from the common sort; but they take flight like birds, which is particular to them. They are about an inch in length, and of a grey colour. The last year the earth in some places was covered 4 fingers thick with them, in the morning before the heat of the sun was considerable; but as soon as it began to be hot, they took wing, and fell upon the corn, eating up both leaf and ear, and that with such expedition, by reason of their great number, that in three hours they would devour the corn of a whole field; after which they again took wing; and their swarms were so thick, that they covered the sun like a cloud, and were whole hours in passing. They flew against the wind, and went over the castle, which is very high, and seized on another field of corn, which they destroyed like the former. After having eaten up the corn, they fell upon the vines, the pulse, the willows, and even the hemp, notwithstanding its great bitterness. About the end of August they ceased flying, and copulated, and the female stuck her tail into the hard earth, where she cast a foam, and made with it in the ground a hole as large as that of a goose quill, and about an inch long, wherein she laid her eggs, which are of the size of millet seed; there would be sometimes 50 of these eggs in a hole, which are so covered over with the same earth, that the water does not get in. After this, all these insects died and stunk very much. They began this year to hatch in the month of April, and some are not yet hatched. In March we thought of destroying their eggs, which lie not above a finger's breadth in the earth, and we took of them 180 quintals, being 9 tons: it had been well if we had thought of this expedient sooner. Since their hatching, they have taken above 15 tons of the young grasshoppers, which are not yet larger than flies. There are yet a multitude that have escaped us, because they are in the corn, which is too forward

* It is impossible to determine the particular species of locust (or grasshopper, as it is here termed) whose ravages are described in the present paper.

to be gone into, without spoiling it. They have ruined the people of these parts, who had no harvest the last year, and it will cost above 3000 livres to destroy them this year. They have been taken in abundance in the neighbouring villages. If this care had not been taken, there would have been enough of them to have eaten up the corn of the whole province.

An Extract of two Essays in Political Arithmetic concerning the Comparative Magnitudes, &c. of London and Paris. By Sir William Petty, Knt. R.S.S. N° 183, p. 152.

The author of these two essays has, in several former ones of the same nature, made it appear that mathematical reasoning is not only applicable to lines and numbers, but affords the best means of judging in all the concerns of human life. In the present he endeavours to prove that London is the most considerable city now in being, by showing it much to exceed Paris, both in people, houses, and wealth. The first he proves by comparing the bills of mortality, by which he finds that the people of London are as many as those of Paris and Rouen put together. The second, by comparing the number of houses, which by the chimney books are found above 80,000 in London, whereas a great author among the French reckons but 50,000 houses in Paris. As to the third, viz. the wealth, he conceives that there is yet a much greater disproportion, there being no comparison between them for trade, &c.

An Historical Account of the Trade Winds and Monsoons, observable in the Seas between and near the Tropics; with an Attempt to assign their Physical Cause. By E. Halley. N° 183, p. 153.

The whole ocean may most properly be divided into three parts, viz. 1. The Atlantic and Ethiopic Sea. 2. The Indian Ocean. 3. The great South Sea, or the Pacific Ocean. And though these seas do all communicate by the south, yet, as to our present purpose of the trade winds, they are sufficiently separated by the interposition of great tracts of land, the first lying between Africa and America; the second between Africa and the Indian islands and New Holland; and the last, between the Philippine isles, China, Japan, and New Holland, on the west, and the coast of America on the east.

I. In the Atlantic and Ethiopic Seas, between the tropics, there is a general easterly wind, all the year long, without any considerable variation, excepting that it is subject to be deflected some few points of the compass towards the north or south, according to the position of the place. The observations which have been made of these deflections, are the following:

1. That near the coast of Africa, as soon as you have passed the Canary isles you are sure to meet a fresh gale of wind at N. E. about the latitude of 28° north, which seldom comes to the eastward of the E. N. E. or passes the N. N. E. This wind accompanies those ships bound to the southward, to the latitude of 10° north, and about 100 leagues from the Guinea coast, where, till about the 4th degree of north latitude, they fall into calms and tornadoes.

2. That those bound to the Caribbee isles find, as they approach the American side, that the said N. E. wind becomes still more and more easterly, so as sometimes to be east, sometimes east by south, but yet most commonly to the northward of the east a point or two, seldom more. It is likewise observed, that the strength of these winds gradually decreases, as you sail to the westward.

3. That the limits of the trade and variable winds in this ocean are farther extended on the American side than on the African: for whereas you meet not with this certain wind till after passing the latitude of 28° on this side; on the American side it commonly holds to 30, 31, or 32° of latitude; and this is likewise verified to the southward of the equinoctial; for near the Cape of Good Hope, the limits of the trade winds are 3 or 4° nearer the line than on the coast of Brasil.

4. That from the latitude of 4° north to the said limits on the south side of the equator, the winds are generally and perpetually between the south and east, and most commonly between the south-east and east; observing always this rule, that on the African side they are more southerly, on the Brasilian more easterly, so as to become almost due east, the little deflection they have being still to the southward. In this part of the ocean, I found the winds constantly about the south-east, the most usual point south-east by east; when it was easterly it generally blew hard, and was gloomy, dark, and sometimes rainy weather; if it came to the southward it was generally serene, and a small gale next to a calm, but this not very common. But I never observed it to the westward of the south, or northward of the east.

5. That the season of the year has some small effect on these trade winds; for when the sun is considerably to the northward of the equator, the south-east winds, especially in the strait of this ocean, between Brasil and the coast of Guinea, vary a point or two to the southward, and the north-east become more easterly; and, on the contrary, when the sun is towards the tropic of Capricorn, the south easterly winds become more easterly, and the north easterly winds on this side the line veer more to the northward.

6. That as there is no general rule but admits of some exception, so there is in this ocean a tract of sea, wherein the southerly and south-west winds are per-

petual, viz. all along the coast of Guinea for above 500 leagues together, from Sierra Leona to the isle of St. Thomas; for the south-east trade wind having passed the line, and approaching the coast of Guinea within 80 or 100 leagues inclines towards the shore, and becomes S. S. E. and by degrees, as you come nearer, it veers about to south, S. S. W. and in with the land south-west, and sometimes W. S. W. These are the winds which are observed on this coast, when it blows true; but there are frequent calms, violent sudden gusts, called tornadoes, from all points of the compass, and sometimes unwholesome foggy easterly winds, called hermitan by the natives, which too often infest the navigation of these parts.

7. That to the northward of the line between 4 and 10° of latitude, and between the meridians of Cape Verde, and of the easternmost islands that bear that name, there is a tract of sea wherein it were improper to say there is any trade wind, or yet a variable one; for it seems condemned to perpetual calms, attended with terrible thunder and lightning, and rains so frequent, that our navigators from thence call this part of the sea the Rains; the little winds they have are only some sudden uncertain gusts, of very short continuance, and less extent; so that sometimes each hour there is a different gale, which dies away into a calm before another succeeds; and in a fleet of ships, in sight of one another, each will have the wind from a different point of the compass; with these weak breezes ships are obliged to make the best of their way to the southward, through the aforesaid 6°, wherein it is reported some have been detained whole months for want of wind.

From the last three observations is shown the reason of two notable occurrences in the East India and Guinea navigations. The one is, why, notwithstanding the narrowest part of the sea between Guinea and Brasil is about 500 leagues over, yet ships bound to the southward, especially in the months of July and August, find sometimes a great difficulty to pass it. This happens because of the south-east winds at that time of the year commonly extending some degrees beyond the ordinary limit of 4° north latitude, and coming also so much southerly as to be sometimes south, sometimes a point or two to the west; there remains then only to ply to windward; and if on the one side they stand away W. S. W. they gain the wind still more and more easterly; but there is danger of not weathering the Brasilian shore, or at least the shoals on that coast. But if, on the other tack, they go away E. S. E. they fall into the neighbourhood of the coast of Guinea, from which there is no departing, without running easterly, as far as the isle of St. Thomas, which is the constant practice of all the Guinea ships, and which may seem very strange without considering the sixth remark, which shows the reason of it. For being in with the coast the wind blows generally at S.W. and W.S.W. with which winds they cannot go to the northward for the land, and on the other tack they can lie no nearer the wind than S.S.E. or S.; with these courses they run off the shore, but in so doing they always find the winds more and more contrary; so that when near the shore they could lie south; at a greater distance they can make their way no better than S.E. and afterwards E.S.E. with which courses they fetch commonly the isle of St. Thomas and Cape Lopez, where finding the winds to the eastward of the south, they keep them favourable by running away to the westward in the south latitude of 3 or 4°, where the south-east winds are perpetual.

For the sake of these general winds, all those that use the West Indian trade, even those bound to Virginia, reckon it their best course to get as soon as they can to the southward, that so they may be certain of a fair and fresh gale, to run before it to the westward; and for the same reason those homeward bound from America endeavour to gain the latitude of 30° as soon as possible, where they first find the winds begin to be variable, though the most ordinary winds in the northern part of the Atlantic ocean come from between the south and west.

What is here said is to be understood of the sea winds, at some distance from the land; for upon and near the shores the land and sea breezes are almost every where sensible, and the great variety which happens in their periods, force, and direction, arises from the situation of the mountains, valleys, and woods, and from the various texture of the soil, more or less capable of retaining and reflecting heat, and of exhaling or condensing vapours.

II. In the Indian Ocean the winds are partly general, as in the Ethiopic Ocean, and partly periodical, that is, half the year they blow one way, and the other half nearly on the opposite points, and these points and times of shifting are different in different parts of this ocean; the limits of each tract of sea subject to the same change or monsoon are certainly very hard to determine; yet the following particulars may be relied on, being the result of great care and diligence:—

1. That between the latitudes of 10° and 30° south, as between Madagascar and New Holland, the general trade wind about the south-east by east, is found to blow all the year long, after the same manner as in the same latitudes of the Ethiopic Ocean.

2. That the aforesaid south-east winds extend to within 2° of the equator, during the months of June, July, August, &c. to November, at which time, between the south latitudes of 3° and 10°, being near the meridian of the north end of Madagascar, and between 2° and 12° south latitude, being near Sumatra and Java, the contrary winds from the north-west, or between the north and

west, set in and blow for half the year, viz. from the beginning of December till May; and this monsoon is observed as far as the Molucca isles.

3. That to the northward of 3° south latitude, over the whole Arabian or Indian Sea, and Gulph of Bengal from Sumatra to the coast of Africa, there is another monsoon, blowing from October to April, on the north-east points; but in the other half year, from April to October, on the opposite points of S.W. and W.S.W. and that with rather more force than the other, accompanied with dark rainy weather; whereas the north-east blows clear. It is likewise to be noted that the winds are not so constant, either in strength or direction, in the Gulph of Bengal, as they are in the Indian Sea, where a certain steady gale scarcely ever fails. It is also remarkable, that the south-west winds in these seas are generally more southerly on the African side, and more westerly on the Indian.

4. There is a tract of sea to the southward of the equator, subject to the same changes of the winds, viz. near the African coast, between it and the island Madagascar, and from thence northward, as far as the line; wherein from April to October there is found a constant fresh S.S.W. wind, which, as you go more northerly, becomes still more and more westerly, so as to fall in with the W.S.W. winds mentioned before, in those months of the year to be certain to the northward of the equator.

5. That to the eastward of Sumatra and Malacca, to the northward of the line, and along the coast of Camboia and China, the monsoons blow north and south, that is, the north-east winds are much northerly, and the south-west much southerly. This constitution reaches to the eastward of the Philippine isles, and as far north as Japan. The northern monsoon setting-in in these seas in October or November, and the southern in May, blowing all the summer months. Here it is to be noted, that the points of the compass from whence the wind comes in these parts of the world, are not so fixed as in those lately described, for the southerly will frequently pass a point or two to the eastward of the south, and the northerly as much to the westward of the north; which seems occasioned by the great quantity of land interspersed in these seas.

6. That in the same meridians, but to the southward of the equator, being that tract lying between Sumatra and Java to the west, and New Guinea to the east, the same northerly and southerly monsoons are observed; but with this difference, that the inclination of the northerly is towards the north-west, and of the southerly towards the south-east. But the points from which the winds blow are not more constant here than in the former, viz. variable 5 or 6 points. Besides, the times of the change of these winds are not the same as in the Chinese seas, but about a month or 6 weeks later.

7. That these contrary winds do not shift all at once; but in some places the time of the change is attended with calms, in others with variable winds; and it is particularly remarkable, that the end of the westerly monsoon on the coast of Coromandel, and the last two months of the southerly monsoon in the seas of China, are very subject to be tempestuous; the violence of these storms is such, that they seem to be of the nature of the West India hurricanes, and render the navigation of these parts very unsafe about that time of the year. These tempests are by our seamen usually termed, the breaking up of the monsoons. By reason of the shifting of these winds, such as sail in these seas are obliged to observe the seasons proper for their voyages; of which, if they miss, and the contrary monsoon sets in, they are forced to give up the hopes of accomplishing their intended voyage till the winds become favourable.

III. The third ocean, called Mare Pacificum, whose extent is equal to that of the other two, is that which is least known to our own or the neighbour nations; what navigation there is on it, is by the Spaniards, who go yearly from the coast of New Spain to the Manillas, and that only by one beaten track. What the Spanish authors say of the winds they find in their courses, and which is confirmed by the old accounts of Drake and Cavendish, and since by Schooten, who sailed the whole breadth of this sea in the south latitude of 15 or 16°, is, that there is a great conformity between the winds of this sea, and those of the Atlantic and Ethiopic seas; that is, that to the northward of the equator, the predominant wind is between the east and north-east; and to the southward thereof, there is a constant steady gale between the east and south-east; and that on both sides the line, with so much constancy, that they scarcely ever need to attend the sails; and with such strength, that it is usual to cross this vast ocean in ten weeks time, which is about 130 miles a day; besides, it is said that storms and tempests are never known in these parts: so that some have thought it might be as short a voyage to Japan and China, to go by the straits of Magellan, as by the Cape of Good-Hope.

The limits of these general winds are also much the same as in the Atlantic sea, viz. about the 30th degree of latitude on both sides; for the Spaniards, homeward bound from the Manillas, always take the advantage of the southerly monsoon, blowing there in the summer months, and they run up to the northward of that latitude, as high as Japan, before they meet with variable winds, to shape their course to the eastward. And Schooten and others, that have gone about by the Magellan straits, have found the limits of south-east winds, much about the same latitude to the southward; besides, a further analogy between the winds of this ocean and the Ethiopic, appears from their being always southerly, as they are found near the shores of Angola.

In the foregoing history are contained several problems, that merit well the consideration of the acutest naturalists, both by reason of the constancy of the effect, and its vast extent. The chief of these problems are, 1. Why these winds are perpetually from the east in the Atlantic, Ethiopic, and Pacific oceans, between the latitudes of 30 north and south. 2. Why the said winds extend no farther, with constancy, than to the latitudes of 30°. 3. Why there should be a constant south-westerly wind upon and near the coast of Guinea. 4. Why in the north part of the Indian ocean the winds, which for one half year agree with those of the other two oceans, should change in the other half year, and blow from the opposite points; while the southern part of that ocean follows the general rule, and has perpetual winds about south-east. 5. Why, in these general trade-winds, it should always hold, that to the northward of the equator, it is inclined to the northward of the east; and in south latitudes, to the southward of it. 6. Why, in the seas of China, there should be so great an inclination from the east to the north, more than elsewhere.

To attempt a solution of the foregoing phænomena, it may be observed that wind is most properly defined to be the stream or current of the air; and where such current is perpetual and fixed in its course, it is necessary that it proceed from a permanent unintermitting cause. Therefore some have been inclined to propose the diurnal rotation of the earth on its axis; by which, as the globe turns eastwards, the loose and fluid particles of the air, being so exceedingly light, are left behind; so that, in respect of the earth's surface, they move westwards, and become a constant easterly wind. This opinion seems confirmed by these winds being found only near the equinoctial, in those parallels of latitude where the diurnal motion is swiftest; and I should readily assent to it, if the constant calms in the Atlantic sea near the equator, the westerly winds near the coast of Guinea, and the periodical westerly monsoons under the equator in the Indian seas, did not show the insufficiency of that hypothesis. Besides, the air being kept to the earth by the principle of gravity, would acquire the same degree of velocity that the earth's surface moves with, as well in respect of the diurnal rotation, as of the annual about the sun, which is about 30 times swifter.

It remains therefore to substitute some other cause, capable of producing a like constant effect, not liable to the same objections, but agreeable to the known properties of the elements of air and water, and the laws of the motion of fluid bodies. And such seems to be the action of the sun beams on the air and water, as he passes every day over the ocean, considered together with the nature of the soil, and situation of the adjoining continents. First then, according to the laws of statics, the air, which is less rarefied or expanded by heat, and consequently more ponderous, must have a motion towards those parts, where it is more rarefied and less ponderous, to bring it to an equilibrium; and secondly, the presence of the sun continually shifting to the westward, that part towards which the air tends, by reason of the rarefaction made by his greatest meridian heat, is with him carried westward, and consequently the tendency of the whole body of the lower air is that way. Thus a general easterly wind is formed, which being impressed on all the air of a vast ocean, the parts impel one another, and so keep moving till the next return of the sun, by which so much of the motion as was lost is again restored, and thus the easterly wind is made perpetual.

From the same principle it follows, that this easterly wind should, on the north side of the equator, be to the northward of the east, and in south latitudes to the southward of it; for near the line the air is much more rarefied than at a greater distance from it; because the sun is twice a year verticle, and at no time distant above 23° and a half; at which distance, the heat, being as the sign of the angle of incidence, is but little short of that of the perpendicular ray. Whereas under the tropics, though the sun continue long vertical, yet he is as long 47° off; which is a kind of winter, when the air is so cooled, that the summer heat cannot warm it to the same degree with that under the equator. Therefore the air to the northward and southward, being less rarefied than that in the middle, it follows, that from both sides it ought to tend towards the equator: this motion, compounded with the former easterly wind, answers all the phænomena of the general trade winds, which, if the whole surface of the globe were sea, would undoubtedly blow all round the world, as they are found to do in the Atlantic and Ethiopic oceans.

But as such great continents interpose and break the continuity of the oceans, regard must be had to the nature of the soil, and the position of the high mountains, which I suppose the two principal causes of the several variations of the winds, from the former general rule: for if a country lying near the sun prove to be flat, sandy, low land, such as the desarts of Lybia are reported to be, the heat occasioned by the reflection of the sun beams, and its retention in the sand, is incredible to those that have not felt it; by which the air being exceedingly rarefied, it is necessary that the cooler and more dense air should run thither to restore the equilibrium. This I take to be the cause, why near the coast of Guinea the wind always sets in upon the land, blowing westerly instead of easterly, there being sufficient reason to believe that the inland parts of Africa are prodigiously hot, since its northern borders were so intemperate, as to give the ancients cause to conclude that all beyond the tropic was uninhabitable by excess of heat. From the same cause it happens, that there are such constant calms in that part of the ocean called the Rains: for this tract being placed in the middle, between the westerly winds blowing on the coast of Guinea, and the easterly trade-winds, blowing to the westward of it, the tendency of the air here is indifferent to either, and so stands in equilibrio between both; and the weight of the incumbent atmosphere being diminished by the continual contrary winds blowing from hence, is the reason that the air here holds not the copious vapours it receives, but lets them fall in so frequent rains.

But as the cool and dense air, by reason of its greater gravity, presses on the hot and rarefied, it is demonstrable, that this latter must ascend in a continued stream, as fast as it rarefies, and that being ascended, it must disperse itself to preserve the equilibrium; that is, by a contrary current, the upper air must move from those parts where the heat is greatest: so by a kind of circulation, the north-east trade wind below will be attended with a south-westerly above, and the south-easterly with a north-west wind above. And that this is more than a bare conjecture, the almost instantaneous change of the wind to the opposite point, which is frequently found in passing the limits of the trade winds, seem to assure us; but that which above all confirms this hypothesis, is the phenomenon of the monsoons, by this means most easily solved, and without it hardly explicable.

Supposing therefore such a circulation as above, it is to be considered that to the northward of the Indian ocean there is every where land within the usual limits of the latitude of 30°, viz. Arabia, Persia, India, &c. which for the same reason as the midland parts of Africa, are subject to excessive heats when the sun is to the north, passing nearly vertical; but yet are temperate enough when the sun is removed towards the other tropic; because of a ridge of mountains at some distance within the land, said to be frequently in winter covered with snow, over which the air, as it passes, must needs be much chilled. Hence it comes to pass, that the air, coming according to the general rule out of the north-east in the Indian seas, is sometimes hotter, sometimes colder, than that which by this circulation is returned out of the south-west, and by consequence, sometimes the under current or wind is from the north-east, sometimes from the south-west.

That this has no other cause, is clear from the times when these winds set in, viz. in April, when the sun begins to warm those countries to the north, the south west monsoon begins, and blows during the heats till October; when the sun being retired, and all things growing cooler northward, and the heat increasing to the south, the north-east winds enter, and blow all the winter till April again.

And it is undoubtedly from the same principle, that to the south of the equator, in a part of the Indian ocean, the north-west winds succeed the south-east, when the sun draws near the tropic of Capricorn. But I must confess, that in this latter occurs a difficulty, not well to be accounted for, which is, why this change of the monsoons should be any more in this ocean, than in the same latitudes in the Ethiopic, where there is nothing more certain than a south-east wind all the year.

It is likewise very hard to conceive why the limits of the trade wind should be fixed about the 30° of latitude all round the globe; and that they should so seldom exceed or fall short of those bounds; as also that in the Indian sea, only the northern part should be subject to the changeable monsoons, and that in the southern there should be a constant south-east wind.

A Dioptric Problem, why four Convex-glasses in a Telescope show Objects erect. By William Molyneux, of Dublin, Esq. R. S. Soc. N° 183, p. 169.

In the Journal des Sçavans for Sept. 17, 1685, we find this passage: "As perspectives of one convex glass make objects appear upright, which those of two convex-glasses invert, and again those of three rectify; so it should seem that those of four ought to invert: and yet experience shows us that objects appear upright through these glasses. The singularity of this phenomenon obliges all skilled in dioptrics to inquire the reason of it, but hitherto they have found none. Mr. Regis, who applies himself particularly to this part of Natural Philosophy, believes that he has hit upon the reason, and makes us hope that he will suddenly publish it."

To me this phenomenon appears very easily explicable, from the consideration of placing glasses in a tube; which is thus: after the object-glass, the eye-glass is placed so much distant, towards the eye, from the focus of the object-glass, as is the focus of the eye-glass; then the middle eye-glass is placed at such a distance from the focus of the first eye-glass, as is the focus of this middle eye-glass; lastly, the nearest eye-glass is placed so much distant from the focus of this middle eye-glass, as is the focus of this nearest eye-glass; and the eye, looking through them all, is placed in the focus of this nearest eye-glass. Therefore, first, one single convex-glass cannot properly be said by itself to show objects erect or reverse, but in respect of placing of the eye that looks through it. For if the eye be placed nearer to it than the focus of the glass, the objects are erect; if the eye be placed just in the focus, the objects are neither erect nor reversed, but all in confusion, and between both; and if the eye be placed further from the glass than the focus, the objects are reversed. I mean here by distant objects, the rays flowing from any point of which may be counted to come parallel towards the object-glass.

Secondly, the object-glass of a telescope reverses the object, both to the eye-glass and the eye that looks through it: for the eye-glass is placed farther from the object-glass than is the focus of the object-glass. But the eye-glass contributes nothing towards the rectification or reversion, the eye being placed just in its focus. Thus we see that the reversion of objects, in a telescope of two convex-glasses, proceeds wholly from the object-glass and its position; and the eye-glass has no concern in the effect; for were the eye itself in the place of the eye-glass, it would see the objects inverted through the single object-glass.

To come now to consider the second eye-glass placed after the first, which is that next the object-glass: it is here manifest, that if we place our eye nearer to this middle eye-glass than its focus, the eye sees the object inverted and confused: place the eye in the focus, it sees the objects all in confusion, neither erect nor reversed; for here again there is a distinct representation of the objects to be received on a piece of paper, as in the focus of the object-glass; and the eye, being situated at any time at this place, which is usually called the distinct-base, sees all in confusion. But let the eye be placed farther from this middle glass than its focus, then it perceives the objects erect and confused.

Lastly, the third or immediate eye-glass adds nothing towards the erecting or reversing the species, which it receives erect from the middle eye-glass; no more than, in a telescope of two convex-glasses, the eye-glass adds to the species it receives from the object-glass, as was shown before. The reason why this last, or immediate eye-glass, has no concern in erecting or reversing the species is the same, as in a telescope of two convex-glasses, viz. that the eye is placed in its focus, and therefore sees the species as it is represented in the distinct-base; that is, the species is inverted in the distinct-base of the object-glass, and therefore a single convex eye-glass brings it to the eye inverted; but in the distinct-base of the middle, or second eye-glass, the species is erect, and therefore the third or immediate eye-glass brings it to the eye erect.

Wherefore we are to consider the telescope, consisting of an object-glass and three eye-glasses, as two telescopes, each consisting of two convex-glasses. The first consists of the object-glass, and first eye-glass, and this inverts the species; that is, the species is inverted in the distinct-base of the object-glass, and so brought to the eye. The second telescope consists of the two immediate eye-glasses, and this erects what the former inverted, that is, the species in the distinct-base of the middle eye-glass is erect, and is so brought to the eye by the eye-glass; the eye-glasses themselves, in neither case, having any thing to do with the erecting or inverting, but merely in representing in the same posture the species immediately before them.

The French problem therefore should not have broken a telescope of four convex-glasses into four pieces, but into two, and then the case would have been plain; whereas by breaking it into four perspective-glasses, they attribute that to two of them, which neither of them effects, viz. inverting and erecting. Hence then, in general, one convex-glass, as posited in a telescope, inverts; the second, that is the first eye-glass, does nothing towards erecting or reversing, but represents the image as it is in the distinct-base of the object-glass before it, that is, inverted. The third glass erects, or rather restores what was before inverted. The fourth represents the image as it receives it from the distinct-base of the third, that is, erect.

An Inscription on the Basis of a Pillar, lately dug at Rome; with an Interpretation. By Dr. Vossius. N° 183, p. 172.

This inscription was sent by that excellent philosopher and mathematician Mr. Adrian Auzout, who copied it from the stone, to Mr. Justel, by whom it was communicated to the Royal Society, together with the sentiments of Dr. Vossius upon it.

Observations of the Eclipse of Jupiter by the Moon, March 31, 1686, *N. S.* N° 183, p. 175.

Some observations have already been given of the occultation, viz. in N° 181; and the same is farther explained and extended by these additional observations of the French and other continental astronomers, published in the Journal des Sçavans.

An Occultation of Jupiter by the Moon, An. 1686, *April* 10, *N. S. Observed at Dantzic by M. Hevelius.* N° 183, p. 178. *Translated from the Latin.*

Although for 56 years past I have not omitted any observation of importance, I have not been able properly to take and note down more than three of Jupiter's occultations. The first of these was An. 1646, Dec. 24 in the evening; when I could only observe the end of it. The second was An. 1679, June 5 in the morning; when every thing succeeded as well as possible. The third was this present year 1686, April 10 in the evening. Of this occultation, among other things, the following circumstances are chiefly remarkable, viz.

that it did not happen just at the full moon, but about a day after it: and, which is very extraordinary, being a coincidence that hardly ever happens, it occurred at the same point of time, and with the same appearance, as in the first occultation above-mentioned, of 1646; when the moon had decreased for 2 days, and doubtless exhibited the same libration also; for the section of light and shadow was just the same, and passed through the same spots, which I cannot sufficiently admire; that is, at the greater and less hyperborean lake, also at the Riphean mountains, through Palus Mæotis, by the great Caspian lake, and its lower bay to mount Nerosus. On the other hand, the second occultation, of 1679, was under different circumstances, as it happened not at the full, but near the new moon, being about 3 days before the conjunction. Of the present occultation, of 1686, the principal phænomina are the following: viz. At

11^h	7^m	9^s	the beginning, or external contact.
11	7	54	the central immersion.
11	8	39	internal contact, or complete immersion.
11	49	15	beginning of the emersion.
11	50	0	the central or half emersion.
11	50	45	the complete emersion.

In this occultation, it appears that Jupiter's path line behind the moon, was a chord of near 104°, in the north part of the moon, the diameter of which, measured by a micrometer, was 31′ just. From the observations, M. H. derived the diameter of Jupiter, viz. 51″ 42‴, and which then measured about 50″ by means of the lunar spots; whereas in the year 1679, when he observed a similar occultation, the diameter of Jupiter was only 30″ 53‴, which he prefers to the former measure, as, being made in the day time, the adventitious rays of the stars and planets were much dispelled by the sun's light.

Methodus Figurarum lineis rectis et curvis comprehensarum quadraturas determinandi. Authore J. Craige, 4to, Lond. 1685. N° 183, p. 185.

The great use of drawing the tangents of curve lines, has induced the modern mathematicians to endeavour to discover general methods of finding the tangents of curve lines; as may be seen from the several ways invented by Descartes, M. Fermat, Dr. Barrow, Dr. Wallis, Tschurnehuys, and Leibnitz. But no one has attempted to invert this problem generally, that is, having the tangent, to find the curve line whose tangent it is. Therefore the author of this treatise, perceiving that this would give a general method of determining the quadrature of any curvilinear space, has laid down a rule for inverting Slusius's method mentioned in the Philosophical Transactions, N° 90. He has illustrated his method of quadratures by several figures which have been already considered by geometricians. As for the circle and hyperbola, he asserts that their indefinite quadratures are impossible; and therefore, in these and such like cases, he expresses the area by an infinite series, which is easily done by his method, except the series consist of irrational terms; for in these he has recourse to Leibnitz's method of finding tangents, where the calculation will be more tedious. By resolving the area of the hyperbola into an infinite series, he arrives at the same expression with that of N. Mercator: and in measuring the zone of a circle, his expression coincides with that invented by Mr. Isaac Newton, as Mr. David Gregory relates in his treatise. He has subjoined a method of measuring the curve superficies made by the rotation of any curve upon its axis; with a small animadversion on the method of quadratrices, published in the Acta Lipsiensia Eruditorum of October, 1683.

An Account of the Course of the Tides in the Port of Dublin, in Ireland. By William Molyneux, Esq. R. S. S. With a remark upon it. N° 184, p. 192.

At the bar of Dublin, on the new and full moons, a S.S.E. moon makes high water, that is, at half an hour after 10. At Ring's-End, at three quarters after 10.—At the Custom-house at Dublin, at 11.

On the quarter days, high water on the bar at 5 o'clock.—At Ring's-End, at a quarter past 5.—At the Custom-house, half an hour past 5. A southerly wind between S.S.E. and S.S.W. blowing fresh, makes it flow near half an hour longer than its usual course.

Note. This observation makes the tides, on the quarter moons, come in later, in respect of the moon's southing, than on new and full moons, by half an hour: whereas in the River Thames, as high as London, the quarter moons make high water above an hour and quarter sooner in that respect, than the new and full; as may be seen by the accurate tide-tables of Mr. Flamsteed: but it is from hence evident that the same tables are not applicable to the sea-ports, where there is not the same reason for the anticipation of the neap tides on the quarter moons. The cause of this phenomenon seems to be, that the impulse of the ocean in the quarter moons is not so vigorous as in the new and full; nor the motion of the waters so quick: hence it happens, that in the open sea, and in ports on the sea-coast, as this of Dublin, the high water is later than when the motion is more rapid in the new and full; but on the contrary, in rivers, at any considerable distance from the sea, the resistance of the weight of the fresh water, which is kept suspended during the time of the flood, is longer overcome by the more potent impetus in the new and full, than by the weaker in the quadratures: and from hence this difference should be still more and more considerable, as the port is farther removed from the sea.

A Demonstration of the Velocity with which the Air rushes into an exhausted Receiver. By Dr. D. Papin, Reg. Soc. S. N° 184, p. 193.

There being several occasions wherein it would be useful to know the velocity of the air, according to the several pressures that may urge it, the Royal Academy at Paris has attempted it by experiment, viz. by means of a bladder, sometimes filled up with water, and sometimes with air, they found that, although the weight to squeeze out these fluids, and the hole to discharge them were the same, yet the bladder, when full of air, could be emptied in the 25th part of the time that was required for emptying the water; whence they concluded, that the velocity of the air is 25 times greater than that of water, when both fluids suffer the same pressure. This experiment was very ingenious, but not quite perfect. For the air yields much, and so the bladder, filled with it, will become pretty flat as soon as a considerable weight is laid upon it. It is plain therefore, that the weight, bearing on a large surface does not press every part with the same force, as it would do if the bladder did for a while remain distended, as it does when full of water; besides, the water itself, being heavy in the bladder, makes some pressure: so that it appears, that the pressure in this experiment was not quite so great upon the air as on the water. I have therefore thought of another way, which I think better, founded on this hydrostatical principle, that liquors have a power to ascend as high as their source, and although the resistance of the medium always hinders jets d'eau, in the open air, from reaching quite so high, yet the liquor at its first spouting out, has the necessary velocity to carry it to that height.

Prop. 1.—From this principle may easily be deduced this proposition, viz. that of two different liquors, urged by the same pressure, that which is specifically lighter must ascend higher than that which is heavier, and their heights will be reciprocally in the same ratio as their specific gravities. Thus quicksilver being $13\frac{1}{2}$ times heavier than water, bears as much pressure when its spring is one foot above the hole, as water does when it is $13\frac{1}{2}$ feet high, and the height to which mercury shall ascend, will be $13\frac{1}{2}$ times less than the height to which water shall be driven by those equal pressures.

Prop. 2.—From the foregoing proposition another may easily be deduced, viz. that of different liquors under the same pressure, those specifically lighter must acquire a greater celerity, and their different velocities be to one another, as the roots of the specific gravities of the said liquors. For, by prop. 1, the heights to be attained, are reciprocally in the same ratio as the specific gravities: now Galileo, Huygens, and others have demonstrated, that the velocities of bodies are to each other, as the square roots of the heights to which they may ascend: and so in this case they are also reciprocally as the roots of the specific gravities.

If therefore we would know what is the velocity of air when driven by any degree of pressure whatever, we need only find what would be the velocity of water under the same pressure, and then take the square roots of the specific gravities of these two fluids, because, as much as the square root of the specific gravity of water exceeds the square root of the specific gravity of air, so much in proportion will the velocity of air exceed the velocity of water. For example, when I would compute what would be the velocity of a bullet shot by the pneumatic engine, described in Philosophical Transactions, N° 179, I should first compute what was the velocity of the air itself that drove the bullet: I therefore observe that on this occasion the air sustains a pressure much about the same as that of water when its head is 32 * feet high; now such water would spout out with a sufficient velocity to ascend 32 feet perpendicular, and therefore it has the velocity of 45 feet in a second. It remains therefore only to know the proportion of the gravity of air to that of water: this we have found not to be always the same; because the height, the heat, and the moisture of the atmosphere are variable; yet we may say in general, that the ratio of the specific gravities of water and air is about 840 to 1. Taking then their square roots, which are 29 and 1, we may conclude that the velocity of air must exceed that of water by 29 times; and so multiplying 45, the velocity of water, by 29, we shall find that the velocity of the air, driven by the whole pressure of the atmosphere, is about 1305 feet in a second.

Extract of a Letter from Mr. J. Flamsteed, Astr. Reg. and Reg. Soc. S. giving his Calculation of the Eclipses of Jupiter's Satellites for the Year 1687. With a Table of the Parallaxes of the Orbit, and an Ephemeris of Jupiter's Geocentric Place for the same Year. To which is added an Observation of the Eclipse of the Moon, Nov. 30, 1685, made at Lisbon, and Mr. Flamsteed's own Observation of the Eclipse of Jupiter by the Moon, on March 31, 1686. N° 184, p. 196.

The calculated times of the eclipses and of the planet's places cannot be of any use now. But the observations of the lunar eclipse and of Jupiter's occultation by the moon are as follow:—

* This number is too small, as the mean pressure of the atmosphere is now known to be about that of a column of $33\frac{3}{4}$ feet of water, and this will give for its velocity $46\frac{1}{2}\frac{9}{7}$ feet, instead of 45; which multiplied by the 29 gives near 1348 feet, for the more correct velocity with which air will rush into a vacuum.

Lunar Eclipse observed at Lisbon, Nov. 30, 1686. By Mr. Henry Jacobs.

	h.	m.
The beginning	8	2
Immersion	9	6
Emersion	10	50
The end	11	57

The Observation of Jupiter's Occultation by the Moon, March 31, 1686; *made by Mr. Flamsteed.*

At 9	32	30	.. the first contact of the limbs,
9	33	42	.. the complete immersion,
10	30	30	.. beginning of the emersion,
10	31	36	.. end, or complete emersion.

Account of the Natural History of Staffordshire. By Robert Plot, LL. D. Keeper of the Ashmolean Museum, and Professor of Chemistry in the University of Oxford. N° 184, p. 207.

There is very little or no need to take notice of the method of this work, since it is drawn up and conducted after the same manner as the Natural History of Oxfordshire, written some years before by the author, of which an account has been given in these Abridgments, vol. ii. p. 394.

Account of Sciotericum Telescopicum, or a new Contrivance of adapting a Telescope to a Horizontal Dial, for observing the Moment of Time by Day or Night. By Will. Molyneux, Esq. R.S.S. Dublin, 1686, *in* 4to. N° 184, p. 213.

The author first declares the use and advantage of this new contrivance, which he conceives to be very great, especially when the observation of the exact moment of time is so necessary, that neither geography, navigation, or astronomy, can be brought to perfection, nor the longitude or the truth of astronomical tables fully discovered. The methods which commonly are used for observing the moment of time, are either by dials, or by taking the sun's altitude by day, or that of stars by night; or by observing the altitude and azimuth of the sun or stars, or by the transits of the sun or stars over the meridian, or the coming of some circumpolar stars to the same vertical with the pole star; all which methods are attended with many difficulties, which the author thinks his way will avoid; at least the most material ones, which commonly arise in the practice. Here is no need of any calculation of oblique spherical triangles, all being done by a plain and simple observation, and by the addition and subtraction of two or three small numbers, and that to such exactness, that not a quarter of a minute or 15 seconds can be wanting, and performed both by day and night. The contrivance consists in making a very large horizontal dial, adapted to the latitude of the place where the observation is to be made, capable of receiving divisions into minutes, and parts of a minute, fitted with a large, strong, and double gnomon. This dial is furnished with 2 pair of sights, one to serve in the morning, or for stars on the eastern side of the meridian; the other to serve in the afternoon, or for stars on the western side of the meridian. Each of these consists of 2 moveable rulers: one called the horizontal ruler, the other the gnomonic or stile ruler. These 2 rulers are so adapted, that their 2 edges, which are next to the gnomon, may be perpetually in the same plane with their correspondent edge of the gnomon. On the stile ruler are set telescopic sights, with cross-hairs in their due place. As to the manner of observing the time after the dial is justly levelled and stated; this is to be done by looking at the sun through the telescopic or stile ruler, and bringing the mensurator on the sun's centre; then the horizontal ruler will cut the hour, minute, and part of a minute most exactly in the dial. By the same telescopic sight, the motion of the sun will be perceived so quick and exact, that 2 beats of a second pendulum may be determined, and the time of the day or night to 3, 5, or 7 seconds discerned. The way of using this dial on the stars by night is much the same, only that for these are requisite certain tables of the sun and stars' temporary right ascensions; for in looking at a star through the telescopic ruler, the horizontal ruler cuts the star's horary distance from the meridian, to which adding the star's right ascension, and from the sum subtracting the sun's right ascension, the remainder gives the hour, minute, and second of the night.

A Table showing the Time of High Water on the Coasts, and in the Ports of France, on the Day of the new and full Moon. Taken from the French Ephemerides called La Connoissance des Temps, for the Year 1687. N° 185, p. 220.

The times of high water on the coasts are now much more accurately set down in all treatises on navigation, and other books.

The verbal Process on the Discovery of an Ancient Sepulchre, in the Village of Cocherel on the River Eure in France. Communicated by Mr. Justell, R.S.S. N° 185, p. 221.

This ancient sepulchre was discovered in 1685 by digging for stone. It was covered and inclosed by some large stones. In it were found the bones of about

20 bodies of men, of the ordinary stature, between $5\frac{1}{2}$ feet and 6 feet tall, except two youths of about 15 or 16 years of age. All these bodies lay extended north and south, the arms stretched down close by the bodies, and the heads all placed along two stones. In an angle were 2 bodies, separated from 2 others by a stone, of about a foot thick, 4 feet broad, and $5\frac{1}{2}$ feet long, which lay in the manner of a tomb-stone on the 2 bodies underneath. All these heads had very fair sound teeth, and the cranium and other bones of the head were much stronger and thicker than those of ordinary heads; which argues them to have been of strong well constitutioned men: amongst them all there was not any woman's head.

In examining the sepulchre further, it was observed, that at the same distance from the superficies of the earth, and from those bodies thus buried, there were 3 little earthen pots, of about 4 inches diameter, and between 4 and 5 inches high, of a black earth, as soft as wax; which could not be separated from the other earth without breaking them, and the pieces being brought into the open air, turned of a greyish colour, and became hard: these pots were full of wood, coals, and ashes.

In the place where lay the two heads of the bodies on the tomb-stone there were found two stones; the one about 6 inches long, and 15 lines broad in its broadest part, and about 4 lines thick, framed like the head of a pike, very sharp and cutting at both ends, and on the sides; it was a yellow flint, of which the best firelock stones are made, being almost as hard as an agate. The other stone, which was likewise under one of these heads, was shaped like the head of an axe, about 4 inches long, and 3 inches broad, having a hole at the narrowest end, and about 6 lines thick, very sharp and of a greenish stone, spotted with white spots, also as hard as agate; the French lapidaries call it pierre de jade, or the nephretic stone.

Under the two heads which were under the tomb-stone, there were also found two other stones; the one much of the same nature with that first described, but something longer, and the sharp end a little dulled. The other was likewise in the shape of an axe head, very sharp and cutting, of about 3 inches long, and 2 and a half broad, and 6 lines thick, with a hole in it at the narrow end: the stone was of a dark green colour, and by the lapidaries called oriental serpentine.

On the left side of the sepulchre which was open were 16 bodies, in the same situation as the first, placed north and south, their heads along by a great stone, and the arms extended along the bodies. The bones were all entire, though they appeared very ancient, and after 2 days in the air, fell all to dust. All the bones of these heads were very thick; there was one that had been pierced by some blow, and nature had repaired the wound; within, the hole was round, as having been made by some sharp round weapon, which argued likewise the wounded to have been a soldier. Under every one of these heads, there was a little stone.

There were also found, under these heads, three stones; two of which were of a grey pebble, such as are found by the sea side, shaped like axes' heads, sharp and polished, about 4 or 5 inches long, and 4 broad at the broadest end, about an inch and a half at the narrowest, and in the middle about an inch thick. These stones were, by their narrow end, to be put into a piece of stag's horn, fitted to receive them, as appeared by several pieces found in this sepulchre, which had an oval hollow at the end to receive one of these stones; these pieces were about 6 inches long, and had a hole at the other end, by which they might be fastened to a longer stick. The third stone was of the shape of the former, but of a black pebble, like a flint, with which this country abounds; and it was also remarked that the pieces of stag's horn were worn at the end, and polished on some stone, but not cut with iron. Under all the other heads there were 10 little stones, of black flint, one under each head, cut all in the same shape, smooth on one side and sharp on the other, which were probably used as knives. There was likewise found in the same place, under one of the heads, a stone, which within was of black flint, having the outside of a white substance, as that sort of stone used to be; this had two eminences like teeth, supposed to be natural. All these stones, thus placed under their heads, shewed that they were held in great esteem. Amongst these dead bodies has been also found some bones sharpened, to put at the end of a stick, or at the end of an arrow; one was of the smaller bone of a horse's leg, and the other was made of the sharp end of the andouilleres of a stag's horn. Among all these stones there has been found no sort of inscription, sculpture, or character, either in relievo or otherwise.

On further digging on the left side of the sepulchre, it has been discovered that its bottom was raised, and not so deep by a foot and a half as that part where the bodies were placed. And it is perceivable, that in this place several bodies have been burnt, whose ashes and burnt bones have been thrown confusedly into this hole: and it is observable that all along the sepulchre, there is a vein of coal or ashes, which runs about two feet below the superficies of the earth, and all these ashes and bones are under this bed of coals and ashes, which are so salt and pungent, that they cause a sneezing; and when the bones are handled, they produce a tingling in the fingers' ends, as if by handling the sharpest salt-petre.

There has not been time yet to finish entirely this discovery, by digging into the earth that has been put into this sepulchre, which was dug on purpose in the chalk to bury these bodies, and likewise the ashes and bones of those others that were burnt. So that it seems difficult to reconcile the two ceremonies of burying and burning; unless perhaps there has been a battle in this place, between the Gauls and some barbarous nation, who had invaded them; that the Gauls have burned their dead, and sacrificed to their manes the prisoners taken in war, whom they buried with the ceremonies proper to those barbarians. The thickness of whose sculls shows that they went bare-headed; and their arms show that they had not the use either of iron or brass, to make arms of; but using such as nature afforded first, as some Indian nations do now.

Extract of a Letter from Rome, dated the 16th of Nov. last, to Signior Sarotti, concerning a Discovery made by the Inundation of the Tevere. Translated out of the Italian. N° 185, p. 227.

The inundation of the river has done considerable damage, all about this city, spoiling several fine houses, and large aqueducts, by breaking down their conduits, &c. In several places by breaking the ground, it has discovered vaults unknown before; many of them full of earthen urns, and sepulchres; and in a place, within two miles of this city, where there was some large ancient ruins, the water having pierced a strong thick wall, joining to a great country palace, and passing under the same, broke out at a corner of an aqueduct by the said house, where there was found a small vault of an oval figure, in which was a pretty large stone sepulchre, with the following inscription, P.M. R.C. cum uxore, and some more sculpture not legible. Near it was a large earthen urn, shut up very close; which being opened there came out such a strong smoke, as made the person near it giddy; the smell was like bitumen, but being quickly dispersed, there was found in the bottom of the urn, an earthen pot, made up as a lamp, full of a materia oleosa, which by degrees, as the cold air entered, grew hard.

Account of a petrified Glandula Pinealis, lately found in the Dissection of a Brain; communicated by Sir Edmond King, Knt. M.D. F.R.S. N° 185, p. 228.

Mr. Robert Bacon, master of arts, of Corpus Christi College in Oxford, above 75 years of age, formerly employed in transcribing and publishing the posthumous works of Dr. Robert Gell, and who had been a preacher at Bustleton, near Bristol, and afterwards at Windsor; was of a sanguine habit of body, and of a cheerful temper.

About 12 years since he was observed to bend double to his right side, in walking; yet he was always temperate.—He was often troubled with a dread of insanity. In his latter years his appetite inclined to canine, and he had great thirst.—He often complained of pain in his bowels.—Was fond of having his head rubbed.—Of late years, he voided his urine and stools involuntarily. Hung his head, which was very hot, in a prone sleeping posture. He sweat much every night.—For a long time before his death, he appeared to be in a state of fatuity.

He died of a fever, Nov. 4, 1686. On opening the body, the liver was found pretty well coloured and firm:—the spleen firm and good, but shrivelled;—the stomach firm, large, and strong;—the intestines all well coloured;—the omentum whole, but ill coloured;—the pancreas very firm and good;—the mesentery well enough;—the right kidney sound, with a few small stones;—the left kidney two parts of three wasted, with some coarse gravel, but both kidneys very fat; —the gall-bladder filled with one stone only, and that no larger than a long nutmeg;—the urinary-bladder sound, but some little coarse gravel and small stones in it;—the middle venter being opened, the lungs were well enough, only by the stagnation of blood discoloured, and filled in several places with ichorous spumy matter;—the heart strong and vigorous;—the pericardium very thin, and too tender, and too little water in it; very little blood in the ventricles; no adhesion of the lungs to his ribs; the auricles of his heart perfectly sound and strong.

On opening the head, the dura mater appeared extremely hard, thin and white, a slender embroidery of vessels;—the pia mater all full of seeming turgid glands, and a great distention of lymphæ-ducts, full of coagulated lympha;—the substance of the brain loose and shrunk, very white, very little of the cineritious colour to be seen;—the corpus callosum very flaccid, more than ordinary;—the whole body of the brain was shrunk about a third part;—between the two meninges of the brain, was near a pint of extravasated serum, which must needs oppress the brain very much:—the ventricles of the brain full of serum; —the plexus choroides extremely large, in length as well as breadth and thickness;—the nates and testes very small, and shrunk;—the thalami nervorum opticorum plump and fair;—the corpora striata large and fair, being full of large striæ;—the glandula pinealis firm and fair, well coloured, of the exact figure, and ordinary size: it felt harder than ordinary; and in it was found a stone in a film, or rather a petrified gland in a film; which stone was taken out and kept as a great rarity;—the glandula pituitaria was half wasted; the part left was very hard and brittle, not having the tone of a true gland, nor substance, unless of a vitiated gland;—the cerebellum seemed well enough, and all down the cauda medullæ oblongatæ;—the other parts of the brain had nothing remarkable.

An Eclipse of the Moon, Nov. 19, 1686; *observed at Dublin, by Wm. Molyneux, Esq. F. R. S.* N° 185, p. 236.

A sensible penumbra at	9^h	15^m
Beginning of the eclipse about . . .	9	27
End of the eclipse	12	4

The times were taken by a pendulum clock, corrected by the fixed stars. The quantity eclipsed was about 6 digits.

A further Assertion of the Propositions concerning the Magnitude, &c. of London, contained in two Essays on Political Arithmetic, mentioned in Philos. Trans. Numb. 183; *with a Vindication of the said Essays from the Objections of some learned Persons of the French Nation. By Sir W. Petty, Knt. R. S. S.* N° 184, p. 237.

The ingenuous author of the Novelles de la Republique des Lettres, says, that Rey in Persia is far larger than London; because in the 6th century of christianity, I suppose an. 550, it had 15,000, or rather 40,000 Mosques or Mahometan Temples. I hope this objector is only in jest, for Mahomet was not born till about the year 570, and had no Mosques till about 50 years after.

The excellent Auzout, from Rome, is content, that London, Westminster, and Southwark, with the contiguous houses, may have as many people as Paris and its suburbs; and but faintly denies, that all the houses within the bills may have almost as many people as Paris and Rouen; but adds, that several parishes inserted in these bills, are distant from, and not contiguous to London, and that Grant so understood it. To which it may be answered, 1. That the London bills appear in Grant's book to have been, since the year 1636, as they now are. 2. That about 50 years since 3 or 4 parishes, formerly distant, were joined by interposed buildings, to the bulk of the city, and therefore then inserted in the bills. 3. That since 50 years the whole buildings, being more than double, have perfected that union, so as there is no house within the said bills, from which one may not call to some other house. 4. That there are but three parishes under any colour of this exception, which are scarcely a two and fiftieth part of the whole.

M. Auzout alleges, that there are 23,223 houses in Paris, containing above 80,000 families. But, supposing $3\frac{1}{2}$ families to live in each of the said houses, one with another, the number of families will be 81,280; and Mons. Auzout allowing 6 heads to each family, the utmost number of people in Paris, according to Mr. Auzout's opinion, will be 487,680.

The medium of the Paris burials was allowed by M. Auzout to be 19,887, and that there died 3506 unnecessarily out of L'Hotel Dieu, wherefore deducting the said last number, the neat standard for burials at Paris, will be 16,381: so that the number of people there, allowing but one to die out of 30, (which is more advantageous to Paris than M. Auzout's opinion of one to die out of 25) the number of people at Paris will be 491,430; more than by M. Auzout's last mentioned accompt.

The medium of the said two Paris accompts 488,055.

The medium of the London burials is 23,212, which multiplied by 30, the number of the people there will be 696,360.

The number of houses in London appears by the register to be 105,315. To this adding a 10th part, or 10,531, as the least number of double families that can be supposed in London, the total of families will be 115,840: and allowing 6 heads for each family, as was done for Paris, the total of the people in London will be 695,076.

The medium of the last 2 London accounts is 695,718.

The people of Paris according to the above account is 488,055.

Of Rouen according to M. Auzout's utmost demand, 80,000.

Of Rome, according to his own report, 125,000.

The sum of these three is 693,055.

So that there are more people in London, than in Paris, Rouen, and Rome, by 2663.

Memorandum, that the parishes of Islington, Newington, and Hackney, for which only there is any colour of non-contiguity, is not a 52d part of what is contained in the bills of mortality; and consequently London without them, has more people than Paris and Rouen put together, by 114,284.

Description of an Invention, by which the Divisions of the Barometer may be enlarged in any given proportion. By Mr. Robert Hook, R. S. S. and Profess. Geom. Gresham Col. N° 185, p. 241.

Since the discovery of the alterations in the weight of the atmosphere, by means of the Torricellian tube, there has been several contrivances devised, to render the more minute variations, in the pressure of the air, sensible.

And first, the wheel-barometer, invented and published by Mr. Hook, An. 1665, in his micrography: but this did not fully answer the designed exactness, both because the mercury being apt to adhere to the sides of the glass, would rise and fall by jerks, or all at once; and because it is very difficult to adjust the apparatus of this instrument; as also that it is very apt to be out of order, on which account it is at present almost wholly laid aside.

Upon this, in June 1668, he bethought himself of another device, which was to increase the divisions, by putting coloured spirit of wine, or some other liquor not liable to freeze on the mercury, which liquor was made to rise as the mercury fell, and fall as it rose, in a narrow tube, so as to make the utmost limits about two feet asunder. Mr. Hook, however, was not yet satisfied, till he had found out the means of increasing the divisions of the barometer at pleasure, which was done in the following manner.

Fig. 12, pl. 9, represents the glass of this baroscope: the cylinder A may be of any diameter you please, the larger the better; but it need not be above 2 inches long. The tube AB must be so long, that the upper part of the cylinder B may be 29 inches + such a part of the height of the other tube B C, as the weight or specific gravity of the liquor which is to fill that tube, is to the specific gravity of mercury, below the line a b in the cylinder A. The third cylinder C may be as high as you please above the cylinder B, but is most conveniently made so that the square of the diameter of the tube B C, be to the square of the diameter of the cylinder's B or C, which must be exactly equal, as the rise of the mercury in the cylinder B, is to the whole length of the tube B C: for in this case there will be nothing superfluous, but the divisions enlarged to the utmost advantage.

One way of filling this barometer, is to leave a small hole at the top of the cylinder A, and another near the top of the cylinder B: this latter being well stopped, pour in as much mercury at the other hole in A, as shall fill both tubes as high as the level of the said hole; which done, stop, either by hermetically sealing it, or else by a drop of sealing-wax, the hole A; then opening the hole in B, draw off as much of the mercury of the tube B C, till it will run no longer; which done, stop firmly the hole in B, and you will have the cylinder A evacuated of air; and the height of the mercury will be as is usual in the ordinary plain and wheel-barometers. Then pour into the tube B C as much spirit of wine tinged with cochineal and oil of turpentine, equal parts of each, as shall stand above the surface of the mercury, so many feet as you make the enlarged scale of your barometer, or as is between the middle of the cylinders B and C; and you will find the mercury sink in the tube B C, and rise in the other tube A B, in such proportion, that each 13 feet of oil and spirit will raise the mercury 10 inches: this done, you must pour on, by the tube B C, as much mercury as may fill up the cylinders A and B to such heights, that the surface of the mercury in both, may, at the utmost limits, (which have not in England been found to exceed 30.6 and 28.6 inches) always fall within the bodies of the cylinders, and never enter into the tubes.

The effect of this baroscope will be, that when the atmosphere is heavy, and the mercury raised high in the cylinder A, and retired out of B, the spirit of wine will descend into the cylinder B, and the oil of turpentine will fill the tube, so as to make the partition of the two liquors near the cylinder B. But, on the contrary, when the air is light, the mercury will sink in A and rise in B, so as to drive the spirit of wine into the tube, and the oil of turpentine into the cylinder C, so that the section of the 2 liquors will be near C, and the variation of the height of the mercury will be enlarged into almost the length of the tube, without having the counter-pressure from the fluids in the least altered; the height and weight of the incumbent cylinders being always the same.

The small alteration that may happen by the dilatation and contraction of the spirit of wine by heat and cold, which ought to be accounted for, may be best discovered by a thermometer hanging by it, containing the same quantity of spirit of wine, and whose tube is, as near as possible, of the same diameter with the tube BC in the barometer, whose descent and ascent must be added and subtracted, to reduce it to a rigorous exactness; but it is still worth while to inquire if the mercury itself do not shrink and swell with cold and heat, so as not to need this correction.

Le grand et fameux Probleme de la Quadrature du Cercle resolu Geometriquement par le Cercle et la Ligne droite; par M. Mallement de Messange. A Paris, in 12°, 1686. *With a Refutation of the same, by M. D. Cluverius, Reg. S. S.* N° 185, p. 245.

This author is one of those unhappy geometricians, who without having acquired a thorough understanding of the principles, have yet thought themselves able to master the abstrusest difficulties in this nice mathematical science, where the least oversight or mistake subverts the whole superstructure. Hence it is, that the true quadrature of the circle here pretended to, is one of those vain attempts, which the less knowing and more opinionated of their own skill have produced, in this and the last century.

Voyage de Siam des Peres Jesuites, envoyez par le Roy, aux Indes et a la Chine. A Paris, 1686, 4to. N° 185, p. 249.

This is a second relation of the voyage and embassy of the French to the King of Siam, in the year 1685. The former was composed by Le Chevalier Chaumont, the ambassador, and now this by le Pere Tachart Jesuite, who was one of six fathers of his order, sent with the ambassador, as missionaries to China. These six were mathematicians, and by the king's letters patents were so stiled; their instructions being, besides their spiritual function, to prosecute the business of the Royal Academy of Paris, by accurately observing the curious things in art and nature, and particularly to make observations for discovering the longitudes of the places where they pass; for which purpose they are well provided with instruments. The variation of the needle observed by them at Siam, in 1685, was 30 minutes west. And the longitude of that place, deduced from an observed eclipse of the moon, is 98° 30′ east of Paris.

Account of a Comet seen at Leipsic, Sept. 1686, *taken from the Leipsic Acta Eruditorum for the Month of Nov.* 1686. N° 186, p. 256.

This comet was observed at Leipsic by Mr. Kirck; in whose ephemerides for this year there is likewise a brief account of it. He saw it only twice, viz. on the 8th and 9th of September, O. S. 1686; and observed it as follows. Sept. 8, 4h. morn. about day-break, he found the comet in the constellation of Leo, to the right hand of the Lucida in Lumbis ♌, and resembling that star in colour and magnitude, with a thin and short tail, extended upwards. Over the comet, in the same vertical, was the star θ ♌ of Bayer, or 21 Tychoni, exactly 1° distant from it, by the micrometer; and a line drawn from the Lucida in Lumbis ♌, to the comet, passed about half a degree to the right hand of the said θ Leonis. The distance of the comet from Regulus, taken by a radius, was about 17°. The next morning, Sept. 9, the comet appeared again obscurer, and more difficult to observe, than before, on account of the day-light: however, at 3h. 58m. its distance from θ ♌ was found by the micrometer 2° 23′$\frac{1}{2}$, and at 4h. 40m. again 2° 25′$\frac{3}{4}$. To verify the times, the altitude of the Lucida in Lumbis ♌ was observed 11° 10′ at 4h. 8m. morn. A right line drawn by the comet and the said θ Leonis, towards β Leonis, or the Lucida Colli, left that star a little to the right hand. The following days being cloudy, no more could be observed.

This comet was seen by a countryman, who first gave notice thereof, from the 6th to the 12th of September; the result of whose observations is, that the comet was direct in motion, that it moved about 1$\frac{1}{2}$ degree per diem, and that it seemed rather to decrease in latitude. On the 7th of Sept. it was about 24′ distant from θ Leonis.

This star θ Leonis, was then in 9° 2′ of ♍, with north latitude 9° 41′$\frac{1}{2}$. Whence at the time of the first observation it may be concluded that the comet was in 9° 55′ of ♍, with north latitude 9° 15′. And at the second observation the longitude of the comet will be found about 11° 20′ in ♍, with nearly the same north latitude as before.

A Method of casting Statues of an extraordinary Thinness. By Mr. John Weichard Valvasor, of Carniola. N° 186, p. 259.

First, I form out of good clay that will endure the fire, and not crack either in drying or burning, any figure or statue desired; when this is well dried I make all over the figure small holes of no great depth, but both in size and depth proportioned to the size of the statue; into these holes I put small pieces of metal, and with some of the same clay to fix them firmly in the holes; the use of these bits of metal, marked in fig. 1, pl. 10, *aaaaa*, is to keep the core and mould from touching each other, or falling together when the wax runs out; and that they may remain constantly in the same fixed posture. This done, I scrape away with some proper instrument as much of the clay in thickness as I design for the thickness of the statue, and then laying it in a furnace, the core, or first clay statue, is burned till it be red hot. When cold, the core is rubbed all over with that sort of earth or colour, used by German potters to colour the joints of the tiles when they set stoves of tiles. This colour resembles black-lead, and being mixed with water, it is daubed all over the core, because the metal is found to run freely upon it. This done, I lay upon the core as much yellow wax, mixed with pitch or rosin, as will make the thickness of the intended statue, which I form in the wax with all the exactness possible.

Next I put all over upon the surface of this statue of wax, little pieces of wax, which I call the little channels, *cccccc*, all of them contrived so as to enter into the great channels, *ddd*. This done, I cover the core and wax all over with the same sort of clay that will endure the fire without cracking, and thus the concave statue or mould is made. Upon this are laid the great channels *dddd*, both upright and transverse, formed likewise in wax, and placed according to judgment, so as best to receive the ends of the little channels, *ccccc*, for the easier distribution of the metal. The great channels must all meet at the top of the statue, so as to come out by one hole, at *e*, where the metal is to be poured in; it is also necessary to have a channel or two to let out the air as the metal enters, as those marked *ff*; and there must be a hole or two left at the foot, as *gg*, where the great channels and waxen statue join; and at which, when the mould is burnt, the wax both of the statue and of the channels may run out. The great channels being thus placed, the mould must be again laid over with the same sort of clay, and when this mould is well dried I heat the whole red hot.

The mould being thus burnt I stop with the same clay the two holes *gg*; and then bury it in a pit, and proceed as is usual in casting of bells and the like; but care must be taken that the metal be very well melted. If it be a small statue, not above a foot or two high, whose mould may be managed in the hands, then I make a concave statue of wax of the thickness desired, and place upon it all the great and small channels as before, which done, I put it all together into a liquid substance, made of plaister and tile or brick-dust tempered with water.

If the statue be intended very thin, I take copper, and when it is well melted I mix with it a good quantity of zink, the more, the better the metal runs. I have sometimes for small and thin statues put in above a third part of zink, and I have found by experience that this mineral makes the metal run more freely, and gives it a fair golden colour.

The statue being cast, I take off the mould, and cut off all the little channels; all which, both great and small, are filled with metal, which may be kept for further use; in these there is much more metal than in the whole statue; for if the statue be very thin, there must be more and larger channels; and so the cheaper the statue the more weighty the channels, and the more metal remains.

To know the quantity of metal requisite for my intended work, I take a lump of the same mixture of wax and pitch, with which I make the mould of my statue; and having weighed it I make a mould upon it, and cast in the same a lump of metal of the same size, which I weigh, and thereby compute the proportion of the weight of the metal and wax; then observing how many pounds of wax I use about the figure and channels, I can calculate nearly how much metal I should melt.

The Answer of Dr. Papin to several Objections made by Mr. Nuis against his Engine for raising Water by the Rarefaction of the Air, of which a Description is given in N° 178 *of these Transactions.* N° 186, p. 263.

Those objections were in the Nouvelles de la Republ. des Lettres of the month of December last.

In the first objection Mr. Nuis says, that it would be a very hard matter to hinder some receptacles from being filled too much. To this I answer, that it being necessary to let out the water of the highest receptacle, I thought it might be conceived that the water may also be let out of the inferior receptacles by inserting into each of them a crooked pipe, reaching a certain way downwards, and having its lower aperture shut up with a valve, by which the water might run out when the receptacle should be filled to a certain height.

The second difficulty lies in the great quantity of air to be rarefied. Mr. Nuis, by his computation, finds that the pumps should every one contain 84 cubic feet of rarefied air to raise water at 12000 feet distance. Let the distance, as he supposes, be 12000 feet, and the capacity of each receptacle about $\frac{1}{4}$ cubic foot; I might make the wheel with the axis to make their revolution in one minute of time, and so order all things that the air under the ascending plugs might come to be rarefied to such a degree, that by its elasticity it might not counterpoise more than 7 feet of water, but at the same time the air in the receptacles A A, B B, would even in its greatest dilatation be able to counterpoise 17 feet; so it is plain that the air will be driven from the receptacles into the pumps by a strength equivalent to 10 feet of water; now if we compute after the method published in the Transactions of the month of October last, what should be the velocity of the air driven by such a pressure, we shall find that the said velocity will be about 740 feet in a second; so that in $\frac{1}{2}$ minute, during which the plug ascends, this air might pass above 22000 feet, although it were not rarefied at all; but being rarefied, as we suppose it to be, it might go a great deal further.

In great distances, there should be made as many pumps as receptacles, as I had propounded in the first explication of my engine: and to raise water but 60 feet high, there should be required 13 or 14 receptacles, and as many pumps. Some people may take this for a great difficulty. But I answer that in this engine this is not so much as it seems at first; because the pressure being all from without, there is no need of any great strength to resist it, and so the metal for the pumps will cost but little.

As for the third objection, wherein Mr. Nuis says that it does not appear how the water in our engine may, by rarefaction, ascend higher than 32 feet. I answer, that the water does not at any time ascend higher than from a lower receptacle into the next upper one, which is but 12 feet high: so that it is plain the pressure of the air may be sufficient to force it up. It is indifferent whether it be by rarefaction, or otherwise, that the water comes into the receptacle; it is enough that the water is there, and that the air presses upon it with such a strength, as will prevail against all that opposes it, as I have shown above.

To the fourth difficulty I answer: that although the use of the pipes be merely for the conveying of air, they may nevertheless easily be filled with water when need requires, and so the defects in them may as well be found out as in the pipes that are used for the conveying of water.

An Answer of the same to the Author of the perpetual Motion. N° 186, p. 267.

I find, in the Nouvelles de la Republ. for December last, that the author still persists to urge some new contrivances, which being added, he conceives his engine must succeed. To this I answer, that I undertook only to show that his first device would fail; which I should scarcely have done, if I had thought a dispute of this nature could have lasted so long. To come therefore to the point, where he says that this engine may well succeed without alteration, because he has tried with liquors put into bellows immersed in water: I again say that I grant him the truth of the experiments, but deny the consequences he would draw from them. I have already given the reasons of my dissent, which this gentleman is not pleased to understand.

An Occultation of Saturn by the Moon, March 19, 1687, *observed at Totteridge, near London, by Mr. Edw. Haines, F.R.S.* N° 186, p. 268.

At 1^h 18^m 0^s The moon's limb touched the western ansa of Saturn.
1 18 30 Immersion of Saturn's centre a little below Palus Maræotis.
1 19 0 Saturn is now quite hid.

On the Measure of the Air's Resistance to Bodies moved in it. By John Wallis, S.T.D. et R.S.S. N° 186, p. 269.

1. In order to compute the air's resistance to all projectiles, this lemma is to be premised, viz. that, supposing other things equal, the resistance is proportional to the celerity. For in a double celerity, there is to be removed, in the same time, twice as much air, which is a double impediment; in a triple time, thrice as much; and so in other proportions.*

2. Suppose then the force impressed, and consequently the celerity, if there were no resistance, as 1; the resistance as r; and therefore the effective force, at the first moment, is to be reputed as $1 - r$; that is, so much as the force impressed exceeds the impediment or resistance.—3. Let it be, as $1-r$ to 1,

* This first postulate, being very erroneous, must vitiate the whole future calculation, the resistance of a medium being in a much higher proportion than simply that of the celerities. Though this might not be generally known at the time, when this article was written by Dr. Wallis; it has however been known, since the first publication of Newton's Principia, that the ratio of the resistances must be at least that of the squares of the velocities; and the reason is manifest, viz. that in the same time there is struck or removed a proportional quantity or number of particles of the fluid, and each particle is also struck with a like proportional force. Such then is the case when the fluid is void of tenacity or viscocity, and its particles perfectly independent of each other. But that is not the case in any fluids, and particularly in the atmospherical medium, the particles of which are pressed together by a considerable force; and where any body, moving through it, has not only to remove the particles in its way, and that are struck, upon which the law of the duplicate ratio of the velocity depends, but also to overcome the weight of the medium by which its particles are compressed together, if not also some degree of tenacity and friction. On these accounts then another force of resistance is added to the former, or to that which is proportional to the square of the velocity, and which additional force does not respect the same law as the former; but is probably to be expounded by some compound function of the velocity, the discovery of which must chiefly depend on proper experiments.

so 1 to m; which m is therefore greater than 1.—4. Therefore the effective force, and consequently the celerity, as to a first moment, is to be the m^{th} part of what it would be, had there been no resistance.—5. This m^{th} part, or $\frac{1}{m}$, is also the remaining force after such first moment; and this remaining force is, for the same reason, to be proportionally abated as to a second moment: that is, we are to take $\frac{1}{m}$ of it, that is $\frac{1}{mm}$ of the impressed force: and for a third moment, at equal distance of time, $\frac{1}{mmm}$; for a fourth, $\frac{1}{m^4}$; and so on infinitely.

6. Because the length dispatched, in equal times, is proportional to the celerities; the lines of motion, answering to those equal times, are to be as $\frac{1}{m}$, $\frac{1}{m^2}$, $\frac{1}{m^3}$, $\frac{1}{m^4}$, &c. of what they would have been, in the same times, had there been no resistance.—7. This therefore is a geometrical progression, continually decreasing.—8. This decreasing progression, infinitely continued, is yet of a finite magnitude, and equal to $\frac{1}{m-1}$ of what it would have been in so much time, if there had been no resistance. For the sum or aggregate of a geometrical progression, is $\frac{rz-a}{r-1}$, supposing z the greatest term, a the least, and r the common ratio; that is, $\frac{rz}{r-1} - \frac{a}{r-1}$. Now in the present case, supposing the progression infinitely continued, the least term a becomes infinitely small, or $= 0$; consequently $\frac{a}{r-1}$ also vanishes, and then the aggregate becomes barely $= \frac{rz}{r-1}$; that is, by division, $z + \frac{z}{r} + \frac{z}{r^2} + \frac{z}{r^3} +$ &c. $= \frac{rz}{r-1}$, supposing the progression to begin at $z = 1$. That is, dividing all by r, that so the progression may begin at $\frac{z}{r} = \frac{1}{m}$, $\frac{z}{r-1} = \frac{z}{r} + \frac{z}{rr} + \frac{z}{r^3} +$ &c. That is, in the present case, (because of $z = 1$, and $r = m$) $\frac{1}{m} + \frac{1}{mm} + \frac{1}{m^3} +$ &c. $= \frac{1}{m-1}$; that is, (putting $n = m - 1$), $\frac{1}{n}$ of what it would have been, if there had been no resistance.

9. This infinite progression is fitly expressed by an ordinate in the exterior hyperbola, parallel to one of the asymptotes; and the several members of that, by the several members of this, cut in continual proportion. For let SH, fig. 2, be an hyperbola between the asymptotes AB, AF: and let the ordinate DH, in the exterior hyperbola, parallel to AF, represent the impressed force undi-

minished; or the line to be described in such time, by a celerity answerable to such undiminished force: also let BS, a like ordinate, be $\frac{1}{m}$ of it. In AB, which is put $= 1$, let Bd be such a part of it, as BS is of DH. Now because all the inscribed parallelograms, in the exterior hyperbola; AS, AH, &c. are equal; and therefore their sides reciprocally proportional; consequently as $Ad = 1 - \frac{1}{m}$ is to $AB = 1$, or as $m - 1$ to m, so is $BS = \frac{1}{m} DH$, to dh, which is therefore equal to $\frac{1}{m-1}$ of DH; that is, by division, $\frac{1}{m} + \frac{1}{mm} + \frac{1}{m^3}$, &c. of DH. Or, if Bd be taken beyond B; then as $Ad = 1 + \frac{1}{m}$, is to $AB = 1$, or as $m + 1$ to m, so is $\frac{1}{m}$ DH to dh, which is therefore equal to $\frac{1}{m+1} DH$; that is, by division, $= \frac{1}{m} - \frac{1}{mm} + \frac{1}{m^3} -$ &c. of DH.

10. Let such ordinate dh, or its equal in the asymptote AF, fig. 3, be so divided in L, M, N, &c. by perpendiculars cutting the hyperbola, in l, m, n, &c. as that FL, LM, MN, be as $\frac{1}{m}$, $\frac{1}{mm}$, $\frac{1}{m^3}$, &c.; that is, so continually decreasing, as that each antecedent be to its consequent, as 1 to $\frac{1}{m}$; or as m to 1. This is done by taking AF, AL, AM, &c. in such proportion. For the differences of continual proportionals are also continually proportional, and in the same proportion.

11. This being done; the hyperbolic spaces Fl, Lm, Mn, &c. are equal. So that Fl, Lm, Mn, &c. may fitly represent equal times, in which are dispatched unequal lengths, represented by FL, LM, MN, &c.—12. And because they are infinite in number, though equal to a finite magnitude, the duration is infinite; and consequently the impressed force, and motion thence arising, never to be wholly extinguished, without some further impediment, but perpetually approaching to A, in the nature of asymptotes.—13. The spaces Fl, Fm, Fn, &c. are therefore as logarithms, in arithemetical progression increasing, answering to the lines AF, AL, AM, &c.; or to FL, LM, MN, &c. in geometrical progression decreasing.

14. Because FL, LM, MN, &c. are as $\frac{1}{m}$, $\frac{1}{mm}$, $\frac{1}{m^3}$, &c. infinitely terminated at A; therefore their aggregate FA or dh, is to DH, as 1 to $m - 1 = n$.—15. If therefore we take, as 1 to n, so AF to DH; this will represent the length to be dispatched, in the same time, by such undiminished force.—16. And if such DH be supposed to be divided into innumerable equal parts, and therefore infinitely small, these answer to those (as many)

parts unequal in *FA* or *hd*.—17. But what is the proportion of r to 1, or of $1 - r$ to 1, or 1 to m, remains to be inquired by experiment.

18. If the progression be not infinitely continued; but end suppose at N, and its least term be $a = MN$: then, out of $\frac{z}{r-1} = \frac{1}{m} + \frac{1}{mm} + \frac{1}{m^3}$, &c. is to be subducted $\frac{a}{r-1}$, that is, by division, $\frac{a}{r} + \frac{a}{r^2} + \frac{a}{r^3}$, &c.; that is, in the present case, $\frac{a}{m} + \frac{a}{mm} + \frac{a}{m^3}$, &c. And so the aggregate will be $\frac{1-a}{m} + \frac{1-a}{mm} + \frac{1-a}{m^3}$, &c. $= \frac{1-a}{n}$.

And thus much as to the line of projection, in which, secluding the resistance, the motion is accounted uniform; dispatching equal lengths in equal times. Consider we next the line of descent.

19. In the descent of heavy bodies, it is supposed, that to each moment of time, there is superadded a new impulse of gravity to what was before; and each of these, without considering the air's resistance, to proceed equally, from their several beginnings, through the succeeding moments. Hence arises, for the first moment 1, for the second $1 + 1$, for the third $1 + 1 + 1$, and so on, in arithmetical progression; as are the ordinates, in a triangle, at equal distances; and such are also the continual increments of the diameter, or of the ordinates in the exterior parabola, answering to the interior ordinates, or segments of the tangent, equally increasing. As is known, and commonly admitted.

20. If we take in the consideration of the air's resistance; we are then for each of these equal progressions, to substitute a decreasing geometrical progression, in like manner, as in the line of projection.—21. Hence arises, for the first moment $\frac{1}{m}$; for the second $\frac{1}{m} + \frac{1}{m^2}$; for the third $\frac{1}{m} + \frac{1}{m^2} + \frac{1}{m^3}$, &c. And such is therefore the descent of a heavy body falling by its own weight; the several impulses of gravity being supposed equal. That is, as *FL, FM, FN*, &c. in the line of descent, answering to *FL, LM, MN*, &c. in the line of projection.

22. But though the progressions for the line of projection, are like to each of those in the line of descent, it is not to be thence inferred, that therefore $\frac{1}{m}$ in the one, is equal to $\frac{1}{m}$ in the other; but in the line of projection, suppose, $\frac{1}{m}f$, such a part of the force impressed, and a celerity answerable, in the line of descent, $\frac{1}{m}g$, such a part of the impulse of gravity.—23. Those

for the line of descent, of the same body, are all equal, each to each: because g, the new impulse of gravity, in each moment, is supposed to be the same.

24. But what is the proportion of f to g, that of the force impressed, to the impulse of gravity in each body, remains to be inquired by experiment. This proportion being found for one known force; the same is thence known as to any other force of which the proportion to this is given, in the same uniform medium. And this being known as to one medium; the same is thence known as to any other medium, the proportion of the resistance to that of this being known.

25. If a heavy body be projected downward in a perpendicular line; it descends therefore at the rate $\frac{1}{m}$, $\frac{1}{mm}$, $\frac{1}{m^3}$, &c. of f, the impressed force, increased by $\frac{1}{m}$, $\frac{1}{m}+\frac{1}{m^2}$, $\frac{1}{m}+\frac{1}{m^2}+\frac{1}{m^3}$, &c. of g the impulse of gravity; because both forces are here united.—26. If in a perpendicular projection upwards; it ascends at the rate of the former, abated by that of the latter: because here the impulse of gravity is contrary to the force impressed.—27. When therefore this latter, continually increasing, becomes equal to that former, continually decreasing, it then ceases to ascend; and thenceforth descends at the rate by which the latter continually exceeds the former.

28. In an horizontal or oblique projection: if to a tangent the increments of which are as FL, LM, MN, &c. that is as $\frac{1}{m}f$, &c. be fitted ordinates, at a given angle, the increments of which are as FL, FM, FN, &c. that is as $\frac{1}{m}g$, &c. the curve answering to the compound of these motions, is that in which the projectile is to move. This curve, being hitherto without a name, may be called linea projectorum; the line of projects, or things projected; which resembles a parabola deformed.—29. The celerity and tendency, as to each point of this line, is determined by a tangent at that point.—30. And that against which it makes the greatest stroke or percussion, is that which, at that point, is at right angles to that tangent.

31. If the projection, at art. 21, be not infinitely continued, but terminate, suppose, at N, so that the last term in the first column or series erect be a; and consequently in the second, $m a$; in the third, $m m a$, &c. each series having one term fewer than that before it; then the aggregates of the several columns, or series, will be $\frac{1-a}{n}$, $\frac{1-ma}{n}$, $\frac{1-mma}{n}$, and so on, till, the multiple of a becoming $= 1$, the progression vanish.

32. Now all the abatements here, a, $m a$, $m m a$, &c. are the same with the

terms of the first column taken backward. For a is the last, ma the next before it; and so of the rest. And the aggregate of all the numerators is so many times 1, as is the number of terms, suppose t, wanting the first column; that is $t - \frac{1-a}{n}$, or $\frac{nt-1+a}{n}$; & this again divided by the common denominator n, becomes $\frac{nt-1+a}{nn}$. Therefore $\frac{nt-1+a}{nn}g$, is the line of descent by its own gravity.

33. If therefore this be added to a projecting force perpendicularly downward, or subducted from such projecting force upward; that is, to or from $\frac{1-a}{n}f$; the descent in the first case will be $\frac{1-a}{n}f + \frac{nt-1+a}{nn}g$; and the ascent in the other case $\frac{1-a}{n}f \quad \frac{nt-1+a}{nn}g$. And in this latter case, when the negative part becomes equal to the positive part, the ascent is at the highest; after which it will descend.

34. In an horizontal or oblique projection, having taken $\frac{1-a}{n}f$ in the line of projection, and thence, at the angle given, $\frac{nt-1+a}{nn}g$ in the line of descent; the point in the curve answering to these, is the place of the project answering to that moment.

35. I am aware of some objections to be made both to some points of the process, and to some of the suppositions. But I saw not well how to avoid them without making the computation much more perplexed. And in a matter so nice, and which much depend upon physical observations, it will be hard to attain such accuracy as not to stand in need of some allowances. Somewhat might have been further added to direct the experiments above suggested. But that may be done at leisure, after deliberation had, which way to attempt the experiment. The like is to be said of the different resistance which different bodies may meet with in the same medium, according to their different gravities, extensively or intensively considered, and their different figures and positions in motion.

36. As to the question proposed whether the resistance of the medium do not always take off such a proportional part of the force moving through it, as is the specific gravity of the medium to that of the body moved in it. I think this can by no means be admitted. For there are many other things of consideration herein, besides the intensive or specific gravity of the medium. As for instance, a viscous medium will resist more than one more fluid, though of like intensive gravity; and a sharp arrow will make its way more easily through a medium than a blunt headed bolt, though of equal weight; and the same py-

ramid with the point than with the base forward. But this I think may be admitted, viz. that different mediums equally fluid, other circumstances being alike, do resist in such proportion as is their intensive gravity; because there is, in such proportion, a heavier object to be removed by the same force. And again, the heavier project once in motion, being equally swift, and all other circumstances alike, moves through the same medium in such proportion more strongly, as is its intensive gravity; for now the force is greater in such proportion, for the removal of the same resistance. But where there is a complication of these considerations, and of many other circumstances, whereof each is severally to be considered, respect must then be had to them all.*

On the Grains resembling Wheat which fell lately in Wiltshire. By Mr. Wm. Cole, of Bristol. N° 186, p. 281.

This city and country round about are filled with reports of raining wheat about Warminster, and other places within 6 or 8 miles of it; and many believe it. I have procured several parcels of it, and carefully examined them, and find it to be the seeds of ivy berries, which from towers and churches, chimnies, walls, and high buildings, were, lately by very fierce tempests of wind and hail, driven from the holes, chinks, and other parts, where birds had brought them, especially starlings and choughs. It were to little purpose to tell you the prodigious stories which have been made of it. But I have, by all the ways I can imagine, examined and compared them with the seeds of ivy berries, as by the taste, smell, size, and figure; and with the assistance of magnifying glasses viewing them in both the superficial and inward parts.

Extract of a Letter, dated Dec. 4, 1686, written by Mr. Veay, Physician at Toulouse, to Mr. de St. Ussans, concerning a very extraordinary Hermaphrodite in that City. Communicated by Dr. Aglionby, Reg. Soc. S.† N° 186, p. 282.

Une chose fort extraordinaire, m'est arrivée il y a quelques jours dans l'hos-

* To the note before given on the first article of this paper, may here be added, that Dr. Wallis in the conclusion of it, by these remarks shows that he was aware of many other circumstances to be considered, which could only be derived from proper experiments; which experiments he was then either not aware how they were to be conducted, or had not the means of performing. And nearly in the same state has this important part of philosophy continued to the present time, uncompleted by any series of experiments, excepting those of which a small specimen is given in Dr. Hutton's Philosophical Dictionary, art. RESISTANCE, where the real experimented resistance is given for every degree of velocity, from the smallest up to the greatest, of 2000 feet per second of time, and of which experiments a fuller account may possibly one day see the light.

† This communication is reprinted in the original French, it being judged improper to appear in English.

pital S. Jacques, au quartier des femmes. On apporta une servante malade hermaphrodite. Elle est du lieu de Pourdiac a sept lieues de Toulouse. Elle a eté baptisée en qualité de fille, sous le nom de Marguerite. Son pere est pauvre homme de Pourdiac, qu'on appelle Malause. Elle est agée de 21 a 22 ans, ayant bien la mine exterieure d'une fille, mais les marques réelles d'un homme bien puissant. Son visage est feminin et assez agreable, la gorge bien jolie, et les mammelles aussi bien faites qu'on les puisse desirer a une fille, les fesses et les cuisses grandes comme aux femmes, les parties honteuses tout comme celles d'une femme, mais elle n'est percée que de la profondeur de deux petits travers de doigts ; et au milieu de cette fente, il pend un membre viril d'une grosseur fort considerable, et qui dans l'erection lui sort au dehors d'environ huit pouces. Ce membre est bien formé, hormis qu'il n'a point de prepuce, et qu'il n'est pas accompagné de testicules apparens. L'urine et la semence en sortent comme aux hommes, et ce qu'il y a de particulier, c'est que le sang menstrual coule aussi par ce méme conduit de la verge.

J'aurois eu de la peine a le croire, si je ne l'avois vu moi-méme, et examiné fort exactement dans le temps que ses menstrues couloient, lesquelles lui surviennent presque tous les mois assez regulierement, ne passant gueres deux mois de temps sans les avoir ; mais presque toujours avec de grandes douleurs et une tension aux bas ventre qui marque une espece d'inflammation dans ces parties.

J'ai fait voir cela a plusieurs de nos medecins, et apres avoir consulté Messieurs les Vicaires Generaux, nous lui avons fait prendre un habit d'homme, sous le nom d'Arnaud Malause, et on va presentement lui faire apprendre quelque metier. Il n'y avoit pas a hesiter la dessus, parce que notre hermaphrodite peut fort bien faire la fonction d'homme, et point du tout celle de femme.

Account of Books. N° 186, p. 283.

I. Historia Plantarum, species hactenus editas aliasque insuper multas noviter inventas et descriptas complectens, &c. Autore Joanne Rajo e Societate Regia. Tomus primus, Lond. 1686, fol.

Ray's system, like that of Morison, was meant to comprise as many natural classes as possible. The ingenious Dr. Milne, in his excellent work, entitled *Institutes of Botany*, very justly observes, that, eminent as is the rank of this author in the list of botanical writers, several instances might be adduced in which the execution of his system is inferior to the plan, and in which vegetables are erroneously referred to the particular divisions.

Ray, as appears from some of his letters, considered Morison as not perfectly qualified for the publication of the great work which bears his name; and seems from this consideration to have entered with the greater zeal on the publication of his own method. It has been followed by Sloane in his Natural History of Jamaica, by Petiver in his Herbal, by Dillenius in his Synopsis of British Plants, and by Mr. Martyn (father of the present Professor Martyn) in his Catalogue of the Plants that grow in the neighbourhood of Cambridge.

It should be observed that the system announced in the present article differs considerably from the more finished one afterwards published.

II. Philosophiæ Naturalis Principia Mathematica, Autore I. Newton, Trin. Coll. Cantab. Soc. Matheseos Professore Lucasiano, et Soc. Reg. Sod. 4to. Londini.

This* incomparable author having at length been prevailed on to appear in public, has in this treatise given a most notable instance of the extent of the powers of the mind; and has at once shown what are the principles of natural philosophy, and so far derived from them their consequences, that he seems to have exhausted his argument, and left little to be done by those that shall succeed him. His great skill in the old and new geometry, assisted by his own improvements of the latter, (I mean his method of infinite series) has enabled him to master those problems, which for their difficulty would have still lain unresolved, had one less qualified than himself attempted them.

This treatise is divided into three books, whereof the first two are intitled de Motu Corporum, the third de Systemate Mundi. The first begins with definitions of the terms made use of, and distinguishes time, space, place, and motion, into absolute and relative, real and apparent, mathematical and vulgar: showing the necessity of such distinction. To these definitions are subjoined the laws of motion, with several corollaries from them; as concerning the composition and resolution of any direct force out of, or into any oblique forces, by which the powers of all sorts of mechanical engines are demonstrated; the laws of the reflection of bodies in motion after their collision; and the like.

These necessary præcognita being delivered, our author proceeds to consider curves generated by the composition of a direct impressed motion with a gravitation or tendency towards a centre: and having demonstrated that in all cases the areas at the centre, described by a revolving body, are proportional to the times; he shows how, from the curve described, to find the law or rule of the decrease or increase of the tendency or centripetal forces as he calls it, in different distances from the centre. Of this there are several examples: as, if

* This account of Newton's Principia has much the appearance of having been drawn up by the masterly talents of Dr. Halley, who about that time it seems was the person who published the Transactions, and who had been chiefly instrumental in procuring the publication of the above great work of Newton.

the curve described be a circle passing through the centre of tendency; then the force or tendency towards that centre is in all points as the 5th power, or squared-cube, of the distance from it reciprocally: if in the proportional spiral, reciprocally as the cube of the distance: if in an ellipse about the centre of it, directly as the distance. If in any of the conic sections about the focus; then he demonstrates that the vis centripeta, or tendency towards that focus, is in all places reciprocally as the square of the distance from it; and that according to the velocity of the impressed motion, the curve described is an hyperbola; if the body moved be swift to a certain degree, than a parabola; if slower, an ellipse, or a circle in one case. From this sort of tendency or gravitation it follows likewise, that the squares of the times of the periodical revolutions, are as the cubes of the radii or transverse axes of the ellipses. All which being found to agree with the phænomena of the celestial motions, as discovered by the great sagacity and diligence of Kepler, our author extends himself upon the consequences of this sort of vis centripeta; showing how to find the conic section which a body shall describe when projected with any velocity in a given line, supposing the quantity of the said force known: and laying down several neat constructions to determine the orbs, either from the focus given, and two points or tangents; or, without it, by 5 points or tangents, or any number of points and tangents, making together 5. He then shows how, from the time given, to find the point in a given orbit answering to it; which he performs accurately in the parabola, and, by concise approximations, comes as near as he pleases in the ellipse and hyperbola: all which are problems of the highest concern in astronomy. Next he lays down the rules of the perpendicular descent of bodies towards the centre, particularly in the case where the tendency to it is reciprocally as the square of the distance; and generally in all other cases, supposing a general quadrature of curve lines: upon which supposition likewise he delivers a general method of discovering the orbits described by a body moving in such a tendency towards a centre, increasing or decreasing in any given relation to the distance from the centre; and then with great subtilty he determines in all cases the motion of the apses, or of the points of greatest distance from the centre, in all these curves, in such orbits as are nearly circular. Showing the apses fixed, if the tendency be reciprocally as the square of the distance; direct in motion, in any ratio between the square and the cube; and retrograde, if under the square: which motion he determines exactly from the rule of the increase or decrease of the vis centripeta.

Next the motion of bodies in given surfaces is considered, as likewise the oscillatory motion of pendules; where it is shown how to make a pendulum vibrate always in equal times, though the centre or point of tendency be never so near; to which, the demonstration of Mr. Huygens de Cycloide is but a corollary. And in another proposition is shown the velocity in each point, and the time spent in each part of the arch described by the vibrating body. After this, the effects of two or more bodies, towards each of which there is a tendency, is considered; and it is made out that two bodies, so drawing or attracting each other, describe about the common centre of gravity, curve lines, like to those they seem to describe about each other. And of three bodies, attracting each other, reciprocally as the square of the distance between their centres, the various consequences are considered and laid down, in several corollaries of great use in explaining the phænomena of the moon's motions, the flux and reflux of the sea, the precession of the equinoctial points, and the like.

This done, our author, with his usual acuteness, proceeds to examine into the causes of this tendency or centripetal force, which from undoubted arguments is shown to be in all the great bodies of the universe. Here he finds that if a sphere be composed of an infinity of atoms, each of which have a conatus accedendi ad invicem, which decreases in duplicate proportion of the distance between them; then the whole congeries shall have the like tendency towards its centre, decreasing, in spaces without it, in duplicate proportion of the distances from the centre; and decreasing within its surface, as the distance from the centre directly; so as to be greatest on the surface, and nothing at the centre: and though this might suffice, yet to complete the argument, there is laid down a method to determine the forces of globes composed of particles whose tendencies to each other decrease in any other ratio of the distances; which speculation is carried on likewise to other bodies not spherical, whether finite or indeterminate. Lastly, is proposed a method of explaining the refractions and reflections of transparent bodies, from the same principles; and several problems solved of the greatest concern in the art of dioptrics.

Hitherto our author has considered the effects of compound motions in mediis non resistentibus, or wherein a body once in motion would move equally in a direct line, if not diverted by a supervening attraction or tendency towards some other body. Here is demonstrated what would be the consequence of a resistance from a medium, either in the simple or duplicate ratio of the velocity, or else between both: and to complete this argument, is laid down a general method of determining the density of the medium in all places, which, with a uniform gravity tending perpendicularly to the plane of the horizon, shall make a project move in any curve line assigned; which is the 10th prop. lib. 2. Then the circular motion of bodies in resisting media is determined, and it is shown under what laws of decrease of density, the circle will become a proportional spiral. Next, the density and compression of fluids is considered, and the doctrine of hydrostatics demonstrated; and here it is proposed to the contemplation of natural philosophers, whether the surprising phænomena of the elasticity of the air, and some other fluids, may not arise from their being composed of particles which fly each other; which being rather a physical than mathematical inquiry, our author forbears to discuss.

Next, the opposition of the medium, and its effects on the vibrations of the pendulum are considered, which is followed by an inquiry into the rules of the opposition to bodies, as their bulk, shape, or density may be varied: here with great exactness is an account given of several experiments tried with pendula, in order to verify the foregoing speculation, and to determine the quantity of the air's opposition to bodies moving in it.

From hence is proceeded to the undulation of fluids, the laws whereof are here laid down, and by them the motion and propagation of light and sound are explained. The last section of this book is concerning the circular motion of fluids, wherein the nature of their vortical motions is considered; and from thence the Cartesian doctrine of the vortices of the celestial matter carrying with them the planets about the sun, is proved to be altogether impossible.

The third and last book is intitled de Systemate Mundi, wherein the demonstrations of the two former books are applied to the explication of the principal phænomena of nature: here the verity of the hypothesis of Kepler is demonstrated; and a full resolution given to all the difficulties that occur in the astronomical science; they being nothing else but the necessary consequences of the sun, earth, moon, and planets, having all of them a gravitation or tendency towards their centres proportional to the quantity of matter in each of them, and whose force abates in duplicate proportion of the distance reciprocally. Here likewise are indisputably solved the appearances of the tides, or flux and reflux of the sea; and the spheroidical figure of the earth and Jupiter determined, from which, the precession of the equinoxes, or rotation of the earth's axis is made out, together with the retrocession of the moon's nodes, the quantity and inequalities of whose motion are here exactly stated a priore. Lastly, the theory of the motion of comets is attempted with such success, that in an example of the great comet which appeared in $168\frac{0}{1}$, its motion is computed as exactly, as we can pretend to give the places of the primary planets; and a general method is here laid down to state and determine the trajectoriæ of comets, by an easy geometrical construction; upon supposition that those curves are parabolic, or so near it that the parabola may serve without sensible error; though it be more probable, says our author, that these orbits are elliptical, and that after long periods comets may return again. But such ellipses are, by reason of the immense distance of the foci, and smallness of the latus rectum, in the parts near the sun where comets appear, not easily distinguished from the curve of the parabola: as is proved by the example produced.

The whole book is interspersed with lemmas of general use in geometry, and several new methods applied, which are well worth the considering; and it may be justly said, that so many and so valuable philosophical truths, as are herein discovered and put past dispute, were never yet owing to the capacity and industry of any one man whatever.

A Receipt to cure Mad Dogs, or Men or Beasts bitten by Mad Dogs. Communicated to the Royal Society. By Sir Robert Gourdon.* N° 187, p. 298.

Take agrimony roots, primrose roots, dragon roots, single peony roots, the leaves of box, of each a handful; the star of the earth two handfuls; the black of crabs' claws prepared, Venice treacle, of each one ounce; all these are to be beaten and bruised together, and boiled in about a gallon of milk, till the half be boiled away; then put it into a bottle, unstrained, and give about 3 or 4 spoonfuls of it at a time, to the dog or beast, three mornings together before new and full moon. It will be necessary, the day before administering the medicine, to take away a little blood. Some of these roots and herbs being difficult to be got in the winter, they may be gathered in their season, and being dried and well powdered, may be given, mixed with the crabs' claws and Venice treacle, with sallet oil or butter, and it will do as well.

For men or women bitten by mad dogs; take the same ingredients in the same quantities, and the roots and herbs being bruised altogether, with the crabs' claws and Venice treacle; let them be infused warm in two quarts of strong white wine, for at least 12 hours. This being strained, the party bitten is to take about a quarter of a pint evening and morning, three days before the new and full moon; it may be sweetened, either with sugar or some cordial syrup.

N. B. The plant in this receipt called star of the earth, and which is the chief ingredient, is known among the botanists, by the name of sesamoides salamantica Parkinsoni, sive lychnis viscosa flore muscoso, Casp. Bauhini; in English, Spanish catch-fly. It grows plentifully about Thetford, and about the mills near Newmarket.

* This receipt, it is to be feared, would be of little avail in cases of canine madness. Other opportunities will occur of offering some remarks on this subject.

The Theory of the five Satellites of Saturn corrected. By M. Cassini. N° 187, p. 299. *Translated from the French.*

The distance of the first satellite of Saturn from his centre appeared variable, and its motion sensibly unequal, being swifter in the western semicircle than in the eastern. At length M. Cassini determined its mean distance at $\frac{3}{4}$ of the diameter of Saturn's ring, and its diurnal motion at $6^s\ 10^\circ\ 41'\ 31''$. So that, if its motion were uniform, the duration of its conjunction with Saturn, that is, the whole time of passing over his ring, would be 7h. 46m.; but by observation it has appeared greater. He calculated the epoch of its motion Dec. 31, 1685, at noon, for the meridian of Paris, in Capricorn $24^\circ\ 50'$.

The distance of the second satellite from the centre of Saturn appeared more uniform, which he determined at a diameter and a quarter of the ring. Its motion also appeared more equal; and its diurnal motion he calculated at $4^s\ 11^\circ\ 31'\ 30''$: so that the duration of its conjunction would be 8h. 36m. He could never observe this satellite approach nearer the planet's ring than $\frac{1}{4}$ of an ansa. And as this satellite is for the most part to be seen within the confines of the distance of the first, to which it is equal in magnitude, and alike in colour, it proved a very difficult matter to distinguish them. The epoch of this planet, for the noon of Dec. 31, 1685, he determined, in Virgo $9^\circ\ 10'$.

The distance of the 3d satellite from the centre of Saturn appeared to be $1\frac{3}{4}$ of the diameter of the ring. Its diurnal motion was $2^s\ 18^\circ\ 41'\ 50''$. So that its conjunction ought to last for 10 hours. And the epoch of its motion for the noon of Dec. 31, 1685, was in Virgo $9^\circ\ 39'$.

The distance of the fourth satellite from Saturn's centre was 4 diameters of the ring. Its diurnal motion $22^\circ\ 34'\ 38''$; the duration of its conjunction 15h. 6m.; and the epoch of its motion, for the same time and place as the others in Pisces $18^\circ\ 1'$.

The distance of the 5th satellite from the centre of Saturn, was 12 diameters of his ring; its diurnal motion $4^\circ\ 32'\ 17''$; its conjunctions continue for 24 hours; and the epoch for the same time and place as above, in Pisces $16^\circ\ 19'$. From which principles may tables and ephemerides be constructed.

Observations and Experiments concerning the Growth of Trees. By Thomas Brotherton, of Hey, in the County of Lancaster, Esq. Communicated by R. H. F. R. S. N° 187, p. 307.

A crab tree, about 4 inches in diameter, was hacked round with a hatchet, so as to cut pretty deep into the wood, besides cutting off the bark, for about

4 inches wide. After which it was the same year observed to increase above the hacking very considerably, and to shoot in length of wood, about one foot; the next year it increased considerably, and shot in length about 9 inches: but the third year it died to the very root. The like was observed in another, part of its bark being eaten off by a canker: the lower part stood, without increasing; and by degrees the wood rotted and mortified; but the upper part increased to the 3d year, when it died also.

A Scotch fir of 3 years growth, having a ring of the bark cut off, of the breadth of 3 inches, near the bottom of the stem, below the uppermost joint, was observed to grow and shoot out its top, about half a yard; and the parts all about the ring to increase very much in thickness, the same year the section was made, and much more than it would have done if the section had not been made; but all that part of the stock between the said ring and the knot next below it, increased not at all, and that part below the next joint increased somewhat, though not so much as if the ring of bark had not been cut off. The 2d year it also increased considerably, though not so much as the first; but the 3d year it died. The usual time for making this section was either in March or the beginning of April.

Trial was made on some young trees, cutting a heliacal swath of the bark, about half an inch in breadth, by leaving a like heliacal swath of bark to communicate between the upper and under part. In this trial, the difference of growth succeeded not, but the remaining swath of the bark swelled downwards, and by the end of the year covered the bared part of the wood. It was observable, that as the upper bark grew downwards, so it increased also in thickness, whereas the bark below the section thickened not at all.

He observed also that all the poplars that had been pruned, died in the great frost, 1684; so that of 25 that were so ordered, he observed 19 of them killed by it, and the remaining to be very weak, and hardly able to recover, and increased very little in the following years. These poplars were about 30 feet high, and had only a small head left at the top unlopped, of about 4 or 5 feet, and were pruned the spring before the great frost. He observed also, that divers of those which had been pruned two summers before the frost were killed by it; but none of those which had not been pruned at all were hurt by it. He took notice also, both in Lancashire and Cheshire, that trees of 60 feet in height, that had been pruned, and had only a small top left, were also killed by the said frost; whereas those trees of the same kind and height, which stood near them, but had not been pruned, continued to flourish and suffered no harm. Several of those branches of about an inch diameter, and trees that had been barked round, as above, the spring before the great frost, out-lived the violence of the same, and the preceding winter.

When these prunings had been tried on trees 20 feet high, the difference of their increase was sensible the following summer; but in 7 or 8 years time, the difference is prodigious, the unpruned trees growing several times larger than the pruned, both in body and branches.

He has often observed also, that when the top branches would shoot out and grow 2 feet or more in length, the lower branches would not shoot above 4 inches. And further, that in the branches of the Scotch fir, the joints above the rings barked round would increase and grow much larger in 3 years, than they would in 5 years if the said rings were not cut off.

On considering these and several other observations and experiments, Mr. Brotherton is of opinion, 1. That the sap (most of it, if not all) ascends in the vessels of the ligneous part of the tree, and not in the cortical part, nor between the cortical and ligneous parts.—2. That the increase and growth of a tree in thickness is by the descent of the sap, and not by the ascent; and if there were no descent, a tree would increase but very little, if at all.—3. That there is a continual circulation of the sap all the summer season, and during such time as the sap is stiring, and not a descent at Michaelmas only, as some have held.

To me it seems very probable, that the bodies of plants, as well as those of moving animals, are nourished and increased by a double food; the one an impregnated water, and the other an impregnated air; and that without a convenient supply of these two, the vegetable cannot subsist, at least not increase. These mutually mix and coalesce, and parts of the air convert to water, and parts of water convert to air. To this purpose all plants, as well as animals, have a twofold kind of roots, one that branches and spreads into the earth, and another that spreads and shoots into the air; both kinds of roots serving to receive and carry their proper nourishment to the body of the plant, and both serve also to convey and carry off the useless recrements, useless I mean any further within the body of the plant, though useful to it when they are separated and without it, the one for seasoning the earth and water wherein it is planted, and the other for seasoning and preparing the air, the method of which I have elsewhere explained.

Concerning the apparent Magnitude of the Sun and Moon, or the apparent Distance of two Stars, when near the Horizon, and when higher elevated. By Wm. Molyneux, Esq. N° 187, p. 314.

It is well known, that the mean apparent magnitude of the moon is 30′ 30″; we may take it at the round number 30′, at a full moon in the middle of winter, and when she is in the meridian, and at her greatest northern latitude, and consequently her utmost elevation in our horizon. It is also well known, that when she is in this situation, and viewed by the naked eye, she appears to be about a foot broad. But the same moon being viewed just as she rises, appears to be 3 or 4 feet broad; and yet, if with an instrument we take her diameter, both in one position and the other, we find that still she is only 30 minutes.

The celebrated Descartes attributes this appearance rather to a deceived judgment than to any natural affection of the organ or medium of sense; for the moon, says he, being near the horizon, we have a better opportunity and advantage of making an estimate of her, by comparing her with the various objects that effect the sight, in its way towards her; so that though we imagine she looks larger, yet it is a mere deception: for we only think so, because she seems nearer the tops of the trees, or chimneys, or houses, or a space of ground, to which we can compare and estimate her by; but when we bring her to the test of an instrument, then we find our estimate wrong, and our senses deceived. These thoughts seem much below the usual accuracy of Descartes; for if it be so, we may at any time increase the apparent size of the moon, though in the meridian; for it would be only getting behind a cluster of chimneys, a ridge of hills, or the tops of houses, and comparing her to them in that position, as well as in the horizon: besides, if the moon be viewed just as she is rising from the horizon determined by a smooth sea, and which has no more variety of objects to compare her to, than the pure air; yet she then seems large, as if viewed over the rugged top of an uneven town, or rocky country. Besides, all variety of adjoining objects may be taken off, by looking through an empty tube, and yet the deluded imagination is not at all helped thereby.

I come next to the solution given by the famous Thomas Hobbs, which is as follows. In fig. 4, pl. 10, let the point G be the centre of the earth, and F the eye on its surface; on the same centre G, describe the two arches, the one EH determining the atmosphere, and the other AD to represent that azure surface in which we imagine the fixed stars; and let FD be the horizon. Divide the arch AD into three equal parts, by the lines BF, CF. Then it is manifest that the angle AFB is greater than the angle BFC, and this again greater than the angle CFD. Wherefore, says he, to make the angle CFD equal to the angle CFB, the arch CD must be greater than the arch CB; and consequently, that the moon may in the horizon appear under the same angle as when elevated, she must cover a greater arch, and therefore seem greater; that is, the moon in the meridian appearing under the angle BFC, that she may appear under an equal angle in the horizon, as suppose CFD, it is necessary that the arch CD be greater than CB; and consequently, though she appear to subtend a greater arch, when in the horizon, than when elevated, yet she appears under the same angle: and all this without refraction. Now the geometry of this figure is most certainly true and demonstrable; but it clears up no more our present difficulty, than if nothing at all had been said: for the philosopher has here made a figure of his own, and from thence he argues as confidently, as if nature would accommodate herself to his scheme, and he not obliged to accommodate his scheme to nature; for here he has made the circle G F, representing the earth, very large in proportion to the circle A D; and then indeed, taking the point F in the earth's surface, and by lines from thence dividing the angle A F D into a number of equal parts, the intercepted arches A B, B C, C D will be unequal. But if he had considered that the earth is, as it were, a point in respect of the sphere of the fixed stars; nay, the very annual orbit of the earth itself is almost, if not altogether imperceptible, he would have found that the lines F B, F C, F D, must be all conceived as drawn from the point G; and then equal angles will intercept equal arches, and equal arches equal angles; and so it happens, at least as to sense, to the eye on the surface of the earth. So that his drawing his lines so far from G as F is, and to another concentric circle so near as A D, deceived him in this point.

The famous Gassendus has written 4 large epistles on this subject; the substance of all which is, that the moon when near the horizon, and viewed through a more foggy air, casts a weaker light, and consequently forces not the eye so much as when brighter; and therefore the pupil enlarges itself the more, thereby transmitting a larger projection on the retina. In this opinion I find he is not alone; for in the Journals des Sçavans, a French Abbé follows this sentiment of Gassendus; with this addition, that this contracting and enlarging of the pupil causes a different shape in the eye; an open pupil making the crystalline flatter, and the eye longer: but a narrower pupil shortening the eye, and making the crystalline more convex; the first attends our looking at objects which are remote, or which we think so; the latter accompanies the viewing objects near at hand. Likewise an open pupil and flat crystalline attend objects of a gentler light, while objects of more forcible rays require a greater convexity, and a narrower pupil. From these positions the Abbé endeavours to account for the phenomenon in the following manner. When the moon is near the horizon, by comparison here, with interposed objects, we are apt to imagine her much farther from us, than when more elevated; and therefore, says he, we dispose our eyes as for viewing the object farther from us; that is, we enlarge the pupil a little, and thereby make the crystalline flatter; moreover, the duskiness of the moon, in that position, does not so much strain the sight; and consequently the pupil will be larger, and the crystalline flatter:

hence a larger image will be projected on the bottom of the eye, and therefore the moon will appear larger. But the moon's imaginary distance and duskiness gradually vanishing as she rises, a different species is hereby introduced into the eye, and consequently she seems gradually less and less, till again she approaches near the horizon. These two opinions, of Gassendus and the Abbé, being so near a-kin, may be considered together. And first I assert, that a wider or narrower aperture of the pupil neither increases nor diminishes the projection on the retina. I know Honoratus Faber, in his Synopsis Optica, endeavours to prove the contrary to this my assertion, and that after this manner. In fig. 5, A B is an object, E F the greater aperture of the pupil, admitting the projection K I on the retina, whereas the lesser aperture C D admits only the projection G H; but G H is less than K I; wherefore a smaller aperture diminishes the projection. It is surprising that H. Faber, who undertook to write of optics more accurately than all that went before him, should be guilty of so very gross an error; and that the celebrated Gassendus, and the noble Hevelius should be of the same opinion: for though the aforesaid demonstration hold most certainly true in direct projections, as in a dark room with a plain hole; yet it will not hold in projections made by refraction, as it is in those on the retina in the eye, by means of the crystalline, and other coats and humours of the eye. For let A B, fig. 6, be a remote object, and E F the crystalline at its large aperture, projecting the image I M on the retina. Let then C D be the smaller aperture of the pupil before the crystalline; then the image I M will be projected as large as before; for the cone of rays E A F consists partly of the cone of rays C A D, therefore where the former, E A F, is projected, the latter C A D, as being a part of the former, will be projected also. So that no more is affected by this narrow aperture, but only that the sides of the radiating cones are intercepted, and consequently the point I will be affected with less light, but it shall still be in the same place. What is said of that cone, and that point, may be said of all other cones and other points of the object. From hence appears, first, the invalidity of the account given of the moon's appearance by Gassendus; 2. the reason appears, why a telescope's greater or smaller aperture makes no difference in the angle it receives: for imagine E F to be an object-glass of a telescope, and the thing is plain. 3. It is evident why a greater or less aperture in a telescope should make the objects appear brighter or darker, for thereby, more or less rays are admitted for defining the projection of each point. And this is sufficient for a confutation of Gassendus and Faber. But the Abbé superadds to a greater or smaller aperture of the pupil, as a necessary consequence, a greater and less convexity of the crystalline, as also a lengthening and shortening the tube of the eye, And this might do something to the purpose, if we find it true in our case. First then, says he, the duskiness of the moon near the horizon admits the pupil to enlarge itself, the crystalline to flatten, and the eye to lengthen: but what if we change our object, and instead of the moon take the distance between some of the fixed stars, as suppose those of Orion's girdle; we shall find the same phenomenon in them, and yet I hope neither he nor Gassendus will assert, that they at one time strain the eye more than at another, or that at any time their lustre strains the eye at all. But perhaps he will then say, that this other reason holds, which is, 2dly, that the greater imaginary distance, at which we think the moon near the horizon than when more elevated, makes us contemplate her as if really she was so, viz. with enlarged pupils, &c. But this has been sufficiently refuted in my remarks against Descartes, and if there were any thing in this surmise, the horizontal moon should be fancied near to us rather than farther from us.

Riccioli, in his treatise on refraction, asserts, that he and Father Grimaldi had often taken the horizontal sun and moon's diameters by a sextant, when to the naked eye they appeared very large, Grimaldi directing his sight to the left edge, and Riccioli to the right, and that even by the instrument they always found the diameters greater than when more elevated, the sun often subtending an angle of almost a degree, and frequently 45 minutes, the moon also 38 or 40 minutes. Now this is directly contrary to the matter of fact, which I have before alleged, and directly repugnant to the matter of fact asserted by the French Abbé in the aforecited journal. Whether of us be in the right, I leave to accurate experiment to determine, and submit the whole to the decision of the illustrious Royal Society. Only had Riccioli's experiments been accurately prosecuted, he should have tried them when the horizontal moon had looked 10 times larger in diameter than ordinary; and then if it be true, that even by an instrument she will be found proportionally broader than really, she should subtend an angle of 300 minutes, or 5 degrees; for very often I have seen the moon when she appeared 10 times broader than ordinary, which the small addition of 8 or 10 minutes to her usual diameter could never cause.

The Sentiments of Dr. John Wallis, R. S. Soc. on the aforesaid Appearance. Communicated in a Letter to the Editor. N° 187, p. 323.

As to the last inquiry, concerning which, you say, the Royal Society would be glad to know my opinion about the apparent magnitude of the sun near the horizon, greater than when considerably high; the inquiry is ancient, and, I remember discoursing of it near 40 years ago with Mr. Foster, then professor of astronomy in Gresham College, who then assured me, from his own observation, I suppose, for I have never examined it myself, that the apparent magnitude taken by instrument, however the fancy may apprehend it, is not greater at the horizon than when higher. And Mr. Caswell here affirms the same thing. And, though I have not myself made the observation, I do not doubt but the thing is so. For it is agreed, that refraction near the horizon, though as to appearance it alters the altitude of the thing seen, yet it alters not the azimuth at all. And it must needs be so: for, since this equally respects all points of the horizon, let the refraction be what it will, the whole horizon can be but a circle; so that there is no room for the breadth of a thing, as to the angle at the eye, to be made greater, whatever its tallness may, the refraction not equally affecting all parts in the circles of altitude. Nor is there any reason why this should rather thrust the other, than that the other thrust this, out of place. But in the altitude it is otherwise, for while what is near the horizon is enlarged, that which is further off is thereby contracted, which as to the azimuth or horizontal position cannot be.

In spectacles indeed it is otherwise, for they represent the object every way enlarged, and do thereby hide the adjacent parts. But in refraction by vapours, supposing all parts of the horizon equally affected by them, one part cannot be expanded in breadth, whatever it may be as to the height, without thrusting out another; for the whole horizon can be but a circle, and why one part rather than another? Unless we would say, that the rays of a lucid body expand themselves every way, to the prejudice of the parts adjacent, by covering them.

But supposing that the sun or moon's apparent horizontal diameter, taken by instrument near the horizon, is the same as taken in a higher position, I take its imaginary greatness, which is fancied near the horizon, to be only a deception of the eye, or rather the imagination from the eye. For the imagination does not estimate the greatness of the object seen only by the angle which it makes at the eye; but by this compared with the supposed distance. True it is, that, cæteris paribus, we judge that to be the greater object which makes at the eye the greater angle, but not so if apprehended at different distances. For if through a casement, or smaller aperture, we see a house at a 100 yards distance, this house, though seen under a less angle, does not to us seem less than the casement through which we see it, or this greater than that, because it makes at the eye the greater angle; but the imagination makes a comparative estimate from the angle and distance jointly considered. So that, of two things seen under the same or equal angles, if to one of them there be ought which gives the apprehension of a greater distance, that to the imagination will appear greater.

Now one great advantage for estimating of a thing seen is, from the variety of intermediate objects between the eye and the object. For then the imagination must allow room for all these things. Hence it is, that if we see a thing over two hills, between which there lies a great valley unseen, it will appear much nearer than if we see the valley also, and it will appear as just beyond the first hill. And if we move forward to the top of the nearest hill, so that the valley may be seen, it will then appear much farther off than before. And on this account it is, that the sun setting, appears to us as if it were but just beyond the utmost of our visible horizon, because all between that and the sun is not seen. And, on the same account, the heaven itself seems contiguous to the visible horizon.

Now when the sun or moon is near the horizon, there is a prospect of hills and valleys, of plains, woods, rivers, and a variety of fields and inclosures, between it and us; which present to our imagination a great distance capable of receiving all these. Or, if it so happen that in some position these intermediates are not actually seen; yet having been accustomed to see them, the memory suggests to us a view as large as is the visible horizon. But when the sun or moon is in a higher position, we see nothing between us and them, unless perhaps some clouds, and therefore nothing to present to our imagination so great a distance as the other is. And therefore, though both be seen under the same angle, they do not appear to the imagination of the same magnitude because not both fancied at the same distances, but that near the horizon is judged larger, because supposed farther off, than the same when at a greater altitude.

It is true, that as to small and middling distances, beside this estimate from intermediates, the eye has a faculty within itself to make some estimate of the distance. As, when we already know the size of a thing seen, to which we have been accustomed, as a man, a tree, a house, or the like; if such thing appear to us under a small angle, and indistinct and faintly coloured, the imagination allows such distance as to make the thing so to appear. And if this through a prospective glass be represented to us under a larger angle, and more distinct, it is accordingly apprehended as so much nearer. But the case is otherwise when we do not, by the known magnitude, judge the distance; but, by the supposed distance, judge of the magnitude, as in the case before us. And accordingly different persons, according to different fancied distances, judge very differently. As, if two stars be showed to ignorant persons, and you ask how far they seem to be asunder, one perhaps will say, a foot: another a yard, or more; and one shall say, the sun appears to him as large as a bushel, another, as large as a Holland cheese; each estimating according to the fancied distance. Again, in our two eyes, when the object is seen by both, there is yet another means of estimating how far it is off; and it is this by which we judge of distances, namely, there are, from the same object, two different visua cones, terminated at the two eyes; whose two axes contain at the object different angles, according to different distances, an acuter angle at a great distance, and more obtuse when nearer. Now, that such object may be seen by both eyes clearly, it is requisite that the eyes be put into such a position as that the sight of each eye may receive the respective axe at right angles. Which requires a different position of the two eyes, according to the different distance of the object. As will manifestly appear if we look with attention on a finger, or other small object, at 2 or 3 inches distance from the eye; and then upon another like object at 3 or 4 yards beyond it; and this alternately several times; for it will be manifest, that while we look intently on the one, we do not see the other, or but confusedly, though both be just before us. And as we change our view from one to the other, we manifestly feel a motion of the eyes, by their muscles, from one position to another. And according to the different position in the eyes, requisite to a clear vision by both, we estimate the distance of the object from us. And hence it is, that those who have lost the sight of one eye, are at a great disadvantage as to estimating distances, from what they could do, while they had the use of both.

But now when the distance grows so great as that the position of these visual axes become parallel, or so near to parallel as not to be distinguishable from it; this advantage is lost, and we can thenceforth only conclude, that it is far off; but not how far. Hence it is, that our view can make no distinction of the moon's distance from that of the other planets, or even of the fixed stars; but they seem to us as equally remote from us, though we otherwise know their distances from us to be vastly different. Because the parallax from the different position of the two eyes is quite lost and undiscernible, in distances much less than the least of these. And so of the fixed stars amongst themselves, which, though they seem equally remote from us, may for ought we know be at distances vastly different. Nor can we tell which of them is nearest, unless perhaps we may reasonably guess those to be nearest which seem largest. Because here, not only the parallax from the distance of the two eyes, and that from the earth's semidiameter, but even that from the semidiameter of the earth's great orbit is quite lost, and none remaining, whereby to estimate their distance from us.

But, to return to the case in hand; though as to small distances, we may make some estimate from the known magnitude of the object; and, as to middling distances from the parallax arising from the interval of the two eyes; yet even this latter will hardly reach beyond, if so far as, the visible horizon:

and all beyond it is lost. So that, there being nothing left to assist the fancy in estimating so great a distance, but only the intermediate objects; where these intermediates appear to the eye, as, when the sun or moon are near the horizon, the distance is fancied greater, than where they appear not, as when farther from it: and consequently, though both under the same or equal angles, that near the horizon is fancied the greater. And this I judge to be the true reason of that appearance.

The sun's eclipse May 1st, was observed here about $\frac{1}{2}$ digit; between one and two o'clock afternoon.

Account of a Book, viz. a Continuation of the New Digester of Bones; its Improvements, and the Uses it has been applied to, both at Sea and Land; with some Improvements and new Uses of the Air-Pump, tried both in England and Italy. By D. Papin, M.D. F.R.S. N° 187, p. 329.

This treatise is divided into three sections. The first contains the improvements made by the author on the digester; with the new uses it has been applied to: and he does not think that any thing better can be made for such things, as must be stewed in their own juices. But for other things that must be boiled with water, as pulse, gellies, &c. he gives the description of another engine, which he finds to be much preferable to the former; so as that a small engine of this fashion, if it holds but 6 or 7 pounds of water, will be sufficient to make 150 pounds of gelly in 24 hours; and will not consume above 11 pounds of charcoal. He gives also the description of an engine for distilling per descensum in several degrees of rarefaction and condensation of air; and he gives an account of some experiments which he has made with this instrument; from whence it appears, that in some cases the condensation of the air will be of great advantage for a quick distillation.

In the second section are explained the improvements made by the author, on the pneumatic engine. And, by the by, he answers Mr. Bernoulli, who has written something against Mr. Boyle, about the weighing of the air in a bladder; and afterwards he comes to the new uses this engine has lately been applied to.

The third section gives account of what has been done in 2 years time, in Mr. Sarrotti's academy at Venice; which had some relation to the matter treated of in this book. There may be seen several new and curious experiments about matters of moment: but I shall only relate two of them, from whence the reader may judge of the rest. The first is, that two equal pieces of iron were put, at the same time, into two equal quantities of aquafortis, the one in vacuo, and the other in the open air; and being afterwards taken out at the same time;

it was found that the iron in the open air had been 16 times more dissolved, than the iron in vacuo. The second experiment is, that two equal quantities of roses were put into two instruments for distillations, like each other; but the one was exhausted of air, and the other was full; the distillation was abundantly greater and quicker in the evacuated instrument, than in the other, although they were both heated by the same warm water. It was also observable, that the rose-water distilled in vacuo did congeal, which does not happen in ordinary distillations. So it is plain, that in some circumstances the vacuum helps distillations, but that in other circumstances the compression of air is more advantageous.

Observations of what preternaturally occurred on opening the Body of Mr. Smith of Highgate, July 8th, 1687. Communicated by Dr. Edward Tyson, F.R.S. N° 188, p. 332.

In the first place, dividing the abdomen, immediately upon incision made into the peritonæum, we discovered the bladder very scirrhous and thick, viz. $\frac{1}{4}$ of an inch; of a præternatural figure, and distended to the size of a child's head: and at the entrance of the ureters, on each side, were two protuberances, of the size of a hen's egg; the ureters were of the width of the small guts in children; so that they could easily admit two fingers into their cavity. They were both replete with urine, or a serous matter; which, upon pressure, easily regurgitated into the kidneys, but would not pass at all into the bladder. The kidneys were of their natural size and figure; but so emaciated, that they were rather large bags, than of a fleshy substance; the cavity of the pelvis being so large as to contain above 3 ounces of water. But to return to the bladder; therein upon opening we discovered a very strange sort of cysts or bags, of the exact figure of eggs, of several dimensions, some larger than goose eggs, others as large as hen's eggs, to the number of 12 in all; and about 8 of them whole and replete with a limpid serum: the coats of these bladders were some of them considerably thick, others very thin and tender; all of them loose and free, without the least adhesion, either to each other, or to the coat of the bladder. There was little or no urine in the bladder, but what was contained in these bags. Nor could we imagine that this miserable patient could possibly make any water, but what happened on the breach of some of these watery tumours, when the bladder was crowded beyond its dimensions; for that the passage by the ureters into the bladder was impervious: and though the ureters were full of serum, yet could none be forced into the cavity of the bladder.

This liquor contained in these bags, we conjectured to be of the nutritious juice of the body; and on trial, by boiling a small quantity of it, we found it thicken, and come to the consistence of a stiff and glutinous gelly. These vesiculæ * were doubtless formed from the tenacity of the matter between the membranes of the bladder, in its oblique passage through them; for that being so glutinous, it was here detained till its surface was condensed into a firm coat, and so by the coming of more matter was forced into the cavity of the bladder. This I suppose, from our finding two of these ova in a distinct sinus from the rest, between the coats of the bladder, at the entrance of each ureter.

The liver we found very large and hard, of the colour and substance of a boiled one. It adhered to the peritonæum on the external part, and by its vast size had so straitened the thorax, that there was very little room for the lungs. The lungs were of a livid colour, adhering close to the pleura on the right side; upon incision, we found them wholly replete with a purulent matter, and a stone of the size of a cherry-stone in one lobe.—Dividing the pericardium, we found a fungous substance covering the heart all over; and fibres from it, that ran to the pericardium in a great number; so that they were by these fibres every where united.—The heart was very large; the right auricle and ventricle were one large undivided cavity, and therein a large polypus; which ran up the descending branch of the vena cava, to the very jugular; another part was distributed to the pulmonary artery. In the left ventricle was another polypus, not so large as the former: it had two branches, one in the pulmonary vein; another in the arteria magna or aorta. One of the vesiculæ being opened, had a large cluster of small ova, as large as grapes, all replete with liquor. All the rest contained nothing but serum.

On an extraordinary Effect of the Power of Imagination. Communicated by Mr. Edward Smith, Sec. to the Phil. Soc. at Dublin, as it was brought before that Company. By Mr. St. George Ash, R. S. S. N° 188, p. 334.

One Elizabeth Dooly, of the county of Kilkenny, aged 13 years, in January last. Her mother, when with child of her, was frightened by a cow, as she milked it; thrown down, and struck on the temple, near the eye, by the cow's teat. This child has exactly in that place a piece of flesh resembling a cow's teat, about 3 inches and a half in length. It is very red; and has a bone in the midst about half its length. It is perforated, and she weeps through it. When she laughs, it wrinkles up, and contracts to two thirds of its length; and it grows in proportion to the rest of her body. She is as sensible there as in any other part.

* It is probable that these vesiculæ were hydatids.

On the Construction of Solid Problems, or of Equations of the third and fourth Degree, by means of only one given Parabola and a Circle. By Mr. Edmund Halley. N° 188, p. 335. *Translated from the Latin.*

How all equations, that involve the third or fourth power of the unknown quantity, may be constructed by means of any given parabola and a circle, is shown and clearly demonstrated by Descartes in the third book of his geometry: but first he directs to take away the second term of the equation, if such there be, and then by a rule there given, to find the roots of the equation so reduced. But since that operation appeared too laborious, some have devised a like construction, without any such previous reduction; among whom Fr. Schooten might be thought to have discovered a very easy and simple method for constructing cubic equations howsoever affected, if by unfolding the principle from whence he derived his rule, he had paid a greater regard for the memory of his readers, which he overcharges with many perplexed cautions. But our countryman Mr. Thomas Baker, in an entire treatise written on these constructions, has comprised in one general rule, not only cubic, but also biquadratic equations of any kind; and this rule he has sufficiently illustrated by demonstrations and examples in all cases; and towards the end he subjoined a method of investigating that general rule; but he has not shown the very method, by means of which he obtained his Universal Geometrical Clavis, or at least might have obtained it with much more ease; and since this rule of Baker's is no less perplexed with cautions about the signs + and −, than that of Schooten, so that no person can pretend to perform these constructions aright without having the book by him; I thought it would neither be unpleasant nor unprofitable to young students, to explain the foundations of both, and by some amendment of the method, to clear up as much as possible so difficult a subject. Descartes's construction, which very easily discovers the roots of all cubic and biquadratic equations, where the second term is wanting, may be supposed as known; but as it is the key to what is to follow, it may not be improper to add here his rule, with some alteration for the better. When the second term is wanting, all cubic equations are reduced to this form $z^3 * apz.\ aaq = o$; and biquadratic ones to this form $z^4 * apzz.\ aaqz.\ a^3 r = o$; where a denotes the parameter of any given parabola, used in the construction; or else taking a for unity, the equations are reduced to these forms; viz. $3^3 * pz.q = 0$, or $z^4 * pzz.\ qz.\ r = 0$. Now the parabola FAG, fig. 7, pl. 10, being given, whose axis is ACDKL, and latus rectum a or 1, let AC be taken $= \frac{1}{2}a$, and let it be always set off from the vertex A towards the inner parts of the figure; then take CD $= \frac{1}{2}p$, in that line AC produced towards C, if it be $-p$ in the equation, or towards the contrary point, if it be $+ p$. Moreover, from the point D, or from C if the quantity p be not in the equation, $DE = \frac{1}{2} q$ is to be erected perpendicular to the axis, to the right hand if it be $- q$, but on the other side of the axis if it be $+ q$; and then a circle described on the centre E with the radius AE, if it be only a cubic equation, will intersect the parabola in as many points F and G, as the equation has true roots; of which, the affirmative ones, as GK, will be on the right side of the axis, and the negative ones, as FL, on the left. But if the equation be a biquadratic, the radius of the circle should be either augmented or diminished, by adding if it be $- r$, or subtracting if $+ r$, from its square, the rectangle ar, which rectangle is the product of the latus rectum and the given quantity r, which is very easily done geometrically. And, letting fall from them perpendiculars to the axis, the intersections of this circle with the parabola will give all the true roots of the biquadratic equation, the affirmative on the right side of the axis, and the negative on the left. Here it is to be observed, that I endeavour to have the affirmative roots on the right side of the axis, to avoid the confusion necessarily arising from a great number of cautions, where the reason of them is not evident.

Having premised these things, in order to make way for the construction of these equations, where the second term is found, we are to consider the rule for taking away that term, and reducing the equation to another, such as might be constructed by the preceding method. Now all cubic equations of this class, are reducible to this form, $z^3 . bzz . apz . aaq = 0$, or to this, $z^3 . bzz . * . aaq = 0$; biquadratic equations to this form, $z^4 . bz^3 . apz^2 . aaqz . a^3 r = 0$, or to this, $z^4 . bz^3 . * . aaqz . a^3 r = 0$, or $z^4 . bz^3 . apz^2 . * . a^3 r = 0$, or lastly to this form, $z^4 . bz^3 . * . * . a^3 r = 0$; from all which there arises a great variety, according as the signs $+$ and $-$ are differently connected, and hence the general rule serving for all these cases is rendered very obscure and difficult, unless it be cleared up by the following method, and freed from its intricacies.

The second term in biquadratic equations is taken away by putting $x = z + \frac{1}{4} b$ if it be $+ b$ in the equation, or $x = z - \frac{1}{4} b$ if it be $- b$; hence $x - \frac{1}{4} b$ in the first case, and $x + \frac{1}{4} b$ in the second, is $= z$; and instead of z, in any proposed equation, substituting its equal, there will arise a new equation, wanting the second term, of which all the roots x are either greater or less than the required roots z, by the given difference $\frac{1}{4} b$.

Example 1. If $z^4 + bz^3 - apzz - aaqz + a^3 r = 0$.

Put $x - \frac{1}{4} b = z$. Then it will be,

$$x^2 - \tfrac{1}{2} bx + \tfrac{1}{16} b^2 = z^2,$$
$$x^3 - \tfrac{3}{4} bx^2 + \tfrac{3}{16} b^2 x - \tfrac{1}{64} b^3 = z^3,$$
$$x^4 - bx^3 + \tfrac{3}{8} b^2 x^2 - \tfrac{1}{16} b^3 x + \tfrac{1}{256} b^4 = z^4.$$

$$
\begin{aligned}
\text{Hence } x^4 - bx^3 + \tfrac{3}{8}b^2x^2 - \tfrac{1}{16}b^3x + \tfrac{1}{256}b^4 &= +z^4,\\
+ bx^3 - \tfrac{3}{4}b^2x^2 + \tfrac{3}{16}b^3x - \tfrac{1}{64}b^4 &= +bz^3,\\
- apx^2 + \tfrac{1}{2}apbx - \tfrac{1}{16}apb^2 &= -apz^2,\\
- a^2qx + \tfrac{1}{4}a^2qb &= -a^2qz,\\
+ a^3r &= +a^3r.
\end{aligned}
$$

The sum of all these is a new equation, wanting the second term, and which consequently may be constructed by Descartes's rule, by taking, instead of $\frac{1}{2}p$, half the co-efficient of the 3d term divided by the parameter a, that is $-\frac{3b^2}{16a} - \frac{1}{2}p$; and instead of $\frac{1}{2}q$, half the co-efficient of the 3d term divided by a^2, that is $+\frac{b^3}{16a^2} + \frac{pb}{4a} - \frac{1}{2}q$. The members of which that have the sign $+$, are to be set off from the axis towards the left hand, and those that have the sign minus ($-$) to the right, in order to find the centre of the circle requisite for the construction, and of which the intersections with the parabola, drawing perpendiculars to the axis, may denote all the true roots x, viz. the affirmative on the right hand side of the axis, and the negative on the left. But since $x - \frac{1}{4}b = z$, by drawing a line parallel to the axis, on the right hand side of it, and at the distance of $\frac{1}{4}b$, the perpendiculars terminated by this parallel, will denote all the roots required z; viz. the affirmative ones on the right, and the negative on the left.

As to the radius of the circle, it is had, by adding the negative, and deducting the affirmative parts of the 5th term divided by a^2, from the square of the line AE, drawn from the centre E, to the vertex A of the parabola: which is best done, by taking, instead of AE, the line EO, terminated at O, the intersection of the parabola and the above-mentioned parallel; for its square comprehends all the parts of the 5th term brought into the new equation by the taking away of the 2d term, as is easily proved. And there only remains to increase the square of EO, if there be $-r$ in the equation, or to diminish it if it be $+r$, by the addition or subtraction of the rectangle ar, from whence the square of the radius of the circle sought is composed.

This is the method of investigating Baker's central rule, which is sufficiently easy and free from all cautions; and the only difference arises from hence, that I determine the centre of the circle by the axis, and Mr. Baker by a parallel to the axis; and that I always find the affirmative roots on the right side of the axis, which he has sometimes on one side and sometimes on the other.

As to cubic equations, they are to be reduced to biquadratics, before they can be constructed by the same general rule; which is done by multiplying the proposed equation by its root z, whence arises a biquadratic equation, wanting the last term r; therefore taking away the 2d term, and finding the centre E, the line E O is the radius of the circle; viz. since $ar = 0$, the whole 5th term, in the new equation, arises from the taking away of the 2d term. As for

Exam. 2. If $z^3 - bz^2 + apz + a^2q = 0$. This multiplied by z, it becomes $z^4 - bz^3 + apz^2 + a^2qz = 0$. To take away the 2d term, let $x + \frac{1}{4}b = z$; then it will be

$$\begin{aligned} x^4 + bx^3 + \tfrac{3}{8}b^2x^2 + \tfrac{1}{16}b^3x + \tfrac{1}{256}b^4 &= + z^4, \\ - bx^3 - \tfrac{3}{4}b^2x^2 - \tfrac{3}{16}b^3x - \tfrac{1}{64}b^4 &= - bz^3, \\ + apz^2 + \tfrac{1}{4}abpx + \tfrac{1}{16}apb^2 &= + apz^2, \\ + a^2qx + \tfrac{1}{4}a^2qb &= + a^2qz. \end{aligned}$$

In this new equation, the half co-efficient of the 3d term divided by a, viz. $-\frac{3b^2}{16a} + \frac{1}{2}p$, is to be substituted for $\frac{1}{2}p$; and the half co-efficient of the 4th term divided by a^2, the square of the parameter, viz. $-\frac{b^3}{16a^2} + \frac{bp}{4a} + \frac{1}{2}q$, instead of $\frac{1}{2}q$, in Descartes's construction; from hence the centre E is determined. Then drawing a parallel to the axis, at the distance of $\frac{1}{4}b$ on the left hand side, because $x + \frac{1}{4}b = z$, and let O be its intersection with the parabola; then a circle described with the centre E and radius E O, will either cut or touch the parabola in as many points as the equation has true roots: which roots, or z, are perpendiculars drawn from these points to the parallel to the axis, the affirmative on the right side, and the negative on the left.

If the 3d or 4th term, or both, be wanting in the equation, in investigating the central rule, there is no manner of difference at all to be observed; but the quantity p or q being wanting, those parts of the lines C D, and D E, somehow deduced from that quantity, will be wanting too; and we are to proceed with the remaining co-efficients of the third and fourth terms in the new equation, according to the method prescribed in the preceding examples.

Hitherto Mr. Baker's general method has been considered, than which none more easy and expeditious can be expected, assuming for the construction either a parabola, or any other curve, viz. when the equation rises to a biquadratic: but while I was writing these things, I hit upon a geometric effection of the central rule, which is expeditious beyond what can be hoped for, and will sufficiently satisfy the curious in those matters. Having described the parabola N A M, fig. 8, pl. 10, whose vertex is A, axis A B C, and parameter a, let the equation be reduced to this form $z^4.\ bz^3.\ apzz.\ aaqz.\ ar = 0$; or to this, if it be only a cubic, $z^3.\ bz^2.\ apz.\ aaq = 0$; then at the distance B D $= \frac{1}{4}b$ let the line D H, meeting the parabola in D, be drawn parallel to the axis to the left if it be $-\ b$, and to the right if $+\ b$; and from D let fall the perpendicular BD to the axis. In the line AB, produced towards B, make BK = $\frac{1}{2}a$, and draw the infinite line DK. Further, let KC be = to 2 AB, always in the axis produced beyond K; and if the quantity p have the sign —, take towards the same part CE = $\frac{1}{2}p$, or towards the contrary part, if it be + p, and from the point E let the perpendicular EF be erected to the axis, (or from the point C, if the quantity p be wanting) meeting the line DK in the point F, produced if needful, which is the centre of the circle sought, if the quantity q be wanting; but if q be in the equation, then take, in the line FE produced, if need be, the line FG = $\frac{1}{2}q$, to the left hand, if it be + q, but to the right, if — q; and the point G will be the centre of the circle proper for the construction; and its radius, if the quantity r be wanting, that is, if it be only a cubic equation, will be the line GD; the square of which, in biquadratic equations, is to be augmented by the addition of the rectangle under r, and the Latus Rectum, if it be — r; or to be diminished by the same rectangle, if it be + r. The circle being thus described, and perpendiculars being let fall on the line DH, from its intersections with the parabola; those on the left, as NO, always denote the negative roots of the equation, and those on the right, the affirmative.

Cubic equations are otherwise, and something more simply, constructed, according to Schooten's rule, in which also the roots are referred to the axis; but because the author himself neither explains the method of investigation, nor the demonstration, it will not be improper in this place to show the foundation of it, and at the same time render the geometrical construction more elegant, and clear it of those cautions, with which it is incumbered. This rule is derived hence, viz. that all cubic equations may be reduced to biquadratics, in which the second term is wanting; and this is done, by multiplying the proposed equation into $z - b = 0$, if it be + b in the equation; or into $z + b = 0$, if it be — b; and the new equation thus produced, will have the same roots with the cubic; and besides, another = to — b, if it be — b in the equation; or contrariwise. Let the equation, $z^3 - bz^2 + apz + aaq = 0$, be proposed to be constructed; this, multiplied into $z + b$, becomes

$$\begin{aligned} z^4 - bz^3 + apz^2 + a^2qz. \\ + bz^3 - b^2z^2 + abpz + a^2bq. \end{aligned}$$

Here now the second term is wanting, and the co-efficient of the third term, $-bb + ap$, gives $-\frac{bb}{2a} + \frac{1}{2}p$, instead of $\frac{1}{2}p$ or CD in Descartes's construction; and from half the co-efficient of the fourth term is made $+\frac{1}{2}q + \frac{bp}{2a}$ instead of $\frac{1}{2}q$ or DE: therefore the centre of the circle sought is determined; and because one of the roots of the new equation is given, viz. $\mp b$, a point in the circumference will be also given, and consequently its radius. Lastly, having described the circle, perpendiculars let fall from its intersections with the parabola, to the axis, will give the roots of the equation, both affirmative and negative, in the same manner as above.

Now the centre of the circle is investigated by a very easy construction, which is to be preferred to all others in cubics. Let A, fig. 9, pl. 10, be the vertex, and A F the axis of the described parabola A M D; at a distance equal to b, let D K be drawn parallel to the axis, to the right hand, if it be $+ b$ in the equation; and to the left, if it be $- b$, which may meet the parabola in the point D; on the centres D and A, with equal radii, describe on both sides two faint arches, intersecting each other, and through the points of intersection draw the indefinite line B C, a perpendicular to the middle of the supposed line A D, and which may meet the axis in the the point E; from E set off E F $= \frac{1}{2} p$ downwards, if it be $- p$ in the equation, but afterwards towards A, if it be $+ p$; and from F (or E if p be wanting) erect the perpendicular F G, meeting the line B C in G; and in G F produced, take G H $= \frac{1}{2} q$, to the right hand, if it be $- q$ in the equation; but to the left, if $+ q$, and H will be the centre sought, and H D the radius of the circle, which, letting fall perpendiculars upon the axis from its intersections with the parabola, will show all the roots, as L M, of the equation.

On a new kind of Magnetical Compass, with several curious Magnetical Experiments. By M. de la Hire, of the Royal Academy of Sciences at Paris, N° 188, p. 344.

There is nothing which creates so much trouble in long voyages on the sea, as the variation of the magnetical needle, both because this variation is different in different places, and because in the same place it changes considerably in process of time. It seems that if we had exact observations of the irregularities of this variation made all over the earth, and at considerable intervals of time. we might discover some period of this motion, and establish a system which might be of great use in navigation. But since our oldest observations were made but about 100 years since, and in some particular places only, they only serve to show, that if there be a regular motion, it must needs be very slow; so that we can conclude nothing certain for the time to come from all that has been hitherto observed. This is not because of any difficulty that there is in ascertaining this variation by observation, since it is found to change but few minutes in a year; but too much reliance must not be laid upon the observations of pilots, by reason of the gross errors which it is not easy for them to avoid. For it often happens that near the place where the compass is, there is much iron, which draws the needle, and causes it to show a point on the horizon much different from what it would, were it farther from the iron: which makes it appear as if there is a considerable variation, where perhaps there is none at all. We cannot therefore hope to be secure of any thing from the observations we have at present, and especially from those made at sea, which are the most considerable. This put me upon finding out some means, independent of observations, to discover the variation at sea; but having considered that several learned men of this age had proposed divers ways of making magnetical needles, which should not be subject to variation, and that all these projects had failed of effect; I judged that after all that they had done by means of the loadstone, it was not to be hoped to draw any further advantage from it, since the stone itself, as far as might be guessed from the experiments hitherto made, was subject to the same variation.

I had quite given over this inquiry, when there accidentally came to my hands a terella, or spherical loadstone, of 3 inches diameter, with which trying some experiments, with a small needle, the foot of which might easily be placed upon the stone, I soon observed what has been already noted by several, viz. that this globe of magnet caused the needle to have the same changes as are found in the compass in different parts of the world, as well in respect of the direction towards the two poles, as of the inclination towards that which is next it; and on trial I satisfied myself that it was not possible to find the point where the needle would stand indifferently in any position, which point would have exactly showed me the pole of the stone; but that the needle, however placed, always directed itself one certain away. I determined by this means, as well as I could, the point called the south pole; but I was much surprised to find it 18° distant from a cross deep engraven on the stone, which probably had heretofore been the pole of this stone, as it had been observed by the person who cut it. This change of the poles of this stone having revived my former thoughts concerning the variation of the needle, I believed that if it were true that the poles of the magnetical virtue changed in the loadstone, as we see they change in the earth, we might hence derive great advantages, as to the variations of the magnetical needle. For if this change of the poles in the loadstone were certain, and that it was analogous to the change of the poles of the magnetic virtue of the earth, it is not to be doubted but a terella, being suspended at liberty, would remain immoveable, and that one point thereof would regard the pole of the world, which might be called the true pole of the stone, while the poles of its virtue would pass successively from one part to another, after the same manner as they change in the earth.

Having well considered this hypothesis, and cleared up some doubts which I had concerning the position of the stone at the time when its pole had formerly been determined; I concluded that this former pole was distant from the point I call the true pole 13° towards the east, in the place where it had been marked, and which is unknown to me, since that at this time, in this country, the needle varies about 5° westward.

On this hypothesis I have invented a new sort of needle for the compass, which may have the same alterations as a spherical loadstone, and at the same time the same conveniences as the ordinary needle. I caused a ring of 3 inches diameter to be made of steel wire, from which issued three radii of very fine brass wire, meeting at the centre in a cap perfectly like that of an ordinary compass, that so this circle might rest on a pin in its centre, and be at full liberty to turn round, its centre being fixed. This done, I gave the magnetical touch to this steel ring, by applying indifferently to a point of it one of the poles of a strong loadstone, and the other pole of the stone to the opposite point, to give the greater virtue to the ring. The ring then became strongly magnetical, and the point called the south pole readily turned itself towards the north, and after several vibrations stopped there; and it had also the same inclination towards the pole which is found in needles after they have been touched. Lastly, I fixed upon the ring a small fleur-de-lis of brass, in the point which exactly respected the north, the ring being first well settled.

If the poles of the magnetic virtue in the loadstone change after the same manner as they do on the earth, it seems likely that the same thing would happen to this ring, and that one point thereof should always exactly respect the north. But to inform myself if a steel ring had the same effects as a terrella, I made the following experiment: having touched a steel ring and laid it on a paper, I strewed the filings of steel upon it; and then gently shaking the paper, I saw that the direction of the magnetical matter passed directly cross the ring from one pole to the other, and that there were two vortices on the sides, as it is observed in the spherical magnet, which seems very surprising; for according to the common hypothesis of the magnet, the magnetical virtue passing more easily in the steel than in the air, should run on both sides of the pole round the ring, and only form a pole opposite to the first. But I was further confirmed in this opinion by applying a flat and pointed piece of iron, like the blade of a knife, to a loadstone, so as the point of the iron reached beyond the stone; and having afterwards presented this point to the magnetical ring, I observed that different points of this ring applied to the point of the iron, according as the several parts of it had been applied to the stone; which happens not in the magnetical needle, for that always presents one of its ends to the point of the iron, being not disposed, by reason of its length, to receive the magnetical matter in all the parts thereof analogous to those of the stone. It must only be noted, that in an irregular stone the magnetical virtue appears stronger towards the angles than in the other parts, which may cause some irregularity in this experiment, if it be tried with a stone that is very uneven.

These experiments gave me the curiosity of making another, by touching two semicircles of steel. Having joined the two ends touched by the same poles, I observed by the steel-dust the same effect as in the ring. But having joined the ends differently touched, I found that immediately the two half rings run together and adhered to each other; and by the steel-dust strewed on paper, I observed that there were four vortices, one in the middle of each semicircle, and one at each of the places where they joined, and that the two latter were less than the others, and much stronger. I saw likewise that there were four poles, each of which was within a vortex, and that each retained in its semicircle the virtue of the ends of the half-rings.

Having touched a steel wire that was straight, I tried to make a ring of it; but I found that it had quite lost its virtue, which cannot be attributed to the junction of the poles, since they ought to adhere together, according to the other experiments which have been made; but only to what has been already noted, that when a magnetical virgula is a little bent, it loses its virtue, which cannot happen but from the alteration in the pores of the steel.

I further remarked, that a ring of steel touched, for a long time retains its virtue, though it be put in a position contrary to its poles. And this experiment is confirmed by another, much more considerable, which is, that a ring of steel, touched with a strong loadstone, cannot without difficulty receive a contrary touch from a magnet less strong than the first; but that in time by degrees it resumes its former virtue, much as we see magnets do, which being applied to another stone, by the poles of the same denomination, lose their first virtue, and take the contrary; which they afterwards lose by degrees, to re-assume the first.

After having presented this new system of the magnet to the Academy, there were made some experiments on a terrella, of much the same diameter with mine, but whose poles were not diametrically opposite; and on a half globe, much larger than the terrella. But no considerable difference or alteration of poles were found in Paris, April 26, 1687.

This letter having been read before the Royal Society, it was ordered that that the terrella, which has been in their repository these 25 years, the gift of their royal founder king Charles II. should be examined, to see if there be any sensible alteration in its poles: and on trial it was found, that the points which are marked on it with crosses, were as near as could be discerned the true poles of the stone; notwithstanding that the variation has changed at London full 4 degrees since this terrella has been in the society's custody; and perhaps many more since it was marked: and had there been a similar change in the poles of the load-stone, it must needs have been perceived in this, whose diameter is about $4\frac{1}{2}$ inches. However, to put this matter past dispute, care was taken to find out exactly, and mark the poles of the society's great load-stone, the sphere of whose activity is above 9 feet radius, and whose poles are 13 inches asunder, by which, if this translation of the poles be real, it cannot fail of being made very sensible in future times. As to the supposition that the points in which the iron has received the magnetical virtue may change place, after the same manner as the poles of the earth's magnetism are observed to do; though it was considered as an ingenious hint, and worth prosecution, yet some of the company, well skilled in magnetics, were of opinion, rather that such a circular needle would librate on its centre, so as to respect the magnetical meridian with the points that had at first received the touch, than that the ring remaining immoveable, the directive virtue in it should be transferred from place to place, either by length of time, or by transporting this compass into those parts where the variation of the needle is considerably different.

On the great Effects of a new Burning Speculum, lately made in Germany. N° 188, p. 352.

The outer circle of this concave burning speculum is near 3 Leipsic ells in diameter, and was made of a copper plate scarcely twice as thick as the back of an ordinary knife; and may therefore be easily removed from place to place, and ordered for use. The polish of it is very good, and represents by distinct reflections, all those appearances which arise from its concave figure; representing a dwarf like a giant, or the head or other part of a prodigious magnitude. The eye being placed nearer the speculum than its focus, all objects are seen within it, in an erect posture, and as at a great distance; but the eye being farther off than the focus, all things appear inverted, and without the speculum: and because the focus is 2 ells off, it is pleasant to see objects distinctly as it were hanging in the air; and if a sword be drawn against the speculum, a spectator not used to such optical delusions, would be apt to be frightened, imagining a pass to be made at his face.

The force of this speculum in burning is incredible. For, 1. A piece of wood, put in the focus, flames in a moment, so as a fresh wind can hardly put it out. 2. Water, applied in an earthen vessel, presently boils; and the vessel being held there some time, the water evaporates all away. 3. A piece of tin, or lead 3 inches thick, as soon as it is put in the focus, melts away in drops, and held there a little time is in a perfect fluor, so as in 2 or 3 minutes to be quite pierced through. 4. A plate of iron or steel, placed in the focus, is immediately seen to be red hot; and soon after a hole is burnt through. 5. Copper, silver, and the like, applied to the focus, melt, in 5 or 6 minutes. 6. Things not apt to melt, as stones, brick, and the like, soon become red hot like iron. 7. Slate at first is red hot, but in a few minutes turns into a fine sort of black glass, of which if any part be taken in the tongs, and drawn out, it runs into glass threads. 8. Tiles which had suffered the most intense heat of fire, in a little time melt down into a yellow glass, as do, 9. Pot-shreads, not only well burnt at first, but much used in the fire, into a blackish-yellow glass. 10. Pumice-stone, said to be that of burning mountains, in this solar fire, melts into a white transparent glass. 11. A piece of a very strong crucible put in the focus, in 8 minutes was melted into a glass. 12. Bones turned into a kind of opake glass, and a clod of earth into a yellow or greenish glass.

It was tried what effect the beams of the full moon, concentred with this speculum, would have, at the time when she was at her greatest altitude; but there was not found any degree of heat, though the light was not a little increased.

Mr. Hook was of opinion, that if such a speculum were made of many feet diameter, its effects must needs be prodigious; and might be of great use in perfecting the art of pastes or factitious jewels, which require the most intense degree of heat, to bring them to an exact mixture. He conceives such a one might be made very large for a small price, being hammered out of a copper plate, and tinned over with a mixture of tin, lead, and tin-glass, which is found to bear a very good polish.

Account of some Saxon Coins found in Suffolk. Communicated by Sir P. S. R. S. Soc. N° 189, p. 356.

In May 1687, at Honedon, near Clare in Suffolk, as the sexton was digging a grave in the church-yard, he met with a skull; and near it his spade broke a yellow earthen pot, wherein were many silver pieces of Saxon money, the inscriptions on which are so various, that there are scarcely any two alike, though they are generally of the same size, viz. that of a silver groat, and about the same weight. I guess this variety of inscriptions arises from the many masters of the mint who were appointed to coin money in several places, and who might each of them have a different stamp: and I find this conjecture countenanced by a passage in king Æthelstan's laws, printed by Lambard.

These Saxon coins were, denarii, or pennies; for Greaves, of the denarius, p. 117, says, in Ethelred's time it was the 20th part of the silver ounce troy, and bigger than three of our present pennies; and our goldsmiths weigh by this penny-weight or 24 grains. Five of the Saxon pennies made a shilling, and therefore 48 of those shillings made a pound, and 240 pennies made a pound; which is the present proportion of our penny and pound, though the intrinsic value be about three to one different.

Sir Henry Spelman, in his Glossary, speaks of sterling and denarius to be the same; and he directs to the statute made an. 1302, 31 Edw. I. wherein the penny is called sterling, and the weight of the sterling is 32 grains of dried wheat; and I have weighed 32 grains of wheat, and they are equal to 24 grains troy-weight, which is our Saxon penny. And an. 1496, 12 Hen. VII. cap. 5, there is another statute, wherein the sterling is of the same weight.

I am credibly informed, that some of Egbert's and Ethelbert's coins were found amongst them: these I saw, were Æthelstan's, who began his reign about the year 925. Edmund Etheling's his brother, (for I take the Edmund's to be his), who began his reign 940; Edred, another brother, who began his reign 946.

An Estimate of the Quantity of Vapour raised out of the Sea by the Warmth of the Sun; derived from an Experiment shown before the Royal Society, at one of their late meetings: by E. Halley. N° 189, p. 366.

That the quantity of aqueous vapours contained in the medium of the air, is very considerable, seems most evident from the great rains and snows which are sometimes observed to fall, to that degree, that the water thus discharged out of the interstices of the particles of air, is in weight a very sensible part of the incumbent atmosphere: but in what proportion these vapours rise, which are the sources not only of rains, but also of springs or fountains, as I design to prove, has not, that I know of, been any where well examined, though it seem to be one of the most necessary ingredients of a real and philosophical meteorology; and as such, to deserve the consideration of this honourable society. I thought it might not be unacceptable, to attempt by experiment, to determine the quantity of the evaporations of water, as far as they arise from heat; which, upon trial, succeeded as follows.

We took a pan of water, about 4 inches deep, and $7\frac{9}{10}$ inches diameter, in which was placed a thermometer, and by means of a pan of coals, we brought the water to the same degree of heat, which is observed to be that of the air in our hottest summers; the thermometer nicely showing it. This done, we affixed the pan of water, with the thermometer in it, to one end of the beam of the scales, and exactly counterpoised it with weights in the other scale, and by the application or removal of the pan of coals, we found it very easy to maintain the water in the same degree of heat precisely. Doing thus, we found the weight of the water sensibly to decrease; and at the end of 2 hours we observed that there wanted half an ounce troy, all but 7 grains, or 233 grains of water, which in that time had gone off in vapour; though one could hardly perceive it smoke, and the water not sensibly warm. This quantity in so short a time seemed very considerable, being little less than 6 ounces in 24 hours, from so small a surface as a circle of 8 inches diameter. To reduce this experiment to an exact calculus, and determine the thickness of the skin of water that had so evaporated, I assume the experiment alleged by Dr. Edward Bernard to have been made in the Oxford society, viz. that the cubic foot, English, of water, weighs exactly 76 pounds troy; this divided by 1728, the number of inches in a foot, gives $253\frac{1}{3}$ grains, or $\frac{1}{2}$ ounce $13\frac{1}{3}$ grains, for the weight of a cubic inch of water; wherefore the weight of 233 grains is $\frac{233}{253}$ or 35 parts out of 38 of a cubic inch of water. Now the area of the circle, whose diameter is $7\frac{9}{10}$ inches, is 49 square inches; by which dividing the quantity of water evaporated, viz. $\frac{35}{38}$ of an inch, the quotient $\frac{35}{1862}$ or $\frac{1}{53}$ shows, that the thickness of the water evaporated was the 53d part of an inch: but we will suppose it only the sixtieth part, for the facility of calculation. If therefore water as warm as the air in summer, exhales the thickness of a 60th part of an inch in 2 hours from its whole surface, in 12 hours it will exhale the $\frac{1}{10}$ of an inch; which quantity, will be found abundantly sufficient to serve for all the rains, springs, and dews, and account for the Caspian seas being always at a stand, neither wasting nor overflowing; likewise for the current said to set always in at the Straits of Gibraltar, though those Mediterranean seas receive so many and so considerable rivers.

To estimate the quantity of water arising in vapour out of the sea, I think I ought to consider it only for the time the sun is up, for that the dews return in the night, as much, if not more, vapours than are then emitted; and in summer the days being longer than 12 hours, this excess is balanced by the weaker action of the sun, especially when rising, before the water be warmed: so that if I allow $\frac{1}{10}$ of an inch of the surface of the sea to be raised per diem in vapours, it may not be an improbable conjecture.

Upon this supposition, every 10 square inches of the surface of the water yields in vapour per diem a cube inch of water; and each square foot half a wine pint; every space of 4 feet square, a gallon; a mile square, 6914 tons; a square degree, supposed of 69 English miles, will evaporate 33 millions of tons; and if the Mediterranean be estimated at 40 degrees long and 4 broad, allowances being made for the places where it is broader by those where it is narrower, (and I am sure I guess at the least,) there will be 160 square degrees of sea; and consequently, the whole Mediterranean must lose in vapour, in a summer's day, at least 5280 millions of tons. And this quantity of vapour, though very great, is as little as can be concluded from the experiment produced. And yet there remains another cause, which cannot be reduced to rule, I mean the winds, whereby the surface of the water is licked up sometimes faster than it exhales by the heat of the sun; as is well known to those who have considered those drying winds which blow sometimes.

To estimate the quantity of water the Mediterranean sea receives from the rivers that fall into it, is a very hard task, unless we had the opportunity to measure their channels and velocity; and therefore we can only do it by allowing more than enough; that is, by assuming these rivers greater than in all probability they are, and then comparing the quantity of water voided by the Thames, with that of those rivers whose water we desire to compute.

The Mediterranean receives these considerable rivers, the Iberus, the Rhone, the Tiber, the Po, the Danube, the Neister, the Borysthenes, the Tanais, and the Nile, all the rest being of no great note, and their quantity of water inconsiderable. These nine rivers, we will suppose each of them to bring down 10 times as much water as the river Thames, not that any of them is so great in reality, but to comprehend with them all the small rivulets that fall into the sea, which otherwise I know not how to allow for.

To calculate the water of the Thames, I assume that at Kingston bridge, where the flood never reaches, and the water always runs down, the breadth of the channel is 100 yards, and its depth 3, it being reduced to an equality; in both which suppositions I am sure I take with the most. Hence the profile of the water in this place is 300 square yards; this multiplied by 48 miles, which I allow the water to run in 24 hours, at 2 miles an hour, or 84480 yards, gives 25344000 cubic yards of water, to be evacuated every day; that is, 20300000 tons per diem; and I doubt not but in the excess of my measures of the channel of the river, I have made more than sufficient allowance for the waters of the Brent, the Wandel, the Lea, and Darwent, which are all that are worth notice, that fall into the Thames below Kingston.

Now if each of the aforesaid 9 rivers yield 10 times as much water as the Thames does, it will follow that each of them yields but 20300000 of tons per diem, and the whole 9 but 1827000000 of tons in a day; which is but little more than $\frac{1}{3}$ of which is proved to be raised in vapour out of the Mediterranean in 12 hours time. Now what becomes of this vapour when raised, and how it comes to pass that the current always sets in at the mouth of the Straits of Gibraltar, is intended, with leave, for a farther entertainment of this honourable company; in the mean time it is needful to advertise the reader, that in making the experiment herein-mentioned, the water used had been salted to the same degree as is the common sea-water, by the solution of about a 40th part of salt.

Some Observations of a Solar Eclipse, May 1, 1687, O. S. made in several Places. N° 189, p. 370. *Translated from the Latin.*

This eclipse, though a small one, and could not be perceived by the naked eye, yet it seems to be very convenient for the accurate determination of the moon's parallax and latitude. The observations were the following:

1. At London, separately by Messrs. Hook and Halley. Because of the oblique incidence of the moon, the moment of the beginning could not be rightly determined: but at 1h. 16m. the eclipse was very sensibly begun. The middle of the eclipse was about 1h. 40m. The chord of the part eclipsed, or that between the horns, was 9′ 30″, answering to an arch of 30°; but in the diameter only 1′ 30″. The end exactly at 2h. 3m.

2. At the Royal Observatory, Greenwich, by Mr. Flamsteed. At the middle, the chord of the eclipsed part was 9′ 54″. The end 2h. 4m. 15s.

3. At Totteridge, near London, Mr. Haines saw the end at 2h. 2m. The quantity half a digit from the south.

4. At Bridgetown, Barbadoes, Mr. Frank found the end at 7h. 56m. 45s. A. M. He judged the greatest quantity to be 2 digits from the south.

5. At Nuremburg, by J. P. Wurtzelbaur. The beginning 1h. 58½m. The middle at 2h. 36½m. when the quantity was 2 digits. The end 3h. 13m. 33s.

6. At Ulm, in Suabia, by M. Honold. The beginning at 1h. 48m.; the greatest quantity 2⅓ digits. The end 3h. 16m.

7. At Leipsic, by M. Kirck, the eclipse was observed at 2h. 20m. 10s.; at 2h. 47½m. the digits were about 1⅓. The end exactly at 3h. 15m.

8. At Breslaw, in Silesia, D. G. Schultz observed the greatest obscuration rather before 3h. 12½m.; the digits were 1½. The end at 3h. 37m.

Memoirs for a Natural History of Animals; containing the Anatomical Descriptions of several Creatures, dissected by the Royal Academy of Sciences at Paris. Translated by Alexander Pitfield, Esq. R. S. Soc. To which is added an Account of the Measure of a Degree of a great Circle of the Earth. Published by the same Academy, and englished by Richard Waller, Esq. R. S. Secr. N° 189, p. 371.

This book, containing the anatomical observations of 28 species of animals, and about 70 individuals, was published in two very large folios, by the Royal Academy at Paris, and owned by them as their united labours, as a body. The difficulty of procuring copies of the French edition, few of the learned having ever seen the book, though printed some years since, was no small inducement to this translation.

Of the first 12 species of animals, viz. two lions and a lioness, a camelion, a dromedary, a bear, five gazellas or antelopes, a pard cat, a sea fox, a castor, an otter, two civet cats, an elk, and a coati mondi, a large account has been already given by Mr. Oldenburg, in the Philosophical Transactions, N° 49 and 124.* Some account of the 16 remaining species, all published in the 2d vol. of the French edition, are as follows:

The 13th species then is the sea calf, which is of two kinds, the larger from the ocean, the lesser from the Mediterranean, of which sort this was. The epiglottis was much larger than in other animals; its ventricle like an intestine; it had all the organs for secretion of urine, and the kidneys seemed composed of several glands, each provided with a particular pelvis; it had lungs like other amphibious animals, and the foramen ovale giving passage to the blood from the cava to the aorta; the crystalline more convex before than is common; and several particularities in the formation of the eye, favouring the opinion of the reception of the visual species on the retina.

The 14th, the Barbary cow, an animal something resembling a deer, it had but two teats, four ventricles, like other ruminating animals, a very large cæcum, and no distinct lobes in the liver. It was in several particulars like the common cow.

The 15th is the cormorant, wherein the shortness of the legs is remarkable, and structure of the feet, for swimming with one foot, while the other holds the prey; the largeness of the œsophagus, want of the two cæcums, found in most birds; the kidneys separated from the other viscera by a particular membrane, the tongue and eye very small, this water fowl being to feel for its food under water, rather than discover it from afar.

The 16th, the chamois or rupicapra, in whose ventricle a ball was found, whence they take occasion to discourse of the balls found in the stomachs of creatures, as cows, horses, &c. and observe that they are composed of lignous fibres and not hair, as is usually thought; besides several other observables, the cornua uteri were very long and winding; the heart had a callous apophysis, &c.

The 17th and 18th are the porcupine and hedgehog, a comparison being made between these two animals. They observe the external ear of the porcupine to be like a man's; the end of the tongue armed as it were with teeth; the

* Vol. i. p. 369, and vol. ii. p. 289, of this Abridgment.

skin provided with an extraordinary muscle for erecting the quills. The musculus carnosus of the hedgehog serves to bring the head round into the breech like a foot ball; and whereas in the porcupine the cæcum was very large, in the hedgehog there was none at all; the epididymis, in the porcupine, was separate from the testis; in the hedgehog united to it; in the hedgehog was a large crystalline filling almost the whole globe of the eye.

The 19th are four monkeys. In general it is observed that this animal more resembles a man in his outward shape than inward formation of the parts, which in many things are like a dog; the genital parts of the male like neither; of the female much like woman; the anfractuosities of the brain like man's, but the processus mammillares were hard and membranous, which they are not in man; the muscles very much resemble those of men.

The 20th is the stag of Canada and the Sardinian hind. In the stag, the length of the intestines is observable, being in all 96 feet; and indeed, generally all grazing animals have long guts. In the hind, the four ventricles were more distinguishable than in the stag; the cornua uteri long and winding, as in the chamois; in the trunks of the jugulars were found 16 valves, which were in situation contrary to the circulation of the blood. In the carotides were observed several transverse incisures.

The 21st, ten pintadoes. Several parts are like the common hen; the pancreas wanting; the bladders in the lower belly were raised by blowing into the aspera arteria, whence they hint at the use of respiration.

The 22d, three eagles. The intestines, after the usual manner of voracious animals, were slender and short, as also the kidneys; some had the cæcum, others none; the globe of the eye was large, and the cornea very prominent. In this subject they first discovered that the spinal marrow in the middle of the back was divided in two, with a ventricle like those in the brain between; this was afterwards found common to all birds.

The 23d, two Indian cocks, not our Turkey cocks. In one there were two pancreases, with three choliducts, and two pancreatic ducts into the intestine, in the other was but one pancreas and a single duct; the intestines were 12 feet long, and the cæcum 6; the aspera arteria made a fold in the craw-bone, after a most particular manner.

The 24th, six bastards; in which the craw was scarcely distinguishable from the œsophagus, and furnished with a great number of glands most conspicuous in this, but to be found in most birds; a third cæcum near the rectum or the ursa Fabritii, between the cornea and sclerotica, a cartilaginous circle was observed.

The 25th, six demoiselles of Numidia, a kind of crane, in which they found the liver very large, and without gall bladder in some subjects. In the female a kind of gland, besides the ovary, resembling the testicles of the male. Amongst other observables, the structure of the wind-pipe was very unusual, entering with a winding into the bone of the sternum, at its union with the lungs it had a kind of larynx; the punctum lachrymale in the eye was double, &c.

The 26th, eight ostriches, in which they very largely discourse on the structure of the feathers of birds. The foot of this animal seems contrived for a speedy course, in which its wings are of great use; the different length of the intestines is observable, in some being 50, whereas in another they were but 29 feet; the cæcum, which was double, was wreathed like a screw, and the inside of the colon provided with valves or semilunar leaves, like membranes. At the extremity of the rectum was found a bladder filled with urine. In this description they discourse largely of the uterus and genital parts of birds, as likewise of the lungs, and its divisions or diaphragms, and its communication with the bladders containing the ventricle and intestines, with the manner and use of breathing in birds, explaining it by a pair of double bellows, &c.

The 27th, the cassowary, a bird but lately known to the Europeans; it has no quills or feathers for flying, and indeed but short wings; it is without the musculous gizzard, though a granivorous animal, which might in some sort be supplied by the number of ventricles.

The 28th, or last, is a very large land tortoise, being $4\frac{1}{2}$ feet from the extremity of the head to the tail. Among the internal parts, the structure of the urinary bladder is very curious for its exterior tunicle, being membranous; the inside strengthened with an infinite number of musculous fibres, not unlike those in the ventricles of the hearts of animals. This contrivance seems necessary for the pressing out of the urine in this animal, which has an unyielding belly, not capable of compression; nor was the formation of the heart less observable; it had three ventricles, communicating with each other by holes in the septum; the vena cava had two branches into two of the ventricles, which likewise received blood from two venæ pulmonares, to be transmitted to the aorta, &c.

Confucius Sinarum Philosophus, sive Scientia Sinensis Latine exposita, Studio et Operâ Patruum Societatis Jesu, &c. Adjecta est Tabula Chronologica Sinicæ Monarchiæ ab hujus Exordio ad hæc usque Tempora.* N° 189, p. 376.

The famed Chinese philosopher Cum-fu-cu, or as we call him Confucius,

* This philosopher, whose memory is so justly venerated by the Chinese, flourished about 500 years before the birth of Christ. He was a mandarin, and was employed in the government of the kingdom of Lou. Observing great licentiousness in the manners of the court, and excessive abuses in the administration of public affairs, he projected a plan of reform both in religion and legislation; but endeavouring in vain to prevail with the emperor to adopt it, he resigned his ministerial situation,

being in so great esteem in his own nation, and having never yet appeared in an European dress, could not but be gratefully received by the curious, especially since the version is performed by very ancient missionaries sufficiently accomplished in the knowledge of the Chinese character, and at the command of the king of France.

The subject of this book consists chiefly of moral and political precepts and apophthegms of that philosopher, whose memory is still precious in China; so that in respect to him, his posterity, after above 2200 years, enjoy certain great privileges, never granted but to the royal family: are exempt from all taxes, and whoever is advanced to the degree of doctor, gives, as a mark of his respect for the great Confucius, some present to the eldest of his family, who is now 68 generations removed from him. It is here stated that Confucius was born Anno 551 before Christ, and lived 73 years; so that he was contemporary with the most ancient Greek philosophers, and not long after Pythagoras.

Of the Chinese chronology, wonderful relations have been brought into Europe; this matter the author of this part of the book, P. Couplet, seems well to have examined, and to have sifted the credible from the fabulous. They begin their account with the years of the reign of king Fohi, who was the founder of their empire, about the year before Christ 2952. Fohi is said to have reigned 115 years, and to have invented the character now used in China, and his successor Xinnum is made to govern 140 years; these two kings are by our author, by reason of some manifest fables in their history, reputed doubt-

and devoted himself wholly to philosophy, delivering lectures on subjects of religion, morality, and politics. He made numerous converts, propagating his doctrines by travelling into various provinces, and still further promoting the diffusion thereof by missionaries, selected from among the best informed and most zealous of his disciples. He died in the 73d year of his age. He wrote the following treatises enumerated (from du Halde) by the compilers of the General Biographical Dictionary, viz. 1. Ta Hio, or the Grand Science, which treats of the care we ought to take in governing ourselves, that we may be able to govern others. It is chiefly designed for princes and grandees. 2. Tchong Yong, or the Immutable Mean; treating of the mean which ought to be observed in all things, and showing it to be the right path for attaining the summit of virtue. 3. Yun Lu, or the Book of Maxims; a collection of sententious and moral discourses. 4. Meng Tsee, (a work on political administration) or the Book of Mentius, the name of one of his disciples. 5. Hiao King, i. e. of Filial Reverence. 6. Sias Hio, or the School for Children; a collection of sentences and examples from ancient and modern authors. Of these treatises the first four are called the Canonical Books.

Confucius will ever rank high among philosophers for his political maxims and moral precepts; which, it must be allowed, excel those of the philosophers of antient Greece and Rome, in the genuine principles of equity and benevolence. The example which he sat by his own conduct, was not less excellent than his precepts. He was remarkably temperate and regular in his mode of living, unremitting in his assiduities as a teacher, and though high-born and gifted with a most superior understanding, so remote was he from pride and arrogance, so simple and condescending in his manners, and so communicative in his disposition, that he was beloved and venerated by his pupils and followers with a degree of enthusiasm, which showed that they regarded him rather as one who partook of a divine nature than as a human being.

ful; wherefore they, as from a more certain æra, choose to begin their annals with the third king Hoam-ti, and the year before Christ 2697. This Hoam-ti is said to have instituted the sexagenary cycles, or periods of 60 years, according to which this chronology is adjusted. Since this institution, there are now 73 periods elapsed, and the 74th is current; in which time they account that there has been 234 kings of China, sprung from no less than 22 several royal families; the king now reigning being the second of the race of the Tartars, who within these 50 years have thoroughly subjected China.

The third king, Chuen-hio, is said to be the author of the Chinese calendar, and to have appointed the beginning of the year to be on the new moon next the beginning of the spring, which the Chinese account to be when the sun is in 5° of Aquarius; this account is still in use, though instituted 2500 years before Christ. About 700 years after, the king Chim-tam reduced the beginning of the year to the winter solstice, but the former was restored about 100 years before Christ, and still continues.

The years of this account are luni-solar, or consisting of 12 lunar months, half of 30 days, and the rest of 29 days, with the intercalation of 7 months in 19 years; so that 7 years in each cycle have 13 months. This distribution of months was ordained by king Yao, about 2300 years before Christ, and is, if rightly intercalated, a more exact measure of the celestial motions than our Julian account, or old style, for that fails a day in 131 years; whereas this account of the Chinese (which is nearly the same with the Jewish) fails but a day in 225 years, or 4 days in 900 years. It is here said, that the famous wall of China, extending above 400 leagues, was begun by king Xi-Hoam-ti, about the year before Christ 210, to hinder the incursions of the Tartars, which in all ages have infested this country.

On the Numbers and Limits of the Roots of Cubic and Biquadratic Equations. By Mr. E. Halley. N° 190, p. 387. *Translated from the Latin.*

It appears that, both in cubic and biquadratic equations, the roots may be expounded by perpendiculars let fall upon the axis, or a given diameter of a given parabola, from the intersections of that curve with the circle; and since a circle, cutting a parabola, must necessarily intersect it either in four or in two points, it is manifest that in biquadratics there always are either two or four true roots, affirmative or negative; unless the circle happen to touch it, in which case the equality of two roots, of the same sign, is concluded. But in cubic equations, because one of the intersections is required for the construction, therefore either only one, or the three remaining roots denote one or three, as in the case of contact; whence it appears, that there are found two equal roots, and that the problem, from whence the equation results, is really a plane one. Therefore, all cubic equations, however affected, are explicable either by one or by three roots, and which are always possible, if we admit negative roots for true ones; so biquadratics, whose last term r is affected with the sign $-$; are explicable by two or four; but if it be $+r$ in the equation, and it be so great, that $\sqrt{GD^2 - ar}$, fig. 8, pl. 10, be less than that the circle, described with that radius, and on the centre G, can touch the parabola in some point, the given equation is altogether impossible, nor is it explicable by any affirmative or negative root; but of this more hereafter.

Since then there is so great a difference between the cases of cubic and biquadratic equations, that they cannot be comprised together; we shall first treat of cubics, and then of the others. Now cubic equations are constructed by an infinite number of circles in a given parabola; but biquadratics by one only, at least, by these methods, and because, putting $z - e$, or any other indeterminate, equal to nothing, a cubic equation is reduced to a biquadratic, having the same roots with the cubic; and besides another equal to e; whence it is, that a cubic equation may be constructed by as many different circles as we can imagine quantities, e, that is an infinite number; and of all these constructions, the easiest is that which I gave towards the end of my preceding discourse on the construction of cubic and biquadratic equations; yet the following is little inferior to it, which seems better accommodated to the design of determining the number of the roots and their limits, and which arises from the taking away of the second term, by putting, after the common manner, $x = z + $ or $- \frac{1}{3}$ of the co-efficient of the second term, and it is this: having given the parabola ABY, fig. 10, whose vertex is A, axis AE, and latus rectum a, reduce the equation to the usual form, viz. $z^3 .\ bz^2 .\ apz .\ aaq = 0$; then at the distance of $\frac{1}{3}b$, let BK be drawn parallel to the axis, to the right hand if it be $+b$, otherwise to the left, meeting the parabola in B; and let the indefinite line DP be raised perpendicular to the supposed line AB, meeting the axis in G. From the point B, let fall upon the axis the perpendicular BC, and let GE be always = to AC, and be set off towards the lower side. From E, set off EH $= \frac{1}{2}p$; upwards if it be $+p$ in the equation, but downwards if $-p$; and from the point H (or E, if the quantity p be wanting) let the perpendicular HQ be produced meeting the indefinite line DP in the point O. Lastly, in the indefinite line HQ, take OR $= \frac{1}{2}q$, to be set off from O to the right hand if it be $-q$, but to the left if $+q$; and a circle, described from the centre R, and with the radius RA, will cut the parabola in so many points as the proposed equation has true roots; and they will be the perpendiculars ZY let fall from the intersections Y to the line BK parallel to the axis; of which those on the right hand of the line BK are affirmative, and those on the left, negative.

The advantage of this construction consists in this, that it is performed by a circle passing through the vertex, in the same manner as if the second term were wanting; and therefore to determine the number of roots, it is sufficient to know the properties of the locus or curve, which distinguishes the space. wherein, if the centre of the circle, which passes through the vertex of the parabola, be placed, its circumference will intersect the parabola, either in one or three other points; that is, to define the nature of that curve, on which the centres of all the circles, passing through the vertex and then touching the parabola, do fall.

Now that locus is the paraboloid, which, with Dr. Wallis, we may call the semicubical, or in which the cubes of the ordinates are as the squares of the corresponding abscisses; whose latus rectum is $\frac{27}{8}$ of the latus rectum of the given parabola, and its vertex the point V, the line AV, being half the latus rectum of the same parabola. That is, if we put unity for the latus rectum of the given parabola, then $\frac{8}{27}$ of the cube of the ordinate will be equal to the square of the absciss; or the cube of $\frac{2}{3}$ VH = the square of HR, viz. if R be the centre of the circle which passes through the vertex of the parabola, and touches the same afterwards. This is that curve which our countryman Mr. Neil first of all demonstrated to be equal to a given right line; and which described on both sides of the axis of the parabola, as VNL, VPX, comprehends a space, in which, if the centre of the circle, which passes through the vertex A, be placed, it will cut the parabola in three other points; but the spaces more remote from the axis afford centres for circles, which will cut the parabola only in one point besides the vertex.

These things being well understood, we shall now proceed to determine the number of the roots. And first, let the second term be wanting, and let the latus rectum be 1, or AV = $\frac{1}{2}$; in the construction VH is = $\frac{1}{2}p$, and RH = $\frac{1}{2}q$; and since, if it be $+p$ in the equation, $\frac{1}{2}p$ is to be set off from V towards the upper parts, the centre of the circle is always without the space LVX; and therefore explicable by one root only, which is affimative if it be $-q$, negative if $+q$, which indeed are the roots found by Cardan's rule; but if it be $-p$, then VH = $\frac{1}{2}p$ is set off towards the lower parts; but it may be that HR may fall between the axis and the curve VX or VL, viz. if the cube of $\frac{2}{3}$ VH, or of $\frac{1}{3}p$, be greater than the square of $\frac{1}{2}q$, or $\frac{1}{27}p^3$ be greater than $\frac{1}{4}qq$; in which case there are three roots, two negative if it be $-q$, and one affirmative equal to the sum of the others; but if it be $+q$, then there are two affirmative roots and one negative; but if $\frac{1}{27}ppp$ be less than $\frac{1}{4}qq$, then there is but one root, affirmative if it be $-q$, negative if $+q$.

Now let all the terms be complete, and first let there be proposed, for example, this equation, $z^3 - bz^2 + pz - q = 0$, for which fig. 10 serves. In the construction of this, BC = $\frac{1}{3}b$, VG = $\frac{1}{2}$AC = $\frac{1}{18}bb$, VE = $\frac{1}{6}b^2$, VH = $\frac{1}{6}bb - \frac{1}{2}p$, GH = $\frac{1}{9}bb - \frac{1}{2}p$, or $\frac{1}{2}p - \frac{1}{9}bb$; hence HO = $\frac{1}{27}b^3 - \frac{1}{6}bp$, or $\frac{1}{6}bp - \frac{1}{27}b^3$, and

HR, or the distance of the centre R of the circle from the axis, is always the difference between $\frac{1}{6}bp$ and $\frac{1}{27}b^3 + \frac{1}{2}q$; which if they are equal, then the centre falls in the axis; if $\frac{1}{6}bp$ be greater than $\frac{1}{27}b^3 + \frac{1}{2}q$, then it falls on the left side of the axis, if less, on the right. If therefore the square root, or $\sqrt{ddd}$, of the cube of $\frac{2}{3}$ VH, that is, of $\frac{1}{9}bb - \frac{1}{3}p$, calling it d, be greater than HR, or the difference between $\frac{1}{27}b^3 + \frac{1}{2}q$ and $\frac{1}{6}bp$; the centre R is found within the space NPV, circumscribed by the paraboloids VPX, VNL, and the indefinite right line DNP; and therefore the circle will cut the parabola in three points Y, Y, Y, on the right side of the line BK, and so the equation have three affirmative roots. But the centre being without this space NVP, it is explicable by only one affirmative root. Here we are to observe, by the bye, that the right line DP touches the paraboloid VPX in the point P, EP being $\frac{1}{27}b^3$; but cuts the other paraboloid VNL in the point N, such that letting fall NF perpendicular to the axis, VF is $\frac{1}{4}$ of EV, or $\frac{1}{24}bb$, and NF $\frac{1}{108}b^3$. But VW, which, from the point V being raised perpendicular to the axis, meets the line DP in W, is $= \frac{1}{54}b^3$ or $\frac{1}{2}$ EP.

Hence we may safely conclude, that if in the equation, either p be greater than $\frac{1}{3}b^2$, or q be greater than $\frac{2}{27}b^3$, that there will be found only one root, and that an affirmative one. Cartes's rule then, p. 70, Edit. Amsterd. 1659, fails, in which he determines that there are always as many true roots, as there are changes of the signs + and — in the equation; though Schooten in his commentaries, would excuse this mistake; for innumerable more equations of the preceding form, with three changes of signs may be devised, which have rather one than three roots. Also Prop. 5, Sect. 5, of our countryman Harriot's Ars Analytica, and Prob. 18 of Vieta's Numer. Potest. Resol. is hardly founded; since, from the limitations they have there set down, that must agree to the whole parallelogram PIVW, which we have just now proved to agree only to the space NVP alone, that is, to afford a centre to the circle intersecting the parabola in three other points, besides the vertex.

But the quantity q, or the last term, (b and p given, so that p be less than $\frac{1}{3}b^2$) is accurately limited from the preceding equation $\sqrt{ddd} = \overline{\frac{1}{27}b^3 + \frac{1}{2}q}$ $\backsim \frac{1}{6}bp$; viz. when the circle touches the parabola; therefore $\frac{1}{2}q$ should be less than $\frac{1}{6}bp - \frac{1}{27}b^3 + \sqrt{d^3}$; but if p be greater than $\frac{1}{4}bb$, and that $\frac{1}{2}q$ should be greater than $\frac{1}{6}bp - \frac{1}{27}b^3 - \sqrt{d^3}$, then the centre does not fall in the space NVW: and under these conditions the equation will always be explicable by three roots; otherwise by one only. But whether there be three or one, they are always affimative, because of the position of the centre R to the right hand of the line DP.

And this is the most difficult case; so that those who understand what has been said above, will easily comprehend what follows. Now let the equation

$z^3 - bz^2 + pz + q = 0$ be given. In this case, that there may be three roots, the centre of the circle should be found somewhere within the space PNΔ, terminated by the right lines PN, PΔ, and the curve of the paraboloid NΔ; therefore, since EF is $= \frac{1}{4}bb$, p must be less than $\frac{1}{4}bb$; now to determine the quantity q, d being $= \frac{1}{9}b^2 - \frac{1}{3}p$ as before, $\sqrt{d^3} + \frac{1}{27}b^3 - \frac{1}{6}bp$ should be always greater than $\frac{1}{2}q$, that so the centre of the circle may be posited in the forementioned space PNΔ; which when it is so, such an equation has two affirmative roots, and one negative: but if p be greater than $\frac{1}{3}bb$, or $\frac{1}{2}q$ greater than $\sqrt{d^3} + \frac{1}{27}b^3 - \frac{1}{6}bp$, it is explicable only by one, and that a negative root.

Now let the equation $z^3 - bz^2 - pz - q = 0$ be proposed. That this equation may have three roots, the centre of the circle must be found somewhere in the indefinite space between the right line DPD and the curve of the paraboloid PX: the quantity p is not here liable to limitations; but $\frac{1}{2}q$ should always be less than $\sqrt{d^3} - \frac{1}{27}b^3 - \frac{1}{6}bp$, supposing d to be $= \frac{1}{9}bb + \frac{1}{3}p$: by this means there are given two negative roots, and one affirmative; but otherwise, if $\frac{1}{2}q$ be greater than $\sqrt{d^3} - \frac{1}{27}b^3 - \frac{1}{6}bp$, the equation is explicable only by one affirmative root. Fourthly, let the equation $z^3 - bz^2 - pz + q = 0$, be proposed, which has two affirmative roots and one negative, if the centre of the circle be found in the indefinite space between the right lines PΔ, PD, and the curve of the paraboloid ΔL; that is, (putting $d = \frac{1}{9}bb + \frac{1}{3}p$) if $\frac{1}{2}q$ be less than $\sqrt{d^3} + \frac{1}{27}bbb + \frac{1}{6}bp$; but if $\frac{1}{2}q$ be greater than this quantity, there is but one negative root.

Now the four remaining equations, in which we have $+ b$, do not differ from those that have been mentioned already, as to the limitation of the number of the roots, if the sign of the last term be changed, retaining the sign of the third term; but then the roots that were affirmative in the former will be negative here, and contrarywise. Thus, in the equation $z^3 - bz^2 + pz - q = 0$, the affirmative roots were either one or three; but in this equation $z^3 + bz^2 + pz + q = 0$, there is either one or three negative roots under the same conditions, but no affirmative root at all. So also in the equation $z^3 + bz^2 + pz - q = 0$, there are two negative roots and one affirmative, if p be less than $\frac{1}{3}bb$, and $\frac{1}{2}q$ be less than $\sqrt{d^3} + \frac{1}{27}b^3 - \frac{1}{6}bp$; just as in the equation $z^3 - bz^2 + pz + q = 0$, there were two affirmative roots and one negative root; but p and q exceeding those prescribed measures, there is only one affirmative root in this latter, which in the former was negative. After the same manner, in the equation $z^3 + bz^2 - pz + q = 0$, either there are two affirmative roots and one negative, or one negative only. Lastly, for the same reason, in the equation $z^3 + bz^2 - pz - q = 0$, there are two negative roots and one affirmative root, or one affirmative only; while, in the equation $z^3 - bz^2 -$

$pz + b = 0$, there were two affirmative roots and one negative root, or one negative only; viz. accordingly as $\frac{1}{2}q$ is either greater or less than $\sqrt{d^3} + \frac{1}{27}b^3 + \frac{1}{6}bp$.

If the third term, or pz, be wanting, the centre R always falls in the line IPEΔ; wherefore, if it be $z^3 - bz^2. * . - q$, or $z^3 + bz^2 * . + q$, there can be but one root, affirmative, if it be $- b$, negative if $+ b$. But if it be $z^3 - bz^2. * . + q$, or $z^3 + bz^2. * . - q$, there may be two affirmative roots and one negative in the former, or one affirmative and two negatives in the latter, the centre falling in the line PΔ between P and Δ, that is, if $\frac{1}{4}q$ be less than $\frac{1}{27}b^3$; but if it be greater, there can be only one negative root in the former, or one affirmative in the latter.

Hitherto we have fully considered the number of roots in cubic equations; it remains that we add something concerning the quantity of the roots. And here it is first to be noted, that every equation, having three roots, may be expeditiously resolved by means of the tables of sines, viz. by the trisection of an angle; that is, putting $\sqrt{\frac{4}{9}bb - \frac{4}{3}p}$, or $\sqrt{4d}$ = the radius of the circle, if it be $+ p$ in the equation, or $\sqrt{\frac{4}{9}b^2 + \frac{4}{3}p}$, if $- p$; and the angle trisected, will have its sine in the tables of sines $\frac{\frac{1}{27}b^3 + \frac{1}{6}bp + \frac{1}{2}q}{\sqrt{d^3}}$. This angle being found, the sine of its third part, as also the sine of the third part of its supplement to a semicircle, their sum will be given from the table of sines, and these sines are to be multiplied into the radius $\sqrt{\frac{4}{9}bb + \frac{4}{3}p}$, and thus will be obtained the quantities, y, y, y, in the figure; the sum, or difference of which and $\frac{1}{3}b$, as the case requires, will give the true roots of the equation. All these things are deduced from Descartes's discoveries. But that all the cases may be comprehended with as much brevity as possible; the centre R, in the first formula of equations, falling in the space VGP, the two intersections YY, fall between A and B, and consequently either of the lesser roots is less than $\frac{1}{3}b$, but the third and greater always exceed $\frac{1}{3}b$, but are exceeded by b: but if the centre fall in the space GNV, there are two greater than $\frac{1}{3}b$, but less than $\frac{2}{3}b$; and the third is b — the two others, and consequently less than $\frac{1}{3}b$; but using the limitation of the quantity p, the roots are included in narrower bounds; for the greatest root is less than $\sqrt{\frac{4}{9}b^2 - \frac{3}{4}p} + \frac{1}{3}b$, but greater than $\sqrt{\frac{1}{4}bb - p} + \frac{1}{2}b$; but when $\frac{1}{4}bb$ is less than p, that limit becomes $\sqrt{\frac{1}{9}bb - \frac{1}{3}p} + \frac{1}{3}b$. The middle root is always less than $\sqrt{\frac{1}{4}b^2 - p} + \frac{1}{2}b$, but greater than $\frac{1}{3}b - \sqrt{\frac{1}{9}b^2 - \frac{1}{3}p}$; and the least root never exceeds this limit, but vanishes with the quantity q.

In the second formula, according to the prescribed laws, there are two affirmative roots and one negative; and the centre falling in the space GPE, one of the affirmative is greater and the other less than $\frac{1}{3}b$, but the greater does not exceed b; and the negative cannot be greater than $\sqrt{\frac{1}{3}bb} - \frac{1}{3}b$, being the difference between b and the sum of the affirmative roots. But the centre being within the space ENGΔ, either of the affirmative is greater than $\frac{1}{3}b$, but less than $\sqrt{\frac{1}{3}bb} + \frac{1}{3}b$; and the negative always less than $\frac{1}{3}b$. But the limits of the greatest affirmative root become nearer from the given quantity p; being always less than $\sqrt{\frac{1}{4}bb - p} + \frac{1}{2}b$, and greater than $\sqrt{\frac{1}{9}bb - \frac{1}{3}p} + \frac{1}{3}b$; but the other affirmative root, which diminishes with the quantity q, is less than this limit, also the negative root is always less than $\sqrt{\frac{1}{9}bb + \frac{1}{3}p} - \frac{1}{3}b$, and it vanishes when the quantity q is wanting.

In the third formula, there are two negative roots and one affirmative root. In this, as in the fourth, the roots are not limited by the quantity b: but the affirmative root is ever less than $\sqrt{\frac{1}{9}bb + \frac{1}{3}p} + \frac{1}{3}b$, yet greater than $\sqrt{p + \frac{1}{4}bb} + \frac{1}{2}b$; and the greatest of the negative is always greater than $\sqrt{\frac{1}{9}bb + \frac{1}{3}p} - \frac{1}{3}b$, but less than $\sqrt{p + \frac{1}{4}bb} - \frac{1}{2}b$; and the lesser of the negatives is always lessened with the diminished quantity q.

In the fourth formula, the centre falling within the space LΔPD; if there be two affirmative roots and one negative, the greatest of the affirmative cannot be greater than $\sqrt{p + \frac{1}{4}bb} + \frac{1}{2}b$, nor less than $\sqrt{\frac{1}{9}bb + \frac{1}{3}p} + \frac{1}{3}b$; but the lesser root is diminished by these limits, the quantity q being decreased. Also the negative root is less than $\sqrt{\frac{1}{9}bb + \frac{1}{3}p} - \frac{1}{3}b$, and greater than $\sqrt{p + \frac{1}{4}bb} - \frac{1}{2}b$.

But here it is to be noted, that the negative roots are every where marked with the affirmative sign, because these are the affirmative roots of those four equations, wherein $+ b$ is found, and q affected with the contrary sign, as was hinted above. The demonstration of all this follows from hence, that wherever the centre R of the circle falls on the curves VPX or VΔL, its circumference touches the parabola in a point, whose distance from the axis is $\sqrt{\frac{4}{3}}$ VH, and cuts it on the other side of the axis at the distance of $2\sqrt{\frac{4}{3}}$ VH; but when the centre falls on the line DPD, one of the roots is $= 0$, and consequently the cubic equation is reduced to a quadratic, or to $z^2 - bz + p = 0$, whose roots give the limits, where the quantity q vanishes: and by how much the less q becomes, by so much the nearer the roots approach to those limits. The equation is also a quadratic, when the centre falls in the axis; that is, when $\frac{1}{2}q = \frac{1}{6}bp - \frac{1}{27}b^3$ in the first formula, or $\frac{1}{2}q = \frac{1}{27}bbb - \frac{1}{6}bp$ in the second; in the third it is impossible; but in the fourth, when $\frac{1}{2}q = \frac{1}{27}b^3 + \frac{1}{6}bp$, in which case, the lesser of the affirmative roots is $\frac{1}{3}b$, and the greater $\sqrt{\frac{1}{3}bb + p} + \frac{1}{3}b$; but the negative $\sqrt{\frac{1}{3}bb + p} - \frac{1}{3}b$. In the first formula, the roots are $\frac{1}{3}b$, and $\frac{1}{3}b \pm \sqrt{\frac{1}{3}bb - p}$. But in the second, $\frac{1}{3}b$ and $\sqrt{\frac{1}{3}bb - p} + \frac{1}{3}b$ are affimative, and $\sqrt{\frac{1}{3}bb - p} - \frac{1}{3}b$ is negative.

And this may suffice for cubic equations; but because of the excellent use of the method, in which, by means of a table of sines, the roots of these equations are found; it was thought proper to add an example or two, by which the compendium of that practice may appear. Let then the equation $z^3 - 39z^2 + 479z - 1881 = 0$ be proposed, to find the roots z, $\sqrt{\frac{1}{9}bb - \frac{1}{3}p} = \sqrt{9\frac{1}{3}} = \sqrt{d}$, whose double $\sqrt{37\frac{1}{3}}$ is the radius of the circle; and

$$\frac{\frac{1}{27}b^3 + \frac{1}{2}q - \frac{1}{6}bp}{\sqrt{d^3}} = \frac{2197 + 940\frac{1}{2} - 3113\frac{1}{2}}{9\frac{1}{3}\sqrt{9\frac{1}{3}}}, \text{ or } \frac{24}{9\frac{1}{3}\sqrt{9\frac{1}{3}}}$$

is the tabular sine of an angle, that is, making a division by means of the logarithms, log. 9.9251560, to which answers the angle of 57° 19′ 11½″; the third part of this is, 19° 6′ 24″; and ⅓ of its supplement 40° 53′ 36″; the sines give the logarithms 9.514983, and 9.816011, which multiplied into the radius $\sqrt{37\frac{1}{3}}$, produce Y & and Y &, log. 0.301030 = 2, and log. 0.601059 = 4, but the third Y & is equal to their sum, or 6. Therefore the roots are 13 − 4 = 9, 13 − 2 = 11, and 13 + 6 = 19, of which several quantities the above-mentioned equation is composed. Where it is to be observed, that the two lesser roots do not exceed $\frac{1}{3}b$, or 13, because the centre R in the construction falls on the right hand side of the axis; that is, $\frac{1}{6}bp$ is less than $\frac{1}{27}b^3 + \frac{1}{2}q$.

For another example, let $x^3 - 15x^2 - 229x - 525 = 0$ be an equation, whose roots are sought. Here $\sqrt{\frac{1}{9}b^2 + \frac{1}{3}p} = \sqrt{101\frac{1}{3}} = \sqrt{d}$, and the radius of the circle $= \sqrt{405\frac{1}{3}}$; also $\frac{\frac{1}{27}b^3 + \frac{1}{6}bp + \frac{1}{2}q}{\sqrt{d^3}} = \frac{125 + 572\frac{1}{2} + 262\frac{1}{2}}{101\frac{1}{3}\sqrt{101\frac{1}{3}}} = \frac{960}{101\frac{1}{3}\sqrt{101\frac{1}{3}}}$ = the tabular sine of an arch, whose log. is 9.9736426, and the arch itself 70° 14′ 22″; the third part of it is 23° 24′ 47″½, and of its supplement, is 36° 35′ 12″½; whose log. sines are 9.599183, and 9.775275, to which adding the log. of $\sqrt{405\frac{1}{3}}$, we have the log. 0.903089 = 8, and log. 1.079181 = 12, whose sum is = 20. Hence we conclude that 20 + $\frac{1}{3}b$ or 25 is equal to the affirmative root, and 8 and 12 − $\frac{1}{3}b$, that is 8 and 7, are equal to the negative roots. But if the equation had been $x^3 + 15x^2 - 229x + 525 = 0$, then 8 and 7 would have been the affirmative roots, and 25 the negative: as for the other cubic equations, which are explicable by one root only, they are to be resolved by Cardan's rules, after the second term is taken away, nor do I see that the thing can be done with less calculation. But if this root be desired to be expressed by the quantities b, p, q; in the first formula it is, $\frac{1}{3}b$ + or − the sum or difference of the cubic roots of

$\sqrt{\frac{1}{4}qq - \frac{1}{108}p^2b^2 + \frac{1}{27}b^3q - \frac{1}{6}bpq + \frac{1}{27}p^3} \pm \frac{1}{27}b^3 + \frac{1}{2}q - \frac{1}{6}bp$ (viz. +, if $\frac{1}{27}b^3 + \frac{1}{2}q$ be greater than $\frac{1}{6}bp$, otherwise −) the sum, when $\frac{1}{3}bb$ is greater than p, the difference when less. And in the other formulas, the root always consists of the same parts, changing the signs + and −, as will be easily seen upon trial.

But these roots are readily enough found by means of the logarithmetical table of versed sines; viz. if the coefficients be either surds or fractions, and the roots not to be expressed in numbers, as most commonly is the case. And this is the rule; in the first and second formulas, if $\frac{1}{3}bb$ be less than p, let $\frac{1}{3}p - \frac{1}{9}bb = d$, and putting, for the radius, the difference between $\frac{1}{6}bp$ and $\frac{1}{27}b^3 + \frac{1}{2}q$ (that is, HR) in the first formula, and the difference between $\frac{1}{6}bp + \frac{1}{2}q$ and $\frac{1}{27}b^3$ in the second; and find the angle whose tangent is $d\sqrt{d}$. Then as the co-sine of that angle is to its versed sine, so the difference, which is taken for radius, to a fourth term; the cube root of which is had by taking $\frac{1}{3}$ of its logarithm. Then dividing $\frac{1}{3}p - \frac{1}{9}bb$ by this cube root, let the divisor be subtracted from the quotient, the remainder will be the quantity Y &; the sum of this remainder and $\frac{1}{3}b$ will be the root sought, if the centre fall on the right hand of the axis; otherwise their difference will be the root. But if $\frac{1}{3}bb$ be greater than p, making HR radius, let $d\sqrt{d}$, or the distance of the paraboloid from the axis, be the sine of some arch, and let its versed sine be multiplied into the radius, or $\frac{1}{6}bp - \frac{1}{27}b^3 \pm \frac{1}{2}q$, and taking $\frac{1}{3}$ of the logarithm of the product, its cube root will be obtained, by which let $\frac{1}{9}bb - \frac{1}{3}p$ be divided; the sum of the quotient and divisor, after the same manner added to or subtracted from $\frac{1}{3}b$, will give the root sought. And the like holds in the third and fourth formulas, except that $\frac{1}{27}b^3 + \frac{1}{6}bp \pm \frac{1}{2}q$ is to be taken for radius, and $\frac{1}{9}bb + \frac{1}{3}p$ into $\sqrt{\frac{1}{9}bb + \frac{1}{3}p}$, or $d\sqrt{d}$, for the sine. But these rules will perhaps be better understood by examples.

Suppose the cubic equation $z^3 - 17z^2 + 54z - 350 = 0$, and let the root z be sought. Here $\frac{1}{3}bb$ is greater than p, but q is greater than the cube of $\frac{1}{3}b$, and therefore it is explicable by one affirmative root only: now $\frac{288}{9} - \frac{54}{3}$ is d, and $\frac{127}{9}\sqrt{\frac{127}{9}}$ is to be taken for the sine, to the radius $\frac{4913}{27} + 175 - 153$, that is, $\frac{5507}{27}$; and the arch answering thereto is 15° 3′ 49″, the log. of its versed sine is 8.5362376, which added to the log. of the radius 2.3095913, makes 0.8457889, the third part of which 0.2819276 is the log. of the cube root 1.91394, by which, as a divisor, dividing $\frac{127}{9}$, or d, the quotient is 7.37281; the sum of the quotient and divisor, increased by the addition of $\frac{1}{3}b$, is the root sought, viz. 14.9534, &c.

Having thus dispatched cubic equations, let us proceed to biquadratics. Now these have always either none, or 2, or 4 true roots, the determination of which depends partly on the coefficients, and partly on the sign and magnitude of the absolute number given. In the construction of the equation $z^4 - bz^3 +$

$pz^2 - qz + r = 0$; make BD, fig. 11, $= \frac{1}{4}b$; AB $= \frac{1}{16}b^2$; BK $= \frac{1}{2}$, or $\frac{1}{2}$ the parameter; KC $= 2$AB $= \frac{1}{8}bb$; KE $= \frac{1}{8}bb - \frac{1}{2}p$; AE $- \frac{1}{2} = \frac{1}{16}bb - \frac{1}{2}p$; FE $= \frac{1}{16}b^3 - \frac{1}{4}bp$; and EG $= \frac{1}{16}b^3 - \frac{1}{4}bp + \frac{1}{8}q$; which done, a circle from the centre G with the radius $\sqrt{GD^2 - r}$ will intersect the parabola, either in none, or 2, or 4 points; from whence perpendiculars let fall on DH, will give all the roots z. But that they may be 4, it is evident that the centre of the circle should be found somewhere within a space, from any point of which, three perpendiculars may be let fall upon the curve of the parabola; and also that the radius be less than the greatest of those perpendiculars, and greater than the middle one. But if the centre be posited without this space, so that only one perpendicular can be let fall on the parabola, and the radius be greater than it; or if it be less than the middle one of the three perpendiculars, but greater than the least of them, then there can be only two roots; but there is none at all, when the radius $\sqrt{GD^2 - r}$ is less than the least of the three, or one, as often as there is but one.

Now it remains to inquire, of what kind this space is, by what limits it is distinguished, and under what conditions the radius of the circle is less or greater than the above-mentioned perpendiculars. And first of all we must show how a perpendicular is to be let fall upon the parabola. Let ABC, fig. 12, be a parabola; AE its axis; AV half the parameter; G the point from whence the perpendicular is to be let fall; let GE be drawn perpendicular to the axis, and let VE be bisected in F, and erecting the perpendicular FH on the same side of the axis, make FH $= \frac{1}{4}$GE; then the circle described on the centre H, with the radius HA, will intersect the parabola either in three points, or one, Z, to which the right lines GZ being drawn, will be perpendicular to the curve of the parabola.

But that there may be three such intersections, the centre H, fig. 10, should be so posited, as that it may be within the space included by the paraboloids, that is, that FH may be less than $\sqrt{\frac{8}{27}VF^3}$, or FH^2 less than the cube of $\frac{2}{3}$VF; and so GE $= 4$FH will be less than $4\sqrt{\frac{8}{27}VF^3}$, or $4\sqrt{\frac{1}{27}VE^3}$; that is, the square of GE will be less than $\frac{16}{27}VE^3$. Therefore these limits coincide with two paraboloids of the same kind with those which were used in cubic equations, but whose parameter is twice less; viz. $\frac{27}{16}$ of the parameter of the parabola, that is $\frac{27}{8}$ of AV; and therefore it is that very curve, by whose evolution the parabola is described, as Huygens has demonstrated; and which is always touched by the line DF, fig. 11, a perpendicular to the parabola in the point D. And the point P, in which the right line DF touches the paraboloid, is the centre of a circle, which being described with the radius DP, coincides with the parabola in the point D, or has the same curvature with it, as is manifest.

Having therefore described such paraboloids VXP, VNΔ, on each side of the axis, it is plain, that unless the centre of the circle be placed within these limits, it cannot intersect the parabola in more than two points; from whence we may determine, under what conditions, the coefficients of the intermediate terms are restrained in biquadratic equations, so that there may be 4 roots. And at first sight it is plain, that p cannot be greater than $\frac{3}{8}bb$ (viz. in those formulas wherein it is $+ p$) nor q than $\frac{1}{16}b^3$. But in general, $\frac{1}{16}b^3 \mp \frac{1}{4}pb \mp \frac{1}{2}q$, that is EG, the distance of the centre from the axis, should be less than $EH = 4\sqrt{\frac{1}{27}VE^3}$, that is, (because $VE = \frac{3}{16}bb \mp \frac{1}{2}p$) than $\frac{1}{4}bb \mp \frac{2}{3}p \sqrt{\frac{1}{16}bb + \text{or} - \frac{1}{6}p}$; the signs $+$ and $-$ being left doubtful, that they may be varied according to the nature of any equation, as was shown above in cubits.

As for the limitation of the last term r, it cannot be found with the same ease; because the letting fall a perpendicular on the curve of a parabola is a solid problem, which cannot be resolved without the solution of a cubic equation. Therefore, in the first place, let the second term be wanting, or if it be in the equation, let it be taken away, so that it may have this form $z^4.\ *.\ pz^2.\ qz.\ r = 0$. Now if it be $- r$, it is always explicable by two or four roots; and that there may be four, the centre of the circle should be posited within the above-mentioned paraboloids, or that it may be $- p$, and qq may be less than $\frac{8}{27}p^3$, or the cube of $\frac{2}{3}p$; then let the roots of this equation $y^3.\ *.\ \frac{1}{2}py.\ \frac{1}{4}q = 0$ be found, the quantities p and q having the same signs as in the biquadratic; and these roots are found expeditiously enough by the table of sines. But having found those three y, (which are ordinates to the axis of the parabola, from the points where perpendiculars fall on its curve, viz. YZ, fig. 12,) then $pyy - 3y^4$ from the lesser y, will denote the greatest quantity r, if it be $- r$, than which if r be less, the equation will have four roots, otherwise only two. But if it be $+ r$, it should be less than $3y^4 - pyy$ from the middle y; for if it be greater, it can have but two roots; at least if r be less than $3y^4 - pyy$ from the greatest y. But if it be greater than this, the equation is not explicable by any true root at all. And these same limits are otherwise expressed by the quantity q; viz. $\frac{1}{4}qy - y^4$ in the first case, $y^4 - \frac{1}{4}qy$ in the second, and $y^4 + \frac{1}{4}qy$ in the third.

But it may happen, that the two lesser quantities y may not be far distant from each other, and thence both the perpendiculars greater than the right line GA, viz. when qq is greater than $\frac{4}{27}p^3$, but less than $\frac{8}{27}p^3$, the centre falling within the space contained between the paraboloids of both the figures 10 and

11. In this case, if it be $+ r$, there can be only two roots, $y^4 + \frac{1}{2}qy$ from the greatest y, being greater than r, otherwise none. But if $\frac{1}{2}qy - y^4$ from the least y, will be greater than r marked with the sign $-$; but r greater than $\frac{1}{2}qy - y^4$ from the middle y, then there will be four roots; but two only, if r be found greater than the former, or less than the latter.

But if in the equation it be $+ p$ or $- p$, and qq be greater than $\frac{8}{27}p^3$, the equation $y^3. * . \frac{1}{2}py. \frac{1}{4}q$ is explicable by only one root y; that is, there can be only one perpendicular let fall from the centre of the circle; whence it may be certainly concluded, that there can be only two roots in the given equation, whose sum, if it be $- r$, is increased with the quantity r; but if it be $+ r$, having obtained the quantity y, that quantity r should be less than $y^4 + \frac{1}{2}qy$; for if it be greater, the equation proposed is absurd and impossible.

It would be both tedious and needless to run over all the equations of this kind, since it is evident from what has been already said, which are negative and which affirmative roots; and that the limits of these roots are derived from the quantities y found. But for an example, which any one may imitate in other cases; let it be proposed to discover the limits or conditions, under which, there may be four affirmative roots in a biquadratic equation. Now this will be the case as often as the centre of the circle G falls within the space VPK, fig. 11, and also as often as it is $+ r$, or the radius of the circle is less than GD: whence it is plain that the equation, here designed, is of this formula, $z^4 - bz^3 + pz^2 - qz + r = 0$; and that p cannot be greater than $\frac{3}{8}bb$, nor $\frac{1}{4}pb$ in this case greater than $\frac{1}{16}b^3 + \frac{1}{2}q$; again, it is necessary that $\frac{1}{4}bb - \frac{2}{3}p\sqrt{\frac{1}{16}bb - \frac{1}{6}p}$ should be greater than $\frac{1}{16}b^3 + \frac{1}{2}q - \frac{1}{4}bp$; and from these limits it will be plain, that the centre is contained within the space VPK. But to determine the quantity r, this cubic equation must be first solved, $y^3. * . -\overline{\frac{3}{16}b^2 - \frac{1}{2}}. py = \frac{1}{32}b^3 + \frac{1}{4}q - \frac{1}{8}pb$; and we shall have the points upon which perpendiculars, from the centre, fall on the curve of the parabola.

Now having found the three values of this y, the quantity r should be less than $\frac{3}{256}b^4 + \frac{1}{4}bq - \frac{1}{16}bbp + 3y^4 - \frac{3}{8}b^2yy + pyy$ from the middle y, but greater than $\frac{3}{256}b^4 + \frac{1}{4}bq - \frac{1}{16}bbp + 3y^4 - \frac{3}{8}b^2y^2 + pyy$ from the least y. But if r exceed these limits, there can be only two roots obtained. Lastly, if $\frac{3}{256}b^4 + \frac{1}{4}bp - \frac{1}{16}bbp + 3y^4 - \frac{3}{8}bbyy + pyy$ from the greatest y, be less than r, then the equation proposed is impossible. It may also happen that there are four affirmative roots, when the centre G falls in the little space VTS; viz. drawing RTS perpendicular on the middle of the supposed line AD; but this happens when p is greater than $\frac{5}{16}bb$, and $\frac{1}{4}bb - \frac{2}{3}p\sqrt{\frac{1}{16}bb - \frac{1}{6}p}$ greater than $\frac{1}{8}pb - \frac{5}{128}bbb - \frac{1}{2}q$; in which case always two, sometimes three of the roots are greater than $\frac{1}{4}b$.

But it is to be noted here, that the limit arising from the least y, is sometimes negative, or less than nothing, viz. as whenever the greatest of the three perpendiculars exceeds GD, fig. 11. If this happen, the quantity $+ r$ may be diminished to nothing by the limit prescribed from the middle y. But the defect of a limit from the least y shows how great $- r$ may be in the equation, if there be three affirmative roots and one negative; which if it exceed, there can be only two, one affirmative and the other negative. And all this is demonstrated from hence, that the above-mentioned limits of the quantity r, are the differences between the squares of the line GD, and of the perpendiculars on the curve of the parabola.

But because of the perplexing cautions arising from the diversity of signs in these equations, it is always better to take away the second term, and then to seek out the number of roots and the signs, according to the rules already delivered; especially if these quantities are not greatly different from each other. And of these four affirmative roots, two are always less than $\frac{1}{4}b$, and two greater; viz. if DG be less than AG, or $\frac{1}{4}pb$ than $\frac{3}{64}b^3 + q$. But three are always less than $\frac{1}{4}b$, whenever the mean perpendicular, or that found from the mean y, is greater than AG, or $\frac{3}{8}b^2 y$ greater than $3 y^3 - py^2$ from the same mean y. The fourth and greatest root is greater than the greatest $y + \frac{1}{4}b$, and it is equal to the difference between b and the sum of the other three roots, and therefore it is less than b.

Perhaps those who more perfectly understand the nature of the parabola, may be able to do all these things after a more compendious manner; but there is some reason to doubt whether all these quantities b, p, q, r, can be rightly determined without the resolution of a cubic equation; for whatever is done in this affair by plane equations, exhibits not the true limits, but some approximations only.

That the Obliquity of the Ecliptic and the Elevation of the Pole continue unaltered. From some Observations of M. P. Wurtzelbaur. N° 190, p. 403.

Whether the poles and axis of the earth be really fixed in the globe, or subject to be transferred from one place to another, is an old inquiry, though now lately revived by Mr. Hook, in his ingenious essays on the great mutations and catastrophies, which in all appearance have happened on the earth's surface. A necessary consequence of such a translation of the poles, would be the change of the latitudes of places, which would increase in those regions towards which the poles approach, and decrease in those from which they recede: and under the meridian 90° removed from that in which the poles shift, the latitudes continuing the same, the meridian line only would alter; but no two places considerably differing in latitude can be supposed, wherein if there be any sensible motion of the poles, it shall not be perceived by the alteration of the latitude of one or both of them.

The accurate Mr. Wurtzelbaur has lately furnished us with the means of examining this hypothesis by observation, having sent us the meridian altitudes of the sun, taken at Nurenburg, about the two solstices in the year 1686. June the 10th he found the meridian altitude of the sun 64° 2′ 20″, and the next day 64° 2′ 25″, and on December 14th, 3 days after the solstice, wherein the sun was got two minutes higher, he found the meridian altitude 17° 9′ 10″; therefore the solstitial altitude was 17° 7′ 10″. These heights were taken by an instrument of 6 feet radius, of brass; and the skill and diligence of the observer are not to be doubted.

To compare with these, I find among Bernard Walthers' observations, made in the same city of Nurenburg, 200 years before, viz. in the year 1487, that the meridian altitude of the sun, in the summer solstice, was observed by Ptolomy's parallactic instrument, when the chord of the sun's distance from the zenith was observed 44890 parts of 100000 radius; the same being confirmed by the concurrence of the observation of several years both before and after. The arch answering to this chord, gives the sun's distance from the zenith 25° 56′ 30″, and consequently the meridian altitude, its complement to a quadrant, 64° 3′ 30″. Again the same year 1487, the chord of the meridian distance of the sun from the zenith, on the day of the winter solstice, was found 118790, confirmed likewise by many subsequent observations; the arch answering to this chord is 72° 52′ 40″, and its complement 17° 7′ 20″, the meridian height of the sun in the winter solstice.

Hence it appears, that the solstitial heights were very nearly the same at Nurenburg 200 years ago, as they are now, that of the summer solstice being but one minute different, the other only 10″, both which may possibly arise from the defects of the instruments of these observers, being made with plain sights; but what I shall necessarily conclude from hence is, that if there be such a motion of the poles, it is either very slow, or else nearly at right angles to the meridian of Nurenburg; in which latter case, the latitudes of places about Tunking, Siam, Malacca, and Java on the one side, and in our American plantations of New-England, Virginia, Jamaica, &c. on the other, ought to change fastest; but I have never yet heard of any such thing observed by any of our navigators; whence if there be such a change of the earth's poles, it must necessarily require a long time to become sensible.

Besides, from these observations it appears that the obliquity of the ecliptic has continued unaltered for these 200 years last past, that is to say, that the angle which the earth's axis makes with the plane of the ecliptic, or orbit wherein she moves annually round the sun, has been without sensible change in all that time; which will be very hard to conceive, if we allow a translation of the earth's poles; for the direction of the axis being perfectly at liberty, it must be purely casual, if it happen, that after such change, it make the same angle with the ecliptic as before.

A farther argument of this slowness of the change of the poles, is the latitude of Alexandria, the habitation of those famous astronomers of antiquity, Eratosthenes, Timocharis, Hipparchus, and Ptolomy, and for that reason it may be concluded that this, of all the latitudes the ancients have left us, ought to be one of the most correct. This by Ptolomy is said to be 30° 58′ north, which he uses in all his computations in his Almagest, and seems derived from the proportion of the gnomon to its equinoctial shadow, as 5 to 3; but in his geography he uses 31° just. In the year 1638, the curious and ingenious Mr. Greaves, when he went to visit the Egyptian pyramids, with a sufficient instrument observed the latitude of Alexandria, and found it 31° 4′, or 6 minutes more than it is reputed by Ptolomy, and before him by Eratosthenes; so that in about 2000 years the latitude of Alexandria has altered only a few minutes, and so few that the accuracy of the observations of the ancients may well be questioned: but both being granted, this motion will amount to no more than a degree in 20,000 years.

This is said not with intent to invalidate what Mr. Hook has, from so good grounds, advanced, viz. that the ball of the earth, at least the fluids of it, being necessarily of the figure of a prolate sphæroid, or flat oval, whose shortest diameter is the axis, and greatest circle the equinoctial; if the poles be supposed changed, the equinoctial will be so too; and consequently the water must rise and cover those parts from which the poles recede, and fall off and leave bare those places towards which the poles approach. By this means it may be accounted for, how such strange marine things are found on the tops of hills, and so deep under ground; and scarcely any other way. But from these and the like observations it will follow, that if these inundations are produced by any regular motion of the poles, it would require a prodigious number of ages to effect those changes we may be certain have been. Besides, if the access and recess of the sea were after such a gradual manner, as when produced by such an easy translation of the poles, as can by observation be admitted, those inundations could never be fatal to the inhabitants, as they would always give notice of their coming, so that the people might provide for their safety. But both sacred and prophane tradition agree, that the last great deluge was produced in a few days, without any previous notice; so that it could not be accounted for by this hypothesis, without the supposition of a great and sudden alteration in the poles of the earth's diurnal revolution; for effecting which, whether we should have recourse to the intelligent powers that first impressed this whirling motion on the ball; or suppose it to be performed naturally, by the casual shock of some transient body, as a comet or the like, by which the former axis might be lost, and a new revolution produced, differing both in time and position from the old, I shall not undertake to dispute: such a supposition would include likewise a change in the length of the year, and eccentricity of the earth's orbit; for which yet we have no sort of authority.

On the diseases of Dogs; with several Receipts for the Cure of their Madness, and of those bitten by them. Extracted from the Papers of Sir Theodore Mayern, and communicated to the Royal Society. By Sir Theodore de Vaux, Knt. and F.R.S. N° 191, p. 408.

Dogs are subject to these several sorts of madness, or rather diseases. 1. The hot madness, which is incurable. They fly at every thing, and can hold out but 4 days. 2. The running-madness, which is likewise incurable. They fly only at dogs, and that by fits, and they may sometimes hold out 9 months. 3. La rage mue, which is a disease in the blood. 4. The falling madness, which seizes on the head, and is a kind of epilepsy. 5. The blasting or withering; this lies in the bowels, which shrink up exceedingly. 6. The sleepy disease, caused by little worms in the mouth of the stomach; these dogs die sleeping. 7. The rheumatic disease; this swells the head very much, and makes the eyes yellow. These last five are not properly madness, but other diseases. In them the dogs will not eat, nor at any time when they are sick; but they live 8 or 9 days without hurting any body, and then die of hunger. The first two diseases are communicated by the breath of dogs, as the plague among men; the latter are likewise contagious, but curable.

A never-failing Remedy.—Take Virginia snake-root and flowers of St. John's wort, gathered in their prime, equal parts of each; let them be made into very fine powder. The dose is from a scruple to a dram, to be taken in any sort of decoction prepared with specifics. To a horse give 2 drachms; to a dog, from one to 1½ drachm; and this before the 9th day after the bite.

Another.—Take leaves of rue, picked from the stalks and bruised, 6 ounces; Venice treacle, garlic peeled and bruised, and fine filings of tin, each 4 ounces. Put them into 4lb. of Canary or good white wine; or in case of a nice or hot constitution, into the same quantity of strong and well-worked ale, in an earthen vessel well stopped. Then let them gently digest or boil in balneo, for 4 hours, keeping in the steam. Then press it and strain it. The dose is from 2 to 3 ounces, or more for some persons, to be taken every morning for 9 days. The patient must fast for three hours after it; and the dregs that remain after expression must be bound upon the wound; renewing it every 24 hours. That the 9th day after the bite must not be let slip, before this medicine be taken, lest the poison seize the blood too strongly. It must be given cold, or at least only a little warmed. A double quantity may be given to a beast soon after the bite. This remedy having been administered several times, was never found to fail.

Another.—Pluck the feathers from the breech of an old cock, and apply it bare to the wound. If the dog were mad, the cock will swell and die, and the patient will do well; but if the cock dies not, the dog was not mad. If the wounds be very small, it is proper to open them with a lancet.

Another.—Let the patient be nine times plunged in the sea, while he is fasting, as soon as possible after the bite. Let the bitten part be washed with a lye of the ashes of oak wood and urine, and apply a cataplasm of good treacle, alliaria, or hedge garlic, rue and salt.

Take dried rue and scordium, of each 2 drachms, Virginia snake-root $1\frac{1}{4}$ drachm, flowers of St. John's wort 3 drachms, fine filings of tin and garlic cut small, each 4 drachms, good treacle 1 ounce: let them be all beaten and exactly mixed together, adding syrup of lemon peels sufficient to make it into an electuary; divide this into 9 equal parts, one to be taken every day, drinking after it a small draught of good strong ale. Let him walk after it, and not eat till 4 hours after. Use as little of the syrup of lemon peels as may be: if it cannot be had, a syrup made of Malaga-wine, adding as much sugar as it can dissolve, may serve the purpose. Make up of this electuary $4\frac{1}{2}$ ounces at a time, that so the dose may be half an ounce.

The Zirchnitzer Sea in Carniola described. By M. J. Weichard Valvasor. N° 191, p. 411.*

This lake was by the ancients called Lugea Palus, by the moderns Lacus Lugeus, though at present its Latin name be Lacus Cirknicensis, in high Dutch Zircknizer Sea, and in our Carniolan tongue Zirknisko Jesero. It is at the distance of 6 German miles from the capital city of the province Labac, and is a good German mile long, and about half as much in breadth. Its ordinary depth is 10 cubits, its least 5 or 6, rarely 3, its greatest is 16 cubits. It is every where surrounded with woody mountains, which on the south and west

* See N° 54, p. 409, vol. i. of this Abridgment.

side are very high, and 3 miles broad, running far into the Turkish country, and afford nothing but horrid stony deserts, overgrown with trees.

In the mountain called Iavornik, near the lake, there are two holes, or exceedingly deep precipices, in which many thousand wild pigeons roost all the winter; entering in autumn, and coming out with the first of the spring; what they live upon in these caverns is unknown, but perhaps the nitrous sand. On the other hill called Slivenza, is a hole of an unknown depth, out of which there often breathe noxious steams, attended with tempests of thunder and lightning and hail. This lake being every where surrounded with mountains, and nowhere running over, nature has given it two visible channels or stony caverns, by which the water runs under the mountain; and a third concealed subterraneous passage, which without doubt communicates with the other two under ground. This water having run half a German mile, comes out at the other side of the mountain, in a desert place at a stony cave, and forms the river called by the inhabitants Jesero, that is the lake. This river having run half a quarter of a mile enters a wide stony cavern, running slowly under the hill for the space of a good musket shot, then coming out again on the other side, after it has run through a small plat, it enters a third cavern or grotto; wherein having passed 50 paces, it runs no longer peaceably as before, but with great noise and roaring falls down a very steep channel of stone.

About the feast of St. John Baptist, or St. James tide, and sometimes not till August, the water runs away, and it is dry; but it fills again most commonly in October or November; yet so as not to observe any certain time; for sometimes it has been dry twice or thrice in a year, which makes the fishing very considerable. Sometimes again, though but seldom, it has happened to be 3 or 4 years together full of water, and then is the best of the fishing. But it never yet was observed that this lake was dry for a whole year together.

In this lake there are many pits in the shape of basins or cauldrons, which are not all of the same depth or breadth; the breadth of them being from 20 to 60 cubits, more or less, and the depth from 8 to 20 cubits. In the bottom of these pits are several holes, at which the water and fishes enter when the lake ebbs away. In the months of June, July, and August, when this lake begins to draw off, it grows quite dry in 25 days, if no great rains intervene. And the pits are all emptied one after the other, in a certain and never-failing order of time.

When the lake begins to sink, which appears by a certain stone which they observe, the inhabitants of the town called Oberdorff or Seedorf, give notice thereof to all the neighbouring fishermen, that are appointed by the several lords having right in this fishing. The people of this town have orders not only to watch the falling away of the water, but likewise to take care that nobody presume to fish in the lake when it is full of water, that being forbidden.

The first pit, called Maljoberch, is only a depression of the bottom, without any holes in it; but there grow much grass and weeds, and many fish are caught there. Three days after the water begins to ebb, this pit is emptied. Of this the parish clerk of Seedorf gives notice by tolling a bell, and all the inhabitants of the town, old and young, men and women, lay aside all other business and go to fishing, quite naked as they were born, without any regard to modesty or shame. The fish they catch they divide in halves, one part they give to the prince of Eggenberg, as the lord of the manor, the other half is their own. The pit Velkioberch is emptied the third day after Maljoberch, the manner and right of fishing as in that. Four hours after this, the pit Kamine begins to empty; here they generally fish with a trawl, as in several other pits of lesser note, having first purchased leave of the lord of the manor. Here, as also in the pit Sueinskajamma, which sinks one hour after Kamine, are many fish caught, and abundance of large crabs, but they are lean and of no good taste. The fifth pit, Vodonos, dries five days after Kamine. In this and the other pits which follow, they fish with a long net or sayne. Here they can have no more than five or six hawls, by reason of the great swiftness with which the water runs away at the holes in the bottom, which is such that a horse can hardly keep pace with it, and carries away the fish with great violence under the earth. Sometimes when the fishermen are not nimble, they can scarcely get two hawls before the water is gone. The pit Louretschka evacuates a day and a half after Vodonos; the fishing is after the same manner, and the same caution necessary, because of the sudden recess of the water. The water leaves the pit Kralouduor 12 hours after Louretschka, and 3 days after that the pit Rescheto. In this latter, in the year 1685, after the lake had been some years without being dry, there were taken at the first hawl 21 carts of fish, at the second 17, and at the third 9. The pit Ribeskajamma falls dry at the same time with Rescheto, which is that next to it. In this pit they fish under ground, which is a curiosity not unpleasant, and differing from all the rest. For there is in the bottom a great hole in the stone, by which men may easily go down with lighted torches, as into a deep cistern; and there is under ground a large cavern like a vault, the bottom or pavement whereof is as it were a sieve full of little holes, whereby the water runs away, leaving the fish dry, where they are caught. The pit Rethje is empty 2 hours after Ribeskajamma, and is of no great consequence for fish. An hour after this, the pit Sittarza, and in 5 or 6 hours more Lipauza falls dry.

The third day after Rescheto the pit Gebno empties; in this they rarely fish with nets, but let it fall dry, and the holes in the bottom being so small, that they exceed not the size of a man's arm, all the great fish are left behind in the pit. Two days after Gebno the pit Koteu becomes dry; in this they sometimes take the fish as in the former, but the holes, being larger, let more fish pass. The pit Ainz empties 4 or 5 hours after Koteu; in this they seldom let the water run away without using their nets, as in Gebno, because of one great hole in the bottom, by which many great fishes may escape. The pit Zeslenza sinks 3 hours after Ainz; in this they always fish with nets, as in Pounigk, which is emptied the next day after Koteu.

The last pit, called Leuische, is evacuated the third day after Pounigk, that is, the 25th day from the beginning of the recess of the water of the lake, so that in 25 days the fishing of this lake is over. In this last pit, about 17 years since, there fell a flash of lightning, about the time of fishing, which stunned a multitude of large fishes, so as they filled 28 one-horse carts with them. These fish are not properly thunder struck, but only stunned with the violence and sulphureous vapour of the lightning, which makes them rise and swim as dead on the top of the water; but if they be taken up and put in fresh water, they soon recover, otherwise they die; this is no uncommon accident in this lake.

The fishing being thus ended, a signal is given, by tolling the bell in the chapel of St. John Baptist, near the town of Cirkniz. Upon which all the inhabitants of the neighbouring villages and of Cirkniz, without regard either to age or sex, go mostly quite naked into the lake, and look for fish among the weeds and sedge, and in the smaller pits; and many creep into the subterraneous cavers and passages, and find many large fishes there.

There are, besides these, some other pits in the lake, in which they fish likewise, as also in Mala-karlouza and Velka-karlouza; in both these they go far under ground with lighted torches and find fish. In Velka-bobnarza one may go in at great holes, and descend many fathoms under ground. These two names Velka and Mala-bobnarza signify in the Carniolan tongue the greater and lesser drummer; nor is it without reason that these pits are so called: for when it thunders, there is heard in these two pits as it were the sound of many drums beating.

The two pits Narte and Piauze are never emptied, but always remain fenny, when the rest of the lake is quite dry. It is believed, that in these pits the fish lay their spawn, and therefore it is prohibited to fish in them. In them is an incredible number of horse-leeches. These often stick to the people in the fishing time, some of them being dispersed all over the lake, and the method they ake to get them off is to get some other person to make water upon the leech, which makes it let go its hold.

There are in the mountain near the lake, but something higher than it, two great and terrible stony caves, which, though far distant from each other, have yet the same effect, viz. when it thunders, these two caves emit water with a wonderful and incredible force, and with it sometimes a great quantity of ducks with some fish. It is not to be wondered that the lake fills so fast, for considering the violence with which the water rushes, it is like a great river; this cave being a fathom wide, and higher than a man. It is dangerous to enter into this cave, because the waters come so suddenly, that it is sometimes impossible to escape them.

When it rains moderately, the water spouts with great violence 2 or 3 fathoms perpendicularly out of the pits Koteu and Keslenza. It comes likewise forcibly out of the spring Tresenz, as likewise out of Velkioberch, bringing with it at this latter abundance of fish, and some ducks. But when it rains very hard and long together, especially with thunder, then the water breaks out with very great force, not only from all the aforesaid pits, holes, and caves, but likewise at several thousand other little holes, which are all over the bottom of the lake, and which, when the lake is dry, drink up the waters of the eight rivulets that run into it, spouting several fathoms high, from some perpendicularly, from others obliquely, making a very pleasant sight. And out of the pits Vodonos, Rescheto, and some others, having great holes at the bottom, there comes with the water a great quantity of fish. In case of great rains, the eight rivulets running into it are likewise much increased; so that, all things concurring, this lake in 24 hours time will, from quite dry, be full of water, and sometimes in 18 hours; though at other times it has been known to be 3 weeks in filling; but it is a constant observation, that thunder helps much to fill it speedily.

This lake being thus by turns wet and dry, serves the inhabitants for many purposes. For first, while it is full of water it draws to it several sorts of wild geese and ducks and other water fowl, as herons, swans, &c. which may be shot, and are very good meat. Next, as soon as the lake is emptied, they pluck up the rushes and weeds, which make excellent litter for cattle. 20 days after it is fully dry, they cut a great quantity of hay upon it. After the hay is off, they plough it and sow millet, which sometimes by the too sudden coming of the water is destroyed, but it generally comes to maturity. While the millet is on the ground, they catch a great number of quails. The millet being off, there is a good pasture for cattle. When the lake is dry, there is great variety of hunting; as there comes out of neighbouring woods and mountains plenty of hares, foxes, deer, swine, bears, &c. as soon as the water is gone. When it is full, one may fish in it. In winter time it will be so firmly frozen as to bear all sorts of carriages, which is a great convenience to the people to fetch their wood and other necessaries. Lastly, at the time when the water goes away, it yields great abundance of fish, as beforesaid. And that which is most wonderful is, that all this comes to pass in the same place, and the same year, viz. if the lake be early dry, and it fill not too soon; but it is to be noted, that the hay does not grow, nor is the millet sown all over the lake, but only in the more fertile places.

There are only three sorts of fish taken in this lake, which are very well tasted. They are the mustela fluviatilis or eel-pout, some of them weighing 2 or 3 lb.; tench, some of them weighing 6 or 7 lb.; and, thirdly, pikes, in very great plenty, of 10, 20, 30, and some of 40 lb. weight; in the bellies of these it is common to find whole ducks. Crabs are found no where but in the pits Kamine and Sueinskajamma.

The cause or rather modus of all these wonderful phænomena in the lake of Zirknitz, is probably as follows. There is under the bottom of the lake another subterraneous one, with which it communicates by the several holes described; there are also some lakes under the mountain Javornik, whose surface is higher than that of the lake of Zirknitz. This upper lake is perhaps fed by some of those many rivers, which in this country bury themselves under ground, and has a passage sufficient to carry the waters they bring unto it; but when it rains, especially in thunder showers, which are the most hasty, the water is precipitated with great violence down the steep valleys, in which are the channels of these rivulets; so that the water in this lake, being increased by the sudden coming in of the rains faster than it can empty, swells presently; and finding several holes or caverns in the mountain higher than its ordinary surface, it runs over by them, both into the subterraneous lake under that of Zirknitz, into which the water comes up by the several holes or pits in the bottom of it, as likewise by visible passages above ground.

That some of these passages bring fish, some ducks and fish, others only water, seems to depend on the position of the inward mouths of these subterraneous channels: for if they be so constituted as to draw off the water from the surface of the upper lake, on which the ducks swim, they must needs be drawn away by the stream into these caverns, and come out with the water; but if the channels open into the upper lake under the surface of the water, and from thence ascend obliquely for some space before they come to descend; then the water they carry is drawn from below the surface, and consequently can bring with it no ducks, but only fish. Those pits which yield only water may well be supposed to be fed by passages too narrow to let the fish pass, though their multitude may make the quantity of water they emit to be very considerable.

The manner of the falling away of the water or emptying of the lake I thus explain: After a long drought, or want of rain, all the springs that feed the upper lake under Javornik are much diminished; so that wanting fresh supplies it ceases to run over by the several channels; hence the lake of Zirknitz, and that under it are fed only by the eight rivulets that always fall into them; and then the water draws off faster than it comes in, both by the channels of Mala and Velkakarlouza, as also by a concealed subterraneous passage out of the under lake, which latter alone is able to transmit more water than the said eight rivulets afford. Consequently the lake must sink, and that in a certain proportion of time, depending on the quantity of water to be evacuated, compared with the excess of that which runs out above what enters it, in the same time. Those pits that are higher are soonest dry, the lower latest, and so come to be emptied in the order above described. And when the lake is all dry, then the said rivulets soak by several little holes in the bottom into the under lake, and all their water is carried away by the subterraneous passage.

The ducks so often mentioned, and which are cast out with the water, are generated in the lake under the mountain Javornic; when they first come out, they swim well, but are stark blind, and have no feathers on them, or but few, and therefore are easily caught; but in 14 days time they get feathers, and recover their sight yet sooner, and afterwards fly away in flocks. They are black, only white on the forehead; their bodies not large, resembling ordinary wild-ducks, and are of a good taste, but too fat, having near as much fat as lean. I killed some of them as soon as they had been cast out at Sekadulze; and opening their bodies, I found in them much sand, and in some few small fishes, in others green stuff like grass or herbs; which was the more strange, because I never found any green thing growing in any of the subterraneous grottos or lakes in Carniola. Almost every year, at a hole in the mountain called Storseg, about half a German mile from the lake of Zirknitz, near the town of Laas, whenever there happen great floods of rain, this sort of ducks is cast out in great abundance, by the water gushing out with much force.

A Conjecture at the Quantity of Blood in Men; with an Estimate of the Celerity of its Circulation. By Allen Moulin, M.D. F.R.S. N° 191, p. 433.

In a sheep which alive weighed 118lb. we found but 5¼lb. of blood, which is but $\frac{21}{472}$ of the weight of the sheep. In a lamb, weighing 30¼lb. when living, there was but 1½lb. of blood, which is nearly a 20th part. In a duck weighing alive 2lb. 14oz. 50gr. we found 1½oz. and 53gr. of blood, which is less than a 28th of the whole weight of the living duck. In a rabbit, weighing 10 oz. 7 dr. 50 gr. we found 2 dr. 57 gr. of blood; which is about a 30th part.

In the right ventricle and auricle of the heart of a dog, I found 6 oz. of blood; after I had injected into the jugular vein a liquor that coagulated the blood. I found a greater quantity of blood in the heart of another dog, which I treated after the same manner. The hearts were much distended by the blood found in them. I shall therefore suppose that 4 oz. only were received at a time by these hearts without force, that is, naturally; and lest I should suppose a greater quantity of blood to be admitted at a time than really is, I will suppose a man's heart, which is much larger, and has much larger vessels, to receive but 4 oz. at each diastole. Allowing 75 pulses to every minute, there will be 4500 in an hour, and 18000 oz. of blood transmitted in that time. This last number is the product of the foregoing 4500, being multiplied by 4, the number of ounces at a diastole.

Now if we suppose that a man's blood bears the same proportion to his weight as that of any of the aforesaid animals had to its weight, which in a lamb was the greatest, being $\frac{1}{20}$ part, it will follow that the quantity of circulating blood in a man weighing 160 lb. will not exceed 8 lb. or 128 oz. According to which computation the blood will circulate 140 times in an hour. But let us suppose that instead of 8 lb. the mass of blood in such a man is 12 lb. it will follow that it will circulate between 93 and 94 times in an hour; which is a circulation and half, and somewhat more, every minute. I take this last computation, to be very moderate, especially when it is considered that in the lamb when opened there was scarcely a drachm of blood; in the sheep not 3 oz. to be seen. From the celerity of the motion of blood now mentioned, we may give a good account of the sudden refection with victuals, and particularly such as are liquid; we may also hence account for the quick passing of urine, and also the quick motion of the chyle into the breasts of nurses, without supposing unknown passages, from the stomach or any other part, into the bladder and breasts. Half an ounce of blood at a diastole is the greatest quantity that I remember any anatomist supposes to get into the heart, and they suppose the quantity of blood in the body to be between 15 and 25 lb. by which it will appear how their computations and mine differ.

Account of Books.—I. Propositiones Hydrostaticæ ad illustrand. Aristarchi Samii Systema destinatæ, et quædam Phænomena Naturæ Generalia. Auth. Francisco Jessop, Arm. Lond. 4to. 1687. N° 191, p. 440.

This book contains an hypothesis, and a pretended demonstration, concerning the nature of bodies and their motions. The author defines a dense body to be that which has least vacuity. A bubble a rarer fluid in a more solid, a drop a denser fluid in a more rare. His position is, that a dense body is more potent than a rare. His supposition is, that in a body mixed of these two, both endeavour to recede from the centre of it; and thence his first proposition is, that the most dense parts will get out most, the less dense will remain in inner stations, which he calls natural. 2. He supposes, any other body immersed in this will find and take its natural station, according to its comparative density, the endeavour to go to this natural station, if downwards, is gravity, if upwards, is levity. The conflict between the included and passing particles will create a vortex, whose included matter will move exactly as the elliptical hypothesis of the planets supposes, and answer not only to that but to any other hypothesis. To many other fanciful explanations he adds four new lemmas, which he applies to the explication of Cartesius's system, supposing the matter between the vortices that join to that of the sun, to influence that by its ingress, so as to keep the sun in the focus of the elliptic vortex. This premised, he subjoins his first lemma, where he presents the sun in the centre of gravity of its system, without vortex or turbinated motion; in the second, he directs it to the focus of the ellipse; in the third, he generates the vortex by the conflict of the entering and contained matter, exactly agreeing with the phænomena, and turns round the sun by the motion of the vortex, giving the planets their exact motions, which they cannot deviate from.

Lastly, he generates earths or planets by the coalition of many smaller into greater bodies; these he explains more particularly from his principles, and then answers two objections which may be made against it, and a third, which a demonstration of Mr. Newton's, upon a supposition of his, directly opposes; to which he answers, that that hypothesis ought to be corrected, and gives his reasons, which he thinks sufficient. He disapproves of the hypothesis of the planets gravitating upon each other, and explains his reasons from the similitude of a ship in the water; and ends his epistle with this conclusion, that though the moon were a thousand times larger than the earth, it would not be able to move the least sand out of its place if that were the centre of the earth. The like he supposes of the other planets, with respect to the sun.

II. *Tabularum Astronomicarum Pars prior, de Motibus Solis et Lunæ, necnon de Positione Fixarum, ex ipsis Observationibus deductis, cum Usu Tabularum, &c. Authore Ph. de la Hire, Regio Matheseos Professore ac Regiæ Scientiarum Academiæ Socio. 4to. Par.* 1687. N° 191, p. 443.

This author, long since eminent for his skill in geometry, now succeeds the accurate Mr. Picart in the Royal Observatory at Paris, and this book is the first fruits of his astronomical endeavours. It is chiefly designed to teach an accurate method of calculus for eclipses, especially solar, where he makes use of the contemplation of the constructions of them by the parallel of latitude supposed to be projected into an ellipsis, which is here attributed to Mr. Cassini, though first published in English by Mr. Flamsteed in Sir Jonas Moor's works, under the title of The Doctrine of the Sphere, and there asserted to its first inventor, Sir Christopher Wren.

What is most considerable in this book is the large table of the longitudes and latitudes of places, chiefly in France, which have been taken by the king's order, with great exactness. 2. A table of the right ascensions and declinations of 63 principal fixed stars, to the year 1686 complete, deduced from new and accurate observations. 3. An empirical table of the moon's equations in the new and full; deduced simply from observations of eclipses. 4. A correction of the moon's motion, arising from the distance of the moon from the apogæon of the sun, or, which is the same, from the anomaly of the sun; the discovery of which is amply shown in the appendix to the catalogue of the southern stars, published in London by E. Halley in the year 1679, and soon after translated into French, and printed in Paris; wherein is contained not only the form of this equation of the lunar motions, but the very quantity, viz. 13 m. exactly as Mr. De la Hire has it. And whereas Mr. De la Hire seems to conclude, that this equation ceases in the quadratures, and is greatest in the new and full moons, when he comes to the consideration of the lunar motions extra syzygias, which is here promised, he will find it no less requisite in the quadratures than in eclipses, several undoubted observations showing the necessity of it.

Among the precepts for the use of the tables there is a pretty remark concerning refractions, which this author says he has often experienced, viz. that the beams of the stars being observed in a deep valley to pass near the brow of the hill, are always more refracted than if there were no such hill, or the observation was made on the top of it; as if the rays of light were bent downwards in a curve, by passing near the surface of the mountain.

The Report made to his Majesty by the Company of Parish Clerks of London, of the Number of Christenings and Burials in the Years 1686 *and* 1687. N° 191, p. 445.

Anno 1686.	Christened.	Anno 1687.	Christened.
Males......	7575	Males......	7737.
Females.....	7119	Females....	7214.
In all.......	14694	In all.......	14951.

Buried.		Buried.	
Males......	11828	Males......	11174.
Females....	10781	Females....	10286.
In all......	22609	In all......	21460.

END OF VOLUME SIXTEENTH OF THE ORIGINAL.

An Account of an Observation of an Eclipse of the Moon, observed at Moscow in Russia, on April 5, 1688, compared with the same observed at Leipsic; by which the Longitude of the former Place is ascertained; with the Latitude of several principal Places in the Empire of Russia. N° 192, p. 453. *Vol. XVII.*

The Royal Society being desirous to contribute what they can to the rectifying of geography, and determining the longitudes of places, among others recommended the procuring an eclipse to be observed at Moscow, to an eminent merchant trading there; at whose instance one Mr. Timmerman, a mathematician at the place, returned the following account.

April 5, 1688, half a quarter of an hour after 7 in the evening, the moon arose clear, without any sign of eclipse; at 7¼h. the moon went into a thick cloud, but was again clear at 7h. 38m. when the under side of the body of the moon was begun to be obscured in a clear sky; she being then 6½° above the horizon.—At 9h. the whole under side of the moon was eclipsed, and about 8m. after 9 it was at the height, or rather seemed to decrease. At 9½h. there was still a third part of the moon eclipsed.—About 10 it decreased apace, and at 10½h. there was but little to be seen; at 10h. 45m. it was certainly ended, the moon being then about 22° high. Thus far the observer.

The duration of this eclipse is here made from 7h. 38m. to about 10h. 45m. which agrees within 8 or 10m. with our tables, that never err sensibly in the continuance of eclipses; and so much ought to be allowed to an observer not sufficiently instructed to distinguish the penumbra from the true shadow, though a small telescope was used in this observation, as we are since informed. Let us conclude then that the end was at 10h. 40m. at Moscow. This eclipse was observed at Leipsic by Mr. Gotfrid Kirch, and published in his Ephemerides for the year 1689, where the end is determined at 8h. 54m. P. M. Hence Moscow will be 1h. 46m. to the eastward of Leipsic; and the difference of meridians between London and Leipsic being already determined 49m. it will follow that Moscow is 2h. 35m. to the east of London, or 38° 45′ of longitude, which from other accounts we find to be very near that of the city of Aleppo in Syria.

From the same person we have received the latitudes of the following places, observed with a large quadrant.

Moscow 55° 34′, Yereslaw 57° 44′, Wologda 59° 19′, Woslak 61° 15′, Arkangel 64° 30′.

On the most seasonable Time for felling Timber. By Robert Plot, LL. D. and R. S. Soc. N° 192, p. 455.

The custom of felling timber here in the south of England, differs from that of Staffordshire, only in two things, viz. In the time of felling, and manner of barking. It being felled here in the spring, as soon as the sap is found to be fully up, by the trees putting out, and then barked after the trees are prostrate, the sap yet remaining in their bodies: whereas, there it is first barked, (in the spring as here) but before it is felled, the trees yet living and standing all the summer, and not felled till the following winter, when the sap is fully in repose.

Now in the spring season, and some time after, all the trees are pregnant, and spend themselves, as animals do in their respective offsprings, in the production of leaves and fruit, and so become weaker than at other times of the year; their cavities and pores being then turgid with juices or sap, which, the trees being felled at that time, still remain in the pores, having no way of being otherwise spent, and there they putrify; not only leaving the tree full of these cavities, which render the timber weak; but breeding a worm, as testified by both Pliny and Mr. Evelyn, which will so exceedingly injure it, as to become altogether unfit for great stress. Now all timber felled at this time of year, whether the juices putrify, or otherwise evaporate, or dry away, is not only subject to rift and gape, but to shrink so considerably, that a piece of such timber, of a foot square, will usually shrink in breadth $\frac{3}{4}$ of an inch; than which, says Vegetius, nothing is more pernicious if used for the building of ships. To which Julius Cæsar adds, that though ships may be made of such moist timber, felled in the spring, yet they will certainly be slugs, not near so good sailers as ships made of timber felled later in the year.

In all which circumstances most of the ancients so very nearly agree, that none of them advise the felling of timber for any sort of use before autumn at soonest; others, not till the trees have borne their fruit, which, says Theophrastus, must always be proportionably later, as their fruits are ripe later in the year: others again not till mid-winter: not till November says Palladius: nay, not till the winter solstice, says Cato; and then too in the decrease or wane of the moon, between the 15th and 23d day of her age, says Vegetius; or rather, according to Collumella, between the 20th and the new moon. In general, says Theophrastus, the oak must be felled very late in the winter, not till December, as the emperor Constantinus Pogonatus positively asserts, the moon too being then under the earth, as it is for the most part in the day-time in the first part of its decrease. And the felling of oak within those limits, they call tempestiva cæsura, felling timber in season, which they all unanimously pronounce, if thus felled, it will neither shrink, warp, nor cleave, nor decay in many years; it being tough as horn, and the whole tree in a manner, as Theophrastus asserts, as hard and firm as the heart; with whom also agrees Mr. Evelyn. If you fell not oak, says he, till the sap is in rest, as it is commonly about November and December, after the frost has well nipped them, the very saplings thus cut, will continue without decay, as long as the heart of the tree.

And the reason of this is briefly given by Vitruvius, because the winter air closes the pores, and so consequently consolidates the trees; by which means, the oak, as he and Pliny both express it, will acquire a sort of eternity in its duration; and much more so, if it be barked in the spring, and left standing all the summer, exposed to the sun and wind, as is usual in Staffordshire and the adjacent counties; by which they find, by long experience, the trunks of the trees so dried and hardened, that the sappy part in a manner, becomes as firm and durable as the heart itself.

And though this way of barking and felling of timber were unknown to the ancients, as perhaps it is to all the world except those few counties, yet they seem not unacquainted with the reason of the practice: for Seneca observes, that the timber most exposed to the cold winds, is most strong and solid, and that therefore Chiron made Achilles's spear of a mountain tree. Homer also tells us, that the spear of Agamemnon was made of a tree so exposed; for which Didymus gives for reason, that being continually weather-beaten, it becomes harder and tougher. And Pliny says expressly as much for the sun, as they for the wind, viz. That the wood of trees exposed to the sun-shine is the most firm and durable; for which reason also it is, that Vitruvius prefers the timber on the south-side the Apennine, (where it winds about and incloses Tuscany and Campania, and strongly reflects the constant heats of the sun upon it, as it were from a concave,) incomparably before that which grows on the north side of the same hill, in the shady moist grounds: and his reason is, that the sun not only exhales the superfluous moisture of the earth, whence the trees are supplied in such shady places with too great a quantity, but in great measure the remaining juices out of the trees themselves, rendering the timber of them the more close, substantial, and durable; which certainly it would do much more effectually, if the bark were taken off in the spring of the year, as in Staffordshire, where the people use this method for their timber, though but for private uses. And much more should it be done for so public a concern as the building of ships, where tough and solid timber is much more necessary than in ordinary buildings. There is indeed an Act of Parliament, 1 Jac. 1, chap. 22, which forbids felling timber for ordinary uses, in consideration of the tan, at any other time than between the 1st of April and last of June, when the sap is up, and the bark will run. To which I readily answer, that perhaps the legislators did not consider that the bark might be taken off in the spring, and that the tree would live and flourish till the ensuing winter: so that though the tree be not felled till the winter solstice, or January following, yet the tanner is not at all defeated of his tan, but has it in due season. And in that very act of parliament there was an exception of the timber to be used in ship building, which might be felled in winter, or any other time; as I am told all the ancient timber remaining in the Royal Sovereign was, it being still so hard that it is no easy matter to drive a nail into it.

It is true indeed, that the barking or peeling the tree standing, is somewhat more troublesome, and therefore rather more chargeable, than when they are prostrate; and that it is likely people have therefore usually felled their timber, as well for shipping as other uses, in the spring of the year, for the sake of the more easy and cheap barking it, rather than any thing else. It is also true, that timber is harder to fell in winter, it being then so compact and firm, that the axe will not make so great an impression, as in the spring; which will a little increase the price of the felling, and its sawing afterwards; but how inconsiderable these things are, in comparison of the great advantage of this manner of felling, is self-evident.

The greatest objection that can be urged against this practice, is, that if the timber be not felled till mid-winter or January, where it grows in coppices and woods, they cannot perhaps inclose their young sprigs so soon as some may imagine needful, and therefore they may be backward to fell their timber at that season. To which I answer, that the timber so felled in woods or coppices may be easily carried off, before the second spring, and so the prejudice be small: but what will quite remove this inconsiderable difficulty, is, that perhaps it may be expedient, that no timber whatever, growing in woods or coppices, be at all bought for the king's yards, because that timber growing in such shady places, and so fenced from sun and wind, as timber in woods for the most part is, cannot be so good as that which comes from an exposed situation, such as it usually has in forests, parks, and hedge-rows or open fields; where at least it is indifferent, if not better for the proprietor, that it be felled in winter, when the grass and corn is off the grounds, than in the spring itself. And the officers designed for that purpose may buy all their timber under the conditions of its being felled in winter, after the bark has been taken off in the spring in due time.

A Description of the Pimenta or Jamaica Pepper-Tree, and of the Tree that bears the Cortex Winteranus. By Hans Sloane, M. D. and Reg. Soc. S.* N° 192, p. 462.

The myrtus arborea foliis laurinis aromatica,† pimenta, Jamaica-Pepper, or

* Sir Hans Sloane, baronet, so celebrated in the annals of Natural History, was a native of Ireland, and was born at Killileagh, in the north of that country, in the year 1660. He displayed a very early propensity to the study of physic; but his pursuits were interrupted by a spitting of blood with which he was affected in his 16th year, and which confined him to a state of domestic seclusion for more than two years. After this he devoted himself to his professional studies, and having attended in London the usual anatomical and chemical lectures, and pursued the study of botany in the Chelsea garden, then but just established, he went to Montpelier in France, where he formed an acquaintance with the celebrated Tournefort, and collected the plants of the south of France, &c. In the year 1680 he returned to London, where he was patronized by Sydenham, in whose house he resided for some time. He became a member of the College of Physicians, and was elected a Fellow of the Royal Society. He was soon afterwards induced, through a desire of improving his knowledge in Natural History, to attend the Duke of Albemarle to Jamaica in character of his physician, where, during a residence of little more than a year, he collected a vast variety of rare and curious plants, &c. On his return he resumed the practice of physic, became eminent in his profession, and was chosen physician to Christ's Hospital; devoting, as it is said, the sum he received from this appointment, to the relief of the poorest patients in the Hospital, being unwilling to enrich himself by the gains of a charitable institution. In 1693 he was elected Secretary to the Royal Society, and revived the publication of the Philosophical Transactions, which had been suspended for some time. His Museum, which he began early to collect, became, by incessant attention, superior to any thing of the kind at that time in England, and at length received a great accession by the valuable collection of shells belonging to Mr. Courten, at whose death they became by purchase the property of Sloane. Having now arrived at a considerable age, he was created a Baronet by George the First, was chosen a member of the Royal Academy of Paris, president of the College of Physicians of London, and on the death of Sir Isaac Newton, president of the Royal Society. At length, the infirmities of age increasing, he retired, at the age of 80, to Chelsea, where he continued the remainder of his life, dying on the 11th of January 1752, in the 91st year of his age. In his person, he is said to have been tall and well made; in his manners polite and engaging. He bequeathed to the Company of Apothecaries, the entire freehold of the Botanical Garden at Chelsea; his Museum to the public, on condition that the sum of 20,000l. should be paid to his family, a sum which is said by some of his biographers to have been scarcely more than the intrinsic value of the gold and silver medals in his collection. It is also to be observed that his library, consisting of more than 5000 volumes, was included. In the exercise of his profession, Sir Hans Sloane is said to have been remarkable for the certainty of his prognostics, and the hand of the anatomist frequently verified the predictions of the physician.

† Myrtus Pimenta. Linn.

all-spice-tree, has a trunk as thick as a man's thigh, rising straight up about 30 feet high, covered with a very smooth skin of a grey colour, and branched out on every hand, having the extremities of its twigs set with leaves of several sizes; the largest being 4 or 5 inches long, and 2 or 3 broad in the middle where broadest, decreasing to both extremities, and ending in a point, smooth, thin, shining, without incisures, of a deep green colour, and standing on long footstalks; when bruised, they are very odoriferous, and in all things like the leaves of a bay-tree. The ends of the twigs are branched into bunches of flowers, each footstalk sustaining a flower made up of four herbaceous or pale-green petala, bowed back or reflected downwards, within which are many stamina of the same colour. To these succeeds a bunch of crowned or umbilicated berries (the crown consisting of four small leaves) larger, when ripe, than juniper-berries; at first, when small, they are greenish; but when ripe, they are black, smooth, and shining, containing, in a moist, green, aromatic and biting pulp, two large acini or seeds, separated by a membrane lying between them, each of which is a hemisphere, and both together form a globe; whence Clusius makes it one seed divisible into two parts.

This tree grows on all the hilly parts of Jamaica, but chiefly on the north-side. It flowers in June, July, and August; but in several places sooner or later, according to their situation and different season for rain: and after it flowers, the fruit soon ripens; but it is to be observed, that in clear open grounds, it is sooner ripe than in thicker woods.

There is no great difficulty in the curing or preserving of this fruit for use. It is for the most part done by the negroes; who climb the trees, and pull off the twigs with the unripe green fruit, and afterwards carefully separate the fruit from the twigs, leaves, and ripe berries: after which, they expose them to the sun for many days, spreading them thin on cloths, and turning them now and then, and carefully avoiding the dews, which are there very great. By this means they become a little shrivelled and dry, and from a green, change to a brown colour, and then they are fit for the market. They are of different sizes, but generally of the size of black pepper, something resembling cloves, juniper berries, cinnamon, and pepper, in smell and taste, or rather having a peculiar mixed smell, somewhat akin to them all, whence the name of all-spice. The ripe berries are very carefully separated from those to be cured; whence, these berries coming always unripe dried into Europe, naturalists have thought them to be fructu umbilicato sicco. The smaller and more fragrant they are, they are accounted the better.

On the Wild Cinnamon Tree. By Dr. Hans Sloane. N° 192, p. 465.

Arbor baccifera, laurifolia, aromatica, fructu viridi calyculato ramoso, or the wild cinnamon tree,* commonly, but falsely, called cortex winteranus, has a trunk about the thickness of a man's thigh, rising to about 20 or 30 feet high, with many branches and twigs hanging down from it, forming a beautiful top. The bark consists of two parts, one external and the other internal. The former bark is thin as a milled shilling, of a whitish ash or grey colour, with some whiter spots here and there upon it, and several shallow furrows of a darker colour, running variously through it, which make it rough; it is of an aromatic aste. The inner bark is much thicker than cinnamon, being as thick as a milled crown piece, smooth, of a whiter colour than the outer, and of a much more biting and aromatic taste, something like that of cloves, and not glutinous like cinnamon, but dry and crumbling between the teeth. The leaves come out near the ends of the twigs without any order, standing on foot-stalks an inch long, they are each of them 2 inches in length, and 1 inch broad near the end were broadest, and roundish, being narrow at the beginning, from whence they increase in breadth to near the end; they are of a yellowish green colour, shining and smooth, without any incisures about the edges, and somewhat resembling the leaves of bay or laurocerasus. The ends of the twigs are branched into branches of flowers, standing something like umbellæ, each of which has a footstalk, on the top of which is a calix, consisting of some small leaves, in which stand five scarlet or purple petala, within which is a large stylus. To these succeed so many calyculated berries of the size of a large pea, roundish and green, and containing, within a mucilaginous pale green thin pulp, four black shining acini or seeds, of an irregular figure.

All the parts of this tree, when fresh, are very hot, aromatic, and biting to the taste, something like cloves. It grows in the low-lands or Savanna woods, and very frequently on each side of the road between Passage Fort and the town of St. Jago de la Vega, in Jamaica, as also in Antigua and the other Caribbee islands. The bark of this tree is what is chiefly in use, both in the plantations of the English between the tropics in the West Indies and in Europe; and is easily cured by only cutting off the bark, and suffering it to dry in the shade.

On the Circulation of the watery Vapours of the Sea, and the Origin of Springs. By Mr. E. Halley. N° 192, p. 468.

Some time since I showed an experiment of the quantity of water raised in

* Winterana Canella. Linn.

vapour from the surface of the sea in a day's time, which was so far approved by some honourable members of this society, that I have received their commands to prosecute those inquiries, and particularly in relation to the method used by nature to return the said vapours again into the sea; which is so justly performed, that in many hundreds of years we are sufficiently assured that the sea has not sensibly decreased by the loss in vapour, nor yet abounded by the immense quantity of fresh water it receives continually from the rivers. To demonstrate this equilibrium of receipt and expence in the whole sea, is a task too hard for me to undertake, yet in obedience to those whom I have the honour to serve I shall here offer, what to me has hitherto seemed the most satisfactory account of this grand phenomenon. I have formerly attempted to explain the manner of the rising of vapour by warmth, by showing that if an atom of water were expanded into a shell or bubble, so as to be ten times as large in diameter as when it was water, such an atom would become specifically lighter than air, and rise as long as that flatus, or warm spirit, that first separated it from the mass of water, shall continue to distend it to the same degree; but that warmth decreasing, and the air growing cooler, and so specifically lighter, the vapours consequently will stop at a certain region of the air, or else descend; which may happen on several accounts, as will appear below. Yet I assert not that this is the only principle of the rise of vapours, and that there may not be a certain kind of matter whose conatus may be contrary to that of gravity; as is evident in vegetation, wherein the tendency of the sprouts is directly upwards, or against the perpendicular. But whatever be the true cause, it is in fact certain that warmth does separate the particles of water, and emit them with a greater velocity as the heat is more intense, as is evident in the steam of a boiling cauldron, wherein likewise the velocity of the ascent of the vapours visibly decreases till they disappear, being dispersed into and assimilated with the ambient air. Vapours being thus raised by warmth, let us in the first place suppose, that the whole surface of the globe were all water to a great depth, or rather, that the whole body of the earth were water, and that the sun had its diurnal course about it. It would follow, that the air of itself would imbibe a certain quantity of aqueous vapours, and retain them like salts dissolved in water; that the sun warming the air, and raising more plentiful vapours from the water in the day time, the air would sustain a greater proportion of them, as warm water will hold more dissolved salts, which, upon the absence of the sun in the nights would be all again discharged in dews, analogous to the precipitation of salts on the cooling of the liquors, nor is it to be thought that in such case there would be any diversity of weather, other than periodically, every year alike; the mixture of all terrestrial, saline, heterogeneous vapours being taken away; which, as they are variously compounded, and brought by the winds, seem to be the causes of those various seasons which we now find. In this case the region of air, every where at the same height, would be equally replenished with the proportion of water it could contain, regard being only to be had to the different degree of warmth, from the nearness or distance of the sun; and an eternal east wind would blow all round the globe, inclining only to the same side of the east, as the latitude does from the equator; as is observed in the ocean between the tropics.

Next, let us suppose this ocean interspersed with wide and spacious tracts of land, with high ridges of mountains, as the Pyrenean, the Alps, the Apennine, the Carpathian in Europe, Taurus, Caucasus, Imaus, and several others in Asia; Atlas and the Montes Lunæ, with other unknown ridges in Africa, whence come the Nile, the Niger, and the Zaire; and in America the Andes, and the Apalatean mountains, each of which far surpass the usual height to which the aqueous vapours of themselves ascend, and on the tops of which the air is so cold and rarefied, as to retain but a small part of those vapours brought thither by the winds. Those vapours therefore that are raised copiously in the sea, and by the winds carried over the low lands to those ridges of mountains, are there compelled by the stream of the air to mount up with it to the tops of the mountains, where the water presently precipitates, gleeting down by the crevices of the stone, and part of the vapour entering into the caverns of the hills, they are collected, as in an alembic, into the basins of stone they find there, which being once filled, all the overplus of water that comes thither runs over by the lowest place, and breaking out by the sides of the hills forms single springs; many of these running down by the valleys or guts between the ridges of hills, and coming to unite, form little rivulets or brooks; many of these again meeting in one common valley, and gaining the plains, being grown less rapid, they become a river; and many of these being united in one common channel, make such streams as the Rhine, the Rhone, and the Danube; which latter one would hardly think the collection of water condensed out of vapour, unless we consider how vast a tract of ground that river drains, and that it is the aggregate of all those springs which break out on the south side of the Carpathian mountains, and on the north side of the immense ridge of the Alps, which is one continued chain of mountains from Switzerland to the Black Sea. And it may generally pass for a rule, that the magnitude of a river, or the quantity of water it discharges, is proportionable to the length and height of the ridges, from whence its fountains arise.

Thus then is one part of the vapours, blown upon the land, returned by the rivers into the sea, from whence they came. Another part, by the cool of the night, falls in dews, or else in rains, again into the sea, before it reaches the land, which is by much the greatest part of the whole vapour, because of the great extent of the ocean, which the motion of the winds does not traverse in a very long space of time. And this is the reason why the rivers do not return so much into the Mediterranean as is extracted in vapour. A third part falls on the lower lands, and is the pabulum of plants, where yet it does not rest, but is again exhaled in vapour by the action of the sun, and is either carried by the winds to the sea, to fall in rain or dew there, or else to the mountains, to be there turned into springs; and though this does not immediately happen, yet after several vicissitudes, of rising in vapour and falling in rain or dews, each particle of the water is at length returned to the sea from whence it came. Add to this, that the rain waters, after the earth is fully sated with moisture, by the valleys or lower parts of the earth finds its way into the rivers, and so is compendiously sent back to the sea. After this manner is the circulation performed, and I doubt not but this hypothesis is more reasonable than that of those who derive all springs from the rain waters, which yet are perpetual and without diminution, even when no rain falls for a long space of time; or than that which derives them from a filtration of the sea waters through certain imaginary tubes or passages within the earth, wherein they lose their saltness. This latter hypothesis, besides many others, labours under this principal absurdity, that the greatest rivers have their most copious fountains farthest from the sea, and whither so great quantities of fresh water cannot reasonably be derived any other way than in vapour. This, if we may allow final causes, seems to be the design of the hills, that their ridges being placed through the midst of the continents, might serve as it were for alembics to distil fresh water for the use of man and beast, and their heights to give a descent to those streams to run gently, like so many veins of the macrocosm, to be the more beneficial to the creation.

Now this theory of springs is not a bare hypothesis, but founded on experience, which it was my luck to gain in my stay at St. Helena, where in the night time, on the tops of the hills, about 800 yards above the sea, there was so strange a condensation, or rather precipitation of the vapours, that it was a great impediment to my celestial observations; for in the clear sky the dew would fall so fast as to cover, each half quarter of an hour, my glasses with little drops, so that I was necessitated to wipe them so often, and my papers on which I wrote my observations would immediately be so wet with the dew, that it would not bear ink.

On the Modern Theory of Generation, by Dr. George Garden, of Aberdeen. N° 192, p. 474.

You know how unconclusive men's conjectures were on this head until this age, when Dr. Harvey discovered the proper place of the formation of the chick in the cicatricula of the egg, and the formation of the parts, so far as was discernible by the naked eye; and after him Malpighi, by the help of exact glasses, observed the first rudiments of it there, both before and after incubation: and R. de Graaf and others, having upon many observations concluded, that the testes fœminei were the ovaries of females, and consequently that all animals were ex ovo; they began from hence to infer, that the rudiments of each animal were originally in the respective females, and that the male contributed only to give a new ferment to the mass of the blood and spirits, by which means a spirituous liquor (which the blood in its ordinary ferment could not produce) insinuated itself to the same ducts and pores of the rudiments of those animals which were in greatest forwardness in the ovary, and so extended and enlarged all their parts, and at last brought them to perfection, as M. Perrault ingeniously discourses in the 3d part of his Essais de Physique; till now at last Leuwenhoeck has discovered an infinite number of animalcula in semine marium of all kinds, which has made him condemn the former opinions about the propagation of all animals ex ovo.

Now upon comparing the observations and discoveries which have been made with each other; these three things seem to me very probable. 1. That animals are ex animalculo. 2. That these animalcules are originally in semine marium, et non in fœminis. 3. That they can never come forward, nor be formed into animals of the respective kind, without the ova in fœminis.

The first of these seems probable from these three observations. 1. That some such thing has been so often observed by Malpighi in the cicatricula of an egg before incubation, as the rudiments of an animal in the shape of a tadpole, as may be seen in his first, and in his repeated observations de formatione pulli in ovo. 2. The sudden appearance and displaying of all the parts after incubation makes it probable, that they are not then actually formed out of a fluid, but that the stamina of them have been formerly there existent, and are now expanded. The first part of the chick which is discovered with the naked eye, is the punctum saliens, and that not till 3 days and nights of incubation be past, and then on the 5th day the rudiments of the head and body appear. After an incubation of 30 hours, are visible by glasses the head, the eyes, and the carina, with the vertebræ, distinct, and the heart. After 40 hours, its pulse is visible, and all the other parts more distinct, which cannot be discerned by the naked eye before the beginning of the 5th day; from whence it seems very probable, that even the so early discovery of those parts of the fœtus by the microscope is not the discerning of parts newly formed, but only more dilated and extended by the receiving of nutriment from the colliquamentum; so that they seem all to have been actually existent before the incubation of the hen.

But the 2d thing which later discoveries have made probable is, that these animalcules are originally in semine marium, et non in fœminis. And this I collect from these considerations. 1. That there are innumerable animalcula discovered in semine masculo omnium animalium. Mr. Lewenhoeck has made this so evident by so many observations, that I do not in the least question the truth of the thing. The reason of their multitude, and some of the difficulties which thence arise, he has cleared to very good purpose, so that I shall not repeat them. 2. The observing the rudiments of the fœtus in eggs, which have been fecundated by the male, and the seeing no such thing in those which are not fecundated, as appears from Malpighi's observations, make it very probable that these rudiments proceed originally from the male, and not from the female. 3. The resemblance between the rudiments of the fœtus in ovo, both before and after incubation, and the animalcule, makes it very probable that they are one and the same. The same shape and figure which Mr. Leuwenhoeck gives of the animalcule, Malpighi likewise gives of the rudiments of the fœtus, both before and after incubation, and even the fœtuses of animals appear so at first to the naked eye, that Dr. Harvey acknowledges that all animals, even the most perfect, are produced from a worm. 4. This gives a rational account of many fœtuses at one birth, especially that of the Countess of Holland; and how at least a whole cluster of eggs in a hen are fecundated by one coition of the male. 5. The analogy between the manner of the propagation of plants and animals likewise makes this probable. Every herb and tree bears its seed after its kind, which seed is nothing else but a little plant of the same kind, which being thrown into the earth, as into its uterus, spreads forth its roots, and receives its nourishment; but has its form within itself; and we may rationally conjecture some such analogy in the propagation of animals.

The 3d particular which later discoveries make probable, is that animals cannot be formed of these animalcules without the ova in females, which are necessary for supplying them with proper nutriment: and this is evinced from these considerations. 1. It is probable, that an animalcule cannot come forward, if it do not fall into a proper nidus. This we see is the cicatricula in eggs, and though a million of them should fall into an egg, none of them would come forward, but what were in the centre of the cicatricula, and perhaps the nidus necessary for their formation is so proportioned to their bulk, that it can hardly contain more than one animalcule; and this may be the reason why there are so few monsters. This we see is absolutely necessary in the oviparous kinds; and the only difference which seems to be between them and the viviparous, in this matter, is in this, that in the latter the ova are properly nothing more than the cicatricula with its colliquamentum, so that the fœtus must spread forth its roots into the uterus to receive its nourishment; but the eggs in the oviparous may be properly termed a uterus in relation to the fœtus; for they contain not only the cicatricula, with its amnion and the colliquamentum, which latter is the immediate nourishment of the fœtus, but also the materials which are to be converted into that colliquamentum, so that the fœtus spreads forth its roots no farther than into the white and yolk of the egg, from whence it derives all its nourishment. Now, that an animalcule cannot come forward without some such proper nidus, will not readily be denied; for if there were nothing needful but their being thrown into the uterus, I do not see why many hundreds of them should not come forward at once, at least, whilst scattered in so large a field. 2. That this cicatricula is not originally in the uterus, seems evident from the frequent conceptions which have been found without the uterus: such as the child which continued 26 years in the woman of Toulouse's belly, mentioned in N° 139, of the Phil. Trans.; and the little fœtus found in the abdomen de St. Mere, together with the testicle torn and full of clotted blood, recorded N° 150, both taken out of the journal des Sçavans: such also seems to be the fœtus in the abdomen of the woman of Copenhagen, mentioned in the Nouvelles des Lettres, for Sept. 25, page 996, all the members of which were easily to be felt through the skin of the belly, and which she had carried in her belly for 4 years; and the 7 years gravidation related by Dr. Cole, N° 172, of the Transactions. That these two were undoubtedly without the uterus is uncertain, because the last was not opened after her death, and the former may be yet still alive. Now granting once the necessity of a proper nidus for the formation of an animalcule into the animal of its respective kind; these observations make it probable, that the testes are the ovaria appropriated for this use; for though the animalcules coming thither in such cases may seem to be extraordinary, and that usually the impregnation is in the uterus; yet it may be collected from hence, that the cicatriculæ or ova to be impregnated, are in testibus fœmineis; for if it were not so, the accidental coming of animalcules thither could not make them come forward more than in any other part of the body, since they cannot be formed and nourished without a proper nidus. 3. It is acknowledged by all, that the fœtus in the uterus, for some considerable time after conception, has no connection with the womb; that it sits wholly loose from it, and is perfectly a little round egg with the fœtus in the midst, which sends forth its umbilical vessels by degrees, and at last lays hold on the uterus. Now from hence it seems evident that the cicatricula, which is the fountain of the animalcule's nourishment, does not sprout from the uterus, but has its origin elsewhere, and falls in thither as into a fit soil, from whence it may draw nutriment for the growth of the fœtus: otherwise it cannot be easily imagined how it should not have an immediate connection with the uterus from the time of conception. If we join all these three considerations together, viz. that an animalcule cannot come forward without a proper nidus or cicatricula; that there have been frequent fœtuses extra uterum; and that they have no adhesion to the uterus for a considerable time after conception; they seem to make it evident that animals cannot be formed from animalcules, without the ova in fœminis. To all these I shall subjoin the proposal of an experimentum crucis, which may seem to determine whether the testes fœminei be truly the ovaria, viz. Open the abdomen of the females of some kinds, and cut out these testicles, and this will determine whether they be absolutely necessary for the formation of animals.

There are some difficulties proposed against this conjecture, which I think may be easily resolved. Some object the distance between the tubæ or cornua uteri and the testicles; but to this is opposed, by Swammerdam and others, the like distance between the infundibulum, in hens and frogs, and the ovary, and yet it cannot be denied that the eggs are transmitted through this into the uterus: and besides R. de Graaf, and others, have by repeated observations found that the cornua uteri do at certain times after conception embrace the testes on both sides the uterus. They object in the second place the great disproportion between the pretended eggs in the ovary, and the aperture of the tubæ or cornua uteri, the former being a great deal larger than the latter: but both R. de Graaf and Malpighi have cleared that matter by showing, that these bladders in the ovary are not the ova, but serve to form the glandules within which the ova are formed, which break through a small papilla opening in the glandule, which bears a proportion to the aperture of the tube. They object, 3. The difficulty to conceive how these eggs should be impregnated per semen maris, both because there is no connection between the tubæ and the ovary for its transmission, and because Dr. Harvey could never discover any thing of it in utero. As to the last, Mr. Leuwenhoeck has cleared that difficulty by the discovery of innumerable animalcula seminis maris in cornubus uteri, and those living a considerable time after coition. N° 174 of the Transactions. And as to the former, we may either suppose that there is such an inflation of the tube or cornua uteri tempore coitionis, as makes them embrace the ovaria, and such an approach of the uterus and its cornua, as that it may easily transmit the seed into the ovary; or else that the ova are impregnated by the animalcules after they descend into the uterus, and not in the ovary; the former seems probable for this reason, that at least a whole cluster of eggs in a hen will be fecundated by one tread of the cock: now this fecundation seems to be in the vitellary, and not in the uterus, as the eggs pass along from day to day, for it can hardly be supposed that the animalcules should subsist so long, being scattered loosely in the uterus, as to wait there for many days for the fecundation of the eggs as they pass along. The latter conjecture has this to strengthen it, that the animalcules are found to live a considerable time in the uterus, and that if they should impregnate the ova in the ovary itself, the fœtus would increase so fast, that the ova could not pass through the tubæ uteri, but would either burst the ovary, or fall down into the abdomen from the orifices of the tubæ; and that from hence proceed those extraordinary conceptions in abdomine extra uterum. But, 4. Mr. Leuwenhoeck, N° 147 of the Transactions, to weaken the third consideration, about the conceptions being like to an ovum in the womb, proposes a parallel between these animalcules and insects, and insinuates that as the latter cast their skins, and appear of another shape, so the other which at first seem like tadpoles, may cast their outer skin, and then be round, and that this may be the occasion of the round figure of the conception in the womb. To this it may be replied, that according to Mr. Leuwenhoeck's own sentiment, the animalcules cannot come forward, if they do not find the punctum or proper place for their nourishment, to which it seems they must have some adhesion. Now the conception in viviparous animals is not fastened to the womb for many days, nor adheres to any point of it; so that it seems this roundish body is not the animalcule, thus changed after having cast an outer skin, but is rather the cicatricula, or little egg, into which the animalcule has entered, as its punctum or place of nourishment: otherwise I do not see why they should not be adhering to the womb from the first conception, or why many hundreds of them are not conceived and formed together.

The Transit of the Planet Mercury over the Sun, October 31, 1690, *in the Morning, observed at Nuremberg. By Joh. Phil. Wurtzelbaur.* N° 192, p. 483.

8^h 30^m The sun emerged out of the clouds; above his disc, in the field of observation, at the distance of more than half a digit from the vertical to the right hand, though really to the left, Mercury appeared quitting the sun's limb.

8^h 36^m After Mercury had adhered a minute of time to the sun's undulating limb, he quitted it at 14° from the zenith towards the north.

8 49 The sun's altitude was observed 10° 5'.

The ratio of the diameters of the sun and the nucleus of Mercury, while he continued in the sun's lucid disc, as well as could be judged through the thick air, was as 1000 to $8\frac{1}{3}$. After he had arrived at the sun's limb, and had adhered near a minute to the undulating limb, and had recovered his genuine roundness, (which, from the light of the sun's disc, had before appeared elliptical), that ratio was as 1000 to $12\frac{1}{3}$.

An Experiment of the Injection of Mercury into the Blood, and its ill Effects on the Lungs. By A. Moulin, M.D. N° 192, p. 486.

I give an account of an experiment I made on a dog at Mr. Boyle's last autumn, which I take to prove that mercury is an enemy to the lungs. I injected into the jugular vein about $1\frac{1}{2}$ oz. of crude mercury, and observed the dog soon after to have a dry short cough, which seized him. I sewed up the wound, and sent away the dog to be looked after, observing no other effect of the quicksilver at that time. But about 2 days after I saw him, and found him troubled with a great difficulty of breathing, making a noise like that of a broken-winded horse; there was no tumor about the root of his tongue, neither was there any swelling found in the maxillary or parotid glands, though I diligently sought for it: neither was he observed to drivel, though I ordered him warm broth, in expectation of a salivation. The 4th day after the injection of the mercury, he died, being for the 2 days before so troubled with an orthopnæa, that he could sleep only when he leaned his head against something. I opened him, and found about him a pint of bloody serum extravasated in the thorax. I found also the outside of the lungs in most places blistered; for what I at first took to be some preternatural dilatations of the vesiculæ of the bronchia, were only blisters, or a separation of the common integuments of the lungs from their substance. Some of these were larger than a rouncival-pea, others were smaller, but most of them contained mercurial globules, to be seen even without opening in several of them, through the outward skin; opening discovered it in most of those that I had the curiosity to examine. Several of these I found broken, and on a little pressure observed the mercury to run out, and with it a little sanies; but upon a pretty strong pressure, I observed that a great quantity of that sanies issued out.

On opening the right ventricle of the heart, I found some particles of the quicksilver in the very midst of coagulated blood lodged there, and in that also contained in the arteria pulmonalis. I observed moreover blood coagulated after a very different manner (which I want words to express) from what I have seen at any other time, notwithstanding the various methods I had used to coagulate it, and this in the interstices between the columnæ of the aforesaid ventricle; and in this a greater quantity of quicksilver than any where else in the dog. This coagulum was in the vertex of the ventricle, adhering pretty closely to the columnæ and parietes.

Opening the left ventricle, I found a very tenacious blood coagulated, and sticking firmly to the great valve, including the tendons of it, and a little resembling a polypus. In this ventricle I searched diligently for mercury, but found none; whence it may appear, that the mercury passed no farther than the extremities of the arteria pulmonalis: this occasioned the aforesaid blisters, and forced its way through the common coat of the lungs, partly by its weight, and partly by the propulsion of fresh blood to the same extremities, which by the mercury was stopped in its motion, and consequently forced its passage through that which most readily gave way, namely the common coat of the lungs.

I opened the aspera arteria down to the very bronchia, but could find no mercury in it, though I searched diligently for it. Each of the subdivisions, as well as divisions, of the bronchia, was filled with a sanies, which when I washed away, I found globules of mercury in many places under the bronchia, and on examination they proved to be in the arteria pulmonalis. I have pressed these globules backwards and forwards, and made some of them get out at the holes made in the vesiculæ above described. I took some pains to find where the sanies was received into the bronchia, but could not satisfy myself. From hence may appear the danger of using mercury in human bodies, so as that it may get into the mass of blood, especially into the lungs; as they want that brisk strong motion which the muscles have in other parts, which are able to force it along with the blood, in order to the raising a salivation. Their lax spongy texture makes them extremely unfit for clearing themselves of so troublesome a guest as mercury is. That it has this effect on human lungs, is plain from what we daily see in persons that have been often fluxed, who are after observed to die of consumptions, which will not yield to medicine.

Medicina Hydrostatica, or Hydrostatics applied to the Materia Medica, showing how by the Weight that divers Bodies used in Physic have in Water, one may discover whether they be Genuine or Adulterate. By the Hon. Robert Boyle, F.R.S. Lond. 8*vo.* 1690. N° 192, p. 488.

The honourable author designs in this treatise to show, that by weighing bodies in water, comparing their weight in air, and from thence deducing the proportion of weight to water, the specific gravity of bodies may thence be more exactly determined; counterfeits distinguished from genuine, and the mixture of mineral particles in stone discovered. Archimedes first observed that a body heavier than water, weighs less in water than in the air, by the weight of as much water as is equal to it in bulk. The difference then of the weights in air and water, gives the weight of so much water, and dividing the greater number by the less, the quotient compared to unity, will be the proportion of the weights of the solid body and water.

The author having shown in what manner all solid bodies heavier than water may be compared to one another, and not only their specific gravity, but their qualities very often detected: he next considers, 1. How bodies lighter than water may be examined in it, viz. by adding lead to wax or fir-wood, and subducting for the heavy body so added. 2. How fluids, as mercury, or chemical oils of cloves, &c. or bodies dissoluble in water, as sublimate, alum, vitriol, or fragments of any brittle body, viz. by a small glass bucket or phial stopped, which may receive these bodies, adding so much water as will fill the spatiola of the fragments, up to the brim of the glass; for which allowance must be made in the computation of the weight, both in the air and water.

Having examined bodies by water and oils, the author proceeds to examine the weight of liquors, by weighing solids in them. For if a heavy body in water lose so much of its weight as the quantity of fluid weighs that is of an equal bulk with the body, the proportion of the weight of all fluids will be easily obtained. Of waters, rain water seems the lightest, and scarcely 1000th part difference discovered in any of them. This is particularly applied to the famed water of the Ganges, though travellers assert an extraordinary lightness in it.

In the last place the bulk of solid bodies may be found out by this method. For since a cubical inch of water weighs 256 gr. and as much of oil of turpentine 221 gr. if a body of any magnitude, and irregular shape, lose so much or more times that weight in water or oil, it is of a magnitude equal to one or more cubical inches.

On the Time and Place of Julius Cæsar's Descent upon Britain. By Mr. E. Halley. N° 193, p. 495.

The authors that mention this expedition with any circumstances, are Cæsar in his Commentaries, lib. 4, and Dion Cassius, in lib. 39; Livy's account being lost, in whose 105th book might possibly have been found the story more at large. It is certain that this expedition of Cæsar's, was in the year of the consulate of Pompey and Crassus, which was in the year of Rome 699, or the

55th before the usual era of Christ: and as to the time of the year, Cæsar says that Exiguâ parte æstatis reliquâ, he came over only with two legions, viz. the 7th and 10th, and all foot, in about 80 sail of merchant ships, 18 sail that were ordered to carry over the horse, not being able to get out at the same time from another port, where they lay wind-bound. He says that he arrived about the 4th hour of the day, viz. between 9 and 10 in the morning, on the coast of Britain, where he found the enemy drawn up on the cliffs ready to repel him, which place he thus describes: Loci hæc erat natura, adeo montibus angustis mare continebatur ut ex locis superioribus in littus telum adjici possit, by which the cliffs of Dover and the South Foreland, are justly described, and could be no other land, being, he says, in the 5th book of his Commentaries, in Britanniam trajectum esse cognoverat circiter millium passuum triginta à continenti, the cliffs of the North Foreland being at a much greater distance. Here he says he came to an anchor, and staid till the 9th hour, or till between 3 and 4 in the afternoon, waiting till his whole fleet was come up; and in the mean time called a council of war, and advertised his officers, after what manner they were to make their descent, particularly, in relation to the surf of the sea, whose motion he calls celerem atque instabilem, quick and uneven. Then, viz. about 3 in the afternoon, he weighed anchor, and having gotten the wind and tide with him, he sailed about 8 miles from the first place, and anchored against an open and plain shore.

Here he made his descent; and having told us the opposition that was made, and the means he used to get on shore, he comes to say, that after he had been 4 days in Britain, the 18 ships with his horse put to sea, and were come in sight of his camp, when a sudden tempest arose, with contrary winds, so that some of the ships put back again, others were driven to the westward, not without great danger, and coming to anchor, they found they could not ride it out: so when night came on, they put off to sea, and returned from whence they came. That same night it was full-moon, which makes the greatest tides in the ocean, and they being ignorant thereof, their galleys, which were drawn on shore, were filled by the tide, &c.

Then he says that the day of the autumnal equinox being at hand, after some days stay, wherein there passed no action, because he kept close in his camp by the shore; and not thinking it proper to stay till the winter came on, he returned into Gallia. The next year he made a further expedition, with 5 legions and a good body of horse; but there is but little in the history thereof serving to our purpose, excepting that he says he set sail from the Portus Icius about sun-set, with a gentle S. W. wind, leni Africo profectus; that about midnight it fell calm, and being carried away with the tide, by the time it was day, he found he had left Britain on the left hand; but then the tide turning, they fell to their oars, and by noon reached that part of the island where he landed before, and came on shore without opposition; and then marched up into the country, leaving his ships at anchor in littore molli et aperto.

This is all in Cæsar that is any thing pertinent, and I find no where else any thing to guide us further, except one passage in Dion Cassius, who speaking of the first landing of Cæsar, says ὀ μέντοι και ᾗ ἔδει προσέχειν, that is, as I translate it, "but he landed not where he intended, for that the Britons hearing of his coming, had possessed all the usual places of landing." Ἄκραν οὖν τινὰ προέχουσαν περιπλεύσας ἑτέρωσε παρεκομίσθη. Κανταῦθα τοὺς προσμίξαντας οἱ ἐς τὰ τενάγη ἀποβαίνοντι νικήσας, ἔφθη τῆς γῆς κρατήσας, in my English, "Wherefore doubling a certain head land, he made to the shore on the other side, where he overcame those that skirmished with him at the water's edge, and so got safe to land." Here I make bold to translate the words ἐς τὰ τενάγη, "at the water's edge," which in H. Stephens' edition is interpreted "in paludibus," but I have the authority of Suidas, who says τέναγος, πελαγία ἰλὺς, or the sea mud, and is therefore properly the ouse on the sea shore; and by an easy figure, may be put for the shore itself, where such ouse commonly is found.

From these data, that it was in the year of the Consulate of Pompey and Crassus; that it was Exiguâ parte æstatis reliquâ, and 4 days before a full-moon, which fell out in the night time; the time of this invasion will be determined to a day: For by the eclipse of the moon, whereof Drusus made so good use to quiet a mutiny in the Pannonian army, on the news of the death of Augustus, it follows that Augustus died Anno Christi 14, which was reckoned anno urbis conditæ 767; and that this action was 68 years before, viz. in the 55th year before Christ current; in which year the full-moon fell out August 30, after midnight, or 31st in the morning before day; and the preceding full-moon was August 1, soon after noon; so that this could not be the full-moon mentioned, as falling in the day time; nor that in the beginning of July, it being not 10 days after the summer solstice, when it would not have been said exiguâ parte æstatis reliquâ. It follows therefore that the full-moon spoken of, was on August 30, at night, and that the landing on Britain was August 26, in the afternoon, about a month before the autumnal equinox; which agrees with all the circumstances of the story in point of time.

As to the place; the high land and cliffs described, could be no other than those of Dover, and are allowed to have been so by all; it remains only to examine whether the descent was made to the northward or southward of the place where he first anchored. The data to determine this, are, first that it was 4 days before the full-moon. 2. That that day, by 3 o'clock in the afternoon, the tide ran the same way he sailed. 3. That a S. by E. moon makes high-water on all that coast, the flood coming from the southward: hence it will follow, that that day it was high-water there about 8 in the morning, and consequently low-water about 2; therefore by 3 the tide of flood was well made up, and it is plain that Cæsar went with it; and the flood setting to the northward, shows that the open plain shore where he landed was to the northward of the cliffs, and must be in the Downs; and this I take to be little less than demonstration. A second argument is drawn from the wind with which he set out on his second expedition, viz. S. W. as appears by the words leni Africo profectus, with which the navigation of those times would hardly permit a ship to sail nearer the wind than 8 points, or a N. W. course; which would serve indeed to go into the Downs, but would by no means fetch the Lowland towards Dungeness, which is much about west from Calais, and not more than W. N. W. from Boulogne, if it shall be said that that was the Portus Icius from which Cæsar set out. Whence I take it to be evident, that if Cæsar was not bound more northerly than the South-Foreland, he could not have thought the Africus or S. W. wind proper for his passage, which was then intended for the place where he first landed the year before.

Justly to determine which the Portus Icius was, I find no where sufficient grounds; only Ptolemy calls the promontory of Calais-Cliffs by the name of Ἴκιον ἄκρον, whence there is reason to conjecture, that the Portus Icius was very near thereto, and that it was either Ambleteuse on one side, or Calais on the other. The same Ptolemy places Γισοῤῥίακον ἐπίνειον in the same latitude with the Ἴκιον ἄκρον, but something more to the east; which seems to refute those that have supposed the ancient port of Gessoriacum to have been Boulogne, whereas by Ptolemy's position, it must be either Dunkirk or Graveling; but most likely the former, both by the distance from the Ἴκιον ἄκρον, being about 20 miles or half a degree of longitude to the east, or $\frac{2}{5}$ of the whole coast of Flanders, which he makes but a degree and quarter from the Acron Icion to the mouth of the Scheld, which he calls Ostia Tabudæ: as also because that Pliny, l. 4. c. 16, speaking of Gessoriacum, says the Proximus Trajectus into Britain, from thence is 50 miles, which is too much, unless Gessoriacum were something more easterly than Calais: Dion Cassius makes the distance between France and Britain 450 stadia, or 56 miles, and says likewise it is the nearest, τὸ συντομώτατον. But this is in part amended by the explication given in the Itinerary of Antoninus, where the space between Gessoriacum and Rutupium is said to be 450 stadia, for this was the ordinary passage of the Romans into Britain, Rutupium being more northerly, and Gessoriacum more easterly, than the termini of Cæsar's voyage, and consequently the distance greater than 30 miles, which

Cæsar had observed: and now lately an accurate survey has proved the distance, between land and land, to be 26 English miles, or 28½ Roman miles, which shows how near Cæsar's estimate was to the truth.

Another argument, but not of equal force with the former, because of the modernness of the author, who wrote above 250 years after, may be drawn from the words of Dion Cassius, where he says ἄκραν τινὰ προέχουσαν περιπλεύσας ἑτέρωσε παρεκομίσθη, "that after his first anchoring, he sailed about a promontory to the place where he landed; now there are no other promontories on all that coast but the South-Foreland and Dungeness; the latter of which it could not be, because Cæsar says he sailed but 8 miles, and the Ness itself is about 10 miles from the south and nearest end of the Chalk-Cliffs by the town of Hithe; and to have gone round that point to the other side, the distance must have been much greater. So that the promontory spoken of by Dion must needs be the South-Foreland, and Cæsar must anchor near over against Dover, from whence sailing 8 miles, he would double a Head-land, and come to the Downs; which is such a coast as he describes in one place by apertum ac planum littus, and in his 5th book by molle ac apertum littus. As to Dion's words εἰς τὰ τενάγη, what I have already said about it seems sufficient to prove, that he means no more than the water's edge; and the etymologists derive it from τέγγω madefacio, because the wash and breach of the sea always keeps it wet. And this word τὰ τενάγη is used by Polybius for the sea ouse; and in another place he speaks of the difficulty of landing at the mouth of a river, διὰ τὴν τεναγώδη πάροδον, ob limosum accessum, so that it is not to be doubted that it ought to be rendered in this place, ad vadum maris rather than in paludibus. And so this objection against the assertion that Cæsar landed in the Downs, which is known to be a firm champaign country, without fens and morasses, will be removed; and the whole argument will, it is hoped, be admitted by the curious.

A Receipt for the Curing of Castorium, according to the method used in Russia. N° 193, p. 501.

Take the beaver stones,* and get the milk out of them as clean as you can; then set upon the fire a skillet or kettle with water, large enough to contain the quantity of stones to be cured; let the water boil, and put into it half a shovel full of clean wood ashes; then tie the stones together in pairs, and putting them into the water, let them boil there for half a quarter of an hour.

* The term beaver-stones (testes) is exceedingly improper, castor being (as has been already mentioned at p. 244, of this vol. of the abridgment) a substance distinct from them, secreted and contained in bags or follicles of its own.

Then take some birch-bark, and lay it on the fire, and let the stones be well smoked over it for the space of an hour, until they are well dried in the smoke; then hang them up in a kitchen, or in the air, for a weak or more, until they are perfectly dry and hard, after which they may be packed up in a cask, or otherwise, for sale. If there be more stones than will conveniently go into the skillet or kettle, you may make another boiling of them, and add a proportion of fresh ashes, and order them as before.

Observations on the making of Cochineal, according to a Relation of an old Spaniard at Jamaica, who had lived many Years in that Part of the West-Indies, where great Quantities of that rich Commodity are yearly made. N° 193, p. 502.

The insect, of which the cochineal is made, he affirms to be the same that we call the lady-bird, alias cow-lady;* which at first appears like a small blister, or little knob, on the leaves of the shrub on which they breed, which afterwards, by the heat of the sun, becomes a live insect, or small grub. This shrub is allowed by several authors to be the same that we call the prickle-pear, or Indian fig, having thick roundish leaves, that grow out of one another, and full of sharp prickles. These grubs in process of time becoming flies, like our lady-birds, as above, and being come to full maturity, which must be found out by experience in collecting them at several seasons, they kill by making a great smother of some combustible matter, to windward of the shrubs whereon the insects are feeding, having before spread some cloths all under the plants, whereby all the insects being smothered, by shaking the plants, they tumble down on the cloths. Thus they are gathered in great quantities with little trouble. Then they spread them on the same cloths in some bare sandy place, or stone pavement, and expose them to the heat of the sun, until they are dry, and their bodies shrivelled up, which being rubbed gently between the hands, will crumble into grains, and the wings separate from them, which must be garbled out. Others, it is said, expose them to the sun in broad and shallow copper basins, wherein the reflection of the sun dries them sooner. These plants, called the Indian fig, are easily and quickly propagated, by putting a single leaf above half its depth into the ground, which seldom fails to take root and throw out other new leaves at

* On the contrary, it belongs to the Linnæan genus *coccus*, and is the *coccus cacti* of Linnæus. A more particular description will occur in the ensuing volumes. What seems to have given rise to the error in this paper, relative to the real insect, is, that a species of lady-bird, viz. the *coccinella cacti* of Linnæus, is often found on the same plant, and gathered among the cocci.

the top; and of this plant, it is said, in Barbadoes impregnable fences are made. Others say they may be raised from the seed, or small grains, which are to be found in the proper season in the fruit, which is something like a fig, arising out of certain yellow flowers that grow out at the tops of the uppermost leaves, which fruit is full of a red pulp, that when full ripe stains the hands, like mulberries, with a purple colour, whereon, or on the blossoms, some say the insects feed; which perhaps may be the occasion of that rich tincture within their bowels. It may be inquired likewise, whether those grains, which are the seed of the fruit, may not produce some tincture, as well as the dried insects; or whether, whilst they are maggots, or small grubs, being ordered and dried as above, they may not shrivel up like grains, and be as good as when they are become flies with wings.

Some Experiments and Observations made of the Force of the Pressure of Water at great Depths, made and communicated to the Royal Society by a person of Rank. N° 193, p. 504.

Being off Pantalaria, near Sicily, in a calm, I let down a bottle 70 fathoms, stopped with a very good tender cork, well fitted, and bound down; and the cork came up in the bottle, which was three quarters full of salt water. The bottle was then again fitted with a very good cork, but of a woodiness or hardness as some corks are. This cork continued in its place, but as it were bruised, and the bottle, as before, about three quarters full of salt water. I then took a good ox bladder, and bound it four-fold over the mouth of the bottle, without any cork at all, only I put a piece of leather to keep the glass from cutting the bladder, as it might otherwise do, having rather a sharp mouth; and so ordered, it was let down as before, but when taken up again, it was without any water, or the least moisture in it.

Another time being in a dead calm, some leagues distant from the coast of South Spain, off the great hills of Granada, we took a bottle, and spread a leather on the mouth of it, tying over that a single part of a bladder, which was then let down 75 fathoms; but it came up again quite entire.—Whereupon thinking that the leather in great measure had contributed to the resistance the bladder had made, as the same was marked very much by the force of the pressure against the mouth of the bottle; we made a hole in the leather about the size of a large pea, and let the same again down 75 fathoms; but it came up perforated in the vacant place where the leather had the hole in it, and almost full of water. We then bound over another cover of bladder single, and let it down only 30 fathoms, but it came up whole and entire. Whereupon immediately we let it down 50 fathoms, and then it came up broken and full of water.

Then we again fitted the bottle with the said perforated piece of leather and a double bladder, and let it down 50 fathoms; when it again came up entire. Again immediately we let it down 75 fathoms, and then it came up broken and full of water.

Again, being in 39¼° of latitude, and by the ship's account 150 leagues westward of Portugal, I caused a Florence flask to be well stopped with a bladder over the mouth of it, and lowered down 30 fathoms, but it was taken up broken. Whereupon imagining that the roughness of the leads halling so tender a body so violently through the water might be the cause of breaking it, I caused another flask in the like manner to be fitted, and close by it I tied likewise another flask, so as to be borne with the mouth downwards, as were the others, but which was not stopped; and these I caused to be taken up when they had been but 10 fathoms under water, and found them both entire, but the open flask almost full of water. These being emptied, were both let down again, and taken up at 20 fathoms, when the open flask was entire, though full of water, but the other broken to pieces.

Lumbricus Hydropicus;* *or, an Essay to prove that Hydatides often met with in morbid Animal Bodies, are a Species of Worms, or imperfect Animals. By Edward Tyson, M.D. and R. Soc. S.* N° 193, p. 506.

By the opportunity given me of dissecting a gazella, or antelope, brought from Aleppo, I observed several hydatides, or films filled with limpid water, about the size of a pigeon's egg, and of an oval form, which were fastened to the omentum, and some in the pelvis, between the bladder of urine and the rectum. On this occasion I was very desirous of satisfying myself as much as I could of some suspicions I had of the like watery bags, or hydatides, I had met with in other animals; for from what I could then observe, I was apt to believe them to be a particular sort of insect bred in animal bodies, but so different from any observed out of them, that unless upon fuller and further considerations I durst not trust my own thoughts about them.

My present reasons for suspecting them to be insects, or at least the embryos or eggs of them, are these. First, because I observed them included in an outward membrane, like a matrix, so loosely, that by opening it with my fingers or a knife, the inward bladder, containing the lympha or serum, seemed no

* This animal is a species of tænia or tape-worm, and is one of those which have the body terminating in a large dilated cavity or bladder. It is the tænia globosa var. δ. of the Gmelinian edition of the Systema Naturæ of Linnæus, and the hydra hydatula of the 12th edition of that work by Linnæus himself. The most accurate description, accompanied by figures, may be found in the work of Goeze.

where to have any connection or hold to it, but very readily dropped out, still holding its liquor without spilling any of it. On repeated experiments of it in this and other animals, it gave me opportunity to think how it was possible this humour could come into the inward bladder, without any cohesion to the outward, which involved it.

Secondly, observing them further by my naked eye, I perceived that to this inward bladder there was a neck, or white body, more opake than the rest of the bladder, and protuberant from it, but so as I could observe an orifice at the extremity of it, which then to me seemed to be occasioned by the retraction of some part of it inwards. By this I fancied it might, as by a mouth, suck the serum from the outward membrane, and so supply its bladder or stomach.

Thirdly, in this thought I was further encouraged by the assistance of my very good friend Mr. Richard Waller, whose presence I desired at the dissection, and by imparting to him my thoughts that these hydatides might be a peculiar sort of insects, bred in animal bodies, we were resolved to satisfy ourselves of the notion, and having observed what I have before mentioned, we found that this neck, on approaching it to the candle, did really move, and that it did protrude and then shorten itself. But for the more satisfaction, I shall give the figures made by his ingenious hand; both natural as they appeared to the naked eye, and what they were discovered to be by the microscope.

Fig. 1, pl. 11, represents one of these watery bladders, in its natural size, inclosed in its outward membrane or chorion; its shape was almost round, only flatted as a drop of quicksilver by lying on a solid. A shows the neck, seen through the membrane; which, in fig. 2, is more plainly represented, the outward membrane being taken off, but still as appearing to the naked eye; where we observed an open orifice at the extremity of it, and that it is made up of circular rings or incisures; which, in fig. 3, being viewed by a microscope do more evidently discover themselves; this part is granulated with an abundance of fine eminences all over. The orifice at the end seems here to be occasioned by drawing itself inwards, and upon trial we found it so; for, in fig. 4, is represented the neck of this worm, drawn out its whole length, and magnified; where may be observed the lessening of the rings, and its tending to a point at the end. And having opened it, within we found two small strings, A A, proceeding from the neck, and floating in the liquor.

What these two strings may be, is hard positively to assert; leaving others to their own conjectures, I shall deliver mine: that this worm, by protruding its neck, sucks from the outward membrane which involves it, and is furnished with blood-vessels, the moisture or nourishment which is conveyed by these two strings or pipes, into the stomach or bladder, and from whence, as there is occasion, it may be supplied for the nourishment of the whole body of the worm again; for I am apt to believe this bladder is only the stomach of the worm, which will appear less unreasonable if we consider in some insects how prodigiously large the stomach is, in proportion to the other parts of the body; in a leech you may observe not a single, but above 20 stomachs, emptying out of one into another, and running the whole length of the body. And what Malpighi* observes of the silk worm, that it would devour in one day as much as was the weight of its whole body, a leech will do far more at a meal.

Perhaps some may be more inclined to think that the whole is but an egg or embryo of another insect forming, and that this bladder is as it were the amnion, and the outward coat that includes it, the chorion; and could they perfect any such discovery, I should think so too. But formerly in dissecting a rotten sheep, wherein I found many of these hydatides, and opening several of them, I could observe only the same structure exactly in all; and doubtless had it been otherwise, in so many, I could not but have met with some nearer to perfection.

These hydatides therefore I cannot but think are a sort of worms or insects sui generis; and because they contain so much water in them, and are usually to be met with in rotten sheep, which are hydropical, I call them lumbrici hydropici; not that I think all those cysts to be met with in morbid bodies are such; for in some I have not observed this neck and structure of parts, but only a transparent bladder filled with a lympha, and those I take to be of another kind.

Thus, in a patient still living, and enjoying her health better than all her life time before; about 10 years ago I caused her right side to be opened a little below her short ribs, whence issued out abundance of limpid water; but what was most surprising, together with it, a great many hydatides; that first and last, as we guessed, there might come out about 500 of these bladders; most were entire and filled with limpid water; of others that were too large for the orifice, the films were broken, but in none of them could I observe the neck, though I was inquisitive to find it; which makes me think them to be different from our present subject; as are also those I have frequently met with in the ovaria, or testicles, of women who have died hydropical, which I take to be only the eggs contained there, which, by an extravagant flux of humours into them, are often swelled, to that prodigious size, that I have taken sometimes several gallons of liquor out of them. And what is mentioned (Philosophical Transactions, N° 188,) of those bladders of water found in the urine bladder,

* De Bombyce, p. 40.—Orig.

will come into the same number, having observed no neck in any of them. I shall only add, that these lumbrici hydropici I have always found hanging to the membranous parts, rather than included in the body of any of the viscera, as to the omentum, peritonæum, or the outward membranes that cover the diaphragm, stomach, liver, colon, or other intestines. And we may be less surprised at the odd structure in this worm, since what I have observed of the lumbricus latus (Philosophical Trans. N° 146,* and of the teres, N° 147†) is as wonderful, though in a different manner.

On the Visible Conjunctions of the inferior Planets with the Sun. By Mr. Halley, N° 193, p. 511. *Translated from the Latin.*

That Mercury and Venus enter the disk of the sun, and there appear like black spots, is evident, both from the principles of sound astronomy, and undoubted observations; but by what laws, or conditions, or in what period of years these phænomena offer themselves to our view, has not been determined by any of our modern astronomers; on which account I thought it would not be unacceptable to apply seriously to this disquisition, and clear up a subject so perplexed, and so little understood.

It is self-evident that these phases of these planets always happen in their conjunctions with the sun, when retrograde, viz. when the sun is so near their nodes, that the latitude of the planet, in its conjunction with the sun, does not exceed the semidiameter of the latter. Now, that the limits and conditions of these conjunctions may be the more easily investigated, and since the elements of the calculation are entirely different, each planet is to be treated of apart; and therefore to begin with Mercury: It is certain that, according to late and accurate observations, the ascending node of this planet, in this century, viz. March 1691, is found near the 15° of Taurus, or rather at 15° 44′ from the first star of Aries; and the opposite descending at 6ˢ 15° 44′ from the same star; the inclination of the plane of Mercury's orbit to the ecliptic, according to Kepler, is 6° 54′, which is nearly exact. Now it appears from the most approved hypothesis, that the distance of Mercury from the sun, when in the ascending node, is 31365 parts, of which the mean distance of the sun from the earth is 100000; but when it is in the other node, that distance, measured in the same parts, is 45308: the sun, when opposite to the ascending node, is distant from the earth, in conjunction therewith, 98955 parts; but in the other node, the same distance becomes 101007; and therefore Mercury, in conjunction with the sun at the ascending node, is distant from the earth 67591 parts, but at the descending node 55699; which widely differing from

* Vol. II. p. 591, of this abridgment. † Ibid. p. 605.

each other, those conjunctions, that happen in different nodes, are also to be considered separately.

Let Mercury, when retrograde, be in a central conjunction with the sun in the ascending node, in the month of October, and from the above hypothesis we shall have as follows:

Longitude of the sun from the first star of Aries	6ˢ	15°	44′	0″
Longitude of Mercury seen from the sun	0	15	44	0
Distance of Mercury from the sun 31365				
Ditto from the earth 67591				
Angle of the inclination of Mercury's orbit	0	6	54	0
Mercury's motion seen from the sun, in 6 hours	0	1	30	58
Sun's motion in the same, 6 hours	0	0	15	5
Hence Mercury's motion from the sun, in 6 hours	0	1	15	53
The same as seen from the earth, in 6 hours	0	0	35	12
Angle of Mercury with the ecliptic seen in the sun	0	8	15	0
Hence Mercury's motion in his visible orbit, in 6 hours	0	0	35	40
Therefore Mercury's motion in a sidereal year, besides 4 revolutions, is	1	24	45	8
And therefore in 13 years	11	21	46	44
Therefore less than 54 entire revolutions, by	0	8	13	16
Which space Mercury runs over in	2ᵈ	0ʰ	11ᵐ	0ˢ
In which the sun's place is advanced, and Mercury, in the node, is as much distant from the conjunction of the earth	0ˢ	2°	1′	0″
But that arch seen from the earth is	0	0	56	10
Hence from the given angle of the visible way, 8° 15′, arises the base, or distance from the visible conjunction	0	0	45	34
Which arch is run over by Mercury in	0ᵈ	9ʰ	21ᵐ	0ˢ
But 13 sidereal years exceed so many Julian ones, with 3 intercalations, by	0	8	0	0
Therefore Mercury returns to the sun after 13 Julian years, and besides	2	17	34	0
Or with 4 intercalations, if the preceding year be the third after Bissextile	1	17	34	0
But from the arch 56′ 10″, and the given angle, the perpendicular, or Mercury's nearest distance from the sun, is	0ˢ	0°	8′	3″
Therefore Mercury, after seen in the sun, in 13 years advances more northerly by	0	0	8	3
Likewise in 46 sidereal years Mercury moves	11	28	36	8
Therefore wanting of 191 entire revolutions	0	1	23	52

That is in time	$0^d\ 8^h\ 12^m\ 0^s$
By which the sun is advanced	$0^s\ 0^\circ\ 20'\ 41''$
This arch seen from the earth gives	0 0 9 36
The base answering thereto	0 0 9 30
The time in which Mercury runs over the base, is	$0^d\ 1^h\ 36^m\ 0^s$
But 46 sidereal years exceed as many Julian with 11 intercalations, by	0 19 3 0
Therefore Mercury returns to the sun after 46 Julian years and	1 4 51 0
Or, with 12 intercalations, when the preceding year is either the 2d or 3d after bissextile	0 4 51 0
But the perpendicular, by which Mercury advances to the north, is	$0^s\ 0^\circ\ 1'\ 22''$
But the most accurate period of Mercury about the sun, is in 263 sidereal years and	$0^d\ 1^h\ 11^m\ 30^s$
And these sidereal years exceed as many Julian, with 66 intercalations, by	0 10 20 0
Hence after 263 Julian years Mercury revolves to the sun, but later by	0 11 31 30
But if the preceding year be bissextile, add	1 11 31 30
After this interval it advances more northerly by	0 0 0 10

The other wider periods are easily discovered from what has been just now found, and they are either of 6 or 7 years. That of 7 years depresses Mercury 22′ 47″ to the south; and 7 whole days, less by 9m. sooner, if there have been two intercalations; but with one intercalation, viz. when the former year is bissextile, 6 days are to be subducted, adding only 9m. as before. It is more rarely that after 6 years this planet is seen again in the sun's disk; after that period it passes 30′ 50″ more northerly; and later by 8d. 17h. 25m. if the preceding year be the 2d or 3d after bissextile, otherwise 9d. 17h. 25m. are to be added.

In like manner, if the conjunction happen at the descending node in April.

The longitude of the sun from the first star of Aries	$0^s\ 15^\circ\ 44'\ 0''$
Longitude of Mercury seen from the sun	6 15 44 0
Distance of the planet from the sun 45308.	
Its distance from the earth........ 55699.	
Mercury's motion, seen from the sun, in 6 hours	0 0 43 21
The sun's motion in the same time	0 0 14 29
The motion of Mercury from the sun	0 0 28 52
Hence the angle of Mercury's visible way with the ecliptic, seen in the sun, is	0 10 18 0
And the visible motion from the earth in 6 hours	0 0 23 52

Hence, by following the method of the preceding calculus, it is evinced that Mercury after 13 years 3 d. 7 h. 37 m. revolves to its conjunction with the sun; but if the preceding year be the 3d after bissextile, in that case 2 d. 7 h. 37 m. are only to be added; and then Mercury will be found to advance 16′ 55″ more southerly. After 46 years, with 12 intercalations, add 7 h. 14 m. and Mercury will be in conjunction with the sun 2′ 53″ more southerly; but if the former year be bissextile, or the first after it, 1 d. 7 h. 14 m. are to be added, in order to have the conjunction accurately. In like manner, after 263 years, in which Mercury declines 22″ to the south, either 1 d. 11 h. 49 m. or 11 h. 49 m. are to be added, according to the rule prescribed in the former case.

But in 6 or 7 years, because of the nearness of the earth and that planet, and therefore on account of the enlarged arches at this node, it does not return to the sun, so as to be seen within its disk. After 33 years it passes over the sun 14′ 2″ more northerly; and the moment of the conjunction is had by subtracting from the time of the former, 3 d. 0 h. 23 m. if it be the 3d year after bissextile; otherwise subtract 2 d. 0 h. 23 m. only.

Having found these, it will be easy to continue the calculus for all the conjunctions of Mercury with the sun, and that with the greatest certainty; by addition only, the moments of the conjunctions, and the distances of the planet from the centre of the sun are obtained, whence also by the help of a table, the durations of these eclipses are discovered; so that nothing seems to be wanting in this affair.

As to the epocha, these are with more safety had from the industry of observers than by the subtilest calculations; therefore I chose for the first case, that remarkable transit of Mercury which I fully observed at St. Helena, Oct. 28, 1677, O. S. and whose middle I determined there at 4 m. p. m. but at London 28 m. p. m. The way in which the planet seemed to advance was 4′ 40″ more northerly than the sun's centre; in the other case, viz. when Mercury was in conjunction with the sun, April 23, 1661, O. S. he appeared at his least distance from the sun's centre at Dantzic 6 h. 8 m. p. m. and at London 4 h. 52 m. being then in the middle of his transit, and at the same time was distant from the centre 4′ 27″ to the north. Hence, according to the above rules, it was easy to exhibit in order all the visible conjunctions of Mercury with the sun.

A series of the times wherein Mercury, being in conjunction with the sun, is seen within his disk, calculated for the present and ensuing century, with the distances of the planet from the sun's centre.

In the month of April.

Years.	Times of Conjunct.				Dist. of the ☉'s Centre.		
1615	22d	21h	38m	**	7′	20″	N.
1628	25	5	15	*	9	35	S.
1661	23	4	52	*	4	27	N.
1674	26	12	29		12	28	S.
1707	24	12	6		1	34	N.
1720	26	19	43	*	15	21	S.
1740	21	11	43		15	36	N.
1753	24	19	20	*	1	19	S.
1786	22	18	57	*	12	43	N.
1799	26	2	34	**	4	12	S.

In the month of October.

Years.	Times of Conjunction.				Dist. from the ☉'s Centre.		
1605	22d	8h	29m		12′	48″	S.
1618	25	2	4	*	4	45	S.
1631	27	19	37	*	3	18	N.
1944	30	13	11		11	21	N.
1651	23	13	20		11	26	S.
1664	25	6	54	*	3	23	S.
1677	28	0	28	**	4	40	N.
1690	30	18	2	*	12	43	N.
1697	23	18	11	*	10	4	S.
1710	26	11	45		2	1	S.
1723	29	5	19	*	6	2	N.
1730	22	5	28		16	45	S.
1736	30	22	53	**	13	5	N.
1743	24	23	2	**	8	42	S.
1756	26	16	36		0	39	S.
1769	29	10	10		7	24	N.
1776	22	10	19		15	23	S.
1782	1 Nov.	3	44	*	15	27	N.
1789	25 Oct.	3	53	*	7	20	S.

The transits marked with the sign * are partly visible at London, but those with the sign * * are entirely to be seen there.

Note, that the sun's diameter at the ascending node of Mercury, in the month of October, is 32′ 34″; and therefore that the greatest duration of the central transit is 5h. 29m.; but in April the sun's diameter becomes 31′ 54″; whence, on account of the slower motion of the planet, the greatest duration is 8h. 1m. But if Mercury should fall obliquely on the disk, these durations become shorter, in proportion to the distance from the sun's centre. But, that the calculus may be the more perfect, the following tables are subjoined, wherein are exhibited the half durations of these eclipses for each minute of distance seen from the sun's centre; which added to, and subtracted from, the moment of conjunction found in the former table, exhibit the beginning and end of the whole phenomenon.

October.

Minut. Dist.	Semi-durat.	
0′	2^h	44^m
1	2	44
2	2	43
3	2	41½
4	2	39½
5	2	36½
6	2	33½
7	2	28½
8	2	23
9	2	17
10	2	10
11	2	1
12	1	51
13	1	39
14	1	24
15	1	4
15½	0	50
16	0	30

April.

Minut. Dist.	Semi-durat.	
0′	4^h	$0\frac{1}{2}^m$
1	4	0
2	3	58½
3	3	56
4	3	53
5	3	48½
6	3	43
7	3	36
8	3	28
9	3	18½
10	3	7
11	2	44
12	2	38
13	2	19
14	1	55
15	1	21½
15½	0	56

These numbers fitly represent all the observations hitherto made, nor need we doubt of the future; seeing, of all the planets, Mercury is the nearest to the sun, so that it cannot in the least be intercepted by the intervention of the centres of the other planets, nor sensibly disturbed by those deviations, which arise from their systems, and to which the superior planets, particularly Saturn, are obnoxious. I have designedly omitted the parallaxes, as very inconsiderable, and which, being different in different places, could not be applied in a more general calculation; and because their quantity has not hitherto been determined, but from such observations they may rather be more certainly derived. I had also as little regard to Mercury's diameter, because, being extremely small, he seems to adhere for a very few minutes to the limb; for by an accurate observation, Oct. 28, 1677, I found that scarcely two minutes were elapsed, when he entirely quitted the sun; whence I concluded its diameter to be 11″, and according to the ratio of the distances from the earth at the other node, 13½″ nearly; therefore 3½ minutes are spent, while the whole planet directly pervades the sun's limb; but passing obliquely, he continues on it a little longer, in the ratio of the increase of the secants of the angles of incidence. There is also as little occasion to estimate the equations of time, because for many days in each month they continue constant, and as it were invariable.

Of the visible Conjunction of Venus with the Sun.—This sight, which is by far the noblest that astronomy affords, like the secular games, is denied to mortals for a whole century, by the strict laws of motion. It will be afterwards shown, that by this observation alone, the distance of the sun, from the earth, might be determined with the greatest certainty, which, on account of the parallax otherwise quite insensible, has not hitherto been precisely defined. As to the periods, they cannot be so accurately determined as those of Mercury, seeing Venus has only once since the creation been observed within the sun's disk, and that by our countryman Horrox: after having corrected the motions, as much as the ruder observations of the ancients would permit, the sum of the calculation is as follows: The

Longitude of Venus's ascending node from the first star of Aries	1ˢ	15°	16′	0″
Therefore the sun is in conjunction with her in the opposite point, that is, in these centuries, about the end of November	7	15	16	0
Distance of Venus from the sun 71997 parts.				
Ditto, from the earth 26438.				
Inclination of Venus's orbit to the ecliptic	0	3	23	0
Venus's motion in 8 sidereal years, over 13 revolutions	0	1	30	28¼
Ditto, in 235 sidereal years, over 381 revolutions...........	11	29	17	39
Ditto, in 243 sidereal years, over 395 revolutions	0	0	48	8

From these elements, making a calculus according to the method explained in Mercury, the intervals of the times and distances arise as follows: after 8 years, Venus revolves to the sun; viz. after subtracting from the moment of the former transit 2d. 10h. 52½m. and the planet proceeds in a path 24′ 41″ more southerly than the preceding: after 235 years, having added 2d. 10h. 9m. Venus may again enter the sun, but 11′ 33″ more northerly; but if the preceding year be bissextile, 3d. 10h. 9m. are to be added: after 243 years, Venus may likewise pass over the sun, subtracting only 43m. from the time of the former; but she passes 13′ 8″ more southerly; but if the preceding year be bissextile, add 23h. 17m. And in all these appulses of Venus to the sun in the month of November, the angle of the visible way of Venus with the ecliptic is 9° 5′, and her horary motion in the sun 4′ 7″; and since the semidiameter of the sun is 16′ 21″, the greatest duration of the transit of the centre of Venus is 7h. 56m.

Again, let the sun and Venus be in conjunction at the descending node in the month of May; then according to the same numbers, the same intervals are computed: after 8 years, 2d. 6h. 55m. are to be subtracted, and Venus will pass 19′ 58″ more northerly: after 235 years, add 2d. 8h. 18m. or, if the former year have been bissextile, 3d. 8h. 18m. and Venus will pass 9′ 21″ more southerly. Lastly, after 243 years, add 1 h. 23 m. or if the former year have been bissextile, 1 d. 1 h. 23 m. and Venus is again found in conjunction with the sun, but in a path 10′ 37″ more northerly.

In all the transits at this node, the angle of the visible way of Venus with the ecliptic, is 8° 28′; and her horary motion 4′; and the semidiameter of the sun subtending 15′ 51″, the greatest duration of the transist of the centre is 7 h. 56 m. precisely the same as at the other node. As to the epochs; it is concluded from the ingress of Venus seen in the setting sun, and which was only observed by Horrox, that Venus was in conjunction with the sun at London 1639, November, 24 d. 6 h. 37 m. but that she passed 8′ 30″ towards the south. But in the month of May, Venus has not hitherto been seen by any mortal within the sun; but from my calculations it appears, that Venus for the next time will enter the sun 1761, May, 25 d. 17 h. 55 m. viz. in the middle of the eclipse, and then be distant from his centre towards the south 4′ 15″. Hence, and from the premised revolutions, all the phænomena of this kind are easily computed for a thousand years, as appears from the following table:

November.

Year.	Time of Conj.			Dist. from ☉'s Centre.		
918	20ᵈ	21ʰ	53ᵐ	6′	12″	N.
1161	20	21	10	6	55	S.
1396	27	7	20	4	38	N.
1631	26	17	29	16	11	N.
1639	24	6	37	8	30	S.
1874	26	16	46	3	3	N.
109	29	2	56	14	36	N.
2117	26	16	3	10	5	S.

May.

Year.	Time of Conj.			Dist. from ☉'s Centre.		
1048	24ᵈ	13ʰ	45ᵐ	3′	50″	N.
1283	23	8	14	5	31	S.
1291	25	15	9	14	27	N.
1518	25	16	32	14	52	S.
1526	23	9	37	5	6	N.
1761	25	17	55	4	15	S.
1769	23	11	00	15	43	N.
1996	28	2	13	13	36	S.
2004	25	19	18	6	22	N.

As to the duration of these eclipses of Venus, they may, with respect to the centre, be calculated in the same manner as those of Mercury were; but as the diameter of Venus is pretty large, and the parallaxes may cause a considerable difference as to the time, there must necessarily be a peculiar calculation for each place. The diameter of Venus is so great, that while she adheres to the sun's limb, almost 20 minutes of time are spent, viz. when she directly enters the sun; but falling obliquely into him, she makes a longer stay on his limb. Her diameter, according to the observation of Horrox, is 1′ 18″, when she is in conjunction with the sun at the ascending node; and 1′ 12″ at the descending node.

The principal use of these conjunctions is accurately to determine the distance of the sun from the earth, or his parallax, which astronomers have by several methods attempted in vain, while the smallness of the angles sought do easily elude the nicest instruments; but in observing the ingress of Venus into, and egress from, the sun, the space of time between the moments of the internal contacts may be obtained to a second of time, that is, to $\frac{1}{15}$ of a second, or 4‴ of the observed arch, by means of an ordinary telescope and clock that goes accurately for 6 or 8 hours.

Some Observations on the Spawn of Frogs, and the Production of Tadpoles. By Richard Waller, Esq. Reg. S. Secret. N° 193, p. 523.

About the 12th of March, 1689, I took some frog-spawn out of a ditch, which I suppose might have been spawned about 14 days, and kept it in a gallipot of water, which I shifted every day or two, and kept them in a window where the sun shone some part of the day.

At the first they appeared as fig. 5, pl. 11, being a round black globule, encompassed with a clear liquor, and a membrane encompassing that liquor, and that again surrounded with a larger sphere of a more mucous liquor. The 2d and 3d days they appeared as fig. 6; the fourth day as fig. 7. And about the 6th day several of them were loosed from their eggs; and on the 7th and 8th more of them; when they appeared of the shape of fig. 8, which in fig. 9, is represented larger than the life, that the position they lay in may be seen the better. On the 7th and 8th days, upon pricking them with the point of a needle, they would contract themselves; and some of them on the 8th day would of themselves bend their bodies, but not move out of their place, which was the bottom of the pot they were kept in.

When they first got through their egg, which I suppose they did by eating their way, they hung fast to the outside of it, by that part which I afterwards found to be their mouth, and when loosed from their hold, they sunk to the bottom of the water, and could not rise again. On the 9th day they were not visibly increased in bulk, only they moved themselves more freely at the bottom of the vessel. At about 14 days end they appeared as fig. 10, at which time they swam about in the water by moving their tails, as fig. 11, and some rudiments of their fore legs were visible, which looked forked and like a sprig of a plant. At three weeks end their mouths were to be seen, which they opened and shut, and emitted fæces from the other end. At a month's end the eyes were to be discerned, at which time they would swim near the top of the water, and opening their mouths let out a small bubble of air, and I suppose take in fresh.

The liquor which was contained in the innermost membrane, was more transparent than the other, which was a mucous liquor, and like the white of an egg; the whole was a little heavier specifically than water; for a single egg sunk when loosed from the rest, but when they were fastened a great many together, they swam, every three eggs leaving a little space, which, being filled with air, made them specifically lighter than common water.

Account of a Ruminating Man. By Frederic Slare, M. D. and F. R. S.
N° 193, p. 525.

This ruminating man lived at Bristol. He would begin to chew his meat over again within a quarter of an hour after his meals, if he drank upon them; if not, it was some time longer. This chewing, after a full meal, lasted about an hour and a half. If he went to bed presently after meals, he could not sleep, till the usual time of chewing was over. The victuals, on their return, tasted somewhat more pleasant than at first. Bread, meat, cheese, and drink seemed to return much of the same colour as if they were mixed together in a mortar. Liquids, as broth and spoon-meat, returned to his mouth all one as dry and solid food. The victuals seemed to the patient to lie heavy in the lower part of his throat, untill they had undergone the second chewing; afterwards they would pass clean away; and he always observed, that if he eat variety of things, what he swallowed first would again come up first to be chewed. If this faculty intermitted at any time, it portended sickness, and he was never well, till it returned again. The patient was always thus affected, since he could remember; his father sometimes chewed his cud, but in small quantities, and nothing like his son.

History is very sparing as to many instances of this kind: Fabricius ab Aqua pendente is (I think) the first that mentions one, being a nobleman of Padua that ruminated in his days, whom he had the luck to outlive, and the leave to dissect; and what is very strange, he found only one large but very rugous ventricle. He also notes a monk of that place to have had the same faculty. Sennertus also takes notice of one; and so does Salmuth: as also Velshius, who names one Damy, a Welshman, that lived in London, but of these they give no particulars. Ludovicus, a Franckfort physician, who lately lived and practised there, describes a person that to him seemed to ruminate, but this sort of rumination seemed rather a disease, for this man did it with aversion, he rather disgorged than ruminated. Of this kind I have known several in London, who throw up an ill-tasted and bitter mass, half an hour or an hour after feeding, and that to their great disgust: but in true rumination it returns pleasant, and they chew it the second time with delight. Peyerus, who has written at large, and very ingeniously, about rumination, found two persons in his country that were alive when he wrote that book, and had been noticed to ruminate.

A Letter from Mr. John Ray, giving an Account of the Phytographia of Leonard Plukenet, M. D. Lond. fol.* 1691. N° 193, p. 528.

The Method the Indians in Virginia and Carolina use to dress Buck and Doe Skins. Communicated by the Hon. Sir Robert Southwell, Knt. President, R.S. N° 194, p. 532.

The pelt being taken off, it is first stretched out by lines on a kind of rack for drying them; and the brains of the deer are taken out, and laid on moss, or dried grass, and then dried in the sun, or by a fire, to preserve them. When the hunting time is over, the women dress the skins; first, by putting them in a pond, or hole of water, to soak them well. Then with an old knife, fixed in a cleft-stick, they scrape off the hair while they are wet. The skins being thus prepared, they are put into a kettle or earthen pot, and a certain proportion of the dried brains into a kettle over a fire, till they are more than blood-warm; which will make them lather and scour perfectly clean; which done, with small sticks they wring and twist each skin, as long as they find any wet drop from them, letting them remain so twisted for some hours; and then they untwist each skin, and put them into a sort of rack, like a clothier's rack, consisting of two small poles set upright, and two more put athwart; then they stretch them out every way by lines; and as the skin dries, with a dull hatchet, or a stick flatted, and brought to a round edge, or a stone fitted by nature for that purpose, rub them all over, to force all the water and grease out of them, till they become perfectly dry: which is all they do.

And one woman will dress 8 or 10 skins in a day; that is, begin and end them. I intimate this because the men never do it.

* Leonard Plukenet, an eminent and zealous English botanist, is said to have been born about the year 1642. It does not appear that he attained to any celebrity in the profession of physic, being altogether absorbed in the study of plants, and devoted his whole time to the composition of his Phytographia and other works. He spared no pains to procure specimens, and had correspondents in almost all countries. He was one of those botanists to whom Ray was indebted for assistance, in the arrangement of the second volume of his history of plants. With Sloane and Petiver he seems to have been at variance, censuring their writings with much asperity. He was himself at the charge of all his own engravings, and published the whole work at his own expence, (except a small subscription of about 55 guineas towards the conclusion). In the latter part of his life he is said to have been assisted by the Queen, and had the superintendance of the garden at Hampton-Court, with the title of royal professor of botany. The time of his death is not certainly known, but it was probably about the year 1705. His works are, 1. Phytographia. 2. Almagestum Botanicum. 3. Almagesti Botanici Mantissa. 4. Amaltheum Botanicum. The works of Plukenet contain upwards of 2740 figures, of unequal merit, and too small for the more accurate purposes of botanical science. His Herbarium is preserved in the British Museum.

Observations on the Weight of a Land-Tortoise at the Time of its retiring under Ground in Autumn, compared with its Weight on its Re-appearance in Spring; repeated for a Series of Years. By George Ent, M. D. Communicated by Robert Pitt, M. D. F. R. S. Translated from the Latin. N° 194, p. 533.

Oct. 7, 1652.—The tortoise being weighed before it withdrew to its winter retreat, its weight was found to be exactly 4 lb. 3 oz. 7 drs.

Oct. 8, 1652.—Being dug up the day after it had gone under ground, it weighed 4 lb. 6 oz. 1 dr.

March 16, 1653.—(Being the day on which it came out of its hole) it weighed 4 lb. 4 oz.

Oct. 4, 1653.—After it had fasted for some days and had gone under ground, it was found, on being dug up, to weigh 4 lb. 5 oz.

March 18, 1654.—On which day it again came out of its retreat, it weighed 4 lb. 4 oz. 2 drs.

Oct. 6, 1654.—When it was about going into winter-quarters again, it weighed 4 lb. 9 oz. 3 drs.

On the last day of Feb. 1655.—On again coming out of its hole, it weighed 4 lb. 7 oz. 6 drs. It therefore lost of its former weight 1 oz. 5 drs.

Oct. 2, 1655.—When it was on the point of retiring again, it weighed 4 lb. 9 oz.; but it had previously left off taking food, for some time.

March 25, 1656.—On its re-appearance it weighed 4 lb. 7 oz. 2 drs.

Sept. 30, 1656.—When it was about to bury itself again, it weighed 4 lb. 12 oz. 4 drs.

March 5, 1657.—On again coming above ground, it weighed 4 lb. 11 oz. $2\frac{1}{4}$ drs.

Of the Thickness of Gold on Gilt-Wire, and the exceeding Minuteness of the Atoms or constituent Particles of Gold. By E. Halley. N° 194, p. 540.

What are the constituent parts of matter, and how there happens to be so great a diversity in the weight of bodies, to all appearance equally solid and dense, such as are gold and glass, whose specific gravities are nearly as 7 to 1, seems a very hard question to those who rightly consider it: for, from undoubted experiment, gravity is in all bodies proportionable to the quantity or matter in each, and there is no such thing as a propension of some more, others less, towards the earth's centre; since, the resistance of the air being removed, all bodies descend, be they ever so loose or compact in texture, with equal velocity. It follows therefore, that there is 7 times as much matter in gold as in a piece of glass of the same magnitude; and consequently, that at least 6 parts of 7 in the bulk of glass, must be pore or vacuity: this, some favourers of the atomical philosophy have endeavoured to solve, by supposing the primary or constituent atoms of gold to be much larger than those of other bodies, and consequently the pores fewer; whereas in other bodies, the great multitude of the interspersed vacuities diminishes their weights.

Being desirous to examine this notion of the magnitude of atoms of gold, I bethought myself of the extreme ductility of that metal, which is seen in the beating of it into leaf, and above all in the drawing fine gilt-wire; by means of which I believed I might most exactly obtain the true thickness of the coat of gold, that appears, even with the microscope, so well to represent gold itself, that not the least point of silver appears through it. In order to this, I informed myself among the wire-drawers, what gold they used to their silver, and they told me, that the very best double gilt-wire was made out of cylindric ingots 4 inches in circumference, and 28 inches long, which weigh 16 pounds troy; on these they bestow 4 ounces of gold, that is, to every 48 ounces of silver, one of gold; and that 2 yards of the superfine wire weighs a grain. Hence at first sight it appeared, that the length of 98 yards is in weight 49 grains; and that a single grain of gold covers the said 98 yards; and that the 10000th part of a grain is above $\frac{1}{3}$ of an inch long; which yet may be actually divided into 10, and so the 100000th part of a grain of gold be visible without a microscope. But being desirous to compute the thickness of the skin of gold: by means of the specific gravities of the metals, viz. silver $10\frac{1}{3}$, and gold $18\frac{2}{3}$, I found the diameter of such wire the $\frac{1}{386}$ part of an inch, and its circumference the $\frac{1}{123}$ part; but the gold in thickness not to exceed the $\frac{1}{134500}$ part of an inch; whence it may be concluded, that the cube of the hundredth part of an inch would contain above 2433000000, (or the cube of 1345) of such atoms. And it is very surprizing, that gold, being stretched out to so great a degree as is here demonstrated, should yet show itself of so even and united a texture, as not to suffer the white colour of the silver under it to appear through any the least pores; which argues that even in this exceeding thinness, many of those atoms may still lie one over the other.

Some Observations on a diseased Spleen. By Nehemiah Grew, M.D. F.R.S. An abridged Translation from the Latin. N° 194, p. 543.

The daughter of Mr. Tho. Sedgwick, a London merchant, put herself, at the age of 14, under the care of a person who taught embroidery; for which she showed so great a turn, that she spent almost the whole of her time for

2 years in that employment alone. Her natural melancholic temperament was hereby much increased; she became pale, with loss of appetite, mensium defectus, and cough. After 3 years, she was affected with a dull pain (continuing till the day of her death) in the left hypochondrium. When she was entering on her 20th year, she died of a fever.

On opening the body all the viscera appeared to be in a healthy condition, except the spleen; which was swelled and enlarged to an extraordinary degree; being above 2 fingers thick, 4 broad, and about 10 long; so that this viscus, which when in a healthy state seldom weighs, in the human subject, more than 5 oz. weighed in this instance above 25 oz. Moreover, in the generality of cases in which the spleen is diseased, it is found full of dark-coloured scirrhous tumors; whereas, in the present instance, it was in a putredinous state, emitting an offensive smell, and so tender, as to fall to pieces on being handled, like clotted blood. Nevertheless it was of a good red colour, both internally and externally, and without any abscess or purulency.

This narrative is followed by some reflections on the influence which so sedentary a mode of life, giving rise to a depraved nutrition, must have had in producing this disease; especially in a female subject at a period of life, quâ menses primò effluere solent. Hence (it is observed) the superfluous blood which should have been eliminated ex utero, was in part diverted to the spleen.

The author concludes with this general remark, that the most injurious effects are produced on the constitutions of young women at the age of 14 and upwards, by the neglect of bodily exercise.

An Account of a Book. Osteologia Nova, or some Observations on the Bones, &c. Communicated to the Royal Society, in several Discourses, read at their Meetings. By Clopton Havers, M. D. and R. S. Soc. Lond.* 1691, 8vo. N° 194, p. 544.

The author offering to give some account of the manner in which the bones are first formed, supposes that they, as all the other parts of the body, are formed in the egg before the female is impregnated, and that the seed of the male does only put those particles, which are the first principles of the spirits and humours, into motion, by which motion they begin a icrculation; and being expanded, they dilate the containing parts, whose dilatation both causes

* Biographers have not recorded any particulars respecting the life of this English anatomist, whose treatise abovementioned abounds in good observations on the bones, marrow, and periosteum; and to whom we are further indebted for a description of those glands which secrete a mucilaginous liquor, that serves to lubricate the cartilaginous extremities of the bones, and thereby to facilitate the play and flexion of the joints.

an increase in the dimensions of the animal, and makes them more capable of the accession of new particles to nourish and augment them.

Coming to describe the nature and structure of the parts, which are the subject of the discourse, he begins with the periosteum, or membrane which invests the bones, which consists of two sorts of fibres, one of which lying next to the bone itself, is derived from the dura mater, the other from the tendons of the muscles. The use of this membrane is to cover the bones; to convey spirits into them for their sense, and to assist in their nutrition, to which end it has fibres inserted into them; to limit their growth; to keep some of them conjoined; to join the bones and their cartilages together; to fasten the heads and tendons of the muscles to the bones; and lastly, for the safety and security of the bones against injuries, as it serves to make them sensible, and so gives the animal a quick apprehension of any mischief that threatens those parts, and directs us in our application of remedies when they are injured.

The bones, though they are at first gelatinous, and afterwards cartilaginous, are when they come to their true and proper nature, solid and hard, consisting of terrestrial and saline particles. These particles, being in their several series united at their extremities, form strings, and these strings being united make distinct plates, which lying one over another make the whole thickness of the bone. In and between these plates he observes two sorts of pores, some which run through every plate, others which are formed between them for the dispensation of the medullary oil to the substance of the bone. The superficies of the bones is unequal, being rendered so by some superficial cavities, and by passages which penetrate into them, the first of which are for enlarging their surface, and strengthening the adhesion of their membrane to them; the other for the ingress of blood vessels into their substance or cavities. The cavities of the bones are in some large, in others small and numerous, whose partitions are formed of plates propagated wholly from those plates which make the sides of the bones, in such as have long cavities; and in those which are spongy, from plates which run up the whole length of the bone in that manner. And in some bones there are fasciculi of strings, which run off from the plates in the sides, and make a sort of cancelli, or net-work in the cavity.

In the bones there are many blood-vessels, which serve for their nourishment; the arteries entering at one end, and the veins coming out in vast numbers, either at the contrary extremity, or in some intermediate parts: and there are both veins and arteries belonging to the marrow.

In the teeth he observes a twofold substance, one of a stony nature, which is the cortical, or exterior part, of so much of the teeth as stands out of the gums; the other truly bone, consisting of laminæ or plates, as the other bones do.

In the second discourse is given an account of the manner of accretion and nutrition in general, and then particularly in the bones. The matter which gives an increase to the animal is originally from the chyle, the particles of which designed for nourishment being elaborated in the mass of blood, and so reduced nearer to the nature of a succus nutritius, and disposed for a separation, are secerned from the sanguineous mass, by glands seated on the sides of the arteries all over the body. And here the author takes occasion to speak of glandular secretion.

He then goes on with the affair of accretion, and the account is in short this, that the nutritious particles, being separated by the glandules placed in the sides of the arteries, are carried into those small nervous pipes, or interstices of the fibres where the spirits move, so that they fall in the way of the spirits motion.

Nutrition he makes to be no reparation of the loss of the substance of the solid parts ordinarily, but only a continual succession and supply of spirits, and of all those fluid parts, which fill the containing parts, and preserve them distended.

The third discourse concerns the marrow, which has blood vessels, both veins and arteries. The organs by which the medullary oil is separated, are small vesicles or glandules, which are conglomerated into distinct lobules, contained in several membranes or bags, which lie in one common membrane, investing the whole marrow: all which vesicles, bags and membrane are propagated from the exterior coat of the arteries. The passage of the medullary oil from all parts of the marrow to the bone is not by ducts, but by pores formed in the vesicles, by which it passes from one to another, till it arrives at the sides or extreme parts of the bone. The medullary oil, which is supplied to the interstices of the joints, passes into them by passages penetrating through the bone into these cavities, and formed for this end. The use of the medullary oil, is either common to all the bones, or more proper to the joints. It is necessary to preserve the temper of the bones, and prevent them from being brittle. In the articulations, 1. It lubricates the extremities of the bones, and so makes them more easy to be moved. 2. It preserves the ends of the articulated bones from an inordinate incalescence. 3. It prevents the attrition of those parts of the bones, which are rubbed one against another. It is likewise beneficial to the ligaments of the joints in preserving them from dryness and rigidity, and lubricating those parts of them, which slide upon the bone. Those cartilages also which are joined to any of the bones it preserves flexible.

In the 4th discourse is an account of a particular sort of glands, which he calls the mucilaginous glands seated in the joints. These are of two sorts; some are small, and in a manner miliary glands, being glandules placed all upon the same surface of the membrane, which lies over the articulations. The other sort are conglomerate, or many glandules collected, and planted one upon another, so as to make a bulk, and considerable glands. In some of the joints there are several of them, and others but a single gland. They have their blood vessels, as other glands, but their veins have a particular flexure in their course, for retarding the return of the blood from the glands, that the mucilaginous liquor, which is not separated with the greatest expedition, mayh ave time to penetrate the secretory pores of the glandules. The large mucilaginous glands are variously seated, some in a sinus formed in the joint, others stand near or over against the interstice between the articulated bones: but in general they are so placed as to be squeezed gently, and lightly pressed in the inflexion or extension of the joint, so as to separate a quantity of mucilage proportionate to the motion of the part, and the present occasion, and yet without any injury. Here is also some description of the commom membrane of the muscles; how it is every where set thick with the small mucilaginous glands; and about some joints which are often moved, and where the tendons run backward and forward more considerably, it has some larger or conglomerate glands.

The design of all these glands is to separate a mucilaginous kind of liquor, that serves principally to lubricate the joints, to make them so slippery as to be moved with the greatest facility imaginable. It serves likewise to preserve the ends of the articulated bones from attrition, and an immoderate incalescence. But all these things it performs in conjunction with the medullary oil. Of which two ingredients is made a composition admirably fitted for those ends: for the mucilage adds to the lubricity of the oil, and the oil preserves the mucilage from growing too thick and viscous.

Then follows an account of some experiments made with the mucilage; most of which come to this, that all acids coagulate it, as all austeres, and austere acids; but with this difference, that the coagulum or curd made with acids only is tenderer than that which is produced by an austere only, or an austere acid. These experiments being made and described in order to explain the nature and causes of a rheumatism, and the gout, these distempers are next treated of.

The generation of the tophaceous matter, in the nodose gout, is accounted for, from the experiments made with acid-austere liquors mixed with the mucilage; so that where the gout is nodose, the mucilage is first coagulated by some acid austere matter, and the coagulum made by such a matter is not so easily dissolved as when it is made by an acid only. This coagulum therefore being imprisoned, and the more gross and earthy parts being incapable of evaporation, or being otherwise spent, are concentrated and indurated by the evaporation or flowing off of the moist particles, and so produce a hard and chalky substance.

The last discourse is on the cartilages, which approach near to the nature of bones, but differ from them in their formation and in their flexibility; this flexibility is from the figure and order of their parts, which are such that the particles, as they must slide one way or other in the inflexion of a body, may move without interrupting the continuity of the whole, even when they recede from one another, unless they are forced too far. The cartilages have a membrane every way like to the periosteum, and is a continuation of it, where they are joined to any bones. They have also a great number of blood vessels.

The use of the cartilages in general is to give a strength to some parts, which did stand in need of such bodies as are both flexible and rigid. Those of the ribs are designed to make them capable of an elongation, that so they may protrude the sternum one way, and drive back the vertebræ of the breast the other, so making an addition to the capacity of the breast. These gristles serve likewise in the contraction of the breast; for having their natural figure altered in the elevation of the ribs, and the ampliation of the cavity of the thorax, they naturally endeavour to regain their first figure, and to return into their proper position.

Of the several Species of Infinite Quantity, and of the Proportions they bear to one another. By Mr. E. Halley. N° 195, p. 556.

That all magnitudes infinitely great, or such as exceed any assignable quantity, are equal among themselves, though it be vulgarly received for a maxim, is not yet so common as it is erroneous; and the reason of the mistake seems to be, that the mind of man coming to contemplate the extensions of what exceeds the bounds of its capacity, and of which the very idea includes a negation of limits; it comes to pass that we acquiesce generally, and it suffices to say, such a quantity is infinite. But if we come more nearly to examine this notion, we shall find, that there are really, besides infinite length and infinite area, no less than three several sorts of infinite solidity; all which are quantities sui generis, having no more relation or proportion to one another than a line to a plane, or a plane to a solid, or a finite to an infinite; but that among themselves, each of those species of infinites are in given proportions, is what I now intend to make plain, if possible.

And first, infinite length, or a line infinitely long, is to be considered either as beginning at a point, and so infinitely extended one way, or else both ways from the same point; in which case the one, which is a beginning infinity, is the one half of the whole, which is the sum of the beginning and ceasing infinity, or as I may say of infinity à parte ante and a parte post, which is analogous to eternity in time or duration, in which there is always as much to follow, as is past, from any point or moment of time, nor does the addition or subduction of finite length, or space of time, alter the case, either in infinity or eternity, since neither the one nor the other can be any part of the whole.

As to infinite surface or area, any right line, infinitely extended both ways on an infinite plane, divides that infinite plane into equal parts, the one to the right and the other to the left of the said line; but if from any point, in such a plane, two right lines be infinitely extended, so as to make an angle, the infinite area, intercepted between those infinite right lines, is to the whole infinite plane, as the arch of a circle, on the point of concourse of those lines as a centre, intercepted between the said lines, is to the circumference of the circle; or as the degrees of the angle to the 360 degrees of a circle. For example, two right lines meeting at a right angle, include, on an infinite plane, a quarter part of the whole infinite area of such a plane. And if two parallel infinite lines be supposed drawn on such an infinite plane, the area intercepted between them will be likewise infinite; but at the same time it will be infinitely less than that space which is intercepted between two infinite lines that are inclined, though with never so small an angle, since in the one case the given finite distance of the parallel lines diminishes the infinity in one degree of dimension; whereas in a sector there is infinity in both dimensions: and consequently the one quantity is infinitely greater than the other, and there is no proportion between them.

From the same consideration arise the three several species of infinite space or solidity; for a parallelopipid or a cylinder infinitely long, is greater than any finite magnitude, how great soever; and all such solids supposed to be formed on given bases, are in the same proportion to one another as those bases. But if two of these three dimensions are wanting, as in the space intercepted between two parallel planes infinitely extended, and at a finite distance; or with infinite length and breadth, with a finite thickness; all such solids shall be as the given finite distances to one another; but these quantities, though infinitely greater than the other, are yet infinitely less than any of those wherein all the three dimensions are infinite. Such are the spaces intercepted between two inclined planes infinitely extended; the space intercepted by the surface of a cone, or the sides of a pyramid, likewise infinitely continued, &c. of all which notwithstanding the proportions to one another, and to the τὸ πᾶν, or vast abyss of infinite space (wherein is the locus of all things that are, or can be; or the solid of infinite length, breadth, and thickness taken all manner of ways) are easily assignable. For the space between two planes is to the whole, as the angle of those planes is to the 360 degrees of the circle. As for cones and pyramids, they are as the spherical surface, intercepted by them, is to the surface of the sphere, and therefore cones are as the versed sines of half their angles, to the diameter of the circle; these three sorts of infinite quantity are analogous to a line, surface and solid, and after the same manner cannot be compared, or they have no proportion to one another.

Besides these, there are several other species of infinite quantity, arising from the contemplation of curves and their asymptotes, which, by reason of the difficulty of the subject, cannot be made so plain to most readers; but what has been already said, may be sufficient to evince what we undertook to explain.

On the Defects and the Musical Notes of the Trumpet, and Trumpet Marine. By Francis Roberts, Esq. R.S.S. N° 195, p. 559.

The trumpet so famous in all ages for its use in war, the loudness and nobleness of its sound peculiarly suiting it to that purpose, is nevertheless to be reckoned among the imperfect musical instruments. For though it has a large compass, the greater part of the intermediate notes are wanting, and some of them imperfect. The extent of this instrument cannot be strictly determined; it reaches as high as the strength of the breath can force it; but by considering its notes within the ordinary compass of the scale of music, from double C fa-ut to C sol-fa in alt, the nature of the higher notes will plainly appear. These are all set down in the table, fig. 12, pl. 11, where the pricked notes are imperfect, not being exactly in tune, but a little flatter or sharper than the places where they stand, according as f or s is set over them.

Here we may make two inquiries. 1. Whence it is, that the trumpet will perform no other notes in that compass besides those in the table, which are usually called by musicians trumpet notes. 2. What is the reason that the 7th, 11th, 13th, and 14th notes, are out of tune, and the others exactly in tune? In this matter we may receive some light from the trumpet marine, an instrument though as unlike as possible to the trumpet in its frame, one being a wind instrument the other a monochord, yet it has a wonderful agreement with it in its effect. The sound is so like as not to be easily distinguished by the nicest ear, and as it performs the very same notes, so it has the same defects as a trumpet; for if the strings be stopped in any part but such as produces a trumpet note, it yields a harsh and uncouth (not a musical) sound.

Now with regard to the first inquiry, it is a known experiment of two unison strings, that striking one of them, the other moves; which probably proceeds from hence, that the impulses of the air which are made by one string, do more easily set another in motion which lies in a disposition to have its vibration synchronous to them, than a third whose motion would be cross. We may improve this a little, by observing that a string will move not only on the striking of a unison but an 8th or 12th, though after a different manner. If a unison be struck, it makes one entire vibration in the whole string, as in fig. 13, and the motion is most sensible in the middle at m, for there the vibrations take the greatest scope. If an 8th be struck, it makes two vibrations, as in fig. 14, and then the point m is in a manner quiescent, and the most sensible motion at n, n. If a 12th be struck, then it makes three vibrations, as in fig. 15, and the greatest motion at q, m, q, and hardly to be perceived at p, p. All which may be plainly experimented by putting a little piece of paper upon the several parts of the string, to make the motion more conspicuous. So that, in short, this experiment holds, when any note is struck, which is a unison to some aliquot part of the string; as in the former examples, an 8th is unison to half the string, and a 12th to a third part of it.

In this case, the vibrations of the equal parts of a string being synchronous, there is no contrariety in their motion to hinder each other; whereas it is otherwise if a note is unison to s, in the fig. 16, which does not divide the string into equal parts; for then the vibrations of the remainder r not suiting with those of the other parts, immediately make a confusion in the whole.

Now in the trumpet marine, you do not stop close as in other instruments, but touch the string gently with the thumb, by which there is a mutual concurrence of the upper and lower parts of the string to produce the sound; which is sufficiently evident from this, that if any thing touch the string below the stop, the sound will be as effectually spoiled as if it were laid upon that part which is immediately struck with the bow. From hence therefore we may collect, that the trumpet-marine yields no musical sound, but when the stop makes the upper part of the string an aliquot part of the remainder, and consequently of the whole, otherwise, as before remarked, the vibrations of the parts will cross one another, and make a sound suitable to their motion, altogether confused.

Now, that these aliquot parts are the very stops, which produce the trumpet notes, will be plainly shown in treating of the second inquiry, viz. what is the reason that the 7th, 11th, 13th, and 14th notes, are out of tune, and the rest exactly in tune?

All writers on the mathematical part of music agree,

That by shortning a string	a half	the sound is raised	an eighth
	a third part		a fifth
	a fourth		a fourth
	a fifth		a sharp third
	a sixth		a flat third.

From this foundation all the other notes are derived. The flat and sharp sixth are to be the flat and sharp third to the fourth, and the seventh the like to the fifth, the second to be a fifth to the fourth below, &c. By this rule let us examine what notes a monochord fretted in its aliquot parts will produce.

Suppose the monochord, fig. 17, to consist of 720 parts, and its tone double C fa-ut, the first note in the table, then half of it will be 360, and a third part 240, &c. Now fretting, or stopping with the thumb, at 360, must produce C fa-ut; because 360 being half 720, the sound will rise an eighth from double C fa-ut. Again 360 being C fa-ut, 240 must make G sol-re-ut, the third note in the table; because 240 being just a third part less than 360, the sound will rise a fifth from that note. After the same manner, proceeding step by step, it will be evident that,

180	which is less than	240	by just	a 4th	produces	C sol fa-ut the	4th	note in the table.
144		180		a 5th		E la-mi	5th	
120		144		a 6th		G sol-re-ut	6th	
90		180		half		C sol-fa	8th	
80		120		a 3d		D la-sol	9th	
72		90		a 5th		E la-mi	10th	
60		90		a 3d		G sol-re-ut	12th	
48		60		a 5th		B fa-bi-mi	15th	
45		90		half		C sol-fa	16th	

By the same reason.

100	which is less than	120	by just	a 6th	produces	B fa-bi-mi flat
$67\frac{1}{2}$		90		a 4th		F fa-ut
54		$67\frac{1}{2}$		a 5th		A la-mi-re
50		100		half		A fa-bi-mi flat.

And consequently,

$102\frac{6}{7}$ the	7th	note in the table is a little	flatter	than	B fa-bi-mi flat,
$65\frac{5}{11}$	11th		sharper		F fa-ut
$55\frac{5}{13}$	13th		flatter		A la-mi re
$51\frac{3}{7}$	14th		flatter		A fa-bi-mi flat.

Which answers the second inquiry.

Now to apply this to the trumpet, where the notes are produced only by the different force of the breath; it is reasonable to imagine that the strongest blast raises the sound by breaking the air within the tube into the shortest vibrations, but that no musical sound will arise unless they are suited to some aliquot part, and so by reduplication exactly measure out the whole length of the instrument, as in fig. 15; for otherwise a remainder will cause the same inconvenience in this case, as in fig. 16. To which if we add, that a pipe being shortened according to the poroportions above-mentioned, the sound is raised in the same ratio, it renders the case of the trumpet exactly the same with the monochord.

As a corollary to this discourse, we may observe that the distances of the trumpet notes, ascending continually, decreased in proportion of $\frac{1}{1}\,\frac{1}{2}\,\frac{1}{3}\,\frac{1}{4}\,\frac{1}{5}$ in infinitum, for,

The $\left\{\begin{matrix}\text{second}\\ \text{third}\\ \text{fourth}\end{matrix}\right\}$ Note in the Table, differs from the $\left\{\begin{matrix}\text{first}\\ \text{second}\\ \text{third, \&c.}\end{matrix}\right\}$ by $\left\{\begin{matrix}\frac{1}{2}\\ \frac{1}{3}\\ \frac{1}{4}\end{matrix}\right\}$ of the String, &c.

On the Cause of the Change in the Variation of the Magnetic Needle; with an Hypothesis of the Structure of the Internal Parts of the Earth. By Mr. Edmund Halley. N° 195, p. 563.

Having published, in these Transactions, N° 148, a theory of the variation of the magnetic needle, in which, by comparing many observations, I came at length to this general conclusion, viz. That the globe of the earth might be supposed to be one great magnet, having four magnetical poles or points of attraction, two of them near each pole of the equator: and that in those parts of the world, which lie near any of those magnetical poles, the needle is chiefly governed thereby; the nearest pole being always predominant over the more remote. And I there endeavoured to state and limit the present position of those poles on the surface of our globe. Yet I found two difficulties not easy to surmount: the one was, that no magnet, I had ever seen or heard of, had more than two opposite poles; whereas the earth had visibly four, and perhaps more. And secondly, it was plain that these poles were not, at least all of them, fixed in the earth, but shifted from place to place, as appeared by the great, changes in the needle's direction within this last century of years; not only at London, where this great discovery was first made, but almost all over the globe of the earth; whereas it is not known, or observed, that the poles of a loadstone ever shifted their place in the stone, nor, considering the compact hardness of that substance, can it easily be supposed.

These difficulties made me quite despair of ever being able to account for this phenomenon, when in an accidental conversation I stumbled on the following hypothesis. It is sufficiently known and allowed, that the needle's variation changes; and that this change is gradual and universal, will appear by the following examples. At London, in the year 1580, the variation was observed by Mr. Burrows to be 11° 15′ to the east; in 1622, the same was found by Mr. Gunter to be only 6° 0′ to the east; in 1634, Mr. Gellibrand found it 4° 5′ to the east; in 1657, Mr. Bond observed that there was no variation at London; anno 1672, I observed it 2° 30′ to the west; and this present year 1692, I again found it 6° to the west. So that, in 112 years, the direction of the needle has changed no less than 17 degrees.

At Paris, Orontius Finæus, about the year 1550, reckoned it about 8° or 9° east variation; in 1640, it was found 3° to the east; in 1666 there was no variation there; and in 1681, I found it to be 2° 30′ to the west.

At Cape d'Agulhas, the most southerly promontory of Africa, about the year 1600, the needle pointed due north and south without variation, whence the Portuguese gave it that name; in 1622 there was 2° west variation: in 1675 it was 8° to the west; and this year 1691, it was accurately observed to be not less than 11° degrees to the west.

At St. Helena, about the year 1600, the needle declined 8° to the east; in 1623, it was but 6° to the east; in 1677, when I was there, I observed it accurately on shore to be 40′ east; and now this year it was found about 1° to the westward of the north.

At Cape Comorin, in India, in the year 1620, there were 14° 20′ west variation; in 1680, there were 8° 48′; but in 1688, it was no more than 7° 30′; so that here the needle has returned to the east about 7° in 70 years.

In all the other examples the needle has gradually moved towards the west, and the places are too far asunder to be influenced by the removal of any magnetical matter, which may by accident be transplaced within the bowels or on the surface of the earth. From these, and many other observations, it is evident that the direction of the needle is in no place fixed and constant, though in some it changes faster than in others. And where for a long time it has continued as it were unaltered, it is there to be understood that the needle has its greatest deflection, and is become stationary, in order to return, like the sun in the tropics. This at present, viz. 1692, is in the Indian sea, about the island Mauritius, where is the highest west variation, and in a tract tending from thence to the N.N.W. towards the Red Sea and Egypt. And in all places to the westward of this tract, all over Africa and the seas adjoining, the west variation will be found to have increased; and to the eastwards thereof, as in the example of Cape Comorine, to have decreased, viz. all over the East Indies and the islands near it.

In like manner, in that space of east variation, which, beginning near St. Helena, is found all over South America, and which at present is highest about the mouth of Rio de la Plata, it has been observed, that in the eastern parts of it, the variation of the needle gradually decreases; but whether, on the contrary, it increases in those places which lie more westerly than that tract wherein the highest east variation is found; or how it may be in the vast Pacific Sea, we have not experience enough to ascertain; only we may by analogy infer, that both the east and west variations gradually increase and decrease after the same rule.

These phænomena, being well understood and duly considered, sufficiently evince, that the whole magnetical system has one, or perhaps more motions; that the moving force is very great, as extending its effects from pole to pole; and that its motion is not per saltum, but a gradual and regular motion.

Now considering the structure of our terraqueous globe, it cannot be well supposed that a very great part of it can move within it, without notably changing its centre of gravity, and the equilibrium of its parts, which would produce very wonderful effects in changing the axis of diurnal rotation, and occasion strange alteration in the surface of the sea, by inundations and recessions, such as history never yet mentioned. Besides, the solid parts of the earth are not to be supposed permeable by any other than fluid substances, of which we know none that are any ways magnetical. So that the only way to render this motion intelligible and possible, is, to suppose it to turn about the centre of the globe, having its centre of gravity fixed and immoveable in the same common centre of the earth: and there is yet required, that this moving internal substance be loose, and detached from the external parts of the earth, whereon we live; for otherwise, were it affixed to the earth, the whole must necessarily move together.

So then the external parts of the globe may well be considered as the shell, and the internal as a nucleus, or inner globe, included within ours, with a fluid medium between. Which having the same common centre and axis of diurnal rotation, may turn about with our earth each 24 hours; only this outer sphere having its turbinating motion some small matter either swifter or slower than the internal ball. And a very minute difference in length of time, by many repetitions becoming sensible, the internal parts will by degrees recede from the external, and not keeping pace with each other, will appear gradually to move either to the east or west by the difference of their motions.

Now supposing such an internal sphere, having such a motion, we may solve the two great difficulties in my former hypothesis. For if this exterior shell of earth be a magnet, having its poles at a distance from the poles of diurnal rotation; and if the internal nucleus be likewise a magnet, having its poles in two other places distant also from the axis; and these latter, by a gradual and slow motion, change their place in respect of the external, we may then give a reasonable account of the four magnetical poles, as also of the changes of the needle's variations.

The period of this motion being wonderfully great, and there being hardly a century since these variations have been duly observed, it will be very hard to bring this hypothesis to a calculus, especially since, though the variations increase and decrease regularly in the same place, yet in different places, at no great distance, there are found such casual changes of it, as can nowise be accounted for by a regular hypothesis; as depending on the unequal and irregular distribution of the magnetical matter within the substance of the external shell or coat of the earth, which deflects the needle from the position it would acquire from the effect of the general magnetism of the whole. Of this, the variations at London and Paris afford a notable instance; for the needle has been constantly about $1°\frac{1}{2}$ more easterly at Paris than at London; though it be certain that, according to the general effect, the difference ought to be the contrary way. Notwithstanding which, the variations in both places change alike.

Hence, and from some other of the like nature, I conclude, that the two poles of the external globe are fixed in the earth, and that if the needle were wholly governed by them, the variations would be always the same, with some little irregularities on the account just now mentioned; but the internal sphere, having such a gradual translation of its poles, influences the needle, and directs it variously, according to the result of the attractive or directive power of each pole; and consequently there must be a period of the revolution of this internal ball, after which the variations will return again as before. But if it shall in future ages be observed otherwise, we must then conclude, that there are more of these internal spheres, and more magnetical poles than four, which at present we have not a sufficient number of observations to determine, and particularly in that vast Mar del Zur, which occupies so great a part of the whole surface of the earth.

If then two of the poles be fixed, and two moveable, it remains to ascertain, which they are that keep their place; and though I could wish we had the experience of another century of years to found our conclusions upon, yet I think we may safely determine, that our European north pole, supposed to be near the meridian of the Land's End, and about 7° from it, is that which is moveable of the two northern poles, and which has chiefly influenced the variations in these parts of the world: for in Hudson's Bay, which is under the direction of the American pole, the change is not observed to be near so fast as in these parts of Europe, though that pole be much farther removed from the axis.

As to the south poles, from the like observation of the slow decrease of the variation on the coast of Java, and near the meridian of the Asian pole, I take the Asiatic pole, which I place about the meridian of the island of Celebes, to be the fixed one, and consequently the American pole to be moveable. If this be allowed, it is plain that the fixed poles are the poles of this external shell or cortex of the earth, and the other two the poles of a magnetical nucleus, included and moveable within the other. It likewise follows, that this motion is westwards, and by consequence that the aforesaid nucleus has not precisely attained the same degree of velocity with the exterior parts in their diurnal revolution: but so very nearly equals it, that in 365 revolves the difference is scarcely sensible. This I conceive to arise from the impulse by which this diurnal motion was impressed on the earth, being given to the external parts, and from thence in time communicated to the internal; but not so as perfectly to equal the velocity of the first motion impressed on, and still conserved by the superficial parts of the globe.

As to the quantity of this motion it is almost impossible to define it, both from the nature of this kind of observation, which cannot be very accurately performed; as also from the small time these variations have been observed, and their change discovered. It appears by all circumstances, that its period is of many centuries of years, and as far as may be collected from the change of the place, where there was no variation by reason of the equilibrium of the two southern magnetical poles, viz. from Cape d'Agulhas to the meridian of St. Helena, which is about 23° in about 90 years, and of the place where the westerly variation is in its greatest deflection, being about half so much, viz. from the isle of Diego Roiz to the south-west parts of Madagascar. We may with some reason conjecture, that the American pole has moved westwards 46° in that time, and that the whole period of it is performed in 700 years, or thereabouts; so that the nice determination of this, and of several other particulars in the magnetic system, is reserved for remote posterity; all that we can hope to do, is to leave behind us observations that may be confided in, and to propose hypotheses, which after ages may examine, amend, or refute. Only here I must take leave to recommend to all masters of ships and all others, lovers of natural truths, that they use their utmost diligence to make, or procure to be made, observations of these variations in all parts of the world, as well in the north as south latitude, after the laudable custom of our East India commanders, and that they please to communicate them to the Royal Society, in order to leave as complete a history as may be, to those who are hereafter to compare all together, and to complete and perfect this abstruse theory.

And by the way it will not be amiss to correct a received error in the practice of observing the variation, which is, to take it by the amplitude of the rising and setting sun, when his centre appears in the visible horizon; whereas he ought to be observed when his under limb is still above the horizon about $\frac{2}{3}$ of his diameter, or 20 minutes, on account of the refraction, and the height of the eye of the observer above the surface of the sea: or else the amplitudes are to be wrought as the azimuths, reckoning the sun's distance from the zenith 90° 36′: this, though it be of little consequence near the equator, will make a great error in high latitudes, where the sun rises and sets obliquely.

But to return to our hypothesis, in order to explain the change of the variations, we have adventured to make the earth hollow, and to place another globe within it; and I doubt not but this will find opposers enough. I know it will be objected, that there is no instance in nature of the like thing; that if there was such a middle globe it would not keep its place in the centre, but be apt to deviate from it, and might possibly shock against the concave shell, to the ruin or at least endamaging of it; that the water of the sea would perpetually leak through, unless we suppose the cavity full of water; that were it possible, yet it does not appear of what use such an inward sphere can be of, being shut up in eternal darkness, and therefore unfit for the production of animals or plants; with many more objections, according to the fate of all such new propositions.

To these and all other objections that I can foresee, I briefly answer, that the ring environing the globe of Saturn is a notable instance of this kind, as having the same common centre, and moving along with the planet, without sensibly approaching him on one side more than the other. And if this ring were turned on one of its diameters, it would then describe such a concave sphere as I suppose our external one to be. And since the ring in any given position, would in the same manner keep the centre of Saturn in its own, it follows that such a concave sphere may move with another included in it, having the same common centre. Nor can it well be supposed otherwise, considering the nature of gravity; for should these globes be once adjusted to the same common centre, the gravity of the parts of the concave would press equally towards the centre of the inner ball, which equality must necessarily continue till some external force disturb it, which is not easy to imagine in our case. This perhaps I might more intelligibly express, by saying that the inner globe being posited in the centre of the exterior, must necessarily ascend which way soever it may move; that is, it must overcome the force of gravity pressing towards the common centre, by an impulse it must receive from some outward agent; but all outward efforts being sufficiently fenced against by the shell that surrounds it, it follows, that this nucleus being once fixed in the common centre, must always remain there.

As to the leaking of the water through this shell, when once a passage shall be found for it to run through, I must confess it is an objection seemingly of weight; but when we consider how tightly great beds of chalk or clay, and much more stone do hold water, and even caves arched with sand; no man can doubt but the wisdom of the Creator has provided for the macrocosm by many more ways than I can either imagine or express, especially since we see the admirable and innumerable contrivances wherewith each worthless individual is furnished, both to defend itself and propagate its species. What curiosity in the structure, what accuracy in the mixture and composition of the parts, ought we not to expect in the fabric of this globe, designed for the lasting habitation of so many various species of animals, in each of which there want not many instances, that manifest the boundless power and goodness of their divine author; and can we then think it a hard supposition, that the internal parts of this bubble of earth should be replete with such saline and vitriolic particles, as may contribute to petrefaction, and dispose the transuding water to shoot and coagulate into stone, so as continually to fortify, and if need were to consolidate any breach or flaw in the concave surface of the shell.

And this perhaps may not without reason be supposed to be the final cause of the admixture of the magnetical matter in the mass of the terrestrial parts of our globe, viz. To strengthen and maintain the concave arch of this shell; for by what the excellent Mr. Newton has shown in his Principia Philosophiæ, it will follow that according to the general principle of gravity, visible throughout the whole universe, all those particles which by length of time or otherwise, shall moulder away, or become loose on the concave surface of the external sphere, would fall in, and with great force descend on the internal, unless those particles were of another sort of matter, capable, by their stronger tendency to each other, to suspend the force of gravity; but we know no other substances capable of supporting each other by their mutual attraction, besides the magnetical, and these we see miraculously to perform that office, even where the power of gravity has its full effect, much more within the globe where it is weaker. Why then may we not suppose these said arches to be lined throughout with a magnetical matter, or rather to be one great concave magnet, whose two poles are the poles we have before observed to be fixed in the surface of our globe?

Another argument, favouring this hypothesis, is drawn from a proposition of the same Mr. Newton, where he determines the force with which the moon moves the sea in producing the tides, where he says the density of the moon is to that of the earth, as 680 to 387, or as 9 to 5 nearly: therefore the body of the moon is denser than our earth, &c. Now, if the moon be more solid than the earth, as 9 to 5, why may we not reasonably suppose the moon, being a small body and a secondary planet, to be solid earth, water, and stone, and this globe to consist of the same materials, only four ninths thereof to be cavity, within and between the internal spheres.

To those that shall inquire of what use these included globes can be, it must be allowed, that they can be of very little service to the inhabitants of this outward world, nor can the sun be serviceable to them, either with his light or heat. But since it is now taken for granted that the earth is one of the planets and that they all are with reason supposed habitable, though we are not able to define by what sort of animals; and since we see all the parts of the creation abound with animate beings, as the air with birds and flies, the water with the numerous varieties of fish, and the very earth with reptiles of so many sorts; all whose ways of living would be to us incredible, did not daily experience teach us; why then should we think it strange that the prodigious mass of matter, of which this globe consists, should be capable of some other improvements, than barely to serve to support its surface? Why may not we rather suppose that the exceeding small quantity of solid matter, in respect of the fluid ether, is so disposed by the Almighty wisdom, as to yield as great a surface for the use of living creatures, as can consist with the conveniency and security of the whole?

But still it may be said, that without light there can be no living, and therefore all this apparatus of our inward globes must be useless: to this I answer, that there are many ways of producing light, which we are wholly ignorant of; the medium itself may be always luminous, after the manner of our ignes fatui. The concave arches may in several places shine with such a substance, as invests the surface of the sun; nor can we, without a boldness unbecoming a philosopher, adventure to assert the impossibility of peculiar luminaries below, of which we have no sort of idea.

Lastly, to explain yet farther what I mean, I have adventured to add the following scheme, fig. 18, pl. 11, where the earth is represented by the outward circle, and the three inner circles are made nearly proportionable to the magnitudes of the planets Venus, Mars, and Mercury, all which may be included within this globe of the earth, and all the arches be more than sufficiently strong to bear their weight. The concave of each arch, which is shaded differently from the rest, I suppose to be made up of magnetical matter; and the whole to turn about the same common axis p p, only with this difference, that the outer sphere still moves somewhat faster than the inner. Thus, the diameter of the earth being about 8000 English miles, I allow 500 miles for the thickness of its shell, and another space of 500 miles for a medium between, capable of an immense atmosphere for the use of the globe of Venus:

Venus again I give a shell of the same thickness, and leave as great a space between her concave and Mars; so likewise from Mars to Mercury, which latter ball we will suppose solid, and about 2000 miles diameter.

Since this was written, a discovery I have made in the celestial motions, seems to render a farther account of the use of the cavity of the earth, viz. To diminish its specific gravity in respect of the moon: for I think I can demonstrate, that the opposition of the ether to the motions of the planets, in long time becomes sensible; and consequently the greater body must receive a less opposition than the smaller, unless the specific gravity of the smaller do proportionably exceed that of the greater, in which case only they can move together; so that the cavity I assign in the earth may well serve to adjust its weight to that of the moon; for otherwise the earth would leave the moon behind it, and she become another primary planet.

A Paper of the Hon. Robert Boyle, deposited with the Secretaries of the Royal Society, Oct. 14, 1680, *and opened since his Death; being an Account of his making Phosphorus, &c.** N° 196, p. 583.

Sept. 30, 1680, there was taken a considerable quantity of human urine, because it yields but a small proportion of the desired quintescence, and a good part of this at least had been digested for a pretty while, before it was

* At p. 489, Vol. II. of this abridgment, it has been mentioned in the biographical anecdotes concerning Kunckel, that Mr. Boyle deposited with the secretaries of the Royal Society, some years before his death, an account of a process, whereby he succeeded in procuring phosphorus from urine. This he did at a time (1680) when the processes of Brandt and Kunckel were not known, or if known were kept a profound secret. It is true that Mr. Boyle had previously seen some specimens of phosphorus, both solid and liquid, which had been brought to England by Kraaft; but this person was ignorant of chemistry; and all that Mr. Boyle could collect from him was, that the phosphorus which he exhibited was produced from "somewhat that belonged to the human body." Mr. Boyle therefore appears to be entitled to the claim of having discovered a method of obtaining this curious product, equally with the German chemists, Brandt and Kunckel. Nor is this claim by any means invalidated by the circumstance mentioned by Stahl, viz. that Kraaft told him he had communicated the process to Mr. Boyle; as Kraaft's general character, and in particular his treacherous conduct towards Kunckel, destroy the credit of such an assertion; add to this, that Mr. Boyle's candour and probity stand too well attested, to leave the slightest suspicion that he would have concealed such a circumstance, had it been true. But to remove all doubt on this subject, we shall insert the account of this transaction in Mr. Boyle's own words: "After Mr. Kraaft had shown me (says this distinguished philosopher) both his liquid and consistent phosphorus; being by the phænomena I then observed made certain, that there is really such a factitious body to be made as would shine in the dark, without having been before illumined by any lucid substance, and without being hot as to sense; I considered in what way it might be most probable to produce, by art, such a shining substance. Mr. Kraaft indeed gave me, in return of a secret I communicated to him, a remote hint of the principal matter of his phosphori, by saying it was somewhat that belonged to the body of man.—But I made many fruitless attempts, with many unlucky accidents, before I could bring the thing to

used. Then this liquor was distilled with a moderate heat, till the spirituous and saline parts were drawn off; after which, the superfluous moisture also was evaporated, till the remaining substance was brought to the consistence of a somewhat thick syrup, or a thin extract. This done, it was well incorporated with thrice its weight of fine white sand; and the mixture being put into a strong stone-retort, to which a large receiver, in a great measure filled with water, was so joined, that the nose of the retort almost touched the water: then the two vessels being carefully luted together, a naked fire was gradually administered for 5 or 6 hours, that what was either phlegmatic or volatile might come over first. When this was done, the fire was increased, and at length for 5 or 6 hours made as strong and intense as the furnace was capable of giving: which violence of fire is a circumstance not to be omitted in this operation. By this means there came over a great deal of white fumes, almost like those that appear in the distillation of the oil of vitriol; and when those fumes were passed, and the receiver became clear, they were a little after succeeded by another sort, which seemed in the receiver to give a faint bluish light, almost like that of little burning matches dipped in sulphur. And last of all, the fire being very vehement, there passed over another substance, that was judged more ponderous than the former, because it fell through the water to the bottom of the receiver; whence being taken out, and partly even whilst it stayed there, it appeared by several effects, and other phænomena, to be such a kind of substance as we desired and expected.

On the Wonderful Dome Temple at Delos. By Dr. Wallis. N° 196, p. 584. *Translated from the Latin.*

In the island of Delos a temple, consecrated to geometry, was erected on a

bear; till at length being confident upon the nature of the thing, I would not believe the skilful labourer when he told me with concern, that what I expected was not produced; but going myself to the laboratory, I quickly found, that by the help of the air or some agitation of what had passed into the receiver, I could in a dark place, though it was then day, perceive some glimmerings of light. But this tedious pursuit only gave me a liquid phosphorus, not such a solid one as that of Mr. Kraaft; and I was willing to think that this defect might be imputed to the cracking of the retort, before the operation was quite finished. However, I have by this means found a substance that needs nothing but the air to kindle it, and that in a moment."—In another place Mr. Boyle says: "I will not positively affirm, that the matter I employed is the very same that was made use of by the ingenious German chemist, &c." Again, "The foregoing observations were made upon that substance, which *I guess* to be at least the chief employed by the Germans, &c."—Boyle's Works abridged by Shaw. Vol. III. p. 174, 175, and 208.—It is scarcely necessary to add, that the tedious and disgusting process of extracting phosphorus from urine is superseded by the new-discovered method of procuring it from bones.

circular base, and covered by a hemispherical dome, having 4 windows in its circumference, with a circular aperture at the top, so combined, that the remainder of the hemispherical surface of the dome was quadrable, or equal to a rectilineal figure; also in the cylindric part of the temple was a door, absolutely quadrable. It is proposed to determine by what geometrical means the architect could accomplish the construction of this monument. This problem being proposed to Dr. Wallis, he resolves it on the following principles, viz. on the relation between a triangle and a portion of a circle in the form of a lune, as determined by Hippocrates of Chios; and then by the relation between circular spaces and the curve surfaces of spheres and cylinders, as demonstrated by Archimedes.

Thus, if the semicircle ABD, fig. 19, pl. 11, be divided into the two quadrants, ACD, BCD, the chord AD of the quandrantal be drawn, and this be bisected in H by the radius CE; and on the centre H there be described the semicircle ADF: then it will be (because the square of AD is equal to half the square of AB), that the semicircle ADF is half the semicircle ABD, and therefore equal to the quadrant ACD. And hence, by taking away the common segment ADE, the remaining lune AEDF is equal to the triangle ADC; and consequently 4 such lunes equal to 4 such triangles, that is, to the whole square ADBG inscribed in the circle.

Again, by what has been demonstrated by Archimedes, the superficies of a sphere, is equal to 4 great circles of the same sphere; and therefore the surface of the hemisphere equal to 4 such semicircles, and so the surface of a quadrant of the hemisphere equal to one such semicircle.

Now let the circle ADBG be the base of the hemispherical curve surface; of which surface let P be the pole, fig. 20, the axis CP perpendicular to the plane of the base, and of which also DPA is a quadrant, which is bisected by the plane EPC passing through the axis.—Now, for the easier calculation, put r for the radius of the circle, its diameter $d = 2r$, its circumference c, and a proposed arc a. And putting the quadrantal arc DEA $= a = \frac{1}{4}c$; then the semicircle ABD is $= ar = \frac{1}{4}cr$; the triangle ADC $= \frac{1}{2}r^2 = \frac{1}{4}dr$; and, deducting the triangle, the remainder of the semicircle $\frac{1}{4}cr - \frac{1}{4}dr$; and this taking away from DPA (the quadrant of the hemispherical curve surface, which is equal to the semicircle ABD) the remainder will be equal to the proposed triangle ADC.

There are various ways of performing this; one is as follows: since the segments of spherical superficies, cut by parallel planes, have the same ratio as the segments of the axis; if there be taken in the axis CP, fig. 21, as the semicircle $\frac{1}{4}cr$, is to the same diminished by the triangle, $\frac{1}{4}cr - \frac{1}{4}dr$, that is, as c to $c - d$, so is CP : CY; or, which comes to the same thing, as c to d, so CP to PY: then the plane YZ parallel to the base, will cut off, from the curve surface, a portion adjacent to the pole, equal to the triangle ADC. And since the same holds in the other quadrants, therefore the whole surface round about the pole, so cut off, will be equal to the whole square inscribed in the base; which was to be done.

Or shorter, thus. The curve surface of the hemisphere, because equal to two of its great circles, is $= cr$; and the square inscribed in a great circle $= 2rr = dr$; and the former to the latter as c to d. Therefore (because the segments of the surface cut off by parallel planes, are proportional to the segments of the axis) taking CP to PY as c to d, then the whole surface being cr, the portion next the pole, cut off by the plane YZ, will be equal to dr, the square inscribed in the base.

Dr. Wallis adds also some other methods of performing the same thing.

Abstract of two Letters, sent some Time since by Mr. Anth. Van. Leuwenhoeck, to Dr. Gale and Dr. Hook. N° 196, p. 593.

Having examined the testicles of a rat, and the liquor pressed out of their seminal vessels, I found a great number of animalcules, long and serpent-like. The liquor itself in which they floated was transparent and oily, full of irregular parts, besides the animalcules, which I conceived were the rudiments of those animalcules which were generated in the thread-like vessels of the testicles. I suppose the manner of their generation differs from that of the eels I have formerly observed in vinegar, which carry their young in their body, whereas I believe these are produced from an egg. And possibly they may have their beginning with the generation of the animal, and come to perfection together with it, remaining in the seed in the testicles in fieri, till the animal itself is fit for copulation; as in man till about 14 years, and then first have life and perfection.

The particular coat of these thread-like vessels is exceedingly thin; and the contained animalcules so very minute, that 10,000 of them equal not one thread; whence may be computed the vast number one testicle is capable of containing. These threads are more conspicuous in the testicle of a rat than any other animal.

Endeavouring to examine the seed of muscles in March, when they spawn, I was somewhat prevented; only in the liquor contained in their shells, I found many small living animals; and the muscle itself had two thin films, consisting of long slender threads, with little knobby parts on them, something like the beards or fins of oysters, but were more easily separable from the fish: these strings were thick beset with hairs, continually moving; and through the threads themselves I saw a great number of small animals.

In the liquor in oysters I found several sorts of animalcules, but in their beards or gills none, possibly because the oysters were dead.

Observations on the Dissection of a Rat. By Mr. R. W. S. R. S. N° 196, p. 594.

The fore-feet of a rat resemble those of the castor. The hair, as in that animal, is also some of it fine, some coarse. The tail scaly, with hairs between every scale, like the castor's; which shows these two animals have some resemblance; and indeed the water-rat comes very near the beaver, and makes its holes in the sides of ponds after the same manner. The penis in the rat has a particular passage near the navel, as in squirrels; and not at the anus, as in the castor. The liver is full of little specks, as large as pins' heads, which are the small glands. There was no gall-bladder, only a ductus felleus; possibly the bladder was inclosed in the parenchyma of the liver, as it is in some animals. The cæcum was much larger than the stomach, and in shape like that of the castor. The testicles lay not behind, but in the groins, on the os pubis. These were like a bottom or skein of thread, rumpled up together, which was visible through the coats of the testicle. This thread continued of near the same size in the epididymides, only towards the deferentia it grew larger. It was tender, and not easy to be unravelled, so that I could not draw out above $\frac{3}{4}$ of a yard. The prostatæ lay under the spermatic arteries. The kidneys were whitish, with their succenturiati. At the neck of the bladder were inserted the vesiculæ seminales, transparent and filled with the semen. Toward the end of the penis, which had a bony gristle, were two large glands, emptying themselves near the extremity of the penis, and contained a substance like cream, as in the dormouse, observed by Swammerdam.

Fig. 1, pl. 12, represent the parts, viz. AA the kidneys; aa the renes succenturiati; bb the ureters; cc the crural veins and arteries; d arteria magna; e vena cava; f the bladder; gg the spermatic vessels, veins, and arteries; hh the testes, with the branches of the veins and arteries; ii the epididymides; kk the deferentia; l the penis; mm vesiculæ seminales; nn two glands, from whence a thick juice might be pressed out; o the balanus.

An Estimate of the Degrees of Mortality of Mankind, drawn from curious Tables of the Births and Funerals at the City of Breslaw; with an Attempt to ascertain the Price of Annuities on Lives. By Mr. E. Halley, R. S. S. N° 196, p. 596.

The contemplation of the mortality of mankind has, besides the moral, its physical and political uses, both which have been some years since most judiciously considered by the curious Sir William Petty, in his natural and political observations on the Bills of Mortality of London, owned by Capt. John Graunt; and since, in a like Treatise on the Bills of Mortality of Dublin. But the deductions from those bills of mortality seemed, even to their authors, to be defective: first, as the number of the people was wanting; secondly, as the ages of the people dying were not mentioned; and lastly, as both London and Dublin, by reason of the great and casual accession of strangers who die there, as appeared by the great excess of the funerals above the births, rendered them unfit to be standards for this purpose; which requires, if it were possible, that the people we treat of should not at all be changed, but die where they were born, without any adventitious increase from abroad, or decay by migration.

This defect seems in a great measure to be rectified by the late curious tables of the bills of mortality at the city of Breslaw, where both the ages and sexes of all that die are monthly delivered, and compared with the number of the births, for 5 years last past, viz. 1687, 88, 89, 90, 91, and seeming to be executed with all exactness and fidelity.

This city of Breslaw is the capital of the province of Silesia; or, as the Germans call it, Schlesia, and is situated on the western bank of the river Oder, anciently called Viadrus; near the confines of Germany and Poland, and nearly in the latitude of London. It is very far from the sea; whence the confluence of strangers is but small; and the manufacture of linen employs chiefly the poor people of the place, as well as of the country round about; whence comes that sort of linen we usually call your sclesie linen; which is the chief, if not the only commodity of the place. For these reasons, the people of this city seem most proper for a standard; and the rather because the births a little exceed the funerals. The only thing wanting, is the number of the whole people, which in some measure I have endeavoured to supply by comparison of the mortality of the people of all ages, which I shall from the said bills trace out with all the accuracy possible.

It appears that in the 5 years mentioned, viz. from 87 to 91 inclusive, there were born 6193 persons, and buried 5869; that is, born per annum 1238, and buried 1174; whence an increase of the people may be argued of 64 per annum, or of about a 20th part, which may perhaps be balanced by the levies for the emperor's service in his wars. But this being contingent, and the births certain, I will suppose the people of Breslaw to be increased by 1238 births annually. Of these it appears, by the same tables, that 348 die yearly in the first year, and that but 890 arrive at a full year's age; and likewise, that 198 die in the five years between 1 and 6 complete, taken at a medium; so that only 692, of the persons born, survive 6 whole years. From this age, the infants, being arrived at some degree of firmness, become less and less mortal; and it appears, that of the whole people of Breslaw, there die yearly, as in the following table; where the upper line shows the age, and the next under it the number of persons of that age dying yearly.

7	.	8	9	.	.	14	.	18	.	21	.	27	.	28	:	.	35	.
11	.	11	6	.	5½	2	3½	5	6	4½	6½	9	.	8	.	7	7	.

36	.		42	.			45			.	49	54	.	55	:	56		.		63
8	.	9½	8	.	9	.	7	.	7	.	10	11	.	9	.	9	.	10	.	12

	70	71	.	72		77		81		84	.			90		91	.
9½	14	9	.	11	9½	6	.7	.3	.4	.2	.	1	.	1	.	1	.

98	.	99	.	100.
0	.	⅕	.	⅗

And where no figure is placed over, it is to be understood of those that die between the ages of the preceding and subsequent column.

From this table it is evident, that from the age of 9 to about 25, not above 6 per annum die of each age, which is much about one per cent. And whereas in the 14, 15, 16, 17 years, much fewer appear to die, as 2 and 3½, yet that seems rather to be owing to chance, as are also the other irregularities in the series of age, which would rectify themselves, were the number of years much more considerable, as 20 instead of 5. And by our own experience in Christ-Church Hospital, I am informed there die of the young lads, much about one per cent. per annum, they being of the foresaid ages. From 25 to 50 there seem to die from 7 to 8 and 9 per annum of each age; and after that to 70, growing more infirm, though the number be much diminished, yet the mortality increases, and there are found to die 10 or 11 of each age per annum: from thence the number of the living becoming very small, they gradually decline, till there be none left to die; as may be seen at one view in the table.

From these considerations I have formed the following table, the uses of which are manifold, and which gives a juster idea of the state and condition of mankind, than any thing of the kind yet extant. It exhibits the number of people in the city of Breslaw of all ages, from the birth to extreme old age, and thereby shows the chances of mortality of all ages; and likewise, how to make a certain estimate of the value of annuities for lives, which hitherto has been only done by an imaginary valuation; also the chances there are, that a person of any age proposed, may live to any other age given; with many more, as I shall hereafter show. This table shows the number of persons that are living in the ages current as annexed to them.

Age. Curt.	Persons.	Age. Curt.	Persons.	Age. Curt.	Persons.	Age. Curt.	Persons.	Age. Curt.	Persons.	Age. Curt.	Persons.	Age.	Persons.
												7	5547
1	1000	15	628	29	539	43	417	57	272	71	131	14	4584
2	855	16	622	30	531	44	407	58	262	72	120	21	4270
3	798	17	616	31	523	45	397	59	252	73	109	28	3964
4	760	18	610	32	515	46	387	60	242	74	98	35	3604
5	732	19	604	33	507	47	377	61	232	75	88	42	3178
6	710	20	598	34	499	48	367	62	222	76	78	49	2709
7	692	21	592	35	490	49	357	63	212	77	68	56	2194
8	680	22	586	36	481	50	346	64	202	78	58	63	1694
9	670	23	579	37	472	51	335	65	192	79	49	70	1204
10	661	24	573	38	463	52	324	66	182	80	41	77	692
11	653	25	567	39	454	53	313	67	172	81	34	84	253
12	646	26	560	40	445	54	302	68	162	82	28	100	107
13	640	27	553	41	436	55	292	69	152	83	23		
14	634	28	546	42	427	56	282	70	142	84	20	Total	34000

Thus it appears, that the whole people of Breslaw consist of 34000 souls, being the sum total of the persons of all ages in the table. The first use hereof is to show the proportion of men able to bear arms, which are those between 18 and 56, rather than 16 and 60; the one being generally too weak to bear the fatigues of war, and the weight of arms, and the other too infirm from age, notwithstanding particular instances to the contrary. By the table there are found in this city 11997 persons under 18, and 3950 above 56, which together make 15947; so that the remainder to 34000, being 18053, are persons between those ages; at least one half of these are males, or 9027: so that the whole force this city can raise of fencible men, as the Scotch call them, is about 9000, or $\frac{9}{34}$, or somewhat more than a quarter of the number of souls: which may perhaps pass for a rule for all other places.

The second use of this table is to show the different degrees of mortality, or rather vitality, in all ages; for if the number of persons of any age, remaining after one year, be divided by the difference between that and the number of the age proposed, it shows the odds there is, that a person of that age does not die in a year. As for instance, a person of 25 years of age has the odds of

560 to 7, or 80 to 1, that he does not die in a year; because, out of 567 living of 25 years of age, there die no more than 7 in a year, leaving 560 of 26 years old.

So likewise, for the odds that any person does not die before he attain any proposed age; take the number of the remaining persons of the age proposed, and divide it by the difference between it and the number of those of the age of the party proposed; and that shows the odds there is between the chances of the party's living or dying. As for instance, what is the odds that a man of 40 lives 7 years? take the number of persons of 47 years, which in the table is 377, and subtract it from the number of persons of 40 years, which is 445, and the difference is 68; which shows that the persons dying in that 7 years, are 68, and that it is 377 to 68, or $5\frac{1}{2}$ to 1, that a man of 40 lives 7 years. And the like for any other number of years.

Use III. But if it be inquired at what number of years it is an even lay that a person of any age shall die, this table readily performs it; for if the number of persons living of the age proposed be divided in two, it will be found by the table at what year the said number is reduced to half by mortality; and that is the age, to which it is an even wager, that a person of the age proposed shall arrive before he die. As for instance, a person of 30 years of age is proposed; the number of that age is 531, the half of which is 265, which number I find to be between 57 and 58 years; so that a man of 30 may reasonably expect to live between 27 and 28 years.

Use IV. By what has been said, the price of insurance on lives ought to be regulated, and the difference is discovered between the price of insuring the life of a man of 20 and 50, for instance: it being 100 to 1 that a man of 20 dies not in a year, and only 38 to 1 for a man of 50 years of age.

Use V. On this depends the valuation of annuities on lives; for it is plain that the purchaser ought to pay for only such a part of the value of the annuity, as he has chances that he is living; and this ought to be computed yearly, and the sum of all those yearly values being added together, will amount to the value of the annuity for the life of the person proposed. Now the present value of money payable after a term of years, at any given rate of interest, either may be had from tables already computed, or almost as compendiously, by the table of logarithms; for the arithmetical complement of the logarithm of unity and its yearly interest (that is, of 1,06 for 6 per Cent. being 9,974694) being multiplied by the number of years proposed, gives the present value of one pound payable after the end of so many years. Then, by the foregoing proposition, it will be, as the number of persons living after that term of years, is to the number dead; so are the odds that any one person is alive or dead. And consequently, as the sum of both, or the number of persons living of the age first proposed, is to the number remaining after so many years (both given by the table) so the present value of the yearly sum payable after the term proposed, to the sum which ought to be paid for the chance the person has to enjoy such an annuity after so many years. And this being repeated for every year of the person's life, the sum of all the present values of those chances is the true value of the annuity. The following table, computed on this principle, shows the value of annuities for every 5th year of age to the 70th.

Age.	Years purchase.	Age.	Years purchase.	Age.	Years purchase.
1	10,28	25	12,27	50	9,21
5	13,40	30	11,72	55	8,51
10	13,44	35	11,12	60	7,60
15	13,33	40	10,57	65	6,54
20	12,78	45	9,91	70	5,32

Use VI. Two lives are likewise valued by the same rule; for the number of chances of each single life, found in the table, being multiplied together, become the chances of the two lives. And after any certain term of years, the product of the two remaining sums is the chances that both the persons are living. The product of the two differences, being the numbers of the dead of both ages, are the chances that both the persons are dead. And the two products of the remaining sums of the one age, multiplied by those dead of the other, show the chances there are, that each party survives the other; whence is derived the rule to estimate the value of the remainder of one life after another. Now as the product of the two numbers in the table for the two ages proposed, is to the difference between that product and the product of the two numbers of persons deceased in any space of time, so is the value of a sum of money, to be paid after so much time, to its value under the contingency of mortality. And as the aforesaid product of the two numbers, answering to the ages proposed, is to the product of the deceased of one age multiplied by those remaining alive of the other; so is the value of a sum of money to be paid after any time proposed, to the value of the chances that the one party has that he survives the other, whose number of deceased you made use of in the second term of the proportion. This perhaps may be better understood, by putting N for the number of the younger age, and n for that of the elder; Y, y the deceased of both ages respectively, and R, r for the remainders; also $R + Y = N$, and $r + y = n$: then shall Nn be the whole nnmber of chances; $Nn - Yy$ the chances that one of the two persons is living, Yy the chances that they are both dead; Ry the chances that the elder person is dead and the younger living; and rY the chances that the elder is living and the younger dead. Thus, two persons of 18 and 35 are proposed, and after 8 years these chances are required. The numbers for 18 and 35 are 610 and 490, and there are 50 of the first age dead in 8 years, and 73 of the elder age. There are in all 610 × 490 or 298900 chances; of these there are 50 × 73 or 3650 that they are both dead. And as 298900 to 298900 — 3650, or 295250; so is the present value of a sum of money to be paid after 8 years, to the present value of a sum to be paid if either of the two live; and as 560 × 73, so are the chances that the elder is dead, leaving the younger; and as 417 × 50, so are the chances that the younger is dead, leaving the elder. Wherefore as 610 × 490 to 560 × 73, so is the present value of a sum to be paid at 8 years end, to the sum to be paid for the chance of the younger's survivance; and as 610 × 490 to 417 × 50, so is the same present value to the sum to be paid for the chance of the elder's survivance.

This possibly may be yet better explained by expounding these products by rectangular parallelograms, as in fig. 2, pl. 12, where AB or CD represents the number of persons of the younger age, and DE or BH those remaining alive after a certain term of years; whence CE will answer the number of those dead in that time; so AC, BD may represent the number of the elder age; AF, BI the survivors after the same term; and CF, DI those of that age that are dead at that time. Then shall the whole parallelogram ABCD be Nn, or the product of the two numbers of persons, representing such a number of persons of the two ages given; and by what was said before, after the term proposed the rectangle HD will be as the number of persons of the younger age that survive, and the rectangle AE as the number of those that die. So likewise the rectangles AI, FD will be as the numbers, living and dead, of the other age. Hence the rectangle HI will be as an equal number of both ages surviving. The rectangle FE being the product of the deceased, or Yy, an equal number of both dead. The rectangle GD or Ry, a number living of the younger age, and dead of the elder. And the rectangle AG or rY a number living of the elder age, but dead of the younger. This being understood, it is obvious, that as the whole rectangle AD or Nn is to the gnomon FABDEG or $Nn - Yy$, so is the whole number of persons or chances, to the number of chances that one of the two persons is living; and as AD or Nn is to FE or Yy, so are all the chances to the chances that both are dead; whence may be computed the value of the reversion after both lives. And as AD to GD or Ry, so the whole number of chances to the chances that the younger is living and the other dead; whence may be cast up what value ought to be paid for the reversion of one life after another, as in the case of providing for clergymen's widows and others by such reversions. And as AD to AG or rY, so are all the chances, to those that the elder survives the younger. I have been the more particular, and perhaps tedious, in this matter, because it is the key to the case of three lives, which of itself would not have been so easy to comprehend.

VII. If three lives are proposed, to find the value of an annuity during the continuance of any of those three lives. The rule is, as the product of the continual multiplication of the three numbers in the table, answering to the ages proposed, is to the difference of that product and of the product of the three numbers of the deceased of those ages, in any given term of years; so is the present value of a sum of money to be paid certainly after so many years, to the present value of the same sum to be paid, provided one of those three persons be living at the expiration of that term. Which proportion being yearly repeated, the sum of all those present values will be the value of an annuity granted for three such lives. But to explain this, together with all the cases of survivance in three lives; let N be the number in the table for the younger age, n for the second, and ν for the elder age; let Y be those dead of the younger age in the term proposed, y those dead of the second age, and υ those of the elder age; and let R be the remainder of the younger age, r that of the middle age, and ϱ the remainder of the elder age. Then shall $R + Y$ be equal to N, $r + y$ to n, and $\varrho + \upsilon$ to ν, and the continual product of the three numbers $Nn\nu$ will be equal to the continual product of $R + Y \times r + y \times \varrho + \upsilon$, which, being the whole number of chances for three lives, is compounded of the eight products following, viz. 1. $Rr\varrho$, which is the number of chances that all three of the persons are living; 2. $r\varrho Y$, which is the number of chances that the two elder persons are living, and the younger dead; 3. $R\varrho y$, the number of chances that the middle age is dead, and the younger and elder living; 4. $Rr\upsilon$, being the chances that the two younger are living, and the elder dead; 5. ϱYy, the chances that the two younger are dead, and the elder living; 6. $rY\upsilon$, the chances that the younger and elder are dead, and the middle age living; 7. $Ry\upsilon$, which are the chances that the younger is living, and the two others dead; and lastly and 8thly, $Yy\upsilon$, which are the chances that all three are dead. Which latter subtracted from the whole number of chances $Nn\nu$, leaves $Nn\nu - Yy\upsilon$ the sum of all the other seven products, in all of which one or more of the three persons are surviving.

To make this yet more evident, I have added fig. 3, pl. 12, where these eight several products are at one view exhibited. Let the rectangled parallelopipedon ABCDEFGH be constituted of the sides AB, GH, &c. proportional to N the number of the younger age; AC, BD, &c. proportional to n; and AG, CE, &c. proportional to the number of the elder, or ν: then the whole parallelopipedon will be as the product $Nn\nu$, or our whole number of chances. Let BP be as R, and AP as Y; let CL be as r, and Ln as y, and GN as ϱ, and NA as υ; and let the plane PRea be made parallel to the plane ACGE, the plane NVbY parallel to ABCD, and the plane LXTQ parallel to the plane ABGH; then our first product $Rr\varrho$ will be as the solid STWIFZeb; the 2d, or $r\varrho y$, will be as the solid EYZeQSMI; the 3d, $R\varrho y$, as the solid RHOVWIST; and the 4th, $Rr\upsilon$, as the solid ZabDWXIK; 5th. ϱYy, as the solid GQRSIMNO; 6th, $rY\upsilon$, as IKLMGYZA; 7th, $Ry\upsilon$, as the solid IKPOBXVW; and lastly, AIKLMNOP will be as the product of the three numbers of persons dead, or $Yy\upsilon$. I shall not apply this in all its cases, for brevity sake, but only show in one how all the rest may be performed; let it be demanded what is the value of the reversion of the younger life after the two elder proposed. The proportion is, as the whole number of chances, or $Nn\nu$, to the product $Ry\upsilon$, so is the certain present value of the sum payable after any term proposed, to the value due to such chances, as the younger person has to bury both the elder, by the term proposed; which therefore he is to pay for. Here it is to be noted, that the first term of all these proportions, is the same throughout, viz. $Nn\nu$; the second changing yearly according to the decrease of R, r, ϱ, and increase of Y, y, υ; and the third are successively the present values of money, payable after one, two, three, &c. years, according to the rate of interest agreed on. These numbers, which are in all cases of annuities of necessary use, I have set in the following table, being the decimal values of one pound payable after the number of years in the margin, at the rate of 6 per Cent.

Years.	Present value of 1*l.*	Years.	Present value of 1*l.*	Years.	Present value of 1*l.*	Years.	Present value of 1*l.*
1	0,9434	14	0,4423	27	0,2074	40	0,0972
2	0,8900	15	0,4173	28	0,1956	45	0,0726
3	0,8396	16	0,3936	29	0,1845	50	0,0543
4	0,7921	17	0,3714	30	0,1741	55	0,0406
5	0,7473	18	0,3503	31	0,1643	60	0,0303
6	0,7050	19	0,3305	32	0,1550	65	0,0227
7	0,6650	20	0,3118	33	0,1462	70	0,0169
8	0,6274	21	0,2941	34	0,1379	75	0,0126
9	0,5919	22	0,2775	35	0,1301	80	0,0094
10	0,5584	23	0,2618	36	0,1227	85	0,0071
11	0,5268	24	0,2470	37	0,1158	90	0,0053
12	0,4970	25	0,2330	38	0,1092	95	0,0039
13	0,4688	26	0,2198	39	0,1031	100	0,0029

It were needless to advertise, that the great trouble of working so many proportions will be very much alleviated by using logarithms; and that instead of using $Nn\nu - Yy\upsilon$ for the second term of the proportion, in finding the value of three lives, it may suffice to use only $Yy\upsilon$, and then deducting the fourth term so found out of the third, the remainder will be the present value sought; or all these fourth terms being added together, and deducted out of the value of the certain annuity for so many years, will leave the value of the contingent annuity on the chance of mortality of all those three lives. For example, let there be three lives of 10, 30, and 40 years of age, proposed; then the proportions will be thus:

As 661 × 531 × 445, or 156190995, or $Nn\nu$,
to 8 × 8 × 9, or 576, or $Yy\upsilon$ for the first year, so 0,9434 to 0,00000348
to 15 × 16 × 18, or 4320, for the second year, so 0,8900 to 0,00002462
to 21 × 24 × 28, or 14112, for the third year, so 0,8396 to 0,00008128
to 27 × 32 × 38, for the fourth year, so 0,7921 to 0,00016650
to 33 × 41 × 48, for the fifth year, so 0,7473 to 0,00031071
to 39 × 50 × 58, for the sixth year, so 0,7050 to 0,00051051

And so on, to the 60th year, when we suppose the elder life of 40 certainly to be expired; from whence, till 70, we must compute for the first and second only, and from thence to 90, for the single youngest life. Then the sum total of all these fourth proportionals being taken out of the value of a certain annuity for 90 years, being 16,58 years purchase, will leave the just value to be paid for an annuity, during the whole term of the lives of three persons of the ages proposed. And note, that it will not be necessary to compute for every year singly, but that in most cases every 4th or 5th year may suffice, interpolating for the intermediate years.

It may be objected, that the different salubrity of places hinders this proposal from being universal, nor can it be denied. But by the number that die, being 1174 per annum in 34000, it appears that about a 30th part die yearly, as Sir William Petty has computed for London, and the number that die in infancy, is a good argument that the air is but indifferently salubrious. So that by what I can learn, there cannot perhaps be one better place proposed for a standard. At least it is desired that in imitation hereof the curious in other cities would attempt something of the same nature, than which nothing perhaps can be more useful.*

* This paper of Dr. Halley's lays a good foundation for that part of Political Arithmetic relating to births, deaths, and the numbers of the people, as well as the calculation of annuities on lives. On these principles several other ingenious men have since written, both in the latter volumes of the Philos. Trans. and elsewhere, particularly Mr. Simpson and Dr. Price. Which volumes will hereafter afford an opportunity of comparing the several rules and deductions together.

The Wisdom of God manifested in the Works of the Creation; in two Parts. By John Ray, Fellow of the Royal Society. The second Edition enlarged. London, 8vo. 1692.* N° 196, p. 611.

Three Physico-Theological Discourses, concerning, 1. *The Primitive Chaos, and Creation of the World.* 2. *The General Deluge, its Causes and Effects.* 3. *The Dissolution of the World. By John Ray, F. R. S. The second Edition. Lond.* 8vo. 1693. N° 196, p. 615.

In the first discourse concerning the chaos, the author produces the testimonies of several heathen writers, to prove the production of all things out of it, which they considered self-existent, and unproduced, as he thinks, erroneously, which opinion he shows consentaneous to Moses, there being a gradual formation of things related, which were all produced, as he supposes, out of pre-existing seeds, which he says were first created by God. As to the separation of the land and water, which at first covered the face of the earth, he proposes, that it might be effected by the same causes which raise mountains now, viz. subterraneous fires and flatuses, such as Ovid in the 15th Metamorph. describes near the city Trœzen; and a later instance near Puzzuolo, of a new mountain; which last he describes from his own observation. He mentions several other hills raised, and now oft shaken by earthquakes and subterraneous fires, as the Andes, Alps, &c. Taking notice of an extraordinary one, which in the time of Valentinian shook the whole world, with some passages out of Strabo and others, he shows from a passage out of Julius Ethnicus, and father Kircher, that there may be a communication from one burning mountain to another, though at a great distance, by vaults under the sea; the bottom whereof, except where it is rocky, he by the way asserts to be very even. Of submarine plants he observes, there are none at great depths for want of air. This depth usually answers the height of the adjoining hills and lands. He treats of the use and necessity of mountains. Coming in the next place to the creation of animals, he proposes some questions, as, Whether God made at first the seeds only of all animals, and scattered them over the earth, or made the first set of animals in perfection, giving each species a power to generate? then, Whether he made a great many of a sort, or only two, a male and a female? And from these another question arises, Whether the ovaries of the first animals actually included in them the whole number to be produced by that species to the end of the world? Which he inclines to, and seems to make

* This being a well-known work, it seems unnecessary to give any thing more than its title.

the female the chief agent in generation; each egg containing an animalcule, the arguments for and against this hypothesis make up the remainder of this discourse; though he confesses himself not fully satisfied as to all doubts that may be raised, but ends with his reasons for dissenting from Leuwenhoeck, that all animals proceed from an animalcule in the male sperm.

The author takes the same method in the second discourse on the general deluge, bringing first the testimonies of the ancient heathen writers concerning it, endeavouring to show, that by Deucalion's they understood Noah's flood, which they also make universal, though he owns there was in Thessaly such a particular flood as they call Deucalion's, about 700 years after Noah's, and that of Ogyges in Attica, about 230 years before Deucalion's. Proceeding to treat of the causes of this general flood, rejecting that of the air's being turned into water, alleged by Kircher in Arca Noæ, he pitches upon those two mentioned in Genesis, the breaking up the fountains of the great deep, and opening the windows of heaven, by the last of which he supposes a great quantity of water may be afforded, taking the waters above the firmament to be waters lodged above the inferior regions of the air. By the fountains of the deep he understands the subterraneous waters. As to the expence of the sea-water by vapour, he concludes the receipts of the Mediterranean to fall short of its expence. He questions whether there be any under-currents in the sea, and proceeding to his present subject of the breaking up the fountains, he by the way dissents from Dr. Plot, in his Nat. Hist. of Staffordshire, That the valleys are as much below the surface of the sea as the mountains are above it, since the rivers run down from those valleys into the sea; and seems dissatisfied with the opinion of an inferior circulation of water, as not sufficiently demonstrated how it can be performed. Having observed that the hills and dry land are so equally dispersed over the world, as to counterbalance each other, so that the centres of motion, gravity, and magnitude, concur in one, he discourses occasionally of the original of springs, all which he holds to be partly from vapours condensed into dews, and partly from rain and snow; giving his thoughts on Mr. Halley's late hypothesis; coming at last to what he thinks the most probable causes of the flood, viz. the changing the centre of the earth at that time, and setting it nearer the middle of our continent, whence the Atlantic and Pacific oceans pressing on the subterraneous abyss, by that means forced the water upwards and compelled it to run out at the wide mouths made at the breaking up of the fountains of the deep. These waters thus poured out upon the earth, the declivity being changed by the removal of the centre, could not flow to the sea again, but stagnate on the earth; and after the earth returning to its old centre, these waters returned also to their former receptacles. He adds another hypothesis, That the Divine Power might at that time so depress the surface of the ocean, as to force the waters of the abyss through the forementioned channels, &c. An hypothesis like the former of these you will find at the end of a treatise, de Potentia Restitutiva, or of springs, published by Dr. Hook, Anno 1678, p. 50. Where, by the removal of the centre toward the Antipodes, he explains the appearance of several islands in our seas, by the recess of the water; which formerly were not observed, &c. In the next place our author, speaking of the effects of the deluge, has a particular chapter on formed stones, sea-shells, and the like bodies found at distances from the shore, and brings the arguments at large on both sides, for and against their being originally shells, bones, &c. He adds the draughts of some of the most different kinds of these bodies, and leaving the matter undetermined, he proceeds to give some account of the changes that have happened to the earth since the general deluge; as, the breaking off of some islands formerly joined to the continent, some places gained from the sea, others covered by it. Other changes happening to the earth by the sinking of mountains, changes by earthquakes, where he touches upon that lately happening in Jamaica, and that in England in September last, and as to earthquakes in England, that they have been very short, and finished at one explosion; an argument that the cavities wherein the inflamed matter is contained are here very narrow. Other changes have been caused in the earth by extraordinary floods, from long and continual rains; others by boisterous winds, and the like; which with some remarks, that the earth does not proceed so fast towards the levelling and general inundation, as the force of these causes seem to require, concludes this second discourse.

The third discourse being more theological, and less related to the design of these tracts, I shall be the more brief in the account of it, and shall only observe, that our author, in order to prove his assertion of a general dissolution by fire, besides scripture proofs, and the opinions of the primitive fathers, brings several from the ancient philosophers, whose opinions were, that the dissolution of the world should be by water and fire, alternately at certain periods, the gods themselves not being free from these catastrophes. Coming in the next place to the question, Whether there be any thing in nature that may probably cause or argue a future dissolution? He grants to the peripatetics, that supposing the ordinary concourse of God with second causes, the world might endure for ever, there being no such decay in nature as might argue the contrary. Proceeding to particulars, he examines the four probable causes of such a dissolution; first, as to the possibility of the water, in process of time, overflowing the earth; from the steeple of Craich, in the Peak of Derbyshire, formerly not to be seen at a certain distance, but now visible by the sinking of an interposed hill, which the rains have gradually lessened and washed down; (with some other observations of the like nature) he argues, that the waters may at last level the whole earth, and bring a total subversion thereof; to which purpose is inserted a large quotation out of Josephus Blancanus. For a second cause he alleges the extinction of the sun, instancing some observations of unusual defects and paleness of the sun for a considerable time, and thinks it not impossible but the maculæ solares may so far prevail, as wholly to rob us of his necessary influence. For a third cause, he brings the eruption of the central fire, the possibility whereof he argues particularly from new stars appearing and disappearing, which phenomenon he thinks may be so solved. A fourth cause may be the dryness and inflammability of the earth in the Torrid Zone, where by the way he explains what fire is, and in the end rejects this cause as insufficient. Coming to that question, how this dissolution will be effected; he answers, by fire, and concludes it will be after a miraculous way, suddenly, &c. He determines not the time, but allows it possibly at a great distance, and thinks it likely that it will be a renovation, and not an utter annihilation, which he proves from several old writers.

Leonardi Plukenetii Phytographia seu Plantæ quamplurimæ novæ et Literis huc usque incognitæ ex variis et remotissimis Provinciis ipsisque Indiis allatæ Nomine et Iconibus. Tabulis Æneis 130, *Fig. ferè* 800 *magnâ cum Industriâ et insigni sanè in Successores Beneficio illustratæ. Fol. Lond.* 1692. *Pars III.* N° 196, p. 618.

Some Experiments on a Black Shining Sand brought from Virginia, supposed to contain Iron, made in March 1689. *By Allen Moulen, M. D. F. R. S.* N° 197, p. 624.

A small phial filled with ordinary white sand, and containing only ʒi. gr. xi. being filled with the Virginia sand, was found to contain ʒii, ϑii, gr. i, that is, ʒi. ss. more than the former. 2. This sand applied to the magnet both before and after calcination, but the latter applied better to it than the former. 3. A parcel of this sand, mixed and calcined with powdered charcoal, and kept in a melting furnace for about an hour, yielded no regulus, but applied more vigorously to the loadstone than either of the former. 4. I fluxed a parcel of this sand with fixed nitre in a melting furnace for above an hour, but could obtain no regulus, nor any substance that would apply to the magnet, excepting a thin crust that stuck firmly to a piece of charcoal that dropped into the crucible when the matter was in fusion. 5. I fluxed it also with saltpetre and powdered charcoal, dropping pieces of charcoal afterwards into the crucible. It continued about an hour in the melting furnace in fusion, without producing a regulus, or a substance that would apply to the magnet, excepting only what stuck to the charcoal, as in the former experiment. 6. I fluxed another parcel of it with saltpetre and flowers of brimstone, without being able to procure any regulus. 7. I poured good spirit of salt on a parcel of this sand, but could observe no luctation thereby produced. 8. I poured spirit of nitre, both strong and weakened with water, on parcels of the same sand, without being able to discover any conflict. 9. I poured single aquafortis on another parcel of it, without being able to perceive any ebullition worth noting. 10. I poured double aquafortis on another parcel of it, which for ought I could discover had no more effect on it than the former. 11. I poured also some aquaregia on a parcel of it, without discovering any sensible effect. 12. I poured good oil of vitriol on another of this sand, but seeing no bubbles produced, I weakened the oil with water, but without any sensible effect. And several other experiments, with no better success.

An Account of the Hon. Robert Boyle's Way of examining Waters as to Freshness and Saltness. To be subjoined as an Appendix to his lately printed Letter about sweetened Sea-Water, Oct. 30, 1683.* N° 197, p. 627.

When I considered that, as I have found by various trials, divers metalline, and other mineral solutions, could be readily precipitated, not only by the spirit of salt, but by crude salt, whether dry or dissolved in water, it was no difficult matter for me to think, that by a heedful application of the precipitating quality of common salt, we might discover whether any particles of it, at least in any considerable number, lay concealed in distilled water, or any other proposed to be examined.

For this purpose, I took some common water distilled in glass vessels, that it might leave its salt, if it had any, behind it, and into 1000 grs. of it, put 1 grain of dry common salt: into a convenient quantity, as 2 or 3 spoonfuls, of this impregnated liquor, I let fall a fit proportion, for instance 4 or 5 drops, of a very strong and well filtrated solution of refined silver, dissolved in clean aquafortis; upon which there immediately appeared a whitish cloud, which, though but slowly, descended to the bottom, and settled there in a white precipitate.†

* N. B. This paper was deposited with the secretaries of the Royal Society, An. 1683, sealed up and opened since the author's death.—Orig.

† And perhaps it may be proper, that I here observe (what is not wont to be taken notice of.) That divers solutions of mineral bodies may be precipitated by dilution; that is, (to explain this ex-

The usefulness of this experiment is not to be estimated only by the examen it helps us to make of dulcified sea-water, but much more by the estimate, that by its means may be made of natural fresh waters, whether of springs, rivers, clouds, lakes, wells, &c. For it being generally granted, that those waters, cæteris paribus are the best, both for the wholesomeness and divers economical uses, as washing, brewing, &c. that are freest from saltness, which is an adventitious, and in most cases a hurtful quality in waters; by this way of examining these liquors, an attentive eye may soon discover whether there be any latent saltness in them; and may enable us especially by the help of a little practice, to give a near guess how much one water is fresher than another; and having once attentively marked what change 4 or 5 drops, for instance, of our discovering liquor, will make in two or three, or some other small determinate number of spoonfuls, or rather of $\frac{1}{2}$ ounce of water; it will not be difficult to make a near estimate, whether any natural water proposed to him, have a greater, an equal, or a less degree of freshness or saltness, than that water chosen for the standard; and how much the proposed liquor is more or less free from saltness, than the other.*

pression) when the solution has time enough allowed to diffuse itself through a great quantity of water, the saline parts are thereby so diluted and weakened, that they are no longer able to sustain the mineral corpuscles, they kept swimming before, but make with them and the water a confused and subsiding mixture, usually of a whitish colour. This may appear when the butter of antimony being put into common water, is thereby quickly and plentifully precipitated in the form of that white powder, which chemists call mercurius vitæ. To which I may add, that I have also produced a powder of that colour, by pouring into common water a strong solution of tin glass made in aquafortis. And by the same way we have precipitated the tincture for solutions of the finer parts of jalap, benjamin, true labdanum, antimonial sulphur, and divers other bodies made in vinous spirits. If it were not for this power, that water has to weaken most solutions of bodies, I could have employed, instead of that silver, either quicksilver dissolved in aquafortis or lead crude, or calcined in the same liquor, or (which is more convenient) in strong spirit of vinegar; since these, and some others, are found to be precipitable by salt-water into whitish powders. But though a very heedful observer may for a shift, make use of these metalline solutions, to guess at the quality of water, as to freshness and saltness, yet the precipitation that is made by dilution is not difficult to be distinguished from that which is performed by a true and proper precipitant, (as in our case by the common salt, that is harboured in the pores of the water) both by the quickness of the effect, and the copiousness of the white substance produced, and on both those accounts is very much inferior to it.—Original.

* The author hints in a note that his precipitant "may much assist men to discover whether a mineral water proposed to be examined do or do not contain some saline substance; and if it do, whether it contain it copiously or not. This I have tried (he adds) upon more than one of our English mineral waters, and thereby found, that one that is reputed of another nature, contained pretty store of saline matter; and that another is impregnated with a surprising plenty of a saltish substance."

But here it may be objected, that the experiments have been made only on waters impregnated with gross or corporeal sea-salt, which perhaps may not hinder, but that they may be imbued with the spirits of marine salt, which, by reason of their activity, may be as unhealthful to the drinker as the grosser salt itself. But though to this surmise I might answer, that a very small proportion of spirit of salt may in many cases make the water seasoned with it rather medicinal than unwholesome; yet I shall answer more directly to the objection, by saying that to manifest its not being well grounded, I took above 1000 grs. of distilled water, and, instead of gross salt, put to it one single drop of moderately strong spirit of salt, and having shaken it into the water, I let fall into a portion of this unequally composed mixture, some drops of our solution of silver, which presently began to precipitate in a whitish form; insomuch that this trial succeeded better than if the water had been impregnated with but 1000th part of gross salt. And to pursue this trial further, I had the curiosity to diffuse 1 drop of spirit of salt into 2000 grs. of distilled rain-water; and on letting fall some drops of our precipitant into it, I found that the success answered my expectation. And then to urge the trial yet further, I added as much of the same distilled rain-water, as by a moderate conjecture made it amount to at least half as much more. So that 1 grain of spirit of salt had a manifest operation, though not quite so conspicuous as the former, on above 3000 grs. of water, whose immunity from common salt was tried apart. And that a drop of the saline spirit we made use of did not equal in weight 1 grain of dry salt, I found by this, that having let fall into a counterpoised piece of glass, 10 drops of that spirit, I found them to want near $\frac{1}{2}$ gr. of 9 grs. weight. The like trial I made by substituting above 1000 grs. of rain-water, in the room of the like quantity of distilled water. And perhaps it will not be impertinent to add on this occasion, that in some places, especially lying in hot climates, it may sometimes be of use to know whether, on account of the sun's heat, or that of the subterraneal regions of the earth, the rain-water is impregnated with volatile (not acid) spirits, like those that are distilled from urine, and which I have, for curiosity's sake, obtained from a mineral body, native sal-ammoniac; on which account I made a trial, that informed me, that if 5 or 6 drops of strong spirit of urine were shaken into 1000 grs. at least of distilled or rain-water, impregnated with but one of salt, our precipitant would make a discovery of some saltishness in the liquor. And for ought I have yet observed, not only such undistilled waters as are generally allowed to be freely potable, but even those that nature herself distils, are not always quite devoid of saltness: for I have found rain-water that I caused to be carefully saved, after the house-tops had been newly well washed with former rain, to grow a little troubled, if any quantity of our precipitant were kept for some competent time in it. And being gently distilled off, it left a residuum, which with a little of our solution afforded a far more suddenly made and copious precipitate, than had been produced with the like quality even of pump-water itself. And, though I have met with rain-water that was more free from salt than any spring or river-water that I remember I have examined; yet, having for curiosity's sake made trial of snow-water, this liquor, which is thought to afford the lightest water of all natural ones, I manifestly found by our way of examining it, not to be devoid of saltness.*

I shall on this occasion observe, that there are many waters that are not considerably brackish to the taste, which yet, by reason of some unheeded saltness, as in most pump-waters, are more frequently, by reason of crudity, not only unfit, or at best less fit for divers economical uses, as washing, boiling of some meats, &c. but are very unwholesome; sometimes to a degree that makes them mischievous to whole communities, and perhaps nations. Of this it were to be wished, that it were harder to give instances. I remember I have seen a notable one in those huge and unsightly tumours about the throat, which are observed by travellers to be exceedingly common, among those that inhabit the lower tracts of ground that lie between the Rhætian, Helvetian, and some other neighbouring mountains; which monstrous swellings are generally imputed to the snow waters that flow from the mountains, and make the usual drink of the meaner sort of people; whence it is observed, that persons of better condition, who drink wine more than water, are either not at all or far less troubled with these disfiguring goitres,† as they call them. But much more notable instances to our present purpose are afforded me by that great traveller M. Tavernier, who, speaking of a nation of Caffres or negroes, that come sometimes to trade with the Portuguese from a remote part of Africa, informs us, " That the water of their country is very bad, which is the reason that their thighs do swell, and

* In this paper Mr. Boyle made the beginnings of a method of examining mineral waters, which has since been carried to so much perfection by Bergman, and other modern chemists. The test or precipitant by which Mr. Boyle ascertained the presence of so small a proportion of sp. of salt (muriatic acid) as one drop in 3000 drops of water, was nitrate of silver. The muriatic acid has so strong an attraction for this metal (silver) that it instantly seizes it, forming with it an almost insoluble compound, which renders the liquor turbid or milky, and gradully falls to the bottom.

† The unsightly disorder termed goitres, so frequent in the Alps and other mountainous regions, consists in an enlargement of the thyroid gland. Its production has been commonly attributed to the use of snow-water; but from the accounts of some late writers, it would appear to be owing to a combination of causes, such as bad air (in the valleys between the mountains, where the disorder is chiefly met with) bad diet, and bad clothing.

it is a wonder to see any one of them free." And again, where he speaks of he African kingdom or empire of Monomotapa, he has this memorable passage, "The natives never live long, by reason of the badness of the waters in the country: for at the age of 25 they begin to be dropsical, so that it is a wonder if any among them live above 40 years." What M. Tavernier delivers being taken for granted, it seems very probable, that these people may be much relieved, and be brought to live as long as other nations, if they had a compendious way to provide themselves plentifully with water, whose crudity is corrected, its grosser and heavier parts separated, and its brackishness destroyed by the fire, as its action is regulated and helped by their invention.

The experiment mentioned in this paper was tried at a meeting of the Royal Society, Feb. 17, 169½, by Dr. Sloane, with a success answerable to the assertions of the hon. author, and that 1 or 2 drops of spirit of salt, mixed with common water, would be by the same method discovered.

At another meeting of the Society, on the 2d of March following, Dr. Hook read a lecture concerning another method of his own, for the discovering the smallest quantity of salt contained in water, from a principle of hydrostatics; and he produced the apparatus which he had prepared to exhibit the same before the persons then present; and it was there shown, that the instrument he applied to that scrutiny did very evidently discover the mixture of a 2000th part of salt added to common water, and it seemed that it would easily have detected half that quantity of salt added to an equal quantity of water.

The method was by means of a large poise of glass, somewhat of the shape of a bolt-head, the ball of which was about 3 inches diameter, but its stem or neck not above half a line, or a 24th part of an inch; this was so poised by red lead put into it, as to make it but a little heavier than fresh water. Then this poise was suspended by the small stem to the end of a slender and nice beam, and being not overcharged with weight, would turn with a small part of a grain. This beam was hung on a steady frame, and the poise hanging at one end of it covered with the water to a certain mark or division made on the small neck, it was counterpoised by some small weights put into the opposite scale of the balance. Then a 2000th part of the water's weight was taken of common salt, and put into the whole 2000 parts of the water, which by being stirred soon dissolved. Then it was manifestly seen that near half an inch more of the neck emerged out of the water so seasoned, than before the 2000th part of salt was dissolved in it. This was only one use of this method of discovering very small alterations in the constitutions of bodies, the same author having long since, viz. Oct. 25, 1677, shown to the Society a method of discovering divers alterations much more minute, viz. to the 167000th part of its weight.

This description will be more easily apprehended by the first figure, fig. 4, pl. 12, where A denotes the beam, B the ball of the poise, CC the neck, D the mark to which it sunk in fresh water, EE the cistern or vessel containing the water, F the scale wherein the weights to counterpoise it were put.

Three Queries relating to Shells, proposed by Mr. Samuel Dale, and answered by Dr. Martyn Lister, R. S. S. N° 197, p. 641.

The Queries.—1. What are the entalia of the shops; by what authors described; under what names; and how they differ from the dentalia?—2. Of what shell is the blatta byzantina,* the operculum or lid?—3. There are divers sorts of purpuræ among authors, which is that of the shops? Likewise, which sort of buccina and umbilici marini ought to be used in the shops?

The Answer to the Three Queries, by Dr. Lister.—1. As to the entalia, I do not remember to have seen any thing in the shops under that name. The descriptions of the dentalia in Scroder, are very faulty; and both those and the entalia by him should seem to be the two species of dentalia, which are figured. By me the dentalium being that which is commonly, and in plenty, found about the island of Guernsey, and elsewhere upon our coast, and the same with that found in the Mediterranean. It is a long, slender, round pipe, a little bending and tapering, hollow and open at both ends, without any crack or flaw, naturally white at one end, and usually a little reddish; very smooth and polished on the outside, and, from thence and the figure, called a dog-like tooth. The entalium, or other species of the dentalia, is much longer and thicker than the former, but much alike in other respects, save that this is streaked with high ridges, and mostly of a greenish colour. This species I guess to come from the Indies. Note, that any thing that is wrought into, or channelled, is in the modern Italian called an intaglia; whence I believe, and the nearness of the word dentalia, arose those distinctions of names.

2. To the second query; I take the blatta byzantina to have succeeded the unguis odoratus, and to have been brought into the shops in its stead. In the time of Dioscorides the best was brought from the Red Sea, viz. the palest and fattest; the blacker and less from Babylon or the Persian Gulf; but it seems that later times took up with those found about Constantinople; whence the present shop blatta had its name. The name of blatta is given to this operculum, I guess, from its being of a dark hair colour, as the common blatta pistinaria, so common in London, is; also this being a broad, thin, flat beetle, like the cover. It is true, that Dioscorides says, the unguis was an operculum,

* The blatta byzantina is, according to Linnæus, the operculum of the *strombus lentiginosus*.

like to that of the purple fish. He means what was used in his time; when it seems the unguis odoratus was lost, or was not brought to Europe. But it will appear from the same Dioscorides, that the unguis was no operculum; and it will be worth the while to show this mistake, and consequently the errors of the moderns in substituting the operculum of a marine turben for the true unguis odoratus. The unguis odoratus, according to Dioscorides is found in the lakes of India, where nard grows; on which account the conchylia feeding on nard are aromatic. It is gathered after the lakes are dried up with the summer heats. And he concludes, that the conchylium itself, burned or calcined, is of the same efficacy with the purpura and buccinum burned. In the chapter on nard, he further says, that the Indian nard grew near the river Ganges, that is, in certain lakes, caused by the overflowing of that river. Hence it appears, 1. That the unguis odoratus was part of a fresh water conchylium. 2. If it was generated in the nardiferous lakes on the river Ganges, how comes it that the same was brought from the Red Sea and Babylon? And why should the shell itself be brought so far as from the river Ganges to Greece, since the operculum was rarely a tenth part of the shell itself? Now if it was not used to be brought and exposed to sale, to what purpose to declare its virtues, or how could the experiment be made? I conjecture therefore, that the true unguis odoratus was something like the half of a pectunculus fluviatilis, so common in the river Thames, of the size and thickness of my thumb nail, and that for these reasons. 1. That the unguis odoratus seems to have been a fresh-water bivalve or muscle, because it was not gathered till the lakes on the river Ganges were dried up. Now bivalves are always buried in the sand and mud, and never rise up and swim about or float as the turbinate snails do, to which latter only the operculum belongs, and which therefore were at all times easily to be caught. 2. He calls this snail conchylium, and by that general name distinguishes it from all the other sorts, concerning which he treats in several chapters; which though in general it take in both kinds, as well turbinate as bivalve, yet it more particularly denotes a concha or bivalve. 3. The onyx is expressly reckoned by Pliny among the bivalves. For (l. 32, c. 11,) he makes all these synonimous, solen, aulos, donax, onyx, dactylus. And again more particularly, (lib. 9, c. 61,) he says that the dactyle are of the bivalve kind, and so denominated from their resemblance to the finger nails. So that in all probability the onyx odoratus anciently brought out of the fresh-water lakes about the Ganges in India, was not unlike the common onyx of the Mediterranean, which was of the solen kind. Whatever the blatta byzantina of our shops is, it has certainly nothing of the characters of the ancient aromatic unguis; which in all probability was lost on account of the difficult passage from the Ganges into Europe. I lament its loss, as I have reason to believe it was a good medicine, from its strong aromatic smell; which is much wanting in our testaceous powders.

As to the third; the purpura of the ancients* is well made out and figured by Fabius Columna: and it is one of the most common murices of the Mediterranean. In this he could not be much mistaken, because he somewhere mentions heaps of those shells, where the officinæ purpuræ anciently were; and also from the purple sanies the fish yields of itself. He mentions one or two more species of turbinate snails, to be found in the Mediterranean, which yield a purple juice. Upon the whole, it is indifferent, what sort of shell we use in the shops, if it is to be calcined, provided it be a sea-shell. Nor do I find either Dioscorides or Ætius distinguish between the ostrea purpura or buccinum calcined; but they ascribe to them all the same caustic virtue; possibly some one species may have it in a higher degree, as the various sorts of lime-stone, if calcined, differ in strength.

Though the species of shell or purpura be scarcely known to our shops at this day, yet the use of the purple juice has been, by tradition at least, transmitted down to our times, and kept as a secret even in these islands, till Mr. Cole discovered and published it. Sir Robert Southwell, now the president of the R.S., told me many years ago, that his own mother in Ireland was famous for marking handkerchiefs with the juice of fish; which mark would never wash out. And the learned Mr. John Beaumont informs me of a passage in Beda's Ecclesiastical History, relating to the purple, as a thing known in his time, viz. l. 1, c. 1; from whence it appears that the purple trade of dyeing was used in England, and very much valued. Of Mr. Cole's you have a cut in the Philosophical Trans. N° 178.† And fig. 5, pl. 12, represents the true purpura of the ancients, by the Italians called Gerusolo.

Extract of a Letter from Mr. Anth. Van Leuwenhoeck, concerning Animalcules found on the Teeth; and on the Scaliness of the Skin, &c. N° 197, p. 646.

I have often endeavoured to discover animalcules in spittle, but in vain: but examining a kind of gritty matter from between my teeth, and mixing it sometimes with rain-water, and sometimes with spittle, both which before had no animalcules, I then discovered with admiration a great number of very small ones moving; also a few larger ones, having a very strong and swift motion in the water, like eels. A second sort, much more numerous, turned themselves round like a top, and moved sometimes in a spiral manner. A third sort some-

* Murex. Trunculus. Lin.

† See p. 254 of this volume; also pl. 8, fig. 9, 10, 11, 12, 13.

times appeared oval, and at other times round; they moved swiftly by each other, like gnats playing in the air; and of these I discerned thousands in a drop of water; they showed no larger than a sand, and in the drop, the water was to the animalcules as 9 to 1. But most of the matter I examined, consisting of long slender parts, all of a thickness, but differing in length; and because I have formerly observed animalcules of this shape in water, I endeavoured to discover if these lived, but could not. I have found the same in the matter taken from between the teeth of other persons, as well such as drank wine and smoked tobacco, as of such as did neither, but not in their spittle, in which I found no animalcules. I found the same after I had washed my teeth very well with vinegar, though the vinegar killed them when they were put into it. Amongst the rest I saw some transparent particles 25 times larger than a blood globule, which had they not sunk down in the liquor, I should have taken for particles of fat.

I examined the supposed worms taken out of the skin of the nose and other parts of the face, but found them only soft brittle particles of fat: and perceiving on my nose several small black specks, I squeezed out some, and found them to be only a bundle of hairs, and in one of these I told 36 small hairs, besides the matter that stuck about them.

In a letter to Mr. Oldenburg, in the year 1674, I mentioned that I could not otherwise perceive, but that the scarf-insensible-skin consisted of small round scales, and declared my sense of the skin's fabric thus, that continually as it was worn away on the outside, it was supplied from beneath, and in several observations since I have found nothing new. These scales I judge so minute, that a sand might cover 200 or more of them; but viewing them with a better microscope, I find they are not formed by the moisture transpiring through the skin, as I then imagined; but that all the upper part of our skin is scaly, if I may be allowed to use that word, for they are very like those of fish, though they are without comparison smaller, and serve to the same purpose, which is, to defend the skin. These scales lay over one another as those of fish: they were five-sided, and I could plainly perceive a border or line about them. I guessed they were about 25 times broader than thick; they lay three double, for there was not above $\frac{1}{3}$ of each visible. The scales of fish lie after the same manner, only they never shed their scales, and our skin peels often, sometimes 1000 scales more and more, together in a flake.

By these observations I have now, as well as formerly, shown, that there are no pores in our upper skin, but that the moisture thrust out of our body gets through between the scales, at which places there may be possibly channels for it to issue at.—If we consider, that 200 of these scales may be covered with a sand, and that each scale may have many pores on the sides, besides the places where it is to be nourished; we may conclude the whole skin to be only as one continued pore.

I examined some of those scales on the inside of my hand, where the skin was thickest, and found that they differed from the others only in that they were thicker, and beset with many globules and stripes; but those on the body are clearer; and as those on the body peel and fall off for nourishment, those on the inside of the hand, by reason of a great quantity of clammy matter, driven out through the skin, are fastened to one another, so as to make a callous substance. Labour and working likewise forcing out a greater quantity of this glutinous matter.

On the Expansion and Contraction of Fluids by Heat and Cold, in Order to ascertain the Divisions of the Thermometer, and to make that Instrument, in all Places, without adjusting by a Standard. By Mr. Edm. Halley, S. R. S. N° 197, p. 650.

Qualities, such as heat and cold, moisture and dryness, and the like, are not otherwise to be estimated, but by their effect on the magnitude of some body they act on, by increasing or lessening its dimensions: or else by the motions they produce; both which subject them to mensuration. But it is still a question, how to ascertain the proportional heat or cold, &c. that is between any two climates or seasons, so as to conclude the one, for example, twice as hot, or twice as cold, as the other, though the instruments now in use abundantly suffice to show when the temper of the air is the same, and when it is warmer or cooler. The reason is, that we know not the causes of the expansion of fluids by heat, or of their contraction by cold, as arising from the nature of their constituent parts, which are so far from being objects of our senses, that they even surpass our most refined reasonings, and extort a confession of our ignorance after all our endeavours. For the same degree of heat does not proportionally expand all fluids; some swelling with a gentle warmth, and others not till they be considerably hot; some boiling with a moderate heat, and others not at all; some capable of great expansion, others increasing very little; so that it may well be concluded, that no one of them increases and diminishes in the same proportion with the heat, and consequently, that the thermometers graduated by equal parts of the expansion of any fluid, are not sufficient standards of heat or cold. This will be more evident from the experiments which I made some time since, with water, mercury, and spirit of wine, wherein the following particulars were very remarkable.

1. I took a large bolt-head, holding about $3\frac{1}{2}$ lb. of water, with a narrow neck, to show the increase more sensible; and having filled it with water, I noted exactly to what mark the water rose; then I immersed it into a skillet of warm water, and let it stand so long, till I concluded the warm water had communicated its temper to the water included in the bolt-head: and I found, that though the water was warm, much beyond the degree of the summer's heat, and notwithstanding it was winter; yet that gentle heat had scarcely any effect in dilating the water, so that it hardly appeared to have ascended in the neck of the bolt-head. Then I took the skillet, and set it over the fire; when it was observable that the water, as it became hot, ascended slowly in the neck, especially at first; but after it began to boil in the skillet, the expansion became more visible, and it ascended apace, till such time as it stopped again, the utmost effort of boiling water being able to raise it no higher. Then, having made a mark at the utmost height to which it had risen, I took it out, and had the satisfaction to observe, that though it was not raised so high without a very strong boiling, yet it subsided very slowly, as retaining some time the space it had acquired from the heat, even after the heat was passed, and the glass was so cool as to be touched without burning the fingers. However, the next morning I found it reduced to the first mark, where it stood when at first put in, having lost nothing sensible by evaporation during the experiment, which I attribute to the length of the neck wherein the vapours were condensed into drops before they reached the top. I then examined how much water would raise that in the neck, to the mark whereto it had been increased by boiling, and found it was a 26th of the bulk of the first water, which on repeated experiment I found to be true. But it was obvious that water, increasing so very little with all the degrees of heat the air receives from the sun, was a very improper fluid to make a thermometer of; and besides, any freezing liquor is useless for this purpose in these northern climates.

2. I took a smaller bolt-head, with a proportional cane or neck, and filled it, after the same manner, with mercury, and having boiled it as above, I observed that 125 ounces of mercury had increased by the space of 810 grains, or a 74th part of its bulk when cold. But it was very remarkable, that whereas a gentle heat had scarcely any effect on water, here on the contrary, the mercury did sensibly ascend at first, and had almost attained its greatest expansion before the water boiled in the skillet. And after it boiled, though I let it stand very long over the fire, I could not discern that the most vehement boiling had any effect on it, above what appeared when it first began to boil. The mercury being taken out, it subsided as it cooled, and in a few hours returned to the mark where it stood before it was put into the water. This fluid being so sen-

sible of a gentle warmth, and also not subject to evaporate without a good degree of fire, might most properly be applied to the construction of thermometers, were its expansion more considerable. However, small as it is, it is sufficient to disturb the precise nicety of the mercurial barometers, showing the counterpoise of the pressure of the atmosphere by a cylinder of mercury; for if mercury be more expanded, and consequently lighter, in warm weather than in cold, it will necessarily follow, that the same weight of atmosphere will require a taller cylinder in summer, and a shorter in winter to counterpoise it. And if the extremity of weather do but occasion an 150th part difference, as it is probable it does, its effect on a barometer will be a 10th of an inch, above and below the mean, or a 5th in the whole.

3. I filled the smaller bolt-head with spirits of wine, and having set it in the skillet of water over the fire, I found that it ascended gradually as the heat increased, but slower at first, and faster after it was well warm. At length, being arrived at a certain degree of heat, it would then boil with great violence, emitting bubbles, which rising into the neck of the bolt-head, would lift all the incumbent spirit, till they had made their way through. And these succeeding one another very fast, would often raise the spirits to the top of the neck, and spill it; so that I found I could go no farther with this liquor, than to that degree of heat which occasioned this boiling, and which wanted very much of that of boiling water, being almost tolerable to the touch. It was however very remarkable, how exactly this degree of heat was determined by the expansion of the spirits; for in the instant it reached a certain mark on the neck, it began to emit its bubbles; and having been taken out a little to cool and subside, it would certainly and constantly fall a bubbling again, when on a second immersion, it was arrived at the aforesaid mark. During this experiment it appeared, both by the dew on the neck, and by the scent in the room, that though the neck was about 30 inches long, yet the spirits evaporated very fast, considering the smallness of the surface of the liquor. And I have often noted the like evaporations condensed in dew within the head of the ordinary sealed thermometers, in very hot weather.

This degree of heat, which caused spirits of wine to begin to boil, being determined so nicely, made me conclude, that this might very properly be taken for the limit of the scale of heat in a thermometer; and its effect in the expansion of any other fluid being accurately noted, might be easily transferred to any sort of thermometer whatever. Only it must be observed, that the spirit of wine used to this purpose, be highly rectified or dephlegmated; for otherwise the different goodness of the spirit will occasion it to boil sooner or later, and so prevent the designed exactness. And by the way give me leave to hint, that the sooner or later boiling of spirits, or spiritous liquors, may possibly be as good a test of their strength and perfection, as their specific gravity, or any other yet used.

The spirit of wine I made use of, was possibly none of the best; but I observed, that at the point of boiling, it had increased a 12th part in bulk: which great dilatation makes it a liquor sufficiently adapted to our purpose, were it not for its evaporation, and for the difference in the goodness of the spirit; and because that in length of time it becomes as it were effete, and loses gradually a part of its expansive power.

All these experiments were made in the months of February and March, about 4 years since, the weather being reasonably cold, and not freezing; but I have not since had an opportunity to try the effect of extreme cold in contracting these liquors; which must be deferred till some sharp winter present us with a season proper for these trials.

4. Several other liquors may be examined after this manner, but the above may suffice to show the different effects of heat on different fluids; and that this power, of dilating and contracting with heat and cold, is as specifically in them, as their gravity, refraction, &c. but in none is it comparably so conspicuous as in that rare elastic fluid the air; for by several experiments that I have made, I find that the heat of summer expands the ordinary air about a 30th part; and Mr. Boyle, in his History of Cold, Tit. 18, § 8, p. 475, alleges his own trials, proving that the force of the strongest cold in England does not contract the air above a 20th part: so that, the sum of a 20th and 30th part being a 12th part, we may conclude that the same air which is extremely cold occupies 12 parts of space, in very hot summer weather will require 13 such spaces; which is as great an expansion as that of spirit of wine when it begins to boil; for which reason, and for its being so very sensible of warmth or cold, and continuing to exert the same elastic power; after being ever so long included, in my opinion it is the properest fluid for the purpose of thermometers.

Now the thermometers hitherto in use, are of two sorts; the one showing the different temper of heat and cold by the expansion of spirits of wine, the other by the air; but I cannot learn that any of them of either sort were ever made or adjusted, so as it might be concluded, what the degrees or divisions of the said instruments did mean; neither were they ever otherwise graduated, but by standards kept by each particular workman, without any agreement with, or reference to one another; so that whatever observations any curious person may make by his thermometer, to denote the degree of heat in the air, cannot be understood, unless by those who have also thermometers of the same make and adjustment. Much less has the way been shown how to make this instrument without a standard, or to make two of them to agree artificially without comparing them together.

I thought to have finished this discourse with showing a method of constructing and regulating thermometers to the best advantage; but finding it necessary to make some experiments with more accuracy than I have yet done, especially on the air's expansions; I crave leave, till one of the next transactions, to inform myself more fully in the matter, being unwilling to leave to the trial of others, what perhaps I have better opportunity to examine myself, especially in what is most difficult in this nice affair. I shall only propose, that whereas the usual thermometers with spirit of wine, do some of them begin their degrees from a point, which is that where the spirit stands when it is so cold as to freeze oil of aniseeds; and others from the point of beginning to freeze water; I conceive these points are not so justly determinable, but with a considerable latitude: and that the just beginning of the scales of heat and cold should not be from such a point as freezes any thing, but rather from temperature, such as is in places deep under ground; where the heat of the summer, or cold in winter, have been found to have no manner of effect.

Account of a Book, viz.—Novæ Hypotheseos, ad explicanda Febrium intermittentium Symptomata et Typos excogitatæ Hypotyposis. Unà cum Ætiologiâ Remediorum; speciatim vero de Curatione per Corticem Peruvianum. Accessit Dissertatiuncula de Intestinorum Motu Peristaltico. Authore Guilielmo Cole, M. D. Lond. in 8vo. 1693. N° 197, p. 657.

This author supposes the paroxysms of intermitting fevers to be produced by "a matter at first inoffensive (but afterwards maturating into a fermentative, acrimonious substance) admitted into the roots of the nerves in the cortical part of the brain, from them propelled into the medullary part, and thence into the tracts of the nerves and fibres, which he takes to be but propagines nervorum." After endeavouring to establish this hypothesis, he proceeds to consider the different types of intermittents, with the method of cure, introducing many observations on the Peruvian bark.

The subjoined discourse concerning the spiral fibres of the intestines, is a Latin translation of the English paper on this subject, inserted in N° 125, of the Philosophical Transactions, and reprinted in Vol. II. p. 295, of these Abridgments.

*Some further Considerations on the Breslaw Bills of Mortality.** *By Mr. Halley.* N° 198, p. 654.

Were the calculus from the bills of mortality founded on the experience of a very great number of years, it would be worth while to think of methods for facilitating the computation of the value of two, three, or more lives, which is perhaps a work of too much difficulty for the ordinary arithmetician to undertake. I have endeavoured to find a theorem that might be more concise than the rules before laid down, but in vain; for all that can be done to expedite it is, by tables of logarithms ready computed, to exhibit the ratios of N to Y in each single life, for every third, fourth, or fifth year of age, as occasion shall require; and these logarithms being added to the logarithms of the present value of money, payable after so many years, will give a series of numbers, the sum of which will show the value of the annuity sought. However, for each number of this series, two logarithms for a single life, three for two lives, and four for three lives, must necessarily be added together.

Besides the uses mentioned in my former, it may not perhaps be unacceptable to infer from the same tables, how unjustly we repine at the shortness of our lives, and think ourselves wronged if we attain not to old age, whereas it hereby appears, that the half of those that are born are dead in 17 years time, 1238 being in that time reduced to 616. So that, instead of murmuring at what we call an untimely death, we ought with patience and unconcern to submit to that dissolution which is the necessary condition of our perishable materials, and of our nice and frail structure and composition; and to account it as a blessing that we have survived, perhaps by many years, that period of life, at which the half of the whole race of mankind does not arrive.

A second observation is, that the growth and increase of mankind is not so much stinted by any thing in the nature of the species, as it is from the cautious difficulty most people make to adventure on the state of marriage, from the prospect of the trouble and charge of providing for a family. Nor are the poorer sort of people herein to be blamed, since their difficulty of subsisting is occasioned by the unequal distribution of possessions, all being necessarily fed from the earth, of which yet so few are masters. So that besides themselves and families, they are also to work for those who own the ground that feeds them; and of such does by far the greater part of makind consist; otherwise it is plain, that there might well be 4 times as many births as we now find. For

* See N° 196, p. 483, of this volume.

by computation from the table, I find that there are nearly 15000 persons above 16 and under 45, of which at least 7000 are women capable to bear children. Of these, notwithstanding there are but 1238 born yearly, which is but little more than a 6th part, so that about one in six of these women breed yearly; whereas, were they all married, it would not appear strange or unlikely, that 4 out of 6 should bring a child every year. The political consequences hereof I shall not insist on, only the strength and glory of a king being in the multitude of his subjects, I shall only hint, that above all things, celibacy ought to be discouraged, as by extraordinary taxing and military service; and those who have numerous families of children should be countenanced and encouraged by such laws, as the jus trium liberorum among the Romans. But especially by an effectual care to provide for the subsistence of the poor, by finding them employments by which they may earn their bread, without being chargeable to the public.

What a complete Treatise of Navigation should contain. Drawn up in the Year 1665. By Sir William Petty, late F. R. S. N° 189, p. 657.

1. What arithmetic, in whole numbers and fractions, as also in decimals and logarithms, is necessary for the same? and what books are best for teaching so much of it? 2. What common practical mechanical geometry, performable by the scale and compass is sufficient? 3. What trigonometry, right lined and spherical, will suffice? 4. How many stars are to be known? 5. What instruments are best for use at sea, with the construction of them, and the manner of using them? 6. The whole skill of the magnet, as to its directive virtues, and all the accidents which may befal it. 7. The hydrography of the globe of the earth, the views of the coast, and the description of the under water-bottom of the sea. 8. The knowledge of winds and meteors, so far as the same is attainable. 9. The history and skill of all sorts of fishings. 10. The art of medicine and surgery, peculiarly for the sea. 11. The common laws of the admiralty, and jurisdiction of the sea. 12. The several victuallings and clothings fit for seamen. 13. The whole science of ebbing and flowing, as also of currents and eddies at sea. 14. Dromometry, and the measures of a ship's motion at sea. 15. The building of ships of all sorts, with the several rigging and sails for each species, and the use of all the parts and motions of a ship. 16. Naval economy, according to several voyages and countries. 17. The art of canting, rowing, and sailing, of all the several sorts of vessels. 18. The gunnery, fire-works, and other armatures peculiar to sea and sea-fights. 19. The art of loading and unloading the chief commodities to the best advantage. 20. The art of weighing sunken ships and goods, as also of diving for sunken

goods in deep water. 21. The general philosophy of the motion and figures of the air, the sea, and of seasons, of timber, iron, hemp, tar, brimstone, tallow, &c. and of their several uses in naval affairs. 22. An account of five or six of the best navies of Europe, with that of the arsenals, magazines, docks, yards, &c. 23. An account of all the shipping able to cross the seas belonging to each kingdom and state of Europe. 24. An account of all the chief commercial parts of the world; with mention of what commodities are originally carried from, and ultimately to, any of them. 25. An account of the chief sea-fights, and all other naval expeditions and exploits, relating to war, trade, or discovery, which has happened in this last century. 26. Of the most advantageous use of telescopes for several purposes at sea. 27. Of the several depths of the sea, and heights of the atmospheres. 28. The art of making sea-water fresh and potable, and fit for all uses in food and physic at sea.

Extract of a Letter from Sir Robert Redding, late F. R. S. concerning Pearl-fishing in the North of Ireland; communicated to the Editor by Dr. Lister, R. S. S. N° 198, p. 659.

I have sent you four or five of the shells,* and a few of the pearls found in them, though clouded and little worth, taken out of the river near Omagh in the county of Tyrone, in which county are four rivers abounding with these muscles, all emptying themselves into Lough Toyle, on which the town of Derry stands, and so into the sea. There are also other rivers in the county of Donegal, and a river near Dundalk, also the Shure running by Waterford, the lough called Lough Lean in Kerry, which afford the like fish; and no doubt there may be many more, that I do not know; all these places are at the feet of very great mountains.

The manner of their fishing is not extraordinary; in the warm months, before harvest is ripe, while the rivers are low and clear, the poor people go into the water and take them up, some with their toes, some with wooden tongs, and some by putting a sharpened stick into the opening of the shell; and although by common estimate, not above one shell in a hundred may have a pearl, and of those pearls not above one in a hundred be tolerably clear, yet a vast number of fair mercantile pearls, and too good for the apothecary, are offered to sale by those people every summer assize. I saw one pearl there that weighed 36 carats, and was valued at 40l. and had it been as clear as some others produced therewith would certainly have been very valuable.

The young muscles have no pearls in them. The shell is fastened with two

* The shell here described is the *mya margaritifera*, Linn.

cartilages, one at each end, whereas the oyster and scallop are with one only in the middle. The natural posture they keep to, neither lying on the side, as those I saw, nor set up in the sand like eggs in salt, with the sharp end downwards, and the opening side turned from the torrent, as the people say, I leave to be further inquired into; but I saw them lying in part opened, and putting forth their white fins, like a tongue out of the mouth, which directs the eye to them in the water, being otherwise black as the stones in the river. The backs of the shells just about the hinges, on which the valves open, are all broken and bruised, and show the several crusts and scales composing the shell, and is probably caused by the many great stones that are driven over them by the floods, which are most impetuous after any little rain. The insides of the shells are of an oriental and pearly colour and substance, like a flat pearl, especially when first opened; and I am informed, that in some shells, under the first coat, lies a liquor very orient and clear, which moves on the pressure of the finger, but that such a muscle never has a pearl.

The part where the pearl lies is in the toe or lesser end, at the extremity of the gut, and out of the body of the fish, between the two films or skins that line the shell. I believe that this pearl answers to the stone in other animals, and certainly like that increases by several crusts, growing over one another, which appears by pinching the pearl in a vice, by which the upper coat will crack and leap away. This stone is cast off by the muscle, and voided as it is able; and many shells that have had pearls in them are found now to have none, which will appear by these instances. The shells that have the best pearls are wrinkled, twisted, or bunched, and not smooth and equal as those that have none. And the crafty fellows will guess so well by the shell, that though you watch them ever so carefully, they will open such shells under the water, and put the pearls in their mouths, or otherwise conceal them.

An Account of two Plants lately brought from the Cape of Good Hope. Communicated by Dr. Sloane, R.S.S. N° 198, p. 664.

1. The silver pine tree.—The twig of this tree * had a great many leaves set round it, very close to one another, so as to hide the twig itself; each of the largest of them being about 4 inches long, and ¾ of an inch broad in the middle where broadest; from whence they decrease towards both extremities, ending in a point, like those of the osier willow, only broader, and all covered over with thickest, finest, and longest white silken hair or down that ever any plant I remember to have seen. The cones are of

* Protea argentea. Linn.

the size of the cedars of Lebanon, and of the same shape; the cuticula, or small skin, of each scale being covered over with a white short down or wool, shining also like silk. Between the scales is lodged the seed, which is almost as large as the pine-nut, nearly the same shape, of a dark brown colour, and having a rising eminent line or belly running through the middle of it from end to end. This seed lies in a thin reddish brown membrane, which has on its top four feathers, like those belonging to the seeds of clematis, which, being between the scales and rising above them, adds a very great beauty to the cone, and may likewise serve for wings, by means of the wind, to loosen or carry the seeds to distant places, thereby propagating itself. This grows at the Cape of Good Hope, where I am assured by Mr. James Pettiver, that it is planted by the Dutch in their famous garden, being thought one of its greatest ornaments.

The first author I have observed that seems to mention this, is Captain Nicholas Dounton, who, speaking of things he observed by the Cape of Good Hope, apud purchas, lib. 3, cap. 12, § 1, p. 276, says, 'In divers places 'scatteringly we saw some trees of small stature, somewhat broad topped, 'bearing a fruit in size and proportion like a pine-apple, but the husk not so 'hard and spongy, the seeds whereof were devoured by the birds, the husks 'remaining on the trees, the leaves whereof were in form of our housleek in 'England, but not so thick.' Mr. Breynius likewise, in his first Century of Exotic Plants, p. 22, mentions a larger sort of frutex æthiopicus conifer foliis cneori, salici æmulus, which perhaps may be this. Dr. Plucknet, in the third part of his Phytographia, tab. 200, has figured this under the name of Leucadendros, Africana arbor tota argentea, sericea foliis integris, atlas tree D. Herman, which was the top of one of these young trees which came to Mr. Doody.

2. Conifera alypi folio, seminibus pennatis pluribus in medio coni conglomeratis, et non inter squammas aliorum conorum more nascentibus!

The branch of this tree had a brown coloured smooth bark, with a whitish hard wood, and small pith. The leaves were round it, without any order, very thick set, having no foot-stalks, being about $2\frac{1}{2}$ inches long, and about $\frac{1}{3}$ of an inch broad near the farther end, where broadest; smooth, hard, and of a brownish or dirty green colour; on the top of the branch comes the fruit, which is surrounded by three or four twigs overtopping it, and with their leaves almost hiding it. It is about 5 inches long, and is made up of many scales, hard and red, inclosing one another, the lowermost and outwardmost being very short the inwardmost 4 inches long, each of them ending in a point; some scales, having on their outsides a gummy juice. In the middle of these scales were the first rudiments of many seeds, not being fully ripe, each of which is set about with a great quantity of yellow fine silken down, ⅔ of an inch long, having a 2-inch long stylus or string, and yellowish membranes enclosing the stylus and tomentum, being feathered at top with feathers for the same purposes, and like the former.

I cannot find this mentioned by any author, unless it be that of Breynius, in the appendix to his first century, p. 22. Frutex Africanus, fruticis terribilis narbonensium folio capitulis oblongis squammosis, gathered by Wilhelmus Ten Rhyne, and sent to him from the Cape of Good Hope.

Extracts of four Letters from Mr. John Banister* † *to Dr. Lister, communicated by him to the Editor.* N° 198, p. 667.

Account of digging and preparing the Lapis Calaminaris, in a Letter from Mr. Giles Pooley to Sir Robert Southwel, President of the R. S. N° 198, p. 672.

The lapis calaminaris, or calamine, is dug and prepared near Wrington, in Somersetshire. The groovers have no certain method of discovering it, either from the surface of the earth, or the nature of the ground; it being sometimes found in meadows, sometimes in arable, and sometimes again in pasture grounds; neither is it discoverable from the colour or taste of the waters running thereabouts, as being much of the same colour, taste, clearness, and wholesomeness with other water; nor from the withering of the grass on the surface of the earth, or of the leaves of the trees, these being as fresh where calamine lies, as in any other place. But I observe that they always dig for it upon, or near the hills; for they expect none in those grounds which have no communication with hills.

* These letters of Mr. Banister contain notices of several plants, and other subjects of natural history, observed at different times in Virginia, but are not of sufficient consequence to be reprinted.

† "John Banister is mentioned by Mr. Ray in very high terms, as a man of talents in natural history. He first made a voyage to the East Indies, where he remained some time, but was afterwards fixed in Virginia. In that country he industriously sought for plants, described them, and himself drew the figures of the rare species; he was also celebrated for his knowledge of insects, and meditated writing the natural history of Virginia, for which Mr. Ray observes that he was every way qualified. He sent to Ray, 1680, a catalogue of plants observed by him in Virginia, which was published in the second volume of Ray's history, p. 1928.

"The world was deprived of much of the fruit of his labours by his untimely death. Banister increased the list of martyrs to natural history. In one of his excursions, in pursuit of his object, he fell from the rocks and perished. His herbarium came into the possession of Sir Hans Sloane, who thought it a considerable acquisition."

Pultney's Historical and Biographical Sketches of the Progress of Botany in England, vol. ii. p. 55.

Their method for finding out a vein, is by digging a trench as deep as the rocks, where they expect it to lie, across the place where they look for a course; which trench they generally dig from about north to south, the courses usually lying from east to west, or at 6 o'clock, as their term is. Though this is not always the case; for sometimes the courses, seams, or rakes, as they call them, lie at 9 o'clock, and sometimes are perpendicular, which they call the high time of the day, or 12 o'clock, and these courses they esteem the best. These seams or courses run between the rocks, generally wider than those of lead ore, unless they are inclosed in very hard cliffs, and then they are as narrow as the veins of lead. The colour of the earth where calamine lies, is generally a yellow grit, but sometimes black; for all countries, as they term their underground works, are not alike. Calamine itself is of several colours, some white, some reddish, some greyish, some blackish, which is counted the best; but when this is broken it is of several colours.* In working for it below, in the countries, they use the same way and instruments as in lead-mines; sometimes they light upon a good quantity of lead, and always find some eyes of lead among the calamine, which in ordering of it they separate: though in lead-mines they do not always find calamine. In landing the calamine, some pieces are larger than others, and of different sizes, as other stones are, and mixed with the gritty earth; but generally it rises in small particles, larger or smaller, and some about the size of a nut, which they call a small calamine. In old works, which are those that have been forsaken, and afterwards wrought again, damps and stenches sometimes arise, but never in new works; which damps arise from the neglect of those workmen that do not carry air along with them, which is done by air-shafts, as in lead-mines.

When they have landed a good quantity of this calamine, which is done by winding it up in buckets from the works, they carry it away to the places where they wash, clean, or buddle it, as their term is, which they perform after this manner: they inclose a small piece of ground with boards or turfs, through which a clear stream of water runs; within this inclosure they shovel the calamine, with the rest of the impure and earthy parts, which parts are carried away by the running water, which comes in at one end of the inclosure, and leaves the lead and the calamine, and the other heavier stony and sparry parts, behind; and for the better cleansing or buddling the calamine, while it is in this inclosure, they often turn it, that the water passing through may wash it the better; when they have thus washed it as clean as they can, after raking up the larger parts, both of the lead and the calamine, they afterwards put the smaller parts,

* A chemical analysis of these Somersetshire and other calamines by Mr. Smithson, inserted in the Phil. Trans. for 1803.

that they may lose none of their ore, into sieves made of strong wire at the bottom; and these sieves, with the calamine, lead, and the remainder of the earthy, sparry, and stony parts, which the water could not wash away, they often dip and shake up and down in a large tub of water, by which means the parts of the lead mixed among the calamine sink or pitch down to the bottom of the sieves, being the heaviest, the parts of the calamine in the middle, and the other sparry, stony, and trashy parts rise up to the top; which as they rise they skim off and throw among the rest of the rubbish: then they take off the calamine, and lastly the lead. When they have thus cleansed the calamine, as well as they can, to cleanse it still more, they spread it on a board, and so pick out with their hands the trash and stones that remain. But all of it does not require so much trouble; for some rises out of the works large enough to be cleansed and picked fit for the calcining oven, without all this charge and pains; and there are several loads of this large calamine, which have no mixture of earth or trash.

After they have prepared the calamine by washing and picking it, they then carry it to the oven, which is much larger than a baker's oven, but made much in the same fashion; only the way of heating, burning, or baking the calamine, is different; for it is not done as bread is, for they cast in their coals into a hearth made on one side of the oven, which is divided from it by a hem or partition, made open at the top, whereby the flame of the fire passes over, and so heats and bakes the calamine. They let it lay in the oven for 4 or 5 hours, the fire burning all the while, according to the strength of the calamine, some being much stronger than others, and so requiring a longer time; and while it continues in the oven, they turn it several times with long iron coal-rakes; when it is sufficiently burnt, baked, and dried, they beat it to a powder with long iron hammers, like mallets, on a thick plank, picking out what stones they find among it; so that at last the calamine is reduced to dust, and then it is fit for sale. The dust of calamine it seems conduces much to the curing of sore eyes; and that it is frequently used for taking films off the eyes of horses and other beasts.

An Arithmetical Paradox, concerning the Chances of Lotteries. By the Hon. Francis Roberts, Esq. F. R. S. N° 198, p. 677.

As some truths, like the axioms of geometry and metaphysics, are self-evident at the first view, so there are others, no less certain in their foundation, that have a very different aspect, and, without a strict and careful examination, rather seem repugnant. We may find instances of this kind in most sciences. As, in geometry, that a body of an infinite length may yet have but a finite magnitude;

in geography, that if Antwerp be due east of London, for that reason London cannot be west of Antwerp; in astronomy, that at Barbadoes, and other places between the line and tropic, the sun, part of the year, comes twice in a morning to some points of the compass; in hydrostatics, that a hollow cone, standing upon its basis, being filled with water, the water shall press the bottom with three times the weight, as it would do if the same water was frozen to ice; and figures might be contrived to make it press a hundred times as much. These speculations as they are generally pleasant, so they may also be of good use to warn us of the mistakes we are liable to, by careless and superficial reasoning.

I shall add one instance in arithmetic, which perhaps may seem as great a paradox as any of the former. There are two lotteries, at either of which, a gamester paying a shilling for a lot or throw; the first lottery, on a just computation of the odds, has 3 to 1 of the gamester, the second lottery but 2 to 1; nevertheless the gamester has the very same disadvantage, and no more, in playing at the first lottery as the second.

It looks very like a contradiction, that the disadvantage should be no greater in playing against 3 to 1 than 2 to 1; but it may be thus resolved.

Let the	1st	Lottery con-	3	Blanks	3	Prizes	16 pence	a-piece.
	2d	sist of	4	and	2	of	2 shillings	

In the first lottery the gamester hazards a shilling to win a groat, and the chances being equal, it is evident there is 3 to one against him. In the second lottery, the gamester ventures a shilling against a shilling, and the lots being 4 to 2, his disadvantage is 2 to 1. And a lot at either of them being truly worth just 8 pence, viz. the 6th part of 3 times 16 pence, or twice 2 shillings, the disadvantage must be the very same in both cases, that is, the gamester pays a shilling for a lot that is worth only 8 pence.

An Account of Books, viz.—I. Horti Indici Malabarici, Pars Quarta, Quinta, et Sexta. With some Remarks upon them by T. R. M. D. S. R. S. N° 198, p. 682.

The first three volumes of this work having been noticed in the Philos. Trans. N° 155,* I shall continue the account of the rest.

This 4th tome contains 61 trees, shrubs and arborescent herbs, together with their figures and descriptions; particularly of their flowers, fruits, seeds, and their vessels, omitted by the ancients.

Among the many rare vegetables, we have here a complete history of that pruniferous tree, called mango by the English, mao or mau by the Indians,

* P. 590, vol. 2, of these Abridgments.

ambo by the Brachmans, which grows above 40 feet in height, and 18 in thickness, exuding an odoriferous gummy substance; its leaves and root are of an aromatic quality; the flowers grow in clusters like those of the vine, are pentapetalous, white, and marked within with a yellow spot, and a honey spicy style, to which succeeds the fruit, of the figure of a kidney, as large as a goose egg, first green and chequered with white points, then yellowish, and afterwards of a golden colour, with a thin downy skin; the stone is oblong, flattish, and lanuginous, the kernel whereof resembles that of an almond; of these there are as many varieties in India, as peaches and plums in Europe: the wood is in use among the natives in burning their dead bodies, to which it is consecrated, and for making their coffins or urns. The Brachmans adorn their habitations with the boughs on festival days, they rub their teeth with the leaves, and use them, as they do the betel and faufel, the first of the pepper, the latter of the palm kind; the gum is given in all sorts of fluxes. The fruit is either eaten out of wine, or pickle, or in conserve, sometimes stuft with fresh ginger, garlic, mustard, &c. and sprinkled with salt, oil and vinegar, and so eaten with rice and other meats, as we do gurchens and olives. They make a meal of the dried kernels, of which they prepare several sorts of meat.

To this we might add the histories of many other exotic and rare trees, described and elegantly figured in this 4th part; as, the adamaran, a sort of almond tree, of which the Indians make cakes, milky emulsions, and an oil by expression; it bears fruit three times in the year. Panem-palka, a species of nutmeg, which the Turkish and Jewish merchants sometimes substitute instead of the true, with the mace on it, and sell the oil for genuine. The samstravadi, with the flowers and fruit, with which the ethnic and superstitious pilgrims deck their bodies, after having consecrated them; the Portuguese call it rosairos, numbering their prayers with them, instead of beads; it is a sort of plum, called jambos. Paënoe, of which, when young, the Indians make their masts, and entire vessels of the grown trunks, capable of holding 60 men; they flourish above 300 years, and afford a sort of gum anime or copal, used by the natives as frankincense in their sacrifices. The timber is not easily subject to the worm. Poerinsii, a sort of soap-tree, with the fruit of which they wash; and make their combs and musical instruments of the wood. Ponga, like the tataiba of Piso, or the fustic-wood tree. Ponna, distilling a substance like the gutta gamba, or gummi gotte, with which it agrees in many qualities. Tsierou-ponna, called kina in the island of Ceylon. Perin-toddali a sort of zyzyphus or jujube distilling the common lacca. Ravapou, a second kind of arbor tristis, a-kin to the jasmins; this is very fragrant, and expands on the rising of the sun. Bengieiri, a wood resembling the lignum moluccense or panave. Aria bepou, which is the true nimbo of acosta, of which there are two species. To these we might add some Indian ricini, cisti, several kinds of limes, althæas, plums, oriental elder, barberry, &c. with their uses.

The 5th volume comprehends 60 trees and shrubs, most of them bacciferous, excellently engraven in large copper plates. The katou karua is a second species of cinnamon, observed wild in several provinces of Malabar, the other sort being described in the first part of this work under the name of Karua, both inferior to the cinnamon-tree growing in the woods of Ceylon, though all alike in leaf, flower, and fruit. Beesha, the leaves of which are arundinaceous, a-kin to the ily or bamboo cane described in the first tome; of the beesha the natives make their baskets, arrows, and writing pens. Nola-ili, a third species of bamboo, sent as merchandise into Persia, Arabia, and the Turkish empire, where they make long pipes of it for smoking their tobacco. Cammetti, a sort of tree, tithymal, or spurge, above 40 feet high, with the decoction of which they kill worms, and cure ulcers: the milky juice of this plant mixed with the powder of carcapula, a sort of gutta gamba or gemou, is said to perform wonders in dropsies. Many of the rest contained in this 5th volume may be referred to our vitis-idæa, to the myrtles, the lauristines or wild bays; to the rhamnus, euonymus, &c. all which the Indians apply to many uses in physic, mechanics, and agriculture.

In the 6th volume are described and figured 61 trees and shrubs, the greatest part of which are siliquose, as the tsetti-mandarum, called by Breynius, frutex pavonius, sive crista pavonis, and by the Portuguese, flos pavonius, from the pride and figure of its flower. Its seed is not unlike that of Aldinus's acacia in the Farnesian garden, yet it seems a-kin to the senna kind. Tsiapangam, called by the Dutch rasphout, not unlike the red wood of Brasil in all its parts, and in the dying trade, for which it is sold; the lobe or pod is figured by Clusius in his Exot. l. 3, c. 16; but here we have the full history of it. Mouricou, commonly called the coral tree, of which there are several species in both Indies; it is spinose, and trifoliate, the flower and seed of a coccineous colour. In the island of St. Maurice it exceeds in height all the other trees, and therefore called elephantina. Its trunk is loaded with snails, and the pepper-shrub often climbs up it like ivy. Of the wood the natives make sheaths for knives and swords; and with it, and a calcined stone, they polish perspective glasses; with the bark they wash their vests called sarassas, and make the confection caril of the flowers. Wellia Tagera, called in several places of India coupang, of great use and success in gouty cases, as the kopang-tree of New England is reported to be by our planters; hence some stile it arbor antiarthritica.

To these we might add the katou-conna, a sort of cassia fistula. The thora paerou or cajan-tree, an arborescent phaseolus or laburnum, much cultivated at the Cape, affording a most refreshing pulse to the seamen. Mandsjadi, with the seed of which they weigh pearls. Niir-pongelion, whose long trumpet flower resembles that of dutroy; parrots are said to be much delighted with the fruit, and the Indians make nets of the boughs and root. Isora murri with its conical pod, wreathed spirally. Here are also many sorts of candle-trees, some of which are a kin to the mangrove of America. The hina-pareti, or rosa sinensis, with many other elegant alceas and althæas. The moringa, with several curious acacias and coluteas. Many of the rest may be referred to Clusius's exotic lobes and fruits, which this volume much illustrates, and the latter part exhibits some Indian jasmines, and large capers, belonging rather to the bacciferous tome.

A Treatise on the Roman Ports and Forts in Kent. By William Somner, &c. To which is prefixed the Life of Mr. Somner. Oxford, 8vo. 1963. N° 198, p. 688.

The author begins with Rutupium, and places it at Sandwich, making Richborough castle a pharus to guide the seamen into the harbour; and makes Gessoriacum, more anciently Portus Iccius, to be Boulogne; which Rutupium he says was then, and sometime after, called Ludenwich, at which place he affirms Cæsar first landed; though the author of the life allows of Mr. Halley's account, published in a late Transaction. As to the Goodwin Sands, he denies them ever to have been firm land; they are more soft, fluid, and porous, and yet tenacious, and consequently more voracious than other sands, which are firmer. Since they are not mentioned by any writers of great antiquity, he believes they may have been caused by the great inundation in Flanders about William the Second's or Henry the First's time; the recess of which water from these places probably left the Goodwin Sands shallower than formerly.

Next our author speaks of Dubris, now Dover: to omit his derivation of the name, he observes it was a Roman port, and has all along so continued. Boulogne on the French coast, and Rutupium on the British, being in time supplanted, the one first by Witsand, and at last by Calais, the other by Dover, at which place our author makes Cæsar to have first attempted to land: the fortification of the place in those days was more from nature than art. The rock being cut into such indentures, as resembled and were instead of walls with battlements, which time has now worn away. Proceeding to the port of Lemanis, which he makes new Romney, so called from being the Romans' port; and which although for some centuries it has lain dry, yet had formerly a fair and commodious river running by it and emptying itself into the sea, much nearer the town than now, where he endeavours to show, that there was formerly a river of the name of Rother, Romney, and Limene, and that it had its mouth at or by Romney; and when, and upon what occasion it forsook its wonted channel. The first two he proves by old records, where he observes that its mouth was wide enough to receive a fleet of 250 sail of Danish pirates, An. 893, who towed up their vessels 4 miles within the land; which it has now so forsaken, that there is neither harbour nor channel, which was turned another way by a great inundation of the sea about the year 1287. Next he gives the derivation of Romney, Apledore, Gilford, and Winchelsey.

As to the forts, he begins with Regulbium, now Reculver, where are still the remains of Roman tiles; here he observes, that all places ending in Chester, arise from the ruins of the old Roman castra, this place being once called Raculf-cester. All the Roman colonies, &c. being upon hills, he believes this might be placed on the ascent where the church now stands, or in the churchyard. 2. The next fort is Rutupium, or Richborough, mentioned before amongst the ports. The remains of the walls of this fort inclose almost as much ground as the Tower of London; here has been, and still is, more Roman coins found, than in any place in England. 3. Dubris or Dover, where he believes the church to have risen out of the old Roman fortress, and the square tower in the middle, fitted with holes for speculation, to have been the very Roman specula or watch-tower, and the Devil's Drop the remains of the Roman pharos. 4. Folkstone, a place famous for Roman antiquities, which he suspects the same with the Lapis Tituli of Ninius. 5. Lim or Limhill, where Stutfall-Castle, with a large circuit of 10 acres, was of old inclosed with a Roman wall; ruined not so much by time, as a seizure of its materials for building Lim-Church, and the archdeacon's castellated mansion. This place is also called Shipwey. 6. The last Kentish fort is Anderida or Anderidos; as to the situation, he is less certain whether at Pemsey, Hastings, or Newenden, though he inclines to one of the former. The Weald was formerly called Sylva Andred, a desert place unpeopled, filled only with herds of deer and droves of hogs. He ends this treatise with an enumeration of the quit-rents formerly paid out of the Weald, as gavel-swine, scot-ale, pannage, gate-penny, sumer-hus-silver, corredy, and danger.

The Specific Gravities of various Bodies. By Mr. J. C. N° 199, p. 694.

Pump-water	1000	Plum-tree (dry)	663	Santalum rubrum	1128
Cork	237	Mastic	849	Ebony	1177
Sassafras wood	482	Santalum citrinum	809	Lignum rhodium	1125
Juniper wood (dry)	556	Santalum album	1041	Lignum asphaltum	1179

Aloes	1177
Succinum pellucidum	1065
Succinum pingue	1087
Jet	1238
The top part of a rhinoceros-horn	1242
Top part of an ox-horn	1840
Blade-bone of an ox	1656
Human calculus	1240
Another calculus	1433
Another	1664
Common brimstone	1811
Borax	1720
A spotted factitious marble	1822
A gallipot	1928
Oyster-shell	2092
Murex-shell	2590
Lapis manati	2270
Selenitis	2322
Wood petrified in Lough-Neagh	2341
Onyx-stone	2510
Turcois-stone	2508
English agat	2512
Grammatius lapis	2515
A cornelian	2568
Corallachates	2605
Talc	2657
Coral	2689
Hyacinth (spurious)	2631
Jasper (spurious)	2666
A pellucid pebble	2641
Rock crystal	2659
Crystallum disdiaclasticum	2704
A red paste	2842
Lapis nephriticus	2894
Lapis amianthus from Wales	2913
Lapis lazuli	3054
A hone	3288
Sardachates	3598
A granite	3978
A golden marcasite	4589
A blue slate with shining particles	3500
A mineral stone, yelding 1 part in 160 metal	2650
The metal thence extracted	8500
The (reputed) silver ore of Wales	7464
The metal thence extracted	11087
Bismuth	9859
Spelter	7065
Spelter solder	8362
Iron of a key	7643
Steel	7852
Cast brass	8100
Wrought brass	8280
Hammered brass	8349
A false guinea	9075
A true guinea	18888
Sterling silver	10535
A brass half-crown	9468
Electrum, a British coin	12071
A gold coin of Barbary	17548
A gold medal from Morocco	18420
A Mentz gold ducat	18261
A gold coin of Alexander's	18893
A gold medal of Queen Mary	19100
A gold medal of Q. Elizabeth	19125
A medal esteemed nearly fine gold	19636

On the Iron-Works in Lancashire, by Mr. John Sturdy. Communicated by Dr. Martin Lister, S. R. S. N° 199, p. 695.

At Milthorp-forge, in Lancashire, they have several sorts of iron-stone, and of different natures; for some make cold-short iron, such as is brittle, when it is cold; another sort makes red-short, such as is apt to break if it be hammered, when it is of a dark red heat, and therefore is never melted down but in mixture, and so it yields an indifferent good sort of iron. Of late they have made it much better, by melting the sow-metal over again, and by using turf and charcoal, whereas formerly their fuel was only charcoal. They once tried pit-coal, but with bad success. The small dusty part of the charcoal is useful for burning the iron-stone; for every 17 baskets of this burned stone, they put in one of limestone unburned, to make it melt freely, and cast the cinder, which they always take off from the melted iron with a coal-rake, at a hole in the furnace-mouth, before they let the metal run. Nothing remains in the bottom of the hearth, all becoming either iron or cinder.

The furnace is built on the side of a hill, the bottom about 2 yards square, and so rises perpendicular for a yard or more; it is also lined within with a wall of the best fire-stone, to keep off the force of the fire from the walls of the furnace: the bellows, which are very large, and played with water, enter about the middle of the focus; the rest of the furnace is raised on this 6 or 7 yards square-wise, but tapering: so that the sides draw towards each other by degrees: and the top-hole, where they throw in baskets of stone and fuel, is but about ½ a yard square. Into this place they put down a pole, to know how far it has subsided after a certain time; and when they find it to have sunk about a yard and ½ they then put in more, till the furnace be full again.

The furnace is much like a common blacksmith's forge, about a yard and ½ over, and of the same height; the earth is all of sow-iron, much of the shape of a broad brimmed hat, with the crown downwards: this hollow place they fill and heap up with charcoal, on which when it is kindled, they put ore, first broken into pieces like a pigeon's egg, all round about the charcoal on the flat hearth, to bake or neal it; and they thrust it in by little and little, into the hollow, where it is melted by the blast, which is continued for about 12 hours feeding it still with new charcoal as it settles. Then they pull out a stopple at the bottom of the wall, whence runs out all the glassy-cinder, which is very liquid, leaving the iron in a lump at the bottom: this they take out with great tongs, and put under heavy hammers, worked with water, by which after several heatings, in the same furnace where it is melted, it is beaten into bars. They get about a hundred weight of metal at one melting, which is the product of about three times as much ore. They use no lime-stone, or any other thing, to promote the flux.

The ore is got in Fourness, at least 15 miles from Milthorp. Some of it is hard, but feels soft and smooth on the outside, like velvet; some of it again is as soft as clay: but all of it is red, and lies in beds like coal. The several sorts of ores lie in one vein or seam, which is sometimes an inch, sometimes a foot, and sometimes 3 or 4 yards broad, and many fathoms deep, between grey limestone rocks; but the hard ores lie usually next the rocks on each side, and the soft ore in the middle. They often use the soft ore, with good success, as a medicine for the murrain in cattle, and for all diseases in swine, giving these a good handful or two in milk.

Steel is not made from that they call steel ore, but iron, such as is made from the rest.*

Concerning the Seeds of Plants, with Observations on the Manner of the Propagation of Plants and Animals. By Mr. Anthony Van Leuwenhoeck. N° 199, p. 700.

In the seed of an ash, represented of the natural size by fig. 1, pl. 13. I observed not only two large leaves, but that part also whence the root takes its rise, was very large; this part of the seed I always found uppermost when growing on the tree, so that the seeds have a contrary situation on the tree, to what they have in the earth. The two leaves of the seed or lobes were filled up with an innumerable quantity of very small globules, except where the fibres were visible; which likewise were composed of much smaller globules, and took their origin from that part whence the root proceeds. This supposed root I cut through the middle, and have represented it fig. 2, where the outward ring represents the bark, the next represents the woody part, full of dark-coloured points, which are its fibres; the innermost oval represents the pith, composed of round bodies. I have found that the size of the seeds of plants do not answer to the size of the leaves, there being very small rudiments of the leaves and plant in the large seeds of the oak and peach, represented by fig. 3, and on the contrary, very considerable ones in the seeds of the ash.

As I have formerly observed in the fleshy fibres of the muscles, that there were no blood-vessels intermixed with them, but that they were placed only in the membranes that encompassed the muscles, and showed how the fleshy muscles might be nourished by these blood-vessels, so I find the leaves of plants to be made up of globules, included in the membrane that makes the superficies of the leaf in all places, but where the fibres are conspicuous. The manner in which I suppose these globules, and consequently the leaf, is nourished, is thus: the liquor or sap is conveyed in the vessel BC, fig. 4, and communicated first to the globule F, from that to G, thence to H, and so on; as if you should put several small pellets of dried clay in a glass vessel, if the water touch but one of them, it will communicate by that to the second, third, and so on till they are all wet.

Now, if in the small seeds of the ash, of which 6 weigh not 4 grains,

* Iron is converted into steel by combining it with carbon. The processes employed for this purpose are described by Mr. Collier, in the 5th vol. of the Manchester Memoirs.

there are to be seen not only perfect leaves with their vessels, but the woody part also, and that from whence the root shoots out, even plainer than in the walnut or hazel, we may well conclude that wise nature proceeds after the same manner in all its operations of generation and propagation; every seed containing not only the rudiments of the future plant, but also a certain fine flour to nourish it, till striking root into the earth, it may thence receive its nutriment. This flour is of an oily nature, and the more oily the longer will the seeds live out of the ground. And as plants are not male and female, not having a matrix for the first reception and sustenance of the young, so the parent tree produces a perfect plant, wrapt up in the seed, which the earth receives and nourishes. I have likewise found that of such trees as are reckoned male and female, very few that bore seeds the last year have born any this year; so that I question whether the trees which we find without seeds may be therefore called male trees.*

I think it now past all doubt, that the generation of animals is from an animalcule in the male sperm: and though I have often fancied that I have discovered the parts and membranes of the fœtus in this animalcule, so as to say there is the head, there is the shoulders, and there the thighs, yet I will affirm nothing herein, till I shall be so lucky as to find an animalcule large enough to discover this truth, which I am not quite in despair of, since I have been so fortunate as to meet with, in the small seeds of the ash, leaves and rudiments of the future plant, far larger than in the seed of any plant I have yet examined.

But to examine the matter a little closer: nature proceeds nearly after the same method in her operations, as to the production of plants and animals: for, as the animalcule of the male sperm cannot live in the matrix, without being wrapt up in the several coverings, and receiving its nourishment; so neither can the seed of the plant subsist without continual nourishment, and has also its coats to encompass it: and, as the fœtus has but one ligament, consisting of several vessels, by which it is fastened and nourished, so all the seeds which I have seen have but one ligament, made up of several vessels also, which is sometimes longer, sometimes shorter. In fig. 5, ABC is the outward membrane of the seed of an ash: AD the place where the seed lies, which is taken out and represented by EF; AF is the ligament by which the seed EF receives its nourishment, the part A only being joined to the tree; and what is more observable, the point of the seed F, where the ligament is fastened, is likewise the place whence the root proceeds: so that the root is the last that parts from the tree, which at first, while the seed is young, is

* Mr. Leuwenhoeck appears to have had very imperfect and indeed erroneous views of the sexual differences, which obtain in the vegetable kingdom.

upwards; but afterwards the seed and fruit growing heavier, it bends the stalk, and turns downwards to the earth. The same thing is in apples, pears, &c. These ligaments, by which the seeds are nourished, have their coats or bark, within which, as I guess, are more than 100 small vessels, in the filberd, all wreathed and twisted up after a spiral manner, as in fig. 6. It is observable, that this ligament is fastened, in almost all seeds, to that part whence the embryo plant arises, as in fig. 7, which represents a filberd larger than natural, that the vessels proceeding from the ligament may be more visible; where the ligament goes from A to B, branching all the way into ramifications, and these again into lesser, all which meet again at the place whence they began, that is, where the plant is to have its beginning.

In the 8th fig. I have shown the ligament of an almond, cut transverse, C D E F G is the cortical part; it is divided into 7 partitions, meeting at H, each of a reddish substance; the vessels to be seen in one of the spaces are represented by F G H, from the view whereof may be collected the great number of vessels in the whole ligament, by means of which the seeds of the almond and filberd are nourished.

If we consider the propagation of animals, and that they are so long nourished in the uterus by means of the umbilical vessels, till they are fit for a more open life, and are then no longer kept prisoners; and again, that the embryo which is to be the future plant, perhaps a tree, is so long contained in that body which we call the seed, and fed by means of a ligament from its matrix, viz. the tree, till it be of a competent growth, and has a sufficiency to provide for itself, and grow when exposed on the bare ground, and then it is no longer kept up. I say, if we consider these two methods of nature, we shall find no other difference between plants and animals, than that the first wanting a locomotive power, cannot couple as animals do, and therefore must contain in the same individual, not only the origin of the future plants, which I compared to the animalcule in the male sperm, but also the maternal nourishment sufficient for it, till it is furnished with a root to provide for itself. This nourishment is a sort of flour, which encompasses the embryo plant, and in the seed makes the two lobes.

If we compare plants with birds, we shall find, that as in birds, which are male and female, it is necessary for the animalcule of the male, already endowed with a living soul, to be placed near the yolk of the egg of the female, to be thence so long nourished till it is fit to receive its food from the mother, or gather it off the ground: so in plants the embryo is placed next to a sort of fine flour, which I compare to the yolk of the egg, which not only defends the young plant, but likewise affords it its first aliment.

We may likewise compare the propagation of trees with fish, and find the same agreement. In fine, the egg in animals seems to be for the same use as the lobes of the seed in plants.

Although I have formerly asserted, that the female served only to afford nourishment to the animalcules of the male sperm, and that plants grow out of the substance with which they are watered; yet I acknowledge for a certain truth, that a great variety is caused in animals by the nourishment received from the mother. So by a horse and she-ass a mule is generated, which is like neither, but participates of both, differing from the horse, especially in the ears and tale; since the ass abounding in that nourishment which produces the ears, and wanting that which gives a long tale, it must necessarily be like the mother in those two particulars. So from a white man and negro woman a Mestico is born; and from a large pigeon or cropper, and a small wild hen pigeon, the young are like neither; the egg of the female is not sufficient to nourish the animalcule of the male, so as to give it the size of its father. And thus plants receive a great alteration from the different soils in which the seeds are planted. So apples brought from France are with us in great esteem; and whatever care we take in the trees themselves, yet they soon degenerate in our soil; which change proceeds from the different salts they meet with in the ground. And I believe if we could take the embryo plant out of one seed and put it into another, so as it would grow, we should have a new plant from thence, like to neither: as if we should take the embryo out of the walnut, which I shall liken to the animalcule of the horse, and so join it to the seed of the chesnut, which I compare to the matrix of the ass, that it would grow, the plant produced by this union would be a new and unknown tree.

Willows are usually planted by thrusting a stake into the ground in wet places, yet finding several young ones on the banks of rivers, I judged these grew from the seed. Wherefore in the beginning of June, examining the downy seeds of these plants, I found several brownish particles, not much larger than sand, which the microscope discovered to be their seeds, which are contained in several little violet-coloured boxes, of which in a little sprig there were 75 placed by one another, each containing 3, 4, or 5 small seeds, encompassed with a pappous down. Fig. 9 represents these seeds of the natural size. The down or pappous part is joined by one common knot or centre first, and so to the seed, and consists of 2, 3, 4, 5, or 6 small threads, which as soon as the capsula breaks on the ripening of the seed, spreads itself every way, as fig. 10, though before the threads were closed up in one, as fig. 11, by which means it easily carries the small seeds on the wings of the wind to great distances. Viewing these seeds more nicely, I saw that part whence the root has its beginning, furnished with very many vessels, consisting only of oblong and round particles. The rest of the seed consisted of two lobes of a dark herby colour, made up of globules; and between these, two very small points rising up, which where the beginnings of the leaves of the tree or embryo plant, which the lobes themselves were to nourish till it should be furnished with a root to provide for itself.

I took some of these very small seeds, and sowed them in wet sand in my closet, in June, the better to discover the manner of their growth. These seeds being very much dried, and thence shrunken, appeared through the microscope, as fig. 12. ABEF is that part whence the root shoots forth. When they had lain in the wet sand 36 hours, they showed as fig. 13, the proportion of the part GHKL being then considerable, and in so short a time 6 roots were shot out from it, and the two lobes HIK began to show themselves. In 72 hours the roots began to divide and ramify, and to take hold on the sand. It is observable in this tree, that the seeds come to their perfect maturity before the leaves of the tree have their full growth, whereas other trees do not perfect their seeds till after the fruit in Autumn; so that this tree has its young plant grown up the first year. The same is observable in the elm; some of its seeds I gathered in May, dried and sowed them, and in three days they sprang up. I tried the same in the downy seeds of the poplar and Indian cotton. If these distinctions of parts are so soon visible in these small seeds, why should we doubt the production of an animal from the so often named animalcules. Indeed we must own ourselves at a stand, when we would find out how these animalcules receive life, and that not before the male has attained a certain age; and the rather, since we hold that the matter whence these animalcules proceed, was likewise in that animalcule itself when it was first committed to the matrix. And indeed that very extraordinary minuteness, by which one creature is transmitted to another, is incomprehensible.

Part of a Letter from Sir R. B. S. R. S. to Dr. Lister, concerning the Giant's Causeway, in the County of Antrim, in Ireland.* N° 199, p. 708.

Old Bawn, April 24, 1693.

The Giant's Causeway is in the county of Antrim, about 7 miles east of

* For a more satisfactory description of these basaltic pillars, the reader is referred to the Rev. Dr. Hamilton's Letters on the Northern Coast of the county of Antrim.

In a postscript subjoined to Mr. Molyneux's notes on the communication relative to the Giant's Causeway, inserted in one of the subsequent numbers of the Transactions (in No. 212), it is remarked, that the above account of these extraodinary basaltic pillars is extremely inaccurate;

Colrain and 31 miles to the east of the mouth of the river Derry. The coast there is very high above the sea, but rising gradually on the land side to the edge of the precipice, it is all covered with an excellent sweet grass; when you come to the precipice, there is no going down, being so perpendicularly steep, but with much labour and some hazard it may be climbed up. By other ways and windings one comes down to the Strand; in which, from the foot of this precipice, there runs out northward, into the main ocean, a raised causeway of about 80 feet broad, and about 20 feet high above the rest of the strand; its sides are perpendicular, and it went on above 200 feet into the sea; that is, it was so far in view; but it does not advance much farther under the superficies of the water. This whole causeway consists all of pillars of perpendicular cylinders, hexagons and pentagons, of 18 or 20 inches diameter, but so justly fitted to each other, that nothing thicker than a knife will enter between the sides of the pillars. The pillars do not consist of joints, but each cylinder is one solid piece, only indeed in breaking it breaks cross-wise or horizontally, and not lengthwise, which we commonly call splitting; and it is by its thus breaking, that the texture of the middle of the causeway is discovered; for pieces have been broken from many of the cylinders that are in the middle, by which one sees so deep the perpendicular sides and edges of the circumjacent columns.

That the pillars do not consist of joints, is manifest from this, that the pieces so broken off, have their bottoms as often convex or concave as flat and even; and many such pieces lie loose on the shore, which the sea has washed down from it. When one walks on the sand below it, the side of this causeway has its face all in angles, the several columns having some two, some three of their sides open to view. The high precipice consists all of pillars; though some shorter and some longer, and all the stones that one sees on that coast, whether single or in clusters, or that rise up any where out of the sand, are all columns though of ever so different angles; for there are also four squared on the same shore. This causeway runs out into the Northern Ocean, having no land over against it any where.

Account of a Storm of Thunder, Lightning, and Hail at Oundle in Northamptonshire, the 20th of March, 1692-3. *By Mr. W. R.* N° 199, p. 710.

A considerable storm of hail, thunder, and lightning, which set fire to the steeple of Oundle, but was soon extinguished again.

inasmuch as the narrator states that the pillars do not consist of joints but are of one entire piece, and calls them cylinders with angles adding that some of them are 4 squared; of which sort of figures Mr. Molyneux asserts, that there is not one to be met with.

Anatomical Observations on the Heads of Fowls. By the late Allen Moulen, M. D. S. R. S. N° 199, p. 711.

In the heads of all the fowls that I had an opportunity to examine, I constantly found only one aqueduct, or passage from the ears to the palate, whereas in men, quadrupeds, and some amphibious fish, there are always two, one on each side below the entrance of the nostrils into the palate, and opening towards the nostrils for the more convenient reception of air, as is supposed. This passage in fowls is exactly in the middle of the palate, below the entrance of the nostrils into it; it is a membranous tube, capable of admitting a raven's, if not a goose quill in larger fowl, such as turkeys, geese, &c. and reaches backward as far as the communication from ear to ear; and hence it comes to serve both; whereas there is a necessity of two in those animals, whose ears do not communicate.

I have constantly found a hollow space between the two tables between the os cuneiform reaching from ear to ear, and as far forward as the aforesaid common aqueduct, or rather ductus aereus, its mechanism seeming more to favour this than the former use. This cavity in all fowl reaches above the labyrinthus on each side, so that whatever impulse is made on the tympanum on the one side, may not only be very readily communicated, by means of the internal air, to the labyrinth of the same, but also to that of the opposite side. Hence probably proceeds the quickness of hearing and vigilancy of fowl, notwithstanding the want of a cochlea; the defect of which seems to be by this structure more than supplied, no other creatures that we know of having any thing of it. It is not improbable that the opposite ear to a sound is altogether as much affected by it as that next it, if not more. There are several laminulæ and pillars of hard bone, between the two tables in these cavities, designed as may be supposed, partly for their maintenance at a convenient distance, and partly for breaking the air, so as to prevent echoes and confused representations of objects, as it has been ingeniously observed by Sir John Hoskins, that pillars in churches very much prevented echoes.

In the heads of woodcocks, besides the passages now described, I found one on each side the bone, which makes the orbit of the eye, proceeding from the ear, and reaching forward towards the setting on of the beak, near which they joined in one, and turned under the skull in a small passage, leading to the cavity by which the ears communicate, and which is above described, into which it enters. These passages are also in the heads of snipes; and besides, one over the sinus longitudinalis, and another over the sinus lateralis of the brain.

In the heads of parrots and paroquets, besides the first described passage, I observed, between the two tables, every where cells opening into others, and those into others, so that there was not any part scarcely of the skull that was not occupied with them. And this did not only appear by pouring into one ear freed from its drum, the other also being removed, a tincture of cochineal, and then blowing it into all these cells, so that no part was free from tincture, but it appeared also to the naked eye, notwithstanding that sometimes it was difficult to trace the communications of them by reason of the numerousness of the laminulæ and pillars.

In singing birds, the structure of these passages is like that of the parrot and paroquet, only that the pillars and laminulæ are less than they should seem to be in proportion to the heads. From whence it is probable, that these birds are by this structure enabled to distinguish sounds and notes, and also imitate them better, having a more musical ear.

In the heads of pullets, geese, and ducks, I found only the first described passage distinctly, but in plovers, bustards, and some others, I found another that went over the sinus lateralis of the brain, from ear to ear. This seems to be designed to make them more watchful than domestic fowl, or yet those that live much on the water, because they are liable to many dangers, which the others are exempt from.

In the ears of all the fowl that I could examine, I never found more than one bone and a cartilage, making a joint with it, that was easily moveable. The cartilage had generally an epiphysis or two, one on each side, which were very flexible, as itself was. The bone was small and very hard, having at the end of it a broad but very thin plate of the same substance, on which it rested as on its basis. I got that of a pullet's ear, represented in fig. 14, pl. 13, where a is the main cartilage, and bb the two epiphyses, c the small bone, and d the basis or broad end of it. In the figure also, part of the drum sticking to it is represented, together with the cartilages.

I observed three pair of nerves in all the broad billed birds, and in all such as feel for their food out of their sight, as snipes, woodcocks, curlews, geese, ducks, teal, widgeon, &c. These nerves are very large, equalling almost the optic nerve in thickness, they begin a little more forward than the auditory nerve from a little protuberance, which seems to be made for them; one of them goes over the optic nerve in the orbit of the eye, the other two go under the eye. Two are distributed near the end of the upper bill, and are there very much expanded, passing through the bone into the membrane, lining the roof of the mouth. The third pair is distributed near the end of the lower bill, and is subdivided like the former. Birds that pick their food where they can see it have not these nerves, and the pair of nerves belonging to the upper bill is considerably smaller, in proportion to the fowls, than those observed above; whence it is probable that these nerves were designed for some great use, both on the account of their number and their size; and that the use to be assigned to them must be to enable them to distinguish, whether by tasting or feeling, I will not now distinguish, their food, there being a necessity of a more exquisite sense in these fowls than in any other. Fig. 15 represents those in a duck's head, where aa denote the edge of the cranium, which was in part removed for the clearer view of these nerves; bb are the cells about the ear, between the two tables above described; cc the brain laid bare with its blood vessels; ddd the three nerves on one side, e the optic nerve; fff the skin and part of the bone, removed to bring the nerve in view; gg the two nerves expanded near the end of the upper bill; hh that in the lower.

All the eyes of fowl and of fish, that I have examined, were more or less cartilaginous; for the sclerotis is a cartilago sui generis, especially near the cornea, in all these animals. And in the larger sorts of both, I remembered to have found the whole sclerotis such a kind of a cartilage.

In the eyes of fish, I observed that the processus ciliaris is not fastened to the joining of the cornea et sclerotis, as in all other animals that I have dissected, so as to hinder the watery humour from going any further backward. For I constantly observed that the humor aqueus may move a good way backward in some, and in others almost as far as the optic nerve.

I have, in as many fish as I could conveniently examine carefully, found a membrane, which covered the tunica cornea, so as not to let any water come to it. This answers the membrane nictitans in fowl, and reaches on all sides to the cutis of the fish to which it is fastened; this is transparent, and pretty thin, and so is also the cornea, if compared to that of the quadrupeds.

I have frequently observed in smaller fowl, that the membrane of the drum was double; for by gently pulling away the membrane lining the tube of the ear, I observed at the bottom of it a transparent membrane, which at first I took to be the membrane of the drum, but on examination I found that this membrane was still entire, and in its proper place. I have sometimes observed this in larger fowl, in a seal, and in some other animals, and am apt to think from a case mentioned in Du Verney's book of the ear, that it is so in men; and if so, it is likely it may be so in most, if not in all animals. The observation was as follows: a person that was deaf for some time died, whose ears Mr. Du Verney examined, in order to find out the cause of his deafness, which he found to be a thick membrane growing in the ear before the drum, which hindered the impulses in the air to be communicated to it. Now I take it to be more likely that the membrane should be double, and that the outward was preternaturally incrassated, than that a membrane should grow in a place where the sides do not touch.

An Account of Books, viz. Πυρετολογια, *Seu Exercitationes de Morbis Universalibus Acutis, Authore Richardo Morton,* Med. D. Regii Collegii Medic. Lond. Socio et Censore. Lond. 8vo.* 1692. N° 199, p. 717.

This treatise is chiefly valuable for the observations on the Peruvian bark, which "met with great opposition when it was first brought into use at London. Some physicians cried it down because it performed the cure (of intermittents) too soon, as they thought; others because they could not reconcile the manner of its operation to their hypotheses and doctrine of humours, declaiming against it as a medicine too hot or too dry, or some way or other not qualified for the purpose." After answering these and other objections, the author declares, that he never, in 25 years observation, saw any ill effect from the cortex, except a temporary deafness, which vanished upon the omission of the medicine. He produces many cases in confirmation of these assertions, and concludes his treatise with a history of the remitting fever, which prevailed epidemically from 1658 to 1692; including also a short mention of the great plague in 1665 and 1666 after which diarrhœas and dysenteries were the prevailing complaints, until 1672; when the measles (more fully described by the author in his second treatise on febrile diseases) became epidemic.

Catalogus Plantarum Horti Academici Argentinensis, in usum Rei Herbariæ Studiosorum, adcurante Marco Mappo Med. Doctore et Professore Seniore, et Archiatro Argentinensi. Argentorati apud Jo. Fredericum Spoor, 1691, 12*mo.* N° 199, p. 729.

A catalogue of plants cultivated in the botanic garden at Strasburg, at the date above-mentioned.

Stephani Chauvini Lexicon Rationale, sive Thesaurus Philosophicus, &c. Rotorod. folio, 1692. N° 199, p. 731.

This philosophical dictionary is contrived in an alphabetical order, and is

* Richard Morton was the son of a clergyman, and was brought up to the church, having studied divinity at Oxford; but he afterwards quitted theology for physic. He died in 1698. Besides the Pyretologia above-mentioned, he wrote a treatise on consumptions, entitled Phthisiologia, which comprehends the different species of tabes as well as pulmonary consumption.

These treatises of his have at various times been reprinted collectively abroad, viz. at Amsterdam. Geneva, and Venice, under the title of Opera omnia.

intended by the author as a key to philosophy, discovering what may be known by the light of nature, expounding the philosophical terms, and their various acceptations according to the ancient and modern opinions. He explains words relating to logic, natural and moral philosophy, especially such as cause the greatest contests in the schools both of the ancients and moderns. Among them are interspersed astronomical, optical, mechanical, chemical, and physico-mathematical words, chiefly such as explain bodies and their several affections; the whole is rendered the more intelligible by several schemes, contained in 30 folio plates, exhibiting to view the several hypotheses, with the structures of many instruments and machines, as barometers, thermometers hygrometers, telescopes and microscopes, made use of by the late inquisitive age, in searching deeper into the nature of bodies.

The Method, Manner, and Order of the Transmutation of Copper into Brass, &c. By Thomas Povey, Esq. F. R. S. N° 200, p. 735.

Calamine is dug out of several mines in the west of England, as about Mendip, &c.* which lie about 20 feet deep. It is burnt or calcined in a kiln or oven, made red-hot; it is then ground to powder, and sifted into the fineness of flour, and mixed with ground charcoal, because the calamine is apt to be clammy, to clod, and not so apt to incorporate. They then put about 7 lb. of calamine into a melting pot, of about a gallon content, and about 5 lb. of the copper uppermost, the calamine must be mixed with as many coals as will fill the pot. This is let down with tongs into a wind-furnace, 8 feet deep, where it remains 11 hours. They cast off not above twice in 24 hours, one furnace holds 8 pots. After melting, it is cast into plates or lumps. 45 lb. of raw calamine produces 30 lb. burnt or calcined. Brass shurff serves instead of so much copper; but this cannot always be procured in quantities, because it is a collection of pieces of old brass, which is usually to be got only in small parcels. The best guns are not made of malleable metal, nor can they be made of pure copper or brass; but it is necessary to put in coarser metals, to make it run closer and sounder, as lead and pot-metal. Bell-metal is copper and tin, pot-metal copper and lead. About 20 lb. of lead is usually put into 100 lb. of pot-metal; but about 6 lb. is sufficient to put into 100 lb. of gun-metal.

The calamine stones were heretofore brought from Poland; but since from hence by the Dutch. The manufacture of brass was privately kept in Germany for many centuries, wherein thousands were employed and well maintained, and some raised themselves to great estates.

* See N° 198 of these Transactions.

Abstracts of two Letters from Mr. David Davies, concerning several Copper Mines; in answer to some Queries proposed by Dr. Lister, S. R. S. N° 200, p. 737.

The thickness of the vein of copper-ore at Gouldscope, in Newlands, was 6 feet.

The 1st work that was found and wrought in by the Dutchmen in Coniston-fells, is called low-work. It has a stulm or shaft, to draw water from the mine. When they ceased working, it was left good, and had been wrought from the day to the evening-end of the said work 40 fathoms, or thereabouts; the seam or vein of copper-ore then was left above three quarters of a yard thick of good ore, which seam or vein went from the evening-end to the morning-end of the said work, and was esteemed an extent of 200 fathoms wrought as the vein went; and was, when left, all near of a breadth or thickness. The copper ore in this work was mixed with some silver, or lead-ore. The 2d work is called white-work or new-work, about 40 fathoms from the first; which was found a little before the works were given over, being wrought about 10 fathoms deep; the seam then left was about 22 inches of good copper ore. The 3d work is called Toung Brow, a little distant from the last, being wrought about 30 fathoms, and the seam about 2 feet thick of like ore. The 4th work is called God's Blessing, or Thurdle-Head, being wrought about 20 fathoms, and is about a mile from the last mine; the thickness of the seam of ore above a yard, when left off; and thought by the workmen much of it to be gold-ore it having been highly prized by their masters at Keswick. The 5th work is called Hen-Cragg, a mile from the last, wrought about 2 fathoms; a small seam, but excellent ore, and expected to prove a large seam. The 6th work is called Sumy-work at Lever's Water, at the water-side; a little above that, Hanch Clocker's-work; a little above that George Towers and William Dixon's work, Bartle Clocker's work; near the last Richard Towers's work, then John Sackloc's work and Hanch Mire's work, being all 7 works, and lie all together, and about a mile from the 5th work abovesaid, and wrought about 10 or 12 fathoms, the seam of ore about 16 inches thick, the stone very soft, the ore very rich, and much of the said ore green, and was very much prized by the head masters at Keswick. The 7th work is called Gray-Cragg-beck, found by William Dixon; wrought but a little, the seam about 18 inches thick, of as good ore as any of the other works, and very hopeful to have a good seam. The 8th work is called John Dixon's work, in Brumfell, then newly found, and wrought about 2 fathoms, the seam about 24 inches thick, and esteemed the best ore, except God's Blessing. The 9th work is called the Wide-Work, or Thomas Hirn's work, wrought about 60 fathoms, and left a seam above 26 inches thick when the work was given over, of very good ore. The 10th work is called Three Kings in Tilburthwait, being 3 works, and wrought above 40 fathoms a-piece, the seam being above 14 inches of very good ore, but a little troubled with water, having no sump to draw it away; but there is fall enough to make one. These are all the works that have been wrought in Coniston-fells. Most of the works here mentioned have small seams near the copper, of a grey sort of ore in small threads.

New places have been discovered lately, and never wrought in, and several found this year, 3 in Torverwel, and about 10 in other places, and all near within two miles of the first work in Coniston-fell, and as hopeful as those that have been wrought in.

Edvardi Luidii apud Oxonienses Cimeliarchæ Ashmoleani, ad Clariss. V. D. Christophorum Hemmer, Epistola; in qua agit de Lapidibus aliquot perpetuâ Figurâ donatis, quos nuperis annis in Oxoniensi et Vicinis Agris adinvenit. N° 200, p. 746.

Observations on Cinnabar and Gunpowder. By Mr. Anth. Van Leuwenhoeck. N° 200, p. 754.

Having exposed native cinnabar to a very strong fire, it soon began to move, many small particles separating themselves from the rest, till they had crept into a cooler place; and notwithstanding the great weight of the cinnabar, several considerable particles, as large as pins' heads, rose up from the fire, and got into cooler places. When the heat was increased, the cinnabar began to evaporate, a black smoke arising, made up of small globules. Examining it when cold, I found several six-sided figures, such as are represented, pl. 12, fig. 6, A, of which some were very regular, others not; they were of different sizes, some of the size of a small sand, others 100 times less; some were of the fig. B. I never found any of these figures in the cinnabar, till it had been exposed to a strong fire, which separated them from it. Some part of the cinnabar that lay next the fire appeared as C D; some particles also were like E, others like F, and some with several points and solid angles, as G H. Besides these figures, there was a blackish matter, which like smoke had been separated from the cinnabar, where I found a great number of exceedingly small globules of quick-silver.

When I burned the cinnabar in the open air, a flame arose very like that of brimstone; but on examination I could not find that the inflammable parts were true brimstone. I then caused the flowers of brimstone to arise, and viewing them, found, among several irregular parts, some globules transparent like oil; and the higher they rose from the fire, the smaller were these globules, till in the end they became undistinguishable. The volatile parts of cinnabar could not be driven very high, though with a great fire, whereas those of brimstone were raised much higher with a small heat. I observed in the brimstone several salt particles, constituted, as I guessed, of many small united globules. For I suppose they were raised in a round figure, which subsiding shoots into angles, especially if they meet with any moisture.

Powdering some cinnabar, I exposed it to the fire as before, and found therein six-sided figures, with some triangular ones, whereof some had one, others more angles broken off; with other different figures with one acute angle, but there were no squares or oblongs. I then poured rain-water on some of this cinnabar that had been raised by the fire without flaming; and when it had stood in the air till part was evaporated, I found a great number of salt particles of a longish figure as are represented fig. 7, I. Among the rest some were pyramidal, constituted on a six-sided basis, and ending in a point like little diamonds. There were salts of some other figures, as oblongs, &c. So that no estimate can be made of these salts. I then poured rain-water on beaten cinnabar, and after some weeks settling, and in part evaporated, I found in it an inconceivable number of salt particles, too small to discern their figures, my best microscope showing them no larger than a sand appears to the naked eye; only I fancied some were sexangular. Boiling some of this water, and evaporating part of it, the salts were to be seen in greater quantity; some of the largest are represented, magnified, as K L L.

I took several clean phials, from 3 to 6 inches long, which I heated, to dry them and rarefy the air; and then put in them one or more of the largest corns of gunpowder, and closed them up to exclude the common air, and placed them in so great a heat that the powder took fire, filling the glass with a white smoke, some of the coal and brimstone sticking to the sides; but putting in more corns, they were carried up much higher, so that I could very distinctly discern the brimstone from the nitre; for it lay so thick in some places, as to exhibit a yellow colour, and might by a good microscope be seen moving circularly in the white smoke, which was the nitre. I then laid the glass along, that the particles of the nitre might be distinct from those of the coal and brimstone; and then I found those particles which before seemed globular, were, when fixed on the sides of the glass, all shot into six-sided salts. Some were like fig. 8, M, N, with others irregular as O, and some of these ended pyramidally like little diamonds. Some of the saltpetre particles, which lay mixed with the others, were long and slender, and looked like little bundles of arrows.

I repeated the experiments with the powder, and immediately after its blowing up, I viewed the glass with a microscope, and could then discern the very sudden change or shooting of the globular particles of the nitre into sexangular salts, and that all at once. The number of these nitrous particles, afforded by one corn of powder, is inconceivably great, besides those of the sulphur and coal. These were best seen when I fired but one corn; for when there were more fired, the greater quantity of nitre blew up so much of the sulphur and coal, that the change and shooting of the salts could not be so well seen. If I fired the powder with heat from below, the coal and sulphur would be blown up; but if with heat from above, but few particles of the coal, and yet fewer of the sulphur, would be forced up.

Next I fired one, two, and three corns of powder in several closed glasses, and suffering them to cool, I opened them, some after 4 or 5 days, and found always compressed air therein, which flew forcibly out. That I might know the quantity of this generated air, I opened some of them after such a manner, that the contained air issued into a bolt-head with a narrow neck, which was filled with water, which, as the air rushed in, was forced out; by which experiment I found the air compressed 8 times more than it was before; or, which is the same thing, when at liberty took up 8 times the room it did before.

I next put one corn of powder in a glass, and closing it up with a very small hole only at the narrow end, which end I placed under the water in the glass vessel as before, and firing the powder, as much air was thereby generated, as forced out 160 grains of water. Now 13 corns of powder weigh but one grain; wherefore multiplying 160 by 13 which makes 2080, we find that gunpowder fired expands itself 2080 times, or takes up so many times the space it did before.*

I observed likewise that the glass wherein the powder was fired would be always filled half full of water immediately after the explosion; the reason of which I conceived to be the great rarefaction of the air, by the heat of the fire and stroke of the powder, which upon cooling takes up less space, and the water enters in to fill up the rest to prevent a vacuity.

From this last observation, I concluded that a bullet cannot be shot with so great a force out of a very long cannon, or other gun, as out of one something

* There seems to be some inaccuracy in this experiment, as to the quantity or numbers, as will appear hereafter by the more accurate experiments of Mr. Hauksbee and Mr. Robins. A great part of the above expansion is to be ascribed to the heat of the inflamed powder.

shorter: and discoursing since with a certain commander upon this subject, he told me he was once present when upon a wager a cannon of 14 feet threw a ball much farther than one of 18 feet.*

As to the reason, how so great a quantity of air comes to be generated, though I thought of several solutions, yet I could not satisfy myself; I sometimes thought that the particles of the air were by the violent motion broken and comminuted into smaller, and so between each particle a much finer substance might be placed, but this did not answer so great an expansion. Upon the whole, I concluded that the greatest improvement that can be made in shooting, is, if possible, so to order the matter, that all, or the greatest part of the powder be fired at once; and when this is effected, a much less quantity will serve than is now used.

To examine yet further this matter of new-made air, I took one grain weight of crabs' eyes, to which I poured wine vinegar, and in 4 hours as much air was generated as filled the space of 44 grains of water; and 3 grains of crabs' eyes produced about 3 times as much. This new-made air kept its expansion for 12 hours that I observed it, whence it appears to have been true air.

Description of the American Tomineius, or Humming Bird, communicated by Nehemiah Grew, M.D. and F.R.S. N° 200, p. 760.

There is nothing in this paper that can justify reprinting it, nor is it possible, from the description given, to determine the particular species meant by the writer.

An Account of Books, viz.—I. Horti Indici Malabarici. Pars Septima, Octava, et Nona; with some Remarks upon them. N° 200, p. 762.

The first 6 tomes being abbreviated in the Philosophical Trans. N° 145, and N° 198; this proceeds with the 7th, 8th and 9th, the productions of the noble Heer Van Rheede. The 7th part treats of scandent and bacciferous shrubs, the greatest part never before noticed or but very imperfectly: as the natsjatam or battavalli, which is the cocculus indicus of our shops, used in ointments or pastes for intoxicating fish, and driving away lice and vermin. It is an evergreen, and grows in sandy places: the leaves are thick-set, of the shape of a heart, and of a bitter taste. The flowers come out in clusters; are monopetalous, with five laciniæ or incisures, all reflected, like those of the night-shade,

* Mr. L. seems to have mistaken the cause of this effect, even if this were generally true, which is not the case. The cause might probably be in the powder; but may sometimes by accident produce the effect, by some friction or resistance within the bore of the gun, as well as other causes from the irregular fligh of cannon balls.

sending forth an odour like that of elder. To these succeed the fruit, resembling grapes, only it is monococ; first green, then white, afterwards red, and lastly, when ripe, black. The Indians beat the whole plant up with ginger, spread it on flannel, and so take off their corns, and soften their feet; sometimes they use only the fumigation of the root burned with buffaloes excrements and the bamboo reed.

Schembra-valli, and vallia-pira pitica seem to be two wild vines, climbing up the trees in thick woods; these, like many other plants in the hot climates, are never divested of leaves and fruit, of which there will be both green and ripe at the same time, always flowering and bearing through the whole year under a kind sun.

Malago-codi is our round, black and white pepper, which are the same only the latter is decorticated. Cattu-tirpali is the long pepper of the Indians, which they chew with calx and the nut taufel, the fruit of a palm named arequa, colouring their spittle with a red tincture.

Cari villandi, a sort of sarsaparilla, for which it is used by the natives of Malabar; it is not unlike the American smilax, called jupecanga by Margrave and Piso, and macapatli by Hermandez and Recchus, who make 4 species of sarsa, all bacciferous.

Mendoni, or the lilium superbum zeylanicum, one of the choicest ornaments of the English and Dutch gardens, growing up to a very high stature.

To these might be added many other rare plants contained in this 7th part; as 4 sorts of Indian ivy, not unlike our trifoliate and quinquefoliate creepers: several exotic night-shades, one resembling our dulcamara; great variety of battatas, or rizophoras, near akin to our potatoes, being also of general use in the kitchens of India. A curious sort of cuscuta or dodder, running up and choking the boughs of trees. A beautiful scandent reed like the rotang, with many others.

The 8th part describes and figures 51 herbaceous and arborescent plants, the greatest part of them pomiferous or leguminous. The first 23 species may be all referred to the pumpions, the coloquintidas, the cucumbers, the balsam apples, the passion flowers or maracocks, of all which there are great varieties in both the Indies.

Modira-caniram contains in its fruit the round flat stone or seed commonly called in our shops the nux vomica: the wood of this tree is said to be the true lignum colubrinum, akin to the caniram of the first tome of this work, which is the famous antidote or specific against the bitings of that Indian serpent, called by the Portuguese cobra copello, whose flat head is marked with the figure of a pair of spectacles. The juice of the leaves, though poisonous, applied externally, drives away the gout, called valvida by the Indians of Malabar.

Among the leguminous plants of this 8th part, the perim-kaku-valli is remarkable, which bears a prodigious large lobe, containing 30 great flat beans, out of each whereof they make entire purses and snuff-boxes: these seeds are called by some cor Sancti Thomæ, and by others fabæ purgatrices. They are so common and plentiful in the islands and continent of India, that whole ships may be loaded with them.

Nai-coranna is the phaseolus surattentis siliquâ hirsutâ pungente, or our cowhage, whose blistering or pricking is soon cured by the herb called thumba. The pod and seed of this kidney-bean are esteemed high provocatives to venery, perhaps for the same reason that cantharides are, and a successful medicine in dropsies. There is another species of this pungent cowhage in Malabar called kaku-valli, akin to, if not the same with, the macouna of Brasil, described by Margrave and Piso.

Schanga Cuspi is the flos clitorius of Breynius, the juice is vomitive, and the root purgative. Konni a sort of abrus or liquorice, whose bean is commended in the hæmorrhoids. Ana-mullu, called by the Portuguese hasticanto and fabas turquesca, remarkable for its many galls or excrescencies full of insects, as also for its strong sharp prickles, with which the Indians bore their ears for the hanging of their various pendants of gold and precious stones. Paeru and katu paeru, called by the Chinese lak goetum, two most delicious phaseoli, or kidney beans, cultivated by the Orientals with extreme diligence, as the best sort of nourishment. Penarvalli, resembling the ahovoi of Thevet in its fruit, hanging down like bandaliers, hence called fruita bandoliera by the Portuguese. This plant is used as a powerful anti-spasmodic by the Indians. The rest of the herbs in this 8th tome may be referred to the halicacabum, aristolochia, clematitis, and the phaseoli, of the last of which there are many species in this volume. The 9th part gives the full history of 87 plants, with their lively icons. The first 17 are all apocynums, some creeping, others scandent, and some arborescent, resembling neriums or oleanders; of these there is a wonderful variety in Malabar, differing in their lanigerous or cotton-like seed vessels, their milky juices are generally corrosive and poisonous.

The todda vaddi of our author is the herba viva of Acosta, which grows in many provinces of Persia; it is esteemed as a great traumatic and lithonthriptic.

Coletta veetla, called by Dr. Herman eryngium zeylanicum febrifugum, floribus luteis. The Indians chew the leaves instead of betel with the faufel or arequa, and give the juice in the apthæ or thrush.

There are also in this 9th volume several sorts of trifoliate and quinquefoliate sinapistrums, many papilionaceous and winged plants, referable to the ferrum equinum, astragalus, onobrychis, to the æschynomenes, herba casta, mimosas, to the sensitive and humble plants, to the genistella tinctoria. Of the crotalaria, there are 5 or 6 different kinds. The rest are akin to the euphrasia, pulmonaria, verbascum, persicaria, digitalis, hedera terrestris, lysimachia. Of the sesamum we have two distinct species in this part, as also of the teucrium, &c. with accurate descriptions, figures, and uses.

2. *A Collection of curious Travels and Voyages, in two Tomes. The first containing Dr. Leonhart Rauwolff's Itinerary into the Eastern Countries, &c. The second taking in many Parts of Greece, Asia Minor, Egypt, Arabia, &c. from the Observations of Monsieur Belon, Mr. Vernon, Dr. Spon, Dr. Smith, Dr. Huntingdon, Mr. Greaves, and others. To which are added three Catalogues of Plants growing in the Levant. By John Ray, S. R. S.* N° 200, p. 768.

A Letter from F. A. Esq. R. S. S. to the Editor, with a Paper of Mr. S. Flower, containing the exact Draughts of several unknown Characters, taken from the Ruins at Persepolis. N° 201, p. 775.

Mr. Flower was agent in Persia for the East India Company, and while on his mission, employed his time partly in coyping and collecting curious specimens of antiquity; but dying suddenly, his papers were dispersed, and mostly lost. One of the remaining papers is here engraven, and contains the forms of 5 or 6 specimens of different literal characters, cut upon stones at Persepolis, in alphabets so old, that they were not legible by the Persians themselves. Two of the specimens were engraven on the breast of two horses, cut out of the mountain of black-marble, near the ancient Persepolis; one of which is said to be Alexander's and the other Rustram's, a famous hero supposed to have lived about the time of Cambyses. Some of the characters seem to be very ancient Greek, some have a resemblance to Hebrew, mixed with other forms; others again resemble the Arabic or Persian character about the 10th or 12th century; one has some affinity with the old Syriac and Arabic; and one is the nail character, or pyramid shape, such as is impressed on some bricks lately found in the neighbouring countries.

Of certain transparent Pebbles, mostly of the Shape of the Ombriæ or Brontiæ. By Dr. Lister. N° 201, p. 778.

These pebbles Dr. Lister calls brontiæ læves pellucidæ, resplendentes, adamantum æmulæ. The figures are taken from certain very clear and transparent stones found in England, of a constant shape. They are called, in some ancient leases of royal mines, rough or mineral pearl; understanding probably by the word pearl, any thing that was resplendent and bright, and particularly figured like a drop of water, which these stones have of themselves naturally, some of them being exactly spherical, others like a half globe, others like a half oval, with an edge raised on the top.

Their natural polish is not to be counterfeited, but very easy to be distinguished by a microscope from the artificial polish of glass and crystals. These stones are of the pebble kind, that is, not to be calcined by simple fire; whereas most other figured stones are calcinable with a very easy fire. They are very hard and solid, and do not consist within of laminæ or flakes, but break every way with great difficulty, and naturally throughout smooth. As they are of a very different nature and texture from all other ombriæ I ever yet saw, and have no vestigia of any spinæ in any part of them, they may be concluded to be stones of their own kind; and though they are in shape like some of the ombriæ, yet they will not come, I think, within the suspicion of having been animal substances.

I am not averse to think, after so manifest and considerable discoveries of this kind, as Augustino Scilla had made in Sicily, that most of the ombriæ have been echini. It is remarkable, there are but two or three echini yet discovered either in ours or the Mediterranean Sea. But of the ombriæ of Europe, besides these present anomalous stones, there are at the least 20 species, figured and described by Aldrovandus, Augustino Scilla, Dr. Plot, &c. and in vast quantities in most counties of England.

An Account of Virginia, and a Voyage thither. By Mr. John Clayton, Rector of Crofton, at Wakefield in Yorkshire. N° 201, p. 781.

The air and temperature of the seasons are much influenced by winds in Virginia, both as to heat and cold, dryness and moisture. The N. and N. W. are very nitrous and piercing, cold and clear, or else stormy. The S. E. and S. hazy and sultry hot; their winter is a fine clear air, and dry, which renders it very pleasant; their frosts are short, but sometimes so very sharp, that it will freeze the rivers over 3 miles broad; it freezes there the hardest, when from a moist S. E., on a sudden the wind passing by the N., a nitrous sharp N. W. blows, not with high gusts, but with a cutting brisk air; and those vales then that seem to be sheltered from the wind, and lie warm, where the air is most stagnant and moist, are frozen the hardest, and seized the soonest, and the fruits are there more subject to blast than where the air has a free motion. Snow falls sometimes in a considerable quantity, but rarely continues there above a day or two; their spring is about a month earlier than in England. May and June the heat increases, and it is much like our summer, being mitigated with gentle breezes, that rise about 9 o'clock and decrease and incline as the sun rises and falls. July and August those breezes cease, and the air becomes stagnant, so that the heat is violent and troublesome. In September the weather usually breaks suddenly and then very considerable rains fall.

There are frequent little sorts of whirl-winds, sometimes not above 2 or 3 yards diameter, sometimes 40, which whisking round in a circle, pass along the earth, according to the motion of the cloud from whence they issue; and as they pass along, with their circular motion they carry aloft the dry leaves into the air, which fall again often in places far remote. I have seen them descend in a calm sun-shine day, as if they had come from the heavens in great showers; so that all the elements seemed filled with them. And I could perceive them to descend from on high as far as I could possibly discern a leaf.

Account of Dr. Burnet's Book, intituled, Archeologiæ Philosophicæ, sive Doctrina Antiqua de Rerum Originibus. Libri duo. Lond. 1692. N° 201, p. 796.

In this treatise the author endeavours to discover what were the sentiments of the ancients concerning the origin or beginnings of this visible world, of which he conceives men in all ages have had a true, if not a divine knowledge, as well as of a divine power, and of the intermediate order, vicissitudes, and ends of all things. And that Pythagoras was not the inventor of the mundane system ascribed to him, but the conveyor only of it from the Orientals, ἀπόρρητα, to the Grecian schools, where yet it received lesser improvement as to particular explications, than it has by the modern inquiries. He endeavours to prove, that most of the ancients held very much the same notion concerning the beginning of things, with that delivered by Moses in the beginning of his writings, with which, he conceives, also that his already published theory is consonant. It was his design also to have written a general body of philosophy, but the sense of his age and approaching death seems to have made him desist, and to satisfy himself with what he has hitherto performed in the preceding books, and in this which he seems to make the seal and consummation of the former.

He divides the whole discourse into two books. In the first he endeavours to discover what were the most ancient doctrines of all nations concerning the beginnings of things, in general. But in the second he endeavours to collect all such passages among them, as seemed most consonant to, and confirming of the doctrines delivered in the first part of his theory, where he had omitted taking notice of them, though in the second part he has intermixed them with the theory throughout.

In the 1st chapter he inquires whence this knowledge is to be brought, which he conceives to be all from the postdiluvian records, which were all conveyed by Noah from the antediluvian, and dispersed among his posterity. Next he inquires where any footsteps of it are to be found among these. And since he finds the ancients divided the nations of the world into four heads, comprehending the whole race of mankind, viz. the Scythians towards the north, the Celti towards the west, the Æthiopians towards the south, and the Indians towards the east, he follows the same order in his inquiry. Among the first he finds no ancient footsteps of philosophy, and he doubts whether ever they had any.

Among the Celti he finds philosophy to have existed, viz. among the Druids and Semnothei. They professed to understand the order and motions of the heavens, and the will of the gods: and that Strabo says, they taught the world's periods by water and fire, and held a transmigration of souls, as the Pythagoreans after them. Of this class were such philosophers as were to be found anciently among the Germans, Britons, Spaniards and Italians; of some of which Strabo affirms, that they had poems of their laws, &c. of 6000 years standing. Those among the Hetrusci, Diodorus Siculus says, studied philosophy: and Plutarch says, they had the notion of the annus magnus, or periods of revolutions, and that the Romans had their sacred rites from them.

Among the Æthiopians the Gymnosophists were famous; but we can find very little information what their philosophy was. They had a very ancient use of letters; and had colleges of priests, who taught philosophy and theology. These are said by Lucian to have been the first astronomers, and to have taught the Ægyptians.

In the 3d chapter he comes to the Orientals or Indians, comprehending all the Asiatics, and some of their neighbours, as the Ægyptians and Greeks. He begins with the most eastern nation, the Seri or Chinese. Celsus and Dionysius call them atheists, because they had no idol-temples or worship; and Barbarians, because they had no commerce with other nations. They have very ancient histories; some that mention the flood and the beginnings of things. Passing from these to the south, he meets with the Brachmans, philosophers celebrated in all ages for their devoting themselves wholly to contemplations; they are of unknown original.

In the 4th chapter he inquires concerning the Assyrians and Babylonians, who were the first empire after the flood: these are accounted the first who cultivated literature and had public schools at Babylon, which continued so till the time of Nebuchadnezzar and Daniel. These learned men were called Chaldeans and Magi; and the chief of them were physiologers and astronomers. To these Pythagoras resorted to learn the motions of the heavens, and the original of the world, says Justin. These were then under the Persians, but the remainders of the Chaldeans and Babylonians. However, nothing is left of their opinions but what Diodorus has hinted, that they believed the matter of the world eternal; but its form, order, and ornament to be constituted by divine providence. And further, that they believed the earth to be of the form of a schiff or tray: he speaks of their antiquity, and of making astronomical observations many ages before Alexander.

In the 5th chapter he speaks of the Persian Magi, who cultivated physiology as well as theology. These had a Theogonia like the Greeks; and from these Pythagoras learnt the origin of the world, and the motions of the heavens. These taught likewise the periods and renovation of the world. The prince of these Magi was Zoroaster, of which he judges there were two. The Persians held the elements, stars, and the heavens to be gods; and worshipped most the sun among the stars, and the fire among the elements: and under the nature of Jupiter they comprehended the whole circuit of heaven. They, as well as the Greeks, Romans, and Hebrews, continually nourished the fire. And the Egyptians, as well as other nations, much honoured it. The Persians supposed this fire to have fallen from heaven; and the Stoicks called it Jupiter, into which all things resolved. Thus their theology was physiology, and all their other rites may be in the same manner resolved.

In the 6th chapter he inquires after what is to be met with among the Arabians and Phœnicians. The first is Job, whom he makes an Arabian, before the time of Moses, who had the knowledge of letters, and of the heavens, and many other parts of the creation, besides that of the worship of the true God. This knowledge lasted to the time of Solomon, as appears by the queen of Sheba: nay, till the birth of Christ, as appears by the Magi that came to worship him. The Zabii he makes to be some of the ancient Arabs, among whom Abraham was bred. These boast of having the religion of Noah. To these, Porphyry says, Pythagoras went: and Pliny affirms the same of Democritus. The Phœnicians he finds very ancient, and early knowing in letters, arithmetic, astronomy, physiology, navigation, foreign trade, and planting. Thales and Zeno were Phœnicians, and to them went divers of the Greek philosophers. Strabo says, that Moschus found the hypothesis of atoms before the Trojan war.

In the 7th chapter he inquires what footsteps of ancient physical learning is to be met with among the Hebrews, and in their Cabala, which he finds to be very little.

From these he passes on, in the 8th chapter, in his search for the ancient learning amongst the Egyptians, where he finds it very ancient, and much celebrated by the Hebrews, who extol both Moses and Solomon, the one for knowing, the other for excelling in it; and by the Greeks, as from whence they had derived their knowledge. Upon a stricter inquiry into it, he finds it to be; first, Geometry, as old as the overflowing of the Nile, on which account it was invented; but he thinks they were only land measurers; and that the Greeks, as Thales and Pythagoras, make it speculative. 2. Astronomy. But this the Chaldeans knew; nor does it appear which were the first; though he conceives both were very ancient, but rude and imperfect, and much improved afterwards by the Greeks. However, he believes they had a true knowledge of the system of the heavens, and understood the motions of the planets, &c. Yet he thinks that they did not understand them so well as to reduce their motions to calculation. They are said also to have cultivated music and physic, but these also without theories, and only by tradition. And though Herodotus says, they had for every disease a particular physician; yet both these were much more improved by the Greeks. They are said by others to be the inventors of chemistry. Lastly, that they were knowing in physics or natural science. They held the earth to be formed out of the confused mass of the four elements, or out of the chaos; and a two-fold destruction of it, one by fire, another by water; that the earth was of an egg form; and what we call the Pythagoric system was the oldest of all others, but kept among their arcana. Their most celebrated philosopher was Hermes Trismegistus. The Egyptians preserved their learning by their priests, who had colleges in several parts of Egypt. They had it inscribed on obelisks, and written in books. The Babylonians had celestial observations for 720 years inscribed on burnt tiles. Democritus transcribed his morals from a Babylonian column. The columns of Hermes in Egypt are famous, on which were inscribed his doctrine. From these the Greeks and Phœnicians had much of their knowledge, and Manetho his sacred history. Ammianus Marcellinus says, these columns were placed in subterraneous vaults, and were there before the flood. Manetho and Josephus assert the same of theirs, both possibly the same. In their books were written their laws, their history, and their philosophy. Some things were communicable, others were arcana, not to be divulged. They had also a two-fold way of writing, one common, another sacred. This was written with hieroglyphics. The Brachmans have not only a different character, but a different language to preserve their philosophy. Pythagoras was not admitted to their secrets, without being first circumcised, and otherwise qualified. The Esseni, and other nations, as Persians, Syrians, and Indians, administered oaths, &c. of secrecy. They further obscured their knowledge by symbols, ænigmas and fables, in which the Greeks also followed them, as did also most other nations in the histories of their gods; of which kind he gives divers instances. Among these are the Mythologies of the Prophets in the Old Testament, and the Parables of Christ in the New.

In the 9th chapter he inquires concerning the Grecian philosophy, of which, that he may the more fully give the history, he produces sufficient proof to show it derived from the Egyptian; not that he denies the Grecians to have much improved several parts of it. That they learned their geometry, astronomy, and arithmetic from the Egyptians, Chaldeans, and Phœnicians, Herodotus, Plato, Aristotle, Diodorus, Strabo, Laertius, Achilles Tatius, and others their own authors testify. It is said, Semiramis, who lived 800 years before the Trojan war, built a high tower in Babylon, on the top of which the astronomers made their observations, when the Greeks had not the use of letters. And Calisthenes sent into Greece from Babylon, celestial observations for 1900 years before Alexander's time. And Epigenes found observations at Babylon for 720 years, and others were brought of 480, as Pliny relates; these were inscribed on baked bricks; whereas the Greek observations began with Hipparchus and Ptolomy. The knowledge of letters was first brought into Greece by Cadmus, not long before the Trojan war. Whereas learning flourished in Assyria, Egypt, Phœnicia, Arabia, Ethiopia, India, and among the Celti, long before that time. Suidas says, that Orpheus held the heavens to be formed of the æther, and the earth out of the chaos, before which he placed time as the measure, but he makes them both coeval; whereas others of the ancients separate them by a multitude of ages. Empedocles makes all the stars to be fires, but Orpheus to be worlds; as the moon was therefore called ἀντίχθον, which Orpheus first asserted habitable, as also that the oval earth was to be destroyed by fire, and then to be renewed.

In the 10th chapter he inquires among the Greek philosophers that succeeded Orpheus, and first among the Ionics. These are the second kind of physiologers who wrote expressly and in prose, not in verse and mythology, as the former. Anaxagoras makes a mind to regulate matter, and move it, and was therefore called Νȣ̃ς. He ranges the four elements by gravity, and is said to have introduced vortices. As for the heavens, the Ionians much promoted the knowledge of them, i. e. they held the heavenly spaces to be æther; the stars, fire; the planets, opake bodies; that the moon had hills and vales, and was habitable, and that it was enlightened by the sun, which was a most pure fire.

He held also the rotation of the earth, as also the annual motion about the middle of the world. To show the reason why it is so difficult to find what were their true thoughts, the author quotes this passage of Plutarch: this doctrine (that is, concerning the heavens) was not celebrated and famous, but hidden and kept secret, and it was discoursed of with great caution among a few, under an oath of secrecy; for philosophisings concerning the heavens would not be endured, because those seemed to restrain and bind up the divine numen to causes without reason, and to blind powers, and to involuntary effects; on which account Protagoras was banished, and Anaxagoras put in irons. Socrates also, for the name of a philosopher, was put to death. Whence the author observes, that in all ages it has been very dangerous for philosophers to speak plain truth among the vulgar.

In the 11th chapter he inquires concerning the doctrine of Pythagoras and the Italic philosophy; where he finds that either Pythagoras wrote nothing, or if he did, even the history of them is lost; so that nothing of his physiology is remaining, save only his theory of the heavens, which is called the Pythagoric system; he placing the sun in the centre, and the earth moving round it; the moon is an antichthone or opposite earth enlightened by the sun, the comets to be above the air, or between that and the planets; that the heavens were fluid æther, and the stars so many worlds.

In the 12th chapter he inquires concerning the opinions of the Eleatic sect, and of the Stoics. This sect was founded by Zenophanes in the times of Anaximander, and consisted of a mixture of various nations and opinions. The doctrine he held was, that there were infinite suns and infinite moons like ours, which he said were habitable, but that they were all eternal. Parmenides held them to be formed out of fire and earth, and men out of clay. They agreed much with the Ionic sect. Leucippus and Democritus were of this sect, who introduced atoms. Leucippus acknowledged the motion of the earth on its axis, and that when the fluid mass settled into a globe, it was covered by a dry skin, which growing thicker, formed the habitable earth; he supposes the axis also at first right, but altered afterward; both which are consonant to the author's theory.

In the 13th chapter the author inquires among the Platonics, Aristotelians, and Epicureans. First, he finds the Platonics, like the Pythagoreans, to be most taken up in high speculations of abstract notions, and in assigning causes of things to numbers and geometrical figures. Thus Plato in his Timæus makes nothing visible but fire, nothing tangible but earth, between these two for their own union are placed air and water. The solid bodies he supposes made up of triangles; fire he makes of pyramids, consisting of four triangles;

the earth of cubes, consisting of six squares, and each square of four triangles; the air of octaedrons, each side divided into six scalenes; then the water of icosaedrons, consisting of twenty triangles, each made up of scalenes. To these he adds a soul, consisting of numbers and proportions.

In the second book the author designs to make use of such testimonies as he can find among the fragments of the most ancient philosophy, to confirm the doctrines of his theory of the earth. And as, in the former book of this present treatise, he has inquired after the ancient doctrines concerning the whole, or the universe, in this he restrains his search after such doctrines as more particularly concern the formation and fabric of the earth only.

On the Food of the Humming Bird. By Dr. N. Grew. N° 202, p. 815.

It is believed the humming bird feeds on some juice he sucks from flowers.* It was supposed for a long while, that the bird of paradise had no legs. Whether may not this bird rather feed on small insects, whereon many birds feed, some whereof lie in the bottom of most flowers, and for which this bird has a bill? whereas a bee that sucks has a siphon or hollow probe. In short, the bird should be opened, and so it will appear either that he has entrails fitted only for liquids, or the same sort of stomachs and guts as other birds, containing the same sort of solid food.

Some Observations made by Mr. Paschal, on the Motions of Diseases, and on the Births and Deaths of Men, and other Animals, in different Parts of the Day and Night. N° 202, p. 815.

Having suspected that the causes of tides at sea exert their power also in other places, though the effects may not be so sensibly perceived on the solid as the fluid parts of this terraqueous globe; for trial hereof I divided the natural day into four senaries of hours; the first consisted of 3 hours before the moon's southing, and 3 after the second of the 6 hours following, and so the third and fourth contained the two remaining quarters of the natural day. I next observed the times of the births and deaths in human creatures, and also in other animals, whether they fell out indifferently in any of these four senaries; and I found none that were born or died a natural death in the first and third senaries, which I shall call first and second tides, but every one either in the second or fourth senaries, which I call the first and second ebbs. I then made observa-

* The tongue of the humming bird consists of a double tube, and appears to be calculated for the conveyance of fluids or juices only; yet in some instances the remains of small insects are said to have been found in the throat. See *Journal de Physique*, 1777.

tions on the motions of diseases, which I could the better do, as I had some in my family visited with agues. Here I found that the tumult of the fits generally lasted all the tiding time, and then went off in gentle kindly sweats in the ebbs. I went on then to take notice of the sex res non naturales, and alterations of the weather, and such accounts as I could meet with of earthquakes and other things; and I have yet met nothing to prevent me from laying down this as a maxim, that motion, vigour, action, strength, &c. appear most, and do best, in the tiding senaries, and that rest, relaxation, decay, dissolution, belong to the ebbing senaries.

Account of a Stone of an extraordinary Size, spontaneously voided through the Urethra by a Woman in Dublin. By Dr. Mullineux. N° 202, p. 817.

Women are made by nature of a more nice composition, and weaklier frame than men, and are therefore liable to many infirmities that men are not the least subject to. Yet in one of the most painful that afflicts the body, the stone in the bladder, they have much the advantage, and are more rarely troubled with it than men. For among the two vast collections of stones, which amount at least to several thousands, kept together in the hospitals at Paris, l'Hotel Dieu, and la Charité, cut out only of such as come thither to be cured, not one in a hundred, I might safely say more, is taken out of a woman. This remarkable difference must proceed from the urinary passage in this sex, being shorter, larger, and more apt to dilate; so that for the most part, when gravel, or a sort of viscous clayey matter, which I take to be the chief cause of the generation of the stone, falls into the bladder, it is suddenly and easy discharged, before it can cohere together and form a stone of any large bulk; which cannot so frequently happen in men, by reason of the narrowness, crookedness, and length of the passage of the urethra.

However it sometimes happens, that even in women, either from a more depending, or less elevated posture than usual, in their bladder, or that the matter forming the stone adheres to some part of its membranes, so that it cannot fall in the urinary passage, till its own size or weight forces it thither, stones of a very considerable bulk are generated. Of this we have lately had here in Dublin a very remarkable example: one Margaret Plunket, alias Weldon, about 60 years of age, on May 29, 1691, voided through the urinary passage, by the help of nature alone, without the use of remedies, or any forcible means whatever, a stone of the shape of fig. 9, pl. 12, somewhat resembling a hard pear, a little pressed or flatted.

Its circumference measured the longest way is $7\frac{3}{10}$ inches; round about the thickest part $5\frac{3}{4}$ inches; its weight at present, according to Troy pound, ℥ii,

℥ii, ℈i, gr. 6, for it has lost considerably both of its first bulk and weight by many little fragments breaking off from the smaller end A, where it is much softer, smoother, whiter, its parts more porous, and so incoherent, that the least force severs them: whereas the larger end, as far as the dotted line across, is of a very different texture, much more close and compact, covered with a yellowish shining crust, rough, granulated, and as hard as the best Portland stone.

It appears that some stones, from their way of generation, must of necessity remain fixed in the bladder; being closely joined and united to the very substance of its membrane, of which sort there are several examples recorded by Schenkius, and other collectors of observations: and I am persuaded this stone I am now describing may be reckoned among them; for about the larger end, there still closely adheres several thin films and carneous filaments, which manifestly show it was formerly united by this part to the membranous substance of the bladder, and that lately by its own weight, or some other accident, it was torn away, and fell into the urethra, through which it was voided; and hence it was that this woman never suspected herself, till very lately, at all troubled with the stone.

For these 3 months past, while it was sticking in the urinary passage, and coming away, she has suffered great pains, and a perpetual strangury, or an involuntary dropping of her water from her; and this infirmity still continues, the largeness of the stone having overstretched the fibres that compose the sphincter of the bladder in its passage through it, whence their tone is so relaxed they have lost all power of retension; and for this reason, I find all women that void stones this way, of any considerable size, are constantly attended with this weakness.

Since my writing the foregoing account, Mr. Thomas Proby, an ingenious surgeon of this town, has lately practised the extraction of such stones entire, or without incision, very successfully, in two remarkable instances. The first instance is the more remarkable, as the girl is very young, and consequently the passage of her urethra strait and small, and as the stone was very long, and considerably large for one of her years. Fig. 10 expresses exactly enough both its shape and size. The child was about 6 years old; for some years past she had been so miserably afflicted with the stone, and a perpetual incontinency of her urine, that her parents at any hazard were willing to attempt relieving her of so violent a pain, and so foul a distemper. In consequence the child being placed in a convenient posture, in a man's lap sitting across a table, with her arms tied down to her legs, by a sort of bandage usual in these cases, the surgeon first passed his catheter into the neck of the bladder, that it might empty itself of all urine, before he inserted his dilating instrument, or his speculum viscæ as it may be called, with which he extended the urethra as much as possible with safety, and without putting the child to extraordinary pain; afterwards by help of a directory and forceps, gently thrust into the bladder, he brought away the stone without any manner of incision, in about 3 or 4 minutes time, from the passing in of his first instrument, and put the patient to so little pain during the operation, that when it was over, and she laid quietly a-bed, she slept without any opiate 7 or 8 hours together, as she had not done for many months before, and is now perfectly well and at ease.

The other instance is a child something older, about 10 years of age, who has been troubled with an involuntary distilling of her urine, and other painful symptoms of the stone, for these 3 or 4 years past; but on June the 12th, 1693, was happily relieved by the extraction of a large stone near the size of a pigeon's egg, after the same manner and method as before described, and with as good success, though not altogether with as quick expedition. The size and shape of it is exhibited in fig. 11.

From these examples we may reasonably conclude, that those of intermediate years as well as childhood and old age, are capable of undergoing this operation of extraction of the stone with safety: and I apprehend it is the only proper and secure way of freeing the female sex from the stone in the bladder, and that such should never be persuaded to undergo the section on any account; since the stone, if it be not of an immoderate size, may be extracted by only dilating the neck of the bladder.

An Account of the Tubera Terræ, or Truffles found at Rushton in Northamptonshire. By Tancred Robinson, M.D. and R.S.S. N° 202, p. 824.

The tubera terræ,* observed lately at Rushton in Northamptonshire, by Mr. Hatton, are the true French truffles, the Italian tartuffi or tartuffole, and the Spanish turmas de tiera, which are not noted by Mr. Ray to be found in our soil. I have seen them thrice as large at Florence, Rome, and Naples, where they eat them as delicious dainties, either fried in slices with butter or oil, salt and pepper; or else out of pickle, and often boiled in their soup.

These observed in England are all included in a studded bark or coat, the tubercules resembling the capsules or seed-vessels of some mallows and alceas, fig. 1, pl. 14, the inward substance is of the consistence of the fleshy part in a young chesnut, of a paste colour, of a rank odour, and unsavoury, streaked

* Lycoperdon Tuber. Lin.

with many white veins or threads; the whole is of a globose figure, though unequal and chinky (fig. 2).

What these truffles are, neither the ancients nor moderns have clearly informed us; some will have them to be callosities, or warts, bred in the earth: others call them subterraneous mushrooms. They are most tender in the spring; though after showers and sultry weather they may be plentifully found in autumn: the wet swells them, and lightning may dispose them to send forth their particular scent so alluring to the swine; for some of the ancients called them ceraunia.

Mr. Hatton observed fibres issuing out of some of these tubera, (as fig. 1) which lay spit deep under ground; so that perhaps they may be plants sui generis, and their furrowed papillæ analogous to, if not real, seed-vessels; for several vegetables bear their seed near the root, as the trifolium subterraneum tricoccum reticulatum flosculis longis albis, most of the arachidnas, and some other pulse which flower above, but seed under ground. As to the truffles lying so deep, that is common to many roots, that shoot up stalks above the earth: to instance only in that lathyrus tuberosus, called commonly chamæbalanus and terræ glandes, in English, pease-earthnut, dug up and eaten by the poor people. The roots of our bulbocastanum, of the umbelliferous tribe, commonly called kepper-nuts, pignuts and gernuts in the north, lie very deep, and fatten hogs, which are very greedy of them. I have often observed the shepherds and boys in Yorkshire digging them up for a delicate dish: perhaps this is the nucula terrestris septentrionalium of Lobel, and the apios of Turner.

Fig. 1, represents one of the tubera terræ whole, the papillæ and fibres being observable. Fig. 2 is the same truffle cut through the middle to show the inside full of whitish veins.

Account of an Earthquake in Sicily: in a Letter from Mr. Martin Hartop at Naples. N° 202, p. 827.

It seems highly probable, that these tremblings of the earth proceed from the same incensed matter, which, finding a way at other times through the Mongibello, breaks out so furiously in smoke and fire; as appears by the tragedy of Catania. The eruptions of these mountains are of two kinds: the one not so violent as much to disturb the adjacent country; and this happens once in two or three months, and lasts three or four days. The other is more furious, and of longer continuance, and is observed at Naples to happen to Mount Vesuvius once in about 80 years. Of these, the last, which was in 1632, was so very violent, that it threw the rocks 3 miles into the air. Now from the burning or not burning of this hill, Naples concludes its safety or danger from earthquakes: for doubtless the matter is continually burning under the mountain; and those vast clouds of smoke which daily issue out at the top, if the cavity happen, by any rock or inward alteration, to be stopped, must deviate through other passages under ground, heaping up continually magazines for a future calamity. Now this combustible matter seems to be nothing but nitre mixed with some other minerals and sulphur: for he that has seen the way of making salt of tartar by deflagration, where an equal quantity of pulverized nitre is mixed, has an exact type of these burning hills: for after each spoonful that is put into the burning crucible, there arises first a black thick smoke, after which the fired mineral boils up, as if it would over-run the top of the crucible.

This I take to be the matter. But in the second place, how this motion of the earth is performed, is not so easily explicable, especially if one considers, that the motion of the earth is not from the perpendicular, but horizontal; as appears by the cracks in the earth, which are now to be found all over Sicily. It is a vibration so quick that it cracks the glass in the windows; and the reciprocations of a lute-string are not more frequent. Now when the vibrations are so quick, and the body moved so large, the motion must be prodigiously violent, and consequently the cause also.

Extract from another Account, concerning the late Earthquake in Sicily. N° 202, p. 830.

The island of Sicily, of 700 miles circuit, and divided into three valleys, began on Friday the 9th of Jan. 1693, to be sensible of the shock in the valley of Mazara: but in the two other valleys of Emone and Noto, the shocks were so terrible, as to throw down some buildings, obliging the inhabitants to seek refuge in the fields, or in the churches. On Sunday following, being the 11th of the same month, the shock became much more terrible and general.

Palermo received damage in most of its buildings, especially in the palace and hospital of St. Bartholomew. The steeple of St. Nicholas, belonging to the Augustines, was ruined, and some injury done to the church.

In Messina, all the buildings of the theatre are shattered, the royal and archbishop's palace, with the seminary, are all cracked. The vast and stately church of the Franciscans broken in many places, and the roof of the vestry fallen: the steeple of the church of the Annunciation thrown down, with the death of the sexton. The top of the spire of the dome cleft, all the other religious houses and public buildings were saved. Many private buildings thrown down, and all the rest obliged to be shored up. A few persons killed.

Troina, Randazzo, Nicosia, cities in the mountains, suffered in their buildings. The first had half of the mother-church destroyed, with the parish church of St. Lucy, and was much damaged in the monasteries. The last had its dome much injured. Castiglione had the castle and many houses thrown down. In Francavilla and Linguagrossa, the greater part of the buildings and some churches. Mascali quite ruined, but not many killed, most of the people being abroad on a procession.

Aidone received considerable damage; two whole quarters, with many of its inhabitants being destroyed. In the quarter of St. Laurence there is not one house standing, and the churches ruined. In that of St. James, the church of the Annunciation and its oratory thrown down, with several other sacred edifices. In the other part of the city, which stands lower, there were not so many houses nor persons lost, yet the church of Pope Leo is quite flat, and the magnificent church of the Dominicans in ruins, with the convent of the Reformati Osservanti, one of the best in the whole province.

Abi Aquilea, commonly called Jaci Reale, situated at the foot of Etna, is almost quite destroyed, and its inhabitants buried in the ruins, with many convents; amongst the rest, the famous one of the Osservanti Reformati. Aci St. Antonio, Aci St. Filippo, S. Gregorio, Pedara Trecastagni, Bonnacorei, Nicolosi, Motta, Mesterbianco, Fenicia, and several other fruitful villages, situated near Mongibello, are destroyed, with all the habitations of the pleasant hills about Cananea, which are now in the dust.

Paterno, about 12 miles from Catania, a populous city at the foot of Mongibello, felt the dreadful effects of the earthquake, losing most of its buildings, all the convents of friars, and a very fine monastery. In the ruins were buried 40 persons. The city of Aderno had the same fate.

Cantabiano Piemonte, in the valley of Emona, Francofonte, Palagonia in the valley of Noto, are little less than wholly levelled, and about 300 persons destroyed; when the Marquis of Francofonte was miraculously saved, by leaping out through the crack in the wall of the falling edifice.

Catania, one of the most ancient and famous cities of the whole kingdom, honoured by the courts of several monarchs, and an episcopal see, even from the time of the apostles, giving place to none for the beauty of its sacred edifices; among which, the dome was the most sumptuous and large in all Sicily, adorned with excellent pictures, and richly furnished, besides having a very high and curiously built steeple. Here were a great many nunneries and monasteries; among the rest, the monastery of the Trinity, and that of St. Benedict, with that prodigy of workmanship the magnificent monastery of St. Nicholas, with its temple, a place famous for several reliques. Next, the

Jesuit's College, the convent of the Minorites, and two of the Dominicans, the beautiful one of Capuchins, the imperial convent of the Carmelites, that of the reformed Minorites, that of the reformed Augustines, with several other friaries, and an infinite number of ancient and modern churches, colleges, and other public buildings, inhabited by about 23000 souls, its nobility also being many and ancient. Learning was here in its glory; the citizens were themselves learned, and lovers of knowledge, assisted with the many privileges granted by the king. The university, where the learned laurel was conferred on the worthy, made this place the Sicilian Athens. This once so famous, now unhappy Catania, had the greatest share in this tragedy. Father Antonio Serrovita, who was to preach at Catania the Lent following, was on his way thither on the 11th; at the distance of a few miles, he observed a black cloud like night, hovering over the city; that there arose from the mouth of Mongibello great spires of flame, which spread themselves all round; that the sea on a sudden began to roar, and raise itself in swelling billows; that there ensued a very great and dreadful explosion, as if all the artillery in the world had been at once discharged; that the birds flew about astonished in the air; that the beasts and cattle in the fields ran crying about affrighted; that his and his companions horses were so startled, that they stood stock still, trembling so that they were forced to alight, which they had no sooner done, than they were lifted from the ground above two palms; and casting his eyes toward Catania, he with amazement saw nothing but a thick cloud of dust in the air. Of this magnificent Catania, all its edifices are levelled with the ground, except the chapel of St. Agatha, the Rotunda, the castle of Ursino, the walls that encompassed it, and a few mean houses. There was a very great destruction of the inhabitants buried in the ruins of the bishop's palace; the steeple and dome, where most of the city affrighted with Friday's earthquake, were assembled together to carry the reliques of St. Agatha in procession. Many of the nobility were saved under the chapel of the saint, and some of the clergy. The number of the dead was about 15000; for though the people had staid in the fields all the Saturday, yet the solemnity obliged them to be in the city on the Sunday, to pay their devotions at the procession. Of the Benedictines about 25 were killed in the choir, of the Jesuits 21, of the Conventuals 11, the number of the Dominicans is not known; the Carmelites were all buried, except one, as they went in procession; and so were the greater part of the other religious orders, and of the nuns few were saved. This was the tragedy of Catania, which was accompanied with dreadful lightnings and thunder from Heaven, and with deluges of rain; and in the ruins were heard nothing but cries, shrieks, and dying groans. On the heaps of stones we may now write, here was Catania.

Lentini, a very ancient city, and for a long time an episcopal see, &c. felt that shock on the 9th with such violence, as threw down and ruined the greater part of its buildings; among which was the ancient convent of Minorites; the Royal Convent, so called from the tomb of one of our queens buried there, under the ruins of which 4 religious were buried; the rest escaped miraculously. But the last earthquake, on the 11th, laid in the dust the remainder of the city, destroying also about 4000 people, who returned thither, after the first shock, to take care of their goods. So that the city is all shattered to pieces, not one house left standing.

Carlentine, a modern city, being as a citadel dependent on Lentine, had the same fate. The beautiful castle of Licodia all ruined, with the Marchioness of Martini and all her children buried therein.—Bizrini, a city of rich inhabitants, is levelled with the ground, with the death of many people; part by the first, and the rest by the last day's earthquake. Sortino and Cassero are quite demolished; in the first about 3000 perished, and in the other a very great number.

Agosta, a trading town, built on an island in a large bay, which makes a capacious port, was all blown up into the air; for besides the damage of the earthquake, there was a great quantity of powder in the castle, which took fire, and killed several of the citizens, that had escaped into the fields, with the stones of the buildings: here perished about 3000. The enraged sea grew terribly boisterous, and tempestuously beat against the walls of the Dominican convent with such fury, that some galleys belonging to the Knights of Malta scarcely escaped shipwreck in the port. In fine, luctus ubique pavor, et plurima mortis imago. The country of Mililli, in the duchy of Montalto, felt the same fate, with the destruction of the inhabitants.

Syracuse, famous of old, an episcopal see; in our time like the phœnix arising from the ashes, standing on a peninsula, by art made an island, having a bridge to the main land; strengthened with a modern fortification, sufficiently populous by reason of its convenient situation for trade, full of nobility, and beautified with churches, convents, monasteries, and palaces, now mourns in ruins. It was sensible of Friday's, but all shaken to pieces by the Sunday's earthquake, with the loss of many thousand persons. Most of the nobility saved themselves by a timely flight. Of the religious not many perished. Scarcely a village in the whole diocese is left: confusion reigns every where; and the misery is increased by the want of food, caused by the granaries and mills being destroyed.

Laferla, Palazzuolo, and Busceni lie in ruins, with many inhabitants destroyed.—Spacaforno, a populous place, situated near the sea, which washes the foot of the promontory Pachino, has lost all its buildings: here they reckoned about 2000 dead.—Giarratana with its fall killed most of the inhabitants. The marquis himself, with his wife and three children, escaping on Friday, were on Sunday buried in the ruins: the marquis and his children were taken out alive to bewail the loss of his lady.—Melitello in the valley of Noto is shaken to pieces, the churches and chief buildings levelled with the ground, and the religious orders all turned out in the open air, or under huts and cabins.—Occhiula escaped not the common calamity.—Mineo, an ancient city, is now no more; and the greater part of the citizens and religious.

Caltagirone, a city conspicuous for its senate and nobility, in this universal calamity suffered the total ruin of its proud edifices: as the principal church with its lofty steeple or spire, the famous college of St. Julian, the temple of St. George, the parish church of St. James, admired for the pictures of Epiphanius, the chapel only remaining, with the image and reliques of the saint. The temple of the Conventuals thrown down, the famous bridge, that joins the convent to the town, shattered to pieces, and the dormitories not to be occupied; the famous convent of St. Bonaventure, the fall of whose temple and spire was the destruction of the lower buildings; the college of the Jesuits and the steeple of that noble church are quite ruined. The Carmelites, Dominicans, Augustines, Crouched Friars, &c. are all without churches and convents. The monasteries of St. Gregory, St. Chiara, St. Salvator, and St. Stephen, with a conservatory of orphans, are all shaken down. In fine, the senate-house, adorned with most curious statues, and all the other buildings, are either fallen, or threaten a sudden ruin. In these desolations, about 1000 people were lost.

Modica, a populous place, and chief of the seniority of the admiral of Castile, has its buildings and famous castle laid in the dust. Signor Abbot Frederick, the procurator general, saved himself in the college of the Jesuits; the cities of Ragusa, Sicily, and Chiaramonte had the same misfortune.—Comiso suffered much in its buildings, though but few persons killed. The convent is down, but the church stands.—Noto, an ancient city, full of nobility and fine buildings, convents, and monasteries, is all ruined; the convents of the Dominicans, Conventuals, Reformati, Carmelites, and Capuchins, which was indeed a wondrous fabric, are all torn to pieces. The church of the Crucifixion, the dome, and all the nunneries are down, with the deaths of many citizens and nobles.

To conclude, there is not a corner in all the valley of N·to that is not wholly ruined, and for the most part with a dreadful slaughter of the people. The southern coasts, as Licati, Terra Nova, and Gircuti, have suffered damage in their buildings. And all the castles of the valley of Emone near Mongibello are cracked and broken, or thrown down.

Extract of a Letter from Mr. Anth. Van Leuwenhoeck, containing several Observations on the Texture of the Bones of Animals compared with that of Wood; also on the Bark of Trees: on the little Scales found on the Cuticula, &c. N° 202, p. 838.

Some years since I conceived the bone to be constituted of globules; but finding my mistake, I retracted that opinion; for what I then took for globules was the tops of the tubes or cylinders, of which the bone is composed. Continuing my endeavours, at length I found plainly, in the thigh bone of an ox, that it consisted of four sorts or sizes of tubes, some of which are so very small and close united, as not easily to be discerned in a bone cut smoothly across, nothing but globules appearing; but when it is broken, some shivers are separated, in which these tubuli may be seen.

Another sort of these tubuli, some of which are 6 times larger than the former, are yet hard to be discovered; for though the knife be very sharp, yet by reason of the hardness of the bone, many particles of them are broken and squeezed together, so as the mouths of the little tubes are closed up. A third sort, much larger than the former, had also their mouths scarcely discoverable; but I found them placed in such order, as convinced me, that the ring of these tubuli was the augmentation of the bone, as I had formerly discovered it to be in wood, especially when I saw, at a little distance, another circle or ring of tubuli. A forth sort exceeded the former very much, and were fewer; so that in the space of three or four sands I scarcely found one of them.

Besides the above-mentioned four sorts of tubes, running the length ways of the bone, I sometimes imagined I saw some in a contrary situation, which seemed to proceed from the middle of the bone, and terminate at the circumference; and that these were of two sizes. Some of these seemed as if perforated by those that run lengthwise. I suppose the periosteum is mostly constituted and nourished by these; especially as we see the same in trees, in which the bark is formed by the transverse fibres that run from the centre, passing between the direct ones. I conceive that the membranes surrounding the bones have their increase from some vessels proceeding from the cavity of the bone of the circumference, where they are dilated into that thin soft skin, defending the bone, as the bark does the tree.

I know many believe the origin and nourishment of the bark is from the root;

but if it were so, we should find the parts of the bark near the root larger, and ramified into smaller and smaller as they run higher, as the arteries and nerves are, the farther they go from the heart and brain; whereas there is no difference between the vessels in the bark of the root and trunk; besides, the vessels of the bark of several trees, as the birch, cherry, peach, &c. run not upwards, as they do in the ash, oak, elm, nut, apple, pear, &c. but circularly round the superficies of the tree. And all bark having the vessels running upwards grows thicker as the tree increases, the outside cracking, grows dead, and sticks to the young bark beneath, which is the only living part of the bark. The contrary is evident in those barks whose vessels run round the tree; for as the tree increases, the vessels not being able to stretch nor separate from each other must necessarily break asunder; so that the whole bark is easily separated and falls off from the new. Wherefore such trees have always a very thin bark, as is most evident in the birch tree.

And as we said of the bark, that it is produced and nourished from the trunk of the tree, so is it in the production of the skin of animals, which is covered over with the scarf-skin, consisting of scaly particles; for having examined the skin of many animals, it seemed to me to be formed by the wondrous interweaving of all the extremities of the vessels that proceed to the extreme parts of the body; from the ends of which a certain matter issues out, forming the scales: the extremities terminating at those scales which stick fast to the vessels, till new ones displace them.

These observations brought me to examine again the scales that cover our bodies. In consequence I find each scale of our body is composed of a great company of vessels interwoven together, after the same manner as the scales of fish. Provident nature, as I have often found, performing her operations usually after the same method. Examining these often, I always found a clear spot in the middle, standing up above the rest of the scale, whence I concluded, that the scales, not only of the mouth but the whole body, were composed, as those of fishes, of vessels, proceeding to this clear part, and nourishing the scales which grow from thence. Now 250 scales are covered by one sand, suppose then every scale to consist of 500 vessels, then will the moisture in the space of a sand be thrust out at 125000 several little pores in the scales, not reckoning the mouths of the vessels between the scales.

Concerning the Parallax of the fixed Stars, in Reference to the Earth's Annual Orbit. By Dr. John Wallis. N° 202, p. 844.

Galileo complains that the parallax of the stars has not been attempted to be observed with such diligence as he could wish, and perhaps there is still the same cause of complaint. I know that Dr. Hook and Mr. Flamsteed have attempted somewhat that way, but have desisted before they came to any thing of certainty.

Galileo has suggested several things towards it. As to the times of observation; that it should be when the sun or earth are in the tropics, or nearly so; because at those times, if at any, will be the greatest difference in their meridional altitude. As to the stars to be observed: that they should be such as are as near as may be to the pole of the ecliptic; for such as are in the plane of it, or nearly so, though they may be sometimes nearer, sometimes farther from us, which might somewhat alter their apparent magnitude, if it were at all observable, yet it would little or nothing alter the parallactic angle. Galileo also observes, that in a business so nice, the ordinary instruments of observation might be insufficient for this purpose; and he proposes, that by the side of some edifice or mountain, at some miles distance, the setting of some noted star, as that of Lucida Lyræ, might be observed at those different times of the year, which might be equivalent to an instrument whose radius is so large. This might be a good expedient if practicable; but I doubt the density of our atmosphere is so great, that it will be hard to discern a star just at the horizon, or even within some few degrees of it, and that the refraction would be there so great, and so uncertain, as not to answer so curious an observation.

What occurred to me on these considerations was this, that some circumpolar stars, nearer to the pole of the equator than our zenith, and not far from the pole of the zodiac, should be made choice of for this purpose. And in case the meridional altitude be discernibly different at different times, so will also be their utmost east and west azimuth, which may be better observed than their rising or setting; and this will be not obnoxious to the refraction, as the meridional altitude is, and we may here have choice of stars for the purpose, which in observations from the bottom of a well we cannot have, being there confined to those only which pass very near our zenith, though very small stars.

I would then take it for granted, as a thing at least very probable, that the fixed stars are not all at the same distance from us; but the distance of some vastly greater than of others, and consequently, though the parallax of the more remote, may be undiscernible, it may perhaps be discernible in those that are nearer to us. And those we may reasonably guess to be nearest to us, which appear largest and brightest, as are those of the first and second magnitudes; and there are, at least of the second magnitude, pretty many not far from the pole of the ecliptic, as that in particular in the shoulder of the Lesser Bear; and in case we fail in one, we may try again and again on some others; which may chance to be nearer to us than what we try at first. And stars of this size may be observed by a moderate telescope, even in the day-time, especially when we know just where to look for them.

The manner of observation I conceive may be thus; having first made choice of the star we mean to observe, and having then considered where such star is to be seen in its greatest east or west azimuth; it may be then convenient to fix very firm and steadily, on some tower, steeple, or other high edifice, in a convenient situation, a good telescopic object-glass, in such position as may be proper for viewing that star; and at a due distance from it, near the ground, build some little stone-wall, or such like place, on which to fix the eye-glass so as to answer that object-glass; and having so adjusted it, as through both to see that star in its desired station, which may best be done while the star is to be seen by night in such situation, near the time of one of the solstices; let it be there fixed so firmly, as not to be disturbed, and the place so secured, as that none may come to disorder it, and care be taken to defend both the glasses, so as not to be endangered by wind and weather. This glass being once fixed, and a micrometer fitted to it, so as to have its threads perpendicular to the horizon, to avoid any inconvenience which might arise from diversity of refraction, if there be any, the star may then be viewed from time to time, for the following year or longer, to see if any change of azimuth can be observed.

The reason why I chiefly recommend, as a convenient star for this purpose, the shoulder of the Lesser Bear, is that there is adhering to it a very small star, which the Arabs call Alcor, (of which they have a proverbial saying, when they would describe a sharp-sighted man, that "he can discern the rider on the middle horse of the Wain;" and of one who pretends to see small things but overlooks much greater, that "he sees Alcor, but not the full moon,") and which Hevelius finds to be distant from it about 9′ and 5 or 10″: so that, besides the advantage of discovering the parallax of the greater star, if discernible, the difference of parallax of that and of the lesser star, being both within the reach of a micrometer, may do the business as well: for if that of the greater star be discernible, but that of the lesser be either not, or less descernible; their different distances from each other, at different times of the year, may perhaps, without farther apparatus, be discerned by a good telescope of a competent length, furnished with a micrometer, if carefully preserved from being disordered in the intervals of the observations; and discover at once, both that there is a parallax, and that the fixed stars are at different distances from us. And here my meaning is not that the instrument or micrometer should be removed for the observing of the lesser star; but that, when the azimuth of the greater star is taken, by a micrometer consisting of several fine threads parallel and transverse, may at the same time be observed the distance of the two stars from each other, in that position, both being at once within the reach of the micrometer; which distance, the instrument remaining unmoved, if it be found at different times of the year not to be the same, will prove that there is a different parallax of these two stars.

This latter part of the observation, viz. of their different distances at different times, I suggest as more easily practicable, though not so nice as the former: for it may be done, I think, without any further apparatus there than a good telescope of ordinary form, furnished with a micrometer, carefully kept unvaried during the interval of the observations. And if this part only of the observation, without the other, be pursued, it matters not though the two observations near the two solstices be, one at the eastern, the other at the western azimuth, whereby both may be taken in the night-time; for the distance must at both azimuths be the same. If, after observing the azimuth of the greater star, it be necessary to move the micrometer for measuring its distance from Alcor, that may be done another night, and it is not necessary to be done at one observation, for that distance cannot be discernibly varied in a night or two.

An Account of a Book: viz.—Synopsis Methodica Animalium Quadrupedum, et Serpentini Generis, Auth. Joanne Raio, S. R. S. N° 202, p. 849.

The design of this present work, is to reduce all quadrupeds into the most proper and natural method, for the more easily comprehending and remembering them; and to add to the several species short characteristic notes, by which they may be distinguished from others of the same kind.

First of all, there is proposed the most exact division of all animals in general, which is first into sanguineous and exsanguineous. Under the exsanguineous, or such as want that red liquor we call blood, are comprehended, 1. All crustaceous fishes, crabs, lobsters, shrimps. 2. All testaceous or shell fishes, as cockles, muscles, oysters, scallops, periwinkles, whilks, &c. as also all land shell-snails. 3. Those called mollia, or soft-fishes, as pulps, cuttle-fishes, naked snails, &c. 4. Insects of all sorts.

The sanguineous kind are divided into such as breathe by lungs, and such as breathe by gills, of which kind are all sanguineous fishes, except the whale-kind. Such as breathe by lungs are divided into those that have but one ventricle in the heart, and such as have two ventricles. Of the former kind, are those called oviparous quadrupeds and serpents.

The latter are again subdivided into viviparous, or such as bring forth living young, and oviparous, or such as lay eggs, which are birds or feathered fowl.

Lastly, the viviparous are divided into aquatic, or the whale-kind, and terrestrial (under which the author comprehends the amphibious) or quadrupeds; although all viviparous animals of this kind are not four-footed, there being one exception, viz. the manati or sea-cow, which has but two feet. But all of them without exception are hairy, and so may be distinguished from the whale-kind; all of which have a smooth skin.

Then follows a division and table of viviparous quadrupeds or viviparous hairy animals: which are either hoofed or clawed. The hoofed are either whole-footed, or whole-hoofed, as the horse and ass; or cloven-footed. The cloven-hoofed are either bisulc, such as have the hoof divided into two parts, as the ox, sheep, and goat, or quadrisulc, which have the hoof divided into four. The former are either ruminant, such as chew the cud; or non-ruminant, viz. the hog-kind. The ruminant are divided in respect of their horns, of which there are three kinds. 1. The beef-kind, which have four teats. 2. The sheep-kind, which bear wool, have but two teats, and have wreathed horns. 3. The goat-kind, which have straighter horns, and are covered with hair, instead of wool; and such as have solid, branched, and diciduous horns, as the deer-kind. The clawed have the foot divided either into two claws, viz. the camel-kind, or into more, called therefore multifidous. The multifidous are divided into such as have broad nails, and a human shape, as apes and monkies, and such as have narrower and pointed nails. These latter, in respect of their teeth, are divided into such as have many fore teeth or cutters in each jaw; and such as have but two, all which last are herbivorous or phytivorous animals, and from the most known creature of this tribe called the hare-kind.

Those that have more fore teeth or cutters are distinguished into greater, which have either a shorter snout, and round or square head; or a longer snout and head: these from their likeness to the most known animals of each kind, the cat and the dog, we call the former the cat-kind, and the latter the dog-kind: to the cat-kind belong the lion, tiger, leopard, ounce, &c. to the dog-kind belong the wolf, fox, badger, otter, &c. and lesser, which from their long slender bodies, like to the weazel, we call the vermine or weazel-kind. To the cat-kind are referred some anomalous animals, viz. the bat, and the American creature called ai or sloth. To the dog-kind, from their having a longer snout, are referred the urchin or hedge-hog, the tatou or armadillo, the mole, the shrew-mouse or erd, i. e. earth-shrew, the tamandua or antbear; which has no teeth at all.

Then are briefly described the several species of all these kinds in order: and to some of them large anatomical descriptions and observations are added.

For the more easy and clear comprehension and distinction of the several kinds of animals, Mr. Ray gives two schemes or tables of them: the first a general one of all animals; the second a particular one of quadrupeds; as below.

Animals are either,

- Sanguineous, that is such as have blood, which breathe either by
  - Lungs,* having either
    - Two ventricles in their hearts;† and those either
      - Viviparous,
        - Aquatic, as the whale kind.
        - Terrestrial, as quadrupeds, or in general all hairy animals.
      - Oviparous, as birds, which have immoveable and perforated lungs.
    - But one ventricle in their hearts, as frogs, tortoises, serpents.
  - Gills, as all sanguineous fishes except the whale kind.
- Exsanguineous, or without blood, which may be divided into
  - The greater, either
    - Naked
      - Terrestrial, as naked snails.
      - Aquatic, as the poulp, or pour-contrel, polypus; the cuttle-fish, sepia; the sleave or ink-fish, lolligo.
    - Covered, with a tegument, either
      - Crustaceous, as lobsters and crab-fish.
      - Testaceous, whether univalve, as limpets, or bivalve, as oysters, muscles, cockles; or turbinate, as periwinkles, snails, &c.
  - The lesser, as insects of all sorts.

* Gills and lungs differ, as gills are only as it were inverted lungs; the air being drawn inwardly into the lungs, whereas it only outwardly touches or slides by the gills.—Orig.

† Animals that have two ventricles in their hearts use a frequent and constant inspiration and expiration, taking in and breathing out the air. But those that have but one ventricle, use no such frequent inspiration and expiration; but draw in and retain the air a considerable time in their lungs, before they breathe it out again.—Orig.

A Table of Viviparous hairy Animals or Quadrupeds.

Viviparous quadrupeds or viviparous hairy animals are either

- Hoofed, which are either
  - Whole-footed or hoofed, as the horse and ass.
  - Cloven-footed, having the hoof divided into
    - Two principal parts, called bisulca, either
      - Such as chew not the cud; as all sorts of swine.
      - Ruminant, or such as chew the cud, divided into
        - Such as have perpetual and hollow horns, of which there are three kinds, viz. 1. The beef-kind; 2. The sheep-kind; 3. The goat-kind;
        - Such as have solid, branched, and deciduous horns; as the deer-kind.
    - Four parts, which we call quadrisulca; as the rhinoceros and Hippopotamus.
- Clawed, or digitata, having the foot divided into
  - Two parts or toes, having two nails, as the camel-kind;
  - Many toes or claws; either
    - Undivided, sticking all together, covered with a common skin, the tips of the toes only peeping out or appearing outwardly, and being covered with nails, as the elephant.
    - Divided from each other a good way down, which have either
      - Broad nails and a human shape, as apes and monkies;
      - Narrower and more pointed nails; and in which respect of their teeth are divided into such as have
        - Many fore teeth or cutters in each jaw; either
          - The greater, which have
            - A shorter snout and rounder head, which we call the cat-kind;
            - A longer snout and head, which we call the dog-kind;
          - The lesser, with a long slender body, and shorter legs, which we call the vermine or weazel-kind.
        - Only two large and remarkable fore teeth, all which are phytivorous; these we call the hare-kind.

For all which, and the many subordinate species under each genus; together with short histories of them, the book itself must be consulted.

An unusual Storm of Hail at Lisle in Flanders. N° 203, p. 858.

There fell in this city, May 25, N. S. 1686, hail-stones which weighed from a quarter of a pound to a pound weight and more. One among the rest was observed to contain a dark brown matter in the middle, and being thrown into the fire, it gave a very great report. Others were transparent, which melted before the fire immediately. This storm passed over the citadel and town, and left not a whole glass in the windows on the windward side. The trees were broken, and some beat down, and the partridges and hares killed in abundance.

Observation of the Solar Eclipse, July 12, 1684. By S. Domin. Gulielmini, at Bolona. N° 203, p. 858. *Translated from the Latin.*

In this observation, S. Gulielmini was assisted by S. Donelli, Joanetti, Manzi and Vanotti, Malisardi and Ferrari. The principal circumstances were the following: the beginning being obscured by clouds, at

3^h 34^m 0^s.. the digits eclipsed were 2^d 30′.
4 2 0 .. 6 digits.
4 27 44 .. greatest obscuration 7_d 12′.
4 51 30 .. 6 digits.
5 27 40 .. the end, very exactly.

In this eclipse it was remarkable, that though more than half the sun's diameter was eclipsed, at the middle of the eclipse, yet the defect of light in the atmosphere seemed to be very small.

Observations and Experiments on the Animalcules in Pepper-water, &c. By Sir Edm. King, M.D. and S.R.S.* N° 203, p. 861.

Having steeped oats in rain-water for some days, and viewing them with the naked eye, I observed a substance like that usually called a mother on other liquors, and laying the quantity of a small pin's head on the object plate of a good microscope, I could very easily and plainly discern 7 or 8 sorts of animalcules, of different sizes and shapes, swimming in this substance.

The thin scum on pepper-water, which resembled flakes of salt on some sorts of human urine, applied in the same manner to the object plate of the microscope, was only clusters of animalcules, which had liquid matter enough to swim in, and it was abmirable to see their numbers, motions, variety, and minuteness.

In a decoction of herbs, that was strained, were seen creatures like little eels, sharp at both ends, and with a wriggling motion like eels. These small creatures resemble the nature of fish in several respects; as first, they flock and lie close together, as if they were in shoals, like carps in a pond. Then they will follow their liquor, to act in, to its last particle; then they will seem to struggle for want of it, till their strength fails them; lastly, after a minute or less, they will seem dead upon the object plate. Thus they will lie as if they were dead for half an hour, or more; after which putting a little water

* The animalcules described in this paper are referrible to the Linnæan genus chaos.

to them, in half a minute they will begin to move themselves again, and by degrees begin to swim, faintly and feebly at first, and then, recovering their strength again, perform their brisk motions as vigorous as ever. These animalcula chuse for the most part the top of the liquor, probably for the sake of the air.

Now as a further testimony that they are animalcules, which some doubt of, I have noted the following observations: If you take a fine needle, and put the point into spirit of vitriol, and prick a small drop with it, these minute animals will, from moving briskly about, spread themselves flat, and appear to fall down dead. Dissolved salt produces the same effect upon them, with this difference, that instead of becoming flat, as those with spirit of vitriol did, they shrink into an oblong round form. Tincture of salt of tartar, used the same way, kills them more immediately; but yet they will seem to be first so sick, as appears by convulsive motions, that they grow faint and languid apace, and then fall down dead to the bottom, without any change in their shape. Ink kills them as soon as spirit of vitriol, but makes them seem to shrink various ways. Fresh blood kills them almost as soon as spirit of vitriol. Urine kills them also in a little time, though not so soon. Sugar, dissolved like salt, kills them also, if used in the same manner; and with it some die flat, and othere round. Sack will kill them, but not so speedily as the others liquors.

The Manner of making and tempering Steel, with a Conjecture at the Method the Ancients used to steel their Picks, for cutting Porhyry. By Martin Lister, M.D. and S.R.S. N° 203, p. 865.

In the Philosophical Transactions, N° 93,* amongst other desiderata and queries, are these; to endeavour to retrieve the art of hardening and tempering steel for cutting porphyry, &c. We know not which way to rough-hew stones of that untractable hardness. Those stupendous monuments of antiquity, the Egyptian obelisks, are of porphyry, and most of them curiously carved with a vast number of figures, which was one way of writing among the ancient Egyptians. These figures show the facility that nation had of engraving in porphyry; a stone which no tool will now touch, nor any thing less than emery or diamond powder affect. Mr. Ray assures us, that all the obelisks at Rome, engraven with hieroglyphics, are of the same kind of stone, viz. a marble † of a mixed colour, red and white, very hard, and without suffering the least injury by the weather for many ages. There is certainly something lost in this age, as to the manner of steeling tools. As for the moderns, there is great abuse in this

* Vol. ii. pp. 59 and 60 of these Abridgments. † Not marble, but either porphyry or red granite.

manufacture, and the processes now used by most nations are fraudulent, and a poisoning of iron, by certain mineral salts, rather than a true making of steel. The most ancient account is that given by Aristotle, Meteorologicor. l. 4, c. 6, which yet is very obscure and imperfect; the passage is this; "Wrought iron itself may be cast so as to be made liquid, and to harden again; and thus it is they are wont to make steel; for the scoria of iron subsides, and is purged off by the bottom: and when it has been often defecated and made clean, this is steel. But this they do not often, because of the great waste, and because it loses much weight in refining; but iron is so much the more excellent the less recrement it has." This account is a little confused, and not easily understood; it is indeed true, that iron is still better the more it is purged; so, in our furnaces in England, those bars which are wrought out of a loop, taken up out of the finery hearth, or second forge, are much better iron than those made in the bloomery, or first hearth, because more purged of the dross, and accordingly fetch a double price. It is also true, that even wrought iron may be melted as often as you please. Again, iron, as often as it is melted and purged, loses much of its weight. But after all, iron of itself, how often soever it is purged and refined, will never become seel; yet of it thus purged the best steel doubtless may be made. And this is the most favourable construction that can be put on this passage of Aristotle.

We shall now give the best account how true steel is made at this day, waving all fraudulent processes. The manner is this, faithfully described by Agricola, De Re Metallica, lib. 9. And to confirm its antiquity, this way of making steel is by Kircher said to be now in use in the island of Elba, a place famous from all ages, even from the times of the Romans, for that metal alone, down to our days. "Make choice of iron which is apt to melt, and yet hard, and may easily be wrought with the hammer; for although iron, which is made of vitriolic ore, may melt, yet it is soft, or brittle, or eager. Heat a parcel of such iron red-hot, and cut it into small pieces, and then mix it with a sort of stone which easily melts: then set in the smith's forge or hearth, a crucible, or dish of crucible metal, a foot and a half board, and a foot deep, fill the dish with good charcoal, and compass the dish about with loose stones, to keep in the mixture of stone and pieces of iron. As soon as the coal is thoroughly kindled, and the dish red-hot, give the blast, and let the workman put on, by little and little, all the mixture of iron and stone he designs. When it is melted, let him thrust into the middle of it 3 or 4, or more pieces of iron, and boil them therein 5 or 6 hours, with a brisk fire; and putting in his rod, let him often stir the melted iron, that the pieces may imbibe the smaller particles of the melted iron, which particles consume and thin the grosser ones of the iron pieces, acting like a ferment to them, and making them tender. Let the workman now take one of the pieces out of the fire, and put it under the great hammer, to be drawn out into bars, and wrought; and then, hot as it is, plunge it into cold water. Thus tempered, let him again work it on the anvil, and break it; and viewing the fragments, let him consider whether it look like iron in any part of it, or be wholly condensed, and turned into steel. Then let the pieces be all wrought into bars, which done, give a fresh blast to the mixture, adding a little fresh matter to it, instead of that which had been drunk up by the pieces of iron; which will refresh and strengthen the remainder, and make still purer the pieces of iron again put into the dish; every which piece, let him, as soon as it is red-hot, beat into a bar on the anvil, and cast it, hot as it is, into cold water. And thus iron is made into steel, which is much harder and whiter than iron."*

There is but one place that I know of, which may give us any sight into the inquiry concerning our tools, and that is in Pliny, lib. 34, c. 14, where speaking of iron, he says, furnacum maxima differentia est: in iis equidem nucleus ferri excoquitur ad indurandum aliter, alioque modo ad densandas incudes malleorumve rostra. From this passage it should seem, that the ancients had one way to make steel, and another way to harden or temper their tools, particularly such at picks and anvils. It is also plain, that nucleus ferri was melted down in both. Again, the difference was in the furnaces, that is, in the manner of ordering the iron to be made into steel, or for the extraordinary hardening of the heads and tips of tools, and not in the matter of which they were made, for both were done by boiling them in molten iron. It cannot be doubted, but by nucleus ferri must be meant well purged iron; the same which Aristotle calls εἰργασμένος σίδηρος; for why else should he tell us that wrought iron itself may be made liquid, so as to harden again, that is, to cast again into sow-metal, if it was not to explain to us the manner of making steel; which they did probably

* The processes for converting iron into steel are different in different countries, and in different works in the same countries. In Sweden it is sometimes obtained, under particular management in the fusion, directly from the iron-ore, and is then called ore-steel or natural steel; but it is commonly prepared either from cast-iron or forged iron; the cast-iron being made to undergo certain changes in furnaces constructed for the purpose, and the forged iron being subjected to the process of cementation. The steel made from forged iron is called cement-steel or blister-steel. This blister-steel, being fused in a particular manner, yields what is termed cast-steel. See Mr. Collier's paper, before referred to, in the 5th vol. of the Manchester Memoirs. The change produced in the iron by all these processes appears to be the same, varying only in degree, viz. a quantity of carbon is combined with it. Further modifications in relation to its valuable qualities of hardness, elasticity, and susceptibility of fine polish, are effected by compression under the hammer, by immersion whilst red-hot in cold water, or other cold liquid, and by annealing.

after the precept above delivered; that is, not only boil the iron in its own sow-metal, or liquid iron, but hammer it also, and after that quench it in cold water.

As to the steeling of their tools, they boiled them in sow-metal, to such a degree of hardness or temper as was requisite, and did not afterwards hammer them. And this seems to be implied in the phrase densare; for although it be generally said, that iron is purged and refined for the making of steel, yet according to the last and truest process, the matter is plainly otherwise; for iron this way made into steel, becomes a kind of electrum, and is filled with an exceeding brittle and hard body of its own nature; iron being spongy and not close; for which purpose, therefore, the word densare is by Pliny aptly and elegantly used. And this way was used when the strongest temper and hardness was required, as for picks and anvils. For which there might be several reasons given; at first, that it is easier to work iron than steel, into any figure, that being far softer and more ductile and loose: again it is certain that iron by ignition is spoiled or corrupted, so that the oftener it is purged, though it were steel, it would the more relent. Whence the ancients knowing well, that in making their tools of steel, they must considerably loosen it, and abate of their temper; they therefore first shaped them, and then gave them a strong body of steel and temper together, and so had nothing else to do, but to finish them on the grind-stone and hone, to set the point or edge.

A Description of certain Shells found in the East Indies. Communicated by Mr. Witzen to Dr. Lister, and by him to the Editor, with some Remarks of his own. N° 203, p. 870.

There are found on the coasts of Malabar and Ceylon certain cockles or shells, containing a fish that lives in the bottom of the sea, fixed to the body of the shell; and, at a certain season of the year, they cast their seed, which produces a kind of matrix or sack, of about 12 inches in length;* this long body, which is wrinkled like an andouille or sausage, is filled with a great number of round cells, which are so many matrices, each producing its little shell-fish; which quit not their cells till they are grown to such a size and maturity, as their weight breaks them off and loosens them from their cells, and so from their common matrix, which remains fastened to the bottom of the sea by the great end, the other end moving about freely in the water, which is flexible every way like an andouille. This matrix has a kind of back-part and belly;

* *Murex canaliculatus.* Linn. The shells of the genera of murex and buccinum produce their young in the manner here described, viz. imbedded in the cell of a long ovarium, or receptacle.

the back is of a greyish colour, the belly is whiter, and is that part filled with the cells from one end to the other: the skin which covers it is very like that of stock-fish, or other dried fish.

Accompanying this, is another shell, found in the river of Goa, which holds a sort of oyster*. It is very rare, and in the Indies, as well as here, the shell powdered is esteemed a good medicine.

The like to this oyster-shell is to be found in the West Indies, whether the same species I cannot determine, having never seen them entire together, to compare them. But considering the hint that is here given, of its being thought medicinal at Goa, where it is found; and also that calcined shells † are the most common entertainment all over the Indies, chewing them all day long with the leaves of a certain hot piperate and spicy plant,‡ and a sort of nut|| mixed; we may reasonably suspect the Goa stone to be made up of them, or such like ingredients.

On some unknown Ancient Characters; by Mr. Flower: with Remarks by Francis Aston, Esq. S. R. S. N° 203, p. 872.

These characters, being 22 in number, are all that could be distinctly collected out of the ancient sculptures even to this day, to be found at the hills of Canary; where there are divers receptacles cut out of the main rock, with incredible industry by the ancient inhabitants of those parts, supposed to be Moors or negroes of Ethiopia, rather than Gentoos; on account of the size of their usual statures, which are at least 8 feet in height, having great lips, full eyes, flat nose, and curled hair. So far Mr. Flower.

The intent of this paper, probably, was to compare these ancient characters with those at Persepolis, the gentlemen having got together several other alphabets, which had been used in the eastern parts of the world. The places here pointed at, are chiefly three, two on the island Salsete, and one on the island Pory, called the Pagode of Olifant, of such a size, that one of them is described by Linschoten to be equal to a village of 400 houses, to consist of 4 ranges of building, one over another, within the mountain; and to contain no less than 300 rooms or habitations; adorned throughout with strange frightful statues of idols, of lions, tigers, elephants, amazons, and a hundred other things, so well designed and curious, that Andrea Corsali, in the year 1515, had thoughts to send some of them to Julyan de Medices. Who were the

* *Ostrea Virginica.* Linn. Gmel.

† Called chinam or chunam.

‡ Betel, a plant belonging to the Linnæan genus piper.

|| The areca-nut, or pinang, the fruit of a tree belonging to the natural order palmæ.

architects, is uncertain. Balbi names the Romans and Alexander the Great; others the Chinese; Mr. Flower the Abissins, who have some few churches cut in the rocks; but Alvarez, who saw them twice, says the country affirms they were made by Egyptians, or other white men. But though their origin be obscure, their end may more easily be declared, for the Portuguese, on building of Goa, began to destroy them, and have continued to do so ever since.

On some strange Effects from eating Dog-mercury;* *by Mr. T. M. in Salop. With some Remarks by Hans Sloane, M. D. S. R. S.* N° 203, p. 875.

About 3 weeks ago, a woman went into the fields, and gathered some herbs, and having first boiled them, fried them with bacon, for her own and her family's supper. After they had been about 2 hours in bed, one of the children fell very sick, and presently after the other two; which obliged the man and his wife to rise, and take the children to the fire, where they vomited and purged, and within half an hour fell fast asleep. They put the children to bed as they were asleep, then they themselves went to bed, and fell faster asleep than ever they had done before. The man waked the next morning, about 3 hours after his usual time, went to his labour, and so by the strength of his constitution carried it off; but he says, he thought his chin had been all the day in a fire, and was forced to keep his hat full of water by him all the day long, and frequently to dip his chin in it, as he was at his work. The woman awoke some time after her husband, and got up, though she was very sick, and has continued so till within these few days; since which she is very well recovered. One of their children slept from that night which was Thursday, till the Monday evening following; and then, having only just opened her eyes, without speaking one word, died immediately, while she was asleep. The other two children slept about 24 hours, and on their wakening fell a vomiting and purging again, which probably saved their lives.

Some of the same herb was sent to the doctors and apothecaries in Salop, who generally say it is dog-mercury, but some say it is a sort of night shade; whatever it be, it is certainly poisonous, and it is observed that the cattle never eat it. The man says ne never eat so pleasant an herb in his life; and his wife says that her old master, Mr. Moxon, did frequently eat of it; but I guess it to be a mistake. I am no botanist, but I observed that the herb is branched and seeded something like spinage or mercury, but leaved rather like lakeweed; and the leaves are dented.

Dr. Sloane procured a dried specimen of the herb; and he found it to be the

* Mercurialis perennis. Linn.

same they thought it was, the stalks, leaves, and spikes agreeing exactly in every thing with those of dog-mercury. He adds, it is described and figured in several authors: Mr. Ray in his history of plants calls it mercurialis perennis repens cynocrambe dicta, p. 163: Gerard calls it cynocrambe, p. 333: Parkinson, mercurialis sylvestris cynocrambe dicta vulgaris, p. 295. Theatr. Botan.; Johan. Bauhine, in his second tome, lib. 23, cynocrambe mas et fœmina, sive mercurialis repens, p. 979: and Caspar Bauhine, in his Pinax, p. 122, mercurialis montana testiculata et spicata.

On the proportional Heat of the Sun in all Latitudes, with the Method of collecting the same. By E. Halley. N° 203, p. 878.

There having lately arisen some discourse about that part of the heat of weather, simply produced by the action of the sun; and I having affirmed, that if that were considered as the only cause of the heat of the weather, I saw no reason why, under the pole, the solstitial day should not be as hot as it is under the equinoctial, when the sun comes vertical, or over the zenith: for this reason, that for all the 24 hours of that day under the pole, the sun's beams are inclined to the horizon in an angle of $23\frac{1}{2}$ degrees; and under the equinoctial, though he come vertical, yet he shines no more than 12 hours, and is again 12 hours absent, and that for 3h. 8m. of that 12 hours, he is not so much elevated as under the pole; so that he is not 9 of the whole 24 higher than it is there, and is 15 hours lower. Now the simple action of the sun is, as all other impulses or strokes, more or less forcible, according to the sine of the angel of incidence, or to the perpendicular let fall on the plane; whence the vertical ray, being that of the greatest heat, being put radius the force of the sun on the horizontal surface of the earth will be to that, as the sine of the sun's altitude at any other time. These being allowed, it will then follow, that the time of the continuance of the sun's shining being taken for a basis, and the sines of the sun's altitudes erected upon it as perdendiculars, and a curve drawn through the extremities of those perpendiculars, the area comprehended will be proportional to the collection of the heat of all the beams of the sun in that space of time. Hence it will follow, that under the pole, the collection of all the heat of a tropical day, is proportional to a rectangle of the sine of $23°\frac{1}{2}$ into 24 hours, or the circumference of a circle; that is, the sine $23°\frac{1}{2}$ being nearly 4 tenths of radius, as $\frac{8}{10}$ into 12 hours. Or the polar heat is equal to that of the sun continuing 12 hours above the horizon, at 53° height, than which the sun is not 5 hours more elevated under the equinoctial.

But that this matter may be the better understood, I have exemplified it by a scheme, fig. 3, pl. 14, wherein the area ZGHH is equal to the area of all the sines of the sun's altitude under the equinoctial, erected on the respective hours from sun-rise to the zenith; and the area ♋ H H ♋ is in the same proportion to the heat for the same 6 hours under the pole, on the tropical day; and ⊙ H H Q is proportional to the collected heat of 12 hours, or half a day, under the pole, which space ⊙ H H Q is visibly greater than the other area H Z G H, by as much as the area H G Q is greater than the area Z G ⊙; which, that it is so, is visible to sight, by a great excess; and so much in proportion does the heat of the 24 hours sun-shine under the pole exceed that of the 12 hours under the equinoctial: whence, cæteris paribus, it is reasonable to conclude, that were the sun perpetually under the tropic, the pole would be at least as warm as it is now under the line itself.

But whereas the nature of heat is to remain in the subject, after the cause that heated it is removed, and particularly in the air; under the equinoctial the 12 hours absence of the sun does very little abate the motion impressed by the past action of his rays, in which heat consists, before he arise again: but under the pole, the long absence of the sun for 6 months, while the extremity of cold obtains, has so chilled the air, that it is as it were frozen, and cannot, before the sun has got far towards it, be any way sensible of his presence, his beams being obstructed by thick clouds, and perpetual fogs and mists; and by that atmosphere of cold, as Mr. Boyle terms it, proceeding from the everlasting ice, which in immense quantities chills the neighbouring air, and which the too sudden retreat of the sun leaves unthawed, to increase again during the long winter that follows this short interval of summer. But the different degrees of heat and cold, in different places, depend in a great measure on the accidents of the neighbourhood of high mountains, whose height exceedingly chills the air brought by the winds over them; and of the nature of the soil, which variously retains the heat, particularly the sandy, which in Africa, Arabia, and generally where such sandy desarts are found, make the heat of the summer incredible to those that have not felt it.

In prosecution of this first thought, I have solved the problem generally, viz. to give the proportional degree of heat, or the sum of all the sines of the sun's altitude while he is above the horizon, in any oblique sphere, by reducing it to the finding the curve surface of a cylindric hoof, or of a given part of it. Now this problem is not of that difficulty, as appears at first sight: for, in fig. 4, let the cylinder A B C D be cut obliquely with the ellipse B K D I, and through its centre H describe the circle I K L M; I say, the curve surface I K L B is equal to the rectangle of I K and B L, or of H K and 2 B L or B C: and if there be supposed another circle, as N Q P O, cutting the said ellipse in the points P, Q; draw P S, Q R, parallel to the axis of the cylinder, till they meet with the said circle IKLM in the points R, S, and draw the lines RTS, QVP, bisected in T and V; then the curve surface RMSQDP is equal to the rectangle of BL or MD and RS, or of 2BL or AD and ST or VP; and the curve surface QNPD is equal to RS × MD — the arch RMS × SP, or the arch MS × 2SP; or it is equal to the surface RMSQDP, subtracting the surface RMSQNP. So likewise the curve surface QBPO, is equal to the sum of the surface RMSQDP, or RS × MD, and of the surface RLSQOP, or the arch LS × 2SP.

This is most easily demonstrated from the consideration, that the cylindric surface IKLB is to the inscribed spherical surface IKLE, either in the whole or in its analogous parts, as the tangent BL is to the arch EL, and from the demonstrations of Archimedes de Sphæra et Cylindro, lib. 1, prop. 30, 37, 39. Now to reduce our case, of the sum of all the sines of the sun's altitude, in a given declination and latitude, to the aforesaid problem, let us consider fig. 5, which is the analemma projected on the plane of the meridian; Z is the zenith, P the pole, HH the horizon, EE the equator, ♋ ♋, ♑ ♑ the two tropics, ♋ l the sine of the meridian altitude in ♋; and equal thereto, but perpendicular to the tropic, erect ♋ 1, and draw the line T 1 intersecting the horizon in T, and the hour circle of 6: in the point 4, and 6 4 will be equal to 6R, or to the sine of the altitude at 6: and the like for any other point in the tropic, erecting a perpendicular to it, terminated by the line T 1: through the point 4 draw the line 4 5 7 parallel to the tropic, and representing a circle equal to it; then will the tropic ♋ ♋ in fig. 5, answer to the circle NOPQ, in fig. 4; the circle 4 5 7 will answer to the circle IKLM; and T 4 1 will answer to the elliptic segment QIBKP; also 6R or 6 4 will answer to SP, and 5 1 to BL, and the arch ♋ T, to the arch LS, being the semidiurnal arch in that latitude and declination; the sine of which, though not expressed in fig. 5, must be conceived as analogous to the line TS or VP in fig. 4.

The relation between these two figures being well understood, it will follow from what precedes, that the sum of the sines of the meridian altitudes of the sun in the two tropics, (and the like for any two opposite parallels) being multiplied by the sine of the semidiurnal arch, will give an area analogous to the curve surface RMSQDP; and thereto adding in summer, or subtracting in winter, the product of the length of the semidiurnal arch, (taken according to Van Ceulen's numbers) into the difference of the above-said sines of meridian altitude; the sun in one case, and difference in the other, will be as the aggregate of all the sines of the sun's altitude, during his appearance above the horizon; and consequently of all his heat or action on the plane of the horizon, in the proposed day. And this may also be extended to the parts of the same day; for if the aforesaid sum of the sines of the meridian altitudes be multiplied by half the sum of the sines of the sun's horary distance from noon, when the times are before and after noon; or by half their difference, when both are on the same side of the meridian; and thereto in summer, or therefrom in winter, be added or subtracted the product of half the arch answerable to the proposed interval of time, into the difference of the sines of meridian altitudes, the sum in one case, and difference in the other, will be proportional to the whole action of the sun during that space of time.

I foresee it may be objected, that I take the radius of the circle on which I erect the perpendiculars always the same, whereas the parallels of declination are unequal; but to this I answer, that our said circular bases ought not to be analogous to the parallels, but to the times of revolution, which are equal in all of them.

It may perhaps be useful to give an example of the computation of this rule, which may seem difficult to some. Let the solstitial heat, in ♋ and ♑, be required at London, lat 51° 32′.

38°	28′	Co-lat.		Ascen.diff.	33° 11′
23	30	Decl. ☉		Estival semidiurn. arch..	123 11
61	58	Sine =	,882674	Hybernal semidiur. arch	56 49 Sin. ,638923
14	58	Sine =	,258257	Measure of the estival arch	2,149955
		Sum	1,140931	Do. of the hybernal arch	991683
		Diff.	,624417		

Then $1,140931 \times ,836923 + ,624417 \times 2,149955 = 2,29734$

And $1,140931 \times ,836929 - ,624417 \times ,991638 = 0,33895$

So that 2,29734 will be as the tropical summer's days heat, and 0,33895 as the action of the sun in the day of the winter solstice.

After this manner I computed the following table for every 10th degree of latitude, to the equinoctial and tropical sun, by which an estimate may be made of the intermediate degrees.

Lat.	Sun in ♈ ♎	Sun in ♋	Sun in ♑
0	20000	18341	18341
10	19696	20290	15834
20	18794	21737	13166
30	17321	22651	10124
40	15321	23048	6944
50	12855	22991	3798
60	10000	22773	1075
70	6840	23543	000
80	3473	24673	000
90	0000	25055	000

Several notable corollaries follow from this rule. As 1. that the equinoctial heat, when the sun becomes vertical, is as twice the square of radius; which may be proposed as a standard to compare with in all other cases. 2. That under the equinoctial, the heat is as the sine of the sun's declination. 3. That in the frigid zones, when the sun sets not, the heat is as the circumference of a circle into the sine of the altitude at 6: and consequently, that in the same latitude, these aggregates of warmth are as the sines of the sun's declinations; and in the same declination of sol, they are as the sines of the latitudes; and generally they are as the sines of the latitudes into the sines of declination. 4. That the equinoctial day's heat is every where as the co-sine of the latitude. 5. In all places where the sun sets, the difference between the summer and winter heats, when the declinations are contrary, is equal to a circle into the sine of the altitude at 6 in the summer parallel; and consequently those differences are as the sines of latitude into, or multiplied by, the sines of declination. 6. From the table it appears, that the tropical sun under the equinoctial has of all others the least force: under the pole it is greater than any other day's heat whatever, being to that of the equinoctial, as 5 to 4.

From the table, and these corollaries, may a general idea be conceived of the sum of all the actions of the sun in the whole year, and that part of heat which arises simply from the presence of the sun be brought to a geometrical certainty: and if the like could be performed for cold; which is something else than the bare absence of the sun, as appears by many instances, we might hope to bring what relates to this part of meteorology to a perfect theory.

An Account of Books, viz.—1. *Diogenes Laertius Græce et Latine, cum Commentariis integris Doctorum Virorum Amstelodami. Typis Henrici Wetstenii, Anno* 1692. N° 203, p. 886.

The ten books of Diogenes Laertius, containing the lives, sayings, and opinions of the most ancient and eminent philosophers of Greece, have been printed a great many times.

This edition is said to have many advantages over all the former; and it is said that this age has not seen any work performed with so great fidelity, accuracy, elegancy, and perfection in all points, as we see meet here in this edition.

2. *Considerations on a Book entituled, the Theory of the Earth: published some Years since by the learned Dr. Burnet. Written by John Beaumont, jun. Gent.* N° 203, p. 888.

Though several persons before had printed some reflections on this theory, the author of these considerations, arguing generally in a way different from them, thought it might not be a thing ungrateful to the learned, if he published what his own thoughts might be towards the clearing of the abstruse matters there treated. He proceeds on each chapter, as the author of the theory has written them, in his English copy, first briefly stating the contents, and then offering what he has to say upon them.

3. *Epistola ad Regiam Societatem Londinensem, qua de nuperis Terræ-motibus disseritur, et veræ eorum Causæ eruuntur. Lond. in 4to.* 1693. N° 203, p. 893.

An astrological treatise, asserting that the late dreadful earthquakes, and other convulsions in nature, are owing to the influences of the stars, &c.

4. *Traité des Moyens de rendre les Rivieres navigables, &c. a Paris,* 1693, *in 8vo.* N° 203, p. 894.

The author of this treatise says, in the preface, that the methods and machines he proposes, are not mere chimeras, but are already put in practice, chiefly by the Hollanders, who have the most of any cultivated this art.

The work treats on the various impediments to navigation in rivers; of their nature, causes, effects, and the means of removing them.

An Experiment, made before the Royal Society, on the Effects of the Air on a transparent Liquor. Applied to explain Changes of Colour in the Blood of Respiring Animals. By Fr. Slare, M.D. F.R.S. &c. N° 204, p. 898.

To make this experiment, put a quantity of fresh filings of copper in a glass phial, of a broad and even bottom, and then pour on them a urinous spirit, either of sal ammoniac, or of urine itself, not made with quick-lime: the glass should not be filled much above half, and then it must presently be so exactly stopped, that no air be admitted; for an error in one circumstance will mar the whole experiment. The foundation of this experiment is justly due to the immortal Mr. Boyle; but I have endeavoured to bring it home to my own profession, to justify a notion of some importance, though much disputed, concerning an alteration made by the air on human blood, both as to colour, and other virtues. In making the experiment, you will observe, for 4, 5, or 6 days, the tincture becoming deeper and deeper, and then it will be at a stand for 2 or 3 days more or less, and afterwards it will gradually decline, until it become quite pale, and void of all colour. When it is in this state, the easiest way of performing the experiment is to decant this clear spirit into a glass, so as to leave all the filings behind, which will show that the filings did not give this tincture anew, but that it is owing to the influence of the air. But in case you are furnished with an air-pump, and can pour off this pale liquid in vacuo, and there stop it up close, you may then preserve it as long as you please, and exhibit it to advantage. You may also observe, that as soon as you let in the air, the upper surface becomes first immediately tinged, and so the tincture descends deeper and deeper, until it has penetrated the whole: and this it does the sooner if the glass be wide, and the liquor consequently have a larger surface. Or if you pour it out of one glass into another, the air makes a more sudden change of the whole.

That liquors should lose their tinctures is not to be wondered at; for even ink itself, by standing still, will lose much of its tincture; and so do the tinctures of many minerals; that of sulphur, and of salt of tartar, will lose their tinctures; and many vegetables are not long to be preserved, but do grow turbid; some becoming pale and colourless, leave their menstruums, and precipitate to the bottom, and are not easily if ever recovered. But in our experiment we have some things very uncommon; a deeply tinged liquor becomes pale and colourless in a few days, without any admission of air, or any other ingredient to disturb it, or to cause any discernible precipitation or separation; and yet upon the re-admission of the air immediately recovers its former blue tincture.

The great influence the air has in this experiment induced me to apply it to the great change that is made upon blood; for it is obvious that there is a great difference in colour between the venal and arterial blood; the former when first let out of the vein is of a dark colour, and requires some time to be exposed to the air before it obtains a florid red, and that only in the surface contiguous to the air. The observation is certain and unerring, that the venal blood, as it passes the right ventricle, at its entrance into the lungs, is of a very opaque and blackish colour, and in its passage through the lungs, before it reaches the left auricle, it is changed into a very florid and bright red; and it has been often observed, that persons that have vomited blood, on a rupture of some capillary vessels of the lungs, have discharged a very frothy blood, and at the same time of a bright scarlet red: its being frothy argues an admixture of air; and its being red was owing to the tinging quality of the air. To expect that this change should be made in the heart by any local ferment, or vital flame, is fruitless: because the change is performed before its arrival there, and because the structure of the heart denotes that it is principally designed for projecting the blood, in order to a circulation through those various arteries which are branched from the heart. If we consider the structure of the lungs, we shall soon discover them to be a pneumatic engine, principally designed for taking in air, and that in great quantities. It is true, we may call the lungs a contexture of veins, arteries, nerves, lympheducts, &c. but we shall find the great bulk of them to be vesicular; which seems to me to be a continuation of the wind-pipe, divided and subdivided into many branches, and these still spun out into smaller and smaller pipes, all of them hollow; and the farther they run, the thinner their sides grow, which upon the inspiration of air swell and grow round, and upon expiration become flaccid, and abate something of that figure. That these pipes should run to a great length hollow, though very small, I the less wonder at, having seen a hollow pipe of glass drawn out at the flame of a lamp so very small, as to be scarcely visible without a microscope, and yet was so hollow as to take up tinged spirit of wine. The sanguiferous vessels are divaricated through all the lobes of the lungs, and very closely accompany each vesicula, in order to receive some considerable benefit from it; and this appears to the eye; for in an instant a dark and foul blood is changed into a bright florid red colour. Thus the very structure of the lungs, the change of colour both in the blood and in our experiment, the one from a dark opaque colour to a noble scarlet, the other from a pale or colourless liquor to a rich ultramarine blue, all show that this alteration is owing to the air. Nor is the air thus infused into the lungs for a bare colour, and of no farther use; but I am apt to believe the great fermentations of the blood, the cause of the motions and actions of the muscles, the animal spirits themselves, the great spring of motions, derive their energy and powers, if not nature, from hence.*

Corollary 1.—The air abounds in volatile salts; but that they must be called nitrous salts, has been scarcely called in question, which this experiment and some others contradict. Nitrous salts seem to me not to have any property of volatile salt: nitre is a salt of so fixed a nature, that it will continue melted in a very strong fire with scarcely any evaporation; but if you put into it charcoal, or brimstone, or give it an accension, you may obtain a great quantity of as fixed a salt as any concrete whatever affords; so that gold seems not of a more fixed nature.

Corol. 2.—A standard of volatile salts should be settled; at present I can think of none better than water. That salt, which in distillation is more fixed than water, ought not to be reckoned among volatile salts: this standard will be justified by good measures, grounded on experience; for all salts that are truly volatile, are really lighter than water; that is, in a chemical sense, do with a less degree of fire sublime in our glasses, or come over the helm, than water does. This is justified in the volatile salt of amber, erroneously so called; for

* Dr. Slare's views concerning the derivation of the florid colour of arterial blood from the air, and the uses of the atmospheric fluid in respiration, have in part been confirmed by the experiments and observations of succeeding physiologists.

it does not come up to the standard of volatility, and is really no volatile salt, as will be made appear, if you take this supposed volatile salt, and distil it in a retort, or head and body, with common water, the water will ascend in such a degree of fire where the salt will not; for you must increase the fire considerably, to make it rise after the water is evaporated and has left the dry salt at the bottom. This made me inquire further into the properties of this salt, which did not at all correspond with volatile salts, for all true volatile salts are alkalies; but on the contrary would ferment with them, and quite destroy the property of true volatile salts, by bringing them to a dull insipid salt, which some call sal neutrum; and also, by fixing their volatile nature, quite destroy their spirituous and stimulating smell, by virtue of which they have been always deservedly esteemed such excellent cephalic medicines. Therefore, examining this salt yet a little further, you will plainly prove it to be an acid, that corrodes iron, turns syrup of July-flowers green, destroys the tincture of lignum nephriticum, and does not ferment with common acids; so that it plainly belongs to the tribe of acids,* and should be struck out of the catalogue of volatile salts; and perhaps out of the number of specific cephalics, and rather be degraded among the diuretics, and even in that rank to have but an inferior station; for it seems to me to be but a dull medicine, and more valuable for its price than great virtue, especially if quite divested of all its oil, in which the great cephalic and cordial virtue must needs be owned to consist.

Corol. 3.—Volatile salts are very powerful in extracting tinctures, and particularly in heightening those colours which are disposed to be red; for though spirit of wine be a very general menstruum, and draws a very deep tincture from cochineal, yet it has been often observed, that if we put to this tincture, when highest, a small proportion of volatile salt, it will be advanced to a great, even to a double degree. Thus I have observed it to heighten the colour of arterial blood; and, which is very curious, if you dissolve it in your blood whilst bleeding at a vein, that blood will become very florid, and like the arterial. Therefore, since nitrous salts produce none of these tinging effects, this corollary seems much to favour the notion, that the effects of the air upon the blood may be due to such salts as are of a volatile alkalisate nature. †

Corol. 4.—Contagious diseases are communicated by the air inspired at the lungs; which seems more probable then what Dr. Needham and others have endeavoured to make out; in attributing it to the air taken in with our food, because of the smallness of the quantity, if compared with what is communi-

* And is accordingly now referred to its proper class in the modern systems of chemistry, under the denomination of succinic acid.

† Here Dr. S. has been betrayed into an error by reasoning from analogy.

cated by the lungs. To conclude, since the vivifying particles in the air seem to be very sparingly disseminated through it, I am apt to believe that the noxious and pestilential are more sparingly scattered up and down; the Author of human nature having taken more care for its preservation than for its destruction; and therefore it may much better be inferred from the premises, that contagious diseases must be communicated to the blood by inspiration into the lungs, rather than by any other way.*

On the Pressure of Water at several Depths; and of a Well that ebbs and flows. By Dr. Wm. Oliver. N° 204, p. 908.

In the Bay of Biscay, June the 8th, in a hundred fathoms of water, we took a quart glass bottle, stopped with a large cork, and after tied down with a strong packthread, fastening the bottle to the plumbing-rope, and with a lead at the end, we sunk it to the bottom of the sea; on drawing it up again, we found the cork quite pressed through the neck of the bottle into its cavity, and the bottle full of salt sea-water. We repeated the experiment with another bottle and cork, in the same manner as before, but the cork proving not sound, the sea-water soaked through it, and the bottle was half full of water; so the cork remained in the mouth of the bottle, not pressed down at all. We repeated the experiment a third time in 90 fathoms of water, with a very sound cork, and much larger than the mouth of the bottle, for we were forced to beat it in with a hammer as far as it would go, leaving about an inch of the cork above the mouth of the bottle, and tied down as before; but it succeeded not so well as at first, though the cork was now pressed down into the neck, and became level with the mouth of the bottle.

Going a-shore one day, I walked about a mile into the country, to see a well much talked of, near Torbay, called Lay-Well, which made me more than amends for the pains I had taken to come at it. It is about 6 feet long, 5 feet broad, and near 6 inches deep; and it ebbs and flows often every hour, very visibly; for from high water to low water-mark, which I measured, I found it somewhat more than 5 inches. I could not see any augmentation above my mark when it flowed, nor fell it below my mark when it ebbed, but always kept the same distance. The flux and reflux, taken both together, was performed in about two minutes; nothing could be more regular, each succeeding the other as the tides of the sea do. I drank of it, and found it a pleasant, delicate, fine, soft water, not brackish at all: which the country people use in fevers as their ordinary diet-drink, which succeeds very well.

* That the particles of contagion are, in numerous instances, drawn into the lungs along with the air cannot be doubted; but it would appear that, entering the mouth along with the air, they often mix with the saliva, and being swallowed with it, are applied to the stomach also.

On a second visit, I observed it performed its flux and reflux in little more than a minute's time, yet it would stand at its lowest ebb sometimes two or three minutes; so that it ebbed and flowed by my watch about 16 times in an hour, and sometimes, I have been told, 20. As soon as the water in the well began to rise, I saw a great many bubbles ascend from the bottom; but when the water began to fall, the bubbling immediately ceased. The whole country adjacent is very hilly all along the coast; from Brixam to the top of the hill is about a mile and a half, the well is about half way up the hill, which hereabout is somewhat uneven and interrupted, and comes out at a small descent, yet considerably higher than the surface of the sea. The water does not seem to be impregnated with any mineral. Its taste is very soft and pleasant, has no manner of roughness in it, and serves for all manner of uses to the country people in their houses.

Emendationes ac Notæ in Vetustas Albatenii Observationes Astronomicas, cum Restitutione Tabularum Lunisolarium ejusdem Authoris. Per Edm. Halley, S.R.S. N° 204, p. 913.

This is an examination of two editions of a Latin translation of Albateni's astronomical observations and tables, from the Arabic; the translation by Plato Tiburtin; Norimberg, 1537, and Bologna, 1645. The Arabic copy of those observations does not appear, by which the translation might be examined, but Mr. Halley, by calculating tables from the principles there delivered, has here detected and corrected above 30 considerable faults in a few pages.

Of the true Cortex Winteranus, and the Tree that bears it. By Hans Sloane, M.D. and S.R.S.* N° 204, p. 922.

Capt. Winter, a commander of one of the ships which went out with Sir Francis Drake, when he sailed round the world, brought into England from the Straits of Magellan an aromatic bark, which had been very beneficial to his crew, both used instead of other spices with their meat, and as a very powerful medicine against the scurvy. Clusius, from some of those persons, gives a description and figure of it in his exotics, lib. 4, c. 1, p. 75, calling it cortex Winteranus, from the commander of the ship, and the tree itself Magellanica aromatica arbor. The writer of the journal of the Dutch ships, which went to the Straits of Magellan about 1599, notices it as growing there, calling it, lauro similis arbor licet procerior, cortice piperis modo acri, et mordenti. And Sebald de Wert, who was there, says, that both leaves and bark were used with their

* Drimys Winteri.

meat and muscles, to correct them in so cold a climate. Dalechampius, in the Historia Lugd. describes and figures this bark likewise, under the name of cortex Winteranus, p. 1858; and so does Parkinson, p. 1652. Caspar Bauhin, in his Pin. p. 461, calls it laurifolia Magellanica cortice acri. And Jonston, in his Dendrologia, p. 232, arbor laurifolia Magellanica. Those likewise who passed the Magellanic Straits with Sir John Narborough, about 1669, took notice of this tree and bark. But no person has given so good or full an account of it as Mr. George Handisyd, who came from thence about 2 or 3 years since. He told me several particulars relating to it, in the following description, by which I cannot reduce it to any of our kind of plants, so well as to the periclymenum, and therefore shall call it, though it differs in many things from the honysuckle, periclymenum rectum foliis laurinis cortice acri aromatico. He assured me that this tree rose to be taller and larger than an apple-tree, spreading very much both in root and branches; the twigs had on them leaves of a light green colour on their upper side, standing on half-inch-long foot-stalks, are an inch and half long, and an inch broad in the middle where broadest, and whence they decrease to both ends, ending blunt. The flowers stand on three quarters inch-long foot-stalks, 2, 3, or more of them together, something like those of the periclymenum; each of them are milk-white, pentapetalous, and smell like jasmine; to which succeeds an oval berry, made up of 2, 3, or more acini or little berries, standing together on the same common foot-stalk, of a light green colour, with some black spots; and in these berries are contained several black aromatic seeds, something like the stones in grapes. It grows very plentifully in the middle of the Straits of Magellan. The leaves of this tree were used with other herbs by Mr. Handisyd for fomentations, in several cases with very good success; but he admired most the use of the bark inwardly, boiling half a dram of it with some carminative seeds, and giving it so to those of the ship under his care who were very much afflicted with the scurvy; it usually sweated them, and they were very much relieved. The same medicine likewise he administered with much success to many of the ship, who were sick by eating a poisonous sort of seal in those parts, called a sea lion, although they had been so ill as to lose most of their skins, which peeled off their bodies by degrees and in large pieces.

By the preceding description, it appears that the cortex Winteranus, commonly sold in the shops, is not he true cortex Winteranus. But I must needs say, though they are the barks of two very different trees, and growing in very different places, and appear quite different, yet their taste is much the same, and I believe they may be used as a succedaneum for each other.

Pharmacologia seu Manuductio ad Materiam Medicam, in qua Medicamenta Officinalia Simplicia, hoc est Mineralia, Vegetabilia, Animalia, eorumque Partes, in Medicinæ Officinis usitata, in Methodum naturalem digesta succinctè et accuratè describuntur, cum Notis Generum Characteristicis, Specierum Synonymis, Differentiis, et Viribus. Opus omnibus Medicis, Philosophis, Pharmacopœis, Chirurgis, et Pharmacopolis utilissimum. A Samuele Dale. N° 204, p. 925.

A compendium of the materia medica better arranged than the treatises on the same subject which had preceded it; but, like them, lavish in the praises of inert or incongruous drugs.

Extract of a Letter from Sir Richard Bulkley, S. R. S. to Dr. Lister, concerning the Improvement to be made by Maize; with a Note on the same by John Ray, S. R. S.* N° 205, p. 928.

A description of the maize plant, its culture, uses, and produce, which is here said to be 2000 fold.†

A Continuation of Mr. John Clayton's Account of Virginia.‡ N° 205, p. 941.

On approaching the Capes of Virginia it is low land, so that at some distance the trees appear as if they grew in the water; and on coming nearer, to emerge thence. For 100 miles up into the country there are few stones to be found; only in some places rocks of iron ore appear. In some places, for several miles together, the earth is so intermixed with oyster-shells, that there may seem as many shells as earth, and how deep they lie is unknown; for at broken banks

* See Philosophical Transactions N° 142, and vol. ii. p. 465, of these Abridgments.

† *Mr. Ray's note on the above Letter concerning Maize.*—I have not much to say: he has made a commendable essay; but whether it will turn to account to plant maize in Ireland, I think there is some reason to doubt; if it be equal in goodness to pease, and an acre planted with it will certainly yield more than one sown with pease, without impoverishing the land, then indeed it will be advantageous to plant it; but if only an equal quantity, then, though one grain should yield a thousand fold, all the advantage will be in the difference of the seed, which is not very considerable, and which the compendium of sowing above setting may in some measure countervail. As for the description of the plant, all the parts of it have been so particularly and exactly described in English as well as Latin, that I think it needless to publish a new description; only I am by this description confirmed in my opinion, that there are two really distinct species of maize; for what I have seen cultivated in gardens, and have myself planted, rises to double the stature he ascribes to this, that is, 7, 8, or 10 feet; and besides, with us, never brings the seed near to perfection; but that I have seen planted in the fields in Germany, is of about the same height with Sir Richard's, and ripens the seed. Lobel also acknowledges two sorts thus differing.

‡ See N° 201 preceding, and p. 544, of this vol. of the Abridgment.

they discover themselves to be continued many yards perpendicular. In several places these shells are much closer, and being petrified, seem to make a vein of a rock. I have seen in several places veins of these rocky shells 3 or 4 yards thick, at the foot of a hill, the precipice of which is 20 yards perpendicular; pieces of these rocks broken off lie there, which may weigh 20 or 30 tons each; and are as difficult to be broken as free-stone. Of these rocks of oyster-shells, that are not so much petrified, they burn and make all their lime, and the quantity seems to be inexhaustible. Often in the looser banks of shells and earth are found perfect teeth petrified; some of which I have seen could not be less than 2 or 3 inches long, and above an inch broad; though they were not maxillary teeth, the part that one might suppose grew out of the jaw was polished, and black, almost as jet; the part which had been fastened in the jaw and gums was brown, and not so shiningly polished, or smooth. If they were, as they seemed to be, really teeth, I suppose they must have been of fishes. The back-bone of a whale, and, as they say, some of the ribs, were dug out of the side of a hill, several yards deep in the ground, about 4 miles distant from James Town and the river. Mr. Banister, a gentleman pretty curious in those things, showed me likewise the joint of a whale's back-bone, and several teeth, some of them found in hills beyond the falls of James river, at least 150 miles up into the country. The soil in general is sandy. The country is one entire wood, consisting of large timber trees of several sorts, free from thickets or under-wood, the small shrubs growing only on lands that have been cleared, or in swamps; and thus it is for several hundreds of miles, even as far as has yet been discovered.

Observations on the Seeds of Cotton, Palm, or Date Stones, Cloves, Nutmegs, Gooseberries, Currants, Tulips, Cassia, Lime-tree; on the Skin of the Hand, and Pores; of Sweat, the Crystalline Humour, Optic Nerves, Gall, and Scales of Fish and the Figures of several Salt Particles, &c. By Mr. Anth. Van Leuwenhoeck. N° 205, p. 949.

Since my former observations on the seeds of plants, (Phil. Trans. N° 199*) I was surprised to find a variety from what I then wrote concerning the mealy and oily substance, as likewise the embryo plant itself, to be nourished by them in the seeds of cotton, which lie 8 or 9 in clusters, in the cotton-wool that comes from India; for having opened the hard shell or rind, with which the seeds are covered, and from whence the cotton proceeds, and stripping them from the curious, thin, whitish coat, which wraps up each seed so as to look

* P. 525, of this volume of these Abridgments.

like a small round egg; dissecting and opening this, I found no mealy substance at all, but four small leaves enwrapping one another, and compassing the root that lay in the midst of them. These leaves were spotted all over with little specks; and inquiring of some that had seen the cotton grow, they told me, the leaves of the plant itself were thus spotted. So that we see that nature, in this subject, not only wraps up the future plant, but such a little plant whose very leaves are the same as on the grown tree, only smaller.

This brought to my mind that I had observed in the eggs of some insects, taken out of their bodies, none of that substance designed for the nutrition of the embryo; but that in these eggs were contained perfect and living animals, so that as these animals are perfect in the egg, even while it is yet in the uterus of the parent, so the cotton-seed contains a perfect plant, even while it yet hangs on the tree; and besides, that part whence the root grows is very large. And as the forementioned animalcules need no yolk in the egg, being already perfect, and fitted to search their food abroad, so the seeds of this plant contain such an embryo plant as is already fit to shift for itself, and as soon as it falls from the tree, the wet it meets with bursts the shell, and it strikes root, and displays its leaves.

Before I had made any observations on date stones, I thought the hard shell was only the covering to the seed or kernel; but I found it quite otherwise, for that very hard part is furnished with plenty of pores, and little tubuli, serving for the nourishment of the embryo plant contained in the midst, which is soft, and easily cut with a sharp knife. Although I have often observed in that part which is to be the root and trunk, many long slender fibrous parts, like vessels, lying by one another, and some of them filled with a white substance, yet I could never discover that part which gives beginning to the young leaves. Some of these date stones I kept in hot moist sand, and after some time, that part which is to be the root and plant, was shot out half an inch; but yet I could not discover the rudiments of any leaves, only the part shot out consisted of long slender particles, something smaller than those that I had observed in the beginning of the stem.

In some large cloves which I judged perfectly ripe, I examined the inside only, and found it to consist of two parts, lying one upon the other, which though they lie with several angles, and each in a different manner, yet they are the kernels, or medullary part; for between these the embryo plant is placed, and is joined to them by ligaments, by means of which it is nourished. The manner of curing cloves in India is by soaking them in salt water, and drying them in the smoke, which makes them look so black; which when I heard of a gentleman that had lived there, it put an end to my further trials.

I examined nutmegs, as well preserved as dry ones, and found always under the mace a thin skin, before coming to the hard shell, and in one place a ligament, by which it was united to the tree, which entered the hard shell, and was joined to the nut at that part whence the root shoots out; which was all I could find, as they are gathered green, and spoiled in the curing, so that they will not grow.

I took the largest gooseberries, and in their seeds, of which there are sometimes near 60 in one berry, each nourished with a peculiar ligament; viewing the embryo plant, I found all the parts as in other seeds. Examining the proportion the embryo plant in these seeds bears to the seed itself, I found the seed 7 times longer, broader, and thicker than the embryo plant, that is, more than 300 times its bulk.

Out of one of the largest black currants I took 63 seeds, each furnished with a particular ligament, and with its embryo plant, with the first two leaves visible. In this I reckon the seed is above 60 times larger than the little plant. Hence we may conclude there is no seed but what has its embryo plant. I have cut open many bulbous roots, but could never meet with any thing material. Examining tulip seeds, I found the origin of the plant, oblong, and round at each end, furnished with ascending vessels. In the seed of the olive we may with the naked eye discern not only the young plant in the kernel, for it is very large, but the membranes enwrapping it, and the ligament, which is of a different colour from the membrane. And in cassia-seed is observable the young plant, and especially the leaves, which I conceive are so large for the better nourishing the root, which is in this seed very small, and by sowing it in wet sand, the root began to shoot down, the leaves displayed themselves, and the young plant appeared between them. The seed of the lime-tree is the most pleasant spectacle; for in these, the young leaves neither lie plain, nor are wrapped up, but wrinkled like the first leaves of trees in the spring, of a pleasing green colour; and, with a microscope, the fibres of the leaves are very visible; and, contrary to most other seeds, the root of the embryo grows next the tree.

I examined the crystalline humour of a horse's eye, and found it little different from that of an ox, hog, sheep, &c. only it was very large, so that its greater axis was $\frac{5}{6}$ of an inch. I formerly observed that there was no cavity in the optic nerve of an ox's eye, but that its substance was made up of many fibres or threads, which were filled with gently flowing globules; and that if one of these globules, in one of these threads nearest to the eye, were moved by the object, by this means not only the next, but so successively all the globules in that thread; and lastly, the brain itself would be moved. I have found the same since in the optic nerves of 4 horses' eyes; and if it be not allowed that the sight is thus performed, yet it must be granted, that great plenty of new matter continually thus flows from the brain for the nourishment and generation of the eyes.

I examined the gall of a trout, and found a great quantity of small globules, smaller than blood-globules, and a still greater number of much smaller ones, scarcely to be discovered by my best microscopes; so that I think it impossible the liquor should be so very fluid, unless these globules sometimes changed figure, and that easily as they pass by each other. But what method soever I made use of, yet I could not discover any salt particles therein, which was my aim.—I observed the skin of a very large eel, and found the scales on the back and belly in straight rows one upon another, but those on the sides were some sloping towards the belly, others towards the back, but all downwards. Examining the matter or slime covering these scales, which is generally thought to come from without to the scales, whereas it is a real part of the body itself, furnished with capillary vessels, and veins admirably interwoven; some of which are so very slender, that if a common blood-globule was divided into 1000 parts, one of these could yet scarcely pass them. That slime also which covers the bream is nothing but a kind of cuticle, though its vessels are so very small as to require a very excellent microscope; and this substance is likewise covered with extreme minute globules. This commonly called slime, does not only proceed from the innermost skin of the fish, but partly out of the scales. I viewed the scales of perch, and found them likewise furnished with a similar kind of slime or skin; the difference being only in that the vessels forming it are much stronger, so that it is not so easily rubbed off, and is defended by little pricks sticking out at the ends of the scales.

I set some beer-vinegar in my closet; and after 8 or 10 days, I found a great number of salt particles in it, sharper than those I had formerly seen in wine-vinegar. In some I saw a little cavity in the middle: others seemed not perfectly formed, wanting one point; others were a great many joined together. I put some crabs'-eyes into the vinegar; and then the salt figures were so small, and all encompassed with little particles, that I could not discover any perfect salt particles.—Juice of lemons and citrons afforded no salt particles; but after freezing, and a little settling at the bottom of the phial, the clear juice yielded plenty of them, much like those of wine and beer-vinegar.—Spirit of sal ammoniac, after a while exposing in my closet began to shoot, and examining the figures, I found a great many small particles coagulated into one little mass, each of which was a little longer than broad, with a little cavity, so that it seemed as if they had been plain at first, and that the sides turned up. I viewed a little of this spirit, until it shot into salts before my eyes, which it did in an instant, like lightning; but the figures by this means made, were like little irregular pipes, so that I am not certain concerning their shape in this spirit. I mixed some of this spirit with an equal quantity of blood, and at first could see no alteration; but in about $\frac{1}{4}$ of an hour several of the blood-globules were dissolved, and the spirit mixed with the serum looked reddish; in another quarter there were but few globules undissolved, and the serum looked redder than before.

An Instance of the Excellence of the Modern Algebra, in the Resolution of the Problem of finding the Foci of Optic Glasses universally. By E. Halley, S. R. S. N° 205, p. 960.

The excellence of the modern geometry is in nothing more evident, than in those full and adequate solutions it gives to problems; representing all the possible cases at one view, and in one general theorem often comprehending whole sciences; which deduced at length into propositions, and demonstrated after the manner of the ancients, might well become the subjects of large treatises: for whatever theorem solves the most complicated problem of the kind, does with a due reduction reach all the subordinate cases. Of this I now design to give an instance in the doctrine of dioptrics.

This dioptric problem, is that of finding the focus of any sort of lens, exposed either to converging, diverging, or parallel rays of light, proceeding from, or tending to, a given point, in the axis of the lens, be the ratio of refraction what it will, according to the nature of the transparent material the lens is formed of, and also with allowance for the thickness of the lens between the vertices of the two spherical segments. This problem being solved in one case, mutatis mutandis, will exhibit theorems for all the possible cases, whether the lens be double-convex or double-concave, plano-convex or plano-concave, or convexo-concave, which sort is usually called a meniscus. But this is only to be understood of those rays which are nearest to the axis of the lens, so as to occasion no sensible difference by their inclination to it; and the focus here formed is by dioptric writers commonly called the principal focus, being that of use in telescopes and microscopes.

Let then, in fig. 6, pl. 14, B E b be a double convex lens, C the centre of the segment E B, and K the centre of the segment E b, B b the thickness of the lens, D a point in the axis of the lens; and it is required to find the point F, where the beams, proceeding from the point D, are collected, the ratio of refraction being as m to n. Let the distance of the object $DB = DA = d$, (the point A being supposed the same with B, but taken at a distance from it, to prevent the coincidence of so many lines,) the radius of the segment towards the object C B or C A $= r$, and the radius of the segment from the object K b or K $a = \rho$, and let B b the thickness of the lens be $= t$; then let the sine of the angle of incidence D A G be to the sine of the refracted angle H A G, or C A f, as m to n; and in very small angles the angles themselves will be in the same proportion. Whence it will follow that, as d to r, so the angle at C to the angle at D, and hence $d + r$ will be as the angle of incidence G A D: again, as m to n, so $d + r$ to $\frac{dn + rn}{m}$, which will be as the angle G A H $=$ C A f; this being taken from A C D, which is as d, will leave $\frac{\overline{m - n}.d - nr}{m}$ analogous to the angle A f D; and the sides being in this case proportional to the angles they subtend, it will follow, that as the angle A f D is to the angle A D f, so is the side A D or B D, to A f or B f: that is B f will be $= \frac{mdr}{\overline{m - n}.d - nr}$, which shows in what point the beams proceeding from D would be collected by means of the first refraction. But if nr cannot be subtracted from $\overline{m - n}.d$, it follows that the beams after refraction do still pass on diverging, and the point f is on the same side of the lens beyond D: or if nr be equal to $\overline{m - n}.d$, then they proceed parallel to the axis, and the point f is infinitely distant.

The point f being found as before, and B f — B b being given, which we call δ; it follows, by a process like the former, that b F, or the focal distance sought, is equal to $\frac{\delta\rho n}{\overline{m - n}.\delta + m\rho} = f$. And instead of δ substituting B f — B $b = \frac{mdr}{\overline{m - n}.d - nr} - t$, putting p for $\frac{n}{m - n}$, after due reduction this following equation will arise, $\frac{mpdr\rho - nd\rho t + npr\rho t}{mdr + md\rho - mpr\rho - \overline{m - n}.dt + nrt} = f$. Which theorem, however it may seem operose, is not so, considering the great number of data that enter the question, and that one half of the terms arise from our tak ng in the thickness of the lens, which in most cases can produce no great effect, however it was necessary to consider it, to make our rule perfect. If therefore the lens consist of glass, whose refraction is a 3 to 2 it will be $\frac{6dr\rho - 2d\rho t + 4r\rho t}{3dr + 3d\rho - 6r\rho - dt + 2rt} = f$. If of water, whose refraction is as 4 to 3, the theorem will stand thus $\frac{12dr\rho - 3d\rho t + 9r\rho t}{4dr + 4d\rho - 12r\rho - dt + 3rt} = f$. If it could be made of diamond, whose refraction is as 5 to 2, it would be $\frac{\frac{10}{3}dr\rho - 2d\rho t + \frac{4}{3}n\rho t}{5dr + 5d\rho - \frac{10}{3}r\rho - 3dt + 2rt} = f$. And this is the universal rule for the foci of double convex glasses exposed to diverging rays. But if the thickness of the lens be rejected as not sensible, the rule will be much shorter, viz. $\frac{p\,d\,r\,\rho}{d\,r + d\,\rho - p\,r\,\rho} = f$, or in glass $\frac{2\,d\,r\,\rho}{d\,r + d\,\rho - 2\,r\,\rho} = f$; all the terms wherein t is found being omitted, as equal to nothing. In this case, if d be so small, as that $2\,r\,\rho$ exceed $d\,r + d\,\rho$, then will it be $-f$, or the focus will be negative, which shows that the beams after both refractions still proceed diverging.

To bring this to the other cases, as of converging beams, or of concave glasses, the rule is ever composed of the same terms, only changing the signs of + and −; for the distance of the point of concourse of converging beams, from the point B, or the first surface of the lens, I call a negative distance or $-d$; and the radius of a concave lens, I call a negative radius or $-r$, if it be the first surface, and $-\rho$, if it be the second surface. Let then converging beams fall on a double convex of glass, and the theorem will stand thus $\frac{-2\,d\,r\,\rho}{-d\,r - d\,\rho - 2\,r\,\rho} = +f$; which shows that in this case the focus is always affirmative.

If the lens were a meniscus of glass, exposed to diverging beams, the rule is $\frac{-2\,d\,r\,\rho}{-d\,r + d\,\rho + 2\,r\,\rho} = f$; which is affirmative when $2\,r\,\rho$ is less than $d\,r - d\,\rho$, otherwise negative: but in the case of converging beams falling on the same meniscus, it will be $\frac{+2\,d\,r\,\rho}{+d\,r - d\,\rho + 2\,r\,\rho} = f$; and it will be $+f$, while $d\,\rho - d\,r$ is less than $2\,r\,\rho$, but if it be greater than $2\,r\,\rho$, it will always be found negative or $-f$. If the lens be double concave, the focus of converging beams is negative, where it was affirmative in the case of diverging beams on a double convex, viz. $\frac{-2\,d\,r\,\rho}{+d\,r + d\,\rho - 2\,r\,\rho} = f$, which is affirmative only when $2\,r\,\rho$ exceeds $d\,r + d\,\rho$: but diverging beams passing a double concave have always a negative focus, viz. $\frac{-2\,d\,r\,\rho}{+d\,r + d\,\rho + 2\,r\,\rho} = -f$.

The theorems for converging beams are principally of use to determine the focus resulting from any sort of lens placed in a telescope, between the focus of the object-glass and the glass itself; the distance between the said focus of the object-glass and the interposed lens being made $= -d$.

I here suppose my reader acquainted with the rules of analytical multiplication and division, as that + multiplied by + makes the product +, + by − makes −, and − by − makes +; so dividing + by + makes the quote +, + by − makes −, and − by − makes +; which will be necessary to be understood in the preceding examples.

In case the beams are parellel, as coming from an infinite distance, which is supposed in the case of telescopes; then will d be supposed infinite, and in the theorem $\frac{p d \rho r}{d r + d \rho - p r \rho}$ the term $p r \rho$ vanishes, as being finite, which is no part of the other infinite terms, and dividing the remainder by the infinite part d, the theorem will stand thus $\frac{p \rho r}{r + \rho} = f$, or in glass, $\frac{2 r \rho}{r + \rho} = f$.

In case the lens were plano-convex exposed to diverging beams, instead of $\frac{p d \rho r}{d r + d \rho - p r \rho}$, r being infinite, it will be $\frac{p d \rho}{d - p \rho} = f$, or $\frac{2 d \rho}{d - 2 \rho} = f$ if the lens be glass.

If the lens be double-convex, and r be equal to ρ, as being formed of segments of equal spheres, then will $\frac{p d \rho r}{d r + d \rho - p r \rho}$ be reduced to $\frac{p d r}{2 d - p r} = f$; and in case d be infinite, then it will yet be farther contracted to $\frac{1}{2} p r$, and p being $= \frac{n}{m - n}$, the focal distance in glass will be $= r$, in water $1\frac{1}{2} r$, but in diamond $\frac{1}{3} r$.

I am sensible that these examples are too much for the complete analyst, though I fear too little for the less skilful, it being very hard, if possible, in such matters, so to write as to give satisfaction to both; or to please the one, and instruct the other. But this may suffice to show the extent of our theorem, and how easy a reduction adapts any one case to all the rest. Nor is this only useful to discover the focus from the other proposed data, but from the focus given, we may thereby determine the distance of the object, or from the focus and distance given, we may find of what sphere it is requisite to take another segment, to make any given segment of another sphere cast the beams from the distance d to the focus f; as likewise from the lens, focus, and distance given, to find the ratio of refraction, or of m to n, requisite to answer those data. All which it is obvious, are fully determined from the equation we have hitherto used, viz. $p d \rho r = d r f + d \rho f - p r \rho f$; for to find d, the theorem is $\frac{p r \rho f}{r f + \rho f - p \rho r} = d$, the distance of the object.

For ρ, the rule is $\frac{d r f}{p d r + d f + p r f} = \rho$.

But for p, it is $\frac{d r f + d \rho f}{d \rho r + f \rho r} = p$, which latter determines the ratio of refraction, m being to n as $1 + p$ to p.

I shall not expatiate on these particulars, but leave them for the exercise of those who are desirous to be informed in optical matters, which I am bold to say are comprehended in these three rules, as fully as the most inquisitive can desire them, and in all possible cases; regard being had to the signs + and —, as in the former cases of finding the focus. I shall only show two considerable uses of them; the one to find the distance whereat an object being placed, it shall by a given lens be represented in a species as large as the object itself, which may be of singular use, in drawing faces, and other things in their true magnitude, by transmitting the species by a glass into a dark room, which will not only give the true figure and shades, but even the colours themselves, almost as vivid as the life. In this case d is equal to f, and substituting d for f in the equation, we shall have $p d r \rho = d d r + d d \rho - d p \rho r$, and dividing all by d, then $p r \rho = d r + d \rho - p r \rho$, that is $\frac{2 p r \rho}{r + \rho} = d$; but if the two convexities be of the same sphere, so as $r = \rho$, then will the distance be $= p r$; that is, if the lens be glass $= 2 r$; so that if an object be placed at the diameter of the sphere distant, in this case the focus will be as far within as the object is without, and the species represented will be as large as the life; but if it were a plano-convex, the same distance will be $= 2 p r$, or in glass equal to 4 times the radius of the convexity. But of this method I may perhaps entertain the curious in some other transaction, and show how to magnify or diminish an object in any proportion assigned, which yet will be obvious enough from what is here delivered, as likewise how to erect the object, which in this method is represented inverted.

A second use is to find what convexity or concavity is required, to make a vastly distant object be represented at a given focus, after the one surface of the lens is formed; which is but a corollary of our theorem for finding ρ, having p, d, r and f given; for d being infinite, that rule becomes $\frac{r f}{p r - f} = \rho$, that is in glass $\frac{r f}{2 r - f} = \rho$; whence, if f be greater than $2 r$, ρ becomes negative, and $\frac{r f}{f - 2 r}$ is the radius of the concave sought.

Those who are wholly to begin with this dioptrical sience, cannot do better than to read with attention a late treatise of dioptrics, published by W. Molineaux, Esq. R. S. S. who has at large shown the nature of optic glasses, and the construction and use of microscopes and telescopes; and though some nicely critical have endeavoured to spy faults, and to traduce the book, yet having long since examined it with care, I affirm, that if I can judge, it has only two things that with any colour may be called faults; the one an over careful acknowledgement of every trifle the author had received from others; and the other, that he labours to make easy this curious subject, so little understood by most, in a manner perhaps too familiar for the learned critic, and which demonstrates that it was written cum animo docendi, both which require but very little friendship or good nature in the reader, to pass for virtues in an author.

But to turn to our first theorem, which accounting for the thickness of the lens, we will here again resume, viz.

$\frac{mpdr\rho - nd\rho t + npr\rho t}{mdr + md\rho - mpr\rho - \overline{m-n}.dt + nrt} = f.$ And let it be required to find the focus, where a whole sphere will collect the beams proceeding from an object at the distance d: here t is equal to $2r$, and $r = \rho$: and, after due reduction, the theorem will stand thus, $\frac{mpdr - 2ndr + 2nprr}{2nd + 2nr - mpr} = f$; but if d be infinite, it is contracted to $\frac{mpr}{2n} - r = \frac{2n - m}{2m - 2n} r = f$; wherefore a sphere of glass collects the sun's beams at half the semidiameter of the sphere without it, and a sphere of water at a whole semidiameter. But if the ratio of refraction m to n, be as 2 to 1, the focus falls on the opposite surface of the sphere, and if it be of greater inequality, it falls within.

Another example shall be, when a hemisphere is exposed to parallel rays, that is d and ρ being infinite, and $t = r$; then after due reduction the theorem results $\frac{nn}{mm - mn} r = f$. That is, in glass it is at $\frac{4}{3} r$, in water at $\frac{9}{4} r$; but if the hemisphere were diamond, it would collect the beams at $\frac{4}{15}$ of the radius beyond the centre.

Lastly, as to the effect of turning the two sides of a lens towards an object; it is evident, that if the thickness of the lens be very small, so as that you neglect it, or account $t = 0$; then in all cases the focus of the same lens, to whatever beams, will be the same, without any difference on the turning the lens: but if you are so nice as to consider the thickness, (which is seldom worth accounting for) in the case of parallel rays falling on a plano-convex of glass, if the plain side be towards the object, t occasions no difference, but the focal distance $f = 2r$: but when the convex side is towards the object, it is contracted to $2r - \frac{2}{3}t$, so that the focus is nearer by $\frac{2}{3}t$. If the lens be double convex, the difference is less; if a meniscus, greater. If the convexity on both sides be equal, the focal length is about $\frac{1}{6}t$ shorter than when $t = 0$. In a meniscus, the concave side towards the object increases the focal length, but the convex towards the object diminishes it. A general rule for the difference arising on turning the lens, where the focus is affirmative, is this, $\frac{2rt - 2\rho t}{3r + 3\rho - t}$, for double convexes of differing spheres. But for a meniscus, the same difference becomes $\frac{2rt + 2\rho t}{3r - 3\rho + t}$; of which I need give no other demonstration, but that by a due reduction it will so follow from what is premised, as will the theorems for all sorts of problems relating to the foci of optic glasses.

Abstract of a Letter, from M. Samuel Dale to Mr. John Houghton, S. R. S. concerning the making of Turnip-Bread in Essex. Dated Braintree, Dec. 6, 1693. N° 205, p. 970.

Take peeled turnips, and boil them in water until they are soft or tender; then strongly pressing out the juice, mix them, being beaten or pounded very fine and small, with their weight of wheat-meal. Then adding salt and yest, of each *q. s.* and warm water, knead it up as other dough or paste: and having suffered it to be a little while to ferment, let it be baked as common bread.

An Extract of a Letter from Sir R. Bulkley, concerning the Propagation of Elms by Seed. N° 205, p. 971.

I have met with a poor labourer who has followed the propagating of elms by the seed, a way, if known, totally neglected among all planters; which seed he finds in the former part of the year; and he has raised in small beds such vast numbers of them, that he is enabled to sell them at a very low rate indeed.

An Account of a Book entitled, Phalænologia Nova, sive Observationes de rarioribus quibusdam Balænis in Scotiæ Littus nuper ejectis, &c. Aut. Roberto Sibbald, Edinburgi, in Quarto,* 1692. N° 205, p. 972.

The author of Scotia Illustrata, has here given a curious specimen of the

* Robert Sibbald (according to the editors of the biographical Dictionary in 15 volumes, London, 1798) was born at his paternal estate near Leslie in Fifeshire, Scotland, in the year 1643, and was educated in the university of St. Andrew's, where he took his degrees, and afterwards travelled into France and Italy. Being extremely curious in his inquiries after knowledge, he obtained the friendship of the most eminent persons in the literary world; and, on his return, to his native country, projected the plan for establishing a royal college of physicians in Edinburgh. He also planted a botanical garden there. In 1686 he is said to have embraced the Roman Catholic religion, but afterwards renounced the errors of Popery. His practice was said to be very extensive, and in his hours of leisure he studied the antiquities and natural history of Scotland. He was knighted by Charles the Second, and had also the title of King's Physician and Geographer Royal conferred upon him. Although (says Dr. Pultney, in his account of this author) Sir Robert Sibbald did not carry his researches so far as to rank high in the character of a naturalist, yet as having led the way in that branch, and singularly promoted the study of the antiquities of his country, he is justly entitled to that honourable station he bears among the writers of North-Britain. His name is immortalized by Linnæus under a genus called *Sibbaldia.*

He published a work entitled *Scotia Illustrata, sive Prodromus Historiæ Naturalis Scotiæ,* folio, besides the abovementioned *Phalænologia Nova, or Observations on some Animals of the Whale Genus lately thrown on the Shores of Scotland.* And in the year 1706 he communicated to the Royal advance he has made in the natural history of that kingdom; the work contains many observations altogether new, and well worthy to be known.

It is divided into 3 sections, in which he treats of the small and large whales. The 1st section is concerning the lesser sort of whales, both those that have a spout, and those that have nostrils; those that have teeth in both their jaws, and those that have them only in the lower.

The 2d section concerns the larger whales, which have teeth only in the lower jaw. In the 3d section is an account of those whales, that have horny plates in their upper jaw, in England improperly called whale-bone.

*A Letter of Mr. John Clayton, Rector of Crofton at Wakefield, to the Royal Society, giving a farther Account of the Soil, and other Observables of Virginia.** N° 206, p. 978.

Some Queries concerning the Nature of Light and Diaphanous Bodies. Proposed to the Royal Society. By E. Halley. N° 206, p. 998.

The late curious book of Mr. Huygens having revived the disquisitions that have formerly been made about the nature and phænomena of light, I thought it not amiss to propose some difficulties that have occurred to my thoughts on this subject, by way of query.

1. In what consists the transparency of glass, crystal, water, &c. And whether the notion of right pores be sufficient to answer all its appearances, especially those of refaction, and those of the transparency of bodies in all positions; whereas the rectitude of pores seems to argue an orderly or regular position of the constituent parts, according to the three dimensions?

2 Why in bodies that have much more pores than glass or water; as deal-shavings or brown paper, the passage of light is wholly obstructed, though several gross particles will penetrate them?

3. Whether or not the light is easier propagated through glass, water, &c. than air or æther, as Descartes and Mr. Hook have maintained; and wherein Mr. Huygens differs from them, asserting that the beams of light are retarded in passing those diaphanous bodies; and thereby so naturally explaining the laws of refraction?

4. Supposing light to be propagated in a wave, how it comes to pass that

Society a description of the species of barnacle called by Boccone *pediculus ceti.* It is the *lepas diadema* of Linnæus.

* This miscellaneous paper contains a general description of the face, soil, cultivation, &c. of Virginia, together with a slight enumeration of some of the most remarkable birds of the country.

this propagation being either retarded or accelerated in a diaphanous body, as glass, &c. does, upon its going out of that medium, acquire again the same degree of velocity it had before it came on, there being no new impulse or impediment to alter the velocity it had in the other medium?

5. Why mercury, being so pure, simple and homogeneous a fluid, is almost the only one that is not transparent?

6. Whether the reflection of light on the surfaces of glass, water, and the shade of the most perfect pellucid being always very discernible, be not arguments that the beams pass their media with more difficulty than they do the air?

7. Whether any texture of atoms of the same materia prima can be supposed to answer to the phænomena of the pellucidity of heavy and opacity of light bodies?

8. Whether, if light (as it is most likely) be a tremour, shake, or undulation of the æther, as sound is of the air; and if the æther do consist of so rarefied parts, as to penetrate all bodies with full liberty, as is generally supposed; most if not all bodies ought not to be transparent?

9. Whether the matter of the universe be not of several kinds in minimis, and not constituted by the various texture and coalition of the same sort of atoms, as it has been held by the Epicurean and Atomical philosophers, which at present obtain in the world.

*An Account of two Books.—I. Pharmacopœia Bateana; or, Bate's * Dispensatory. Translated into English by William Salmon, Professor of Physic; London, 1694, 8vo.* N° 206, p. 1000.

Bate's Pharmacopœia, at the time of its first publication, was considered, although it contains many absurd remedies, to be a valuable collection of extemporaneous medical formulæ, and accordingly it soon went through many editions. But along with pharmaceutical chemistry, the art of prescribing has of late undergone such an entire change, that the Pharmacopœia Bateana is not of the least use to modern practitioners of physic. By way of apology for publishing this book in English, the translator cites the examples of Hippocrates, Galen, Celsus, and others, who all published a system of physic in their own languages;

* Dr. George Bate was born in 1608, and was educated at Oxford, where he graduated and practised for several years, but afterwards removed to London. With more address than principle, he sided, as best suited his interested views, at one time with the royal, at another with the republican party; being successively physician to Charles I. to Cromwell, and to Charles II. He died in 1669. Some years after his death, an apothecary of the name of Shipton published, as his prescriptions, the above-mentioned collection of formulæ, entitled Pharmacopœia Bateana. Dr. Bate was, moreover, author of several political tracts.

as likewise the usage of the French, who treat of all subjects in their own tongue.

II. Phthisiologia Lancastriensis, cui accessit Tentamen Philosophicum de Aquis Mineralibus, &c. Aut. Carolo Leigh, M.D. Lond.* 8vo. 1694. N° 206, p. 1003.

After considering the causes and symptoms of phthisis, the author proceeds to lay down his method of cure, adapted to the several varieties and stages of the disorder. In the incipient stage of what he terms phthisis scorbutica he employs emetics, bleeding, resolvents, balsamics, &c.

In the confirmed stage of this species of consumption, he strongly recommends the Peruvian bark, joined with pectoral medicines, directing the patient at the same time to be nourished with eggs, and broths made from veal, chicken, &c. in preference to milk and its preparations. The author then describes his mode of treating the other species of phthisis, viz. phthisis ab hæmoptysi, phthisis de vomitu sanguinis, phthisis ab hœmorrhagia uterina, phthisis ab impetigine, phthisis a rheumatismo scorbutico, phthisis a rhachitide, phthisis a fluore albo, phthisis a chlorosi, and lastly, phthisis chylosa.

The subjoined essay on mineral waters contains observations on several chalybeate, saline, sulphureous, and other springs, together with some short remarks on the use of cold baths.

END OF VOLUME SEVENTEENTH OF THE ORIGINAL.

Account of the Earthquakes in Sicily, on the 9th and 11th of January, 1692-3. *Translated from an Italian Letter, written from Sicily by Vincentius Bonajutus, and communicated to the Royal Society by Marcellus Malpighius.* N° 207, p. 2. *Vol. XVIII.*

The continual fiery eruptions of Ætna, of which the first that we have any account, happened 500 years before the destruction of Troy, as Diodorus Siculus relates, have been taken for the most likely causes of the horrible shocks that from time to time have laid waste the island of Sicily. One is mentioned by

* Besides the above-mentioned tract on consumptions, Dr. Charles Leigh wrote Exercitationes de aquis medicatis, morbis acutis, et morbis intermittentibus; and he has introduced various medical observations and cases into his natural history of Lancashire, (which was his native county,) Cheshire, and Derbyshire. He appears to have employed the Peruvian bark freely and successfully in intermittents. He died in the beginning of the 18th century.

Fazello, viz. in 1542, Dec. 10, which shook the whole island, and especially Val di Noto, Syracusa, Lentini, Sortini, Mililli, Catania, Agosta, Noto, Caltagirone, Militello; and, in short, the same cities and castles which were so miserably ruined by the two late violent earthquakes of this present year 1693.

The first of which was at five o'clock the next night after the 9th of January. Its motion was of that sort which Aristotle and Pliny call the first species, and is by them compared to the shaking fit of an ague, causing such a motion as shakes the earth from side to side. In this shock almost all the edifices in the country were thrown down, of which some were very high and strong built towers. A great part of the city of Catania, with many others, was demolished, and a great many buildings in Val di Noto. Syracuse was also much shattered, but not ruined. This was not preceded by any darkness in the air, but a pleasing serene warm season, which was the more observable, as being unusual at that time of the year. The evening before, some persons observed a great flame or light, at about an Italian mile distance; and so bright, that they took it for a real fire made by some of the country people; and though they went directly towards it, yet it seemed to keep at the same distance from them. While they were observing this appearance, the earthquake began, and the light quite vanished; and the waves of the sea, which before the shock only beat gently on the shore, began now to make a dreadful noise. The next two days and night the air was overshadowed with darkness, being tinged with a deep yellow, and the obscured sun struck our minds with the melancholy presage of the approaching earthquake; which was the second, and happened on the 11th of January, and lasted about 4 minutes. It was much like the second sort, which Aristotle and Pliny call a pulse or stroke, from its resemblance to the beating of an artery; and by Possidonius, in Seneca, is represented by the name of vibrations, it being a perpendicular lifting up of the earth.

So horrid and amazing a shock was at once spread over all Sicily, and its impulse was so vehement and powerful, that not only many cities and parts of the kingdom of Naples, but the island of Malta shared in its fury. It was impossible to keep upon our legs, and even those who lay along on the ground, were tossed from side to side, as if on a rolling billow. In open places, the sea sunk down considerably, and in the same proportion in the ports and inclosed bays, and the water bubbled up all along the shore. The earth opened in several places in very long clefts, some a hand's breadth, others half a palm, others like great gulfs. From those openings in the valleys such a quantity of water sprung forth as overflowed a great space of ground, which to those that were near it had a sensibly sulphureous smell.

In the plain of Catania, from one of these narrow, but very long, clefts, and about 4 miles off the sea, the water was thrown out quite as salt as that of the sea. In the city of Noto is a street of half a mile long, built of stone, which at present is settled into the ground, and quite hanging on one side, like an inclining wall; and in another street, before the Assent del Durbo, is an opening large enough to swallow a man and horse.

Great rocks were loosened and thrown down from the mountains every where. And in the country of Sortino, inhabited by about 5000 persons, a great number perished in the houses, which were beaten down by the rocks in their way, as they rolled down from the hills. At Syracuse and other places near the sea, the water in many wells, which at first were salt, are become fresh, and fit to drink. The fountain Arethusa for some months was so brackish, that the Syracusans could make no use of it, and now that it is grown sweeter its spring is increased to near double. In the city of Termini all the running waters are dried up, but the hot-baths were augmented by a third part of what they were before the earthquake. A fountain in the very moment of the earthquake threw out its waters tinged of a blood red for 3 hours, and then it dried up, leaving many holes in the mud at the bottom, through which real ashes were thrown out; and the next day the waters returned to the former quality, without the least alteration. The south winds blew very much, which have always been impetuous in the most considerable earthquakes. And from the 11th of January to this 14th of September there have been considerable and strong south winds, preceded by a noise like cannon at a great distance, some of a longer, some of a shorter continuance. It has been observed, that in less solid ground, such as chalk, sand, or loose earth, the damage was incomparably greater than in rocky places. The effects on human bodies have been various; such as foolishness, but not to any great degree, madness, dulness, sottishness, hypochondriac, melancholic, and choleric distempers. Fevers have been common, with many continual and tertian, malignant, mortal, and dangerous ones in great numbers, with deliria and lethargies. The small pox has made great destruction among children. And in short, every age and condition has had its share in so universal a calamity.

The number of persons destroyed by this earthquake in the several towns of Sicily, amounts to 59963 out of 254936, the whole number of the people before this accident, that is very nearly the fourth part of the population.

A Letter from Mons. Buissiere, a French Anatomist and Surgeon, concerning an Egg found in the Tuba Fallopiana of a Woman lately dissected; with Remarks touching Generation. An abridged Translation from the French. N° 207, p. 11.

In this letter it is stated, that on dissecting the body of a female convict, aged 25 or 26, who was executed soon after she had conceived in consequence of an intercourse with one of the men in prison, and before the impregnated ovum had time to get into the uterus, the Fallopian tube on the left side was found surprisingly dilated towards its extremity; and that this dilatation, where it was the greatest, was rather more than an inch in diameter, and upwards of $1\frac{1}{2}$ in extent, diminishing on the side towards the uterus. The part which was thus dilated embraced nearly the whole of the ovarium, to the membrane of which it adhered so strongly that it could not be separated without violence; when the separation was effected, there issued forth a quantity of limpid unctuous fluid, the use of which might be either to relax the membranes of the Fallopian tube, so as to allow it to dilate in such manner as to afford a free passage to the ovum into the uterus; or to facilitate the transmission of the ovum by lubricating the tube, or perhaps it might serve both purposes. This fluid the author supposes to be derived from the ovarium, and that the fibres and small vessels, whether lymphatics or others, which are ruptured in order to give exit to the impregnated ovum, pour out this liquor into the Fallopian tube; so that although a wound is thus inflicted on the ovarium, it is nevertheless useful, producing effects which seem to be indispensably necessary either to the first nourishment of the ovum, or to the furthering and facilitating of its conveyance into the uterus. What corroborates this idea is, that in the females of animals from whose ovarium many ova are detached at a time, this liquor is met with in great abundance. Accordingly on opening a sow, the author found appearances similar to those observed in this woman, viz. the Fallopian tubes on each side, where they embraced the ovarium, contained 3 or 4 ounces of the aforesaid liquor.

The Fallopian tube being detached from the ovarium, and the liquor having run out, the ovum came into view. It was as large as a hazel-nut, surrounded by the liquor in the middle of the dilated cavity of the tube. Three-fourths of this ovum were already excluded from the ovarium by the wound or aperture which it had made; insomuch that it appeared to have no further connection with the ovarium; however, when the author attempted to take it away, he found it still attached by a considerably firm pedicle, through which were transmitted blood-vessels to the ovum. It is by these blood-vessels, he adds, that the fœtus receives its nourishment, not only in the ovarium, but also in the uterus: this pedicle serving to form the placenta (if it be not the placenta itself already formed in the ovarium) by attaching itself to the body of the uterus.*

Remarks, taken on dissecting the Body of A. M. a Maid of about 30 *Years of Age, who died of an Ascites, the* 1*st of August,* 1689. *By Mr. Daniel Turner.* N° 207, p. 15.

We made a perforation of the abdomen, in the most prominent part, as the body lay along, by a paracentesis, extracting through a small canula about 3 gallons. Afterwards we made an incision between the umbilicus and the cartilago ensiformis, dilating still as we emptied, till we had made room for a quart-pot, with which we drew out to the number of 76 quarts, including the 3 gallons which were extracted before, of a subsaline and somewhat austere serum, which amounts to the quantity of 19 gallons, besides what was imbibed with our sponges, probably about 2 quarts more.

While we were emptying this liquor we could perceive none of the viscera fluctuating in the same, which is common with most hydropic bodies. I remember it was the fear of this that once retarded an eminent physician from a paracentesis upon his patient, lest the surgeon in making perforation should puncture the intestines. After drying up the residue of this humour, which in colour and consistence somewhat resembled water, wherein flesh newly killed had been washed, saving that it was of a somewhat deeper red, and had a more crass hypostasis, we plainly perceived that this whole bulk of water was sustained between the cutis and peritonæum, by which there was so great a compression of the intestines and other viscera to the vertebræ of the loins and os sacrum, that it is surprising how there could be any protrusion of the excrements; since of necessity, by the aforesaid compression, the peristaltic motion must be very slow, if any at all; and 2dly, the musculi recti of the abdomen, which are said to be subservient to that necessary excretion, were not only at a very considerable distance, but even quite obliterated, or at best not to be distinguished from the carnous pannicle or common integument of the body; when at the same time the outward covering or cutis itself, notwithstanding so extraordinary a dilation, was full as thick as in a sound body, and in some places much thicker; particularly in the hypogastric region, where the membrana adiposa was observed to be above 2 inches thick, and seemed to be no other

* The original communication is accompanied with engravings intended to represent the appearances above-described, both in the woman who was executed and in the sow; but they are here omitted, not only because the engravings themselves are remarkaoly bad; but also because the description of the appearances is (to anatomists) sufficiently intelligible without them.

than a congeries of little bladders, each of them contained in its proper capsula, and filled with a coagulated lymphatic juice.

The thighs, legs, and feet, were anasarcous to an unusual degree; but the upper parts, as the neck, face, arms, &c. were extremely emaciated. After dividing the peritonæum, the first thing that should have occurred is the omentum; yet in this body it was really and absolutely wasted away. The intestines did not appear vitiated any other ways than in their colour, which was somewhat pallid, and indeed the internals for the most part, as the ventricle, pancreas, liver, spleen, kidneys, &c. looked all of them like flesh half boiled, and the blood absorbed. The intestines were all of them distended with flatus, particularly the cæcum, to a very considerable degree. In the 2 lowermost of the great ones, viz. the colon and rectum, some of the excrements were contracted like little balls, &c. The liver, which by some is adjudged to be particularly and principally affected in this disorder, was not more faulty than the rest of the bowels. The spleen adhered to the peritonæum; its colour and appearance resembled that of the other bowels, except that it was somewhat more livid.

On dissecting the kidneys we discovered nothing, either in the carunculæ papillares, or infundibulum, that could be any impediment to the secretion of the serum sanguinis, in case an attempt had been made upon those parts by a crisis. The bladder was empty, and did not seem capable of any great distension. At its upper end, a little inclining to the left side of the rectum, I perceived the uterus, almost 3 inches in length, and about 2 in breadth, which seemed to be nothing but a little carnous substance. The stomach contained nothing but wind, with which, like a blown bladder, it would return after the least impression. The diaphragm was forcibly impelled upwards into the chest, so that its dilatation must needs be very obscurely assistant to respiration; it was indeed so far contracted, that its convex part bore hard against the lobes of the lungs, whose substance was much decayed, and looked like par-boiled flesh. On cutting open the heart it did not yield the least drop of water or blood more than the rest of the bowels; even the liver itself, which has been by some accounted the store-house of the bloody mass, was destitute of so much as might be thought necessary for its own proper nourishment, and yet its salino-sulphureous particles, which constitute the gall, were deposited into the vesica bilaria, to the quantity of about a spoonful.

An Account of an uncommon Case of Dropsy within the Tunics of the Uterus. By Mr. Turner. N° 207, p. 20.

A woman aged 44 and upwards, some time after she was married, had conceived, as she thought, by some supposed symptoms of pregnancy; and in order to her delivery, at the expiration of the time of her account, her fancied pains came on, and she thought herself very near her labour. Her belly was grown very large, and had gradually increased from the time of her imagined conception; but her illness shortly after wore off, without leaving any prognostic of an approaching birth. Continuing, however, to increase in size for many months longer, after taking many medicines, &c. for about 3 years she removed into the air, where she soon after langushed and died.

On making a crucial incision to open the body, from the navel to the ilia and os pubis, one of the dissectors cutting through the peritonæum, accidentally thrust his knife too far, and immediately there issued out, with impetuosity, a stream of a limpid serum or lymph, as clear as water, rising up a considerable height, to the quantity of more than 2 gallons, enclosed in a thin transparent membrane, which was found to be the external coat of the uterus.

One of the great indications of this woman's pregnancy was a flux of a whitish humour to her breasts, which she could squeeze out at pleasure, and thought it to be the milk generated there as usual. She had likewise laboured for the greatest part of the time under a suppression of the menses.

On the Bite of a Mad-Dog. By Mr. Turner. N° 207, p. 24.

There was brought to us for cure a child of about 3 years of age, who had just then received a large wound on the masseter muscle by the bite of a dog. The wound was treated with digestives for some time, sutures were forborn, though otherwise necessary, that the venom might be more freely discharged. In a short time there was a discharge of a very laudable pus, and the wound incarned as fast as we could desire, without any inflammation or bad appearance. Three weeks after this, the child was seized with a fever, a disorderly pulse, and palpitation of the heart: the ensuing night he grew delirious, and the succeeding day the malignity had made so virulent an impression on the animal spirits, as excited strong irritations in the members of the body; nor was the brain, and its parts, freed from the same morbid taint, which showed itself in an unusual distortion of the eyes, and an extraordinary fierceness in the whole visage, continual watchings, and a constant trembling with a reiterated snatching up of the lower jaw, making signs as if he would have bit at any thing that was offered him. His voice was uttered with a canine hoarseness, and much resembled the barking of a dog. He was also invested with a hickup, and a foaming at the mouth. Thus he continued for most part of the day. On presenting a looking-glass before him, he was extremely disturbed at it, throwing his head backwards with great violence, barking, and snapping at every thing near him: in the evening, notwithstanding all the alexipharmics that were exhibited, he sunk under the oppression of those cruel symptoms. The body was not suffered to be opened. But the abdomen was perceived to be excessively inflated, his limbs convulsed, and the surface of the body of a livid colour; the muscles of the face were contracted into such a form as nearly represented a spasmus cinicus.

Solution of the Florentine Problem, concerning the Testudo Veliformis Quadrabilis. By David Gregory, M. D. and F. R. S.* N° 207, p. 25. *Translated from the Latin.*

The author of this enigmatical problem has now given an ingenious and easy construction of it, in an Italian treatise on the formation and measurement of all vaults and cupolas, dedicated to the Grand Duke of Tuscany; and there giving the initials of his name V. V.† the last disciple of Galileo. But here the author changes the enigma into the following problem: "On the surface of a hemisphere to assign a portion equal to a given square." Which problem he thus constructs:

Let a sphere, whose axis is equal to the side of the given square, be denoted by the circle ACBD (fig. 7, pl. 14) which is vertical in the proposed sphere, the horizontal diameter being AB, and the centre E. Let the sphere be perforated by two right cylinders, whose sections with the plane ACBD are the circles AHEI, BLEG, on the diameters AE, EB: then the thing is done; that is, from any hemisphere, as for instance the upper ACB, four bilinear figures are taken away by the perforating cylinders, two on the anterior side, and two on the posterior, which are similar and similarly posited, so as that the remaining hemispherical superficies is equal to the square on the line AB. And because the hemispherical superficies, when the said 4 bilinear spaces are taken away, resembles a sail filled and extended by the wind, and also a hemispherical cupola admitting light by 4 windows, which being placed on a circular base AEB, rests on it at the points A, E, E, B, this he calls "the quadrable Florentine and veliform cupola." The author then delivers several things re-

* See N° 196, p. 479, of this volume of the Abridgment.

† Vincent Viviani, mathematician to the Grand Duke, was born at Florence in 1622, and died in 1703, in the 82th year of his age. He was an excellent mathematician, and particularly excelled in the branch of the ancient and pure geometry, on which he left some ingenious specimens: viz. 1. "De Maximis et Minimis Geometrica Divinatio; &c. in fol. 1659;" in which Viviani not only guessed what Apollonius had written, in a work of his which is lost, but also extended the subject much further. 2. "Enodatio Problematum universis Geometris propositorum à Claudio Commiers, in 4to, 1677." 3. "De Locis Solidis secunda Divinatio Geometrica in quinque libros injuria temporum amissos Aristeæ senioris Geometræ, in fol. 1701:" a work full of deep rem rks on conics, &c.

lating to the practice of such buildings, as how, by means of the lathe and cylindrical auger to make models of this, as well as of five other cupolas; and for this purpose he constructs some other curious problems; the demonstrations of all which are omitted by the author, but will easily appear from what is delivered below.

It is manifest that the four windows in the hemisphere, constructed as above, are figures that are equal, similar, and alike placed: it only remains therefore to show that the remaining part of the hemispherical surface is capable of a true geometrical quadrature. Now at the point E, conceive a line, equal to A E, to be erected perpendicular to the plane A C B D; and on the periphery A C B D let there be an erect cylindrical surface of the same height. It is well known, that a portion of the spherical surface contained between any two planes parallel to the circle A C B D, is equal to the portion of the cylindrical surface between the same planes; and that like portions of these rings, cut off by planes mutually intersecting in the perpendicular erected at E, are also equal. Now by drawing innumerable planes parallel to the base A C B D, if in the cylindrical surface parts be conceived to be described in the aforesaid manner, equal to the corresponding spherical parts, that which is represented by the perforation of the superficies, and taken away from the opposite side, is equal to it. Hence it appears, that the remaining surface after the perforation, is equal to the remaining cylindrical surface, excepting that which is determined by the said innumerable planes, and opposite to that which is taken away. Let there be drawn then any diameter P M, cutting the periphery A H E in any point H; join A H, and through H draw R T perpendicular to A B, and parallel to C D drawn through E, meeting the periphery A C B D in R and T, and the periphery A H E in I. On the diameter R T describe a semicircle, cut in S and Q by H S and I Q perpendicular to R T; and conceive the plane of this semicircle to be erected perpendicular to the circle A C B D. Whence the periphery R S Q T will be in the hemispherical superficies, and the right line H S, now perpendicular to the plane A C B D, will be the latitude of the perforating cylindrical superficies above the point H of the base. And the same for every point of the perforating cylindrical surface, viz. that its latitude to the surface of the sphere above any point H in the base, is the right line H S drawn as above. But H S is equal to H A, the sine of the arc A M, because each of them is a mean proportional between M H and H P, the one in the circle M A P, the other in a great circle of the sphere passing through the points M S, and P.

If, in the perpendicular to the plane A C B D erected at E, from E there be taken a right line equal to H S or H A, and from its extremity there be drawn lines parallel to P M and V N; then the plane drawn through them will be parallel to the plane ACBD, and these lines will pass through the points S and Q, and being produced as far as the cylindrical surface circumscribing the hemisphere, will cut off from the sides of the cylinder right lines, which will likewise be equal to HS or HA, and they will contain arcs equal and corresponding to the arcs MN and VP. Now if another plane, parallel and very near to this, be conceived in like manner to be drawn, it appears, by what is above shown, that these two will design a portion of a ring in the cylindrical surface, equal to the portion between the same planes, which is taken away from the hemispherical surface by its perforation. Now if the same construction be supposed to be made at every point in the periphery AHE, all the portions in the cylindrical surface circumscribing the hemisphere, drawn and designed as before, will be equal to the spherical surface taken away by the perforation. Therefore the remaining hemispherical surface will be equal to the remaining cylindrical surface, composed of all the right lines HA, erected at the respective points M, N, V, and P, or to the figure of the sines of the semiperipheries ACB, ADB, that is, by what has been long known to geometricians, to 4 times the square of the radius AE, or to the square of the diameter AB. And since the two whole figures contained by the common section of the said perforating cylindrical surface with the spherical surface, are equal, the remaining hemispherical surface, after taking away the 4 bilinear spaces as in the construction, is equal to the square of the diameter AB. QED.

If the semiperiphery AHE be so inflected, that it may coincide with the equal quadrantal periphery ARC; the point H will fall on the point M, because of the equal arcs AH, AM; and HS the altitude at H, of the cylindrical surface insisting on AHE, will coincide with its equal HA, the altitude at M of the figure of sines erected on AMC; and the same thing holds good in all other points: hence the curve which is the common intersection of the spherical surface with the cylindrical surface on the base AHE, although not inflected in the same plane, in the manner before said, yet will coincide with and therefore be equal to the curve terminating the figure of the sines; that is, to the common section of the cylindric surface erected on the quadrantal arc ARC with the plane cutting the plane of the base erected on AB at half a right angle; or to a quadrant of the elliptic curve, whose less axis is AB, and its greater axis double in power of the same. Therefore the perimeter of the quadrable Florentine sail, consisting of 4 such arches, is equal to the perimeter of the said ellipse.

It may also be added, that the surface of two perforating cylinders in the sphere, are equal to the spherical surface remaining after the perforation, or to double the Florentine sail, that is to double the square of the diameter. And this appears from hence, that the Florentine sail is equal to 4 figures of the sines of the quadrant, and the perforating furface is also equal to the same, because it is congruous with it if inflection be made as above.

Further, I shall only add, that the consideration of the figure of the sines (the parts also of which are easily changed into squares) is sufficient for the demonstration of all those things, which are delivered concerning the other solids wrought by the turning-lathe, or perforated by a cylinder, and of their surfaces, by the very acute geometrician V. V. (Vincent Viviani, if I am not mistaken) the very worthy disciple of Galileo, when he treats of the construction and measurement of vaults or cupolas; particularly that the surface of the Roman boat-like cupola consists of 8 figures of the sines of the quadrantal arc, and therefore is equal to the Florentine veliform cupola. Hence it appears how two cupolas may be constituted on equal squares, one of which is inclosed on all sides, the other perforated by windows, each of which is double the square of the base.

An Anatomical Observation respecting a Stone in the [left] Kidney. By Dr. Wittie, F. R. S. N° 207, p. 30. *An abridged Translation from the Latin.*

A lady 31 years of age, had long laboured under loss of appetite and bad health. She not only loathed her food, but whenever she swallowed any, it was immediately returned by vomiting; while at the same time there was a copious discharge of green bile by stool. Some relief was procured from tonic medicines, so that the patient was enabled to take an airing every day for several months; after which she relapsed into her former sufferings, and moreover conceived a strong aversion for all those remedies from which she had before experienced relief; so that the disease was now left to take its course without any assistance from medicine. Indeed I was aware that the abovementioned symptoms were occasioned by a stone in the left kidney, and accordingly I predicted what would happen. In July she went to Epsom for change of air, and to drink the waters there; but after she had been there 2 months, all her symptoms grew worse; insomuch that she could not be recruited either by cordials or food, and was unable to lie on either side, on account of the pain. She died on the 28th of Jan. 1680.

On opening the body the next day, the lungs were found in a state which indicated an approaching phthisis. The stomach was distended to an unusual degree, resembling a blown up bladder; whence the vomiting might be accounted for. The heart was remarkably small, and flaccid or collapsed, like an empty purse; its substance having been softened and worn away (absorbed) by the constant hectic fever with which the patient had been affected. Hence the undulating pulse, which indeed towards the last could scarcely be felt. The liver was surprisingly enlarged, occupying not only the right hypochondrium but the left also; while the spleen was smaller than usual. Moreover there were strong adhesions of the liver, both in the right and left side, so that it could not be detached without much trouble and violence. Hence the difficulty under which the patient laboured of lying upon either side, as well as the irritation of the stomach from the pressure thus occasioned. Not only was that portion of the liver which filled the right hypochondrium provided with a gall bladder, but likewise that which occupied the left, i. e. there were 2 gall-bladders, both turgid with bile. The right kidney was in every respect natural: but out of the left kidney (which I had all along pronounced to be the seat of the disease that would sooner or later prove fatal) we took a stone that was curiously contorted, weighing $\frac{1}{2}$ an ounce, as white as chalk, and divaricated into 3 branches which were joined together in the middle. Hence the excruciating pains with which the patient was afflicted; hence, too, the vomitings, unless indeed they should rather be attributed to the 2 gall-bladders, pouring out a redundancy of bile. That the frequent purgings were owing to the last-mentioned circumstance, cannot be doubted.

An Account of Books: viz.—1. *Osservationi Naturali, &c. Natural Observations, containing several Medico-Physical and Botanical Matters, with divers Natural Productions, several sorts of Phosphori, Subterraneous Fires in Italy, and other curious Subjects, in Familiar Letters. By Paul Boccone,* M. D. Bononia,* 12*mo.* 1684. N° 207, p. 33.

This miscellaneous treatise of the ingenious Boccone, written in Italian, contains 26 observations. The author had formerly published in French, Natural Inquiries and Observations on Coral, Astroites, &c. at Paris, in 1671, reprinted at Amsterdam in 1674; and at Oxford his Icons and Descriptions of the Rare Plants of Sicily, &c. were printed in 1674.

In the first of the observations contained in this tract the author treats of the effects, causes, and preparation of the noctiluca, or phosphorus aëreus, as it was made by Mr. Boyle. The 2d observation is of subterraneous fires, and their phænomena and cause, from the effervescence of an acid and alcali, with an ethereal or subtile matter interposed. The 3d observation is of cures and preservatives from the plague, where he so much extols vesicatories, which in the year 1656 preserved as many as made use of them, when the plague raged at

* This Italian naturalist was born at Palermo in 1633. He travelled through most of the southern parts of Europe, in pursuit of Natural History, and died in 1704.

Genoa. In the 4th observation, he discourses of the iron rings made of the nail (taken out of a stone-horse's near foot before) by hammering only, without fire, as good for the cramp and giddiness, worn on the ring-finger, &c. The 5th observation contains a catalogue of natural effects usually attributed to sympathy. The 6th observation treats of a succedaneum for crabs'-eyes, &c. The 7th observation is of several rare plants, &c. The 8th observation describes a certain smoking liquor made of mercury and tin. The 9th, discourses of yellow and black amber, found in divers places of Italy. The 10th observation is of the bed of a certain insect, found in the midst of an oyster-shell in Mount Marius. The 11th observation is of several animalcules found in little shelly tubes. The 12th treats of the virtues of several plants for divers diseases of beasts. The 13th is of a certain man that after his wife's death suckled his child at his own breasts, in 1633, which he confirms by a credible testimony. The 14th observation gives an account of the several museums or repositories of curiosities to be seen in Italy. The 15th observation discovers the author's method of preparing the Bononian stone or phosphorus; to which is subjoined a curious observation communicated to him by Joh. Baptista Martelli, concerning human urine. The 16th observation enumerates several earthy concretes, good for absorbing and correcting acids. The 17th observation defends the harmless use of antimony, much esteeming the panacæa made of crude antimony with fixed nitre, described by Fr. Lana, in his Prodromus. The 18th observation discovers the wonderful texture of the root of the perfoliata alpina latif. min. Bauhini, made up of many membranes curiously complicated together. The 19th observation is concerning a spring, called Pliny's, and the river by the inhabitants now named Torbidone; which fountain in April, An. 1680, in a quarter of an hour rose 3 inches perpendicular, and sunk down again in half an hour's time, repeating this flood and ebb alternately every two hours; which phenomenon was observed not two days, as is usually there thought, but only a few hours before a shower of rain. In the 20th observation he shows that the musky smell coming from some pismires and flies of Pisa, proceeds only from sulphureous particles mixed with a volatile salt in the dung of those little animals. In the 21st he gives 4 reasons why some plants are green all the year. The 22d observation describes a peculiar sort of insects, which he found on the leaves of the myrtus tarentina at Rome, An. 1678; they were of a grey colour, and a convex figure, like the $\frac{1}{2}$ of a hemp seed, and by the microscope showed scaly or plaited like the millipedes; being rubbed and bruised on a paper, they gave it a curious purple colour; whence he conjectures they are akin to the cochineal insect. In the 23d observation he adds some other reasons of the continual greenness of some plants, which, though defended with but a thin skin, yet resist the cold of the winter. In the 24th observation several prodigious effects of sulphureous exhalations in divers parts of Italy, are related. In the 25th observation he adds some more thoughts concerning the sal ammoniac he found on iron scoria, thrown up by Ætna in the year 1669, to those he had formerly published in Disquisit. et Observ. Physicis circa Corallium, &c. edit. Amst. p. 47; answering a question proposed by Menzelius about the Bononian stone. Lastly, in the 26th observation he asserts the great virtue of a mineral bezoar found in Sicily, confirming it with the cure of an epidemic fever, &c.

2. *Lezioni intorno alla Natura delle Mofette, &c. Discourses concerning the Nature of Damps. By Leonardus Capuanus. Naples, in 4to.* 1683. N° 207, p. 38.

The nature of the several damps or subterraneous vapours here treated of was little understood at this period of time. It is now known that they are for the most part referrible to two classes, the inflammable and uninflammable; the former consisting of hydrogen gas, the latter of carbonic acid gas. In some volcanic caverns there are also sulphureous vapours.

Experiments made with Mr. John Colbatch's Styptic. By Mr. Wm. Cowper, Surgeon.* N° 208, p. 42.

A large dog being provided, an aperture was made through the common integuments of his abdomen, whence the small guts were extruded; after an incision made in one of them, according to its length, they were again reduced; the wound in the abdomen being stitched up, a solution of this powder was applied; the dog continued without any ill symptoms, and became perfectly

* Wm. Cowper was a first-rate surgeon and anatomist of the 17th century. He published two splendid works; viz. a treatise on the muscles entitled Myologia Reformata, first published in 8vo, 1694, and afterwards re-edited with augmentations by Dr. Mead, in folio, 1724; and the Anatomy of Human Bodies, folio, 1697. It has been already mentioned that the plates of this last work were for the most part taken from Bidloo (see the account of that Dutch author, p. 260, of this vol. of these Abridgments); though Cowper in his reply, entitled Eucharistia, wished to make it appear that those plates were Swammerdam's not Bidloo's. Besides these works he furnished several communications to the Philosophical Transactions, relative to chirurgical and anatomical subjects. Cowper dissected a vast number of bodies, and was remarkable expert in making injections of the blood vessels, lacteals, and lymphatics. He exhibited preparations of the bronchia filled with bismuth. He was the discoverer of certain glands situated in the urethra, and which have since gone by his name. He moreover bestowed considerable pains on comparative anatomy, as appears from several of his papers inserted in the Philosophical Transactions, containing dissections of the opossum, &c. He died in 1710.

well in a few days after. The like experiment was also made on another dog, which in like manner recovered, without the application of any medicine.

The leg of a dog was amputated 3 inches above the patella: the expence of blood from the arteries was great, which partly proceeded from the unaptness of the applications; but after two or three attempts, the flux of blood was stopped, and such a bandage made use of as was necessary only to keep on the dressings: the dog continued without any considerable flux of blood, and the next day he was found on his three legs. This experiment raised expectations of the like success on human bodies: therefore it was tried on a man in St. Bartholomew's Hospital, whose diseased arm was amputated above the elbow; but for above a quarter of an hour's time, many successless applications of this styptic were made, and at length a small tent dipped in the powder itself, was inserted into the extremity of the bleeding artery, before the flux of blood would admit the application of bandage. Five hours after, a fresh flux of blood appeared, and a strict bandage was applied. The same morning a boy about 12 or 14 years of age, had his leg taken off below the knee, and to the stump divers successless applications of this styptic were also made, before it was bound up, and in less than an hour after, a fresh flux of blood happened, and a strict bandage was added. Some hours after these operations, both these patients suffered intolerable pains: three days after, the applications were taken off, and had any person, a stranger to what had been done, seen the stumps, he would have supposed nothing less than an actual cautery had been applied, or could have occasioned such large eschars, and so horrid an appearance; which sufficiently proved that this vulnerary powder is a voilent caustic.

Trials of styptics on the bodies of quadrupeds have been commonly practised, to recommend them to the public; but it is not without cause that pretenders to such remedies have made choice of younger animals, as dogs and calves, &c. for that purpose. But since the only standard of their use is their success on the human body, we ought to make our experiments on those animals whose size and age bear a proportion to it: for nothing is more obvious in wounding the arteries of living animals, than that the protrusion of their blood bears a proportion to their bulk: and in dissection, the arteries of the fœtus are remarkably thinner than those of an adult: but those of aged bodies grow still thicker, and frequently become cartilaginous, and at length entirely bony.

Account of Locusts in Wales: in a Letter from Mr. Edw. Floyd, at Oxford, to Dr. Lister. N° 208, p. 45.

You have probably been already informed from some other parts of the kingdom, of swarms of locusts that have lately appeared on our British coasts. They were first seen in Wales about the 20th of October, scattered about the fields in Marthery parish, Pembrokeshire. In North Wales, two vast swarms of them had been seen in the air, not far from Dôl gelheu, a market-town of Merionethshire, and about the same time that those others in Pembrokeshire had been observed in the fields. They are of the very same species with some African locusts in my custody, called the pilgrim locust.

It is in length, from the head to the tips of the wings, 3 inches and $\frac{1}{4}$, of a reddish colour all over except the wings; the eyes are prominent and very large, somewhat of the form and size of gromwell seeds, of a reddish colour, elegantly streaked: the antennæ are about the thickness of a hog's bristle, and curiously geniculated: the first pair of legs are not quite an inch long; the second somewhat more; but the third 2 inches and $\frac{1}{4}$: these hind-legs are very beautiful; for the thighs are hexangular, and elegantly scaled on the outside, with a black list, extended lengthways through the middle of them; the shanks are of a lively red colour, adorned on the hind-part with two orders of small sharp prickles, placed not opposite to each other, but alternately: the wings are about 3 inches long, resembling very much those of the larger libellulæ, or dragon-flies, but all over garnished (the outer wings at least) with reticulated black spots. I see little reason to doubt but that these are the very same species of locusts, so famous in history for their wandering over, and depopulating whole countries.

Extract of another Letter to the same Purpose, from the same. N° 208, p. 48.

Oxford, Feb. 20, 1693-4.—I here send you a note out of a MS. intituled, The History of Pembrokeshire. It was written about the year 1603, by one Mr. George Owen, a gentleman of that county, who seems to have been a person of considerable accuracy and veracity. The extract to this effect: About the beginning of June, in the year 1601, a piece of ground, of about 200 English acres, was suddenly covered, as if the same had fallen in a shower out of the air, with a kind of caterpillars or green worms, (insects ?) having many legs, and bare without hair. They were found in such abundance, that a man could not tread on the ground, without crushing 20 or 30 of them: being opened, there was nothing found within them but grass. The place was on a hill in the parish of Maen-clochog, above Hynnon Dhewi: they went as it were with one accord up the hill, and over the same a quarter of a mile and more: in their way they devoured and consumed the grass, that the ground appeared bare and red like fallow. After they had continued three weeks, there resorted thither an infinite number of sea-mews and crows, which in a few days consumed them all: the swine also fed upon these worms (insects?) eagerly, and became very fat, &c.

On the burning of several Hay-Ricks, by a Fiery Exhalation or Damp: and of the infectious Quality of the Grass of several Grounds; from the same. N° 208, p. 49.

I am wholly intent at present on giving you the best account I can of a most dismal and prodigious accident at Hartech in this county (Pembrokeshire), from the 24th to the 30th of Dec. 1693. It is of the unaccountable firing of 16 ricks of hay, and two barns, one full of corn, the other of hay. I call it unaccountable, because it is evident they were not burnt by common fire, but by a kindled exhalation, which was often seen to come from the sea and lasted at least a fortnight or three weeks; and annoyed the country, both by poisoning the grass and firing the hay, for the space of a mile. It was a weak blue flame, easily extinguished, and did not in the least burn any of the men who interposed their endeavours to save the hay, though they ventured not only close to it, but sometimes into it. All the damage sustained happened constantly in the night. There are three small tenements in the same neighbourhood, where the grass is so infected, that it absolutely kills all manner of cattle that feed on it. The grass has been infectious these three years, but not thoroughly fatal till this last.

Jan. 20th, 1693-4.

Part of a Letter to Dr. Clopton Havers, S. R. S. giving an Account of an extraordinary Hæmorrhage at the Glandula Lachrymalis. N° 208, p. 51.

Since my coming to this place I have met with a very strange case. An hicterical discontented woman having a desire to die, wholly rejected the help of medicine, and within 3 months being near her end, there happened an eruption of blood out of the glandula lachrymalis of one of her eyes, without any external injury; there was an evacuation of 2 lb. of blood within the space of 30 hours. About a week after the same sluice was opened again, and she bled till she died.

On the Quantity of falling Rain. By R. Townley, of Townley in Lancas he, Esq.* N° 208, p. 51.

To collect and measure the quantity of rain, I fixed a round tunnel, of 12 inches diameter, to a leaden pipe, which would admit of no water, but what came through the tunnel, by reason of a part soldered to the tunnel itself, which went over the pipe, and served also to fix it to it, as well as to keep out any wet that in stormy weather might beat against the under part of the tunnel, which was so placed, that there was no building near it that would give occasion to suspect that it did not receive its due proportion of rain that fell through the pipe about 9 yards perpendicularly, and then was bent into a window near my chamber, under which convenient vessels were placed to receive what fell into the tunnel; which I measured by a cylindrical glass at a certain mark, containing just a pound, or 12 ounces Troy, and had marks for smaller parts also. By the help of this cylindrical glass I thus kept my account of what rain fell, and generally twice or thrice a day; when I took several other observations, both of the thermometer, barometer, winds, &c. what rain I found in the receivers, if not more than made what was left in the cylindrical glass a full pound, I again left in it; but if there was more than that quantity I filled it just to the pound mark, which I threw away, and did the like with the remaining water as often as it would allow, still keeping an account chiefly of the pounds thrown away, and noting also the parts of a pound remaining in the glass; by the help of which latter, and the parts remaining at any time before, by numbering the pounds and substracting the parts at the end, for example, of one month, from the pounds thrown away, and the parts remaining at the end of another, I find the quantity of rain fallen between these two times, and that so as to assure me that I erred no more in the quantity of rain of another year than by the mistake in the differences of the parts of a pound in the first and last observation, whereas should I always write down the rain that falls between two observations, I might be subject to make as great a mistake in every one of them, and consequently be much more uncertain of the quantity of rain fallen in many of those added together; besides this addition is longer in performing, and giving the quantity sought, than the method I make use of. It appears that here we have almost twice the quantity of rain that falls at Paris.

* Mr. Townley was an ingenious philosopher, and a useful contributor to the Royal Society from the beginning; but we are not informed of any memoirs of his life. His micrometer for the telescope has been often mentioned; and some ingenious papers of his were printed in several volumes of the Philosophical Transantions, particularly relating to eclipses, to the division of a foot into many thousand parts, to the quantity of rain, to the winds, the weather, the barometerl &c,

This county, and particularly that part of it where I live, is generally esteemed to have much more rain than other parts; for by reason of the very high grounds in Yorkshire and the eastern parts of Lancashire, the clouds driven hither by the S. and S. W. the general winds in this part of the world, are oftener stopped and broken and fall upon us than such as come by an E. or S. E. wind, which broken by the hills are generally spent there, and then little affect us; and this is the reason that Lancashire has often considerably more rain than Yorkshire.

The Table of Rain.

	1677	78	79	80	81	82	83	84	85	86	89	90	91	92	93	Sum
Jan.	472	371	043	512	053	986	238	032	110	472	333	707	197	054	218	4798
Feb.	270	371	161	492	363	135	245	483	042	020	393	171	112	168	078	3504
March	245	250	202	413	235	237	305	087	185	572	875	145	476	342	298	4867
April	325	170	092	222	057	308	402	370	380	305	468	078	386	498	539	4600
May	313	581	105	188	069	315	353	097	201	437	182	244	300	330	093	3808
June	516	257	298	342	397	517	468	192	410	473	302	179	412	416	181	5360
July	351	339	350	302	292	482	412	313	497	188	120	218	285	448	112	4709
Aug.	485	145	835	502	425	385	582	338	398	870	222	402	193	198	668	6648
Sept.	223	527	553	146	607	293	152	199	163	572	442	403	215	605	641	5741
Oct.	333	644	616	570	170	427	330	425	325	293	740	765	165	273	514	6590
Nov.	432	555	127	479	235	525	192	579	522	709	415	717	230	148	627	6492
Dec.	400	057	439	269	423	456	037	299	548	132	368	262	169	892	261	5003
Sum	4365	4267	3821	4428	3326	5066	3716	3414	3781	5043	4860	4291	3140	4372	4230	62120

The above mentioned method of estimating rain by pounds to those of my family gave a sufficient idea of the proportions of the falling rains, and the wetness of the different seasons, though they knew not how high it would raise the water in a cylinder on a base equal to my tunnel; but to inform others of this with little trouble, it must be observed that in the table the pounds, and parts are doubled, and these give both the quantity of half pounds, and the height in inches, according to the general way of estimating the quantity of rain; only with this difference, that for the half pounds only the last figure is a decimal fraction, and the other the number of the half pounds; and for the height the last two figures denote the decimal fraction of an inch, and the remainder the height in inches, so near the truth, that they only fall short of it 1 inch in 200, which defect is easily supplied. To this I need only add, that the numbers on the right hand are the sums of all those in the same line of the whole 15 years; so that the last of them shows the sum both of the half ounces that have fallen during that space of time, and the height the water would have been raised in that time also. Thus, for example, the number for the sum of all the rain in the 15 years being 62120, therefore according to what has been said 6212.0 is the number of half pounds that fell in the compass of the tunnel during those 15 years, and 621.20 the height it would have raised the water during that time. But if you desire greater accuracy add 3.10, its 200th part, and you will have 624.30 for the true height, and 41.62 for the mean height by those 15 years observations; and 414.1 for the mean quantity of half pounds. My way of gauging by weight is grounded on 22.7368 cubical inches of rain-water being equal in weight to one pound, or 12 ounces Troy; so that dividing any superficies in inches, of a vessel for receiving the rain-water, by the before-mentioned number, it will give the pounds and parts that will raise the water on that superficies, with upright sides, just an inch; and thus I found that 4.974 pounds would fill a cylinder equal at the bottom to my tunnel, and 1 inch high, which is nearly 5 pounds, and which will only raise the cylinder higher by $\frac{1}{200}$th part.

P.S. It is remarkable the great harmony there is between the mercurial standards at London and here at Townley; for by a whole month's observations, Mr. Flamsteed was pleased to send me, the mercury always rose and fell both there and here exactly at the same time. I always found it rather more than $\frac{3}{10}$ of an inch lower here than at London, because though we are seated in a seeming valley, in respect of the neighbouring grounds, yet we are considerably higher than the other low lands near the sea, where the standard differs little from that at London. I suppose you may not be displeased with two remarkable observations, made both by Mr. Flamsteed and me at the same time, viz. Nov. 18, 1674, when finding the mercury to descend both very fast and very low, we watched it very nicely, and both of us observed that at 2 in the afternoon it was rather falling, and rather rising at 4, at which times the height was here only 27.63 inches, and at London $\frac{3}{10}$ths higher.

A Case of Lumbago Rheumatica Convulsiva. By Robert Pitt, M. D. F. R. S. Extracted from the Latin. N° 208, p. 58.

A man aged 35, after taking cold was seized with feverish symptoms, which terminated in a most violent pain in the loins and hip. The pain was so acute as to throw him into profuse sweats, which ran down in drops from his hair and face. He could neither lie down on a bed, nor stand upright, nor sit in a chair, but to assuage his pain had recourse to pressing his belly against the bed-stead. Yet he could not long remain even in this posture; but, being seized with convulsions, would fall down on the floor if not prevented by those who were present. After the first convulsive seizure he would remain motionless, and in regard to his limbs as helpless as an infant. His arms and legs would become inflexible, and the mouth so closed, that it would be scarcely possible to get a spoon into it. These convulsions returned by paroxysms, but not periodically, sometimes at one time, sometimes at another, according to the urgency of the pain. When replaced in his favourite position, in which he could mitigate his pain by pressing his abdomen against the bedstead, he appeared for the time quite composed. In this manner the patient went on for about 3 weeks, never lying down in bed, and having scarcely any sleep all that time. He had no vomiting; his pulse was strong and equal; his tongue white, but moist; the blood which had been drawn was like that of pleuritic patients; the urine of a natural appearance. Before Dr. Pitt saw him, which was after the first week, he had been blooded once; one clyster had been administered: one dose of a purgative had been given, and plenty of laudanum; but all to no purpose. Dr. Pitt ordered the blood-letting to be repeated in large quantities, and a purgative medicine the next day; after the operation of which the patient was able to walk upright. The pains and convulsions returning again, the doctor prescribed a strong cathartic of resin of jalap, and mercurius dulcis, with a draught of syrup of buckthorn; which, however, produced no effect. The dose of the jalap and mercury was therefore increased, and the syrup of buckthorn (in the quantity of 3 or 4 ounces) was repeated every other day. By this method the bowels were at length acted upon, and the pains and convulsions abated. Whey was ordered to be drunk ad libitum. The bowels being sufficiently evacuated, laudanum was given. Thus after 8 or 10 repetitions of the purgative medicines, the pains and convulsions entirely ceased; and the cure was completed by the exhibition of some nervous remedies (nervina medicamenta).

On the Bird the Cuntur, of Peru, and on the Coffee-shrub. By Hans Sloane, M. D. S. R. S. N° 208, p. 61.

The size and strength ascribed to the cuntur,* or condor, of Peru, have caused many to doubt of its existence. Capt. John Strong, commander of a ship which went into the South Seas, through the Straits of Magellan, gave me this account of it, together with a wing or quill feather of the bird: that on the coast of Chili they had met with this bird in about 33° S. latitude, not far from Mocha, an island in the South Seas; that his men were much amazed at its size, and that it measured 16 feet from one wing to the other extended. The feather he gave me is 2 feet 4 inches long, the quill part is 5¼ inches long, 1½ inch about in the largest part; it weighed 3 drams 17½ grains, and was of a

* Vultur Gryphus. V. maximus, caruncula verticali longitudine capitis, gula nuda. *Linn. Syst. Nat.* p. 121. See *Museum Levereanum*, vol. 1. p. 1. t. 1. and vol. 2. p. 1. t. 1.

dark brown colour, concave on one side, and convex on the other. The seamen shot it sitting on a cliff, by the sea side, and eat it, taking it for a sort of turkey.

Joseph Acosta, after describing the size of this bird much the same as above, adds, Nature to temper and allay their fierceness has denied them the talons which are given to the eagle, having their feet tipped with claws like a hen; but their beak is strong enough to tear off the hide, and rip up the bowels of an ox. Two of them will attempt a cow or bull, and destroy him; and it has often happened that one of them alone has assaulted boys of 10 or 12 years of age and killed them. Their colour is black-and-white like a magpye. They have on the fore-part of their heads a comb, not pointed like that of a cock, but rather even, in the form of a razor. When they alight from the air, they make a stunning and astonishing noise with their wings.

With respect to the coffee shrub* Mr. Edward Clyve, who was the first who brought a dried branch from Mecca, gives this account of it: The branch was taken off a tree 7 or 8 feet high, is about 5 feet long, and covered with a grey smooth bark; the wood is white, and the pith not very large; the twigs are covered with a dark coloured smooth bark, and arise opposite to each other by pairs, coming out of opposite sides of the branch, and cutting each other at right angles. After the same manner the leaves stand on the twigs as the twigs on the branches, at sometimes an inch and sometimes 2 inches distance from each other; the leaves have $\frac{1}{4}$ inch foot-stalks, being about 4 inches long, and 2 broad, in the middle, where broadest; whence they decrease to both extremities, ending in a point. They are smooth, whole, and without any incisures on their edges, somewhat like the leaves of a bay. The fruit comes ex alis foliorum, hanging or sticking to the twig by $\frac{1}{8}$ inch-long strings or foot-stalks, and sometimes 1, 2, or more at the same place. These shrubs are planted in Arabia Felix, called Jaman, in a rich mould, and are watered in times of drought by artificial channels cut on purpose from rivers; and that after 3, 4, or more years bearing, the inhabitants are forced to plant new shrubs, because the old ones are not so fruitful after that time. They dry them in the sun, and afterwards take off the outward husks of the berries by means of hand-mills; and the Arabians themselves in summer heats use these husks roasted after the manner of coffee-berries, esteeming the drink more cooling, it being sourish to the taste.

* Coffea arabica. C. floribus quinquefidis dispermis. *Linn. Spec. pl.* p. 245.

*An Account of Books, viz.**—*I. Martini Lister, Exercitatio Anatomica, in qua de Cochleis, maximè Terrestribus, et Limacibus agitur. Omnium Dissectiones Tabulis æneis, ad ipsas Res affabre incisis, illustrantur. Cui accedunt Digressiones de Respiratione, Generatione, Androgynâ, Sæpiâ, Loligine, et Polypo, aliisque Rebus naturalibus,* 8vo. 1694. N° 208, p. 65.

II. A Treatise of the Natural Grounds and Principles of Harmony. By Will. Holder, D. D. &c. Lond. 8vo. 1694. N° 208, p. 67.

The author observes, that sounds, the material part of harmony, are natural and physical; but the disposing of them so as to delight the hearing, is the formal part of it; both united make harmony complete; and it may be either in symphony, when many sounds are heard together, or solitary, where the ear finds the agreements of single, proceeding with subsequent harmonious notes. The physical reason of this pleasing agreement of sounds, is the design of this tract.

After treating of the several parts of this science in 8 chapters, in the conclusion he has summed up his whole doctrine: that is, bodies by motion make sound; sound of bodies fitly constituted make tone or tune; tune is acuter by swiftness, graver by slowness of vibrating motions. The proportions of these are best found by strings; if two vibrations be commensurate within the N° 6, they produce concords; if incommensurate, they make discords. Concords are limited, discords infinite.

It was not his design to meddle with the metric part of music, concerning composition, air, and humour: nor to treat of the receptive faculties of persons from the different constitution of their organs of hearing; nor to show why some have and others want musical ears; nor to treat of other curious speculations concerning music, which will afford the acutest philosopher business enough; but only to treat of this ground-work of all, the stating of the true proportions of musical tones, both harmonious and discordant, such especially as are usually comprised in the scales, and applied and used in music.

A Letter from Hans Sloane, M. D. and S. R. S. with several Accounts of the Earthquakes in Peru, Oct. 20, 1687; *and at Jamaica, Feb.* 19, 1687-8; *and June* 7, 1692. N° 209, p. 78.

These papers were 8 in number, mostly written by eye-witnesses of those dreadful events; abstracts of which are as follow:

* In this work of Lister, which is now become rare, the anatomy of several species of snails and slugs is accurately detailed, and is accompanied by many ingenious observations.

N° I. *Extract of a Letter from Father Alvarez de Toledo, a Franciscan Friar, dated Oct. 29, 1687.*— Oct. 20, (N.S.) at 4 o'clock in the morning, came on a dreadful earthquake and noise, by which some houses fell, and some persons were killed under their ruins. At 5 o'clock the same morning was another shake with the like noise. At 6 o'clock the aforesaid morning, when we thought we had been all in safety, came another shake, with great fury and rushing noise; the sea with great bellowings rushed beyond its bounds, the bells rang of themselves, and every building thrown down. Callao, Canete, Pisco, Chancay, and Los Chorillos, are all ruined. More than 5000 dead bodies are found; and more are found daily; so that their number is not known.

N° II. *By Dr. Sloane.*—The inhabitants of Jamaica expect an earthquake every year. Some are of opinion, that they follow the great rains. One of them happened on Sunday the 19th of Feb. 1687-8, about 8 in the morning. I found in a chamber, one story high, the cabinets and several other moveables on the floor to reel, as if people had raised the foundations of the house. Being in a high brick house, I made what haste I could to get out; but before I had passed through two rooms, and got to the stair-case, it was over. It came by shocks; there were three of them, with a little pause between. It lasted about a minute of time in all: and there was a small noise accompanied it. This was generally felt all over the island at the same time, or near it; some houses therein being cracked and very near ruined, others being uncovered of their tiles; very few escaped some injury. The ships in the harbour at Port-Royal felt it; and one who was eastward of the island coming thither from Europe, met with, at the same time, a hurricane. A gentleman being at that time abroad in his plantation, told me, he saw the ground rise like the sea in a wave, as the earthquake passed along, and that it went northward; for soon after he had felt it, he saw by the motion of the tops of the trees on hills some miles distant, that it had then reached no farther than that place. The Spaniards who inhabited this island, and those neighbouring, built their houses very low; and they consisted only of ground-rooms, their walls being made of posts, which were as much buried under-ground as they stood above, purposely to avoid the danger which attended another manner of building, from earthquakes. And I have seen in the mountains afar off bare spots, the effects of earthquakes throwing down part of the hills, which continued bare and steep.

N° III. *On the terrible Earthquake at Port-Royal, in Jamaica, June 7, 1692.*—The terrible earthquake which happened the 7th instant, between 11 and 12 o'clock at noon, shook down and drowned nine-tenths of the town of Port-Royal in two minutes time, and all near the wharf-side in less than one minute; very few escaped there. I lost all my people and goods, my wife and two men, Mrs. B. and her daughter. One white maid escaped, who gave me an account, that her mistress was in her closet, two pair of stairs high, and she was sent into the garret, where was Mrs. B. and her daughter when she felt the earthquake, and bid her take up her child and run down; but turning about, met the water at the top of the garret stairs; for the house sunk down right, and is now near 30 feet under water. My son and I went that morning to Liguania; the earthquake took us in the mid-way between that and Port-Royal, where we were near being overwhelmed by a swift rolling sea, 6 feet above the surface, without any wind; but being forced back to Liguania, I found all the houses even with the ground. The earth continues to shake 5 or 6 times in 24 hours, and often trembling. Great part of the mountains fell down, and fall daily.

N° IV. *From Jamaica, dated Sept.* 23, 1692.—We have had a dreadful mortality since the great earthquake (for we have little ones daily); almost half the people that escaped at Port-Royal, are since dead of a malignant fever, from the change of air, want of dry houses, warm lodging, proper medicines, and other conveniencies.

N° V. *Another Account of the Earthquake of June* 7, 1692.—Great part of Port-Royal is sunk; so that where the wharfs were, is now some fathoms of water: all the street where the church stood is overflowed, that the water stands as high as the upper rooms of those which are standing. The earth when it opened and swallowed up people, some rose in other streets, some in the middle of the harbour, and were saved; though at the same time I believe there were lost about 2000 whites and blacks. At the north about 1000 acres of land sunk, and 13 people with it; all our houses thrown down all over the island, that we were forced to live in huts. The two great mountains at the entering into Sixteen Mile Walk fell and met, and stopped the river, so that it was dry from that place to the Ferry for a whole day; and vast quantities of fish taken up, which was greatly to the relief of the distressed. At Yellows a great mountain split, and fell into the level land, and covered several settlements, and destroyed 19 white people. One of the persons, whose name was Hopkins had his plantation removed half a mile from the place where it formerly stood. Of all wells, from a fathom to 6 or 7, the water flew out at the top, by the great motion of the earth. Since then, it has continued shaking sometimes 2 or 3 times in a day. Our people settled a town at Liguanea side, and there are about 500 graves already, and people every day are dying still.

N° VI. *From the same Place, and on the same Earthquake.*—On Tuesday the 7th of June, 1692, between 11 and 12 at noon, at Port-Royal, we felt the house shake, and saw the bricks begin to rise in the floor. Immediately we ran out, and saw the houses swallowed up, or thrown on heaps. The sand in the street rose like the waves of the sea, lifting up all persons that stood upon it, and immediately dropping down into pits; and at the same instant a flood of water rushed in, throwing down all who were in its way; some were seen catching hold of beams and rafters of houses, others were found in the sand that appeared when the water was drained away, with their legs and arms out. As soon as the shock was over, I endeavoured to go towards my house, on the ruins of the houses that were floating on the water, but could not: at length I got a canoe, and rowed up the great sea side towards my house, where I saw several men and women floating upon the wreck out at sea; and taking in as many as I could, I rowed on till I came where I thought my house had stood, but could not hear of either my wife or family. Next morning I went from one ship to another, till at length I met with my wife and two of my negroes. She told me, when she felt the house shake, she ran out, and called all within to do the same: she was no sooner out, but the sand lifted up: and her negro women grasping about her, they both dropped into the earth together; and at the same instant the water coming in, rolled them over and over, till at length they caught hold of a beam, where they hung, till a boat came from a Spanish vessel, and took them up.

Several ships were over-set and lost in the harbour; and some thrown on the land. A hideous rumbling was heard in the mountains; so that it frightened many negroes that had been run away some months from their masters, and made them return, and promise never to run away again.

The water that issued from the Saltpans Hills forced its passage out from the hill in 20 or 30 several places; some with such violence, that had so many sluices been drawn up at once, they could not have run with greater force; and most of them 6 or 7 yards high from the foot of the hill; and the water was brackish. It continued running that afternoon, all night, and till next morning about sun-rise, at which time the Saltpans were quite overflowed.

The mountains between Spanish Town and Sixteen Mile-Walk, as the way lies along the river, about the mid-way they are almost perpendicular; those two mountains, in the violent shake of the earthquake, joined together, which stopped the passage of the river, and forced it to seek another, which was a great way in and out among the woods and savannas; and it was 9 days before the town had any relief from it: insomuch that before it came, the people were in thoughts of removing into the country, concluding it had been sunk, as Port-Royal was. The mountains along the river are so thrown on heaps, that all people are forced to go by Guanaboa to Sixteen-Mile-Walk.

The mountains at Galloes fared no better than those of Sixteen-Mile-Walk, a great part of one of them falling down, drove all the trees before it; and at the foot of the mountain there was a plantation that was wholly overthrown and buried in it.

N° VII. *Some more Particulars of the same.*—As to the mountains in Leguanee, they fell in several places, and in some very steep; but the steepest mountain that we heard fall, was that at Gallowes, which occasioned much damage. The water in the streets of Port-Royal did not spout up, as you have heard; but in the violent shake the sand cracking and opening, in several places where people stood, they sunk into it; and the water boiled out of the sand, that covered many, and saved others.

N° VIII. *Some other Particulars of the same.*—The year 1692 began in Jamaica with very dry and hot weather, which continued till May, when there was very blowing weather, and much rain to the end of the month, from which time, till the time of the earthquake, it was very hot, calm, and dry; and on Tuesday the 7th of June, about 40 min. past 11 in the forenoon, it being then a very hot and fine day, scarcely a cloud to be seen in the sky, or a breath of air to be felt, happened that great shake, so fatal to this place, and to the whole island, which for its violence and strange effects, may perhaps be compared with the greatest that ever yet happened in the world, and may as well deserve the memory of future ages.

It began with a small trembling, so as to make people think there was an earthquake, which thoughts were immediately confirmed by a second shake something stronger, accompanied all the while with a hollow rumbling noise, almost like that of thunder, which made them begin to run out of their houses. But alas! this was but short warning for them to provide for their safety; for immediately succeeded the third shock, which in less than a minute's time shook the very foundation of Port-Royal so that at least two parts in three of the houses, and the ground whereon they stood, and most part of those who inhabited them, all sunk at once quite under water: and on the place which was left, and is now standing, shook down and shattered the houses in so violent a manner, that at our landing, it looked like a heap of rubbish, scarcely one house in ten left standing, and those so cracked and shattered, that but few of them were fit, or thought safe to live in. All those trees which were next the water, towards the harbour-side where there were excellent wharfs, close to which ships of 700 tons might lie and deliver their lading, where were the best store-houses and conveniences for merchants, where were brave stately buildings, where the chief men of the place lived, and which were in all respects the principal parts of Port-Royal, now lie in 4, 6, or 8 fathoms water.

That part which is now standing, is part of the end of that neck of land which runs into the sea, and makes this harbour, and is now a perfect island; the whole neck of land from the port of Port-Royal now standing, to the pallisadoes, or other end of Port-Royal towards the land, which is above a quarter of a mile, being quite discontinued and lost in the earthquake; and is now also, with all the houses, quite under water. This part of Port-Royal which is now standing, is said to stand upon a rock: but alas! the strange rents and tearings of the mountains here sufficiently evince, that rocks and sand are equally unable to withstand the force of a violent earthquake. The ground heaved and swelled like a rolling swelling sea; by which means several houses now standing were shuffled and moved some yards from their places. One whole street is said to be twice as broad now as before the earthquake; and in many places the ground would crackle, and open and shut quick and fast: of which small openings have been seen 2 or 300 at one time, in some whereof many people were swallowed up; some the earth caught by the middle, and squeezed to death; the heads of others only appeared above ground; some were swallowed quite down, and cast up again by great quantities of water; others went down, and were never more seen. These were the smallest openings. Others, that were larger, swallowed up great houses; and out of some gapings would issue whole rivers of water, spouted up a great height into the air, which seemed to threaten a deluge to that part of Port-Royal, which the earthquake seemed to favour, accompanied with offensive smells, by means of which openings, and the vapours at that time emitted from the earth into the air, the sky, which before was clear and blue, was in a minute's time become dull and reddish, looking like a red-hot oven. All these dreadful circumstances occurring at once, accompanied with prodigious loud noises from the mountains, occasioned by their falling, &c. and also a hollow noise under ground, and people running from one place to another distracted with fear, made the whole so terrible, that people thought the dissolution of the whole frame of the world was at hand. Indeed it is melancholy now to see the chimnies and tops of some houses, and the masts of ships appear above water; and when one first comes ashore, to see so many heaps of ruins; to see so many houses shattered, some half fallen down, the rest desolated and without inhabitants; to see where houses have been swallowed up, some appearing half above ground, and of others the chimnies only; but above all, to stand on the sea-shore, and to look over that part of the neck of land, which for above a quarter of a mile was quite swallowed up; there, where once brave streets of stately houses stood, appearing now nothing but water, except here and there a chimney, and some parts and pieces of houses.

And though Port-Royal was so great a sufferer by the earthquake, yet it left more houses standing there, than in all the island besides, all over which it is said to rage more furiously than at Port-Royal; for it was so violent in other places, that people could not keep their legs, but were thrown on the ground, where they lay on their faces with their arms and legs spread out, to prevent being tumbled and thrown about by the almost incredible motion of the earth, like that of a great sea. It scarcely left a planter's house or sugar-work standing all over the island: I think it left not a house standing at Passage-Fort, and but one in all Liganee, and none in St. Jago, except a few low houses, built by the wary Spaniards. And it is not to be doubted, but that had there been 500, or 5000 towns in Jamaica, the earthquake would have ruined every one. In several places in the country the earth gaped prodigiously; on the north-side, the planters' houses, with the greatest part of their plantations, were swallowed, houses, people, trees, all up in one gape; instead of which, appeared for some time after a great pool or lake of water, covering above 1000 acres, which is since dried up, and now is nothing but a loose sand, or gravel, without any the least mark left whereby one may judge that there ever had stood a tree, house, or any thing else. In Clarendon precinct the earth gaped, and spouted up with a prodigious force great quantities of water into the air, above 12 miles from the sea; and all over the island there were abundance of gapings of the earth, many thousands. But in the mountains are said to be the most violent shakes of all; and it is a generally received opinion, that the nearer to the mountains, the greater the shake. Indeed, they are strangely torn and rent; insomuch that they seem to be of quite different shapes now from what they were, especially, the blue, and other high mountains; thus breaking one mountain, and thereof making two or three; and joining two mountains, and making thereof one, closing up the unhappy valley between. And at Yallowes particularly, some families, who lived between two mountains, were shut up and buried under them. Not far from which place, part of a mountain, after having made several leaps or moves, overwhelmed a whole family, and great part of a plantation, lying a mile off. And a large high mountain, near Port-Morant, near a day's journey over, is said to be quite swallowed up; and in the place where it stood there is now a great lake of 4 or 5 leagues over. Those things happened in lower mountains: but in the blue mountains, and the neighbouring ones, from whence came those dreadful roarings, terrible and amazing to all that heard them, may by reasonably supposed to be many strange alterations of the like nature: but those wild desert places, being very rarely or never visited by any persons, we are yet ignorant of what happened there; but the astonishing noises that came from thence, and their miserable shattered appearance, showing what havoc has been there made. There one may see where the tops of great mountains have fallen, sweeping down all the trees, and every thing in their way, and making a path quite from top to bottom; and other places which seem to be peeled and bare a mile together; which vast pieces of mountains, with all the trees thereon, falling together in a huddled and confused manner, stopped up most of the rivers for about 24 hours; which afterwards having found out new passages, brought down into the sea, and this harbour, several hundred thousand tons of timber, which would sometimes float in the sea in such prodigious quantities, that they looked like moving islands. I have seen several of those large trees on this shore, all deprived of their bark and branches, and generally very much torn by the rocky passages, through which, by the force of a falling stream, and their own weight, they might be supposed to be driven. One great trunk of a tree particularly I have seen pressed as a sugar-cane after it has passed the mill. Some are of opinion that the mountains are sunk a little, and are not so high as they were: others think the whole island is sunk something by the earthquake. Port-Royal is said to be sunk a foot; and in many places in Liganee I have been told are wells, which require not so long a rope to draw water out of them now, as before the earthquake, by two or three feet, which seems a sort of proof, that either the land is sunk or the sea risen, the former of which seems most probable. Two gentlemen happened at the time of the earthquake to be in Liganee, by the sea-side; where at the time of the great shake the sea retired from the land in such sort, that for 2 or 300 yards the bottom of the sea appeared dry, whereon they saw lie several fish, some of which the gentleman who was with him ran and took up, and in a minute or two after the sea returned again, and overflowed great part of the shore. At Yallhouse the sea is said to have retired above a mile. It is thought there were lost in all parts of the island 2000 people, and had the shock happened in the night, very few would have escaped alive; and those that had would in all probability have been knocked in the head by the negroes, and the island to all intents and purposes quite ruined.

It is observed, that since the earthquake, the land-breezes often fail us, and instead thereof, the sea-breezes often blow all night; a thing rarely known before, but since common. In Port-Royal, and in many places all over the island, much sulphureous combustible matter has been found, supposed to have been thrown out, on the opening of the earth, which on the first touch of fire would flame and burn like a candle.

St. Christopher's, one of the Caribee islands, was heretofore much troubled with earthquakes; which, on an eruption of a great mountain there of combustible matter, which still continues, wholly ceased, and have never been felt there since; wherefore many expect some such eruption in some of the mountains here, though we hope there is no necessity for it; the shocks having been observed to lose their force, and to become weaker and weaker ever since the first fatal one.

After the great shock, those who escaped got on board the ships in the harbour, where many continued about two months after; the shocks all that time were violent, and frequent, sometimes two or three in an hour's time, accompanied with frightful noises, both from under the earth, and from the continual falling and breaking of the mountains.

Concerning the Distance of the fixed Stars. By Francis Roberts, Esq. S. R. S. N° 209, p. 101.

The ancient astronomers, who had no other way of computing the distances of the heavenly bodies, but by their parallax to the semidiameter of the earth; and being never able to discover any in the fixed stars, thence rightly inferred, that their distance was very great, and much exceeding that of the planets; but they could go no farther, but by guess. Since the Pythagorean system of the world has been revived by Copernicus, there seemed ground to imagine that the diameter of the earth's annual course, which is at least 40,000 times larger than the semidiameter of the earth, might give a sensible parallax to the fixed stars, and thereby determine their distance more precisely.

But there are some considerations which may make us suspect, that even this is not large enough for that purpose.

M. Huygens tells us, he could never discover any visible magnitude in the fixed stars, though he used glasses which magnify the apparent diameter above 100 times. Now, since in all likelihood the fixed stars are suns, perhaps of different magnitudes, we may, as a reasonable medium, persume they are generally about the size of our sun. Let us then, for example, suppose the Dog-Star to be so: the distance from us to the sun being about 100 times the sun's diameter, as is demonstrable from the sun's diameter being 32 minutes, it is evident that the angle, under which the Dog-Star is seen in Mr. Huygens's telescope, must be near the same with the angle of its parallax to the sun's distance, or semidiameter of the earth's annual course; so that the parallax to the whole diameter can be but double such a quantity, as even to Mr. Huygens's nice observation is altogether insensible. The distance therefore of the fixed stars seems hardly within the reach of any of our methods to determine; but from what has been laid down, we may draw some conclusions, that will much illustrate the prodigious size of it. As,

That the diameter of the earth's annual orbit, which contains at least 160 millions* of miles, is but as a point in comparison of it; at least it must be above 6000 times the distance of the sun: for if a star should appear through the telescope half a minute broad, which is a pretty sensible magnitude, the true apparent diameter would not exceed 18‴, which is less than the 6000th part of the apparent diameter of the sun, and consequently the sun's distance not the 6000th part of the distance of the star. 2. That could we advance towards the stars 99 parts of the whole distance, and have only $\frac{1}{100}$ part remaining, the stars would appear little larger to us than they do here: for they would show no otherwise than they do through a telescope, which magnifies a hundred fold. 3. That at least 9 parts in 10, of the space between us and the fixed stars, can receive no greater light from the sun, or any of the stars, than what we have from the stars in a clear night. 4. That light takes up more time in travelling from the stars to us, than we in making a West-India voyage, which is commonly performed in six weeks. That sound would not reach us from thence in 50,000 years, nor a cannon-bullet in a much longer time.† This is easily computed, by allowing, according to Mr. Newton, 10 minutes for the passage of light from the sun hither, and that sound moves about 1300 feet in a second.

On a Stone of a prodigious Size cut out of a Woman's Bladder, now living, Nov. 1693. By Mr. Basil Wood, Surgeon. N° 209, p. 103.

This stone was taken from Mrs. Henchman, a widow lady of 51 years of age. Its shape is not very unlike to a sort of spring-purse which many people use; and its surface is indifferently smooth, excepting only that there are four protuberances, each about the size of a hazel-nut; these seem to have been at first lesser stones, which falling into the bladder after the great stone was almost grown to its full size, were joined to it, first by adhesion, and at last became one body with it. The length of the stone is $3\frac{3}{4}$ inches; its breadth, where largest, is very near $3\frac{1}{4}$ inches; its thickness is $1\frac{7}{8}$ inch: its weight is 9 ounces and a half avoirdupois.

Dr Molineux, in his account of a large stone spontaneously voided by a

* It is now known to contain nearly 200 millions of miles.

† Or rather much the same time, as usually a cannon-ball flies as swiftly as sound, and sometimes much more so. Also $7\frac{1}{2}$ or 8 minutes is the time of light flying from the sun, and 1142 feet per second the velocity of sound.

woman through the urethra, in N° 202 of the Trans. has mentioned two o three notions, which I suppose this operation confutes. 1st, He thinks that women never breed stones so large as men; the contrary of which seems to be manifest by this operation: for perhaps a stone of so large a size as this was never yet taken out of the bladder of a living man. 2d, He seems to conclude it probable that all women may be freed from the stone by dilatation of the urinary passage, and then forcing away the stone through it: which method I think cannot be depended on, since the stones may prove of so great a size. 3d, He says, that dividing the membranous substance of the bladder, is to be avoided as certain death to the patient; whereas this stone, and many others have proved too large to be extracted through an incision made only within the short neck of a woman's bladder. The patient never had the least ill symptom since her being cut, and is now perfectly well.

Observations on Epidemic Distempers. By Dr. Thomas Molyneux. N° 209, p. 105.

About the beginning of November, 1693, after a constant course of weather moderately warm for the season, upon some snow falling, of a sudden it grew extremely cold, and soon after there succeeded some few days of very hard frost; upon which rheums of all kinds, such as violent coughs, that chiefly affected in the night, great defluxion of thin rheum at the nose and eyes, immoderate discharges of the saliva by spitting, hoarseness of voice, sore throats, with some trouble in swallowing, wheezings, obstructions, and soreness in the breast, a dull heaviness and stoppage in the head, with such like disorders, the usual effects of cold, seized great numbers of all sorts of people in Dublin. Some were more violently affected, so as to be confined a while to their beds; those complained of feverish symptoms, as shiverings and chillness all over them, that made several returns; pains in many parts of their body, severe head-achs, chiefly about their foreheads, so that the least noise was very troublesome; great weakness in their eyes, that the least light was offensive; a perfect decay of all appetite; foul turbid urine, with a brick-coloured sediment at the bottom; great uneasiness and tossing in their beds all night: yet these disorders would usually, without any remedies, abate of themselves, and terminate in universal sweats, that constantly relieved. This more violent degree of the cold was more apt to affect such as were given to excess either in eating or drinking, or inclinable to a scrophulous disposition of body, than on those that were more temperate, and less subject to obstructions. When the cold was but moderate, it was usually over in 8 or 10 days: but with those in whom it rose to a greater height, it continued a fortnight, or three weeks, and sometimes above a month, some way or other it affected every body: those in the country, as well as those in the town; those that were much abroad in the open air, and those that staid much within doors; the robust and hardy, as well as the weakly and tender; and if it were favourable to any, they were the aged, many of whom escaped it.

As it first appeared towards the beginning of November, so it seemed to arrive at its greatest degree of violence, and spread most universally about the middle of it; and by the beginning of the following month it very sensibly abated, so that very few then complained of their colds. So that in the space of 4 or 5 weeks it had its rise, growth, and decay; and though the disorder was so general, none died of it, unless such whose strength was before spent by some tedious fit of sickness, or laboured under some heavier disease complicated with it.

This cold was as general in England, and with the same symptoms, as it seized us in Dublin; but with this difference, that it appeared 3 or 4 weeks sooner in London, (that is, about the beginning of October,) than it did in Dublin, where it was not the least taken notice of till about the beginning of November. Nor was its progress bounded by these islands, for it spread itself it seems still farther, and reached the continent, where it infested the northern parts of France, as also Flanders, Holland, and the rest of the United Provinces, with more violence, and no less frequency then in these countries; so that hardly any epidemic distemper has been observed to extend so far.

No instance of any epidemic distemper seems in all respects to come nearer to this general cold, than the transient fever in the year 1688, which I consider as the most universal fever, as this the most universal cold, that perhaps has ever appeared.

About the beginning of July 1688, this fever was first observed to appear here in Dublin; and it so universally seized all degrees of men whatever, that not above 1 in 15 escaped. It began as fevers generally do, with a chillness and shivering all over, like that of an ague, but not so violent, which soon broke out into a dry burning heat, with great uneasiness, that commonly confined the patients to their beds, where they passed the ensuing night in a very restless manner; they complained likewise of giddiness, and a dull pain in the head, chiefly about the eyes, with wandering pains in their limbs, and about the small of the back, a soreness all over their flesh, a loss of appetite, a nausea, and an unusual ill taste in the mouth, yet little or no thirst: and though these symptoms were very violent for a time, yet they did not continue long; for about the 2d day of the distemper, the patient usually of himself fell into a sweat, unless it was prevented by letting blood, which, however beneficial in other fevers, manifestly retarded the progress of this; and if the sweat was encouraged for 5 or 6 hours, by laying on more clothes, or taking some sudorific medicine, most of the complaints would entirely disappear, or at least very much abate. The giddiness of the head and want of appetite would often continue some days after, but with the use of the open fresh air, they certainly in 4 or 5 days at farthest recovered, and were perfectly well: so transient and favourable was this disease, that it seldom required the help of a physician; and of 1000 that were seized with it, I believe scarcely one died; and by the middle of August following it wholly disappeared; so that its whole period was about 7 weeks.

This fever also spread itself all over England; and it raged as generally in London as in Dublin, and with the same symptoms, but with some difference of time in relation to its first appearance; for whereas they began to take notice of it at London about the middle of May, and it continued there till about the latter end of June; it did not show itself here in Dublin till the beginning of July, after it had wholly disappeared in London. So likewise our late general colds, as beforementioned, were observed to keep such a sort of regular precedence of time, as to their rise and fall at London, in respect of Dublin. Whence we may reasonably infer, that these spreading epidemic distempers take their progress from east to west. But this should be further confirmed by more frequent observations, before we may safely determine any thing on this head generally: however, that it held true in the two foregoing instances is certain: and it is not less certain that the plague and pestilential fevers rage more frequently in the east towards Constantinople and the Levant, than in these more western parts of Europe.

Of a Stone found in the Gall-Bladder of a Woman. By Mr. J. T. N° 209, p. 111.

After throwing up the sternum, and inspecting the parts contained in the cavity of the breast, I found the lobes of the lungs extremely turgid, and its vesicles replete with a grumous blood, and their investing membrane in the upper part adhered firmly to the pleura; the right ventricle of the heart was filled with a large quantity of coagulated blood, but the left seemed exsanguinous: I took but a cursory view of these parts, yet I could not but observe a stagnation, and great extravasation of blood upon the right side of the pleura.

Beneath the diaphragm, in the cavity of the abdomen, I found the ventricle and intestines much inflated, the omentum fair and large, and the spleen so much augmented in bulk, that it could not weigh less than 2 or 3 pounds; upon cutting through its body, there was discharged several ounces of a very fetid and putrefied blood. The liver also was much larger than usual, but its parenchyma firm and sound. When I came to survey the concave part of this bowel, the vesica bilaria seemed full of bile; but more curiously examining it by the touch, I found by the interposition of a solid body, that there was something preternatural ingendered within its cavity: to be satisfied of this, I opened the vesica, and with my forceps extracted a stone very beautifully crusted over with crystallized salts of various figures, conical, cubical, pyramidal, &c. The one half of it lay immersed in bile, which was not considerable; for this lapideous concretion took up the whole cavity of the gall bladder, and weighed, immediately after it was taken from its receptacle, ʒij 15 grs. We discovered in one of the kidneys a large abscess, and discharged a great quantity of wheyish matter.

An Account of Books: viz.—I. Tractatus Mathematicus de Figurarum Curvilinearum Quadraturis et Locis Geometricis. Autore Johanne Craig. Lond. N° 209, p. 113.

This treatise consists of two heads. In the first the author gives a general method for determining the quadratures of curvilinear spaces, which he shows may always be done, by finding another curve-line, from the known property of its tangent; which curve-line he therefore calls the quadratrix of the proposed space. In his calculations he exhibits, by-the-bye, a new method of finding infinite series, without the trouble of division or extraction of roots, by assuming an arbitrary series with unknown coefficients, which are easily determined in the progress of the problem. And since the publication of this treatise we have some instances of the like method of finding series by that excellent mathematician, Mr. Leibnitz, printed in the Acta Eruditorum of April, 1693. Afterwards he gives a method of comparing the areas of figures, with the simplest figures of the same kind; from which he deduces many theorems, each of which exhibits the quadratures of an infinity of figures, without any trouble of calculation. He concludes this first part with a discourse concerning the analytical expression of quadratures, wherein is shown, that though the area answering to the abscissa be that which is commonly sought, yet the general quadrature found by his method is for the most part either greater or less: therefore he gives both a geometrical and analytical rule for knowing whether the said general quadrature be deficient or exceeding, and what that deficient or exceeding quantity is.

The second head of this treatise is concerning the geometric loci: showing how to determine any solid locus, by comparing the equation with a general theorem comprehending all loci of that kind: thus avoiding all those rules about the reduction of the given equation, and the variety of the signs + and —, which have hitherto rendered this part of geometry so troublesome.

II. *The History of the Church of Malabar, from the Time of its being discovered by the Portuguese, in the Year* 1501. *Giving an Account of the Persecutions and violent Methods of the Roman Prelates, &c. Lond.* 8vo. 1694. N° 209, p. 115.

A Letter from Monsieur N. Witsen, to Dr. Martin Lister, respecting the famous Persepolis. N° 210, p. 117.

There is no information of any consequence in this letter.

Dr. Gwither's Discourse of Physiognomy. Communicated by Mr. Owen Lloyd, Sec. of the Phil. Soc. at Dublin. N° 210, p. 118.

Soft wax cannot receive more numerous and various impressions, than are imprinted on the human face by objects moving the affections; and not only the objects themselves have this power, but also the very images, or ideas: that is, any thing that puts the animal sprits into the same motion that the object did when present will have the same effect with the object itself. To prove the first, let one observe a man's face looking on a pitiful object, then on a ridiculous, then on a strange, then on a terrible or dangerous object, &c. For the second that ideas have the same effect with real objects, dreams often confirm.

The manner I conceive to be thus: the animal spirits, moved in the sensory by an object, continue their motion to the brain; whence the motion is propagated to this or that particular part of the body, as is most suitable to the design of its creation, having first made an alteration in the face by its nerves, especially the pathetic, and oculorum motorii, actuating its numerous muscles, as the dial-plate to that stupendous piece of clock-work, which shows what is to be expected next from the striking part: not that I think the motion of the spirits in the sensorium continued by the impression of the object, all the way, as from the finger to the foot; but I conceive it done in the medulla of the brain, where is the common stock of spirits; as in a organ, whose pipes being uncovered, the air rushes into them, but the keys being let go, they are stopped again: now, if by repeated acts or by frequent entertaining the ideas of a favourite passion or vice which natural temperament or custom has hurried one into, the face is so often put into that posture which attends such acts, that the animal spirits find such passages into its nerves, that it is sometimes unalterably set, (as the Indian religious are, by long continuing in strange postures in their Pagodas,) but most commonly such habit is contracted, that the face falls insensibly into that posture, when some present object does not obliterate that more natural impression by a new one, or dissimulation hide it; hence it is, that we see great drinkers with eyes generally set towards the nose, the adducent muscles being often employed to let them see their beloved liquor in the glass, in the time of drinking; which were therefore called bibitory; lascivious persons are remarkable for the oculorum mobilis petulantia, as Petronius calls it. From this also we may solve the Quaker's expecting face, waiting the pretended spirit, and the melancholy face of the Sectaries; the studious face of men of great application of mind; and the revengeful face of bloody men, like executioners in the act: and though silence in a sot may a while pass for wisdom, yet sooner or later Sir Martin peeps through the disguise, and marrs all. A changeable face is observed to show a changeable mind. But I would by no means have what has been said understood as without exception; for I doubt not but sometimes there are found men with great and virtuous souls, under unpromising outsides.

*A Continuation of Mr. John Clayton's Account of Virginia.** N° 210, p. 121.

There were neither horses, bulls, cows, sheep, nor swine, in all the country, before the coming of the English. But now among the English inhabitants there are plenty of horses. They never shoe them, nor stable them in general. The Indians have not yet learned to ride. In the uninhabited parts there are now wild bulls and cows, which have been bred from some that have strayed, and become wild, and are difficult to be shot, having a great acuteness of smelling. Their sheep are of a middling size, pretty fine fleeced in general, and most persons of estate begin to keep flocks of them. There is plenty of good red deer. They have swine now in great abundance; shoats, or porkrels are their general food; and I believe as good as any Westphalia, certainly far exceeding our English. Here are rackoons, opossums, hares, three sorts of squirrels, (one of which is frequently eaten), musk or water-rats, bats of two sorts, some bears, whose flesh is eaten as pork, wild-cats and pole-cats, beavers, many wolves and foxes, numerous land and water-tortoises, frogs of several sorts, one 8 or 10 times larger than those in England, called the bull-frog, from its loud noise; toads, lizards, and snakes of many sorts.

* See Phil. Trans. N° 201, 205, 206.

A new, exact, and easy Method of finding the Roots of any Equations generally, and that without any previous Reduction. By Edm. Halley. N° 210, p. 136. *Translated from the Latin.*

The chief use of the analytic art, is to bring mathematical problems to equations, and to exhibit those equations in the most simple terms. But this art would justly seem in some degree defective, and not sufficiently analytical, unless there were some methods for finding the roots, whether lines or numbers, of those equations, and so the solution of the problems be completed. The ancients knew little of these matters beyond quadratic equations: and what they wrote on the geometrical construction of solid problems, by help of the parabola, cissoid, or any other curve, were only some peculiar effections designed for some particular cases. But as to numerical extraction, there is every where a profound silence: so that whatever we now perform of this kind, is wholly owing to the inventions of the moderns.

And first of all, that great discoverer and reformer of the modern algebra, Francis Vieta, about 100 years since,* showed a general method for extracting the roots of any equation, under the title of "A Numeral Resolution of Powers," &c. Harriot, Oughtred, and other authors, whatever they have written on this subject, it must be acknowledged as chiefly taken from Vieta.† But what the sagacity of Newton's genius has performed in this way, we may rather conjecture, than be fully assured of, from that short specimen given by Dr. Wallis, in the 94th chapter of his Algebra. And we must be forced to expect it, till his great modesty shall yield to the intreaties of his friends, and permit those curious discoveries to see the light.

Lately, viz. 1690, Mr. Joseph Raphson, F. R. S. published his "Universal Analysis of Equations," and illustrated his method by many examples; in which he has given indications of a mathematical genius from which the greatest things may be expected. By his example, M. De Lagney, an ingenious professor of mathematics at Paris, was induced to prosecute the same subject: but being almost wholly occupied in extracting the roots of pure powers, especially the cubic, he adds but little to the extraction of the roots of affected equations; and that rather perplexed too, and not sufficiently demonstrated. Yet he gives two very compendious rules for the approximation of a cubic root, the one a

* A little before the year 1600.

† Improvements are usually gradual and successive: Harriot, Oughtred, &c. added to the improvements of Vieta, as Vieta did to those of Stevinus, and he to those of others that preceded him, &c.

rational, the other an irrational formula, viz. that the side of the cube $a^3 + b$, is between $a + \frac{ab}{3a^3 + b}$ and $\frac{1}{2}a + \sqrt{\frac{1}{4}a^2 + \frac{b}{3a}}$. And the root of the 5th power $a^5 + b$ he thus expresses, $\frac{1}{2}a + \sqrt{\sqrt{\frac{1}{4}a^4 + \frac{b}{5a}} - \frac{1}{4}a^2}$. These rules were communicated to me by a friend, as I have not seen the author's book; but having by trial proved their goodness, and admiring the compendium, I wished to discover the demonstration. This being accomplished, I soon perceived that the same method might be accommodated to the solution of all sorts of equations whatever. And I was the rather inclined to improve these rules, as I saw that the whole business might be explained in a synopsis; and that in this way, at every repetition of the calculus, the figures already found in the root would be at least tripled, which all the former methods only doubled.

Now the fore-mentioned rules are easily demonstrated from the genesis of the 3d and 5th powers. For, supposing the side of any cube to be $a+e$, the cube of this is $aaa+3aae+3aee+eee$. Consequently, if we suppose aaa the next less cube, to any given non-cubic number, then eee will be less than unity, and the remainder b will be equal to the other members of the cube, $3aae+3aee+eee$: hence, rejecting eee on account of its smallness, it is $b = 3aae + 3aee$. And since aae is much greater than aee, therefore $\frac{b}{3aa}$ will not much exceed e; so that, putting $e = \frac{b}{3aa}$, then the quantity $\frac{b}{3aa+3ae}$, to which e is nearly equal, will be found $= \frac{b}{3aa + \frac{3ab}{3aa}}$, or $\frac{b}{3aa + \frac{b}{a}}$, that is, $\frac{ab}{3aaa+b} = e$; and therefore the side of the cube $aaa+b$ will be $a+\frac{ab}{3aaa+b}$; which is M. de Lagney's rational formula. But if aaa were the next greater cube than the given number, the side of the cube $aaa - b$, after the same manner, will be found $a - \frac{ab}{3aaa - b}$. And this easy and expeditious approximation to the cubic root, is only a very small matter erroneous in defect, giving the quantity e rather less than the truth.

The irrational theorem is also derived from the same principle, viz. $b = 3aae + 3aee$, or $\frac{b}{3a} = ae + ee$; so that $\sqrt{\frac{1}{4}aa + \frac{b}{3a}} = \frac{1}{2}a + e$, and $\sqrt{\frac{1}{4}aa + \frac{b}{3a}} + \frac{1}{2}a = a + e$, the root sought. And the side of the cube $aaa - b$, in the same manner, is found to be $\frac{1}{2}a + \sqrt{\frac{1}{4}aa - \frac{b}{3a}}$. And this formula comes rather nearer the truth than the former, but errs in point of excess, as the other in defect; and it seems more commodious for practice, since the repetition of the calculus is only the continual addition or subtraction of the quantity $\frac{eee}{3a}$, as the small supplement e becomes known; so that it may rather be written in the former case

$\frac{1}{2}a + \sqrt{\frac{1}{4}aa + \frac{b - eee}{3a}}$, and $\frac{1}{2}a + \sqrt{\frac{1}{4}aa + \frac{eee - b}{3a}}$ in the latter.

And by either form the figures already known, in extracting the root, are at least tripled; which must prove an acceptable compendium, and therefore I congratulate the inventor upon it. But that the benefit of these rules may the better appear, I shall add an example or two.

Example 1. Required to find the side of the double cube, or of $aaa + b = 2$. Here $a = 1$, $b = 1$, and $\frac{b}{3a} = \frac{1}{3}$; therefore $\frac{1}{2} + \sqrt{\frac{7}{12}}$ or 1,26 will be found near the true root. Now the cube of 1,26 is 2,000376, therefore $0{,}63 + \sqrt{0{,}3969 - \frac{0{,}000376}{3{,}78}}$, or $0{,}63 + \sqrt{0{,}3968005291005291} = 1{,}259921049895$ —, which exhibits the side of the double cube to 13 figures with very little trouble, viz. by only one division and extraction of the square root; whereas by the common way it is well known how laborious it must have been. Hence this calculus may be continued at pleasure, by increasing the square by the addition of $\frac{eee}{3a}$: which correction in this case brings only the increase of a unit in the 14th figure.

Example 2. Required to find the side of a cube equal to the wine gallon, containing 231 solid inches. The next less cube is 216, its root being 6, and the remainder is $15 = b$; therefore the first approximation will be $3 + \sqrt{9 + \frac{5}{6}}$ for the root: and since $\sqrt{9{,}8333\ldots}$ is $3{,}1358\ldots$, it appears that $6{,}1358 = a + e$. Now make $6{,}1358 = a$, then its cube is 231,000853894712, and according to the rule $3{,}0679 + \sqrt{9{,}41201041 - \frac{0{,}000853894712}{18{,}4074}} = 6{,}13579243966195897$, which is the side of the given cube very exactly, being true in the 18th figure, and only falling short in the 19th; which calculation I performed in an hour's time. And this formula is deservedly preferred before the rational one, which, on account of its large divisor, cannot be used without much trouble, in comparison of the irrational one, as manifold experience has informed me.

Now the rule for the root of the pure sursolid, or 5th power, is of a little higher inquiry, and yet performs the business much more perfectly; for it at least quintuples the given figures of the root, and yet it requires not a very operose calculation. But the author has no where given his method of investigation or demonstration, though it seemed most to require it; especially as it is erroneously printed in his book, which may easily mislead the unskilful. Now the 5th power of $a + e$ consists of these parts, $a^5 + 5a^4e + 10a^3e^2 + 10a^2e^3 + 5ae^4 + e^5 = a^5 + b$; hence $b = 5a^4e + 10a^3e^2 + 10a^2e^3 + 5ae^4$, rejecting e^5 on account of its smallness; therefore $\frac{b}{5a} = a^3e + 2a^2e^2 + 2ae^3 + e^4$; then adding $\frac{1}{4}a^4$ on each side, and extracting the square-root, it is $\sqrt{\frac{1}{4}a^4 + \frac{b}{5a}} = \sqrt{\frac{1}{4}a^4 + a^3e + 2a^2e^2 + 2ae^3 + e^4} = \frac{1}{2}a^2 + ae + e^2$; then subtracting $\frac{1}{2}a^2$ from each side, and extracting the square-root again, we shall have $\sqrt{\sqrt{\frac{1}{4}a^4 + \frac{b}{5a}} - \frac{1}{2}a^2} = \sqrt{\frac{1}{4}a^2 + ae + e^2} = \frac{1}{2}a + e$; and adding $\frac{1}{2}a$, it is $a + e = \frac{1}{2}a + \sqrt{\sqrt{\frac{1}{4}a^4 + \frac{b}{5a}} - \frac{1}{4}a^2}$ the 5th root of the power $a^5 + b$. But if it were $a^5 - b$, by taking a greater than just, the rule would be, $\frac{1}{2}a + \sqrt{\sqrt{\frac{1}{4}a^4 - \frac{b}{5a}} - \frac{1}{4}a^4}$.

This rule approximates wonderfully fast, so that there can scarcely be any need of repetition. And while I was considering this matter, I fell upon a general method of forms for any power whatever, which is elegant enough, and which I cannot prevail on myself to conceal, since in the higher powers they at least triple the known figures of the root. And these forms, both rational and irrational, proceed as follows:

$$\sqrt[2]{a^2 + b} = \tfrac{0}{1}a + \sqrt{\tfrac{1}{1}aa + \frac{b}{1a^0}}, \text{ or } a + \frac{ab}{2a^2 + \frac{1}{2}b}.$$

$$\sqrt[3]{a^3 + b} = \tfrac{1}{2}a + \sqrt{\tfrac{1}{4}aa + \frac{b}{3a}}, \text{ or } a + \frac{ab}{3a^3 + \frac{2}{2}b}.$$

$$\sqrt[4]{a^4 + b} = \tfrac{2}{3}a + \sqrt{\tfrac{1}{9}aa + \frac{b}{6a^2}}, \text{ or } a + \frac{ab}{4a^4 + \frac{3}{2}b}.$$

$$\sqrt[5]{a^5 + b} = \tfrac{3}{4}a + \sqrt{\tfrac{1}{16}aa + \frac{b}{10a^3}}, \text{ or } a + \frac{ab}{5a^5 + \frac{4}{2}b}.$$

$$\sqrt[6]{a^6 + b} = \tfrac{4}{5}a + \sqrt{\tfrac{1}{25}aa + \frac{b}{15a^4}}, \text{ or } a + \frac{ab}{6a^6 + \frac{5}{2}b}.$$

And so on for the other higher powers. But when a is assumed greater than the true root, (which will be done with advantage whenever it is nearer the truth than the next less integer number) then by a proper change in the signs, the same expressions of the roots arise, viz.

$$\sqrt[2]{a^2 - b} = \tfrac{0}{1}a + \sqrt{\tfrac{1}{1}aa - \frac{b}{1a^0}}, \text{ or } a - \frac{ab}{2a^2 - \frac{1}{2}b}.$$

$$\sqrt[3]{a^3 - b} = \tfrac{1}{2} a + \sqrt{\tfrac{1}{4} aa - \frac{b}{3a}}, \text{ or } a - \frac{ab}{3a^3 - \frac{2}{3}b}.$$
$$\sqrt[4]{a^4 - b} = \tfrac{2}{3} a + \sqrt{\tfrac{1}{9} aa - \frac{b}{6a^2}}, \text{ or } a - \frac{ab}{4a^4 - \frac{3}{2}b}.$$
$$\sqrt[5]{a^5 - b} = \tfrac{3}{4} a + \sqrt{\tfrac{1}{16} aa - \frac{b}{10a^3}}, \text{ or } a - \frac{ab}{5a^5 - \frac{4}{2}b}.$$
$$\sqrt[6]{a^6 - b} = \tfrac{4}{5} a + \sqrt{\tfrac{1}{25} aa - \frac{b}{15a^4}}, \text{ or } a - \frac{ab}{6a^6 - \frac{5}{2}b}.*$$

And between these two limits always lies the true root, being rather nearer to the irrational than to the rational formula; but the quantity e, found by the irrational formula, always errs in excess, as the quotient resulting from the rational always errs in defect; therefore, when it is $+ b$, the irrational form gives the root too great, and the rational one too little; but the contrary when it is $- b$. And this may be sufficient concerning the finding the roots of pure powers; which, however, for ordinary purposes, may be performed more easily, and accurately enough, by means of logarithms; but whenever the root is to be extracted very accurately, and beyond the extent of the logarithms, recourse must necessarily be had to such methods as these. Besides, as the invention and contemplation of these formulas led me to a certain general rule for the roots of affected equations, which I trust will be of good use to the students in algebra and geometry, I was willing here to give some account of this discovery in as clear a manner as I can.

Now having given in the Philosophical Transactions, N° 188 †, a very easy and general construction of all affected equations, not exceeding the biquadratic, from that time I have always had a great desire of performing the same in numbers. But soon after that Mr. Raphson seemed in a good measure to have satis-

* All the above rational formulas may be reduced to one easy and general expression, which will be easier both to remember and to practise than any of the single individual ones; and which is deduced in the following manner. First, the general theorem for all the above particular ones, and in the same form, is $a + \frac{ab}{na^n + \frac{n-1}{2} b}$, which reduces to $\frac{2na^n + (n+1)b}{2na^n + (n-1)b} \times a$, for the near value of the root, by only reducing the quantity into a common denominator, and where n denotes the index of the power whose root is to be extracted. Now, by putting $N = a^n + b$ the given number, of which a^n is the nearest complete power, a being its root; then by substituting $N \backsim a^n$ for the difference b, the last general form above will become $\frac{(n+1)N + (n-1)a^n}{(n+1)a^n + (n-1)N} \times a$, for the approximated root. Or, in a proportion it is, as $(n + 1) a^n + (n - 1) N : (n + 1) N + (n - 1) a^n :: a : r$ the root sought; that is, in words, as $n + 1$ times the nearest power, added to $n - 1$ times the given number, is to $n + 1$ times the given number added to $n - 1$ times the nearest power, so is the near assumed root to the root required. And this is the same general formula as that first demonstrated, by a different way, in Dr. Hutton's Tracts, published in 1786, p. 49.

† P. 376 of this vol. of these Abridgments.

fied this desire, till M. de Lagney evinced, by what he has done in his book, that the thing might be performed still more compendiously. Now my method is this:

Let the root z of any equation be supposed to be composed of two terms a + or − e, of which a is assumed as near as may be to z; which, however, is not absolutely necessary, but only convenient; then let the quantity a + or − e be raised to all the powers of z found in the given equation, and affixing to each the corresponding numeral co-efficients. Then let the power to be resolved be subtracted from the sum of the given parts in the first column, where e is not found, called the homogeneum comparationis, and let the difference be $\pm b$. Next, take the sum of all the co-efficients of e in the second column, which put $= s$. Lastly, collect together all the co-efficients of e in the third column, the sum of which call t. Then will the near value of the root z be thus, viz. in the rational form, $z = a \pm \frac{sb}{ss \pm tb}$, and $z = a \mp \frac{\frac{1}{2}s \pm \sqrt{\frac{1}{4}ss \mp bt}}{t}$ in the irrational form; which it may be worth while to illustrate by some examples. But, as a convenient help, it may not be improper to have at hand a general table, exhibiting all the powers of $a \pm e$, which may easily be continued further if necessary; which table may justly be called a General Analytical Speculum. The said powers arising from the continual multiplication of $a + e = z$, are as follow, with their annexed co-efficients.

Table of Powers.

$$
\begin{array}{lllllll}
 & & s & & & & \\
cz = ca + & & ce & t & & & \\
dz^2 = da^2 + & 2dae + & dee & u & & & \\
fz^3 = fa^3 + & 3fa^2e + & 3faee + & fe^3 & w & & \\
gz^4 = ga^4 + & 4ga^3e + & 6ga^2ee + & 4gae^3 + & ge^4 & x & \\
hz^5 = ha^5 + & 5ha^4e + & 10ha^3ee + & 10ha^2e^3 + & 5hae^4 + & he^5 & y \\
kz^6 = ka^6 + & 6ka^5e + & 15ka^4ee + & 20ka^3e^3 + & 15ka^2e^4 + & 6kae^5 + & ke^6.
\end{array}
$$

But if it should be $a - e = z$, the table would still be composed of the same terms, but only the odd powers of e must be negative, as e, e^3, e^5, &c. and the even powers, e^2, e^4, e^6, &c. must still be positive. Also let the sum of the co-efficients of the first power e be called s; those of the square $e^2 = t$; of the cube $e^3 = u$; of the biquadrate $e^4 = w$; of the sursolid $e^5 = x$; of the sixth power $e^6 = y$; and so on. And since e is supposed to be only a small part of the root required, all the powers of e will become much less than the like powers of a, therefore for a first process the higher powers may be rejected, as has been shown in the pure powers; then forming a new equation, by substituting $a \pm e$ for z, as before said, it will be $\pm b = \pm se \pm tee$. For the better understanding of which, take the following examples.

Example 1. Let the equation proposed be $z^4 - 3zz + 75z = 10000$. For the first supposition take $a = 10$; hence will arise the following equation:

$$\begin{array}{rcllll}
z^4 & = & + a^4 & 4a^3e & + 6a^2ee & 4ae^3 + e^4 \\
- dz^2 & = & - da^2 & dae & - dee & \\
+ ez & = & + ca & ce & & \\
 & = & + 10000 & 4000e & + 600ee & 40e^3 + e^4 \\
 & & - 300 & 60e & - 3ee & \\
 & & + 750 & 75e & & \\
 & & - 10000 & & & \\
\hline
 & & + 450 & - 4015e & + 597ee & - 40e^3 + e^4 = 0. \\
 & & & s & t & u
\end{array}$$

The signs + and —, of e and e^3, are left doubtful, till it be known whether e is negative or affirmative, which may admit of some difficulty, since in equations that have many roots, the homogeneum comparationis, as it is called, may be increased by diminishing the quantity a, and contrariwise may be diminished when that is increased. But the sign of e is determined by the sign of the quantity b: for the resolvend being taken away from the homogeneum formed of a, the sign of se, and therefore of the parts prevailing in its composition, will always be contrary to the sign of the difference b: hence it will appear whether it should be $-e$ or $+e$, or whether a be assumed greater or less than the true root. Now the quantity e is always $= \frac{\frac{1}{2}s - \sqrt{\frac{1}{4}ss - bt}}{t}$, when b and t have the same sign; but when these have contrary signs, then is $e = \frac{\sqrt{\frac{1}{4}ss + bt} - \frac{1}{2}s}{t}$. Now if it be found that it must be $-e$, then in the affirmative parts of the equation let e, e^3, e^5, &c. be made negative, and in the negative parts let them be made affirmative, that is, let them be all written with the contrary sign. On the other hand, if it be $+e$, let those parts retain their own signs.

Now in our example we have 10450 instead of the resolvend 10000, or $b = +450$, whence it appears that a is assumed greater than just, and therefore it must be $-e$. Hence the equation becomes $10450 - 4015e + 597ee - 40e^3 + e^4 = 10000$; that is, $450 - 4015e + 597ee = 0$. Therefore $450 = 4015e - 597ee$, or $b = se - tee$, the root of which is $e = \frac{\frac{1}{2}s - \sqrt{\frac{1}{4}ss - bt}}{t}$, or $\frac{s}{2t} - \sqrt{\frac{ss}{4tt} - \frac{b}{t}}$; that is, in the present instance, $e = \frac{2007\frac{1}{2} - \sqrt{3761406\frac{1}{4}}}{597}$, whence the root comes out 9,886, nearly true. Then substituting this for a 2d hypothesis, there arises $a + e = z = 9{,}8862603936495$ very accurate, scarcely exceeding the truth by 2 in the last figure,* viz. when $\frac{\sqrt{\frac{1}{4}ss + bt} - \frac{1}{2}s}{t} = e$. And even this, if necessary, might be still further corrected, viz. when it is $+\,e$ by subtracting $\frac{\frac{1}{2}ue^3 + \frac{1}{2}e^4}{\sqrt{\frac{1}{4}ss + tb}}$, or if it be $-\,e$ by adding $\frac{\frac{1}{2}ue^3 - \frac{1}{2}e^4}{\sqrt{\frac{1}{4}ss - tb}}$, from or to the root before found. Which compendium is so much the more valuable, that sometimes from the first supposition alone, but always from the second, the calculation may be continued as far as we please, keeping still the same coefficients. It may be observed that the proposed equation has also a negative root, which is $z = -\,10{,}26$, &c. as may be found on trial.

Exam. 2. Suppose $z^3 - 17zz + 54z = 350$. Then taking $a = 10$, and proceeding according to the rule,

$$\begin{array}{rcllll} z^3 & = & a^3 & +\,3aae & +\,3aee & +\,e^3 \\ -\,dz^2 & = & -\,da^2 & -\,2dae & -\,dee & \\ +\,cz & = & ca & +\,ce & & \\ & & b & s & t & \end{array}$$

$$\begin{array}{rllll} \text{That is,} & +1000 & +300e & +\,30ee & +\,e^3 \\ & -1700 & -340e & -\,17ee & \\ & +540 & +54e & & \\ & -350 & & & \\ \hline \text{or} & -510 & +14e & +\,13ee & +\,e^3 = 0. \end{array}$$

Now since it comes out $-\,510$, it appears that a was assumed less than just, and consequently e is affirmative; hence from $510 = 14e + 13ee$, there arises $\frac{\sqrt{bt + \frac{1}{4}ss} + \frac{1}{2}s}{t} = e = \frac{\sqrt{6679} - 7}{13}$; which gives $z = 15{,}7$, which is too much, because a was taken wide of the truth. Therefore, for a second supposition, take $a = 15$; then by the like process we obtain $e = \frac{\frac{1}{2}s - \sqrt{\frac{1}{4}ss - tb}}{t} = \frac{109\frac{1}{2} - \sqrt{11710\frac{1}{4}}}{28}$; and therefore $z = 14{,}954068$. And if the calculation were repeated a third time, we should find the root true to 25

* Notwithstanding this declaration however the number is erroneous, as the true figures ought to be 9,88600270, &c.

figures. But to be content with fewer, we may write $t\,b \pm t\,e^3$ instead of $t\,b$, or we may add or subtract $\frac{\frac{1}{2}e^3}{\sqrt{\frac{1}{4}ss \mp tb}}$ to or from the root before found. But the equation cannot be explained by any other root, because the power to be resolved, 350, exceeds the cube of $\frac{17}{3}$ or $\frac{1}{3}d$.

Exam. 3. Let there be taken the equation $z^4 - 80\,z^3 + 1998\,z^2 - 14937\,z + 5000 = 0$, which Dr. Wallis finds, ch. 62 of his Algebra, in the resolution of a difficult arithmetical problem, the root of which he obtained very accurately by Vieta's method; and which Mr. Raphson also uses as an example of his method, p. 25, 26. Now this equation is of such a form as admits of several positive roots; and, what increases the difficulty, the coefficients are very great in respect of the given resolvend. But, that it may be the easier managed, let it be divided, and according to the known rules of pointing, let $-z^4 + 8\,z^3 - 20\,z^2 + 15\,z = 0{,}5$, where z is $\frac{1}{10}$ z in the proposed equation; and for a first supposition take $a = 1$; then $+ 2 - 5\,e - 2\,ee + 4\,e^3 - e^4 - 0{,}5 = 0$; that is, $1\frac{1}{2} = 5\,e + 2\,ee$; hence $e = \frac{\sqrt{\frac{1}{4}ss + bt} - \frac{1}{2}s}{t} = \frac{\sqrt{37} - 5}{4}$, and therefore $z = 1{,}27$. Hence it appears that 12,7 is a near root of the equation. Secondly, supposing $z = 12{,}7$; then, according to the table of powers,

$$\begin{array}{llll}
b & s & t & u \\
- 26014{,}4641 & - 8193{,}532\,e & - 967{,}74\,ee & - 50{,}8\,e^3 - e^4 \\
+ 163870{,}640 & + 38709{,}60\,e & + 3048\,ee & + 80\,e^3 \\
- 322257{,}42 & - 50749{,}2\,e & - 1998\,ee & \\
+ 189699{,}9 & + 14937\,e & & \\
- 5000 & & & \\
\hline
+ 298{,}6559 & - 5296{,}132\,e & + 82{,}26\,ee & + 29{,}2\,e^3 - e^4 = 0.
\end{array}$$

Therefore $-298{,}6559 = -5296{,}132\,e, + 82{,}26\,ee$, the root of which is $e = \frac{\frac{1}{2}ss - \sqrt{\frac{1}{4}ss - bt}}{t} = \frac{2648{,}066 - \sqrt{6987686{,}106022}}{82{,}26} = {,}05644080331$, less than just. But to correct it, $\frac{\frac{1}{2}ue^3 - \frac{1}{2}e^4}{\sqrt{\frac{1}{4}ss - bt}} = \frac{{,}0026201}{2643{,}423} = {,}00000099117$, and therefore e corrected is ,05644179448. But if still more figures of the root be desired, from e corrected let there be formed $t\,u\,e^3 - t\,e^4 = 0{,}43105602423$, and $\frac{\frac{1}{2}s - \sqrt{\frac{1}{4}ss - bt - tue^3 + te^4}}{t}$ or $\frac{2648{,}066 - \sqrt{6987685{,}67496597577}}{82{,}26} =$,05644179448074402 $= e$; hence $a + e = z$ the root is very accurately 12,75644179448074402, the same as found by Dr. Wallis.

Here it may be observed that repeating the calculus always triples the true figures in the assumed a, which the first correction, or $\frac{\frac{1}{2}ue^3 - \frac{1}{2}e^4}{\sqrt{\frac{1}{4}ss - bt}}$, quintuples, which operation is easily performed by logarithms. The other correction, after the first, also doubles the true figures; so that on the whole it makes the assumed true figures seven-fold. But the first is commonly quite sufficient for most arithmetical uses. And by what is here said about the figures rightly assumed in the root, I mean that when a is distant from the true root not above a 10th part, then the first figure is rightly assumed; if within a 100th part, the first two figures are right; if within a 1000th part, the first three figures are right; which consequently treated according to our rule, the true figures soon become nine.

It remains now to add something concerning our rational formula, viz. $\frac{sb}{ss \pm tb}$, which seems expeditious enough, and is not much inferior to the former, since it triples the given number of true figures. Now, forming an equation from $a \pm e = z$, as before, it will soon appear whether the assumed a be greater or less than just, since se ought always to have a sign contrary to that of the difference between the resolvend and its homogeneum produced from a. Then supposing that $\pm b \mp se \pm tee = 0$, the divisor is $ss - tb$ when t and b have the same sign, but $ss + bt$ when they have different ones. But it seems most convenient for practice to write the theorem thus, $e = \frac{b}{s \pm \frac{bt}{s}}$, since then the business is performed by one multiplication and two divisions, which otherwise would require three multiplications and one division. Thus, taking an example of this method from the root of the foregoing equation, 12,7, &c. in which

$$\underset{+b}{298{,}6559} \; \underset{-\quad s}{-\;5296{,}132\,e} \; \underset{+\quad t}{+\;82{,}26\,ee} \; \underset{+\quad u}{+\;29{,}2\,e^3} - e^4 = 0,$$ and therefore

$\frac{b}{s - \frac{tb}{s}} = e$, that is, make $s : t :: b : \frac{tb}{s}$, or

5296,132) 298,6559 + 82,26 (4,63875; hence the divisor $s - \frac{tb}{s} = 5291{,}49325$) 298,6559 (0,056441 ... $= e$, viz. five true figures added to the assumed root. But this formula cannot be corrected like the irrational one; so that if more figures of the root be desired, it is better to repeat the calculation by making a new assumption; and then the new quotient, by tripling the number of true figures, will abundantly satisfy even the most scrupulous calculator.

An Epistle written by Josephus de Aromatariis, concerning the Seeds of Plants, and Generation of Animals. N° 211, p. 150.

The contents of this epistle are so little interesting, that it is not thought necessary to specify them.

Observations in the Dissection of a Paroquet. By Mr. Richard Waller. N° 211, p. 153.

There is perhaps not a greater variety of species than in the parrot-kind, whether we consider the country, size, or colour. Johnston treating of these birds says, The curious have observed above a hundred sorts of them; Margrave in his History of Brasil, enumerates several, and his 6th species of paroquets, which he calls tajete, comes very near our subject. Its size is between a sparrow and a blackbird, with a short neck, black eyes, a crooked scarlet bill, greyish legs and feet, with toes, two before and two behind, like the parrot; yet he never stands on one foot to eat with the other, as parrots do. When he stands still on the perch, his breast and belly show of a curious light green; his back and the feathers of his wings are somewhat darker; on his pinions are some short blue feathers, as also several on his rump. His bill is encompassed up to the eyes with a broad beautiful scarlet circle, reaching also down to his throat; this part in the hen is of a pale orange colour, which is the only observable difference. The feathers in the tail, which, as in all small paroquets, is no longer than the wings, are not to be seen but when he flutters or spreads it. They are about 2 inches long, near the quill of a lemon colour, inclining to a green; next that a scarlet of some breadth; then a narrow streak of green on some of them; after that a black, and lastly ending in a light green.

Having opened the thorax and abdomen by blowing into the aspera arteria, a large cavity or bladder was raised up all along the abdomen to the edges of the os ischium, and fastened to the gizzard, containing in it all the guts and gizzard, but excluding the heart and liver. A conformation like this is observed in all birds, and is peculiar to them, and mentioned by Perrault in his Mechanique des Animaux. The aspera arteria differs from that of most other animals, having not only a larynx at the top, as is usual, but another also at its entrance into the breast, where it is divided, and branches itself into two. From this structure, said to be common to all parrots, possibly it may be that they can so readily imitate human voices; but this creature we dissected never attempts an imitation of words, making only a shrill chirping noise, doubling the tone, or making it 8 notes lower, as a stopped organ-pipe is an 8th to the same open. This lower larynx may assist the weak fabric of so small a creature as a parrot to counterfeit so bass a voice as a man's; it being observed by some ingenious persons, that parrots are ventriloquists, and that it may be queried whether all ventriloquous cheats may not by nature be framed for such an imposture. The heart, in proportion to the animal, was large, and the liver small. The tongue was broad and thick, at the end somewhat like a man's, whence a parrot has its name ἀνθρωπόγλωττος; its extremity was armed with a black horny cover. It has, besides the gizzard, two craws, the uppermost being only a receptacle or sack for the food, which is canary seed, to be again returned to its mouth, where it is again chewed, having before been only husked; this animal ruminating as some quadrupeds do; and I have observed this bird, when on the perch, not only bring its food again up into its mouth and there chew it, but when the cock and hen sit together on the perch he will put it out of his into the hen's mouth. Their manner of chewing is thus: the under bill being much shorter shuts within the upper, or against the roof of the mouth, which is fitted with several rows of very small and cross-bars, as the mouths of horses, dogs, and some other animals are; these bars are not soft but horny, being part of the upper bill; so that the bird, by carrying the edge of the under bill and end of the tongue against the ridges in the upper, breaks and reduces to a pap the seeds that have been first moistened in the craw, to expedite which action, the upper bill is jointed just below the eyes. The food, being thus macerated, is by the gula again committed to the second craw, but before its entrance into it, it passes by a number of small glands placed in that part of the gula; that the food may squeeze out of them, in its passage, a juice, of what necessity in digestion may be inquired. From hence the food passes to the gizzard, or proper ventricle, small in comparison of the ingluvies or crop, where by several small stones picked out of the sand given it, by the motion of the gizzard it is comminuted, and thence transmitted to the intestines, on the sides of which within a small distance is placed the pancreas. The proportions of all these parts to each other will be best seen by the following figures, in pl. 14, which are purposely drawn as large as the life.

In fig. 8, a, is the aspera arteria; b, that part which forms as it were another larynx; c, part of the gula; d, the upper craw; e, the heart; f f, the venæ axillares; g g, the jugulars; h, a small gland on one of them; i i, the two auricles of the heart; k k, the liver; l, the gizzard. In fig. 9, a, is the trachea; b b, the larynx, by which parrots are rendered ventriloquists; c c, the two branches of the trachea. In fig. 10, a a, the cornua of the os hyoides; b b, two muscles of the larynx; c, the fissure or glottis; d, the trachea; e, the tongue; f, the horny end thereof. In fig. 11, a a, the testes; b b, the deferentia; c c, the kidneys; d d, the ureters. In fig. 12, a, is the upper part of the gula; b, the first or upper craw; c, that part of the gula whose inside is glandulous; d, the lower craw; e, the gizzard or ventricle; f, the first intestine; g g, the pancreas. In fig. 13, a, is the upper bill; b, the inside of it; d d, the upper jaw; c, the place where the upper bill is moveable; e, a passage to the nostrils; f, the lower bill; g, the upper bill in another posture, to show the small ridges therein. In fig. 14, representing one of the feathers of the tail; a, is that part furnished only with a white down; b, the part that is yellow; c, the scarlet part; d, where it is black; e, the green part.

Abstract of a Letter from Mr. Samuel Dale to Wm. Briggs, M. D. F. R. S. Concerning an obstinate Jaundice, accompanied with a very odd Case in Vision. N° 211, p. 158.

About Christmas, Anno 1689, after much grief and trouble of mind in the foregoing autumn, Grace Dennis, of Braxsted in Essex, was seized with the jaundice; for which, after having for about 9 months used many medicines as advised by her friends and acquaintance, but without success, she in Sept. 1690, applied to me, when I administered some medicines, famous in the most celebrated authors for the cure of the jaundice, yet they were of no benefit; after which she had the advice of several learned physicians in the country and in London, but still without any effect; for her disease yet continues, and her body, which used to be plump and fleshy, is now become lean and emaciated, almost like a skeleton, and her appetite small and depraved.

In May, 1691, after an extraordinary menstrual flux for about 3 months, she began, as soon as the sun was down, to be deprived of her sight by degrees till it was quite dark; when, although ever so large a fire or many candles were in the room, yet she could not discern any object, except a small glimmering of light: and thus she remained until the morning, as one stone-blind, when by little and little, as the light increased, her sight returned, till the sun arose, and then she recovered her perfect sight. And in this case she continued, till August, 1692; when being returned from Epsom, where she had been drinking the waters for about a month, her sight returned to her again, so that she could see in the night perfectly. Thus she continued until January last, when an extraordinary menstrual flux again seizing her, her nocturnal sight likewise left her, and she became blind again as formerly. In July, 1693, she was seized with a fever, when her sight again returned, and continued for about a month, and then left her as formerly; so that now, Oct. 1693, she has her nocturnal blindness, and her jaundice likewise continues.

Librorum Manuscriptorum Academiarum Oxoniensis et Cantabrigiensis, et Celebrium per Angliam Hiberniamque Bibliothecarum Catalogus, cum Indice Alphabetico, cura Edvardi Bernardi. Tomis duobus, in folio, 1694. N° 211, p. 160.

Observations on the Viper, and some other Poisons: written by Sir Theodore de Mayerne. Communicated by Sir Theodore de Vaux, M.D. and S.R.S.* N° 211, p. 162.

The venom of a viper in itself is not mortal to a robust and sound body; and though very unhappy and mischievous accidents attend it, as a great tumor, tension and weight of the part, humidity and variety of colours, frenzies, convulsions and vomitings; yet in 8 or 10 days at most these symptoms are over; and although the patient may be very ill, yet he recovers again; whilst the poison having run through several parts of the body, at last throws itself into the scrotum, swelling it extremely, and causing a great heat and quantity of urine, very hot and sharp, by which the poison is discharged; this evacuation being the ordinary and most certain crisis of the disease.†—It is observable, that the perspiration being obstructed by the poison, a man bit by a viper and swelled up, in 3 or 4 days will weigh almost as much more as he did before. A sickly person under an ill habit of body, or fearful, dies infallibly, and in a short time if not speedily relieved.

In the extreme nervous parts near the pulse and tongue, the bites are dangerous, and the symptoms very painful. Fresh vipers that have not bit, but have the bladders of the gums full of venom, are the most mischievous. Hence mountebanks to impose on the people either make their vipers bite before they bring them out, or with a needle scratch the gums, and press out the poison.

* Sir Theodore Mayerne, Baron of Aubonne, was born at Geneva in 1573, and studied both at Heidelberg and Montpellier; at which last university he took his medical degree; after which he settled at Paris, and became physician to the French king Henry IV. In 1600 he attended the Duke of Rohan in his embassies to Germany and Italy. In 1607 he came over to England with a nobleman of this country, whom he had recovered from a dangerous illness on the Continent. Being introduced to James I. this monarch was so much pleased with him, that he offered him the appointment of first physician to himself and the queen; an offer which was gladly accepted by Mayerne, as in consequence of his religious tenets (he being a protestant) his situation at the French court began to be unpleasant, while he was at the same time at variance with the faculty of medicine at Paris, in consequence of his partiality for certain chemical preparations, which they had condemned. After the death of James I. he continued to enjoy the honor of being physician to the royal family under Charles I. He died at Chelsea in 1655, aged 82, having accumulated by the exercise of his profession a very large fortune. Some years after his death his manuscripts, containing his Consilia, Epistolæ, et Observationes; his Pharmacopœia variæque Medicamentort. Formulæ; Syntagma Praxeos, &c. were published, making altogether a large folio volume.

† According to other accounts the crisis is chiefly by perspiration

For these great and painful swellings the remedy is, to drink the decoction of marrubium, or the powder taken inwardly, and a fomentation with the decoction, applying the rest on the place. Aristolochia is also a strong and powerful antidote against the viper, so that if one be bit on the tongue, he need only take a slice of this root, heat it and apply it, and it cures it.*

Pontæus, a chemical mountebank, a native of England, and an apothecary by trade, from whom I had the above-mentioned observations, was one day accidentally bit by a fresh viper on his right wrist, he thought it would have killed him: one of the greatest pains he suffered was almost an insupportable tension of the palm of his hand. He composed his antidote of extract of juniper berries, drawn with a decoction of roots of round aristolochia, of succisa, marrubium album, flower of brimstone, and white vitriol. For poisons not corrosive, such as those of animals and vegetables, and even for the plague itself (which he believes he can cure by the same remedy) he uses no vitriol. But if the poison be sublimate, which of itself excites vomiting, he adds vitriol, not in a proportion to vomit, as ʒj, but only ℈j or ʒss, the vomiting being assisted by the corrosive poison itself. To increase the value of this antidote with the people, when the experiment is made on dogs, to that dog which he would have die of the bite of the viper, he gives with the antidote a quarter of a nux vomica, not powdered, but only cut in bits, and the next day the dog dies; if it were powdered the dog would die in half an hour. He says nux vomica is so called by contraries, for it never vomits, but shuts up the stomach, and contracts the nerves by its poison. To preserve the dogs alive, you must give them, with the antidote, or any thing else, 3 or 4 grains of sublimate which immediately sets them a vomiting, and so saves them. He much esteems morsus diaboli, succisa, or devil's-bit, against all sorts of poisons; he laughs at the poison of a toad, which he says has none at all, no more than a frog.

His sugar or remedy for worms in children, is 15 grs. of mercurius dulcis, with 5 grs. of scammony, and 2 or 3 times as much sugar made up in lozenges. He says, that this dose, which in France purges grown persons, affects not in England those above 15 years old, and ought to be augmented. His mercurius dulcis is made without vitriol, which though good is yet corrosive; he takes only ashes with decrepitated salt.

The manner of the acting of the viper's poison is thus; in about three quarters of an hour, a syncope or swooning seizes the patient, with tremblings

* The vol. alkali, either in the form of the vol. spirit (spir. ammoniæ vel alkohol ammoniatum) or in the more compound form of what is sold under the name of eau de luce, is now generally considered as the best antidote against the viperine poison.

and convulsions, tingling of the ears, and frequently deafness for a moment: next, as it were, a whitish mist comes before their eyes, which soon vanishes; the part bitten swells at first to the size of a pea, after that it grows as large as a bean or a nut, and increasing spreads itself over the neighbouring parts to a very considerable tumour, and stretching of the flesh, which grows œdematous, and by little and little falls into the scrotum, and leaves the part black, blue, and yellow. It makes as it were bags in the skin, which feel heavy when you walk, as if filled with quicksilver. Use what means you will, the poison will have its course, and it is usually 3 days before it comes to the height, and as long abating. For those great swellings and stretchings of the parts, a fomentation and cataplasm is made with marrubium, thapsus barbatus, and agrimony.

The gall of the viper is nowise venomous, and Pontæus says, he usually gives it without any ill effect or trouble, but its bitterness; all the poison is in a liquor in the gums, which is yellow like oil. The viper is the most venomous of serpents;* the asp is but a species of the viper. The napellus is a very dangerous poison, acting by its acrimony, but you must take a great quantity of it: it burns the throat extremely, as does alum, but it is cured by the antidote. Crude antimony does nothing if attacked by the antidote. The most mischievous of all poisons is opium; † of which having given an excessive quantity to his servant, at first he had convulsions, then strange vomitings, not able to let any thing go down into his stomach, a sleepiness followed; all which time they kept him awake as much as possible. At last all of a sudden he grew well, and called for victuals.

Succisa, or devil's-bit, is excellent for poisons, especially the plague: and it is observable, that it is so powerful a sudorific, that laying the sick person on a bed of that herb, moderately hot, he will sweat till they take him off, and much more if he drinks of the decoction or juce of the herb. He says, that for the dropsy one of the best remedies in the world is to take morsus diaboli, and put it over the fire in a dry kettle, that it may wet it only with its own juice, and of this to apply a quantity to the belly and reins of the patient, covering him up warm, and so provoking sweat; which will come away in great quantity, and may be maintained according to the strength of the patient, and exigency of the case.

* The viper (coluber vipera, Linn.) is not the most venomous of serpents. The rattle-snake (crotalus horridus) and the cobra de capello are far more deleterious. The last indeed belongs to the same genus with the viper, being the coluber naja of Linnæus. See Russel on Indian serpents.

† Neither is opium the most mischievous of all poisons, or of all vegetable poisons, as we suppose the author's meaning to be. The aconitum napellus, and the prunus lauro-cerasus are more certainly fatal.

An Account of several late Voyages and Discoveries to the South and North, towards the Straits of Magellan, the South Seas, the vast Tracts of Land beyond Hollandia Nova, &c. Also towards Nova Zembla, Greenland or Spitsberg, Geenland or Engronland, &c. By Sir John Narborough, Capt. Jasmen Tasman, Capt. John Wood, and Frederick Marten, of Hamburgh. To which are annexed a large Introduction and Supplement. The whole illustrated with Charts and Figures. London, 1694, 8vo. N° 211, p. 166.*

Account of the Giant's Causeway, in the North of Ireland. By the Rev. Dr. Sam. Foley. N° 212, p. 170.

The giant's causeway is about 8 English miles north-east from the town of Colerain, and about 3 from the Bush-Mills, almost directly north. It runs from the bottom of a high hill into the sea, how far is not known; but at low-water its length is about 600 feet, and its breadth in the broadest place 240 feet, in the narrowest 120 feet; it is also very unequal in height, being in some places about 36 feet high above the level of the Strand, and in other places about 15 feet. It consists of many thousand pillars, which stand mostly perpendicular to the plane of the horizon, and close to one another; but we could not discern whether they run down under ground like a quarry or not. Some of the pillars are very long, and higher than the rest; others short and broken: some for a pretty large space of an equal height, so that their tops make an even plane surface; many of them are imperfect, cracked, and irregular; others entire, uniform, and handsome, and these of different shapes and sizes. We found none square, but almost all pentagonal, or hexagonal; only a few had 7 sides; and many more pentagons than hexagons; but they are all irregular, none having their sides of equal breadth; some of the pillars are 15, some 18 inches, some again 2 feet in diameter; none of them are one entire stone, but every pillar consists of several joints or pieces, of which some are 6, some 12, some 18 inches, some 2 feet deep. These pieces lie close upon one another, not joining with flat surfaces, but one of them is always concave in the middle, the other convex. These joints are not always placed alike; for in some pillars the convexity is always upwards, and in others it is always downwards. They always lie as close as possible for one stone to lie on another, so that on the outside of the pillars you can only discern the crack that joins the two stones. When you force them asunder, both the concave and convex surfaces are very smooth, as are also the sides of the pillars, which touch each other, being of a whitish free-stone colour, but a finer and closer grit; whereas on breaking some pieces off them, the inside appears like dark marble. The pillars stand

* So many collections of voyages, and histories of maritime discoveries, are now extant, that it is deemed unnecessary to reprint a detail of the contents of the above-mentioned work.

so close one to another, that a knife can hardly be thrust in between their sides; and though some have 5 sides, and others of them 6, yet their contexture is so adapted, that there is no vacuity between them: the inequality of the numbers of the sides of the pillars being often in a very surprising and wonderful manner, throughout the whole causeway, compensated by the inequality of the breadths and angles of those sides; so that the whole at a little distance looks very regular; and where in many places a good number of the pillars are exactly of the same height, the superficies of their tops looks very like the pavements that are in some gentlemen's halls. Every single pillar retains its own thickness, and angles, and sides, from top to bottom. Those which seem to be entire, as they were originally, are at the top flat and rough; those which lie low to the sea are washed smooth; and others, that seem to have their natural tops blown or washed off, are some concave, and others convex.

Some Notes on the foregoing Account of the Giant's Causeway, serving further to illustrate it. By T. Molyneux, M. D. S. R. S. N° 212, p. 175.

Among the several figured stones already described by authors, I find none that has more agreement with those that compose our Giant's Causeway, than the entrochos, the astroites, or lapis stellaris, and the lapis basanus, or basaltes; and yet they differ very much in some particulars. The entrochos agrees with the pillars of our Causeway in that it is a stony substance, formed by nature columnwise, and consisting sometimes of 20 or 30 several internodia, or joints set on each other; but then it differs in that its outward shape is round and cylindrical, in its having a hole or pith run from top to bottom through all the joints, in the setting on, or way of fitting one joint to another, and in its size and magnitude.

But the make of the astroites, or lapis stellaris, seems to have still a greater affinity in its formation with our Irish stones; for it is not only shaped columnwise, as the entrochos, and jointed with several internodia closely adjusted to one another, but its sides are angular, and the manner of the commissures of one joint to another, in some particulars, more resembles the way nature observes in jointing this stone. It must be observed, that the sides of the astroites are always sulcated, or a little furrowed, and are constantly pentagons; whereas the Irish stone has its sides perfectly smooth, and plain, and sometimes in hexagons and heptagons, as well as pentagons.

But the astroites also, as well as the entrochos, differs extremely from our stone, in its size or magnitude; for the largest that is found of either of those kinds do not much exceed the thickness of a man's thumb, whereas our columns are some of them 2 feet in diameter. Yet this disproportion of bulk is not so considerable a difference, since we observe that nature affects the like disparity in other of her works, and those too nearly allied, and evidently of the same tribe or family.

But nothing among all the fossil tribe, that I have seen or read of, comes so near in all respects, in its formation, substance, size, way of growth, or manner of standing, &c. to our columns, as the lapis basaltes misenus, described by Kentmann, in Gesner de Figuris Lapidum, from whence Boetius takes both his figure and description, of which he says there is a large bed within 3 miles of Dresden in Saxony, and described in such words, that he could hardly have better described the collection of pillars, that make the Giant's Causeway, seeming as if he had seen them on the place; only I find this difference between them and the Misnean basaltes, that its columns were one entire piece from top to bottom, and some of them four-squared; whereas our Irish basaltes is composed of columns, whereof none are four-squared, and all of them divided into many joints.*

Account of the Evaporation of Water, as experimented in Gresham College in the Year 1693; *with some Observations. By Edm. Halley.* N° 212, p. 183.

I caused an experiment to be made of the quantity of vapour arising simply from the warmth of the water, without being exposed either to sun or wind, which has been performed with great care and accuracy by Mr. Hunt, operator to the society: having added together the evaporations of the whole year, I find that, from a surface as near as could be measured of 8 square inches, there evaporated during the year, 16292 grains of water, which is 64 cube inches of water, and that divided by 8 inches the area of the water's surface, shows that the depth of water evaporated in one year amounts to 8 inches. But this is much too little to answer to the experiments of the French, who found that it rained 19 inches water in a year at Paris; or those of Mr. Townley, who, by a long continued series of observations, has sufficiently proved that in Lancashire, at the foot of the hills, there falls above 40 inches of water in a year. Whence it is very obvious, that the sun and wind are much more the causes of evaporation, then any internal heat, or agitation of the water. The same observations also show an odd quality in the vapours of water, which is that of adhering to the surface that exhaled them, which they clothe as it were with a

* Then follows a short postscript wherein are pointed out some errors into which the author of the account of the Giant's Causeway, inserted in No. 199, of the Trans. [p. 529 of this vol.] had fallen; the substance of which postscript is given in the note belonging to that account.

fleece of vaporous air, which once investing it, the vapour rises afterwards in much less quantity: which was showed by the small quantity of water that was lost in 24 hours time, when the air was very still from wind, in proportion to what went off when there blew a strong gale, although the experiment was made, as I said, in a place as close from the wind as could well be contrived. For which reason I do not at all doubt, that had the experiment been made where the wind had come freely, it would have carried away at least three times as much as we found, without the assistance of the sun, which might perhaps have doubled it. By the same experiment it likewise appears, that the evaporations in May, June, July, and August, which are nearly equal, are about three times as much as what evaporated in the four months of November, December, January, and February, which are likewise nearly equal; March and April answering nearly to September and October. This fleece of vapour in still weather hanging on the surface of the water, is the occasion of very strange appearances by the refraction of the said vapours differing from that of the common air, whereby every thing appears raised; as houses like steeples, ships as on land above the water, and the land raised, and as it were lifted from the sea, and many times seeming to over-hang. And this may give a tolerable account of what I have heard of seeing the cattle at high water-time in the Isle of Dogs from Greenwich, when none are to be seen at low-water, (which some have endeavoured to explain by supposing the Isle of Dogs to have been lifted by the tide coming under it.) But the evaporous effluvia of water, having a greater degree of refraction than the common air, may suffice to bring those beams down to the eye, which when the water is retired, and the vapours subsided with it, pass above, and consequently the objects seen at the one time may be conceived to disappear at the other.

A Letter from Sir Dudley Cullum, to John Evelin, Esq. concerning a lately invented Stove for preserving Plants in the Green House in Winter. N° 212, p. 191.

I send you an account of the success of your lately invented stove for a Green House. I have pursued your directions in laying my pipes, made of crucible earth, not too near the fire grate, viz. about 16 inches; and by making a trench the whole length of my house, under the paving, for the air to issue out and blow the fire, of a convenient breadth and depth, that is, 18 inches both ways, covered with an arch of bricks, and at the other end of the trench having a square iron plate answerable to that of my paving, which is 18 inches, to take off and put on, with a round hole at each corner, of about 3 inches diameter, with a lid to slide open, and shut, upon every end of them; so that by opening any of these holes, or all of them more or less, or taking off the whole plate, I can release such a quantity of air out of the house to blow the fire, as to increase or diminish the blasts; and, as you were pleased by letter to inform me, concerning distributing the air at its admission more equally though the house, I have inserted my pipes into a channel all along the wall at the end of the house, with those several openings you mentioned, &c.

On a Whirlwind, in a Postscript of a Letter, dated Aug. 4, 1694, *from Warrington in Northamptonshire, to a Rev. Divine in London.* N° 212, p. 192.

On the first instant, there happened here, between one and two o'clock in the afternoon, a very terrible whirlwind. It took up into the air about 80 or 100 shocks of corn, carrying a great deal quite out of sight, the rest it scattered about the field, or on the tops of the houses or neighbouring trees. I have seen corn, which was carried a mile from the field; and it is reported by persons of good credit, that some was carried 4 or 5 miles distant. The whirlwind continued in Acremont Close full half an hour; I myself, and several other persons, saw at least three or four waggon-loads of corn all at once whirled about in the air.

Extract of a Letter from M. Anthony Van Leuwenhoeck, to the R. S. containing the History of the Generation of an Insect, by him called The Wolf. With Observations on Insects bred in Rain-Water, in Apples, Cheese, &c. N° 213, p. 194.

The wolf* is a small white worm, armed with two red shears or teeth at the fore-part of its head, with which it bores and feeds on the grains of corn, and makes its way through wood itself. Having formerly often inclosed some of these worms with some wheat in small glass tubes, I always found that they died before the time of generation: wherefore in the summer I put some of them with the wheat in a box, and observed that one of them joined 6 or 8 grains together, lodging itself in one of them, the rest being likewise all hollow and eaten out. The worm spins a thread, with which it joins the corn together, and fastens itself to glass or other smooth bodies. When I put them in boxes, they eat their way out; and when I put them in glass tubes, they bored through the corks that stopped the glass tubes; to prevent which, I

* This insect is the larva or caterpillar of the *Phalæna granella*. Lin. It belongs to the tribe of Tineæ, and is thus described by Linnæus, viz. *Phalæna Tinea alis albo nigroque maculatis, capite albo.*

covered the cork with sealing-wax, and to give them air, put a very small glass tube through the wax and cork. The beams of the granaries likewise are all eaten, for they leave the corn, and creep up the walls to the timbers of the ceiling, where they fasten themselves and remain till their change.

The worms in my glass tube, which was about a foot long, and a finger wide, fastened themselves to the sides of the glass, and lay still all the winter; the web that covered them was so thin, that I could with my microscope perceive a small motion of their heads through it. On April 29th following, they began to look red, and somewhat shorter than before; on the 30th they were redder, and changed into aurelias. May 23d they were of a dark red, and the next day one of them was changed into a small moth, leaving its useless skin, or winter coat. This moth had white wings with black specks; my microscope discovered these wings to be covered on both sides with feathers, some of which were tipt with black. The moth had four wings, each wing adorned with three rows of feathers, very long in proportion to the little creature, and each row increasing in size above the other; every feather was not round at the end, but indented tooth-like. May 25th I put into several tubes a male and female moth, which were distinguished by the male being smaller; and after they had coupled, I opened some of the females, and found between 50 and 70 eggs in each. On the 26th I found in my fore-mentioned tube 6 moths flying; on the 27th I found one moth had laid near 70 eggs, each of the size of a small sand, and of the shape of a hen's-egg: soon after they had laid their eggs the moths died. Those eggs that were laid, the 26th of May, by carrying in my pocket next my body in a glass tube, were by too much heat spoiled, the worms that were in them being killed: I therefore put them in a cooler pocket, and on the 3d of June found some of them hatched, and the worms creeping on the glass. I gave them some grains of wheat, in which they soon housed themselves. Those eggs which were laid about the 25th of May, and were not keep warm, but laid in my closet-window, were not hatched till about the 10th of June: so that the warmth of the body hastens their hatching. The corn-merchants observe them not till about August, though they are hatched in about 16 days after the moth flies about, and are not perceived by reason of their smallness, and their hiding themselves in the first grain of corn that they eat into, and are not seen till they quit that for another. These worms are not only destructive to corn, but are also in old timber, books, boxes, woollen stuffs, and the like.

This being so destructive and prolific an insect, for of 70 eggs I found but one barren, and 3 with dead worms, I thought of a way to destroy them, which is thus: I took a glass vessel, and put into it 8 moths, and firing some brimstone therein, they were soon killed by its fume; and 3 hours after putting some more moths into the vessel, the scent that remained therein killed them; from hence guessing at what might serve for a larger room, I took for a granary 24 feet long, and 16 broad, ¼ of a pound of such brimstone, prepared so as wine-coopers use to do for their casks, which I placed in the midst of the room, and setting it on fire, left it, shutting the door, and after two days I came again, and found some moths alive on the wall, but not a tenth of what used to be there, and which I judged might get into the room through the broken glass of the window, or they might have crept out of the aurelia after the smoke was over; wherefore the best way is to smoke the room as soon as the moth appears, and that for some days successively, which is but a small charge. And in autumn it would do well to sweep the worms off the walls, for being a tender insect, they are easily killed.

At the time of the wolt's creeping up the walls, I saw many small animalcules of the size of a great sand on the walls also; they had their hinder part broad and short like a louse, and four horns. These animalcules laid small eggs, in shape almost of a lemon: they were not long-lived, possibly for want of their proper food, for some of them had eaten others for hunger: these eggs were hatched in May, and the young ones were of the same shape with their parents, so that these insects produce their like, as do lice, mites, fleas, &c.

In rain-water I observed a small red worm, and two other kinds of very minute insects; of those of the larger size I judged that 30,000 together would not equal a coarse sand. These I observed for several days, and saw them copulate, the larger dragging the smaller through the water after them, swimming by means of very small fins. I saw likewise another sort of smaller insects in the water coupled, 20 times more in number than the former.

As to insects bred in apples, I observe that in the spring, when the trees begin to bud, a certain black fly lights on the blossoms, and lays its eggs there. This insect flies from one bud to another, and I doubt not but if we observe these black flies, and the caterpillars which soon follow them, we should find that the caterpillars generated by black flies, change to black flies again.

About two years since I put some cheese-maggots* in a glass tube in my pocket, and at last found the maggot turned to a red aurelia from pure white; and these again changed into a black fly; which experiment I often renewed with the same success, though I could not keep these flies till they laid eggs, it may be for want of convenient food. From these and several other observations, I conclude there is no generation but from the parent animal.

* These maggots are the larvæ of the *muscâ putris.* Lin.

*A Catalogue of those Oils that will take Fire with a great Noise and Explosion, when the Compound Spirit of Nitre is poured upon any of them; and of those Oils that only make a great Noise with Explosion, but will not take Fire; and also of those that do not make either Effervescence or Explosion. The first is marked with two Stars * *. The second with one *. The last has no mark at all. By Dr. Fred. Slare.* N° 213, p. 200.

- **Oils.**
  - **Vegetable.**
    - **Essential.**
      - Perfect stillations made by the analysis of chemical fires, where the oleaginous particles are truly separated from all other,
        - Carui ** / Cummin * / Fennel * / Dills * — **Seeds.**
      - Light, or ethereal, which are specifically lighter than water and brandy, and some than spirit of wine, and are usually drawn from the seeds of vegetables: such as from
        - Bay * / Juniper * — **Berries.**
        - Thyme * / Wormwood * / Angelica * / Hyssop * / Lavender * / Rosemary * / Penny-royal * / Rue * / Sage * / Savin * — **Tops of Plants.**
      - Ponderous or heavy, which do commonly sink in water, being distilled from the heavy parts either of the wood or cortex of trees. Such as from †
    - **Not Essential**
      - Imperfect, made by expression, which are decompounded of several parts of the plants,
        - Lemons * / Oranges * / Nutmegs * / Cloves * * — **Fruits.**
      - As of
        - Almonds / Olives / Walnuts — **Fruits.**
        - Line / Rape — **Seeds.**
      - †
        - Sassafras ** / Guaiacum ** / Box ** / Camphire ** — **Wood.**
        - Jamaica Pepper** / Cinnamon ** — **Cortex.**
  - **Animal of**
    - Solid Parts
      - Hartshorn **
      - Man's skull **
      - Hoofs **
    - Fluids,
      - Human blood **
  - **Mineral of**
    - Amber or Succinum.
    - Petroleum.
    - Barbadoes tar.
    - Bees-wax.

Spirit of wine will give some flashes of fire. Balsam of sulphur, a compound body made with oil of turpentine and brimstone, if not too thick, will flame.

† You have 12 sorts of oils that make ebullition, explosion, and flame; 18, ebullition and explosion without flame; 4 that produce neither; by the mixture of our compound spirit of nitre.

Experiments relating to the Production of Fire and Flame, with an Explosion, made by the Mixture of two Liquors actually cold. By Frederick Slare, M. D. Censor and Fellow of the College of Physicians, and F. R. S. N° 213, p. 201.

I gave an account of several experiments * made by the mixture of various liquors, some of them produced much heat, others a great ebullition with a cold effect; others sparks of fire without ebullition, and some few that produced an actual flame; but the difficulty to make the phosphorus, which was used in the greatest of these flaming experiments, is such as has made that sort of experiment scarcely credible by some, and practicable by few; insomuch that I question not but that it will be grateful to the curious virtuoso of this age, to see two clear liquors both of them actually cold, without any intermediate or third body, rise up to a flame by kindling one another; besides having made it easy for them to make or procure the ingredients which perform this, not common effect, it may yet be more acceptable.

To make the experiment of Accension.—Take of any one of the essential oils, set down in the preceding catalogue, one part, of the compound spirit of nitre two parts, and they will with great celerity and a great noise produce a flame, which lasts a very little while, but leaves an insipid caput mortuum, as light and tasteless as a cobweb.

But note, 1. This experiment should be made under a chimney, or any convenient draft, that the offensive steams may evaporate. 2. A gallipot, spacious enough to hold 4 or 5 ounces of water may be a convenient vessel for this experiment, if you only use about a dram of each ingredient; but if you use larger quantities, then you must enlarge the vessel. 3. You must put the oil into the gallipot first, and then pour the spirit on the oil, because the spirit, being heavier, the latter passes through the oil, and makes a more expeditious mixture. This must not be dropped in gradually, but all at once. 4. Hold not your head too near the gallipot, lest the sudden explosion of the matter should strike up some of it into your face. 5. The compound spirit will lose much of its virtue if kept too long.

To prepare the spirits for this experiment.—Take of salt-petre and oil of vitriol equal parts, and distil these out of a retort in a good sand furnace, so that the sand continue red-hot for some hours, for the fire cannot be too great; the fumes will rise of a very deep red colour, and will settle in the receiver, in the form of a liquor, which must be carefully preserved from the air; this being the spirit with which all our experiments were made, which are referred to in the

* Philosophical Transactions for 1683.—Orig.

catalogue. To make the common spirit of nitre, you need only to mix 5 or 6 times as much clay as you take of nitre, and distil them in a retort, and you may obtain a strong spirit of nitre this way, especially if you dephlegm it, and rectify it to the best advantage. With this we have made an experiment of accension succeed sometimes, though with great uncertainty; but the first, which I call the compound spirit of nitre, is only to be relied on.

This compound spirit seems to be the active principle that stirs up the oily or more passive particles to take fire, which will more easily be agreed to, if we consider that our compound spirit of nitre does not only consist of all those igneous particles, to be found in common spirit of nitre, but that it has also all those igneous particles which oil of vitrol contains in it, crowded into our spirit of nitre made this way. For further illustration, let us consider what oil of vitriol is; it is a creature of the fire, that the sulphur, which is plentifully in vitriol or in copperas, is accended, and afterwards distilled over in the form of a liquor, which is a liquid sort of fire, as having many properties of it. If you put it to water, it will make it boiling hot; it burns not only linen and woollen but wood, to a coal, and scarcely spares any thing.

Nitre, the other ingredient of our spirit, is very susceptible of flame, which also incorporates many igneous and corrosive particles, after it has so many hours lain ignited in the fire, comes over by distillation, very highly impregnated with the same fiery particles.—For nitre itself has no dissolving or heating quality, but is a great cooler, and scarcely can be reckoned among acids; but after it comes out of the fire in the form of a spirit, it tears in pieces almost all metals, and brings them to a sort of fusion, as actual fire does; it dissolves animals and vegetables, and minerals, and has many effects of fire. Therefore from the union of these two very fiery spirits results a much greater quantity of igneous matter.

That fire is very apt to incorporate with fluids, and even such as have had but a small communication with it, an experiment which I formerly exhibited at a meeting of the Royal-Society, makes probable. We took of spirit of wine, highly rectified, a wine-glass half full, and placed a tender weather glass or thermometer in the glass, and then put a spoonful of water to it; this immediately warmed the liquor, and made the weather-glass ascend 2 inches at least; the liquor in the weather-glass subsided as the other mixture grew cold. I made it more sensible to the touch by filling the palm of the president's hand with spirit of wine, and putting a small quantity of cold water into the same hand, which made it sensibly warm his hand, as well as others that made the trial. But from this spirit, which is too volatile to endure much communication with the fire, you may expect only a mild tepid heat; I am apt to believe, that there is scarcely any thing which lies long in the fire but is apt to retain some igneous particles; which appears to be so in all fixed salts, in quick-lime, and more particularly in iron. If you take a bar of iron, though of a hundred years old, and file off about a pound of it, and then mix and imbibe these filings with a due proportion of water, enough to make the whole just moist; the fire, which all this while lay concealed in the iron, being more disposed to enter into the fluid, does by these means warm the whole mass. The iron gained this heating quality by fusion in those fierce fires which first separated the metal from the ore. For it is not in the nature of the ore before fusion to emit any heat, as I have found by mixing water with it. There are a great many other instances which make it very plain that fire will add both to the bulk and weight of bodies; but these affect solid bodies more manifestly. The effect itself produced by our fluid does necessarily prove the inherent fiery particles to have caused the accension.

Oils may be distinguished into vegetable, animal, and mineral; we subdivide again the vegetable into those made by expression, and those made by distillation; and of those made by distillation we distinguish those that are made out of the seeds from those that are made out of the trunks, or cortex, or roots, or any other part of the vegetable. We further observe a difference between those seeds that have only a fragrant smell and a pungent taste alone, from those that have both odorous emanations and a brisk taste together, and those that are insipid and have no smart taste. In the first place we must set aside those oils made by expression; for having tried oils of linseeds, of nuts, of olives, of almonds, &c. we found these would not make explosion or ebullition, or so much as any fermentation with our fiery meteor. Nor could I, without much stirring, bring them to incorporate; and when they did incorporate the heat was but just sensible. To examine briefly the mineral oils; there are some, as oil of vitriol, oil of sulphur per campanam, that have not any property of oils, but are rather acids, and corroding menstruums. There are others that have the true property of essential oils, as oleum petrolei, and Barbadoes tar highly rectified, which do not produce any remarkable heat, much less make an ebullition or explosion: nor does that active oil of amber do any thing more. The stillatitious oil of bees-wax had much the same effect when it was incorporated with our compound fiery spirit; and this inclines me to believe that the wax itself may be a compound more belonging to a mineral than vegetable nature. Now of those essential oils, which produce great ebullition, explosion, and flame, with the compound spirit, we have two sorts, vegetable and animal. The true vegetable essential oils all make violent ebullition and explosion, and several actually take fire and flame. If it be inquired what share the oil has in producing this fire, whether it be only a pabulum, or fuel, for the spirit to actuate, and so be merely passive? or whether it contributes any particles that help to excite this flame? In order to the resolving this doubt, we consider, that these essential oils are produced from seeds that have very active or warm parts or spirits, such as will easily ferment and heat. And besides the potential heat of the vegetable, essential oils contain a volatile salt, which gives much of that pungency to the taste. So that our oil is not a bare pabulum ignis, or an unactive principle, but on a double account, both on the score of the incalescent oil and of the inherent salt, conspire with the compound spirit to make this great heat, explosion, and accension.

In the catalogue of experiments we may further observe, that of the light essential oils drawn from seeds of vegetables, all of them make a great ebullition with an explosion, but that few of them do actually take fire, and that all of those that are extracted from trunks, or other parts of our vegetables, do certainly take fire and flame. Wherefore, having observed that those which do not take fire or flame, do yet make as great an explosion and ebullition, and probably as great a heat as those that did, I was apt to impute it to the lightness and too great subtilty and volatility of those essential oils, whose very active particles too soon exhale or fly away. And this conjecture seems to be justified by the addition of a more grumous body, as balsam of sulphur made with oil of turpentine, to our most volatile or subtile oils, which then produce a flame, whose particles being grosser, or more grumous, will detain the more volatile oil from too sudden an explosion, and give more time to the fiery spirit to penetrate, and mix itself with those combustible materials. And this may be one reason why the ponderous oils distilled from the roots or ligneous parts of a plant do all take fire, viz. because the parts of this sort of oil, lying closer together, do not so soon dissipate after the spirit is thrown upon it. And then as to the specific gravity, the difference is also very considerable, which any one may find by this familiar way; if you fill a glass with 1 ounce of the essential oil of the seeds, you will require 9 drams of the ponderous oil of the vegetable to fill up the same space. This is also obvious to any spectator, that most of these oils thus distilled are more ponderous than common water, by their sinking to the bottom; whereas all our essential oils, extracted from the seedy parts, swim on water, and some are lighter than the best rectified spirit of wine, but most are lighter then brandy, which has made the chemists call them ethereal oils.

The ponderous oils have yet one advantage over the lighter volatile oils; for being exposed to a longer and greater degree of fire than the others, they unite more igneous particles, which being put in motion, may contribute something to cause this accension.

The oils distilled from animal bodies do all take fire and flame, but with this difference, that they do not produce so great an explosion as the vegetable do, but they more certainly take fire, and will continue their flame longer, though not so fierce as the other. If we rightly examine the constitution or texture of this oil, we have several properties adapted to the production of this effect. There is a much greater degree of fire required in the distillation of this oil than is necessary for that of the vegetable; also a great quantity of volatile salts pass over with the oil; and you have a ponderous oil that sinks in water; all which being considered and weighed together, make it probable that a more constant accension will ensue from the animal oils than from any other.

Oleum succini is justly put in the catalogue of minerals, and is produced by a strong degree of fire; yet it causes no motion, and scarcely any incalescence, notwithstanding it abounds in volatile salts; the reason is, because these salts are not properly volatile, as alkalies are, but belong to the family of acids, and so can make no fermentation with this compound spirit, which is itself highly acid.

I shall venture here to add one surprising effect of this fiery mixture, as follows:

We took half a dram of the oil of carui-seeds, and poured it into a little gallipot, and put a dram of our compound spirit of nitre in a small phial into the same gallipot, and placed over it a glass that held 3 pints on M. Papin's exhausting engine: and having cleared it of the air, we turned up the phial in order to see what effect would ensue, in this sort of vacuum, on this mixture; but in a moment the receiver was blown up, and the mixture in a flame; which is the more surprising as Mr. Boyle found that in all his experiments the removal of the air did almost always extinguish light, fire, and flame. The blowing up of the glass also makes the experiment the more extraodinary, and puzzles one how to account for so great a quantity of air as was produced from these liquors, which amounted only to a dram and a half; for here was required not only air enough to fill up the capacity of the vessel, but also so great a pressure within as exceeded that great incumbent weight of the air that pressed upon this capacious glass without, whose diameter was 6 inches and the depth above 8, for otherwise it would not have thrown it up into the air.

This fiery mixture and gunpowder agree, in that they both heat, burn, and flame; and also considerably resist and raise up bodies that oppose them; in both the air is much agitated and expanded.

We further made an experiment in pleno, or after this manner: We put a small quantity of the oil in a gallipot, and some of the spirit in an open glass, and fixed a plate of copper upon the gallipot, so as to cover it pretty exactly, and then set a weight upon the plate, and pulling a string, made the spirit to mix with the oil, which at that instant blew up the cover, and threw off the weight. But though in some respects it agrees with gunpowder, yet in others there is a great disparity: for gunpowder will not be made to take fire, or make any explosion in vacuo, both which this mixture performed with the same celerity at it did in the air. Gunpowder is a composition of the most dry and combustible materials we can put together; whereas in our mixture of two fluids, one of them is not easily made to burn by itself, and the other will extinguish common fire. Gunpowder requires actual fire to bring it to an accension; whereas in this you have only two waters or liquors, both cold to the touch, that produce fire and flame by the bare joining and mixing them together.

On the extinction of the flame, produced as above, you will have remaining a light and blackish substance, which will indeed vary both as to the bulk and complexion, according to the difference of the oils. But in this they all agree, viz. to leave behind a spongy and exceedingly light matter, and perfectly insipid. Sometimes it swells up into a great protuberance, as large as a man's fist, above the gallipot, and if you taste it, and macerate it in your mouth, you will find it to be as tasteless as paper, or even paper when burnt to ashes. So that we may safely conclude, that by this powerful mixture, a third solid body results, absolutely differing from either of the two mentioned liquors; and which makes it the more remarkable, that both of these fluids, which have so great an impression on the organs of smell, and a very great one on the organs of taste, should in an instant be destroyed, and terminate in a dry insipid caput mortuum, which will not melt in the air, nor be dissolved by water, nor other corrosive menstruums, but remains as much a caput mortuum as a piece of paper, or a rag burnt to ashes, if not much more.

Mr. Molt, a most ingenious chemist, informs me, that spirit of wine would give a flash of light with this compound spirit, but not burn; and he has observed the same circumstance in his experiment, which I did formerly in the year 1683,* that if you put the spirit of wine to the nitre, you will have a great effervescence immediately ensue; but if you invert the order, and put the spirit of nitre on the spirit of wine, you will not have any ebullition for some time: which circumstance is quite contrary to all the other experiments we have made

* See Transactions, N° 150.—Orig.

about the oils. That the spirit of wine does not take fire, seems to proceed from the same impediment which hinders light oils from rising up to an accension, because they are so suddenly thrown off; and there seems to be a great analogy between ethereal oils and the spirit of wine, both as to specific gravity, and other properties: spirit of wine seems to be a more thin and diluted essential oil, that contains some water and more air in its pores; they seem to own the same material cause; for if you distil an essential oil out of any seed, you shall not then be able to produce any spirit; and, vice versa, if you distil off the spirit first, no oil will follow. There is also a great affinity in texture; for the spirit and oil easily unite and mix together, especilly if the spirit be highly rectified, and have less of water or other heterogeneous matter in it, as any one may find if he will take the pains to shake a true essential oil with spirit of wine, a good proportion of the former will incorporate with the latter.

Postscript.—The pyrites being the mother of our oil of vitriol, which is the principal ingredient in our active fiery meteor, or compound spirit; I will here notice an account I received from an unphilosophical friend, who complained of his great loss, which he attributed as a punishment for his covetousness. He was master of a copperas work at Whitstable in Kent, and engrossed all the pyrites or copperas stone to himself, in order to the breaking of a neighbour's work, so that he had laid up 2 or 300 tun in a heap, and built a shed over it, to keep off the rain: but in the space of 6 or 7 months it first smooked, and then took fire, and burned for a week; it burned down the shed; some of it looked like melted metal, and other parts like red-hot stones: and it discharged so fetid, sulphureous, and stinking exhalations that the people in the neighbourhood were miserably afflicted, and forced to use all their endeavours to extinguish it.

How far this communication will serve the hypotheses of thoss that derive lightning and thunder and earthquakes from the matter of the pyrites, or will account for the rise or continuance of volcanoes, or even the great conflagration of the world, I leave to their consideration.

Extract of a Letter from the Rev. Mr. Thomas Dent, to Sir Edmund King, Kt. M. D. et S. R. S. Concerning a Sort of Worms found in the Tongue, and other Parts of the Body, &c. N° 213, p. 219.

This will in a great measure satisfy you about that distemper on my tongue, for which I have so lately had your advice; the chief cause of those rising tumours fixed on my tongue, proceeds from the disease of worms, as you will perceive from the following account. In reading M. de la Cross's Memoirs for the Ingenious, in the month of July, 1693, letter the 30th, I found an observation, which he has published to the world, concerning the cure of this disease of worms by one Sarah Hastings, who was famous in the discovery of them in the face, gums, and tongue; and which she managed with such dexterous art in the operation, that she took them out of any part affected with a goose-quill. I was hence solicitous to inquire out, if there were any of the worm doctresses now in being; and hearing of one famous at Leicester, I was resolved to write to her, describing all the symptoms as plain as I could explain them; to which I had a return, that she believed my disease to be worms; and being resolved to try the experiment, I took coach for Leicester, where being come, my doctress, Mrs. French, no sooner inspected the place, but instantly declared her opinion, that the distemper proceeded from worms. The next day she fell upon her operation, which was performed in the presence of two aldermen of the town, Mr. Gibbs, my lord of Derby's chaplain, and several others, when piercing the part affected with a lancet, she drew some blood, and soon after with a small spatula, and another instrument with which she opened the orifices, she picked out 5 or 6 worms at a time. She plainly showed them to the spectators as they came out of the flesh; they were all alive, and moved their heads, and are somewhat less then ordinary maggots. Thus in less than 8 days she took out of my tongue more then 100 worms, all nearly of the same size, except two very large ones, which she said were of a cankerous production; she took more than 30 out of my gums, which last operation is her daily practice; persons of good note resorting to her from all parts of the country thereabouts. I was very curious to inquire out what cures she had done of this nature; and I found a very satisfactory account from persons of some quality and note. The cures the woman performs in picking out these worms from all putrefactive ulcers, tumours, and sores, whether in the faces, noses, gums, or tongues of several persons prove that such animals are generated in those parts.

Extract of a Letter from Mr. Edward Lhwyd, to Dr. Lister; giving some farther Account of the Fiery Exhalation in Merionethshire. Dated Oxford, Aug. 23, 1694. N° 213, p. 223.

An intelligent person,* who lives near Harlech in Merionethshire, assured me the fire still continues there; that it is observed to come from a place called Morva-bychan in Caernarvonshire, about 8 or 9 miles off, over part of the sea. That cattle of all sorts, as sheep, goats, hogs, cows, and horses, still die apace:

* See Philosophical Transactions, N° 208.—Orig.

and that for certain, any great noise, as winding of horns, drums, &c. repels it from any house, or barn, or stacks of hay; on account of which remedy, they have had few or no losses in that kind since Christmas. That it happened during this summer, at least one night in a week, and that commonly either Saturday or Sunday; but that now of late it appears something oftener. The place whence it proceeds is both sandy and marshy.

Extract of another Letter from Mr. Anthony Van Leuwenhoeck, to the Royal Society. Concerning the Difference of Timber growing in different Countries, and felled at different Seasons of the Year. N° 213, p. 224.

As to the difference of timber felled in winter from that felled in the summer, the common opinion is, that the former is the stronger, and more lasting, as being more close and firm; but his own opinion is, that there is no difference, except in the bark and outermost ring of the wood, which in the summer are softer, and so more easily pierced by the worm. Wood consisting of hollow pipes, which both in summer and winter are full of moisture, they do not shrink in the winter, and therefore the wood cannot be closer at one time than another, for otherwise it would be full of cracks and clefts. The sudden and unexpected rotting of some timber, he conceives to proceed from some inward decay in the tree, before it was felled; having observed all trees to begin to decay at first in the middle or heart of the tree, though possibly the tree may stand and grow for near 100 years afterwards, and increase in size all along.

He says, he was once of opinion, that trees growing in good ground, but increasing slowly, were the best and strongest timber; and that those trees which in few years grew large, were the softest and brittlest; the contrary to which, on inquiry of experienced workmen, he found to be true, and instances in an elm of 80 years growth, which was 11 feet in circumference, and proved excellent tough timber.

The age of trees is to be known by the number of rings to be seen when the tree is cut across, in each of which is one circle of large open pipes: now the fewer there are of these large pipes, the stronger the timber is; hence those trees that make the largest growth in a year must be the closest and strongest; and therefore those trees that grow in warm countries grow fastest, and are the best and toughest timber, which he confirms by Riga and Dantzic oak, which is of slow growth, and proves spongy and brittle timber; whereas the contrary is observable in English and French oak, which grows faster, and is excellent timber.

Account of a Book, viz.—Myotomia Reformata: or, A New Administration of all the Muscles of Human Bodies; wherein the true Uses of the Muscles are explained, the Errors of former Anatomists concerning them confuted, and several Muscles not hitherto taken notice of described: to which are subjoined, a Geographical Description of the Bones, and other Anatomical Observations. Illustrated with Figures after the Life. By William Cowper, Surgeon. Lon. 1694, *8vo.* N° 213, p. 226.

This excellent work on the muscles is so well known to all anatomists, that it would be superfluous to insert an account of its contents. Cowper's anatomical merits have been already noticed in the account of his life, at p. 615, of this volume of the Abridgment.

Nobilissimo et Doctissimo Viro D. Roberto Southwell, Equiti Aurato, Societatis Regiæ Præsidi Dignissimo, S. D. David Gregory, Astr. Prof. Savilianus. Being a Paper ascribing some Mathematical Inventions to their true Authors. By Dr. Gregory. N° 214, p. 233.

The abbot Galloys having, in the year 1693, asserted that Mr. James Gregory and Dr. Barrow had stolen their general propositions concerning the transformation of curves from M. Robervall; Dr. David Gregory here fully refutes that assertion. For Mr. Gregory published his book at Padua 1668, and Dr. Barrow his Lectiones Geometricæ 1674, which M. Robervall doubtless had a sight of before he died, which was not till October 1675, yet he never complained of any such injury done him.

M. Cassini's New and Exact Tables for the Eclipses of the First Satellite of Jupiter, reduced to the Julian Stile, and Meridian of London. N° 214, p. 237.

Among the books with which the Royal Academy of Sciences at Paris has lately gratified the world, there is one entitled, Recueil d'Observations faites en plusieurs Voyages pour perfectionner l'Astronomie et la Geographie, avec divers Traités Astronomiques. In which those sçavans have set a very commendable example in ascertaining by undoubted observations the true geographical site of all the principal ports of France. The method they have used to determine the longitudes of their places, is by the observation of the eclipses of the first satellite of Jupiter, which they find almost instantaneous, and with good telescopes discernible almost to the very opposition of Jupiter to the sun: and it may be said, that this account of the longitudes observed has put it past doubt that this is the very best way, could portable telescopes suffice for the work. And could these satellites be observed at sea, a ship at sea might be enabled to find the meridian she was in, by help of the tables M. Cassini has given in this volume, discovering with very great exactness the said eclipses, beyond what we can yet hope to do by the moon, though she seem to afford us the only means practicable for the seaman. However before sailors can make use of the art of finding the longitude, it will be requisite that the coast of the whole ocean be first laid down truly, for which work this method by the satellites is most apposite: and it may be hoped that either the true geometric theory of the moon may be discovered, by the time the charts are completed; or else that some invention of shorter telescopes manageable on ship-board, may suffice to show the eclipses of the satellites at sea, at least those of the third satellite, which fall at a good distance from the body of Jupiter, being near three times as far from him as the first.

The last, but most considerable treatise of this collection, gives the aforesaid tables for computing the motions of Jupiter's satellites, but more especially those, for speedily finding the eclipses of the first or innermost. Wherein M. Cassini has employed his skill to make easy and obvious to all capacities the calculation of them, which is otherwise operose to the skilful, and not to be undertaken by the less knowing, who yet perhaps would be willing to find the longitude of the places they live in.

On Magnetism, particularly on the Polarity of a Piece of Iron. By Mr. J. C. N° 214, p. 257.

It is known that a rod of iron held perpendicular to the horizon, or inclining, the lower end is its north pole, or attracts the south end of a magnetic needle; and that the same end held upwards becomes a south pole, or attracts the north end of a needle, and repels the south end. The south I call a mutable pole, which may be north or south, according as you hold it. I call a fixed pole, that which does not change, however you hold it: particularly that is a fixed north pole, which, though held upwards, attracts the needle's south end, and repels the north end: and that is a fixed south pole, which, held downwards, attracts the needle's north end and repels the south end. It is known that the magnet makes such fixed poles. But to do it without the magnet, is what I here chiefly consider.

1. The species of the pole, whether north or south, may be found by passing the iron rod though cork or wood, and then leaving it to swim on water, it will turn to its proper pole. But this way is not nice, being in some cases so slow, that you would think it to be at rest when in motion towards its pole.

A better way to try, for instance, a north pole, is to hold the iron perpendicular to the horizon, and to try whether, being held under the north end of the needle, it attracts it. But a yet better way is to try whether the upper end of the rod attracts the south-end of the needle, for attraction is more sensible than expulsion.

2. A fixed north pole may be made with all the ways and rods that you can make a fixed south pole, but not on the contrary; for there are many cases wherein you can make a fixed north pole, but not a fixed south pole; and whatever way you get a fixed south pole, it is weaker than a fixed north pole made the same way. Applying a needle to an erect bar, beginning at the top, and so down, the needle turns not at the middle, but nearer. Of some rods you cannot make a fixed south primarily, yet you may consequentially: so you may make one end a north pole, and then the other end of those rods may, without more ado, become a fixed south pole; but this does not always hold, for the one may be a fixed north pole, the other may be a mutable pole.

3. Fire destroys all fixed poles, whether made by the magnet, or otherwise; but it increases, or rather less obstructs that magnetism which proceeds from the earth; a wire or rod of iron heated at one end, that end is a mutable pole, but more vigorous while hot than if cold: or the ignited end held downwards, will attract the said end of the needle more vigorous, than if cold; and so if held upwards, it more attracts the north end. The vigour of mutable poles is more in great than small rods, but it is otherwise in fixed poles.

4. Heat the end of a rod of iron red-hot, or heat all the rod, and cool that ignited end northward, it will be a fixed north pole; if cooled south, it becomes a fixed south pole. This Gilbert and others assert from experience. But this holds only in some cases, viz. if the rod is short, you cannot make a fixed pole that way. Take a round wire, its diameter $\frac{1}{5}$ inch, and length 10 inches, you cannot produce a fixed pole by ignition; but if this wire is longer, as suppose 30 inches long, or ever so much longer, it is capable of a fixed pole by ignition. Again, take a round rod 30 inches long, and 1 inch diameter; this rod is not capable of a fixed pole at that length, though the lesser was capable at that length. And thus my experiments give me reason to think that there is no rod or bar of iron ever so thick, but which, if it had length enough, would be capable of a fixed pole, by bare ignition, for of that only I speak in this paragraph; and there is no rod ever so short, but which if you make it sufficiently thin, is capable of a fixed pole. So, when in a rod I could not obtain a fixed pole at 21 inches length in that thickness, I could by making the rod thinner produce a fixed pole, even in the length of one inch and less, and the pole

should be of which kind I pleased. The terminus, or necessary length for every thickness, increases more than one would think.

5. Heat a rod, or its end, red-hot, and thoroughly cool this end downwards, it will have somewhat more magnetism than if cooled horizontally towards the north. But the better way is to cool it a little inclining towards the north. I cannot find that multiplicity of ignitions produces more magnetism than one good ignition: but it must be thoroughly ignited. Nor can I find by many experiments that quenching in water signifies to the producing or hindering magnetism; but many ignitions may accidentally promote it by purifying the iron.

6. Dr. Power says, that if we hold a rod northward, and hammer in that position the north-end, that will become a north pole, i. e. a fixed north pole; contrarily, if you hammer the south end. But this is true only in some cases, viz. it holds in rods only of a certain length: for I say here again, as before of ignitions, that of round bars of the same diameter, there is required a certain length, under which a fixed pole cannot be produced by hammering; but of any length more than that certain length, you may make it; and then if you take a bar shorter than that length of which you cannot make a fixed pole, while you keep that diameter; if you take a rod of the same length but less diameter, you may by blows produce a fixed pole: or if you only beat that thicker bar thinner, you may produce a fixed pole, though the rod be never so short, provided you beat it thin enough.

7. What is said of hammering, is to be understood of filing, grinding, drilling, sawing; or a hard rubbing, or even a soft rubbing, provided it is long, will produce fixed poles; the heavier the blows are, cæteris paribus, the magnetism is the stronger; I say, cæteris paribus, as when the blows are not so heavy in either case, as to flat the iron, for flatting it produces more magnetism, though other things do not vary. A few hard blows will produce as much magnetism as many, as to sense, as if you give never so many blows; yet a soft blow may produce but little magnetism. The utmost magnetism that I could produce in ordinary rods this way, did not exceed that which an ordinary loadstone would have infused.

8. Beating many rods northward, whose lengths I knew sufficient, I never failed of produced a fixed north pole; but hammering the same or like rods southward, I found that I could not produce a fixed south pole, only a mutable pole; nay, hammering one full south, I produced a fixed north pole; the reason I thought might be, that the hammered south-end on the anvil was a little lower than the end which I held in my hand. I then held the end higher, and

so hammering it south upwards, I never failed producing fixed south poles in proper rods.

9. Old drills and punches are fixed north poles, because almost constantly used downwards: but new drills are either mutable poles, or weak north poles; when I say a new drill, I do not mean one made on the spot, for that is probably a north pole, because quenched downwards in water; but then such polarity made by bare ignition is a weak pole, and soon decays, and turns to a mutable pole: but I mean a drill, which though never or little used, yet has been made some days or weeks; drill with this southward horizontally, and it is a chance if you produce a fixed south pole, but much less if you drill south downwards; but if you drill south upwards, you may make it a fixed south pole.

10. The stronger the polarity is, the longer it will last; a weak fixed pole may degenerate into a mutable pole in a day's time: yea, I have known it in a few minutes, while exposed to the air, and held in a position contrary to its pole: on the contrary, we find needles touched with good loadstones hold that virtue a great while, if kept from air, and in a meridional position.

11. The loadstone itself will not make a fixed pole of any iron, only it must have a proper length if it is thick; or if it is short, it must have a sufficient thinness: so, ordinary or weak loadstones cannot fix a pole in a thick short key, which yet they will do in a little key. So in a short thick iron tapering, a loadstone may fix a pole in the little end, when it cannot in the great end.

12. When ignition, hammering, or a loadstone cannot make fixed poles, it must not be thought that it can do absolutely nothing on such rods, for even then it may be found that there is an effect of magnetism in them discernible enough otherwise, though not enough to make fixed poles.

13. When you have the due length for making a fixed pole, you will find the making one a fixed north will consequently render the other a fixed south pole; but if, keeping the same diameter of this rod, you increase its length sufficiently, the making one end a fixed north pole, will not necessarily make the other a fixed south pole, but leave it a mutable pole. So if you by a like primary operation make the second end a fixed pole, the first end will lose its fixity, and become mutable. There is a certain length suited to every thickness of iron, to leave one end mutable, while the other is fixed, and the thicker the iron is, the greater is this length.

14. If you farther increase the length of the same rod, you will attain such length, that the oftener you have fixed a pole on one end, and then go to fix the other end, the fixity of the first will not be destroyed, and that end become mutable as before, but the fixity of the first end will remain, and so you

make both ends two fixed north poles, or two fixed south poles. The shortest length (for there is no terminus of the greatest length) for this is more in thick than in thin iron.

15. The aforesaid lengths are less, according to the strength of magnetism, viz. ignition requires a greater length than when a rod is actuated by a loadstone; and a rod touched with a strong loadstone requires less length than one touched with a weak one.

An Account of a Lamb suckled by a Wether Sheep for several Months after the Death of the Ewe. Communicated by Mr. Tho. Kirke, from Cookridge in Yorkshire. N° 214, p. 263.

The other day I dined with a neighbouring gentleman, Sir Wm. Lowther, where I met with something that seemed remarkable: he had this year a ewe that had two lambs, and she dying, left them young to shift for themselves; one of them was entertained by a wether sheep (aries castratus) among above a hundred other sheep: the lamb sucked the wether, and brought him to milk, and was maintained by him all this summer, till about a month ago that he was weaned. The wether was brought up to us, and we saw his udder, each side of which was about the size of a hen's egg, and he had two considerable teats. I saw milk spurted out of them, to a yard or two's distance, notwithstanding the lamb had been taken from him so long, &c. Sept. 28, 1694.

Yesterday I saw the wether again, his udder is much fallen, each side being now about the size of a walnut; there is milk still in it, enough to stream out above half a yard. There are no tokens at all of an hermaphrodite in him. I compared him with another wether who had teats or paps like him, and differed in nothing but the udder. The lamb it seems was about five weeks old, so it is likely might feed partly on grass, as I suppose other lambs of the like age do, notwithstanding what they suck from their dams, &c. Nov. 2, 1694.

An Account of Books, viz. 1. Reflections upon ancient and modern Learning. By W. Wotton, B.D. R.S.S. and Chaplain to the Right Hon. the Earl of Nottingham. London, 8vo. 1694. N° 214, p. 264.

The design of this book, as the author says himself in his preface, is to state the boundaries of ancient and modern learning, that so men may know which to recur to as their guides, if they would be masters of any particular part of knowledge; it being in his opinion a very pernicious thing, and very destructive to the increase of solid learning, to rest upon the ancients where the moderns have succeeded them: or to study second-hand books upon subjects which have been borrowed from the ancients; especially since he pretends to prove that in some things either of them have so far out-done the other, that it cannot be a matter of dispute among able judges, to which side one ought to give the preference. Hereupon, in order to make out this proposition, he divides his discourse into three general parts. 1. He inquires what sciences the ancients may have been supposed to bring to perfection, chiefly because they got the start by being born first. 2. Wherein the ancients have excelled the moderns, and why they may have been supposed to have done so. 3. Wherein the moderns have out-done the ancients. Under the first head he reckons ethics and politics; under the second oratory and poetry; all the other parts of learning have either been improved by the moderns, or the question cannot well be decided. (Chap. 1.)

He begins with ethics and politics. Here he supposes that the ancients might have been as well skilled as the moderns, since nothing but experience is requisite to understand those things, which the ancient Ægyptians, Greeks, and Romans could not miss of, who lived in formed societies for so many ages; accordingly he instances Aristotle's Ethics to Nicomachus, Xenophon's Cyrus, Theophrastus's Characters, Tully's Offices, and several other ancient books, as master-pieces in their respective kinds, of moral and political knowledge. Yet all this, according to our author, requires no particular strength of genius to complete it, since the Chinese and Peruvians seem to have done as great things towards the raising of wise and lasting governments, which must be the effects of extraordinary skill in this part of knowledge, as any of those nations which are so much commended for civil prudence. (Chap. 2.)

Next he goes to oratory and poetry. He supposes that Virgil, Homer, Horace, and Terence may have been better poets than any of the moderns in their several ways; and that Demosthenes and Cicero have not been equalled by modern orators. He thinks that the excellency of the Greek poetry might at first proceed from the manageableness of the Greek language, and afterwards from the great veneration which was paid to their poets, which made very many put in for the prize, of whom some few, one or two at least of a sort, arrived to so great an excellency, that others have despaired to equal them, since imitation in those things, not only nauseates but clogs men's parts. The constitution of their goverments, which were chiefly republican, obliged them likewise to study oratory, as a likely way to rise in their several states, for which reason, as among many rivals, some grew very excellent; so when their liberty was taken away by the Macedonians, their eloquence decayed along with it. The same reasons may in his opinion be assigned for the rise and decay of the Roman eloquence and poetry. Though in some sort of compositions, histories for example, where oratory has but a secondary share, he believes that the moderns may have equalled the ancients; and he thinks that the memoirs of Philip Comines, and F. Paul's History of the Council of Trent, may be set against any of the histories of the ancients with which it can be proper to compare them. (Chap. 3.)

This leads him to examine M. Perrault's Hypothesis, who asserts that modern eloquence, and modern poetry are preferable to the ancient; there he goes through the several reasons which M. Perrault brings to establish his hypothesis, and concludes that they are insufficient; particularly he seems to think it unfair, that the translations of the pieces of eloquence of the ancients should be set against original pieces of the moderns, since every language has beauties of its own, which can never be reached in another, though ever so exact and elegant. (Chap. 4.)

In the fifth chapter he considers ancient and modern grammar, as it comes under the cognizance of critics, or of philosophers. For the first, which he calls Mechanical Grammar, he supposes that some moderns have understood the analogy of the Greek and Latin as well as any of the ancients; and he thinks that modern tongues have been as critically scanned as any of the ancient ones, especially English and French, which he particularly instances. For Philosophical Grammar, he recommends Bishop Wilkins's Essay towards a real Character and Philosophical Language, and the third book of Mr. Locke's Essay on the Human Understanding, as original pieces that antiquity has nothing to set against. (Chap. 5.)

When he comes to compare ancient and modern architecture, statuary, and painting, he abridges what M. Perrault had said already upon the same subjects in his Parallel of the Ancients and Moderns, wherein he gives the moderns every where the preference, without interposing his own judgment. (Chap. 6.)

After this he comes to inquire into ancient and modern philosophy and mathematics; but before he speaks particularly of them, he examines Sir W. T.'s Hypothesis of the History of Learning step by step, against whose Essay upon Ancient and Modern Learning, a great part of his book seems to be levelled. Sir Wm. Temple had exceedingly commended the learning of Pythagoras and the ancient sages of Greece, as also that of the old Ægyptians, Chaldeans, Arabs, Indians, and Chinese. Our author thinks that Pythagoras's chief excellency lay in political knowledge: and that though he was a better mathematician and philosopher than any man of that time, yet since he is commended chiefly for finding the 47th Proposition of the First Book of Euclid, his skill in those matters was, comparatively speaking, but very indifferent, which he also affirms of the ancient sages. (Chap. 7, 8.)

From them he goes to the Egyptians, of whom he observes that more may be challenged from them, than from other nations, because their pretences to exactness in recording inventions and traditions have been more considerable. Yet he thinks that their history could not be extraordinary, since both the time when the pyramids were built, and that when their great hero Sesostris lived, could never be determined by the most ancient writers now extant: that all the great ancient inventions in geometry, though its original be owing to them, are conveyed to us by Greeks as their own inventions: that their medicine was wholly built upon astrological or magical grounds: that their pretences to the philosopher's stone seem to have been fathered upon them by later alchemists: that their skill in anatomy was so small, that they believed that the heart increased annually two drachms in weight till men were 50 years old, and afterwards decreased as gradually; for which reason, according to them, no man could live above 100 years: that with all their boasted curiosity, they seem never to have sailed 200 miles on the Nile into Æthiopia; since till about Plato's time, they could not give a clear solution of the annual inundations of that wonderful river: and in short, that their greatest skill lay in making wise and prudent laws, which were worth going so far as the Greeks went to fetch them. (Chap. 9, 10.)

He thinks that the Chaldean learning was not so excellent as the Ægyptian; that the Assyrian history, which we have from the Chaldeans, contradicts the Jewish: that the Chaldean astrology was downright knavery: and that for other things, had they been very considerable, there would have been more memorials of them preserved. The Arabian learning is, according to him, all in a manner owing to the Greeks, so that its antiquity or extent cannot here be alledged. (Chap. 11.)

He believes the Chinese natural knowledge to be very inconsiderable, and their speculative skill in medicine entirely fantastical: to prove which, he produces a long citation out of an old Chinese book, called Nuy-Kim, printed by Cleyer in his specimens of Chinese Physic. (Chap. 12.)

He divides the Grecian learning into four parts; Logic, Metaphysics, Mathematics, and Physics. Logic, as it is the art of disputation and method, is, in his opinion, to be ascribed to the ancients; as it is the art of invention, it is more owing to the moderns, since the methods of invention which the ancients made use of seem to be entirely lost. Here he commends Descartes's Meditations, Tschirnhaus's Medicina Mentis, and Mr. Locke's Essay on the Human Understanding. In Metaphysics he thinks the writings of Descartes and his followers may be set against all that the Platonists say upon those subjects, though they, of all the ancients, discourse the most upon spirits and incorporeal substances. (Chap. 13.)

When he speaks of ancient and modern mathematics, he produces a discourse of that excellent geometer, Mr. John Craig, who endeavours to prove that modern geometry is of infinitely larger extent than the ancient: and that it has been enlarged by methods in a good measure unknown to, or at least, not comparatively cultivated by, the ancients, which are, algebra and the method of indivisibles, the particular advantages of the former of which in improving arithmetic and geometry he at large insists upon. (Chap. 14.)

Afterwards when he comes to physics, our author observes that there are several instruments and arts, which are necessary tools to a good philosopher, that have been either invented, or very much improved by moderns, for want of which it was impossible for the ancients to understand nature so well as it has since been understood. Among instruments wholly modern he reckons, 1. Printing, which is useful to all learned men alike. 2. Engraving upon wood and copper, which is peculiarly useful to all writers of natural history and mathematics. 3. Telescopes, first invented by Zacharius Joannides, a spectacle maker of Middleburgh, about the year 1590, whereby the heavens have become more accessible to modern astronomers than they were to the ancients. 4. Microscopes, the invention of the same Joannides, of infinite use in discovering the texture of minute bodies. 5. Baroscopes, by which the comparative gravitation of air upon terrestrial bodies may be found out. 6. Thermometers, to adjust the variations of heat and cold. 7. Air-pumps, very useful in discovering many hidden properties of the air. 8. Pendulum clocks, necessary for astronomers in measuring small subdivisions of time, when they make their observations. (Chap. 15.)

Amongst preliminary arts he reckons chemistry and anatomy; by chemistry he understands the art of separating bodies by fire; and he observes that though the ancients could refine metals from their dross to a good degree, yet for want of aqua-fortis they could not part them from one another so well as can be done at present. He says, chemistry, properly so called, is mostly owing to the Arabs, and that the Greeks knew scarcely any thing at all of it; but yet that the use of chemical preparations in physic is almost entirely owing to the physicians of this and the last age, since the time of Paracelsus. (Chap. 16.)

Of anatomy he says, that the skill of the ancients in it reached only to those parts that are discoverable by the naked eye, and even there not so far as the moderns have carried it in any one particular: that the extent of the ancients' knowledge in that matter may be certainly known from the anatomical discourses of Galen; that if we descend to particulars, the anatomy of the brain was not known to any tolerable degree before Malpighius and Willis: that the ancients knew little of the texture of the eye, in comparison of what may be found in the writings of Dr. Briggs; that the glands which supply it with moisture are by nobody so well described as by Monsieur Nuck; that the ear, with its inner cavities, was little known before Monsieur Du Verney; that the knowledge of the texture of the tongue is owing to Malpighius; of the glands of the mouth, jaws, and neck, to Wharton, Steno, and Nuck; and of the lungs to Malpighius; that the primary use of the lungs was wholly unknown to the ancients, who had no notion of the circulation of the blood; since all that can be collected from the writings of Hippocrates, Plato, and Aristotle, is, that the blood had a constant recurrent motion through the body, which they could not distinctly describe; that this motion through the veins to the right ventricle of the heart, thence through the lungs into the left, and so through the arteries over the whole body till it meets again with the veins, first called by Cæsalpinus the circulation of the blood, was first discovered by Servetus, than pursued somewhat further by Columbus and Cæsalpinus, and at last made perfectly intelligible by Dr. Harvey, whose discoveries were lately made complete by Mons. Leuwenhoeck: that the texture of the heart was first discovered by Dr. Lower, of the coats of the stomach by Dr. Willis, of the fibres of the intestines by Dr. Cole, of the chyliferous vessels by Asellius and Pecquet, of the lacteal vessels in women by Mons. Nuck, and of the liver by Malpighius; that the ductus pancreaticus was first discovered by Wirsungus, and the spleen and the reins found to be glandulous bodies by Malpighius; that the lympha and its ducts were never thought of till they were discovered by Bartholin, Rudbeck, and Jolliffe; that Dr. Havers found out the mucilage in the joints, with the glands which secrete it; and lastly, that the anatomy of brutes has been as carefully examined as that of men, of which he gives several instances. (Chap. 17, 18, 19.)

In treating of philosophy, he begins with natural history, and with that of elementary bodies and minerals as the simplest. He observes that the ancients knew little of the nature of air, that though Aristotle believed it to gravitate, yet his disciples so little understood him, that they asserted the quite contrary; that the doctrine of its spring is owing to our truly noble countryman Mr. Boyle, whose histories of the most conspicuous qualities of terrestrial bodies have much enlarged that part of natural history. In his account of minerals he instances in the loadstone, whose noblest properties were anciently unknown: and he enlarges upon their use, with the increase of wealth and knowledge, which thereby have accrued to these European nations, in these two last ages. Chap. 20.)

Of the natural history of plants he observes, that all ancient descriptions are confused and lame, and in number very deficient; that nothing which they did can be compared with Gerhard's, Parkinson's, and Bauhine's Herbals, much less with Mr. Ray's, who first drew up a methodical history of all the plants yet known. (Chap. 21.)

Of insects he observes, that the ancients only meddled with the most remarkable sorts, and there rarely took notice of any but the most conspicuous things; so that all which Malpighius and Redi say concerning their generation, all that is in the writings of Goedartius and Swammerdam concerning the time and nature of their transmutations, may be looked upon as wholly new. And as for histories of larger animals, he pretends that Willoughby's Histories of Birds and Fishes, Ray's Synopsis of Quadrupeds, besides a great many modern discourses upon particular animals, are without comparison better than the histories of Aristotle, Ælian, or Pliny. (Chap. 22.)

Afterwards he inserts a discourse written by that most excellent astronomer Mr. Halley, concerning ancient and modern astronomy and optics; who says that the Egyptian and Chaldean astronomy was little worth in itself, and the Greek astronomy not much better, if compared with the modern; that Ptolemy's Hypothesis of the Planetary Motions cannot be set against Kepler's and Newton's; nor Hipparchus's Catalogue of the Fixed Stars against Tycho Brahe's and Hevelius's; that the ancients could know but little of optics, since they were so meanly skilled in prespective; and of dioptrics they were wholly ignorant, since they had no notion of the properties of refraction, which Descartes first reduced to a science. (Chap. 23.)

Of music he determines nothing positively, but seems to think that since the grounds of music have always been the same, and that the moderns use more gradations of half-notes and quarter-notes than the ancients; and that the symphonies of the ancients were only consorts of several voices and instruments to the same part; that modern music, considered as an art, is more perfect than the ancient, which was so much extolled by those that heard it, because it was the most excellent they had ever heard, and so had a right to the greatest commendations which they could give it. (Chap. 24.)

Of medicines also he determines nothing, as to ancient and modern methods of practice, only allows Hippocrates to have been a very great genius, perhaps not equalled by any physician that has come since; yet considering how much botany, chemistry, and anatomy, have been enlarged, he thinks that modern theories of diseases are much more valuable than ancient ones, for want of those helps, could possibly be. (Chap. 25.)

In speaking of ancient and modern methods of philosophising; he gives the preference to the latter; because no principles of nature are there allowed, but what are in themselves intelligible; and the business of forming parties is now in a manner out of doors, and mathematical reasonings are constantly urged as valid ways of proof, in all physical inquiries where they can be brought in; hereupon he takes occasion to enlarge upon the design of the Royal Society, and hints at the great things which its members have effected towards the completing of physical knowledge. (Chap. 26.)

Having gone through those science which lie equally open to men of all ages to make discoveries in, he goes on to those which have their whole foundation in antiquity, wherein by consequence the moderns seem to have no pretence to a comparison: such as philology and divinity. Yet he says, that if we consider how much printing has altered the state of learning, how wide a thing antiquity is in all its compass, how many things rarely meeting in one man, are requisite to make a man a thorough critic; we ought to conclude, that though every age of antiquity knew itself better than we can pretend to know it, yet in the gross, the Scaligers, the Vossiuses, the Ushers, the Seldens, the Bocharts, and such modern philologers, have had a greater extent of ancient learning than any single man among the ancients could possibly have. (Chap. 27.)

And as to divinity he says, that though the scriptures and the writings of the ancient fathers be the foundations upon which every divine ought to build, yet if we consider that the text of the Bible itself by a familiar conversation with the Oriental languages is now better understood: that casuistical knowledge lie as obvious now as ever it did: that the art of preaching is more methodical, and by consequence more instructive; that controversies have been managed more nicely and more exactly, we must allow modern divines to have been the better workmen, though the matter which both they and the ancients have jointly wrought upon has been the same. (Chap. 28.)

His last chapter is employed about answering some reasons which Sir W. T. assigns for the decay of learning: such as, 1. Disputes in religion: which in his opinion have rather increased knowledge than otherwise, since the spirit of opposition, when once raised, will show itself in every thing as well as in those matters which first gave it life. 2. Want of favour from great men, of which, according to him, there is not so much reason to complain, considering that the French king, Q. Christina, and several others whom he there mentions, lived all of them in this age. 3. Pedantry, which also he pretends to be in a manner quite banished out of the world.

The whole book seems to be designed for a vindication of that sort of learning, which it is the intention of the Royal Society to promote, for which reason probably he took no notice that he had the honour to be a member of that body.

2. *Horti Malabarici Pars Decima, Undecima, Duodecima, et Ultima. With some Remarks upon them, by T. R. M. D. S. R. S.* N° 214, p. 276.

Nine volumes of this great work being already mentioned in these Transactions, N° 145, N° 198, N° 200, we will here finish the account of the remaining volumes. The 10th part contains 94 plants, with their descriptions, figures, and uses, all natives of the kingdom of Malabar, collected and designed during the memorable goverment of that excellent person, the Heer Van Rheed.

This 11th part comprehends 65 plants, with their stately icons drawn from the life. The first whereof is the kapa-tsiakka, or ananas, called by our American planters, the pine-apple: it was first brought into the East from the West Indies, and grows larger here than in its own native soil. Its delicacy and use are well known.

Kurka, called in Ceylon jusula, a sort of glans terrestris, and eaten in the same manner. It grows plentifully in sandy places.

Kodda-pail, or sedum Indicum palustre, foliis latissimis, crispis, floribus albicantibus pilosis, floats on the water like the stratiotes or aloe palustris, used much in bloody fluxes, coughs, and lumbagoes.

Besides these, many more might be enumerated out of this eleventh volume, of daily use in the Indian pharmacopœia, as some aloes, great varieties of the arisarum, the dracunculus or arum polyphyllum, many nymphæas, gladioli palustres, tribulus aquaticus, several sorts of proud lilies, narcissi, arrow-heads, bastard species of passion flowers, abundance of curious bindweeds, &c. which make up this tome.

This volume concludes the whole work, the noble author dying on ship-board the last year, before Surat, where the Dutch East India Company have ordered a most magnificent monument to be erected for this great benefactor and ornament of their republic. This last volume contains 79 icons, with descriptions and a general index to the whole work. It begins with several beautiful orobanches or abortive orchises growing like missletoe upon trees, used by the Indians in convulsive and feverish cases.

The woods in these climates must afford pleasant prospects, the trees being loaded with variety of herbs, either climbling to the tops of them or shooting out of their trunks and branches, of which this volume gives many species, called properly epidendra, and anadendra.

Here ends the admirable product of the Heer Van Rheed, whose perforances in the vegetable history may perhaps raise up some successor to carry on the like in the animal and fossil, which seem to lie uncultivated, and reserved for the a second Rheed. Besides these another sort of history would become the greatest men that reside in either India, I mean that of the arts and mechanics, practised by the natives or by strangers of equal advantage, to mankind with the natural, both depending on each other.

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C. Baldwin, Printer,
New Bridge-street, London.

ances in the vegetable history may perhaps raise up some successor to [illegible] the like in the animal and fossil, which seem to lie unmeddled, and reserved for the a second Rhedi. Besides the [illegible] sort of history would become the greatest men that reside in either India, I mean that of the arts and manufactures practised by the natives or by strangers of equal advantages, to mankind with the natural, both depending on each other.

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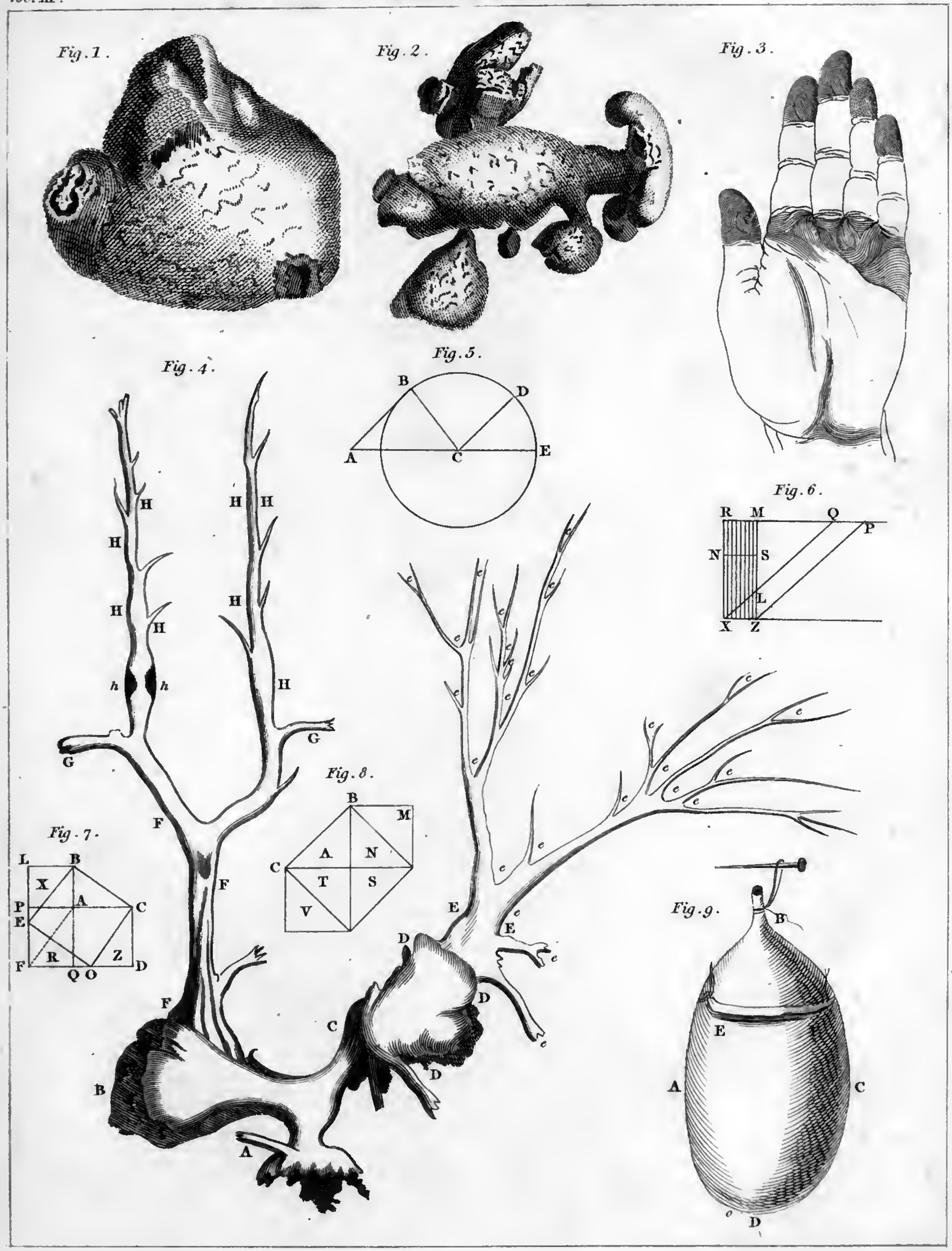

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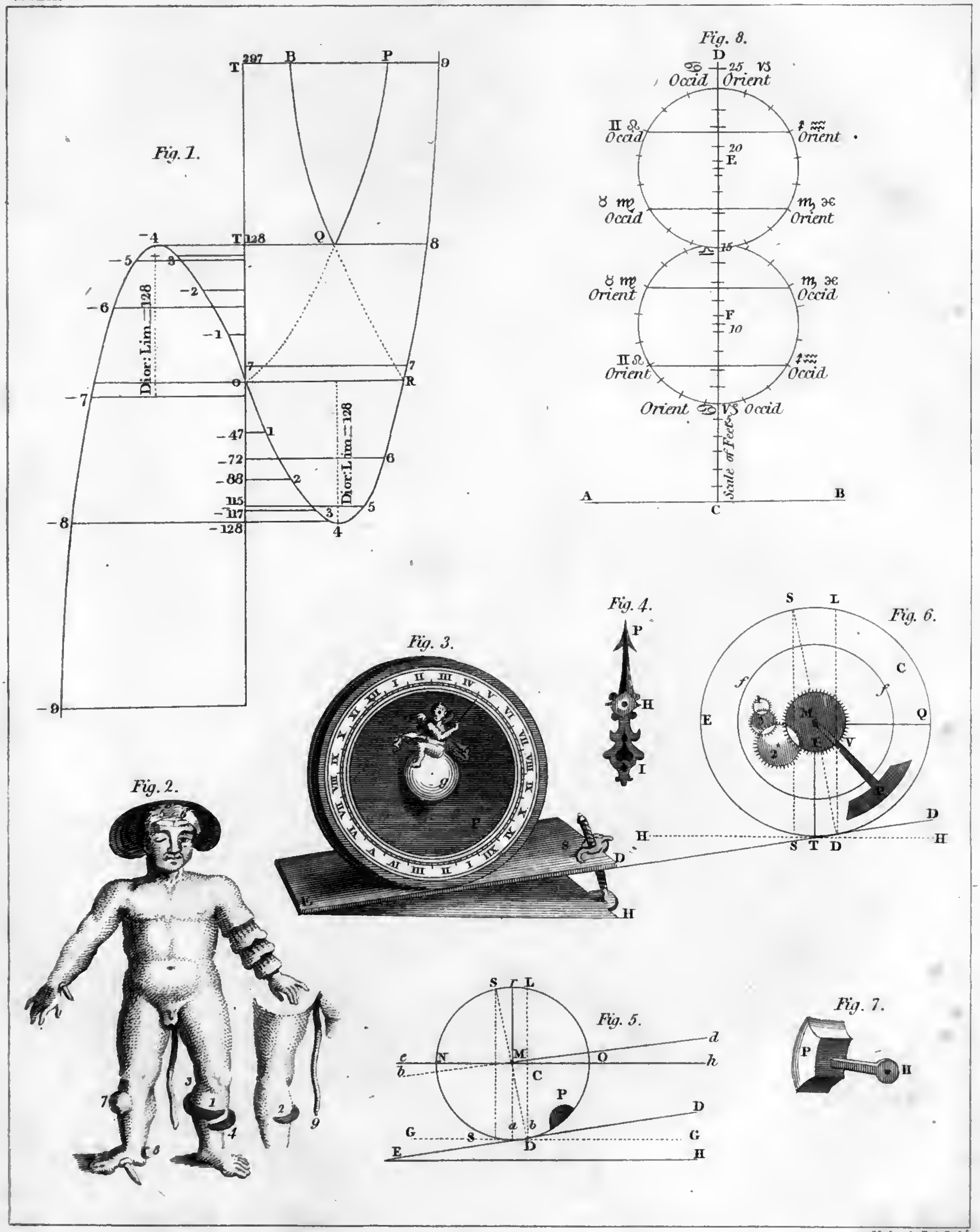

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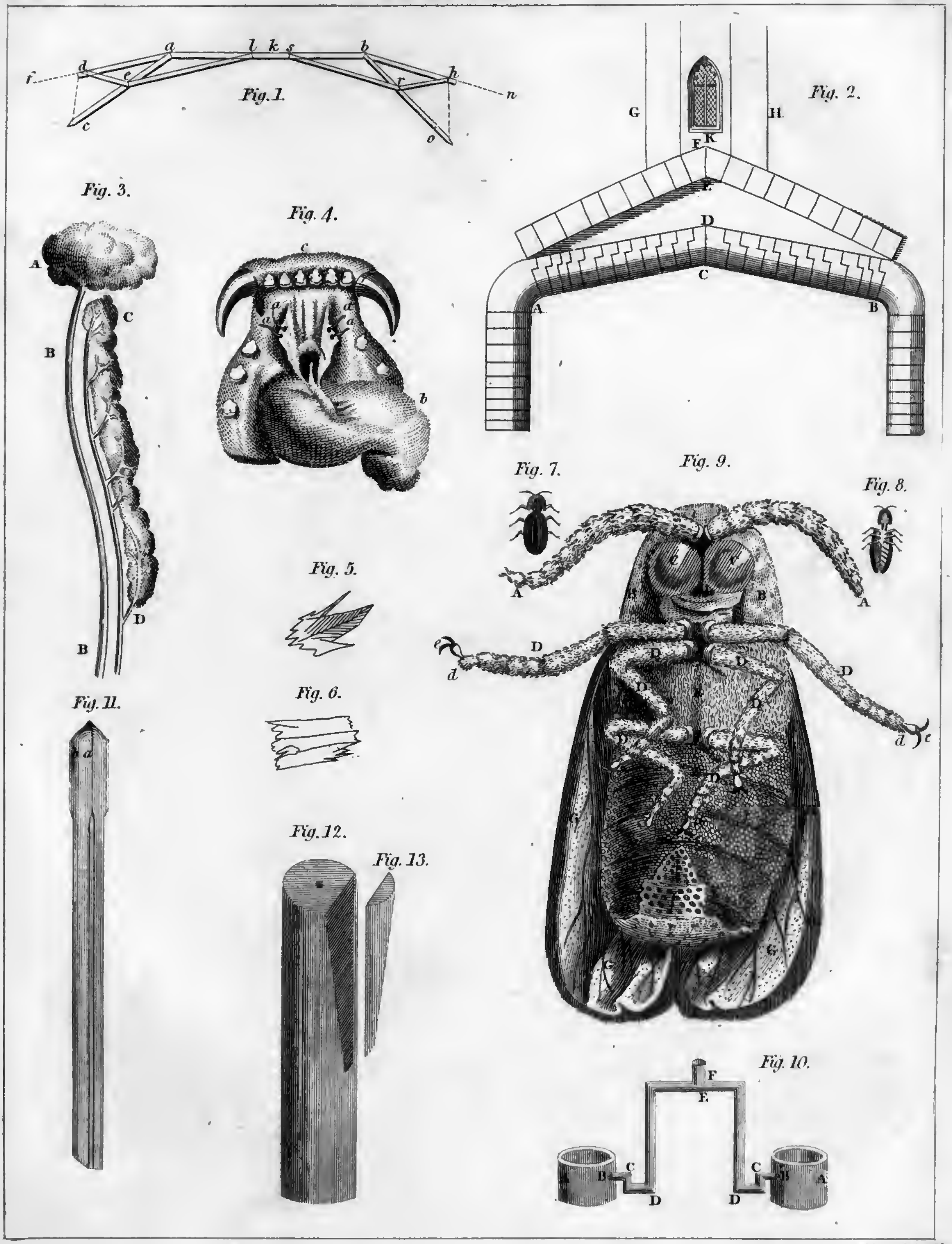

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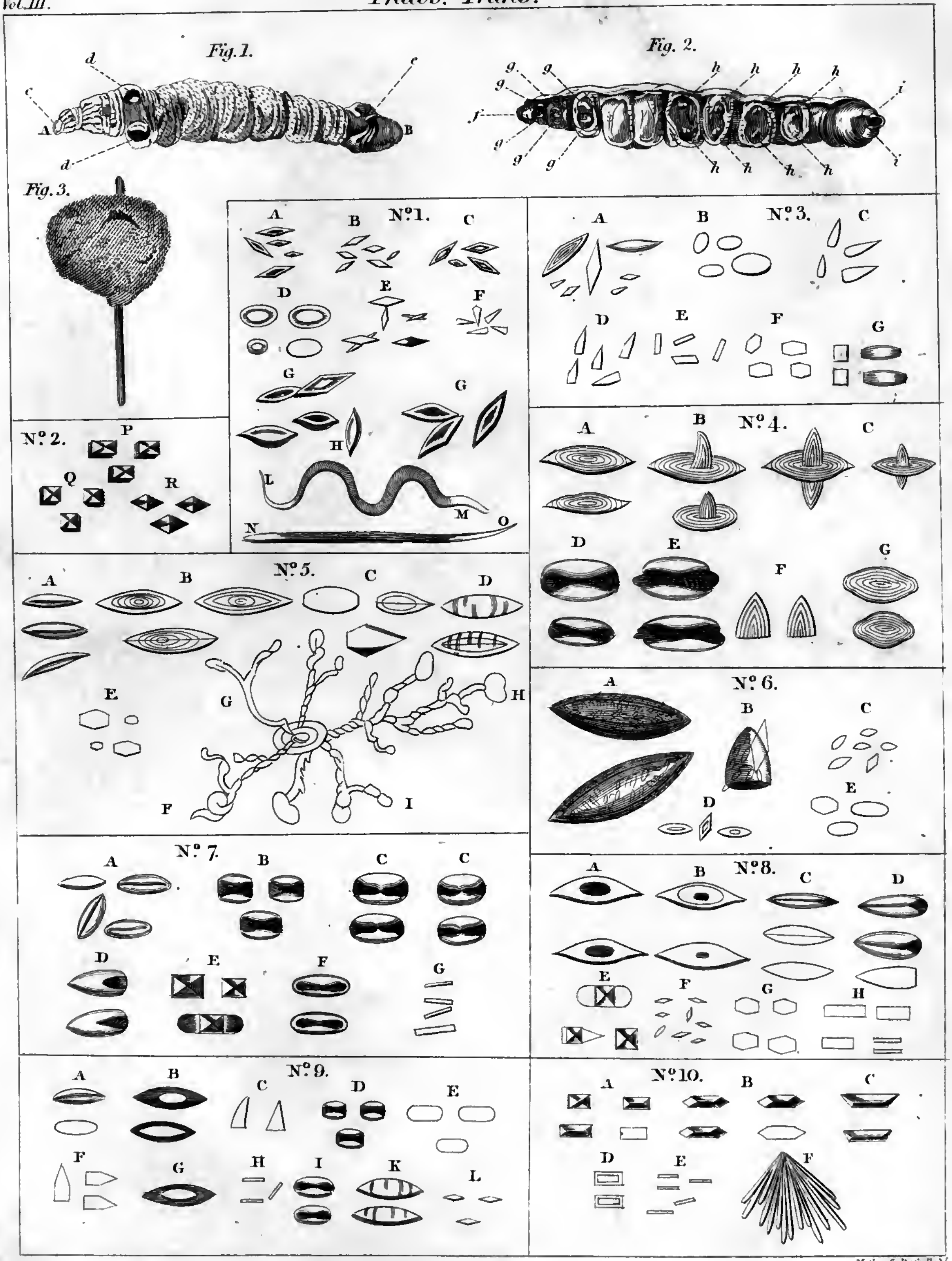

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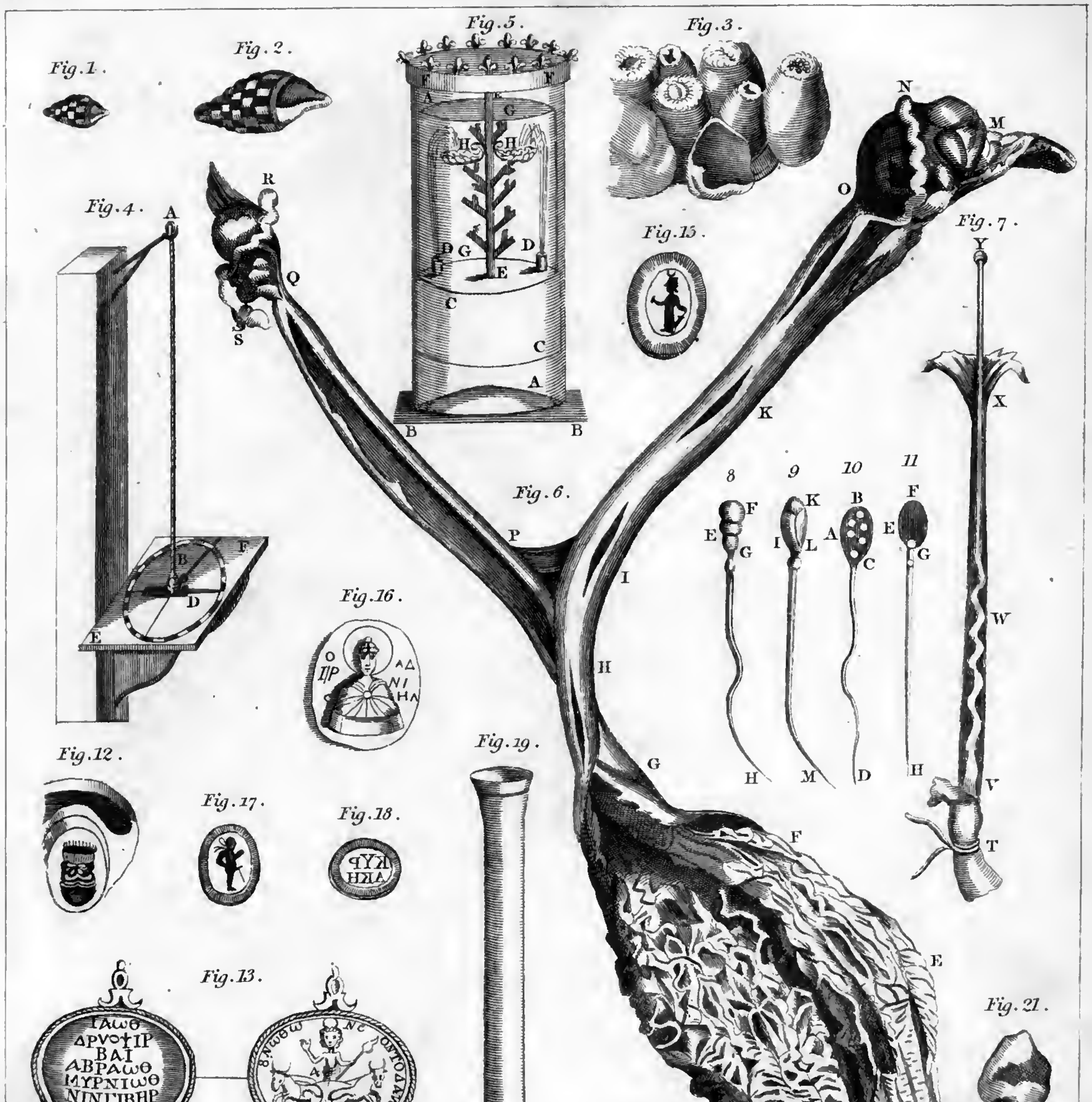

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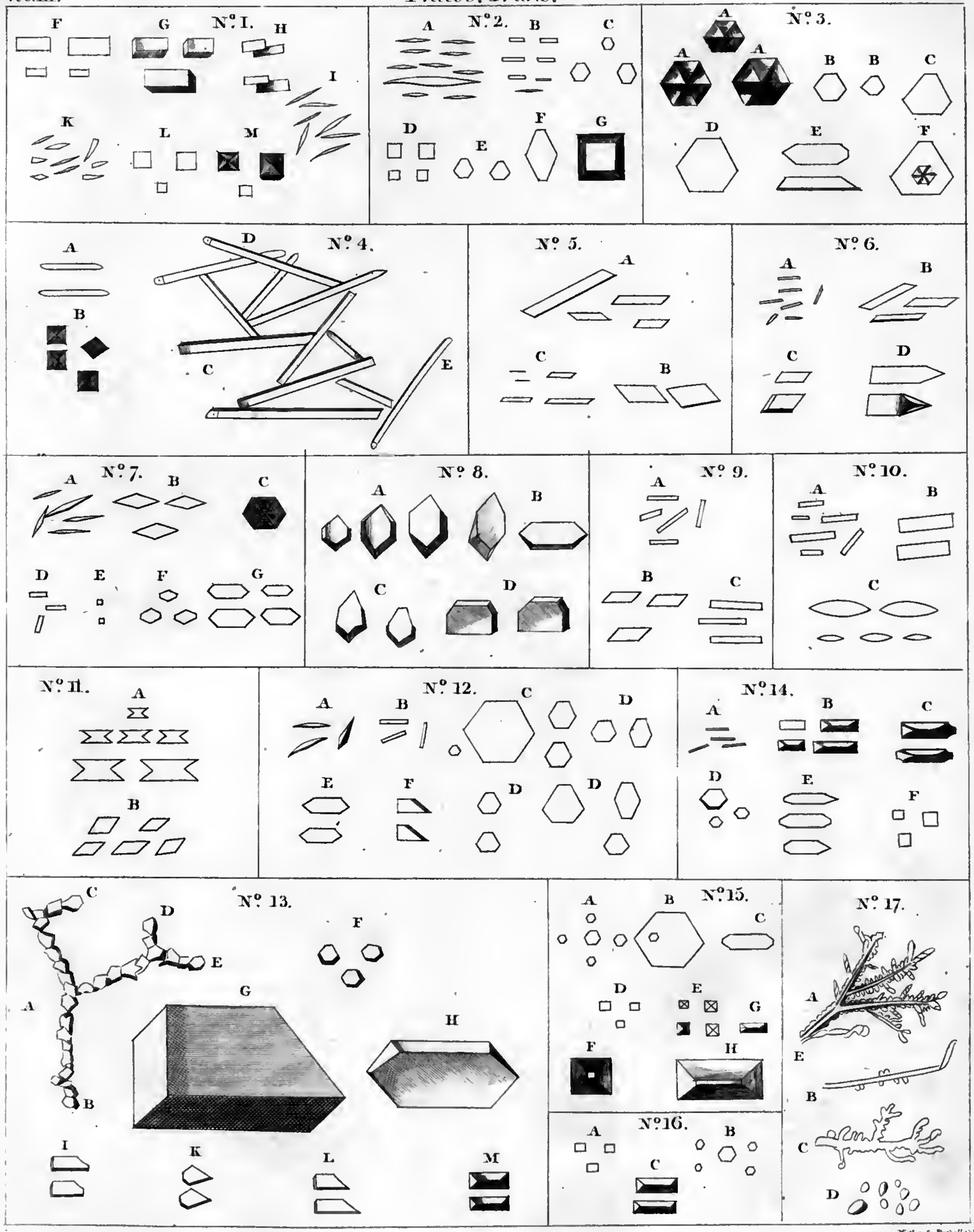

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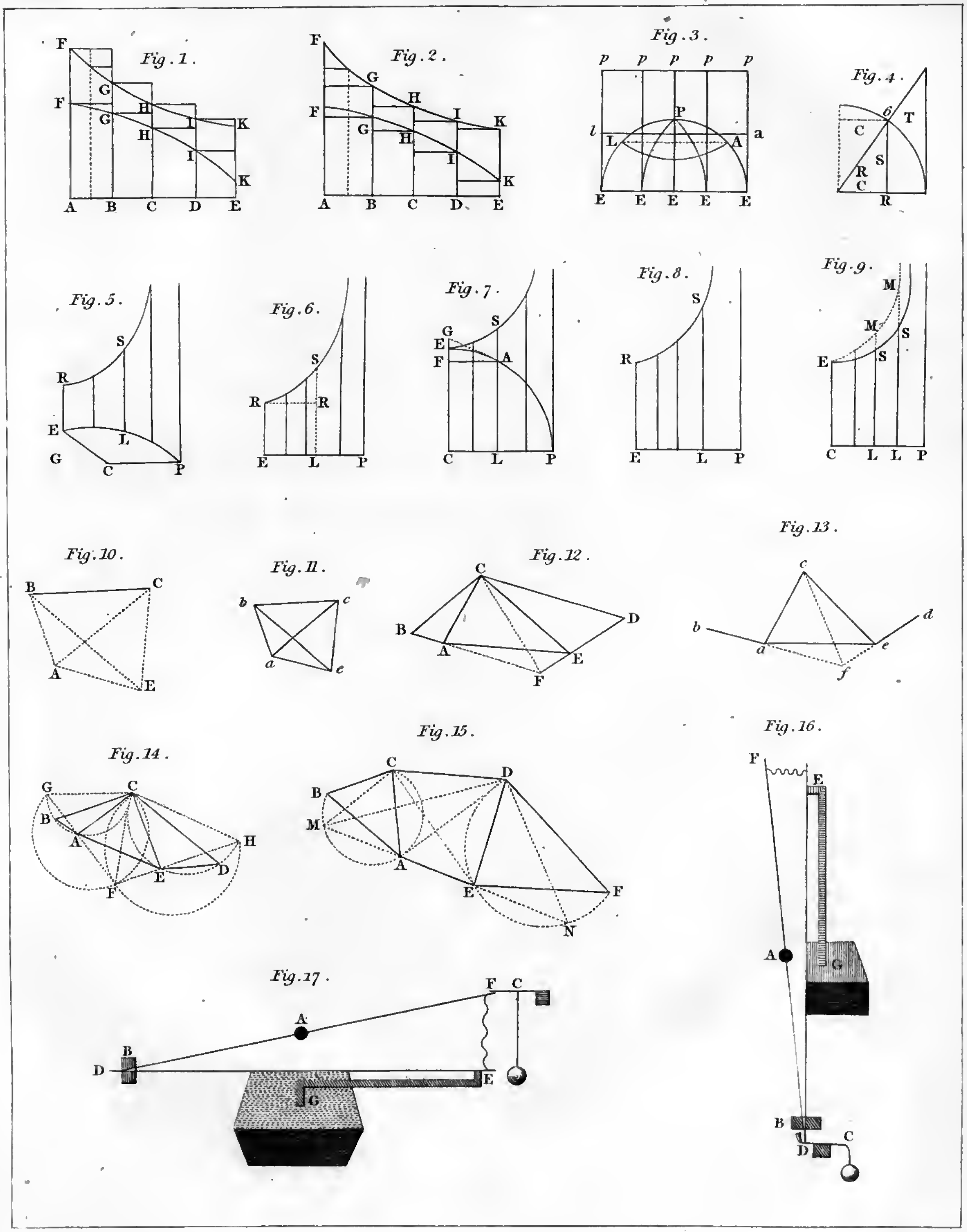

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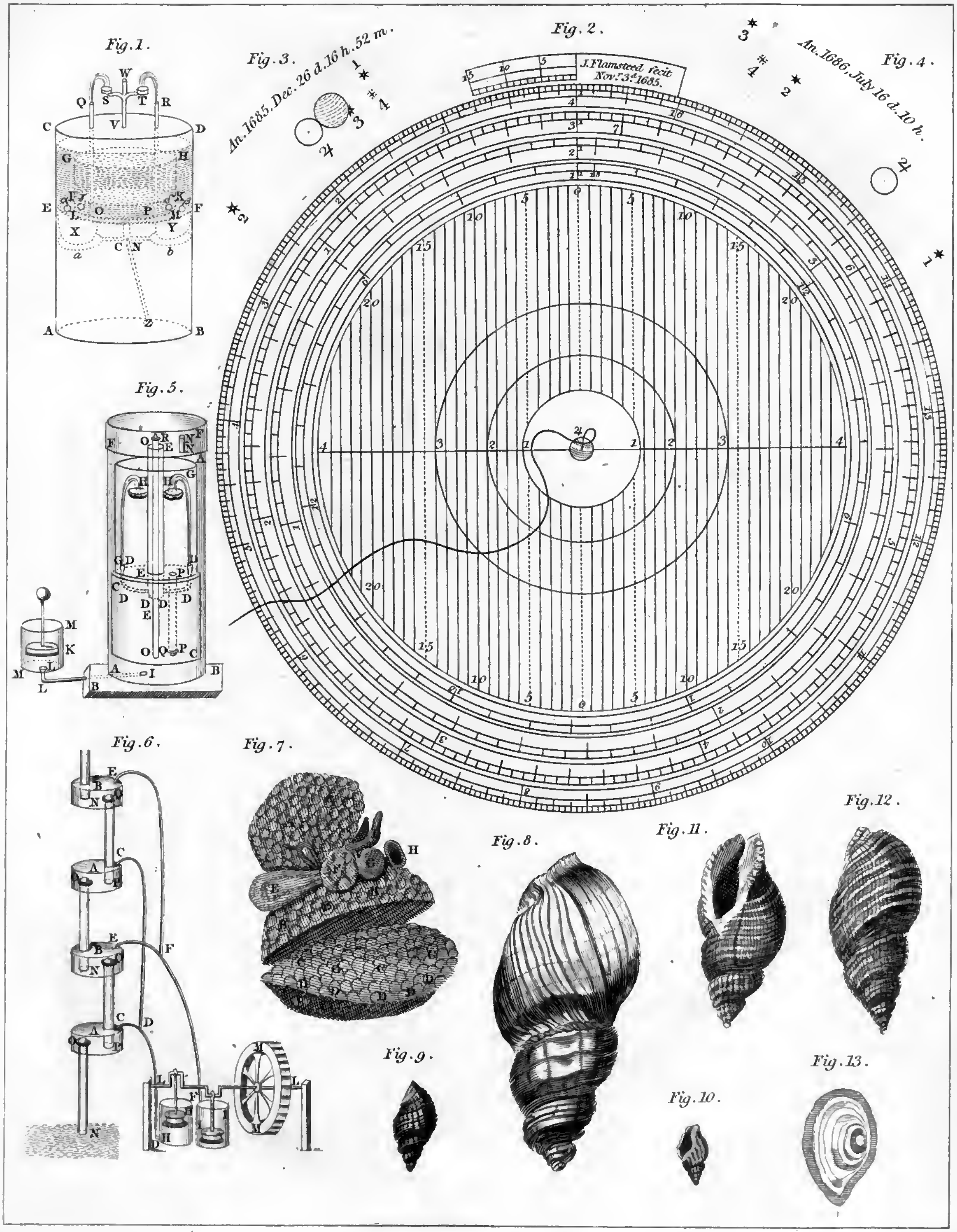

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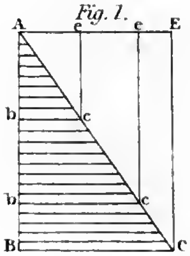
Fig. 1.



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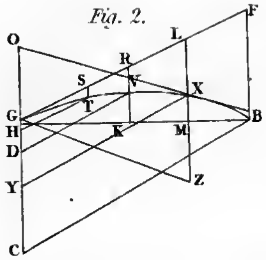

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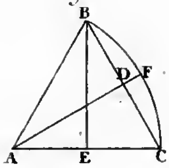

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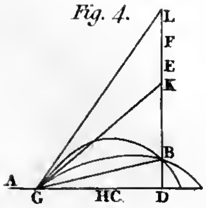

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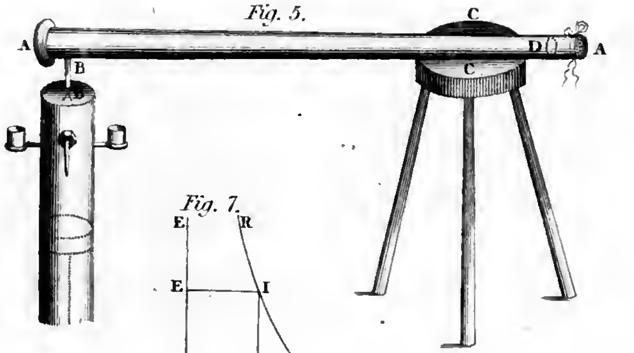

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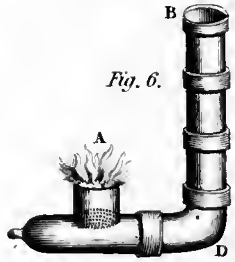

Fig. 12.

Fig. 7.

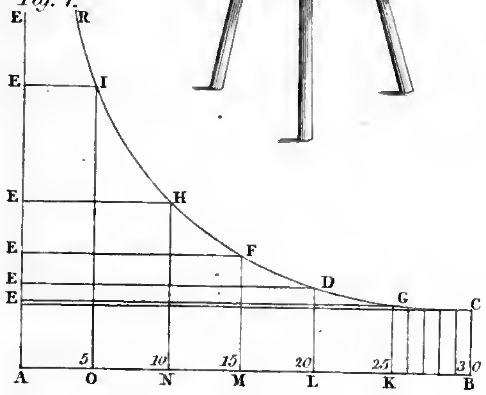

Fig. 9.

The Table of Vowels.

1	\	a
2	/	a
3	–	a
4	(	e
5	)	eu
6	|	i
7	◡	u
8	⌒	ui
9	y	y
10	o	o
11	v	u
12	<	eu
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14	>	ou

Fig. 8.

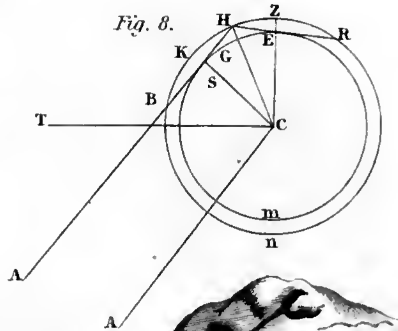

Fig. 11.

Fig. 10.

The Table of Consonants.

	1		2		3		4		5		6		7	
1	7	b	9	d	η	j	y	y	7	=	9	=	p	l
2	h	p	h	t	h	·ch	h	k	h	=	h	=		
3	m	m	m	n	m	g̅n	m	ng	m	=	m	≤		
4	b	=	b	dh	b	j	b	g	b	v	b	z		
5	b	=	b	th	b	sh	b	ch	b	f	b	s	ℓ	th
6			h	n̄										

8		9		10		11		12	
η	h	y	y	η	r	y	n	/	·x

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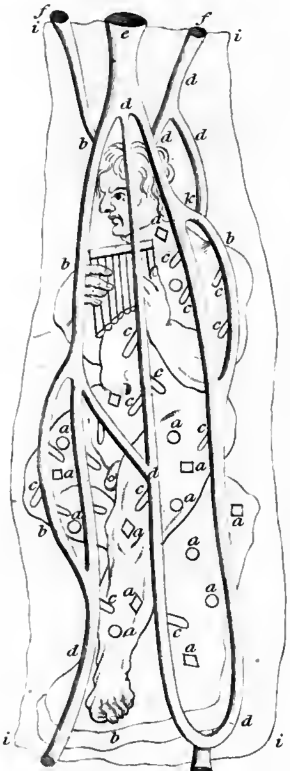
Fig. 1.



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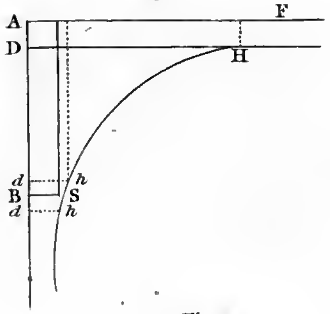

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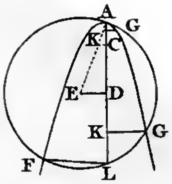

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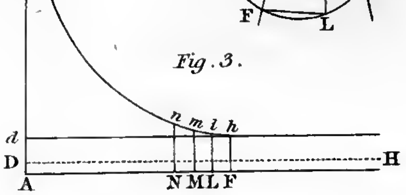

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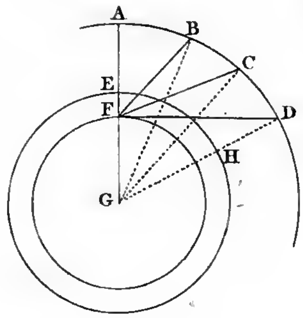

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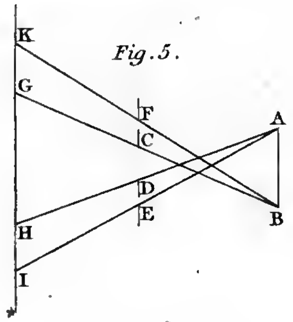

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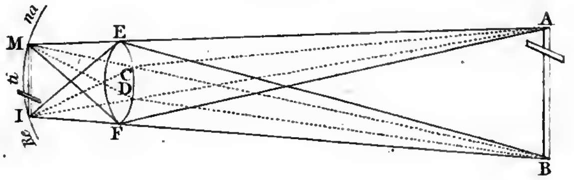

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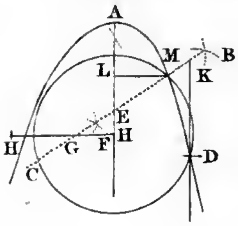

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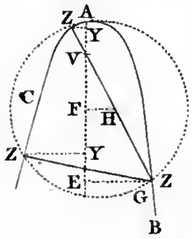

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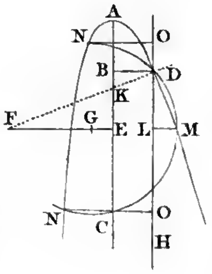

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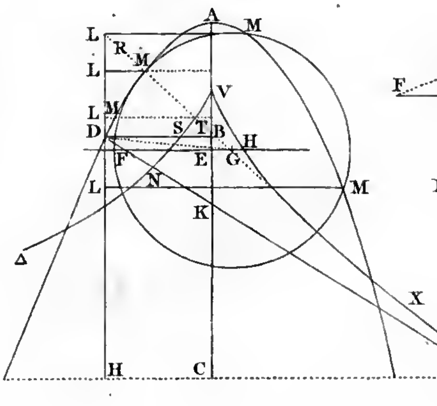

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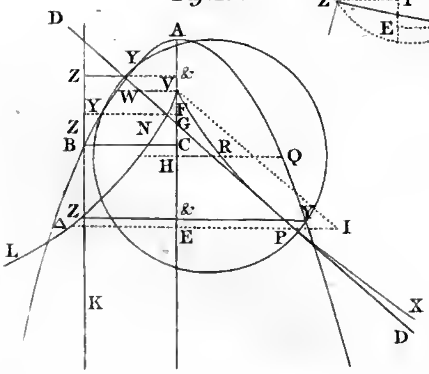

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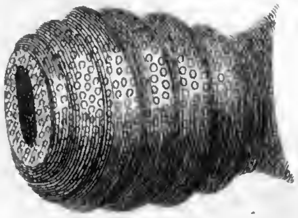
Fig. 1.

Fig. 2.

Fig. 3.



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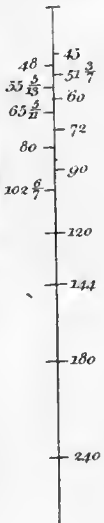

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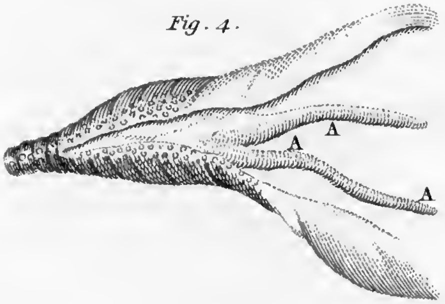


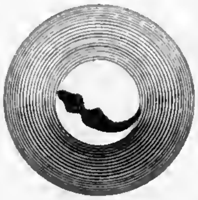
Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.



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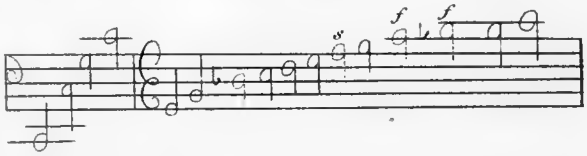
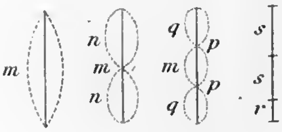


Fig. 10.

Fig. 11.

13 14 15 16



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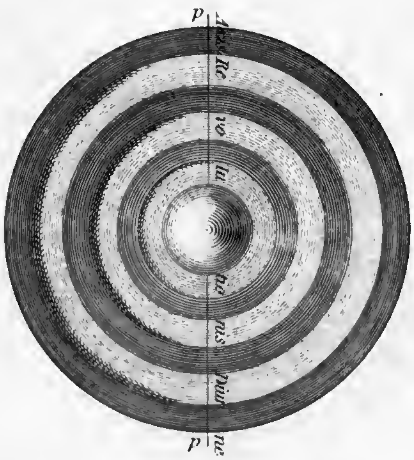

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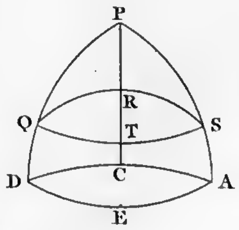

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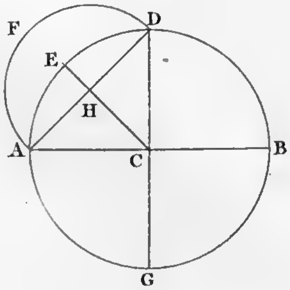

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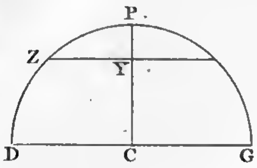

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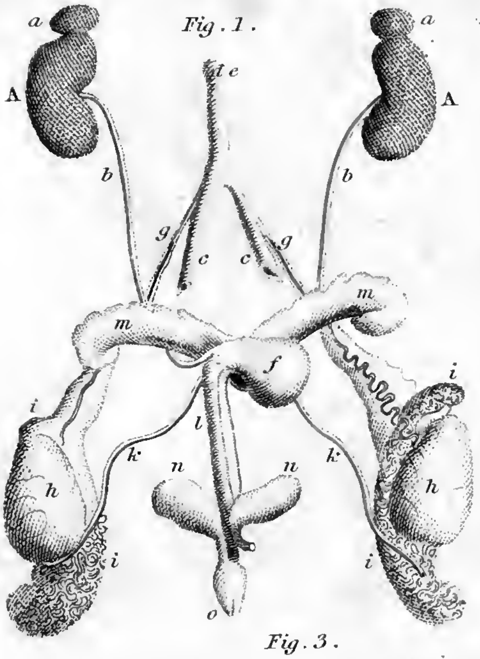
Fig. 1.



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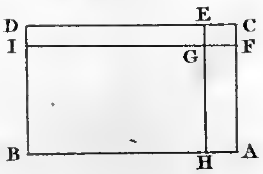

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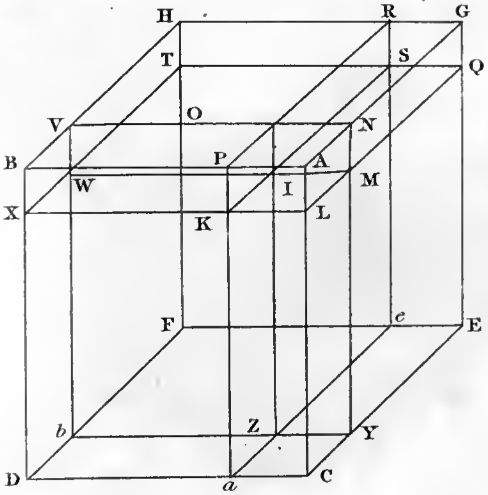

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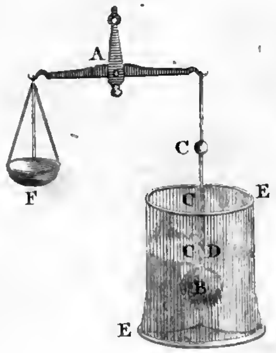

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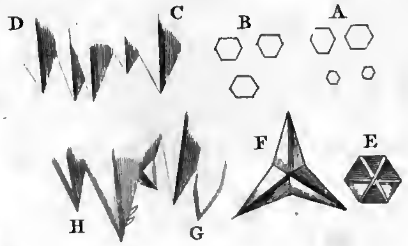

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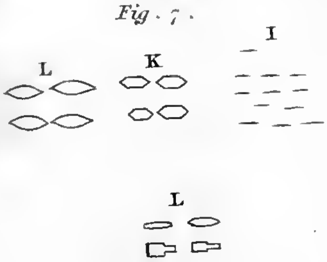

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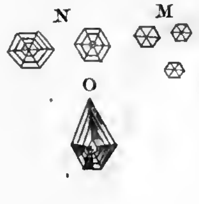

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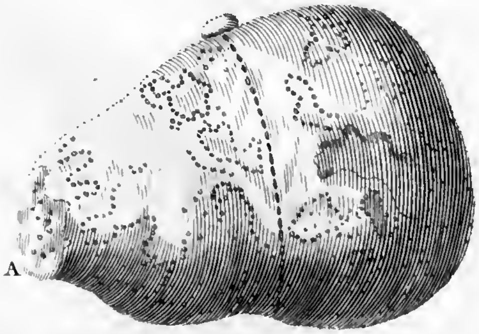

Fig. 10.

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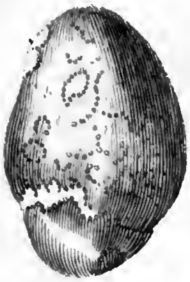

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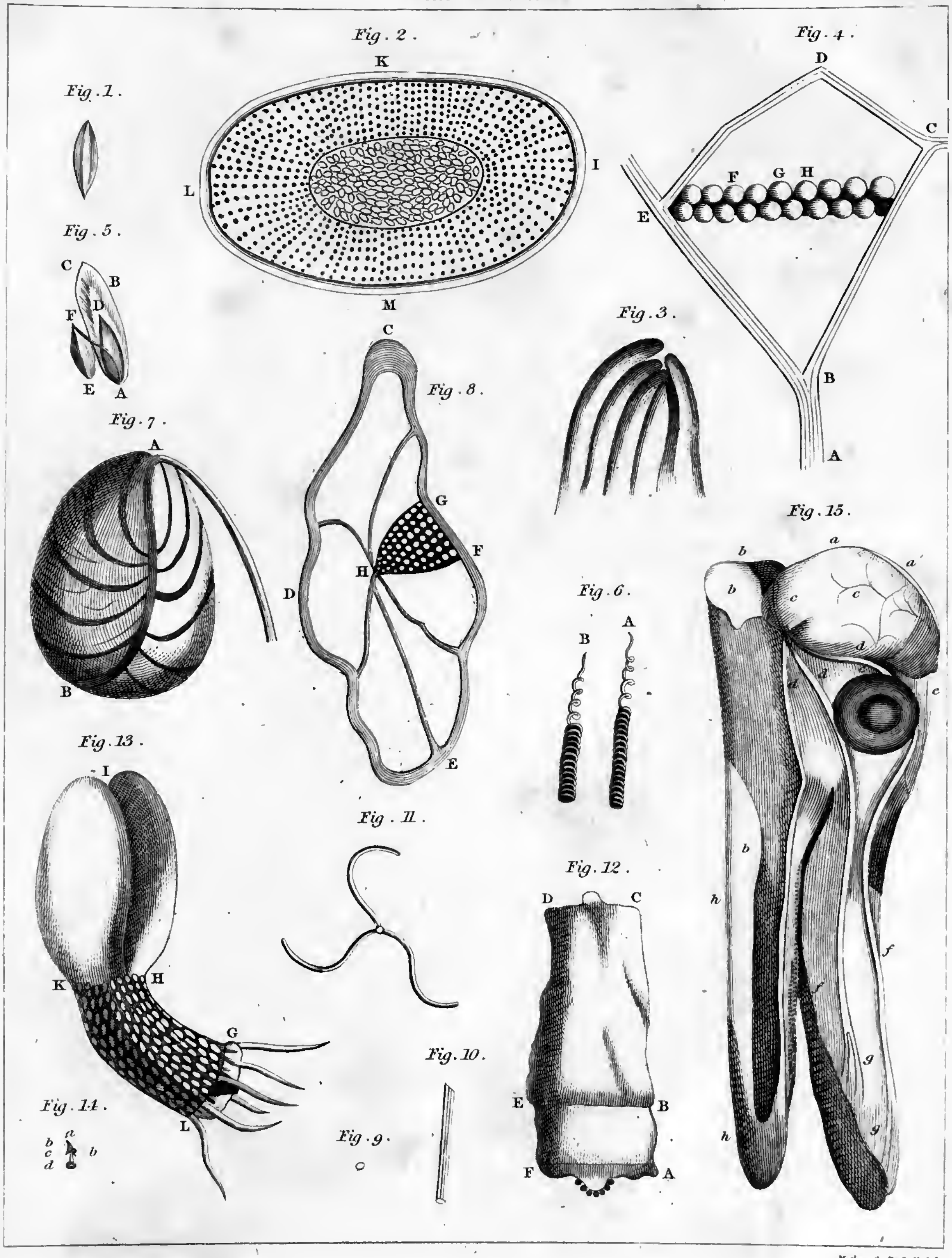

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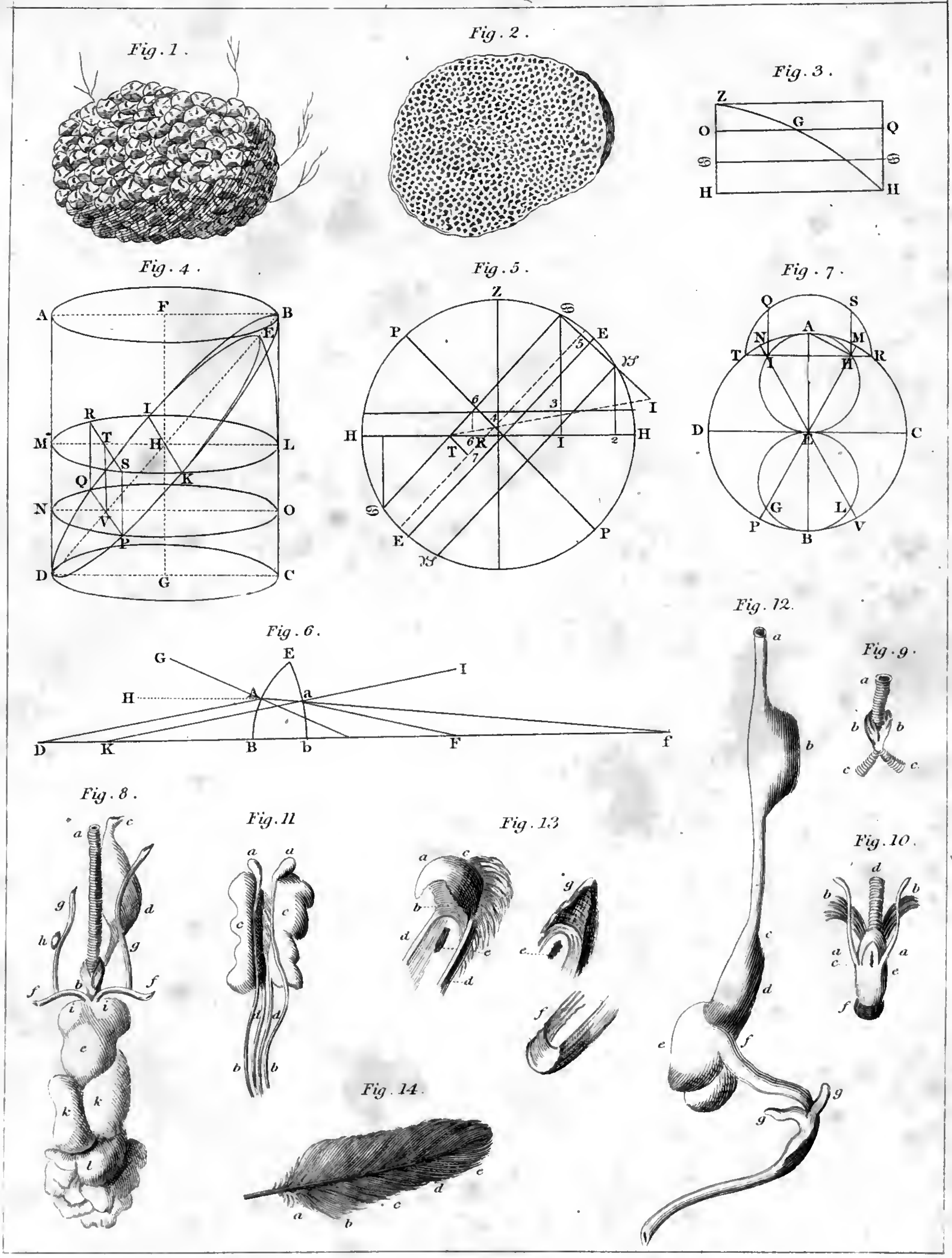

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